Décio Krause
Antonio Videira
Editors

VOLUME 290 BOSTON STUDIES IN THE PHILOSOPHY OF SCIENCE

Brazilian Studies in Philosophy and History of Science

An account of recent works

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An Account of Recent Works

Décio Krause · Antonio Videira
Editors

Springer

Editors
Décio Krause
Department of Philosophy
Universidade Federal de Santa Catarina
Altamiro Guimaraes 360ap. 1001
88015-510 Florianopolis Santa
Catarina, Brazil
deciokrause@gmail.com

Antonio Videira
Institute of philosophy and Human
Sciences
State University of Rio de Janeiro
Rio de Janeiro
Rua Machado de Assis, 17/101
CEP: 2220-060 Rio de Janeiro (RJ)
Brazil
guto@cbpf.br

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Preface

The goal of the present collection of essays is to offer a sampler of the current Brazilian research in the philosophy of science and on foundational issues.

Brazilian contributions to philosophical issues in science have been sporadic at best until the 1960s. We can of course mention the names of Otto de Alencar, Roberto Marinho de Azevedo, and Plínio Sussekind Rocha, but their contribution was restricted to local journals and to influencing students that took their courses. The production in the field became of international level due to the highly prolific contribution of Newton C. A. da Costa, whose original interest in nonclassical logics fanned out to include questions that range from the foundations of mathematics to philosophical themes in physics and economics. We must also mention the creation of CLE – Centre for Logic and Epistemology – at the University of Campinas in the 1970s; CLE became a focus for research in the philosophy of science and its researchers have established a high standard of quality that has spread out over many other university research teams in Brazil.

The present collection includes the work of logicians, of researchers on the foundations of science and on philosophical questions about science, and also of historians of science. It wasn't intended to provide a balanced view of the field; as any such sampling it may sometimes look haphazard, but its wide-ranging scope will certainly give an idea of the manifold research lines that are currently being pursued by Brazilian philosophers of science.

We have asked Professor Michel Paty to write a detailed introduction to the present volume, with comments on the papers that add up to the present book. His long contact with many Brazilian researchers makes him especially suited for the task.

We wish to thank our colleagues who have accepted to contribute to the present text. We must also thank Springer Verlag for accepting our proposal of this particular volume of essays to the Boston Studies in the Philosophy of Science.

The book is organized as follows (more details on the papers can be seen in the Introduction below).

I History of Science

History of science in Brazil was developed for several years by professional scientists who made use of it to show to laymen and also to the government the importance of funding science in order to achieve economic growth.

This closer relationship between history and professional scientists – clearly established in the book *Sciences in Brazil* in 1955, organized by Fernando de Azevedo is related with another thesis, which had been dominant during a major part of the XX century: for instance, that sciences in Brazil owe their development to the foundation of the first Brazilian universities in the decade of 1930. This thesis has been widely criticised since the early 1980s. Nowadays it is fully acknowledged that there has been science in Brazil, which was practiced in organized institutions, since the first half of the 1800s. The main reason why this historiographical perspective turn had occurred was the development of a small group of historians, many of them graduated in the History Department of The University of São Paulo, who were dedicated to explore the history of scientific institutions in the XIX century.

For some years it was common to find philosophers dedicated to the analysis of authors, most of them belonging to the French tradition, like Koyré, Canguilhem and Foucault, who were known for practicing a certain type of historical epistemology or epistemological history of science.

In a certain way, it is possible to summarize 3 main schools of thought in the history of science in Brazil. The first one is dedicated to the development of science in our country, covering a historical period from the latte XVII century to our present day. Conceptual history is seldom used by Brazilian researches. The second school discusses conceptual aspects, mainly related to specific disciplines such as physics and biology. Finally, in the third school, one can find works that can be also qualified as history of philosophy, since they approach issues such as: the notion of space in Newton; a notion of substance in Leibniz and the conception of inertia in Galileo.

This volume brings some contributions from the following Brazilian historians of science:

1. Galileo and Modern Science – Pablo Rubén Mariconda.
2. Newton and Inverse Problems – André K. T. Assis.
3. Isaac Newton, Robert Hook, and the mystery of orbit – Penha Dias & Teresinha J. Stuchi.
4. Sciences in Brazil: an overview from 1870 – 1920 – Maria Amélia Dantes, Silvia Figueirôa & Maria Margaret Lopes.
5. Henri Becquerel and radioactivity: a critical revision – Roberto de Andrade Martins.
6. Regeneration as a Difficulty for the Theory of Natural Selection: Morgan's Changing Attitudes, 1897–1932 – Lilian Al-Chueyr Pereira Martins.
7. Jean Antoine Nollet's contributions to the institutionalization of physics during the 18th century – Cibelle Celestino da Silva.

II Philosophy of Science

In Brazil, there is the National Association of Graduate Studies in Philosophy (Associação Nacional de Pós-Graduação em Filosofia – ANPOF), in which you can find work groups in philosophy of science with 19 researches that belong to 12 Brazilian universities. The works of those researches are developed under different topics, making a thematic and coherent organization possible.

Furthermore, there is at ANPOF at least three work groups that gather researchers that are dedicated to the philosophy of science (including philosophy of mathematics), philosophy of nature, philosophical history of science, logics and the foundations of science. This group division is explained, in some cases, by institutional reasons. The most important centre of history and philosophy of science in Brazil between 1977, the year of its foundation, until the second half of 1990, was the Centre of Logic and Epistemology (CLE) at UNICAMP. As far as we are concerned, it was the first Brazilian institution to offer master degree and PhD in the field of philosophy of science. The CLE had also organized several symposiums and colloquiums, which gathered researches from Brazil and other countries. Nowadays, the CLE continues with intense activity in the field of mathematical logic.

The field of philosophy of science has not been going through a positive moment. Its number of researchers does not grow as other fields in philosophy. In Brazil, philosophy focuses mainly in the history of philosophy. Although philosophy in Brazil grows in a slow pace, it has been passing though a diversified moment. The changes are so eminent that nowadays one can find researchers dedicated to other fields such as: philosophy of mathematics, philosophy of physics, philosophy of biology, philosophy of the social sciences, and philosophy of psychoanalyses, amongst others. This volume brings contributions from the following fields:

8. Natural Kinds as Scientific Models – Luiz Henrique Dutra
9. On the Nature of Mathematical Knowledge – Jairo José da Silva
10. The etiological approach to the concept of biological function – Karla Chediak
11. Human Evolution: Compatibilist Approaches – Paulo C. Abrantes
12. Functional explanations in biology, ecology, and Earth system science: Contributions from philosophy of biology – Nei Freitas Nunes-Neto & Charbel Niño El-Hani
13. On Darwin, Knowledge and Mirroring – Renan Springer de Freitas.
14. Freudian Psychoanalysis as a Model for Overcoming the Duality between Natural and Human Sciences – Richard Theisen Simanke.
15. The Causal Strength of Scientific Advances – Osvaldo Pessoa, Jr.
16. Contextualizing the Contexts of Discovery and Justification: How to do Science Studies in Brazil – Antonio Videira & André L. de O. Mendonça.
17. Echoes from the past: the persisting shadow of classical determinism in contemporary health sciences – Kenneth Rochel de Camargo, Jr.

III Foundations of Science

Until 15 years ago, most of the research lines of investigation were dedicated to conceptual reconstruction of the works of philosophers such as: Bachelard, Kuhn, Lakatos, Popper, amongst others.

From the second half of 1990, this situation has started to change, due to the publishing of thematic studies. The transition from authorial studies to the systematic ones has followed a trend observed in other countries, thus not with the same intensity.

Partly, that is due to Newton da Costa performance and of his group, spread out in different Brazilian and foreign institutions. However, the fields of paraconsistent logic and the foundations of physics are not the most intensely studied amongst those that can be located in the universe of the foundations of the science. Although that opinion might be questioned, there is a prevalence of the works in philosophy of the biology. There has been some research activity related to the matters of foundations of the quantum mechanics and of cosmology. For instance, the following chapters illustrate that tendency:

18. The metaphysics of non individuality – Décio Krause.
19. Einstein, Gödel, and the mathematics of time – Francisco A. Dória & Manuel Dória.
20. A contemporary view of population genetics in evolution, João Carlos M. Magalhães & Cedric Gondro.
21. Continuity and change: charting David Bohm's evolving ideas on quantum mechanics, Olival Freire Junior.
22. Quasi-Truth and Quantum Mechanics, Newton da Costa & Otavio Bueno.
23. The qualitative analysis of differential equations and the development of dynamical systems theory, Tatiana Roque.
24. Samuel Simon Rodrigues – The Problem of Adequacy of Mathematics to Physics: the Relativity Theory Case.

Florianopolis, Brazil Décio Krause
Rio de Janeiro, Brazil Antonio Videira

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Contributors

Paulo C. Abrantes University of Brasília, Brasília, Brazil, abrantes@unb.br; pabran406@yahoo.com.br

A.K.T. Assis Institut für Geschichte der Naturwissenschaften, Universität Hamburg, D-20146 Hamburg, Germany; Institute of Physics 'Gleb Wataghin', University of Campinas — UNICAMP, 13083-970 Campinas, SP, Brazil, assis@ifi.unicamp.br

Otávio Bueno Department of Philosophy, University of Miami, Coral Gables, FL 33124, USA, otaviobueno@mac.com

Karla Chediak Universidade do Estado do Rio de Janeiro, Rio de Janeiro, Brazil, kachediak@gmail.com

Newton C.A. da Costa Department of Philosophy, Federal University of Santa Catarina, Florianópolis, SC 88040-900, Brazil, ncacosta@terra.com.br

Maria Amélia Mascarenhas Dantes Department of History (retired), University of São Paulo/USP, São Paulo, Brazil, madantes@lycos.com

Jairo José da Silva Department of Mathematics, Unesp-Rio Claro, Rio Claro SP, Brazil, jairomat@linkway.com.br

Kenneth Rochel de Camargo, Jr. Instituto de Medicina Social, Universidade do Estado do Rio de Janeiro, Rio de Janeiro, RJ, Brazil and CNPq, kenneth@uerj.br

Renan Springer de Freitas Universidade Federal de Minas Gerais, Belo Horizonte, Brazil, Springer@netuno.lcc.ufmg.br

Penha Maria Cardoso Dias Instituto de Física, Universidade Federal do Rio de Janeiro, Rio de Janeiro, Brazil, penha@if.ufrj.br

Francisco Antonio Doria Advanced Studies Research Group, Fuzzy Sets Laboratory, PIT, Production Engineering Program, COPPE, Universidade Federal do Rio de Janeiro, 21945–972 Rio de Janeiro, RJ, Brazil, fadoria@gmail.com

Manuel Doria Advanced Studies Research Group, Fuzzy Sets Laboratory, PIT, Production Engineering Program, COPPE, Universidade Federal do Rio de Janeiro, 21945–972 Rio de Janeiro, RJ, Brazil, manueldoria@gmail.com

Luiz Henrique Dutra Federal University of Santa Catarina, and CNPq, Florianópolis, Brazil, lhdutra@cfh.ufsc.br

Charbel Niño El-Hani Research Group on History, Philosophy, and Biology Teaching, Institute of Biology, Federal University of Bahia, Ondina, Salvador-BA, Brazil 40170-115, charbel.elhani@pq.cnpq.br

Silvia Figueirôa Instituto de Geociências, Unicamp, Campinas, Brazil, figueiroa@ige.unicamp.br

Olival Freire, Jr. Instituto de Fisica-UFBa-Brazil, freirejr@ufba.br

Cedric Gondro The Institute for Genetics and Bioinformatics, The University of New England, Armidale, Australia, cgondro2@une.edu.au

Décio Krause Department of Philosophy, Federal University of Santa Catarina, 88040-900 Florianópolis, SC, Brazil, deciokrause@gmail.com

Maria Margaret Lopes University of Evora, Evora, Portugal, mmlopes@uevora.pt

João Carlos M. Magalhães Department of Genetics, Federal University of Parana, Curitiba, Brazil, jcmm@ufpr.br

Pablo Rubén Mariconda Departamento de Filosofia, FFLCH, USP, São Paulo, Brazil, ariconda@usp.br

Lilian Al-Chueyr Pereira Martins Programa de Estudos Pós-Graduados em História da Ciência (Pontifícia Universidade Católica de São Paulo); Grupo de História e Teoria da Ciência (Universidade Estadual de Campinas), Brazil, Lacpm@uol.com.br

Roberto de Andrade Martins Group of History and Theory of Science, Universidade Estadual de Campinas (Unicamp), 13083-970 Campinas, Brazil, Rmartins@ifi.unicamp.br

André L. de O. Mendonça Institute of Philosophy and Human Sciences, State University of Rio de Janeiro, Rio de Janeiro, Brazil, andre.o.mendonca@ibest.com.br

Nei Freitas Nunes-Neto Research Group on History, Philosophy, and Biology Teaching, Institute of Biology, Federal University of Bahia, Ondina, Salvador-BA, Brazil 40170-115, nunesneto@gmail.com

Michel Paty Equipe Rehseis, UMR 7596, CNRS et Université Paris 7-Denis Diderot, Paris, France, michel.paty@univ-paris-diderot.fr

Osvaldo Pessoa, Jr. Department of Philosophy, FFLCH, University of São Paulo, São Paulo, Brazil; Visiting the Department of History and Philosophy of Science, Indiana University, Bloomington, IN 47405, USA, opessoa@usp.br

Tatiana Roque Professora do Instituto de Matemática, Universidade Federal do Rio de Janeiro, Ilha do Fundão – Rio de Janeiro, RJ-Brasil, tati@im.ufrj.br

Cibelle Celestino Silva Institute of Physics of Sao Carlos, University of Sao Paulo, Sao Paulo, Brazil, cibelle@ifsc.usp.br

Richard Theisen Simanke Federal University of Sao Carlos, São Carlos, Brazil, richardsimanke@uol.com.br

Samuel Simon Department of Philosophy, University of Brasilia, Brasilia, Brazil, samuell@unb.br

Teresinha J. Stuchi Instituto de Física, Universidade Federal do Rio de Janeiro, Rio de Janeiro, Brazil, tstuchi@if.ufrj.br

Antonio Videira Institute of Philosophy and Human Sciences, State University of Rio de Janeiro, Rio de Janeiro, Rua Machado de Assis, 17/101, CEP: 2220-060 Rio de Janeiro (RJ), Brazil, guto@cbpf.br

Chapter 1
Introduction

Michel Paty

1.1 A First Reflexion

When the organizers of this book kindly asked me to write the Introduction, I felt at the same time honoured and abashed (embarrassé), both because of my nationality, being not a Brazilian but a French. I felt honoured and pleased, for it meant that they consider me to some extent as pertaining to Brazilian culture, and particularly to the Brazilian academic milieu of philosophers. True, other friends and colleagues in this country already told me that, and even use to say it, and I know that they not only say it but think it sincerely. Even if it is not new for me, this adoption makes me always very pleased and I must say that I reciprocally feel at home in Brazil – I mean the country, the people and the culture -, and this reciprocal reconnaissance is grounded on many years (44, indeed) of convivence and work with Brazilian scholars, professors, searchers and students as well, this being not exclusive of course. And I think important that it be not exclusive, for various obvious reasons, among which that one: the academic and intellectual milieu and concern are not separated from the rest of social life, in all its dimensions. This is true also as well of philosophy, "even" philosophy of science if I dare say, and such will be the meaning of my first reflection in this Introduction.

But before coming to it, I must complete the comment about my reaction to the invitation that was made to me. To endeavour a somewhat meaningful presentation of something like the "Brazilian Philosophy of Science", which is the aim of this book, is a difficult and perilous task. Difficult, for one never knows all what has been and is being done in any field of knowledge, one is never in conditions to make a thorough evaluation, and not even an overall grasp of the main meaningful contributions, particularly concerning such a field as Philosophy of Science. Perilous, for besides the unavoidable incompleteness of the information one disposes of, the objectivity of choices and judgements in such matters can never be guaranteed, although it should be eagerly looked for.

M. Paty (✉)
Equipe Rehseis, UMR 7596, CNRS et Université Paris 7-Denis Diderot, Paris, France
e-mail: michel.paty@univ-paris-diderot.fr

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It is in the power of nobody to possess the truth, and the claim of its relativeness cannot hide its exigency and necessity. Philosophy is precisely the search for truth together with the consciousness that truth stands beyond our limitations (without such a conviction the idea of knowledge would be devoid of meaning), and it is therefore also at the same time a reflection on its conditions, but in no case can it lead to a negation of the very idea of it. Truth escapes our prejudices and personal or group interests, and our own point of view is only justifiable insofar as it is aimed at establishing it.

Everyone has his own idea of what is or would be important in such matters as Philosophy of Science and Epistemology, and I by no means would like to close the subject or give the impression that it could be ended up by this or by another contribution. Intellectual modesty and honesty are first of all required when trying to give some account of something that exists, particularly in intellectual matters, when one is aware that the present is an open process and that new and richer ideas are to be expected from the future, that is, from yet "unknown" people, some of whom might be already active, yet unnoticed, searchers, or simply students. This remark is of general application, but suits particularly the Brazilian academic and intellectual context, if we consider that Philosophy of Science, which today is blossoming in Brazil, constitutes a rather recent field of interest in the country. That means, considering the rythm with which it has been growing since three decades, that a lot more of ideas are expected to come to morrow. Provided with what we know, or what we think we know, we are not the owners of knowledge and of ideas, which go their way, escape from us, and finally shall pronounce about the statements we have emitted, and shall in turn evaluate our evaluations.

The organizers have chosen to present a significant sample of recent works by the Brazilian searchers themselves in the domain of the philosophical and epistemological analysis of Science, rather than compiling a synthetic overview of what has been achieved up to now, distributed according to the various themas, in the vein of other titles in the same series. Actually it seems that this modality – to edit a representative sample – is quite appropriate to the peculiarity of the developments in a domain which it is quite recent. For it is admitted that a sample cannot be a thoroughly faithful, i.e. objective and complete, description. And the risk to have omitted important aspects is therefore lesser, due to the conscious limited scope of the enterprise. Furthermore, a sample of present research works, representative enough of the themes of interest, of the ways of thinking and analyzing, brings with it the liveliness of the springing out, the youthfulness of spontaneity.

Now the risk shifts from the organizers to the presentator, whose unavoidable insuficiencies will very easily be charged against him not as inherent to the difficulty of the exercise but as proofs of his prejudices and of the narrowness of his mind. But so many opinions are running anywhere, which are not always guided by a feeling for justice or truth but often by passion, narcissism and lack of indulgency, that with the help of my – already – white hairs experience, I decidedly prefer not worrying any more about it, and try to make my best. I anyhow apologize for the lacks and errors of this essay.

To really introduce such a matter would need actually a true program of research, in particular to get a significant understanding of how things got their place in the context, and through history. This introduction could not have this pretention, but I tried to put in perspective the present contributions, which I shall briefly introduce and eventually comment one by one, after having tried to sketch how, on the whole, they have been made possible thanks to the sequence of previous stages. Among these last stand the "ancestors", and then the first "pioneers" in philosophy of knowledge and of science, and in history of science as well. "Ancestors" and "pioneers" of the country, Brazil, for sure, but without omitting other, external, influences and sources of inspiration, as it is to be expected for a kind of thinking like Philosophy. I shall propose a quick survey of them, and also of some of the intellectual, social and institutional conditions in which they were merged.

In what follows, I shall take the point of view of Philosophy of Science as I see it and understand it, a point of view that is shared in its main lines by an important number of my Brazilian colleagues, probably to a more or less same extent as in my own country, France, and in other European ones, such as Italy, Portugal, Spain, Belgium, Germany, heirs of the so-called "continental" philosophical tradition; it is fair to say that this view is present too, although in a less dominant way, in other places where more univoquely "analytic", to make it short, tendencies prevail. In the conception of Philosophy of Science to which I refer, Philosophy in general and its subdomain Philosophy of Science are not separated from other fields of intellectual and cognitive activity, although they have a different aim and modality than getting more knowledge – they look for meaning, soundness and conditions of possibility, trying to understand critically what is "to know" and how it has been effective – which implies some historical concern. The first ones of these fields to consider are obviously the Sciences in general (Exact, Natural, Human and Social Sciences) and the Techniques and Technology that go along with them. For the precise reflection on these, a specific branch of Philosophy of Science, namely Epistemology (defined as the critical approach of scientific concepts, theories and methods), has been developed.

With this definition of Epistemology, in which the Sciences are considered under the species of their knowledge content, but also as activity and practice in the various dimensions from mental to instrumental, History of Science plays an important role in it, insofar as the reflections on the Sciences are not limited to their present formulation and status, as if Science corresponded merely to a static body of knowledge and to given rigid methodological rules. It will be considered, on the contrary, that Science is always evoluting and in progress – although this last notion is questionable, being linked with other dimensions and problems, such as the nature of Reason and Rationality on one side, and the processes of History, including Social History, on the other. Clearly, Science as a Human activity is connected with its implications in other fields, such as Education in its various levels, from Primary School to University, and, through its applications and uses in Techniques and Technology, which entails considering Economical, Social and Political aspects connected with these, at the local and at more global levels.

The last remark points at the relevance, when one deals with scientific knowledge, of other fields of concern than Sciences, whose preoccupation therefore Philosophy of Science cannot escape. I shall not extend myself here neither on other fields of interest that are more traditional for Philosophy – and not for this of a lesser importance and urgency, if we think for example on human values -: these domains are Metaphysics, Moral and Ethics, Aesthetics, not to omit aspects of Religious and Theological thinking, as belonging to the sphere of man's thought and inquiry.

Having this in mind, a look at the historical development that led to the present state in Brazil of the activity concerning Philosophy of Science, and also History of science which goes in effect with it -, is interestingly illustrative of such interconnections. In Brazil (and most probably in many other places of the world, especially in the "Developing Countries"), the field of reflection and activity corresponding to Philosophy of Science would not be conceivable and livable as a kind of thought and academic activity closed in itself, as if it were nothing more than a kind of formal or scholastic game, separated from the other dimensions of man's destiny and interests. This appears even more obvious for the History of Science and Technique, which are, due to their object and purpose, more directly embedded in the social and historical reality. Philosophy of science, as a rational reflection and analysis of scientific knowledge, would not have unfolded in the country if the necessity of such a reflection had not mobilized minds at the various stages of the society and science development, for reasons that uncover a large spectrum, corresponding to the various aspects and dimensions mentioned above. To show it is the purpose of the rapid sketch that follows.

1.2 A Short Account on the History of the Philosophy and Sciences in Brazil

Generally speaking, Philosophy of Science, as a specification of Philosophy of Knowledge, is a part of Philosophy, and it corresponds to a specialization in the domain of Philosophy that is rather recent. The proper expression "Philosophy of science" is met probably for the first time with the book *Essay on Philosophy of science* (*Essai de Philosophie des Sciences,* 1838), whose author was the French physicist André-Marie Ampère, the founder of the science of Electrodynamics. His book is an attempt to classify scientific knowledge according to some philosophical criteria of his own, in the line – but with significant differences – of the classification of the sciences and human activities proposed in the middle of eighteenth century in the monumental *Encyclopedy* (*Encyclopédie*) of d'Alembert and Diderot (published from 1751 up to 1780). This previous classification, in its turn, was inspired by the one proposed by Francis Bacon at the beginning of the "Scientific Revolution" of seventeenth century, but with important modifications due to the "explosion" of exact, natural, human and social scientific knowledges that had occurred in the interval. Ampère's classification and philosophy, which remained somewhat confidential, was contemporary with another, somewhat different, project, also an heir of the Enlightenment *Encyclopedy*, namely that of Auguste Comte's *Lessons of*

Positive Philosophy (*Cours de Philosophie Positive*), which gave more emphasis to the historical development, and which marks, according to Georges Canguilhem, the beginning of History of Science.[1]

Actually, with Auguste Comte we get into the remote beginnings of Philosophy of Science in Brazil since, as it is well known, Comte's positivist philosophy had a strong influence in the country since the midsts of nineteenth century, not only in the political milieux but also in the juridical, medical, engineer, intellectual and political ones as well.

One should first evoke at this point what has been at that time the place of Philosophy in Brazil, up to the more recent period when Philosophy of Science began its blossoming, for the conditions of appearance and developing of the latter were primarily, as in any other place in the world, prepared by the existence of a philosophical concern and culture, taking Philosophy in all its generality. We should also mention the state of the sciences, which were present also in Brazil already in the nineteenth century, although unequally, and above all in the few existing institutes of applied research such as Mining, Engineer, Military, Law and Medicine Schools and Institutes, Observatories (devoted to geographical and astronomical goals), and Museums of Natural History, all having been created essentially in that period, after the country took its independence from Portugal, and under the Imperial as well as the subsequent Republican regimes. Journals and cultural circles were also places where philosophical matters were discussed, not to omit book reading, favoured by the presence of libraries and of a rather large number of bookshops in the main cities like Rio de Janeiro, São Paulo, Recife, Salvador, Porto Alegre, which diffused books from abroad and, after the Independence, published locally.

The imported books were the main vehicle of the culture from Europe, and much later on from United States, towards Brazil and they contributed to a large extent to the formation of a genuine Brazilian culture of the high and middle classes; they were predominantly written in Portuguese, in French, to a lesser degree in German and in English.[2] These were also the written cultures from which Brazil received its strongest influence, particularly concerning philosophical ideas. In his *Panorama of Brazilian Philosophy* (*Panorama da Filosofia Brasileira*),[3] Ricardo Vélez Rodríguez identifies six "moments" of a large French influence on the Brazilian thought in Philosophy. The first one being the epoch of Enlightenment (second half of eighteenth century and beginning of the XIXth), and scientiific ideas

[1]See G. Canguilhem, *Etudes d'histoire et de philosophie des sciences*, Vrin, Paris, 1968 (Sth ed.,1989, p. 61 sq.); M. Fichant, L'idée d'une histoire des Sciences, *in* M. Fichant et M. Pécheux, *Sur l'histoire des Sciences*, Maspero, Paris, 1969.

[2]Under the colonial regime, Brazil was not authorized to print books neither to create universities, at variance with the Spanish colonies in America – whose Universities were under the control of the Catholic Church. Among the foreign languages and cultures, one should add Italian (since the end of nineteenth century, due to an important immigration, mostly in the State of São Paulo) and Spanish (even more ecent, for Latin-American integration has been favoured only in the lates decades).

[3]Rodriguez (1985–1993).

being present through political arithmetics (with Condorcet and Laplace) and social physiology (with Cabanis, Bichat, Pinel, Vicq d'Azur and Saint-Simon). The three following "moments" would correspond, according to the referred author, respectively to a "spiritual eclectism" (inspired by Maine de Biran and Victor Cousin), to the various doctrines of liberalism related with the constitution of government institutions (doctrinarian liberalism in the vein of Guizot, Royer-Collard, etc.; and democatic liberalism inspired by Tocqueville), and to the "elaboration of traditionalism" (enrooted in Joseph de Maistre and Louis de Bonald). Then come the two last periods described, that of the rising of positivism centered around Comte's idea of a "social physics",[4] followed by a spiritualist anti-positivist reaction (taking its sources in Bergson an Blondel). I don't know to which extent such a description is totally founded, as one could object to it that some of these philosophical currents co-existed in time in parallel or concurrently, motivated by quite different contexts and ideologies. It testifies nevertheless the significant presence of French Philosophy in Brazil, to which actually other sources have to be added. I have emphasized the influences from abroad: needless to say that in the meantime many books written and published in Brazil since the second half of nineteenth century did contribute to the formulation of these and other ideas and doctrines coined locally.

Auguste Comte's *Cours de Philosophie Positive* (6 vols., 1830–1842) and *Système de Politique Positive* (4 vols., 1851–1854), in one or the other of their various re-publications (in French), were largely diffused through the nineteenth century Brazil,[5] where Comte's positivism has been very influential for a long time in the intellectual, social and political elites, being locally adapted through a genuine Brazilian flavor, which included the explicit and zealed adhesion to the "religion of Humanity", with temples, popular pamphlets on all society subjects, and numerous militants, from the most schematic to the most refined minds.[6] The important Comtian positivist tradition in Brazil that continued – although to a lesser degree – up to the middle of twentieth century, was effective in social, juridical and political matters, and marked also generations of engineers and science teachers. It began already around 1830, when some Brazilian jurists and engineers attended in Paris Comte's Lectures on Positivist Philosophy, and back to they country diffused it, the main centre at that time being Recife, from where it reached the rest of Brazil. Among the many local publications disseminating Comte's doctrine, one notes scientific textbooks composed in a strict positivist adherence, particularly concerning mathematics,[7] which endowed a philosophical conception on nature and

[4] An idea, to say it *en passant*, that was more based on a biological static model, that of Anatomy, rather than on a physical – mechanical – one: see Benoit (1999)2007.

[5] An interesting symptoma of that diffusion is its present remnents: those books are still very commonly at sale in the second hand bookshops of the Brazilian cities.

[6] On positivism in Brasil, see: Arbousse-Bastide (1957), Lins (1964), Arantes (1988).

[7] In the line inspired by Pierre Laffitte, a french disciple of Comte, whose book on Arithmetics was re-edited in 1880, not in France but in Brazil – in French. Typical of the brazilian positivist conception of Mathematics are the books written at the turn of the twentieth century by the militar Marshall Trompowski (see for instance Trompowski (1903)).

on knowledge. At the beginning of twentieth century opponents to the rigidity of the positivist doctrine began to react against it, such as the mathematician Otto de Alencar (1874–1912) and, some time later, the engineer and physic-mathematician Manuel Amoroso Costa (1885–1928) (both in Rio de Janeiro).[8] The creation of the Brazilian Academy of Science, contemporary of the debate on the Theory of relativity, which will be evoked below, marked in a way the end of the positivist dominance in science. One may put on behalf of the long-during tight connection in Brazil of Science and Positivism that somewhat characteristic tendency, still holding frequently in the intellectual milieu, to quickly qualify as "positivist", with a pejorative intention, scientific decidedly oriented minds, and even Philosophy of science as well.

Positivism was not, however, even at the turn of nineteenth to twentieth century, the only noticeable philosophical inclination, even considering the preoccupation with science. The so-called "Recife School", for instance, was rather influential as well: it is illustrated in particular by two renown thinkers in the Philosophy of Law that were akin as to their directions of thought, namely Tobias Barreto and Farias Brito. Tobias Barreto de Meneses (1839–1889), initially inclined towards Religious Philosophy, got interested in the idea of evolution through the writings of Ludwig Büchner and Ernest Haeckel. Raimundo Farias Brito (1862–1917), who was also initially oriented towards spirituality, developed a Philosophy of the Mind and of Knowledge,[9] with such books as *The Finality of the World* (*A Finalidade do Mundo*, 3 volumes, 1894–1905), *The Physical basis of the Mind* (*A Base Física do Espírito*, 1912), *The Inside World* (*O Mundo interior*, 1914). Nourrished by the works of Fiedrich Lange – the neo-kantian German philosopher, author of a classical *History of Materialism* and of Théodule Ribot (French author of books on Psychology and on Logics), he was an adept of the "Psychophysics" doctrine. Led by the idea of the reestablishment of Metaphysics, he turned himself towards a naturalist-religious conception inspired by Comte and Spencer.

As to the development of sciences in Brazil, it can be traced back to its very beginning[10] in the period where Institutions were absent, up to a fist of individual personalities acting in scientific research. José Bonifacio (1763–1838), an engineer and Mines Intendent, also diplomat, and political figure – he has been one of the main promoters of the independence of Brazil from the Portuguese Crown -, has probably been the first Brazilian scientist in the proper meaning. As a renown geologist, he was member of various European Academies of Science. On must mention also the first mathematician in Brazil, Joaquim Gomes de Souza (1829–1864), who

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[8] Silva (1898), Costa (1999). See Paty (1992a).

[9] Among the many essays on this author, Farias Brito's work has been extensively studied and situated in its context in Carvalho (1951)1977.

[10] Not to speak of the native traditional knowledges, about which one can get informations in the anthropologists' works (such as those of Jehan Vellard, Alfred Métraux, Luis de Castro Farias, Darcy and Berta Ribeiro, Eduardo Viveiros de Castro, Ugo Maia, and others), and from recent studies on Ethnosciences. A pionneer in Brasil for Ethnomathematics is the mathematician Ubiratan d'Ambrosio (b. 1932).

published a book written in French, on Differential Calculus, in Leipzig (1882), and the many foreign and some Brazilian scientists travelers, mainly naturalists, who pertain also to the early History of Science in Brazil. Some even settled in the country, as Peter W. Lund (1801–1880), the Danish paleontologist who discovered around 1840 the most ancient fossil of man in Americas (the "Man of Lagoa Santa", as old as 12,000 years).

Actually, Science began to be more effective with the already mentioned Institutes for applied research founded in nineteenth century, after the Independence, under the Empire (1822–1889) and the Republic – from 1889 up to and including the first half of nineteenth century. One must quote especially Oswaldo Cruz (1872–1917), a microbiologist and physician – formed in Institut Pasteur in France -, a pioneer in Health Science and health organization and politics in Brazil, and his disciple Carlos Chagas (1879–1934), the discoverer of the cause of the illness named after him (Chagas' illness), to mention just a few,[11] for afterwards the list of brazilian scientists with important contributions would progressively grow up to a large scale, throughout all he branches of science, exact, natural, human and social.[12] But in this transition an important change had occurred: the formation and development of Universities, which would favour also the training in Philosophy and the advance in this discipline.

One notes, however, before this decisive advent, although rather sporadically, discussions and contributions that already belong to the field of Philosophy of Science, by some outstanding personalities, related in particular with scientific advances at the international level which generated debates, such as the Darwinian Theory of Evolution in the domain of Life Sciences,[13] or the Einsteinian Theory of Relativity in the domains of Physics and Mathematics.[14] As to the debate on the Theory of Relativity, which occurred throughout the years 1920–1930, it had been initiated on occasion of the 1919 Eddington's expedition in Sobral – North-East of Brazil, situated in the Equator line -, on behalf of the British Royal and Astronomical Societies, to perform astronomical observations during the Sun's eclipse which occurred that year. The observation of the deflection of light rays from stars when passing in the vicinity of the Sun confirmed the prediction of Einstein's General Relativity Theory of the curvature of space-time due to the gravitational masses it contains. Similarly to what happened in other countries in the World, the somewhat radical renewal of the commonly shared conceptions of space, time and matter impulsed a passionate debate, whose climax was Einstein's visit in Brazil in the year 1925.

[11] See, for instance, on Oswaldo Cruz and the Institute of Research named after him, Stepan (1976) and Delaporte (1999) on Carlos Chagas.

[12] On the History of Science in Brazil, see: Azevedo (1943, 1955), Ferri & Motoyama (1979–1981), Schwartzman (1979), Quipu (1988). On the History of Techniques and Technology in Brazil, see: Gama (1983), Vargas (1994). For a shorter overall scope on History of Science in Latin America, see: Paty (1992b).

[13] Domingues, Romero & Glick (2003).

[14] Costa, M.A (1929) 1981; Moreira & Videira (1995); Paty (1996) 2000

But to speak of Philosophy of Science in the proper meaning, in a significant way, one had to wait the University institutionalization, which came only late. Even when the colonial period was over and with it the prohibition to create Universities in the country, it took a rather long time to get there. The development of Professional Schools came first, priority being given in the independent Brazil to technical formation, a tendency which was encouraged by the positivist-minded leaders. A great debate occurred in the Brazilian society in the years 1880 about the opportunity to develop University: those who advocated in favour of it had in mind the successful model of the German University founded some decades earlier on the lines of Wilhelm von Humboldt's proposal, whence the opponents were adepts of positivism.[15] The institution of University began in Brazil in 1934, with the creation of the University of São Paulo, in the economically richest State of the Federation, and it was supported – as it is up to nowadays – by this State itself, in the context of political circumstances whose consideration escapes the scope of this Introduction. It was followed some time later by the creation of the University of the Federal District in Rio de Janeiro, which was to become later the Federal University of Rio de Janeiro, in 1935, and of the Federal University of Rio Grande do Sul, in Porto Alegre, in 1937. Later on other public Universities would be created in the important cities at the Federal level and to a lesser extent at the States level.[16] Already since their beginnings, the University of São Paulo, and subsequently the University of the Federal District included Philosophy in their curriculae, besides the various branches of positive knowledge and humanities, and this was actually the starting point of a significant development of Philosophy in the country, at the formation as well as at the research levels, in connection with the various other disciplines. Thanks to the existence of an academic philosophical community, cultivating the philosophical exigency in the spirit of university aims, immersed inside the various directions of the sciences, humanities and arts, original and well-formulated reflections and studies on knowledge, on thought, on science and on human activities, could henceforth be produced and, in their turn, contribute to reinforce and enrich the intellectual life with an exigent and precise philosophical worry, disseminating, and able to focus on new themes. Such indeed, has been the process that has possibilited – to keep our concern – the development of Philosophy of Science.

The history of the first, chronologically, and the still most important University in the country, namely the University of São Paulo (sometimes quoted hereafter as USP), is quite eloquent in this respect. Besides the Institutes of Exact and Natural Sciences, a Faculty of Letters, Philosophy and Human Sciences was settled, composed of Departments among which that of Philosophy. To help in getting as soon as possible the desired level, collaboration from abroad was acknowledged, and a mission had been sent to Europe to contract professors on behalf of the project leaders (among whom Julio de Mesquita Filho was a prominent figure) acting for

[15] Barros S.P.M. (1986), p. 341–409.

[16] Private Universities have also multiplied in the last decades, but only a few of them – essentially the catholic Pontifical Unversities – have aims and levels comparable to the public ones.

the Governor of the State of São Paulo, Armando Sales de Oliveira. The main responsible for the prospection and choices was the Brazilian physicist Theodoro Ramos (1895–1936), well trained in Quantum and Radiation Physics, in which he had already given original contributions. He went to Italy, Germany and France, and got help notably from Enrico Fermi: Gleb Wataghin, of Russian origin and a Fermi's close collaborator was contracted and impulsed decisively and successfully the development of Physics at USP; for Mathematics, Luigi Fantappié was contracted. Germany was a problem because of the takeover by Nazism, and attention was given to scientists menaced by prosecution, such as the chemist Reinhardt Reinbolt, of a jewish origin: accepting the offer made to him by the USP, he started a new and fructiferous career in Brazil. Two prosecuted German professors were contracted also in Biology.

As for the Human Sciences, it had been decided that French would be welcome and a large amount of decision was let to the Sorbonne professor in Medicine and social scientist Georges Dumas (1866–1946), who had been involved for years in French-Brasilian cultural cooperation.[17] By his mediation, young promising professors, "agrégés" but not yet doctors, young scholars beginning their academic career, were contracted, among whom in the 1st years Paul Arbousse-Bastide (b. 1899) in Sociology, Claude Lévi-Strauss (1909–2010) in Anthropology and Ethnology, substituted after a few years by Roger Bastide (1898–1974), Robert Garric (1896–1967) and Etienne Borne (1907–1993) then substituted by Jean Maugüe (1904–1990) in Philosophy, Fernand Braudel (1902–1985) in History, Pierre Deffontaines (1894–1978) and then Pierre Monbeig (1908–1987) in Geography, François Perroux (1903–1987) in Economy, the list being not exhaustive. Some of them would stay many years in Brazil – spending the War time there, developing studies on Brazil. Nearly all of them would become, later on, after he end of the Second World War, prominent and renown in their respective fields. Indeed, in all the mentioned disciplines of Exact, Natural, Human and Social Sciences, the choice had been very good. Their teaching and the impulse they gave for investigation to their Brazilian students and collaborators were of a seminal importance – it has often been celebrated.[18]

Needless to say that the contribution of Brazilian teachers and students yet in those 1st years of the new University life was essential to the success of the enterprise: potentialities already present could cristallize in the University ambience[19] and the USP soon became a crucible for knowledge and research. Claude Lévi-Strauss evokes, in its *Tristes Tropiques*, the brilliant personalities he and his colleagues had as students, "a fist of gifted children", who succeeded, in a few

[17] Pettjean (1996).

[18] The french philosophers who tought at the University of the Federal District (UDF), in Rio de Janeiro, were Emile Bréhier (1876–1952), historian of Philosophy. Etienne Souriau (1892–1979), philosopher of Aesthetics, both staying from 1936 to 1939, and Henri Poirier (philospher of Knowledge and of Science, see later on), who stayed from 1939 to 1945 (the UDF had become the University of Brasil).

[19] In this respect on Social Sciences see Queiroz (1996). On brazilian culture in the years 1933 to 1974, see in particular Mota (1977).

decades, to turn upside down the history of their country.[20] The economically leading State of the Country had given the example, and it would soon after stir up emulation, as already mentioned. Clearly Philosophy of Science was not a priority at that time. Nevertheless it is interesting to note that the first foreign professors in the new disciplines of Social Sciences such as Sociology and Anthropology had been formed initially in Philosophy: such was the case of Paul Arbousse-Bastide, Claude Lévi-Strauss and Roger Bastide, and their teaching and works included the preoccupation for the Philosophy of Social Sciences. This flavour has also been present in a number of their students and disciples.

Such have been, anyhow, the very first steps of the settlement of universitary Philosophy in São Paulo and in Brazil. After these very beginnings, Philosophy developed there and in various other places and in various directions, and with it Philosophy of Knowledge and of Science, to which we finally arrive at now.

1.3 The Pioneer Generation in Philosophy of Knowledge and Philosophy and History of Science

With the era of universities, the formative influence and the intellectual dynamics of Brazilian philosophers has been very important since these beginnings in the intellectual nourishing and maturation of philosophers of the younger successive generations. One should evoke now the "pioneer generation" of philosophers in Brazil that has settled philosophy, creating the intellectual conditions for it, and that has made it a living community through the cultivating of philosophical culture and spirit. We shall mention also a number of thinkers who through the decades have been significantly influential in Philosophy by their teaching and publications, even if they we re not philosophers of science stricto sensu: Philosophy of Science is, so to speak, a specialization of the Philosophy of Knowledge which, as to it, is present in many areas of Philosophy, from its History to Contemporary concerns such as Aesthetics, Ethics, Moral, as well as Philosophy of the Right (or Law), of Politics, of Education. . .[21]

We note the importance, in these beginnings, of the Philosophy of Education, which included a strong concern for the Philosophy of Knowledge, and was directly implied in the thinking of knowledge conceived with a university spirit. Anisio Teixeira (1900–1971) has been one of the greatest Brazilian figures in Education and Philosophy of Education. He was a jurist, an educator and as an intellectual personality he took part in social and political debates. Inclined towards the views of the north American philosopher John Dewey, he was a militant for the democratization of Education, and professed the ideas of the vanguard Brazilian "New School" ("Escola Nova") movement, influential in the 1920 and 1930, which considered that

[20] Lévi-Strasuss (1955), 1965 ed., p. 88.

[21] On Philosophy in Latin America, see Dascal (1987); in Brazil, see: Cruz Costa (1945, 1956); Paim (1967, 1979); Reale (1976, 1994); Rodriguez (1985–1993); Severino (1999).

teaching should be public, gratuitous, laic and obligatory, and which favoured in education the development of the intellect and of the capacity of judgment instead of the traditionally favoured memorization.[22] He himself has been the artisan of the reform of the educational systems of the State of Bahia and of the City of Rio de Janeiro, and later on the main inspirer of the innovative project of the University of Brasilia of which he was – for a short time due the political circumstances – the first Rector.[23]

As to Philosophy of the Right and Law, it had been traditionally one of the first grounds for Philosophy in Brazil. One of his best known representants in the considered period was Miguel Reale (1910–2006), who formulated the "Three-dimensional Theory of the Right" (according to which *fact*, *value* and *juridical norm* together make the concept of Right), and was a professor at the University of São Paulo. He had been the theoretician of "integralism" (the doctrine of the autoritarian government of the first Presidence of Getulio Vargas, which might be considered as the brazilian version of Fascism) and later on of the conservative conception of Politics which tried to legitimate the militar dictarioral Goverment of 1964, before he took some distance with it because of its violation of human rights. As Rector of the São Paulo University during a period of the military regime, from 1969 to 1973, he protected to some extent the institution and a number of his colleagues. The political engagement of philosophers is a matter that is worth of consideration but does not enter really in the scope of this presentation essay.[24] Let us mention however that on the opposite political side, Brazilian philosophers and social scientists prosecuted by the dictatorial government – which hold from 1964 up to 1982, before the coming back of democracy –[25]contributed to deepen political reflexion in relation with social and economical studies, such as the CEBRAP Institute (Centro de Estudos Brasileiros *xxx*), devoted to interdisciplinary research on philosophical, economical and political problems, with its referencial Journal *Novos Estudos*; not

[22]Teixeira (1969a, b, 1998).

[23]Anísio Teixeira's committment with the beginnings of the University of Brasilia (UnB) has been in tight collaboration with the ethnologist Darcy Ribeiro. See: Teixeira & Ribeiro (1962), Ribeiro (1978). Unfortunately, the University of Brasilia (UnB), a progressist institution conceived with a special cconcern towards the problems of the developing countries, was violently stopped at the end of 1965, after 2 years of full existence, by the hostility and repression of the militar dictatorial government that was issued from the putsch of april 1964. I personnally had the privilege to participate as a young visiting professor (at that time, in Physics) to the last semester of this "interrupted University", to borrow the expression from one of the main protagonists – and later its historian – of this adventure, which was also somehow an epopeia: Salmeron (1998) 2007.

[24]On the political engagement of philosophers, see Nobre & Rego (2000). See also the critical reading of this book by Ricardo Musse, "Da militância política à filosofia. Um panorama da filosofia brasileira", *Folha de São Paulo*, 10.02.2001.

[25]A large number of universitary professors, in particular in Humanities and Social Sciences, were compulsory dismissed, emprisoned or obliged to go to exile. Among these, in São Paulo University, the professors of Philosophy J. A. Gianotti, Bento Prado de Almeida Ferraz Jr, Ruy Fausto; the professors of Sociology *xxx*, Fernando Henrique Cardoso (later President of the Republic), and a number of others. Gianotti and Cardoso were among the founders of the CEBRAP.

to mention other and numerous engagements in the various components of the Left of philosophers, social scientists and other academics, still effective today.

Philosophy of Knowledge is naturally present in the philosophical activity, particularly when studying the History of Philosophy – from Aristotle to the Scholastics, to Descartes, Leibniz, Locke, Hume, Kant and to more recent thinkers such as Wittgenstein, Husserl or M. Merleau-Ponty. Many contributions have been produced by brazilian philosophers on such authors and on related philosophical questions. As for Philosophy of Science in the proper sense, which includes Epistemology, one can say that it has aroused in a more decisive way by the development of science and scientific research and its correlated need for a philosophical, historical, social and political reflection on science. A significative number of Brazilian scientists, already at the beginning of the considered period, have marked their interest in these questions, either by contributing by personal reflections, either by simply supporting initiatives to link Science with the critical reflection on it. I would like to mention the prestigious figures of the physicist Mario Schenberg (1914–1990), the chemist Simão Mathias (1908–1991), the architect Ruy Gama who wrote important contributions in the History of Techniques and Technology,[26] the biochemist, essayist and Embassador at UNESCO Paulo Carneiro (1901–1982),[27] among others.

An interesting example of the encounter of a philosopher and a scientist about such matters is the epistolar dialogue between Anísio Teixeira, the philosopher of Education already mentioned, and the biologist and professor of Medicine, Mauricio Rocha e Silva – who deserves also the qualification of "philosopher scientist" – about the "logics on knowledge". Notwithstanding the fact that this general theme looks rather a common one in the Philosophy of Knowledge of the time, the main items discussed were not so trivial indeed, as they were on "scientific and artistic creations and their respective contributions to culture".[28] The approaches of both thinkers were free minded more than erudite and conventional, and dispatched an original flavour due to the respective personal commitments of the authors. Rocha e Silva did put forward the similarity between both forms of creation, sitting on a "logics of invention", which he saw as limited to a given moment of the process while the rest would be dominated by the mechanical stage of scientific methodology. As for Anísio Teixeira, he insisted on the difference of the respective grounds ("empirical" for the scientist, "intuitive" for the artist) and on their opposite respective connections with the course of progress (strong connection for the scientist, no connection for the artist, as one cannot speak in the same sense of progress in art).

Among the most influential professors of Philosophy of São Paulo University in these 1st years, one should mention Lívio Teixeira (1902–1975), historian of Philosophy, who worked together with the french philosopher Martial Guéroult, from the latter's staying in São Paulo and even after his return to France, and published books on Spinoza's conceptions of perception and abstraction and on

[26] Gama (1983, 1987, 1993).

[27] Carneiro (1970).

[28] Teixeira & Rocha e Silva (1968); see also Rocha e Silva (1965).

Descartes‘ moral,[29] and João Cruz Costa (1904–1978), an historian of Philosophy in Brazil.[30] Gilda de Mello e Souza (1919–2005), professor of Aesthetics, had an important paper in the strenghtening of universitary Philosophy, after the initial impulsion given by the "French Mission" in the period of foundation of the USP; she has been, in 1968–1973, the head of the resistance of the Faculty and of the Department against the militar dictatorship and its pressure upon the University, and succeeded with her colleagues to preserve the existence of Philosohy.

So to speak, these few but brilliant representant of the first wave of brazilian philosophers formed at University did set the pace – similarly as, and eventually in correlation with, their colleagues of other disciplines, and firstly of humanities and social sciences. In the following years an increasing number of philosophers would be formed, teach and produce original works. At this stage, it becomes already impossible to be exhaustive and we have to restrict our evocation on those personalities and contributions whose interest for the Philosophy of Knowledge have been more focussed on Philosophy of Science. We must not forget, however, that philosophers make a community, that their formative and intellectually sustaining milieu is made of them taken together, in the variety of their components: that is the reason why we shall not be absolutely restrictive and shall overflow when needed the limitations of our sometimes arbitrary classifications.

Let us evoke now some of the philosophers of this and the following generations, Brazilians but also foreigners, who have been at a title or another influential on philosophers and on Philosophy – mainly of Knowledge and Science – in Brazil. Let us begin by the foreigners. Atypical but retrospectively significant because of his further high renown – despite his short staying: 3 months, in the war years – has been the presence of the American philosopher Willard van Orman Quine (1908–2000). He had been invited to give a series of lectures at the University of São Paulo, "under the auspices of the Committee for Inter-American Artistic and Intellectual Relations", just before being mobilized to U.S. Marine. He gave the lectures in june-september of 1942, having prepared them by notes written directly in Portuguese, and a book was issued from these, with linguistic corrections done under his control, which makes it reliable as to its Logics content; it has been published in Brazil in 1944, under the title *The Meaning of The New Logics* (*O Sentido da Nova Lógica*).[31] It was actually Quine's fourth book in his production, and its content was original (he published simultaneously some parts in English). For sure the influence of his lectures has been amplified by the book, which contributed to introduce Brazilian scholars to the new paths of Logics and of its Philosophy.

[29] Lívio Teixeira has impulsed the cartesian studies in Brasil, whose present importance has deserved a presentation and extensive bibliography (by Eneias Forlin) in the *Bulletin Cartésies* n°36 (in *Archives de Philosophie*, Paris) for the year 2005. On Lívio Teixeira, see Ferraz (Bento Prado) (1975).

[30] Costa (1945, 1956),

[31] Quine (1944). W. O. Quine gave a very short account of these circumstances in his preface, written in january 1995, to a second edition published in 1996. Excerpts of it were published in English in Quine's biography, *The Time of My Life*, Quine (1985).

Philosophy of Science, and even of Logics, was also one of the subjects thought by the French philosopher René Poirier (1900–1995), who stayed in Brazil, in the Federal University of Rio de Janeiro a much longer time, from 1939 to 1945, as a member of the French University Mission. He was possibly the first resident philosopher of science in the proper sense in Brazil: the books he published around that time were on the concepts of space and time and on the probability of inductions (his two Doctoral These, 1931), on the concept of number (1938), on logics and modality (1946). Another French philosopher, Martial Guéroult (1891–1976), was professor at the USP after the end of the War, from 1948 to 1950. Historian of Philosophy, he was also philosopher of the History of Philosophy, preoccupied by conditions of possibility of the latter. He has been a master for many outstanding French philosophers and was too for Brazilians.[32] His conception of the structural reading of the texts assuming their inner logics, which he practiced in the study of Maïmonide, Berkeley, Malebranche, Descartes, Leibniz, Spinoza, Kant, Fichte, have been adopted by many Brazilian philosophers.

Gilles Gaston Granger (b. 1920), formed at Ecole Normale Supérieure and a disciple of Martial Guéroult, of Gaston Bachelard and of Jean Cavaillès, began his university career at São Paulo University, where he stayed from 1947 to 1953. He thought General Philosophy as well as Philosophy of Knowledge and of Science, which correspond to his own main orientation. He prepared in Brazil his doctorate thesis on the Social Mathematics of Condorcet, defended at Sorbonne, in Paris; one of his first book has been, written in Portuguese, a treatise of *Logics and Philosophy of Science* (*Lógica e Filosofia da Ciência)* published in Brazil in 1955.[33] Back to France he finally settled in Aix-en-Provence before being called at the Collège de France, in 1986; he is professor honoris causa of São Paulo University. An important number of Brazilian philosophers have been durably marked by his teaching in São Paulo as well as in Aix, such as J. A. Gianotti, Arley Moreno, . . . Other French philosophers of the same generation came to Brazil for shorter times and episodically, among whom Victor Goldschmit (1914–1981, professor at the University of Rennes: philosopher and historian of Ancient and Modern Philosophy, a Gueroult's disciple, he was interested in both History and Structure); Michel Foucault (1926–1984, professor at Collège de France, promotor of a philosophical archeology of knowledge); Claude Lefort (b. 1924, professor at Paris-VIII University, philosopher of Politics, of the critics of totalitarism and of the "democratic invention"): their influence on the Philosophy in Brazil has been longstanding.[34]

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[32] Such as Maurice Merleau-Ponty, Gilles Deleuze, Michel Foucault, Jules Vuillemin, Gilles G. Granger, Georges Simondon, Pierre Bourdieu (also a sociologist), . . . Guéroult's *Dianoématique*, let unachieved and published posthumely, included a History of the History of Philosphy and a Philosophy of the History of Philosphy.

[33] Granger (1955).

[34] V. Goldschmit and C. Lefort were marxist-oriented with independent and critical minds. The Philosophy of Maurice Merleau-Ponty has been widespread in Brazil, in particular through C. Lefort, and also directly for various brazilian philosophers who have attended his lectures at

Michel Debrun (1921–1997), although of a French origin, can be considered fully as a Brazilian philosopher, who shifted to Philosophy of Science in the last part of his career. A former student of Ecole Normale Supérieure, Paris (hereafter: ENS), he came to Brasil in 1956, and happened to settle in the country up to his death. He worked and tought initially in Social and Political Science at the Foundation Getulio Vargas in Rio de Janeiro and at USP, publishing such books (in Portuguese) as *Ideology and Reality* (*Ideologia e Realidade*, 1959), *The Political Fact* (*O Fato Político*, 1962) among other ones.[35] He finally joined in 1970 the Department of Philosophy of UNICAMP in Campinas (São Paulo State), where he contributed to teaching and research in Political Philosophy and Epistemology of Human Sciences. He worked also in the Centre of Logics and Epistemology (CLE, founded in 1977, see further on) of the same University, developing in collaboration with other institutions a program of research about interdisciplinary questions related to the study of complex systems, such as the concept of order, disorder, crisis, autoreference, auto-organization and information and their interrelations, with the preoccupation of the transfer from an area of knowledge to another one, from Philosophy, Logics, Biology, Neurosciences and Psychology to Social Sciences and the Arts.[36] Gérard Lebrun (1930–1999), philosopher and historian of Philosophy came to USP in continuation to Granger so as to ensure the French philosophical presence in Brasil (more specifically in São Paulo), as for him in the direction of the Kantian philosophy, of moral and aesthetics. He stayed many years in Brazil and had quite a deep influence on many younger Brazilian philosophers that became prominent professors.[37] Later, Francis Wolff (b. 1950) stayed 5 years at USP, from 1980 to 1984, teaching on Ancient Philosophy; back to France, he has been named in 1992 professor at ENS, from where he maintains continuous contacts with his Brazilian colleagues.[38]

Let us evoke now some prominent genuine Brazilian philosophers who belong, so to speak, to the last (relatively to nowadays) "pioneer wave" of university philosophers in Brasil. Benedito Nunes (b. 1929), who had been a Maurice Merleau-Ponty's student in France, and is a – now retired – professor of Philosophy and Aesthetics at the Federal University of Para in Belém (north of Brazil), is considered one of

Collège de France. Husserl's Phenomenology represents also an important current, represented at USP by Carlos Alberto Ribeiro de Moura (Moura (1999)).

[35] He then travelled for some years in various places in Brazil and abroad as an expert of UNESCO and of the Brazilian Ministry of Education.

[36] Debrun, Gonzales & Pessoa (2004).

[37] See in particular: Lebrun (1988).

[38] See among his books published in Brazil: Wolff (1997). E. Wolff has ensured the continuation of the official french presence at the Department of Philosophy of USP, succeeding to Gérard Lebrun. After an interruption of 4 years, the franco-brasilian professorship was provisionally reestablished and I myself have been its last titular for 2 years, in 1989 and 1990. Years later, on my retirement from CNRS in France, I have been elected visiting professor for Philosophy by the Faculty of Philosophy, Letters and Human Sciences of the USP, and I stayed there 2 years, from mid 2004 to mid 2006. In both periods I tought Philosophy of Science.

the most important living brazilian philosophers. In his thought and work he links philosophical and literary analysis (on the work of the Brazilian writers Clarice Lispector and Guimaraes Rosa, on Philosophy and Poetry in Heidegger, on Time and Narration); one finds in his writings profound insights on some problems relevant to Philosophy of Science in its connection with human experience and values. Such is, for instance, the problem of time, which is at the center of his inquiry: time as human experience and as scientific concept, not only the natural (or physico-biological) concept, but that one which is dealt with by human and social sciences, and particularly History. His analysis bear also on the question of progress, of the unity of human kind, and on the possibility of a universal History, in which he retakes critically Kant's and Hegel's considerations, which appear fundamental as to the relations between Science and Ethics. He proposes a philosophy of finiteness recognizing the temporal character of human reason, that makes possible to conciliate the natural or cosmic time with the multiple measures of History as referred to human actions. Such are, among others, items dealt with in his collection of articles entitled *A Sieve of paper* (*Crivo de papel*, meaning to filter concepts).[39]

Oswaldo Porchat de Assis Pereira da Silva (b. 1933), was formed at USP, getting his Doctorate in 1967, on the Aristotelian Theory of Science; a disciple of Lívio Teixera and of Victor Goldschmidt, he spent periods in France (Rennes and Paris), in USA (Berkeley) and in Great Britain (London). Professor at USP from 1961 to 1975 and from 1981 to 1998, and in the State University of Campinas (UNICAMP) from 1975 to 1985, he was one of the founders of the Centre of Logics and Epistemology (hereafter CLE) in UNICAMP. His work bears on Ancient and Modern Philosophy and Epistemology, and he devoted himself to the defense and illustration of Skepticism, in a personal and original way. By his teaching and writings, he has been a master for many Brazilian philosophers formed at USP and at UNICAMP.[40]

José Artur Gianotti (b. 1930), a disciple of Victor Goldschmitt and of Gilles Granger, has been dealing with a wide spectrum of the History of Philosophy from Aristotle to Kant, Marx and Wittgenstein (he has been the first translator of the *Tractatus* in Brazil), and on Political Philosophy with an analytical-logical concern. Dismissed in 1969 from his professorship at USP by the militar Government, he was reintegrated in 1979. In the meantime he was active in the CEBRAP Institute of which he was a founder. As a Professor at USP, he has formed many of the now active philosophers of these areas in Brazil.[41]

Bento Prado de Almeida Ferraz Junior (1937–2007), a former student of Lívio Teixeira and of Gilles Granger, and himself a master for many brazilian philosophers, brilliant and eclectic, initially inclined towards Husserl's Phenomenology, was a specialist of Bergson and a philosopher of Language and of Psychoanalysis.

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[39] Nunes (1999).

[40] See in particular: Porchat (1993); Wrigley & Smith (2003).

[41] See, among his works: Gianotti (1980, 1985, 1995).

He was also one of the main essayists of Philosophy and Literature in Brazil.[42] Professor at the USP, dismissed during the militar regime for his progressist ideas, he was a refugee in France – where he got a professional fame – and was named after his coming back to Brazil at the Federal University of São Carlos (São Paulo State).

Marilena Chaui, a disciple of Lívio Teixeira and of Claude Lefort, prepared her Doctoral Thesis on Maurice Merleau-Ponty's phenomenology and has been working also on Political Philosophy.[43] Professor at USP, where she is still very active and animates an intellectually important group of work and reflection attended by her doctorants and collaborators, she got an international renown for her thorough study of Spinoza's philosophy, whose intellectual attitude and farseeing thought continues to be inspiring today for many, scientists among others – Einstein shared his conception of monism which he considered the most consequent one. In her thorough and voluminous analysis of Spinoza's "Rib of the real", she investigates in particular Spinoza's debates on scientific matters with his contemporaneous, and the numerous and copious footnotes are elements of epistemological History of the seventeenth century sciences.[44]

Raul Landim Filho, professor of Philosophy at the Federal University of Rio de Janeiro (UFRJ), who got in 1974 his Doctorate at the Catholic University of Louvain-la-Neuve (Belgium), under the orientation of Jean Ladrière, is a specialist of Logics, Metaphysics and Philosophy of Language. He has studied thoroughly these matters through the works of ancient and classical authors and such as Thomas of Aquino, the Port Royal Logicians, Descartes, Spinoza, Kant, Wittgenstein. In his most recently published book, *Disputed Questions in Metaphyics and Criticism of Knowledge* (*Questões disputadas de metafísica e de crítica do conhecimento*),[45] he tries what he calls a "conceptual analysis" of a given question as it is presented by an author among those he chose to examine, thematizing the these and philosophical arguments and putting them in relation, making so to speak his authors dialogue and, as an effect, making them live anew by enlightening and renewing the present philosophical debates. The notion of *subject* (of the thought) and that of *judgement* are two of the main themes considered through the book. Also professor at the UFRJ, Guido Antônio de Almeida, formed in Philosophy at the Federal University of Minas Gerais, prepared his Doctorate at Albert-Ludwigs University of Freiburg (Germany), submitted in 1970, and is presently professor at the Federal University of Rio de Janeiro and editor of the Journal *Analytica*. He published on the

[42] His last book, published after his death (prepared by Luis Franklin de Matos, himself a specialist of Enlightenment), was on J.J. Rousseau: Ferraz (2008).

[43] She accompanied her academical commitment with a political engagement (along with the Workers Party led by Lula da Silva, since its beginning), undertaken courageously, with enthusiasm and lucidity.

[44] Chaui (1999, 2001, 2002); Chaui & al (1984).

[45] Landim (2009).

Kantian Philosophy, on the Philosophy of Language, on Husserl's Phenomenology, on Philosophy of Knowledge and Ethics.[46]

Other eminent professors of Philosophy who cultivate and teach Philosophy of Language and Analytic Philosophy must be mentioned. Oswaldo Chateabriand Filho, at the Catholic University (PUC) of Rio de Janeiro, got his Ph D. at the University of California in Berkeley (USA), and has developed original analyse on varied subjects, including ontologic ones. Balthazar Barbosa Filho (1942–2007), at the Federal University of Rio Grande do Sul in Porto Alegre, and João Carlos Brun Torres at the same University – both got their philosophical formation in the Catholic University of Louvain-la-Neuve – are also renown for their analytic ability. André Leclerc, originated from Québec in Canada has settled in Brazil, where he is professor at the Federal University of Paraíba (UFPB) in João Pessoa, and teaches and practizes Analytic Philosophy.

Newton da Costa (b. 1929) is a particular figure in the world of the Brazilian philosophers: an outstanding mathematician and logician, he has become also an important contemporary Brazilian philosopher of science. With his first scientific works, in the field of Mathematics and Logics, he gained soon a wide international recognition, for he opened a new chapter in the Formal Sciences with the discovery and elaboration of Paraconsistent Logics. This happened in the 1960s, when he was a young scholar in the Federal University of Paraná in Curitiba.[47] A huge literature has been developed on it and in continuation to it, which is impossible to quote here, if not to select a recent original vivid and rigorous testimony of the French mathematician, his contemporaneous, Marcel Guillaume, who had been committed at that time by Pierre Samuel to make the junction of da Costa and the french Academy of Science, where the fundamental papers on paraconsistency have been presented and published.[48] As a philosopher of science, da Costa's direction of thought could be called "scientific Philosophy" or "exact Philosophy", in which the use of Logics permits to clarify and explore important questions of foundations and decisions, for example through the axiomatization of theories – in Mathematics and Mathematical Physics.[49] N. da Costa's epistemological and philosophical developments on the notions of "quasi-truth" and of "pragmatic truth", are intended towards a conciliation of, on one side, the exactness of the logical thinking and, on the other side, the ordinary thought or common sense. Among many research publications, two books succeed in presenting a synthesis of the essence of his philosophical thought: *Scientific Knowledge* (*O conhecimento científico*, São

[46]See the books: Almeida (1972, 1979), Almeida & Landim (1981).

[47]Da Costa (1964).

[48]Guillaume (1996).

[49]See, for instance: Da Costa & Doria (1991), Da Costa & Sant'anna (2002). See below my commentaries to the contributions to the present book by N. Da Costa & O. Bueno, of F. and M. Doria, and of D. Krause.

Paulo, 1997) and *Classical and non classical Logics, an Essay on the Foundations of Logics* (*Logiques classiques et non-classiques. Essai sur les Fondements de la Logique*, Paris, 1993); to them, other books, alone or in collaboration with disciples, are to be added.[50]

Newton da Costa has been a professor of Mathematics and Logics at USP, then at UNICAMP, and went back to USP where he was elected professor titular of Philosophy in 1990. After his retirement, he moved to the Federal University of Santa Catarina (UFSC) in Florianopolis, still working and teaching. On the whole, one can speak of a Newton da Costa's School in Logics and in Philosophy of Logics and of Science, which is spread all over Brazil and in a large number of places around the world. As its representants in Brazil and Da Costa's disciples, one counts in particular Itala Maria Loffredo d'Ottaviano and Walter Carnielli at UNICAMP, Andrea Loparic at USP, Antonio Doria at UFRJ (Rio), Lafayette de Moraes and Edelcio de Souza at the PUC of São Paulo, Décio Krause at UFSC (Florianopolis).

On the enrichment of the USP philosophical tradition and milieu with respect to Philosophy of Knowledge,[51] it is desirable to mention also a number of personalities, all of them professors at the Department of Philosophy, such as: Franklin Leopold e Silva, particularly on Descartes and Leibniz; João Paulo Monteiro on David Hume, Luiz Henrique Lopes dos Santos on Aristotle, Leibniz and Wittgenstein.[52] And, more sparticularly on Philosophy of Science and Epistemology: Pablo Mariconda, Caetano Plastino, Osvaldo Pessoa.[53] Hugh Lacey, originated from Australia, Professor at Swarthmore College in Pensilvany (USA) is in some way (somewhat like the author of these line), a Brazilian philosopher by adoption. He spent 3 years, in 1969–1972 at the Department of Philosophy of USP, with which he maintained collaboration overs the years, and is regularly present again for frequent periods since his retirement in his USA University.[54] He works on the problem of Science and Values, criticizing the way Science and Technology are practized in the present stage of the Capitalist Economic System – his particular

[50] Da Costa (1997, 1993); Da Costa (1990, 1992); Da Costa, Béziau & Bueno (1998); Da Costa & French (2003).

[51] The limitation is somewhat arbitrary, and I hardly refrain to quote the names of professors of Philosophy at USP with whom I also shared intellectual connvivence and friendship. For example those working on the eighteenth century enlightenment, as Milton Meira do Nascimento (who has also a fundamental and efficient editorial activity in Philosophy with the USP Discurso Publisher), Luis Franklin de Matos, Maria das Graças de Souza; Olgaria Chaim Matos; Gabriel Cohn, on Max Weber; Ricardo Terra on Philosophy of Politics and the Francfort School; Roberto Bolzani Filho, Marco Zingano, Moacyr Novais... I want to mention also the work of the group of sociologists around Jeremias de Oliveira on the Methodology of Social Sciences and the Philosophy of Knowledge.

[52] See, in particular, his 100 p. Introductory Essay to his new translation in Portuguese of Wittgenstein's, *Tractatus Logico-Philosophicus* entitled "The Essence of the Proposition and the Essence of the World" (in Wittgenstein (1921) 1994, p. 11–112).

[53] More on some of them in the presentation of their contributions, below.

[54] Cf. Pessoa (2001).

concern being about agriculture, biotechnologies and transgenic food -, and looking for alternative conceptions which take into account popular movements.

Arley Moreno, professor at UNICAMP in Campinas (SP), philosopher of Language, is one of the best Wittgenstein inspired philosophers in Brazil, going along original and inventive paths. In the book *Wittgenstein: Across Images. Introduction to a Philosophical Pragmatics* (*Wittgenstein: Através das Imagens. Introdução a uma pragmática filosófica*) and in other recent works, he puts emphasis on the pragmatic aspects of language – distinct from syntax and semantics -, that is the uses of language as related with the various elements of the situation of production of statements, such as the interlocutors themselves and their complex social connections, actions, empirical objects, etc.[55] In his turn, Moreno's disciple João Carlos Salles Pires da Silva, professor at the Federal University of Bahia (UFBa), who develops a philosophical pragmatics in the same line.

Among the last waves of philosophers in other Brazilian Universities (whose works I know less, except those of my ancient students[56]), one should mention in particular, those at UNICAMP (Campinas), at the UFSC (Florianóplis) and at the UFBa (Salvador). UNICAMP, the University of Campinas (situated at some 100 km from São Paulo, in the same State), has been created in the 1970s, under the rectorship of Zeferino Vaz, on modern standard (such as a tight relation between teaching and research),[57] hiring professors of other Universities (particularly the USP). The Centre for Logics and Epistemology (CLE) was founded in this University in 1967 on interdisciplinary grounds by distinguished philosophers and scientist-philosophers such as Oswaldo Porchat and Newton da Costa. It attracted young scholars from abroad such as the Belgium Michel Ghins, now Professor of Philosophy (Space-Time, ...) at the University of Louvain-la-Neuve (Be), who has oriented a number of these in Brazil, the British Steven French (from 1984 to 1989), now Professor of Philosophy of Science (Models, Semantic approach, Philosophy of Quantum Mechanic) at the University of Leeds (UK), the New-Zealander Harvey Brown (from 1978 to 1984), Professor of Philosophy of Physics at the University of Oxford (UK).[58] The CLE has been a very active centre for Logics and Philosophy of Logics (I have already mentioned above Itala d'Ottaviano and Walter Carnielli, professors at UNICAMP) and for the Epistemology of the various sciences, a field

[55] Moreno (2005).

[56] I quote here a number of these in Philosophy of Science, Epistemology and History of Science o Brazilians which I have oriented, or to whose orientation I have contributed, either in Brazil or in France: AuranI (1992). Batista (1999), Benoit (1999), Camelier (2000), Chibeni (1997), Freire (1999), Pietrocola de Oliveira (1992), Ramos (1998), Simon Rodrigues (1995), Videira (1992). My ancient students and my olleaagues have offered me a Symposium (in 2003) and a collective book on my "40 years of colloration with Brazil" (Freire & Pietrocola (2005)).

[57] It was inspired by the experience of the late University of Brasilia (see above), of which Zeferino Vaz had been previously the Rector for 1 year, from april 1964 to mid 1965 ("intervening Rector", named by to militar Government to substitute Anísio Teixeira, dismissed), and who had been impressed by its conceptions and realizations (see Salmeron (1998)).

[58] See: Ghins (1991), French, Krause & Doria (2000), Da Costa & French (2003), French & Krause (2006),

illustrated in particular by the works of Fátima Evora, Professor of Philosophy and History of science, of Silvio Chibeni, also Professor of Philosophy, with works on Epistemology of Quantum Physics and Realism.[59] The CLE runs two important periodical printed Journals: *Manuscrito* and *Cadernos de Filosofia e História da Ciência*, an electronic one, as well as an important book series, "Coleção CLE".

Some of the Doctors formed at UNICAMP in Philosophy, such as Luis Enrique Dutra and Gustavo Caponi, and others from USP and from various places, as Cezar Mortari, Décio Krause (for him and Dutra, see below),[60] joined in the 1980s the then young Federal University of Santa Catarina (UFSC) in Florianópolis, contributing to impulse the Epistemology and Logic Research Group (NEL-UFSC) in that University. The group organizes a high level International Journal, *Principia*, and a Symposium every 2 years.[61]

The Federal University of Bahia, in Salvador, counts a number of philosophers and historians of science, among whom Elyana Barbosa, for her work on the Epistemology of Gaston Bachelard; members of the recent wave will be mentioned below with their contributions to this book. This University has become since recently an active centre for the interdisciplinary approach of Science: a program of Post-Graduation in Science, Philosophy, History and Education is now fully operational as the fruit of a collaboration between the Science Departments (in particular, Physics)[62] and the Philosophy Department, together with a number of other Universities in various States of the Nordeste. I cannot help here to evoke the memory of a Bahianese scholar who might be considered as a pioneer in this interconnection between Science and Philosophy, and who would have strongly supported such enterprise if he had lived enough to hear of it. I speak of the geographer Milton Almeida dos Santos (1926–2001), originated from Bahia State, who spent a part of his career at UFBa University, and is the author of a substantial oeuvre in Human Geography,[63] particularly on Geography and the Third World problems, in relation with Politics as well as with Epistemology. His books (in particular

[59] See: Carnielli & Epstein (2009); Evora (1992); Chibeni (1997).

[60] Caponi and Dutra work currently on the Philosophy of knowledge and the Epistemology of Biology. Cupani has also recently worked on the Philosophy of technics. See: Caponi (2009), Cupani (1991), Cupani & Mortari (2002), Dutra (1999).

[61] Among the treated items were: Principles in Philosophy and in the Sciences (1999), The Philosophy of Bertrand Russell (2001), The Philosophy of Willard van Orman Quine (2003), The Philosophy of Donald Davidson (2005), The Philosophy of Bas van Fraassen (2007), Charles Darwin and his impact on philosophy and science (2009).

[62] With the active implication of the Professors Aurino Ribeiro Filho and Olival Freire Jr (on the latter, who contributes to the book, see below).

[63] After beginnings in journalism, he prepared a Doctorate Thesis in Geography, which he defended in the University of Strasbourg (France), on the centre of the City of Salvador (it has been publisher in Portuguese and in French in 1959, the frend edition being prefaced by Pierre Monbeig). During the militaty dictatorship he went for a long exile which he spent mainly in France. Back to Salvador, he received full recognition in his country when he was called to São Paulo University.

Space and Method, 1985, and *The Nature of Space: Techniques and Time, Reason and Emotion*, 1996[64]) contain epistemological analyse on the characterization, in Geography, of the notion of space and time and their relations, and of wholeness,[65] and more generally, reflections on the critical refoundation of Geography.

I have insisted in this first part of my presentation, on Philosophy and Philosophy of Knowledge and of Science, and not so much on History of Science (and Techniques). This was a personal bias, because History of Science can be considered as well from the point of view of History rather than of Philosophy; but, as a matter of fact, the present book is more oriented on the philosophical and epistemological side than on the purely historical one. My bias corresponds also to my purpose in having tried this historical account, which I will clarify in short. The existing and established research and teaching Groups and Institutes that are dedicated to History of Science more than to Philosophy of Science and Epistemology have been founded rather recently, and they were not embedded in a formative "tradition" such as that one I tried to characterize. But there is no doubt that they already reached levels of activities and realizations that shows them as important and promising. I shall content myself in quoting the names of the main Institutions in Brazil dedicated to History of Science: The Nucleus of History of Science of the Department of History, at USP, São Paulo, founded by the professors Shozo Motoyama and Maria Amelia Mascarenhas Dantes[66]; The Casa Oswaldo Cruz in the Oswaldo Cruz Institute in Manguinhos, a suburb of Rio, dedicated to the History of Biology, Medicine and Health Sciences[67]; The Museum of Astronomy and Related Sciences (MAST) in Rio de Janeiro, founded by Ronaldo Rogerio de Freitas Mourão, and animated by Alfredo Tolmasquim, Ana Maria Ribeiro de Andrade, HeloIsa Bertol Domingues, Marta de Almeida, and others, and which ensures the collecting of archives of scientists[68]; the Simão Mathias Centre, at the PUC of São Paulo, animated by Ana Maria Goldfarb and her colleagues.[69]

To conclude this kind of panorama – which intended only to give an idea of the effective circumstances in which Philosophy of Science has emerged and developed in Brazil, and which is far to be exhaustive[70] – I would like to mention what

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[64] Santos (1985, 1996). See also Santos (2004). On him, see: Brandão (2004), Lévy (2007), this last one with the meaningful title of "*Milton Santos, philosopher of the worldwide, citizen of the local*".

[65] In particular, these concepts call the problem of the junction between the natural and the social science in this discipline.

[66] See for ex.: Ferri & Motoyama (1979–1981); Hamburger, Dantes, Paty & Petitjean (1996);

[67] See for ex.: Benchimol (1999); Dantes (2001).

[68] On the Living Memory see: Domingues (2004). For other publications:

[69] See for ex.: Alfonso-Goldfarb & Maia (1995–1996).

[70] The lack of space forbids me to quote all the means (publications, regular meetings, societies, etc.) at the local, regional and international – latino american – levels that favours contacts, collaborations, diffusion of works. They are multiplying and gaining in efficiency.

might be called a “socio-intellectual” change in the academic milieu. Since some 20–30 years -from the 1980s -, among philosophers of science, and even much more among historians of science, an increasing number of them come from a scientific basic formation rather than a pure philosophical one. For the philosophers of science of the recent generations, anyhow, even when they did not begin their studies inside the Departments of Philosophy, they usually come there to complete their philosophical formation, and it is in these Departments that they prepare their masters and doctorates. Actually, notwithstanding the specializations, Philosophy is a whole, and its contacts with the living science through the youngest students and searchers who have been closer to it, do not impoverish it, but on the contrary enriches it. Anyhow the History that I have tried to sketch above shows how the philosophical culture is necessary to think lucidly about meanings in science. The presentation and discussion of the contributions to the book which follows now will complete the picture, by taking both problematizations together, the scientific one – as it is given in History – and the philosophical which underlies the epistemological and critical one, dealt with in most of the contributed papers.

It is difficult to organize in a well definite way the various essays which I shall present now, because precisely, as noted before, the “objects” that Philosophy of Science deals with, and the styles and methods that are operated in it, are diverse and do not correspond to a linear sequence. On could for example decide ordering them by beginning with the more historical and factual and ending with the more formal ones, as the organizers have proposed, and such is more or less the line I decided to follow, in order that this introduction be in coherence with the sequence of the table of contents. But I found difficult in some cases to maintain this choice strictly, which led me to modify sometimes the indicated sequence in my presentation. But I hope that on the whole it will not be perturb too much the reader. I shall now present and comment the contributions according to the following classification: *(i).* On History of Science and Historical Epistemology (§ 4). *(ii).* The concern for the social dimension of History of Science and Epistemology, with two distinct items: The Social History of Science, and: Combining the conceptual and social dimensions in the History and Epistemology of Science (§ 5). *(iii).* Philosophy of the specific Sciences and methodological questions (§ 6). *(iv).* General Problems of Philosophy of Science (§ 7). *(v).* Foundational, formal and logical approaches (§ 8).

1.4 Conceptual History of Science and Historical Epistemology

A first group of contributions to this book is devoted to the “Conceptual History of Science and Historical Epistemology”, with a number of case studies about the specific Sciences, Mathematics, Physics, Biology – including the Theory of Evolution and Population Genetics – and Health Sciences. The contributions are presented and commented in the chronological order of their topics.

We are rightly invited to begin with a study on one of the main founders of Modern Science, Galileo Galilei. No doubt Pablo Rubén Mariconda was the best suited author in Brazil to treat this case, for he dedicated to Galileo studies an important part of his career as a searcher. If his Doctorate thesis beared of the philosophies of Pierre Duhem and of Karl Popper, he defended his Habilitation with his master work, a critical edition, with translation in portuguese language (the first one I think) and a book-size Commentary of Galileo's *Discourse on the Two Great Systems of the World*.[71] As already indicated above, Pablo Mariconda animates a dynamic and numerous group of research with a regular working seminar on Philosophy, Epistemology and History of Science, and he is the editor of the Journal *Scientiae Studia*, which publishes original research articles as well as commented documents (unpublished or rare historical texts in the various domains of science, translations, etc.).

In his paper in this book, entitled "Galileo and Modern Science", Pablo Rubén Mariconda analyses the philosophical commitments of Galileo in his research, achievements and fight of ideas. He diagnosizes four fundamental components of modernity for science activity, which he states as being the following ones. First, practical action (which include the making and/or the use of instruments such as the hydrostatic balance, the geometrical military compass, and the telescope). Second, the link between theoretical thought and experiment (notably in the study of the free fall of bodies with an experimental array – the inclined plane -, but also in the investigations on resistance of materials and in the studies on machines). Third, the mathematization and mechanization of nature through he search for natural laws: this, together with his contemporaries Johannes Kepler and René Descartes, who were Copernicians too, as himself was. Fourth, "freedom of thought anchored in method". These items are examined with care and precision, through thorough inspection of the whole – or nearly so – of the Galilean corpus, now available in Galileo's complete works edited in Italy, which includes an important correspondence.

About the second characterization – keeping together knowledge and practice -, the author emphasizes that it marks "the beginning of a conception of science, linked with a new conception of scientific rationality; in which there is a strict connection between scientific and technical work". And Pablo Mariconda shows convincingly that "Galileo is founding not only a new science, but is defining a new type of professional activity, civil engineering". Concerning the conditions of mathematization, which belong to the third aspect, the author makes a rather stimulating consideration on the Galilean distinction between primary and secondary qualities (made particularly in the *Assayer*) as being directly related with the elimination of subjectivity and then making mathematization to be possible. Regarding the method, invoked in the fourth characterization, the author observes that although Galileo did not propose precise considerations on what scientific method is – at variance from Francis Bacon and René Descartes – he claimed "the sufficiency of scientific method to decide about natural questions for which we (. . .) can apply natural reason". This

[71] Mariconda (2001). See also the book published in collaboration with Julio Vasconcelos (of Salvador Bahia): Mariconda & Vasconcelos (2006).

strong affirmation had to do with the "external factors" that act in the scientific field, namely the theologico-cosmological dispute with the Church: Galileo's liberation of reason for the consideration of natural matters led to the "transformation of the standards of scientific judgment".

Penha Maria Cardoso Dias and Teresinha J. Stuchi then offer us a study on "Isaac Newton, Robert Hook, and the mystery of orbit". They both work in the Institute of Physics of the Federal University of Rio de Janeiro, where Penha is Professor of Physics and History of Physics since many years – after having got her PhD in the USA, in Pittsburgh University under the supervision of Abner Shimony. In their paper, they sustain – quite convincingly – that the peculiar way in which the concept of force is formulated in Newton's *Principia*, by making reference to Galileo's result on the "free fall" (actually, gravity fall) of bodies, "is reminiscent of a method to treat non rectilinear orbits suggested to Newton by Robert Hooke". With Maria Cardosos Dias and Teresinha J. Stuchi we have a careful, minutious and quite erudite argument to understand how Newton was making his thought about changes of motion, incorporating Galileo's result. We met here, moreover, a good example of continuity in the progress of scientific knowledge, important enough even if this is not the general case – and other Newton's achievement exemplify as well discontinuous steps.

In their way of proceeding, the authors first state the historiographical problem about Newton's formulation of the law of central and centripetal forces, putting forward all the erudite references to the existing, classic and most recent, literature on the subject. They then proceed, so to speak, to their own inquiry, in order to know in which direction would go the difference noticed effectively in the solution proposed by Newton with respect to the solutions that he could have possibly used: was Newton, at the time when he knew Hook's method – in the year 1679 -, already in possession of the expression of the centripetal force? The authors of the paper then proceed by calculating these solutions, then comparing with Newton's hand drawing of the trajectory, which makes them conclude in favour of Hooke's influence. This account of inquiry with a historical stake looks somewhat as in the vein of a detective story, albeit of a rather exigent and minutious reading: "Not so elementary, my dear Watson".

This seems to be a very important question, despite its at first sight marginal character, for it may seem curious why Newton when dealing with the centripetal or central force, does not express it directly from geometrical considerations, but always borrows it from the Galilean expression of uniformly accelerated motion. If I dare a personal comment on this subject, such a modality would still be used half a century after by Leonhard Euler – in 1750 – to formulate the fundamental law of dynamics according to the differential calculus, whereas Jean d'Alembert's derivation of the equivalent formula – some time earlier, in 1743 -, by basing himself only on the variables of motion – space, time, velocity -, letting aside the vague and "metaphysical" concept of external force, in order to formulate the causal differential change of motion, made direct use of the geometrical properties of the circle osculatory to the trajectory – by using an explicit representation of time in a space-time diagram, an innovation by then.[72] Possibly – this still is my comment -, Newton's concept of force was so pregnant on himself and on Euler that they thought more spontaneously the elementary change of motion in terms of the first law of dynamics to have been stated, that is Galileo's one.

It is also Isaac Newton's way of working that is at stake in the paper "Newton and Inverse Problems" proposed by André K. T. Assis, a professor of Physics and historian of science at the Unicamp University (Campinas, SP). The thesis defended is that "Newton always considered the inverse aspects of any problem" and is illustrated – rather than really analysed – by a number of Newton's quotations, striking indeed, in the various domains he contributed. Such is, in Mathematics, the inverse problem of formulating the fluxion from the fluent (operation of derivation) and to obtain the fluent from the fluxion (by integration). In Optics, the spectral decomposition of the ray of white light by making it fall on a prism, is supplemented by the inverse operation of recomposition of the various splitted coloured rays reassembled together by the uses of other prisms adequately disposed. In Mechanics, the third Newton's law of motion, that of action and reaction of forces, speaks immediately to the mind (it refers directly to natural motion and forces), but reasoning is adequate to it, as Newton implicitly suggests by the examples he invokes (this being my personal comment).

The author adds to it, as an effect, the way in which Newton reasoned in Mechanics, from the second law of Kepler of astronomy to the inference of the law of gravitational attraction, and conversely deducing from the latter the laws of motion of the planets. Concerning Philosophy, Newton's considerations on the analysis and the synthesis speak immediately also for the thesis suggested. On the whole, we may very well admit, with the author, that the look for inverse problems is a trait of his (natural) philosophical or scientific method, I would say: a trait of his specific scientific style. In this respect, one would wonder whether the philosophical considerations on analysis and synthesis (which have long standing antecedents in the philosophical tradition) would not be those which inspired fundamentally the other more specific ones of the inverse problem for the particular sciences.

Cibelle Ceslestino Silva, of the Institute of Physics of São Carlos, one of the various campus of São Paulo University, dedicates her work to "Jean Antoine Nollet's contributions to the institutionalization of physics during the eighteenth century". The century of Enlightenment is actually a period of the History of Science that is still little explored in Brazil, although it is of a fundamental importance in order to really understand modern science, as it is the period where modernity – in all

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[72]Euler, L: "Découverte d'un nouveau principe de méchanique", *Mémoires de l'Académie des Sciences de Berlin*, 6 (1750), 1752, p. 185–217. Republ. in L. E., *Opera Omnia*, series 2: *Opera mechanica et astronomica*, vol. 5, ed. by Joachim Otto Fleckenstein, Lausanne, 1957, p. 81–109; D'Alembert, J., *Traité de dynamique*, David, Paris, 1743. 2nd ed., modif. and augm., David, Paris, 1758. See M. Paty, "L'élément différentiel de temps et la causalité physique dans la dynamique de D'Alembert", *in* Morelon, Régis & Hasnawi, Ahmad (éds.), *De Zénon d'Elée à Poincaré. Recueil d'études en hommage à Roshdi Rashed*, Editions Peeters, Louvain (Be), 2004, p. 391–426.

domains – has established, after the pioneering and foundational works of the predecessors in the seventieth century. Life science have began recently to benefit of the attention of historical epistemology, for instance with Mauricio de Carvalho Ramos' work on Pierre Louis Moreau de Maupertuis.[73] On the contrary, historians of Philosophy and philosophers of Politics have usually paid much attention to the philosophers and thinkers of that period. Maybe this lack of interest toward science was due to an opinion that has been common among the scientists up to rather recently, that modern science had been settled in seventeenth century for Mathematics and Physics – as the too common place expression "newtonian paradigm" unfortunately suggests, and in XIX^{h} for the science of life and the Human and Social Science. Jean Antoine Nollet, a member of various Academies of sciences, is not considered now as a major figure of the eighteenth century Physics, although he was among the pioneers of the science of electricity, but it is now a common and justified claim that the scope of History of Science must not be limited to the great discoveries and recognized geniuses, and must consider as well other less prestigious scientists and their works as well as the scientific milieu as a whole. Otherwise science would appear as a kind of miracle, like flowers blossoming on a plant without roots and earth, when on the contrary it can only be accounted for by considering the intellectual and social conditions that made it possible.

The case of Jean Antoine Nollet is typical of a reasonably good and recognized scientist of that time, who cultivated the new ideas and eventually contributed somewhat to their progress. Furthermore Nollet's case exemplifies other, rather new, dimensions of science, namely its social impact and its dissemination in the public, through popularization: this should be considered, indeed, as a new kind of social phenomenon. Cibelle Ceslestino Silva recalls well these aspects. But one of the main interests of her paper is perhaps to point at an aspect that is sometimes shadowed by a "paradigmatic conception" of science for a given period, that is the presence, besides the dominating Newtonian current for Mechanics and Astronomy, of a large variety of explorations of nature and of proposed methodologies in the field of other phenomena. Experimental physics in the eighteenth century was one of these domains. Life Science – Natural History as it was called at that time – would be another one. Let me say "en passant" that this diversity was far from being ignored by a good number of significant historians and philosophers of science and of the ideas dealing with that time, unfortunately not quoted in the paper, such as Ernst Cassirer, René Taton, Georges Gusdorf, to name only a few of the most prominent ones. The author sketches Nollet's biography, recalls his works in experimental physics and his famous public spectacular experiments, and reminds us that his "two currents" hypothesis – that of affluent and effluent electrical currents of matter – was widely accepted all Europe through at least for one decade – around 1740–1750 – before being substituted by Benjamin Franklin's alternative

[73] His Doctorate thesis, defended at USP in 1998 (during the preparation of it he spent 1 year in the REHSEIS Lab at University Paris 7-Diderot) bears on Maupertuis and the Generation of Organized Bodies.

conception of one fluid only. She shows how Nollet 's ideas were enrooted in the Cartesian idea of action by contacts and how, at the same time, he was cautious to preserve his independency with respect to the conflicting systems in presence (the Newtonian and Cartesian ones).

Roberto de Andrade Martins proposes a study on "Henri Becquerel and radioactivity: a critical revision". Initially trained in Physics at the University of São Paulo, Roberto Martins got his doctorate in Logics, Philosophy and History of Science at the State University of Campinas. He is a professor at the Gleb Wataghin Institute of Physics of the State University of Campinas, and a very active senior searcher in the field, animating many activities, Seminars, Congresses and Journals at the national and regional levels, and orientating a large number of these. His researches bear on the historical foundations and elaborations on Physics – theoretical and experimental, in particular Classical Mechanics, Electromagnetism and Radioactivity, Gravitation and Relativity Theory -, Astronomy – including Medieval[74] – and other connected sciences, including Chemistry and aspects of Biology, with the objective to investigate the methodology, conceptual base and dynamics of the scientific work. He devotes also part of his activities to the organization of sources for the History of science in Brazil and Portugal.

In his presented paper, Roberto Martins demystifies severely the importance of the scientific contributions of Henri Becquerel, the first founder of Radioactivity, pointing at his frequent mistakes: his first interpretations of the invisible radiation of Uranium salts as an electromagnetic radiation which should reflect and refract are well known, but not the other ones that followed and which are generally forgotten, and on which the paper puts all the light, analyzing them, showing on the spot – from the published papers – how they were erroneous, and the experiments not so well done. Roberto Martins suggests that these errors – in which Becquerel persisted up to the moment when other scientists had corrected the wrong traits – were due to the scientist's pregnant preconceptions, in particular the analogy with luminescent phenomena, deeply studied decades before by his father Edmond Becquerel, and by himself more recently. More than this, Roberto Martins goes on to show that Becquerel tried afterwards to hide his misinterpretations and experimental mistakes by returning them into his advantage – as if he had been early and the first one to recognize a spontaneous emission with its verified properties. It is however not clear how and why the scientific community accepted finally these allegations – or was indulgent to them...

With Lilian Al-Chueyr Pereira Martins' paper, we change the field and shift to the History and Epistemology of Biology at the beginning of the twentieth century. Lilian Al-Chueyr Pereira Martins has learned Biological Sciences in the domain of Genetics at the State University of Campinas, where she specialized afterwards in History of Science, with a Doctorate Thesis prepared in both Universities

[74] Martins (2006). Roberto Martins has many publications in Journals (I select here one of them on gravitation: (1999), and mention various on Maupertuis' principle of least action), various books on the History of Physics, and he has edited a number of collective books.

of Campinas and of Cambridge (Great Britain). She is presently a professor of History of Science at the Catholic University (PUC) of São Paulo and a searcher in History of Genetics and Evolution with a CNPq grant at UNICAMP. In her paper on "Regeneration as a Difficulty for the Theory of Natural Selection: Morgan's Changing Attitudes, 1897–1932", Lilian Al-Chueyr Pereira Martins calls our attention on an episode of the reception of the Darwinian Theory of Natural Selection. She evokes, among the partisans of the theory, August Weissmann, and among the opponents (at least partially, and in a first stage of his thought), Thomas Morgan to whom she devotes the main part of her study. The author follows Morgan's arguments centered on the observed phenomenon of regeneration in some invertebrates, which Morgan himself studied experimentally and to which, so it seemed to Morgan, the hypothesis of natural selection was not suited.

The author follows Morgan's argumentation and points out some fundamental problems in this debate. One of these problems was the difference between an individual process (which regeneration is) and a statistical effect on a population which adaptation and natural selection is. Another one is the link (or absence of link) between the usefulness of a trait for an organism and the necessity of its existence in this organism. But it happened that in his later works, Morgan adopted the theory of evolution through natural selection without mentioning any more his first reserves. The author of the paper makes a parallel with Morgan's attitude towards the chromosome theory of heredity, to which he first strongly objected before adopting it (around 1910) up to becoming his main supporter. In both cases, the objections had not been solved, but he preferred to keep silent about them. In her sober conclusion, Lilian Pereira Martin invokes a "professional strategy" that makes the scientist choose "a successful line of research", letting aside foundational problems. So does, in a way – one could say – finally her study, sending somehow abruptly the reader to a sociological account of the end of a controversy.

Kenneth Rochel de Camargo Jr, a professor and researcher in health matters (he presents himself in his contribution as a "health professional"), gives a contribution on "Echoes from he past: the persisting shadow of classical determinism in contemporary health sciences". The paper bears on general methodology as well as on epistemological and philosophical considerations on science, with special focalization on the case of Health Science, which are typically a combination of scientific knowledge with immediate application, and of practice which has not only technical but also psychological, social and ethical dimensions. What the authors calls "classical determinism" is actually a schematic, rigid and ideological conception of science which correspond to what is generally called scientificism – flourishing in the nineteenth century, and still rather widespread nowadays under a variety of forms. Putting forward the weaknesses of ingenuous realism, as well as the rigid conception of scientific methodology and of rationality (the "schematic" or "essentialist" views of science), he evokes the pragmatic and sociological deconstructions of various authors such as Richard Rorty, Bruno Latour and Michel Callon, and others. As for him, he sets the problem he would like help solving as "how to refuse" such technicism and reductionnism. And following Ludwig Fleck's track, he emphasizes opportunately two key (epistemological) concepts developed by this author: those of “thought collective” and of “thought style”. He analyses them, and seeks a way to solve the mentioned problem with their help, calling finally for a non-normative epistemology. In his argumentation, he emphasizes that Health science is a kind of science that implies not only the knowledge of objective data or representation such as those the natural sciences are used to: other considerations are essential to it, such as those of human and social sciences and practices “that pay attention to what is ‘subjective’”.

I would like to make here a personal observation, as this discussion exemplifies suitably one of the preliminary considerations made at the beginning of this Introduction. As it is rather common when critiques against sciences are motivated by the social behavior and ideological justification of scientists, experts or deciders who use science as an authority argument, it is not in general really science which is at stake, but its ideological deformations and uncritical uses, eventually through the schematical simplifications which are made of them, such as those underlined in K.R. Camargo’s paper. But the author does not differenciate clearly here science from the caricatures which are made of it – sometimes by the scientists themselves. He would answer me, that science is what we observe of it. But to me, considering the sciences themselves, they should be thought and considered in function of their object, that is, the object they aim at describing and understanding, and this is akin to the critical approach that epistemology proposes. Economy, and the science of Economy, which the author refers too – as a typical case of what he denounces, and I agree with him on this -, has itself its own problems, which cannot be charged against the other sciences (mathematics for example) or against rationality in general. Fortunately, tempering his apparent relativism, the author considers that “there is an intrisic value in knowledge” and that Reason, despite the critiques on it, remains necessary.

The virtue of K.R. Camargo’s paper is to call attention, on the whole, on the urgency, when developing the sciences, to develop the critical approaches of them as well – in particular epistemological ones -, for science is not cut away from other human and social life dimensions – as we insisted from the start in this Introduction. And this – again in my view – is a strong refutation of liberal pragmatism such as that of Richard Rorty who, as the author recalls us, “suggests the end of epistemology as a consequence of the pragmatic turn”. I do not resist here to emphasize – this having to be put also in the benefit of a convergence of views, at least as it seems to me -, the evocation, near the end of K.R. Camargo’s article, of Sociobiology, put forward mainly in the last seventies but periodically reappearing, as characteristic “of reductionist and deterministic conceptions regarding human beings and society” which are adequate to promote a social, economical and political view that would come to be dominating: “The selfish gene articulates admirably well with the utility-maximizing agent for neoclassical economy”. Let us however observe that the selfish gene is actually not science but very unrefined ideology, and that Biology, like other well established sciences, lets not itself be mixed up easily with such coarse grain ideology disguised as science. But we must always be aware that there are complex fields of knowledge and practice where charlatans can still rage, and Political Economy, for example, has recently shown once more that it is not immunized against them, although by itself it should be a totally legitimate science.

João Carlos M. Magalhães and Cedric Gondro consider, as for them, a rather new discipline inside the spectrum of Biology, with the purpose of presenting "A contemporary view of Population Genetics in evolution" in a somewhat detailed and at the same time panoramic survey. The problematic is situated inside the frame of the present "Synthetic Theory of Evolution", also called "Neodarwinism", which combine Darwin's Theory of Evolution by natural selection with Mendelian Genetics extended in a Population Genetics, developed in the years 1920–1940, in view of the distribution and dynamics of genetic correlations and variations inside populations – through the use of mathematical models and on statistics. This being stated, the authors first emphasize the peculiarity of this branch of Biology compared with the other ones (adoption of a hypothetical deductive method to delineate evolution processes aiming at a formulation of biological laws, use of mathematics, search for causal explanations, at variance with the descriptive procedures of comparative Biology), and expose some of its characteristic concepts -gene and allele being supposedly known, and their frequencies, they describe the evolution factors such as mutation, drift, selection by adaptation of the organisms to their environment – which is the most difficult part to estimate, dependent on many parameters -, that act as pressures and are quantifiable. Then, they show how this science has been changing rapidly with the advances in modern Molecular Biology and the associated technologies, which include Bioinformatics. We shall not go further in the details in this Introduction, and content ourselves to indicate that the paper is quite informative about the theoretical developments such as the "neutral theory of molecular evolution", the further "nearly neutral theory", which leads to take into account as a factor the size or density of the population; the new views entailed by the structure of the gene and its variations with regards to mutations; and other theories such as the coalescent theory which adds the time dimension to Genetics of Population, bringing it "closer to other branches of evolutionary Biology" – and possibly makes it more realistic.

We enter another different scientific field, namely Mathematics and incidentally Mathematical Physics, with Tatiana Roque's study entitled "The qualitative analysis of differential equations and the development of dynamical systems theory". The article explores also a domain that has been renewed in the last decades but that is enrooted in the mathematical tradition of the study of systems of differential equations and of the "three body problem" of Celestian Mechanics. The introduction of qualitative methods in the most exact of the sciences, Mathematics, was due to Henri Poincaré's seminal work on systems of differential equations and on the three body problem. It has been developed afterwards by other mathematicians such as George D. Birkhoff in USA and by the Russian school, and has led to the development, in the last 40 years, of the so-called dynamics of "chaotic systems" in theoretical physics. The true initiators of the latter were David Ruelle and Floris Takens, in that their treatment, considering a limited number of parameters – instead of an infinite number, as the idea prevailed before -, prepared the possibility of studying such systems in laboratory through experiments.

Brazilian science held here a notable forward position, as the mathematical theory of dynamical systems has been – and still is – a place of excellence in the country, since Maurício Matos Peixoto's first works on the subjects starting in 1959 with his papers on "structural stability". In her paper, Tatiana Roque, who defended her Doctoral dissertation on this subject,[75] has a mathematical strong formation acquired with her Brazilian masters, then got an initiation to epistemology at the University of Paris 7-Diderot and is presently teaching at Rio de Janeiro Federal University, gives a stimulating conceptual analysis of this rich and rather new field of mathematics – notwithstanding its birth more than one century ago. She begins by sketching the essential, conceptually speaking, of Poincaré's achievements in the field – analysis of neighborhood of singular points, "qualitative", i.e. in these beginnings, topological – characterization of the solutions, limit circles, Poincaré's sections -; she drives us thenafter to the early pioneer contributions of G. D. Birkhoff, who first called "dynamical systems" the object under study and proposed the idea of transformations for such systems, which permits to focus on the qualitative aspect, and to specify the problem of stability, enlightened by the concepts of "recurrence" – for solutions – and other ones. Such new concepts were progressively developed afterwards through the contributions of Simon Lefschetz, Alexandre Andronov, Aleksandr Pontryagin, Maurício Matos Peixoto and others, Tatiana Roque analyzing in particular those of "structural stability" and "genericity".

With Samuel Simon Rodrigues' paper "The Problem of Adequacy of Mathematics to Physics: the Relativity Theory Case", we are faced with epistemological problems of contemporary Theoretical Physics, namely Special and General Relativity. The author, who is a professor in the Philosophy Department of Brasilia University has a first formation in Physics and a second one in Philosophy, both acquired at São Paulo University, and then he got his Doctorate at the University of Paris 7-Diderot with a dissertation on the French philosopher Emile Meyerson's epistemology.[76] He first evokes some of the main contemporary reflections of scientists and philosophers on the problem of the intriguing adequation of Mathematics to Physics. He then examines the use of Mathematics in the elaboration of the Theory of Relativity, giving emphasis to the concepts of invariance and covariance and following Einstein's calculations to get at his celebrated equation for the gravitational field of the Theory of General Relativity. Having shown the kind of mathematics used for the elaboration of the theory and how it came to be called for, Samuel Simon then tries to circumscribe the reasons why the theory thus obtained was so successful in the explanation of the considered natural phenomena, by trying to identify the relevant decisive elements. The first of them, according to his analysis, is the "longstanding genealogy of physical concepts" with mathematical expression leading finally to the generalization of covariance and to the use of invariants. Another one is the rule or principle of correspondence with previous theories taken

[75] Roque (2001). See also T. Roque's paper in Freire & Pietrocola (2006), and the Symposium we organized together: Franceschelli, Paty & Roque (2007).

[76] Simon Rodrigues (1995).

as approximations. One more again is the availability of previously existing mathematical concepts which revealed to be appropriate to the purpose – from manifolds and curved space to tensors with parallel transport. And, last but not least, the preliminary critique of the meaning of space-time coordinates and of the privilege of inertial systems in the Special Relativity Theory. About the previous existence of mathematical concepts such as those of Riemannian Geometry, the author makes us observe rightly that these ideas were already originally oriented towards the thought of physical space, and were therefore prepared to get their status in the new physical theory.

I would like to add that what makes a kind of mathematical theory and concepts more appropriate than another one to its incorporation in a physical theory is the structure of its relationship-making between its magnitudes. Physical concepts are also magnitudes – with physical meaning indeed -, and the structure of their system of relationships, i.e. of the theory they form taken together – can be expected to be homeomorphic to the most adequate purely mathematical structure. And for this, if the inventive man proposes, it is nature that in the end disposes. To conclude with this contribution, I would like to draw special attention to the far-reaching idea just mentioned as emphasized by the author, of a "longstanding genealogy of physical concepts" that enter in the formation of the new ones. It means that physical theories are not so abstract, insofar as they are woven on a previously existing canvas, even if they change radically the pattern and figure – and with them the meaning – of that which is represented. Is not this, by the way, a general feature of any construction emanated from man's symbolic thought, gaining its proper reality – as a substitute for us of the external one -, through the density of its stuff of ideas tightly woven along the time of men' history?

1.5 The Concern for the Social Dimension of History of Science and Epistemology

I have put together in the second group of contributions, such as considered in this Introduction, two papers that are quite different in nature one to the other, but have in common a concern that is not obviously present in the other ones of the book, that of the social dimension of scientific knowledge, which I would formulate here as "The specific concern for the social dimension of History of science and Epistemology". The first paper is a contribution on Social History of Science and History of Institutions, which will opportunately remind the reader of an important dimension of Science, as I will argue in my comment on it. The second paper is a rare and meaningful example of a new kind of historico-epistemological analysis which takes directly into account some important social contextual aspects of the scientific activity: it would pertain as well to the precedent group, for it concerns historic-epistemological conceptual studies, but its characteristic socio-institutional dimension makes it belong as well to the now considered theme.

There is another contribution to the book that concerns also the social dimension, namely the one by A. A. Videira and O. Mendonça on the "contexts of discovery and justification" first developed in the field of Philosophy of Science and which gave rise rather recently to the so-called "Science Studies" which in a general way emphasize the social aspects of the constitution of knowledge to the detriment of the epistemological ones. Its main purpose being closer to that of other contributions on general problems of Philosophy of Science, it has been included in that section, and my introductive commentary on it as well. However it is worth noting already in this place that its conclusion is devoted – although briefly – to the question of "How to do Science Studies in Brazil". By asking it, A. A. Videira and O. Mendonça consider the claim of the most salient protagonists of Science Studies that, in putting emphasis on the social aspects of the production of science, their conception would be more adapted to conciliate science with freedom and democracy – henceforth with solutions for development. To this pretention the authors of the mentioned paper give a negative answer, in the sense that "the methodological, epistemological and historiographical methods adopted by Science Studies" have no reason to be especially useful "for the development of science in less developed countries like our own".

Anyhow the two contributions which we shall present and comment now are independent from the so-called "Science Studies" and pertain, like the other ones of the book, to the critical studies on Science that we have shown earlier to be fully useful in the process of scientific development.

The paper entitled "Sciences in Brazil: an overview from 1870 to 1920", by Maria Amélia Dantes, Silvia Figueirôa & Maria Margaret Lopes, is the only one representant in the book of the now well developed academic area in Brazil that Social History of Science has become since more or less 25 years. Indeed, at variance with the Conceptual History of Science, wich is tightly linked with Epistemology, the Social History of Science is more related to History than to Philosophy, and the disciplinary choice made by the editors has prevailed. But History, Philosophy and Epistemology, and the Sciences themselves, are anyhow dynamically intertwined, notwithstanding our practical separations, for the sciences are developed by men in society and through history, and the philosophical reflection on them should be fully aware of it.

Such is the reason why the topics treated in the paper – "Sciences in Brazil" – is welcome in a collective work devoted predominantly to Epistemology and Philosophy of Science in Brazil, for it corresponds so to speak to the background of the picture that is presented. Without it all the rest migh appear as standing in the air, when on the contrary the philosophical, epistemological and historical analysis of science is made possible and meaningful in any given place in the world because it goes along with the permanent development of scientific knowledge activity. What is facinating in the case of Brazil, and of a number of other Latin American countries, and of other developing countries in the world as well,[77] is that the development and growth of scientific activity is acompanied with the consciousness of its necessity for intellectual, social and practical reasons, and with the growing

[77] See, for instance, Paty (1999).

awareness that such knowledge needs also to be understood as for its nature and its conditions, through critical analysis and study – philosophical, social, historical... All this, indeed not being automatically given, but havinbg needed and needing always to fight for it (in any place in the world, be it of the so-called developed or developing countries).

The paper situates the apparition and growth of science in Brazil in the crucial moment of the transition from nineteenth to twentieth century, which corresponds to the beginning of economical growth and modernization of the country. It shows the role of expeditions, surveys and of commissions of study of the country – so vast and still poorly known -, that were systematically organized, the installation of scientific institutions, the development of the professional formation through high schools of engineers, ... In the considered period museums of natural history multiplied, favouring the beginning of scientific research with international exchanges and scientific vulgarization through publications. Health science and medicine developed also with health control related with immigration. Only in the thirties would universities be created as it has been alluded to previously, and a new phase of scientific and educational growth would be opened.[78]

Maria Amelia Mascarenhas Dantes is retired Professor at the Department of History in the University of São Paulo, where she has been, with Shozo Motoyama, one of the first to teach History of Science and to incentivate a number of younger searchers, among whom stand her two co-writers of the presented paper. Silvia Figueirôa and Maria Margaret Lopes are Professors at the Instituto de Geociências and teach also History of Science at the State University of Campinas (UNICAMP). Sylvia Figueiroa has been President of the Latin-American Society of History of Science and has been recently elected President of the International Commission on the History of Geological Sciences (INHIGEO), of the International Union of Geological Sciences (IUGS), created by UNESCO; and Maria Margaret Lopes' researches are oriented towards the history of Museums, of Geology, Mineralogy and Paleontology, and the history of gender in Science.[79]

Now comes the other theme, that of combining the conceptual and social dimensions in the History and Epistemology of Science. It is illustrated by Olival Freire Jr's work on the Epistemology of Quantum Physics. It is an epistemological concern – and an historical one as well – for Contemporary Physics that motivates Olival Freire Jr in his study "Continuity and change: charting David Bohm's evolving ideas on quantum mechanics". Olival Freire Jr, originated from Bahia state, is presently professor of Epistemology at the Physics Department of the Federal University of Bahia in the city of Salvador, and animates the Interdepartment Post-Graduation Programme in Epistemology, History and Teaching of the Sciences, centered in that city but acting in collaboration with other Universities of the Nort-East of Brazil, in Bahia and other States. He has been trained at São Paulo University in Physics and Education before coming to Epistemology and History of Science, thanks firstly to

[78] See also Dantes (2001).
[79] Lopes (1997), Figuerôa (1997).

Amelia Hambuger who oriented his first steps in this direction, which he ponctuated with a Master on Epistemology of Quantum Physics, for whose thesis he already considered aspects of the interpretation of Quantum Physics, in particular that one of Vladimir Fock. He thenafter prepared a Thesis under my orientation[80] on David Bohm's contributions and critiques to Quantum Mechanics, from the proposed alternative "causal theory" of 1952 – known by then as "hidden variable theory" -, up to Bohm's further attempts to elucidate the "implicit order" that according to him Quantum Physics recealed. Olival Freire's thesis has been published in the CLE series – the excellent collection in fundamental questions already mentioned – under the title *David Bohm e a controversia dos Quanta* (*David Bohm and the Quantum Controversy*). Brazilian Physics is not absent from these studies, David Bohm having spent several years at São Paulo University in the sixties, where he had been contracted by the Institute of Physics under Albert Einstein's advice, after having been prosecuted in United States by the too famous Commission for Anti-America Activities led by the Senator Joseph MacCarthy and dismissed from his job. Since the time of his thesis, Olival Freire has worked on the thought and contributions of several physicists having emitted unorthodox or heretic interpretations of Quantum Physics, such as Hugh Everett III, John A. Wheeler, Eugen Wigner, and performed archive inquiries in various countries, adding eventually to the already existing material new interviews done by himself of several important protagonists of the Quantum debate.

In the paper presented in this book, Olival Freire takes anew the case of David Bohm's conceptions, with a twofold purpose. The first one is to show that, through the evolution of Bohm's thought about Quantum Mechanics, from the causal interpretation to his further attempts towards the "implicit order", that is towards the order of quantum phenomena (the explicit order being that of classical, macroscopic ones), one is able to follow a continuity, which can be summarized by the conviction that the idea of Quantum Reality has a meaning. O. Freire evokes Bohm's last endeavours with Basil Hiley – prolongated after Bohm's death by the latter – to study further the implicate order through a mathematical elaboration on algebraic structures from which space-time would emerge. The second purpose of the study is to show how Bohm's inquiry into fundamental Quantum Physics has been influential on many quantum physicists of the further generations, up to the point that the concept of "Bohmian mechanics" is now of a common use to describe a simplified schema of a fully quantum situation, which permits to understand better some characteristics of quantum phenomena. This new understanding of "Bohmian Mechanics" corresponds to a shift of emphasis as to its deep physical meaning, letting aside the "hidden variables" problematic which has been relativized by John Bell's theorem on locality – in Bohm's theory the hidden variables were non-local,

[80] I was an invited professor at the Philosophy Department at São Paulo University when he came to me on Amelia's advice and undertook a Doctorate Thesis bearing on David Bohm's thought and work in Quantum Physics, which he submitted in 1995. When I went back to France, Shozo Motoyama, of the Department of History of the USP, shared with me his orientation.

and therefore compatible with ordinary Quantum Mechanics -, to focalize on the idea of "quantum potential", with various interpretations of it.

In both aspects as evidenced by this work, the link between scientific ideas, general worldviews and "social" working context is taken into account, such as the influence of Bohm's initial Marxism on his deterministic conception in the first period of his thought, and also the change that the "officialization" of the foundational debate in the physics milieu has occasioned, favouring the unexpected revival of Bohm's ideas in various domains. By this twofold interest, O. Freire's paper situates itself within a current of research which seems to me to be rather original, of a combined epistemological and social concern in the historical studies, such as those of his own mentioned above as well as those performed recently together with his students and collaborators in Salvador of Bahia, about the disciplinary acceptation and institutionalization by the physicists of the debate about the foundations of Quantum Physics. This debate, as these studies show, has since some decades acquired an academic status, breaking with the longstanding domination of the orthodox dogmas on interpretation. This combination of the concern for the fundamental questions of epistemology – in this case, of Quantum Mechanics – and for the socio-historical study of the work and pratice of scientists – here: physicists – in the considered domain seems to be something new in the field and which deserves attention.

1.6 Philosophy of the Specific Sciences and Methodological Questions

The next set of contributions is composed of papers that are oriented towards the "Philosophy of the specific Sciences and methodological questions".

Jairo José da Silva's contributes with an essay entitled "On the Nature of Mathematical Knowledge". Jairo da Silva is professor at the Paulist State University 'Julio de Mesquita', having been formed initially in Physics in that University and in Mathematics in São Paulo University, then in Philosophy of Mathematics and Logics in Unicamp and in Mathematatcal Logics in UNESP, hand having studied also at the University of California in Berkeley. His main fields of teaching and research are Philosophy of Mathematics and Logics and Phenomenology (he works also on Edmund Husserl's thought). The contribution he presents here is a clear and deep-reaching essay, despite – or in accord with – the generality of its title. It is rich, concise and goes directly to the essential – in my opinion. The problematic turns around the formal and the symbolic when considering Mathematics.

The author begins by situating himself among the various kinds of answers that have been given along the history of Mathematics to the problem of "the existence of mathematical objects": the realist-platonist – mathematical objects have autonomous existence -, the nominalist – they are linguistic structures -, the formalist – there are no mathematical objects. He argues in favour of "formalist philosophy of mathematics" which is "ontological uncommited" but "epistemologically relevant", by which he means that it facilitates or prepares the possibility of applications. Jairo da Silva actually understands "formal" for mathematics in the sense that "a mathematical truth is formal when it is preserved under isomorphisms". He adopts a secure position by not imposing an already prepared philosophy to Mathematics, but on the contrary considering the goal of Philosophy of Mathematics as "to understand the nature of Mathematics as practized, not to reform its practices", and breaks lances against the Analytic Philosophy of Mathematics which lays on naturalism and empiricism. To him, Mathematics does not deal with objects but with "the structure that underlies domains of objects". This entails almost immediately the question of "applicability" as the latter "has more to do with its [Mathematics'] formal character than with pre-established harmony". And Jairo da Silva rejoins Poincaré and Hilbert in considering that "Mathematics is a free invention only constrained by the consistency requirement", and he proposes also that "Mathematics is a product of rationnally constrained creativity". Incidentally, I want to mention his reflection on "mathematical intuition" which he sees as "formal imagination", or "a sort of formal insight closely connected to the activity of solving scientific and mathematical problems".

I would like to close this presentation of Jairo da Silva's contribution by two short comments. The first is about the kind of formalism he advocates. I see these structured forms, free of content, as they are produced by mathematical activity, as corresponding effectively to a content, but of a special kind: a "formal content", as Gilles Granger named it. And would not their character of products of pure reasoning, inside given constraints, incline us to see in Mathematics a pure structured form of Rationality, if not identically, at least homeomorphical to it? My second comment is about the "applicability of Mathematics", claimed to be privilegiated in Physics. Jairo da Silva considers that it is due to the fact that "Physics is to a large extent a formal science". But this is not obvious from the start, as Physics is about the natural world, and its contents are not formal in essence but intended towards Nature. I would say, for my part, that the adequation between Mathematics and Physics comes from the fact that the concepts of Physics are expressed by magnitudes in the mathematical sense. If it happens to be so, it might be, as Jairo da Silva observes – and I would rejoin him on this point -, because Nature and Mathematics "share common formal properties". I would add that Mathematics being a "formal thinking", one understands better that it enters physical thinking itself, by contributing to structure it – more than being "applied" to Physics as it is rather commonly said.

In the article "Natural Kinds as Scientific Models", Luiz Henrique Dutra examines the problem of species and individuals in "Natural History" or Biology. Luiz Henrique Dutra, who got his Doctorate at Unicamp (Campinas) under Michel Ghins' guidance, is professor at the Philosophical Department of the Federal University of Santa Catarine (UFSC) in Florianópolis, in the south-eastern coast of Brazil. With Cesar Mortari, Gustavo Caponi, Alberto Cupani and others, he has founded, and continues editing, the excellent Journal Principia published by their University and which got an undisputable international fame. He is the author of various essays including several books among which an Introduction to the Philosophy of Science.[81] His own style in doing Epistemology and Philosophy of Science is rather original considering the international scene – I would say that he shares this originality with his colleague Gustavo Caponi who unfortunately could not contribute in time to this volume – in that he succeeds in conciling historical epistemology, to which pertain his works on Claude Bernard, and some tendencies of analytic philosophy. His paper makes uses of these two concerns to present an epistemological-analytical study, historically informed, of some concepts of Biology, and some more general of Science, such as kinds – related with Taxinomy -, individuals, events, phenomena, which for some of them have an ontological flavour – and indeed the compound word "natural kinds" refers directly to Nature, i.e. Reality. Are there truly speaking natural kinds, or are they only construction of man's mind? By asking the question – which seems at first sight akin to the one that could be asked for scientific concepts in general -, one must be aware of the peculiarity of the concepts of Biology, and one of the interests of Luiz H. Dutra's paper is that his conceptual-theoretical analysis takes such specification strongly in account.

Luiz Henrique Dutra begins by situating himself with respect to other authors' positions about the problem set by the concept of "natural kinds", mainly the realist inclined views of Saul Kripke and Hilary Putnam, the more rigid one of Richard Boyd, and those of Willard Quine and Thomas Kuhn directed towards an alternative view to both relativism and realism. Luiz H. Dutra goes also in this latter direction but with the purpose of clarifying it somewhat more. In short, Luiz H. Dutra's thesis is that natural kinds are "scientific models", but conceiving model differently from mere interpretation of a scientific theory, which sets his position at variance from the semantic conception – which gives to models the latter definition. He sees scientific models as abstract replicas of real circumstancies, and shows that as theory develops, kinds get more theoretical. To explore his proposed direction, Luiz H. Dutra discusses the relation between kind and individual and considers a "criterion of ontological density", partly inspired by Quine's "ontological commitment", but which takes into account not only kinds and individuals – as Quine does – but "events" as well. For this he makes use of a Claude Bernard 's distinction between phenomenon and property, inherent to Bernard's methods in Physiology, which provides him a tool for solving ontological problems by extending it to other categories such as phenomena, events, facts, on one side, individuals, things, properties on the other. And then goes the argumentation, which gets to its conclusion that "viewed as a scientific problem, natural kinds are both our constructions and things existing in nature", and that "both realism and anti-realism in what concerns the scientific activity" are avoided. One feels then confronted to a number of questions, which I leave here to the reader for further discussion.

The two next papers are focused on nearly the same subject, "functionalism" in Biology and its relationship with the "etiological" conception, with different

[81] Dutra (1998).

emphasis for each one, and such twofold reading has the advantage of facilitating the unprepared reader, a priori ignorant of these matters, as the writer of these lines is – or was... -, to enter more eagerly into their biological theoretical and epistemological stake.

Karla Chediak's study is entitled "The etiological approach to the concept of biological function". Karla Chediak got her formation in Biology and in Philosophy at the Pontifícia Universidade Católica of Rio de Janeiro and is presently assistent professor at the State University of Rio de Janeiro, with as her themes of interest, evolution, cognition and naturalism. In her article, she explores critically the various characteristics of the "etiological approach" with respect to the concept of biological function. In essence, etiology, as defined by his promotor Larry Wright (1973), is preoccupied by the reasons of the presence of a biological characteristics – trait, organ, behavior,... – endowed with a function. It considers that the biological function must explain the presence of the trait; as it refers the presence and the function to natural selection, "this entails a close link between the concept of biological function and the concept of adaptation". Any function related to a trait would result from the selection pressure and thus be "teleological", but in a non-intentional sense, through a "blind process". At variance with this view, Richard Cummins proposed (in the same years) a "functional analysis" or "analytic approach" of the function that is "independent of evolutionary considerations". Karla Chediak asks whether such an approach is able to account fully for what biological function actually is. To her, the origin of the function, due to natural selection, is an important aspect of what a function is. This exigency of the etiological approach has generated debates, some of which are exposed in the rest of the paper – for example, that about the presence of vestigial traits having lost any function. These debates are dominated by the concurrency between the etiological and the analytic approach of the concept of function. For her part, although she claims the usefulness of both, the author concludes to the weakness of the purely analytical approach.

In Nei Freitas Nunes-Neto's and Charbel Niño El-Hani's essay on "Functional explanations in Biology, Ecology, and Earth System Science: Contributions from Philosophy of Biology", the debate Function/Selection is also considered, balancing the advantages and limitations of each, their relative weights being estimated somewhat differently than in the previous paper, and attention being called to the implications of these respective approaches to the scientific practice in various disciplines such as Biology, Ecology, and Earth System Science. Nei Freitas Nunes-Neto and Charbel Niño El-Hani both pertain the Research Group on History, Philosophy, and Biology Teaching of the Institute of Biology at the Federal University of Bahia, in the city of Salvador. Charbel Niño El-Hani and Nei Freitas Nunes-Neto are both professors at this Institute.

The argumentation takes its start with the same emblematic authors as in Karla Chediak's contribution, Larry Wright for the selectionnist-etiological approach, Robert Cummins for the functional analysis, with a clear preference for the second one as being more secure and efficient, particularly in the applications chosen, insofar as the authors get success in performing functional analysis in the fields of Ecology and Earth System Science, by using as an example a particular biogeochemical system. On the contrary, they show how in a selectionist etiological approach the concept of function is unable to give account of the origin of a biological trait (as it would be expected by the neo-teleological interpretation), which limits severely the range of its usefulness. "As Cummins sums up, traits do not arise because of their functions, but because of their developmental histories", state the authors. If I understand well, function is noted afterwards, after it has appeared and being already operative: it results from selection but without having been programmed. Nevertheless, the etiological approach appears successful in explaining the spread in a population of a newly formed trait, and therefore it cannot be dismissed on the whole; it shows to be pertinent in a limited but actual domain. That is why in their conclusion the authors claim that "both etiological and systemic approaches [are needed] to account for functional explanations in biology and related sciences".

Then comes the paper by Paulo Abrantes, "Human Evolution: Compatibilist Approaches". Paulo Abrantes, originated from Rio e Janeiro, prepared and defended his Doctorate Thesis of Historical Epistemology under the orientation of Marie-Antoinette Tonnelat and Ernest Coumet at the University of Paris-Sorbonne with a Dissertation on Maxwell's work in Electromagnetism. Then he explored Philosophy of Cognitition: among other publications, he is the author of a book entitled *Images of Nature, Images of Science* (*Imagens da natureza, imagens de ciência*).[82] He is presently professor of Philosophy at the University of Brasilia.

In his paper, Paulo Abrantes considers various proposed conceptions from the Philosophy of Cognition dealing with the problem of the evolution of human minds. We are immediately merged into the problematics of Cognitivist Philosophy: human beings are taken "as agents, that is as systems whose behaviour is caused by mental states" and insofar as communication between humans is concened, as "interpreters" by the attribution to other people of mental states: so do we "make sense of ourselves" and of the other ones. Concerning the problem of the evolution of human mind, one is faced with two kinds of facts, one biological – the "wiring" and its connection with the world – the other one cultural and social – the "facts about our habits of interpretation". The conceptions analyzed are all focused on the problem of formulating a theory of the evolutive human mind that would integrate these two "sets of facts" together. For one of the main protagonists of these studies, P. Godfrey-Smith, author of the book *Complexity and the function of mind in nature* (1998) and of a number of papers on the evolution of mental representation and cognition, this coordination task is a philosophical one, for the role of Philosophy is, in his view, "to investigate the relations between different sciences", and "to coordinate common sense and scientific views of the world and ourselves". The latter item is acknowledged by P. Abrantes as defining "compatibilism", which he refers to the Philosophy of Psychology.

Generally speaking, the stake of the problem is to situate the function of cognition – defined as that one to deal with complex environment – in the evolutive

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[82] Abrantes (1994).

adaptation, an unusual concern for Cognitive Philosophy, as the author notices. Then a number of ideas invoked in the recent litteraure on the subject are discussed, such as a "social intelligence hypothesis" acting as a selection pressure toward the hominides; the focus to be put or not on the "folk psychology" by contrast with the scientific one; the non-human animal rationality as a "practical" one – despite the inconvenient of the expression, already afforded with a quite different meaning if we think of one of Kabnt's masterworks, and maybe should one speak instead of an instrumental or instinctive rationality (my comment) -, if animals may be considered in given situations as intentional agents; the "dual inheritance theory" – for social and cultural agents – of human evolution (by P. Richerson and R. Boyd)...

All these discussions are well suggestive, based on the most recent litterature, and the ignorant reader merging into them – as I tried to do – is learning much, discovering a new field – but not unconnected with previous, more common ones. My beotian wonder is to why must it be to Philosophy to consider such problems, and not to a Science, be that one eventually a new discipline at the junction of the two domains of concern – as examples exist in the History of Science. True, in some domains – of Human and Social Sciences – Science has followed from specific philosophical investigations, while in other domains (for instance Quantum Physics), philosophical interpretations external to the scientific theories to which they were added in a first period appear finally superfluous when a renewed inner understanding of the concepts and theories has been obtained. Would it be here the case or not, of a kind or the other, a theme such as that one studied in Paulo Arantes' paper rises anew in its own way the question of the living interrelations between Science and Philosophy.

1.7 General Problems of Philosophy of Science

We enter now in another group of the contributions to this book, dealing with "general problems of Philosophy of Science".

Renan Springer de Freitas, professor at the Federal University of Minas Gerais – in the city of Belo Horizonte -, proposes a paper entitled "On Darwin, Knowledge and Mirroring", where he analyzes and criticizes Richard Rorty's philosophical positions about knowledge as they are presented in the book *Philosophy and the mirror of nature*. The "naturalist project" of Rorty, known as "epistemological behaviourism", considers that there is nothing more in knowledge than beliefs socio-historically constituted: the justification of our beliefs is not "a transaction between the 'knowing subject' and the 'reality'" – in Rorty's own terms, but a social phenomenon. Getting inspiration from the Wittgensteinian pragmatism and the philosophies of John Dewey and Martin Heidegger, Rorty goes to a cruzade against the rationalism of Descartes, Locke and Kant and the epistemological project issued from it.

Renan de Freitas proposes to conciliate the idea that "the mind does not mirror nature" and the inquiry on justification of beliefs, both put forward by Rorty – who monopolizes them according to his peculiar meaning (my remark) -, and the epistemological project that Rorty condemns. Rorty denies that our knowledge – or our beliefs – derive from our cognitive properties, and expresses a strong intersubjective or social relativism of it. To Renan Freitas, Rorty's naturalist and pragmatist conceptions misundersand the philosophical implications of darwinian evolution, in the prolongation of which the statement that science is a self-correcting undertaking takes place. Darwinian evolution is based on the ideas of mutation and of the "selective retention of some mutations": the first is accidental, not the second. But Rorty retains only the accidental, and ignores the selective process; as a consequence, he denounces the "accumulation of errors" in the history of Philosophy that has resulted in the illusion of a link between the knowing subject and the world. Thus Rorty's conception, the author states, is the result of a mutilated Darwinism, and the author pursues in arguing against related aspects of Rorty's nominalism.

The next paper, by Richard Theisen Simanke, professor at the Federal University of São Carlos (São Paulo State), considers another domain of knowledge, namely Psychoanalysis. Entitled "Freudian Psychoanalysis as a model for overcoming the duality between natural and human Sciences", it makes us at the same time get into the problem of the difference and relationship between Natural and Human Sciences, a problem that has been very central and controversial in the philosophical debates about knowledge in the twentieth century. Richard Theisen Simanke performs an epistemological and philosophical analysis of Sigmund Freud's conception of Psychoanalysis as a science, as Dr Sigmund wanted to establish it, and provides at the same time a reflection on the respective status of Natural and Human Sciences. The author begins by recalling the "epistemological dualism" with respect to these two kinds of knowledge proposed mainly by the late nineteenth century neokantian thinkers, and continued in XX$^{\text{th}}$ in various currents such as for instance the "linguistic structuralism and French anthropology of the years 1940–1950", in view of preserving the specificity of human and social sciences against the then prevailing positivist and naturalist tendency to submit them to the same methodology and criteria of scientificity as the exact and natural sciences. This methodological dualism was, according to Richard Simanke, related to an ontological one, being actually referred to an "ontological fracture" between nature and human being.

Richard Simanke then analyses the status of Psychology (marked by a variety of currents belonging to either tendencies, the natural and the cultural ones) and in particular of Psychoanalysis to which the same remark can be done (Lacanians on the anti-naturalist side, neuropsychoanalysis on the naturalist side). But what the most interesting concerns the proper way of Freud to tackle and conceived Psychoanalysis in this respect. The author shows how Freud, who started in his researches on the Science he founded with a naturalist conception, which he always maintained afterwards (ultimately, in his views, its basis should be found in Neurology and Biology), was led actually in his attempt to formulate a theory of Psychoanalysis, to consider problems (of meaning, of language, of interpretation) that pertained traditionnally to the sciences of culture and not to those of nature. Richard Simanke shows how from this apparent contradicion Freud formulated a richer conception of the relationship of the natural and the "cultural", through his maintained reference to consciousness, his "attention to the phenomenological dimension of the mind", his understanding of the need for the child to receive extraneous help for his survival, his fraility and need to communicate with otherrs being "the primal source of all moral motives", in the proper Freudian terms. This corresponds to what the author calls an "integral naturalism", a "renewed concept of scientific naturalism", that includes human dimensions which are absent in the usual reductionnist naturalism. My only reserve concerns the parallel, done *en passant* by the author, between the "rise of sociobiology in its relationship with the social sciences, and the expansion of the *neurosciences*, in relation to sciences of the mind", for reasons mentioned above in my comment on R. Camargo's paper.

Osvaldo Pessoa Jr's paper "The Causal Strength of Scientific Advances" corresponds to a part of a larger project, that of giving account to some extent, in a nearly quantitative way, of "scientific advances" in causal and probabilistic terms, so as to be put in computer language, which would allow performing comparisons between various possible or effective scenarios. The project is motivated, so he tells us, by the difficulty to make a fitting of the results obtained by the work of historians of science, taken as "empirical data" and the theories of knowledge proposed by philosophers. Osvaldo Pessoa, formed in Physics and in Philosophy at USP and UNICAMP, is presently professor of Philosophy of Science at São Paulo University.

The first methodological problem met by the author for his long range purpose, and which is the subject of the presented paper, is "how should the historical information be represented in computer language?". O. Pessoa defines as a basic concept that one of "unit of scientific knowledge", which he calls "advance", and that permits to analyse further the path of science which integrates all of such advances, in a given field. "Advance" in the author's view concerns contents as they are obtained, not the way in which they are obtained, neither the facts that underly them. With this precision, Osvaldo Pessoa rightly chooses, so to speak, to stay on the side of the representation, not on that of the object, a choice that is fully coherent with his purpose. He goes on assuming that advances are causally connected, in a sense which he comments – that of "counterfactual causality", defined in eighteenth century by David Hume as *"if the first object had not been, the second never had existed*" -, this connexion permitting to follow the sequential chain of knowledge improvement. Having in this manner prepared his methodological tools, the author then takes as an example a particular but meaningful historical case, that of the beginnings of spectroscopy in nineteenth century – with which he got familiarized in previous historical studies. He then goes on by specifying his analysis with the concept of "causal strength" of which he proposes a graphic representation and concludes his paper with further methodological considerations.

I cannot tell whether Osvaldo Pessoa Jr will succeed to achieve his project, wich appears as a long-term programme, but one could hardly miss to recognize in his enterprise originality, perseverance and also courage – particularly if one think of the many possible misunderstandings he will probably meet. Osvaldo Pessoa Jr is actually interested since some time by the question of possibility in History of Science, that leads to comparing various scenarios that could have happened, the one that happened effectively not being always that one which could be considered the most probable. If it is not, this might be, I would add, because of the contingencies inherent to historical contexts. One could of course question the schematic kind of historicity that is taken into account in such studies, but the author is well aware of the approximation, which can be summarized in his own terms as seeing the "scientist as a very complex cognitive machine that receives a large number of advances (. . .) as causal inputs and generates new avances (. . .)". His aim, in trying to objectivize and to some extent to quantify scientific progress for a definite and limited field, is rather modest with regard to the effective ways of scientific progress, which would include individual creativity and social dimension, and the author is fully lucid on it.

Antonio A.P. Videira and André L. de O. Mendonça close this series on general problems of Philosophy of Science with a paper on "Contextualizing the Contexts od Discovery and Justification: How to do Science Studies in Brazil". Antonio A. P. Videira studied Philosophy at Federal University of Rio de Janeiro and got in 1992 his Doctorate in Epistemology at University Paris 7-Diderot (under my guidance) with a Thesis Dissertation on "*Epistemological atomism and theoretical pluralism in Boltzmann's thought*".[83] He got since that time projection in the Philosophy of Science activities in Brazil, animating seminars and coordinating publications, publishing in various fields of History and Philosophy of Science, and keeping an interest in the History of Science in Brazil.[84] He is presently professor of Philosophy at the State University of Rio de Janeiro in the Institute of Philosophy and Human Science. André Mendonça has recently defended his Doctoral thesis under Antonio Videira's guidance in the same University.[85]

In their paper, A.A. Videira and A. Mendonça present a critical panorama of the conceptions of Philosophy of Science in relation with social factors, as they have developed and transformed in the two last third parts of twentieth century and up to now, considering essentially the British and North-American contexts – which have dominated the litterature in the domain. This focus relates their contribution with the thema on the social dimension of History of Science and Epistemology, as we have already commented when dealing with it. "Contextualizing" the question of "the contexts of discovery and justification", the authors sketch how this question has informed differently three moments of the philosophical approach of scientific knowledge. The first one is that of the explicit separation, from the logical empiricism and positivism to the critical rationalism, let us say from Reichenbach to Popper. The second moment is that of the "historical philosophy of science", illustrated by the names of Thomas Kuhn, Paul Feyerabend, Imre Lakatos and Stephen

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[83] Videira (1992).

[84] See, for instance, the special issue of the journal *Ciência e Ambiente* on Einstein, which he edited: Videira (2005). A. A. Videira organized the archives of the Austro-Argentine Physicist Guido Beck, and has worked on G. Beck's life and on the History of the CBPF (Brazilian Centre for Research in Physics), a prestigious and efficient Institution founded some 60 years ago. He organized together with the Physicist H. Moysés Nussensveig a Guido Beck Symposium, in Rio, 1994 (Nussensveig & Videira (1994)).

[85] Mendonça (2008).

Toulmin, and characterized by "mixing the two contexts" – I would say: up to a certain point (my comment), for a strong splitting seems still maintained between the socio-historical emphasis of the two first and the rational reconstruction against the effective historical path of the two other. The third moment is that of the "new sociology of science and the new history of science, precisely the Strong Programme and the Science Studies", which pretends to have overcome the splitting – but actually (again my comment), for most of its protagonists, through submitting the justification too to social factors, with the "consensus", and finally dissolving rationality and rubbing out epistemology and philosophy.

A.A. Videira and A. Mendonça develop their analysis of the respective conceptions in this frame, introducing many nuances, for example by showing that the splitting of the contexts of the first phase should not be confounded with an absence of interest towards social concern, and that the enlightenment inspired conception of science (universality and rational criticism) was adequate to democracy, by its claim of "the necessity to avoid political authoritarism and totalitarism". They underline as well that Kuhn "did not aim to deny either rationality or objectivity", and they show an attitude of intellectual respect towards the authors and conceptions they study, even the most extreme ones such as those of the "Strong Program" formulated by B. Barnes and D. Bloor, with its four "principles" of "causality, impartiality, symmetry and reflexivity" which don't let space to arguments of reasoning in the retained scientific procedures. They point at Bloor's purpose which is not only to take into account the social factors in the construction of scientific knowledge but, litterally, to give account by Sociology of "the *specific cognitive content* of science", "to move traditional philosophy of its pretension of describing scientific knowledge", and actually give such paper to Sociology, which is able, "due to its empirical methods", to "naturalize" the cognitive content of science. By doing this, for sure, the distinction between the contexts of discovery and justification is completely overcome, for the benefit of a complete and assumed *social-reductionnism* (this is my expression) – a position highly questionable. Finally A.A. Videira and A. Mendonça evoke the more recent period, since the eighties, which has seen the publication of "a lot of empirical studies with historical, sociological and ethnographic characteristic, in which science appears as a material and cultural practice". I shall not enter in the details of their description, if not to mention the problem, interestingly discussed for example by Peter Galison, of the meaning that is to be given to the expression "social construction", which, although used and abused, might keep a meaning compatible with the epistemological dimension. I already alluded to the conclusion of the paper in the section of this Introduction dealing with the "social concern".

1.8 Foundational, Formal and Logical Approaches

And we finally arrive at the last group of contributions to this book, held together under the rubrique "Foundational, formal and logical approaches".

Décio Krause's paper bears on "The metaphysics of non-individuality". For many years Décio Krause, formerly a Newton da Costa's student, now teaching at the Federal University of Santa Catarina (Florianópolis), has been working on the relation between Logic and Quantum Mechanics, in the vein of the foundational approach of Physics on the side of Logics. His aim is not only to try to map the conceptual characteristics of Quantum Physics, so different from those of Classical or Continuum Physics as everyone knows, in a suitable appropriate logical scheme, but also to analyze as deeply as possible the precise meaning of the quantum concepts. Indiscernability (or indistinguishability) is maybe the core of the specificity of the quantum concepts, that makes Quantum Physics totally irreducible to other branches of Physics, and it is of the first importance to understand exactly what it means, from the physical and epistemological point of view and at the same time under the light of the semantic and logic-mathematical analysis. Décio Krause excels in the latter. He investigates the concepts of "individuality" (and "non-individuality") and "discernability" (and "indiscernability"), with the care of not confounding them, and tries to make explicit what "identity" (and "self-identity") means, finding help in the logical investigation of "higher-order languages" initiated since Leibniz (compared with the traditional Aristotelian one), renewed since Bertrand Russell and Alfred North Whitehead, and after them by Non-Classical Logics, such as Propositional Logics (Hans Reichenbach, George Birkhoff and John von Neumann, Jean-Louis Destouches, Paulette Février), or Quasi-Sets Theory and Non-Reflexive Logics.[86] Needless to say that the works in Logics and logical analysis of his Master Newton da Costa take an important place in this arco-iris of non classical logical commitments, and have highly contributed to nourish his inspiration.

One can disagree on some points or details of his analysis (as I do for example with the denomination of "*metaphysics* of non-individuals", to which I would have preferred "epistemology of undiscernible", for metaphysics is for me something different), but this does not diminish the relevance of the proposed view. I would say, for my part, that indiscernibles are distinctive in a way, in that sense that each one can be numbered (they are "countable") but they cannot be ordered (actually I remember another study of D. Krause where he made the clear distinction between cardinal and ordinal numbers as for the way to number them), which means that they are individuals. The author deals also with the semantics associated with this views and he rejoins in doing so, as I see it, the "conceptual" kind of investigation which puts forward the physical meaning (i.e. content) of the specific quantum concepts which have issued from a historically situated elaboration.

Francisco A. Dória and Manuel Dória present a paper on "Einstein, Gödel, and the Mathematics of Time". Francisco Doria is professor of Theoretical Physics and Logics at the Federal University of Rio de Janeiro. He has performed in particular some very interesting and important works with Newton da Costa on matters at the junction of both concerns, basing themselves on Kurt Gödel's theorem of incompleteness for Arithmetics, hence for all Mathematics, and extending the problematic of decidability to axiomatized theories of Mathematical Physics such as Classical Mechanics, and the Theory of Dynamical Systems or General Relativity

[86] See also French & Krause (2006).

Theory.[87] In their paper, the Dorias, father and son, consider the discussion that took place between the same Kurt Gödel and Albert Einstein about some kinds of solutions of the equations of General Relativity Theory applied to Cosmology that were obtained by Gödel.[88] These solutions were "teratologous" in that sense that they represented closed time-space trajectories around the Universe, meaning that starting from a given space-time point (or event) and going in the normally oriented time direction, they would meet again the origin, namely the same space-time event from where they came. Gödel concluded from this that time has no reality, and Einstein responded that such solutions should be discarded as being non-physical for not respecting the arrow of time, given to us in last instance by Thermodynamics.

Now Francisco and Manuel Dória ask to themselves – and to us as well, insofar as we can follow their very technical logical axiomatization, which I must confess has not been really my case – a question somewhat different in formulation from that which preoccupied Gödel and Einstein. They start by observing that Gödel's Universes "do not have global time coordinate" and that, for such Universes, "it is meaningless to refer to a 'beginning of time'". The authors then ask whether such a situation would not be a common one, and not only one specific of "Gödel-like models of the Universe". In order to explore this problem they – in their own words – "concoct a potion that mixes up ingredients from [axiomatized] differential geometry, from [axiomatized] general relativity and from logic". The description of the potion-making, although it be the essence of the matter, escaped my competence as a reader ignorant of the art of such abstract and refined sorcellery, if not to understand that being given first an axiomatic definition of what a global-time coordinate for a space-time would be, and choosing some "smooth exotic 4-manifolds", modified through the help of other fields of Physics than gravitation to determine the spacetime structure, and other formal considerations, they come to the conclusion that "an arbitrary spacetime" has not "a global time coordinate". This is indeed a formal result, on decidability through algorithms. One is faced however, after it being stated, with the problem of our real world, and the authors express the consciousness of this by concluding in the form of some straightforward questions about the physical meaning of the obtained result. Questions to which the answers are let finally to the real world – so it seems to me – as an echo to their initial evocation of the time direction known from the Physical Cosmology.

Newton da Costa and Otavio Bueno deal in their paper on *"Quasi-Truth and Quantum Mechanics"*, with various interpretations of Quantum Mechanics and submit them to the criterion of "*quasi-truth*", a logical concept (which can be considered equally a logical model) coined and characterized by Newton da Costa and other searchers of his school in previous works in the domain of Non-Classical Logics. This concept is adapted from the logical definition of Truth as given

[87] See in particular: Costa & Doria (1991), and the references quoted in F. & M. Doria's paper.

[88] On this discussion, see Gödel's contribution and Einstein's "Reply..." in P. A. Schilpp (ed.), *Albert Einstein, Philosopher and Scientist*,. I have analyzed Gödel's views and Einstein's and Jacques Merleau-Ponty's comments about them in: Paty, M., La nature du temps cosmologique selon Jacques Merleau-Ponty, *in* Bachta, Abdelkader (éd.), *Jacques Merleau-Ponty: une pensée multiple*, Centre de *Publications Universitaires*, Tunis, 2006, p. 119–159.

by Tarski to "partial" concepts and structures. The interpretations of Quantum Mechanics which N. da Costa and O. Bueno chose to examine are all in agreement with empirical data – actually, they explicitly restricted their inquiry to non-relativistic Quantum Mechanics, which is not a problem from a fundamental point of view, as the main difficulties of interpreting Quantum Mechanics arise already in its non-relativistic formulation. The authors take as a formal "framework to assess [the] interpretations in an objective way" the framework of the *"partial structures approach"* developed elsewhere by N. da Costa and Steven French, and which is organized on the concepts of *partial relation*, *partial structure* and *quasi-truth*, with the purpose of accommodating "the openness and incompleteness of the information that is dealt with in scientific practice". Then the elaboration goes by constructing logical concepts – they may be called *epistemo-logical*, my comment – adapted to the kind of theory-and-interpretation considered. As the authors say: "The idea, intuitively speaking, is that a quasi-truth sentence α does not describe, in a thorough way, the whole domain that is concerned with, but only an aspect of it: the one which is delimited by the relevant partial structure A". This procedure has been already proven useful to characterize a domain of validity for the approximation of a theory – for instance, Newtonian mechanics. Also, "partial structure" allows to admit more flexibility to the idea of empirical adequacy, such as to include the changes that occur "in the course of the history of a scientific theory".

On the whole, the authors find that, even in the absence of empirical grounds to choose among the existing interpretations, there are *"pragmatic factors"*, based on the above-mentioned concepts, that permit to choose among them: these factors are revealed by the "quasi-truth" and "partial structure" analyse, which are proposed as practical tools for logical evaluations and justifications. I would like to make a simple comment on them, particularly on *"quasi-truth"*, to state how rigorous logics adapting its object propositions to a more flexible content may meet with more common rational intuition, namely that one which operates at the epistemological and conceptual level when considering the physical sciences. Newton da Costa's concept of *"quasi-truth"* appears as a logic-and-pragmatic means to continue speaking of Truth with a logical concern despite the fundamental difficulty – actually, the impossibility – to define exactly the notion of Truth in logical terms. It came to me that, in everyday words, the programme of "quasi-truth" is akin to the idea that Truth is out of our reach or that we never possess it, but nevertheless the search for it still has a meaning – we get provisional and conditional truths. Einstein himself was of this opinion, and he used to refer to a statement of the philosopher Gotthold Lessing, according to whom the search for Truth was more secure that the possession of it. As an effect, it seems, from da Costa's and Bueno's paper, that "quasi-truth" can be considered either from a purely pragmatic point of view as from a critical realistic one – as the latter admits the ever imperfect character of knowledge, notwithstanding the postulated existence of a Real external to us and independent of us which we try to get at.[89]

[89] See also: Costa & French (2003),

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Chapter 2
Galileo and Modern Science

Pablo Rubén Mariconda

2.1 Galileo and the Scientific Revolution of the 17th Century

The work of Galileo Galilei (1564–1642) is part of the scientific revolution of seventeenth century. One of the most profound revolutions involving the human mind, it brought with it a radical intellectual change, and the birth of modern science is without doubt its most significant product and expression.

In this context, Galileo is universally acclaimed as the founder of *classical physics*, the domain of modern science that would be developed into a physical-mathematical theory of natural phenomena. This acclaim is justified because of Galileo's substantive contributions to the new science: most notably, his discovery of the law of free fall, his formulation of the theory of uniformly accelerated movement, and his discovery of the parabolic trajectory of projectiles. Galileo elaborated the first *kinematic theory*, providing a mathematical description of how the movement of physical bodies occurs in nature (cf. Galilei, 1933 [1638], 3rd and 4th Days). This theory would be fundamental for the development and consolidation of dynamics (and thus for deepening understanding of movement and of its role in natural events). Galileo, himself, took important steps in this direction, with his discussions of the centrifugal effect caused by terrestrial rotation (Galilei, 1933 [1632]), with his unique principle of the theory of movement that implicitly contains the idea of conservation of energy, and also with his dynamical theory of tides (cf. Galilei, 1933 [1632], 4th Day).

It is also common to regard Galileo as one of the founders of *experimental method*, despite the strong opposition of Koyré in his influential and seductive interpretation of a platonic Galileo, who operates mathematically a priori (cf. Koyré, 1978a, b). From this point of view, it is not only Galileo's positive scientific achievements that count as his contributions to posterity, but also how he conceived of physical science and scientific method and, especially, how he arrived at scientific results. In summary, what characterizes Galileo's scientific attitude – and also the modern scientific attitude – is the quest for mathematically expressible regularities

P.R. Mariconda (✉)
Departamento de Filosofia, FFLCH, USP, São Paulo, Brazil
e-mail: ariconda@usp.br

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in nature, the so called *laws of nature*, and the method of certifying their truth by performing *experiments*. The law of falling bodies that Galileo confirmed by means of experiments with the inclined plane is the exemplary instance of the fruitfulness of this attitude (cf. Galilei, 1933 [1638], p. 175–176; Mariconda and Vasconcelos, 2006, Chapter 2).

In order to evaluate the claims, that Galileo is among the founders of classical physics and also of the experimental method, I will attempt to contextualize them historically, so as to reveal the intellectual and socio-institutional reach of the scientific activities of the great Pisan.

2.2 Active Attitude and Scientific Instruments

It is common to characterize the scientific revolution of the seventeenth century as a profound transformation in the fundamental attitude of the human mind – expressed in the opposition between an *active attitude* and a *contemplative attitude*: modern man tries to dominate nature, while medieval man only contemplates it. This characterization should not be taken absolutely, for doing so would minimize the technological achievements of the Middle Ages, and exaggerate the influence of technology in the scientific development of sixteenth and seventeenth centuries. But, it is certainly true that, much more than ancient and medieval thought, modern philosophy, ethics and religion emphasize action (praxis).

Galileo's tendency towards an active attitude is exemplified with his interest in developing scientific instruments. Early in his scientific work (1586–1587), he invented the hydrostatic balance (Galilei, 1929), an instrument designed to resolve the practical problem of measuring the specific density of materials, defined by Archimedes in his treatise on the floating bodies. Then, during the next 13 years, he contributed to technical developments of the *geometrical-military compass*; and, after 1609 (with a clearly scientific program) he worked with the *telescope*.

Galileo invented the geometrical-military compass, which is fundamentally a compass provided with a rule that permits rapid calculations of distances, depths, altitudes and so on. Obviously this reflects an active attitude. This compass, fabricated in Galileo's workshop at Padua, was sold together with a manual (with instructions on its use) called "The operations of the geometrical and military compass" (Galilei, 1932 [1606]), published in Florence. To sell an instrument with the corresponding manual of use was certainly a novelty, mainly because it reflected an active attitude interested in utility.

Concerning the telescope, although Galileo was not its inventor, he was the first to develop and utilize it in systematic and continuous astronomical observations. In doing so, he provided for an apparatus, which awakened at the time a lot of curiosity and whose military value was immediately recognized (Galileo himself sold it for this utility to the Republic of Venice), a scientific role of great value to astronomy and to science in general (cf. Mariconda and Vasconcelos, 2006, p. 71–74). It is true that Galileo did not address the theoretical problems posed by the use of telescope.

In particular, he ignored the optical theory that explained the functioning of the telescope, a theory that could be found partially in the works of the Italian, Giovanni Battista Della Porta, *Magia naturalis* (1589) and *De refractione* (1593), and completely in the works of Johannes Kepler, *Ad Vitelionem paraliponema* (1604), which presents an exact explanation of the lens properties, and *Dioptrica* (1611), in which Kepler expounds the complete theory of telescope. This lack of interest in the optical theory does not lessen Galileo's merit. It was his effective utilization of the telescope, and demonstration of its utility, that brought about the need to understand its functioning and the importance of the theory that explains its reliability. Galileo was certainly the first to show the tremendous scientific utility of the telescope with the publication of his famous astronomical observations *The Starry Messenger* (1610). Then, over a period of more than 20 years, from the end of 1609 to the publication of the Dialogue, in 1932, Galileo made several sets of systematic and continuous telescopic observations, including of Jupiter's satellites, the rings of Saturn, and the sunspots. His most famous observations are those related with sunspots (cf. Clavelin, 1996, Chapter 4; Mariconda, 2000, p. 83–85), published in *Letters on Sunspots* (1613) a work which collects Galileo's three letters responding to the traditionalist vision of the Jesuit Father Scheiner (Galilei, 1932 [1613]).

The practice of telescopic observation undoubtedly contributed to open the door to gaining better knowledge of the solar system and of the universe, and to cultivating the attitude that emphasizes controlled and systematic observation, made with the aid of instrumental apparatus that is specifically designed to scientific ends. In this way, the telescopic research of Galileo not only influenced the domain of the macroscopic, where it opened the possibility of a new cosmology, but also marked the beginning of microscopic research, of the development of observational knowledge on the microcosm. Galileo himself certainly did not contribute directly to microscopy, but he did to the beginning of a new scientific style, one which combines mathematics and experience or, as in his own case, geometry and experiments, one that operates with experiences constructed by reason (Mariconda and Vasconcelos, 2006, p. 42–52, p. 66–74).

It is thus confirmed that, throughout his entire scientific career, Galileo contributed in major ways to the discovery, development and use of measuring and observational devices – indicating that his work was marked by (1) the application of the experimental method to the study of natural phenomena; and (2) the relation between science and technical practices. Broadly speaking, this justifies the claim that Galileo is one of the founders of the experimental method.

2.3 The Alliance Between Science Technical Practices

The change of attitude, characteristic of the scientific revolution of the sixteenth and seventeenth century, is also marked by a second important feature that challenged the basis on which the contemplative attitude was, in great measure, founded, namely the strict distinction between *episteme* (science) and *techne* (technical practice).

According to the ancient Greeks and (following them) the Medievals, whereas a higher grade of knowledge – certain, necessary and demonstrable knowledge, apodictic science or science in the strict sense – belongs to *episteme*, *techne* corresponds to practical knowledge, know-how, or the arts and technical practices in general. Furthermore, this separation between science and technical practice was associated to a value-laden hierarchy, which maintains that the former activity is clearly superior to the latter. Considering the two types of activities to be completely independent resulted in conceiving of science as basically a theoretical activity, without practical interests or concern for technical consequences. This led to science eventually becoming confused with an activity that involved endless theoretical controversies on the correct interpretation of traditional, mainly Aristotle's texts. This perspective, from the very beginning of universities in the twelfth century, led to emphasizing the importance of the *auctor* and the idea of authority in its original sense – that it exists in certain authors, the authorities, who have superior knowledge and to whom others should submit.

It is natural that this valuation of contemplation, and the consequent separation between science and practice, were profoundly rooted in the institutional organization of knowledge in the sixteenth and seventeenth century. It reflected, on the one hand, the scientific and philosophical tradition that the Church maintained and taught in the universities and, on the other hand, the fact that the technical teaching developed rather independently from the tradition of universities, first, during the Middle Ages, in the artisan guilds, and latter, in the famous schools for artists, and arsenals, in the Renaissance and early modernity.

In the educational organization of universities, Aristotelian physics constituted the systematic introduction to the traditional scientific encyclopedia, because it was considered the only one which could bring unity and theoretical coherence to scientific content that is itself fragmentary. On the other hand, Aristotelian physics is founded on metaphysics, that is, on a system of concepts and universal relations in which the endless variety and apparent accidental character of existing things seem to point to a profound teleological unity of a well ordered cosmos – the unity of the cosmos is teleological because the "perfect order" of the cosmos is a finality which guides the flux of natural events in a determinate direction. This Aristotelian doctrine, whose guarantee is the authority of centuries of union with scholastic theology, remained until the first half of the seventeenth century as the solid foundation of all formal education at the universities of Europe, as the incontestable criterion of truth. Aristotle, thus, remained as the authority in matters of organization of scientific curricula (cf. Mariconda, 2000).

The polemics on the compatibility of the Copernican system and the Bible, connected with the first process against Galileo (1613–1616) that resulted in the condemnation of Copernicanism,- included Galileo's critique against authority and tradition, in particular that derived from Aristotle. We can now see that this also involved an institutional struggle which culminated in Galileo being opposed by the philosophers, who held a special place in the traditional organization of scientific curricula; consequently, Galileo was drawn to oppose all the traditional structure and administration of the universities. As a mathematician, Galileo was obliged to teach

Euclid's Geometry and Ptolomy's Astronomy; as a physicist, he should be a natural philosopher, and so be limited to the exegesis and philosophical interpretation of Aristotle's Physics. Thus, there was no room in university curricula of the first half of seventeenth century for mechanical investigations, considered as eminently technical, and not properly scientific. Such mechanical investigations possessed a secondary value in the organization of scientific knowledge.

But Galileo's science differs from simple t*echne* in the Aristotelian sense. Galileo's science – modern science – does not make a clear separation between *episteme* and *techne*, between *science* and *technical practices.* It is a *utilitarian science*: not only does it have practical consequences, since it includes mathematical treatment of many physical problems that possess practical interest, but also it is capable of being controlled, tested and evaluated by these practical consequences.

In order to appreciate the technical dimension of Galileo's scientific work, it is necessary to take in consideration his scientific trajectory during the so called Paduan period (1597–1610), prior to the discovery of the telescope and the long period he dedicated to astronomy and to the defense of the motion of the Earth. We grasp then that Galileo's science is utilitarian science from the very beginning, long before Copernicanism came to occupy completely Galileo's scientific agenda. At the very beginning of his career, Galileo engaged in mechanical investigations, on the one hand, on aspects of statics that point toward a theory of the resistance of materials and, on the other hand, on the parts and composition of machines. These investigations are reported in two military treatises, in which Galileo aims to show the technical applicability of the new science: *Short instruction on military architecture* (Galilei, 1932a) and *Treatise on fortifications* (Galilei, 1932b), and in a short manuscript, *The mechanics* (Galilei, 1932c), which was circulated widely and published while Galileo was still alive in a French translation by Marin Mersenne in 1634.

This marks the beginning of a conception of science, linked with a new conception of scientific rationality, in which there is a strict connection between scientific and technical work. The transformations that took place in scientific mentality, in particular, in the physics of seventeenth century, originated mainly from new difficulties and ever more precise questions posed by technicians. The technicians wanted to know precisely *how* certain particular phenomena behave, so that we can know how *to act* when confronted with them. That is why, for the technicians as well as for Galileo, the discussions of Aristotelian physicists about the causes of natural phenomena, and the speculations of university philosophers about the final essence of Nature, seem totally devoid of interest and significance.

This alliance between science and technique, of which Galileo was one of the first exponents, led evidently to a radical transformation of scientific institutional organization, to a entirely new characterization of scientific research and of its aims, and to a new style of scientific systematization and exposition. This is not to say, however, that this transformation removed all theoretical considerations from science. Modern science only abandons those theoretical investigations that, in view of their generality and excessively abstract and speculative character, cannot be tested by experience and are maintained only on the basis of authority conferred by tradition. In the new conception of science, those speculations are eliminated that have no relation with experience, opening the way to theoretical considerations that (1) can facilitate the discovery and formulation of natural laws, the making of predictions, and the stipulation of practical rules for action, and (2) can be tested by experience and practical consequences. This means that science not only has a practical function in confronting problems posed by technical practices, but also a theoretical function in connection with the rational justification of specialized (scientific based) modes of engaging in these practices. More and more scientific speculation would be grounded in practical activities, thereby opening the possibility that theories be judged both by their theoretical value and by the possibility of application in technical practices.

Two remarkable examples of the relation theory/practice, characteristic of the union between science and technical practice, can be found in the great final work of Galileo, *Arguments and mathematical demonstrations on two new sciences* (Galilei, 1933 [1638]). In this work Galileo returned to his earlier orientation and produced a work that included, on the one hand, creation of a technical discipline and, on the other hand, formulation of physical kinematics, i.e., a mathematical description of the movement of physical bodies. The union of theory and practice is evident most notably in the 2nd Day, where Galileo presents the first new science of the resistance of materials, and also in the 4th Day, in which he develops an important part of the second science, the theory of projectiles movement. Galileo introduces in the first new science considerations on the "scale effect" that are fundamental for this kind of investigation opening the possibility of laboratory tests with prototypes much smaller than the original. From the knowledge obtained by the science of resistance of materials, one may project great structures with a previous calculus of efforts and rupture points of the kind of material to be used in view of the required effort. The practical contribution of the first new science is therefore decisive. Galileo is founding not only a new science, but defining a new type of professional activity, civil engineering. The same can be said for the practical contribution of the theory of projectile movement of the 4th Day. This theory informs the practice of artillery so as to enable the making of "scientific shots", that is, to plan beforehand the better use of artillery (cf. Mariconda and Vasconcelos, 2006, p. 239–42).

The introduction of the experimental method in scientific practices generated a cycle theory-instrument-experiment that can be schematically represented as follows:

$$
\begin{array}{ccc}
 & \text{Theoretical conceptualization} & \\
\updownarrow & & \updownarrow \\
\text{Instrument or instrumental apparatus} & \leftrightarrow & \text{experimental elaboration}
\end{array}
$$

This cycle, clearly present in Galileo's works, is especially appropriate to promote the union between science and technical practice, which in the long run made it possible for science to penetrate throughout the world we live in, producing our technological-scientific civilization.

2.4 Mathematization of Nature and Mechanization of the World

In addition to his propounding the centrality of practical and instrumental action, and the union of science and technical practice, there is a third aspect to Galileo's contribution (one linked with views found in such authors as Copernicus, Kepler, Galileo and Descartes): his promotion of the mathematization and the mechanization of nature. This view profoundly affected organized culture, and sums up the profound modifications in the conceptions of nature, science, and human capacity that accompanied the rise of modern science.

At this point it is worth considering more closely the reach of the transformation caused by the simple idea of the motion of the Earth, in order to understand more profoundly the link between the mathematization of Nature and Copernicus' conception that the Earth is a planet that, like all the others, revolves around the Sun. There are two sources of the fascination with the Copernican heliocentric system, and they also provoked the reaction and resistance to it, and which together permit Copernicanism to be characterized as a specific type of scientific and philosophical posture. The first has to do with the central and essential role in the history of thought of the so-called Copernican revolution. The second refers to a radical change in the category of appearance intrinsically connected with the constitution of modern scientific observation.

Prior to Copernicus, we may say, the categories of thought themselves were effectively organized around the claim of our central position in the universe, so that the geocentric conception is part of the core of the anthropocentric conception of culture. We perceive – partly because of the structure of our perception and partly because of our anthropological evolution – that the Earth is immovable at the central place of our perception; that is, the immobility of Earth is tied to a concept of observer connected with his central place, which is confused with what is informed by its perception. There is, therefore, a unity between the geocentric conception and the phenomenology of the sensible spontaneously practiced by humans. Even today we continue spontaneously to see that the sun rises in the east and sets in the west. In the Ptolomaic universe, the central place of the immovable terrestrial observer is the law of what exists. The organization of phenomenal reality is the effect of the perception of an observer and depends on his place, so the movement of the sun we see in the sky is taken immediately as what really happens. This means that, although there is here a constituted appearance, the appearance is constituted from being itself and from its categories, so does not depend on the manner by which we can know. But in Copernicus the position and movement of the observer do make a difference. For Copernicus, the movement of the sun that we see from east to west is in reality an optical appearance to the terrestrial observer of his own movement from west to east. So, if we suppose that the earth moves, we must correct the celestial observations taking into consideration the motion of the Earth. To summarize: we *see* the movement of the sun, but *observe* the reflection of the motion of the Earth on its own axis. Scientific observation is a highly elaborated (theoretically, conceptually) process. We can now understand that the Copernican thesis of the Earth's movement, in decentralizing the observer and putting him in movement, would have a profound cultural impact, since it directly opposes established knowledge, science, religion and common sense. At the scientific level, with Copernicus, the movement of the observer comes to have a radical or primitive function so that to "save the appearances" comes to means subsuming the appearances under the principles of physics that explain them and that, therefore, make them possible. There is, thus, in Copernicus' astronomy a proposal about explanation that enters into the field that tradition has reserved to natural philosophy (cf. Mariconda, 2000, p. 92–96; Mariconda and Vasconcelos, 2006, Chapter 3).

This proposal effectively amounts to the claim that the whole set of astronomical observations must be explained in terms of the laws, order, structure and interaction that underlie the phenomena described by those observations, so that these observations are taken as observable effects of underlying unobservable causes (cf. Mariconda and Lacey, 2001). It can be clearly found in the work of the two great Copernicans, Kepler and Galileo; in Galileo particularly in the 4th Day of the *Dialogue on the two great systems of the world,* in his explanation of the tides, according to which the tides are caused by the combination of the double movement of Earth, and so a visible effect of causes unobservable to the terrestrial observer (Galilei, 1930 [1610]; Mariconda, 1999; Mariconda and Vasconcelos, 2006, p. 166–183).

All important authors of the first half of seventeenth century, such as Kepler, Galileo, Descartes, Mersenne, regarded it as necessary to unify the heliocentric astronomy of Copernicus with the mechanical conceptions of the new science. For them, the acceptance of the Copernican system is integral to the intellectual framework of the modern critique, made in name of reason, to traditional astronomy and cosmology, which suppose that Earth and Heaven are essentially separated, with celestial bodies having circular movements, considered perfect (complete), and terrestrial things rectilinear movements, considered imperfect (incomplete). Furthermore, the old tradition separates *astronomy*, understood as simply hypothetical and as a mathematical description of observed celestial movements, and physics (natural philosophy), understood as the study of the causes and essences of changes and transformations that we see happening around us. With their endorsement of Copernicanism, Galileo and Kepler began to criticize the traditional view that the universe is composed of two essentially different regions, and to take an important step in the direction of the conception of a homogeneous universe, according to which all regions of the universe obey the same laws (Galilei, 1932 [1613]; Mariconda, 2005).

One can recognize a strong convergence between Kepler's astronomical research and Galileo's mechanical research: both were searching methodically for mathematically formulated regularities under which observable natural phenomena could be subsumed. The search for natural laws, for regularities under which observable natural phenomena can be subsumed is the mark of modern science. One of the central aims of scientific research has become the formulation of such laws, that is, of precise statements that are verifiable by experience and expressed in mathematical language, about the universal relations that underlie particular phenomena. So, both the mechanical program of Galileo and the astronomical program of Kepler feature integrally in the constitution of a physical science that attempts to formulate universal and mathematical laws of movement, aiming at the unification of astronomy, the theory of planetary motions, with mechanics, the theory of local and terrestrial movements – thereby laying the basis on which Newton would later construct the dynamical explanation of why physical bodies move in the manner in which we see that they move (Galilei, 1932 [1613–1616]; Mariconda, 2005).

It is worth pointing to yet another dimension of Galileo's mechanical program, because it corresponds to the repercussions that the two new sciences of Galileo had, beyond the strictly scientific field, for furthering the modern view of Nature conceived as a mechanism regulated by mathematical laws. This gets us to the core of the mechanist conception that sustains the mathematization of Nature. In effect, these two processes – mathematization and mechanization of Nature – are intertwined in Galileo, as can be seen in the formulation in Galileo's *Assayer* of the effective epistemological conditions for the application of mathematics to experience. These conditions are formulated in the distinction between primary qualities – form, figure, number, movement and contact – and the secondary qualities – colour, smell, taste and sound (Galilei, 1933 [1623], p. 347–352). Following Galileo, the secondary qualities reside not in the observed body, but in the observer; since they have only an existence assured by perceptive subjectivity, they are only names (*flatus vocis*) that are given to sentiments or affections felt by the subject of perception. On the other hand, the primary qualities cannot be eliminated, for since they belong necessarily to the concept of physical body, they exist in physical bodies as a rational element capable of mathematical treatment.

The distinction between primary and secondary qualities, inaugurated by Galileo, clearly proposes the elimination of subjective qualities or their reduction to quantitative terms. This is tantamount to a conception of nature as a mechanism that is capable of mathematical treatment and experimental determination. The drastic reduction of the enormous variety of sensible qualities to properties that can receive mathematical treatment represents, from an epistemological point of view, the assimilation of qualitatively differentiated physical space to the homogeneous geometrical space. This assimilation expresses emblematically the perspective of mathematization of nature and, above all, it circumscribes the ontological basis that is indispensable for proceeding to the mechanization of Nature and of the world.

2.5 Autonomy of Science and Universality of Scientific Method

Finally, the fourth historically important contribution of Galieo that will be considered has to do with his proposals about freedom of thought anchored in method. It is linked to Galileo's commitment to heliocentrism and is integral to what is known as Galileo's case, that is, the condemnation of Copernicus in 1616 (Galilei, 1932

[1613–1616]) and the condemnation of Galileo in 1633 by the Roman Inquisition (Favaro, 1938). In the light of this consideration, the Galilean defense of Copernican cosmology acquires a greater cultural reach that goes beyond the frontiers of the scientific domain, and obtains a broader intellectual impact. It sees the significance of Galileo's commitment to Copernicanism to lie in his explicit rejection of authority – either Aristotle's authority or the authority of the Holy Scriptures – as a criterion of truth in scientific questions, and in his consequent defense of liberty of scientific research. Galileo's defense of liberty of scientific research is tied to his claim that the truth of scientific conceptions, in particular, the truth of Copernican theory, must be decided by sensible experiences and necessary demonstrations; but it is also, to a considerable extent, part of a political cultural program that, starting out from a careful separation of the domains of Theology and Science, has twin aims (cf. Geymonat, 1984). First, the program aims to dismiss the objection that the Copernican system – especially in endorsing the theses of the Sun's centrality and Earth's mobility – is contrary to the Holy Bible and so, as maintained by the orthodox point of view (established by the Council of Trent), under grave suspicion of heresy. Second, it aims to keep the Church from opposing the progress of new science by siding with its traditionist opponents, who impeded spreading new ideas in universities and obstructed the communitarian organization and institutionalization of new scientific disciplines. We may say that Galileo wanted it to be possible to be simultaneously a good Catholic and a Copernican; to believe in God, follow the Bible and to prove that the Earth moves.

Galileo's response to the problem of the supposed incompatibility between Copernican theory and the Bible consists, then, in maintaining that greater authority cannot be attributed to the Bible than to Nature itself, when dealing with natural matters (Galilei, 1932 [1613–1616], p. 283). Furthermore, as mathematical science of Nature possesses an independent (autonomous) method of checking out the truth and reaching rational decisions about controversies on natural matters, it doesn't need to be based in any authority outside of its own sphere of competence. The autonomy of science is based on the proposal of the *sufficiency* of scientific method for evaluating the truth of natural theories. Scientific method enables a critical examination based on "sensible experiences" and "necessary demonstrations", the later identified by Galileo with the kind of argumentation used in mathematical demonstrations (cf. Galilei, 1933 [1640]; Mariconda, 2003, p. 70–73).

Here, it is good to keep in mind that Galileo's methodological claims don't go much farther than affirming that scientific method is composed of sensible experiences and necessary demonstrations. In his *Dialogue concerning the two chief systems of the world*, for example, the role of sensible experiences is articulated around what Galileo considers to be Aristotle's empiricist principle: "sensible experience must be contrasted to whatever discourse is fabricated by the human mind" (Galilei, 1933 [1638], p. 113, p. 131–132). Similar considerations reappear later in a letter written in 1640, in which the critical function of the empiricist principle, as interpreted by Galileo, is emphasized: "to put experience before any discourse" is a precept "for a long time put before the value and force of the authority of all men in the world, a principle which even you admit we do not have to subordinate to the authority of others, but that we should negate such authority ourselves every time we find that the senses show us the contrary" (Galilei, 1933 [1640], p. 249). Evidently that part of scientific method, which refers to sensible experiences, serves as an antidote for having recourse to authority. It is this critical scrutiny by experience that leaves scientific method free from all and whatever authority, even that of the author of the discourse himself (Mariconda, 2003, p. 71–73). The *auctoritas* in which was based Medieval and early Modern universities was finished.

It is, however, important to be clear that Galileo has not vindicated any innovation in the method of science, and he never claims originality or precedence in methodological matters. The questions about precedence that concerned Galileo are all properly scientific: either observational or about the conceptual content of theoretical theses involving the mathematical analysis of experience, as for example related to the discovery of the parabolic trajectory of projectiles. Galileo makes no claim to reform the *Organon*, as Bacon's does, or to consider method to have its own proper domain or to give it a systematic treatment; as Descartes would do, proposing it as propedeutics to scientific knowledge. What Galileo does do is to claim the *sufficiency of scientific method* to decide about natural questions, for which we can use experience, discourse and intellect, in sum, for which we can apply natural reason (Galilei, 1932 [1613–1616], p. 284).

Finally, since nature prevails over the Scripture, for not all which is written in the latter "is tied to obligations as severe as each effect in Nature" (Galilei, 1932 [1613–1616], p. 283), and since science employs an autonomous method for certifying the truth of natural conceptions (the only method which is also accessible to human capacity), natural conclusions must prevail over the letter of Scripture and, in doing so, serve as an element in the determination of true meaning of Scripture. In Galileo's words: "[. . .] it is the task of commentators to find the true meaning of those passages of Scripture, showing its compatibility with those natural conclusions which are manifest to the senses, or based on necessary demonstrations that provide us with certainty and security" (Galilei, 1932 [1613–1616], p. 283–284). Clearly, Galileo associates the sufficiency of scientific method with his affirmation of the universality of scientific evaluation.

The theological-cosmological dispute that unfolded between 1613 and 1616 transcends the internal boundaries of scientific fields and deals with external factors that have intellectual and political dimensions. Hence, Galileo's defense of copernicanism is not just a matter of theoretically preferring the Copernican system over Ptolomy's or Tycho Brahe's system, in accordance with strictly scientific standards, but it is also fundamentally a polemic that involves the transformation of the standards of scientific judgment and a new way of circumscribing the domain of science. Both aspects of his defense inevitably have consequences in the larger domain of culture and in the institutional organization of disciplines and "professional careers" in the universities of the epoch. So, Galileo defends not only that science possesses a method sufficient to make its judgments independent (free) from the principle of theological authority, but (as we might expect from someone defending the autonomy of a scientific field or discipline) he also affirms unambiguously the universality of its judgments. Hence, he concludes, the interpreters of the Bible should try, either to make their commentaries consistent with truths established by science, or else to abstain completely from making judgments about matters that can be contradicted by the knowledge obtained by natural reason.

After his condemnation, Galileo was obliged by Roman Inquisition to abandon his defense of the Copernican system. This happened because the Counter-Reformation was strongly committed to maintain the theological orthodoxy of the Catholic Church against, on the one hand, the churches born with the Reformation and, on the other hand, all suspected forms of heterodoxy of the progressive and lay forces of the new science. Galileo's condemnation represented obviously the end of the political-institutional part of his ambitious program, but did not eliminate its profound cultural impact, expressed in the clear consciousness that the standards for the evaluation of scientific accomplishments must be independent, not only from the theological standards imposed by ecclesiastical institutions, but also from evaluative standards based on the authority of Aristotle that were defended by the tradition of universities.

So, we must also concede to Galileo the merit of having perceived with admirable clearness that the independence of scientific standards of evaluation, and consequently liberty of scientific research, are fundamental for the formation of scientific communities and the process of institutionalization through which the new science would be consolidated in Modern States at the end of 17th and throughout the whole eighteenth century. Modern science arose and prospered on the ruins of the principle of authority, but only at the end of eighteenth century, mainly after the French Revolution, would it be integrated into the university curriculum.

2.6 Conclusion

These four characteristics that are present in Galileo's works – centrality of practical and instrumental action; confluence and union of science and technology; mathematization and mechanization of nature; freedom of thought anchored in method – reveal that the common image of the great Pisan as founder of classic physics and experimental method is substantially correct. But it exaggerates by attributing to the individual, Galileo, more than he could effectively do by himself, for the creation of classical physics and the invention of experimental method are historical-social processes, which depend on the collective action, collaboration and organization of many people. Even so, Galileo is among that class of individuals that personify (in a way appropriate to his epoch) a certain *ethos*, a certain set of practices and procedures that defines the scientific style that is characteristic of early modernity. There is no denying that, with Galileo, a new personage has born in the intellectual and cultural scene: the scientist (cf. Mariconda, 1989; Mariconda and Vasconcelos, 2006, p. 14–19). Or perhaps it is better to say that, during the sixteenth and seventeenth centuries a new kind of intellectual activity was born, the scientific activity, which Galileo undoubtedly personifies.

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Chapter 3
Newton and Inverse Problems

A.K.T. Assis

3.1 Introduction

Isaac Newton (1642–1727) is one of the main scientists which ever lived. His two most important works were the *Principia*, first published in 1687, and the *Opticks*, first published in 1704. He always lived in England, entering Trinity College, in Cambridge, in 1661. He obtained the Bachelor of Arts degree in 1665, becoming in 1669 Lucasian Professor at Cambrigde University.

In this work we consider his research approach. We show that he always considered the inverse aspects of any problem. We show that this way of dealing with physics and mathematics is one of the sources of his immense creativity.

3.2 Inverse Problems in Mathematics

A very important period in Newton's life were the *anni mirabiles*, from 1664 to 1666, during which he obtained his first important results in mathematics and physics. In his own description of this period we can observe clearly his way of thinking (Westfall, 1990, p. 143)

> In the beginning of the year 1665 I found the Method of approximating series & the Rule for reducing any dignity of any Binomial into such a series. The same year in May I found the method of Tangents of Gregory & Slusius, & in November had the direct method of fluxions & the next year in January had the Theory of Colours & in May following I had entrance into y^{e} inverse method of fluxions. And the same year I began to think of gravity extending to y^{e} orb of the Moon & (having found how to estimate the force with w^{ch} [a] globe revolving within a sphere presses the surface of the sphere) from Keplers rule of the periodical times of the Planets being in sesquialterate proportion of their distances from the center of their Orbs, I deduced that the forces w^{ch} keep the Planets in their Orbs must [be] reciprocally as the squares of their distances from the centers about w^{ch} they revolve: & thereby compared the force requisite to keep the Moon in her Orb with the force of gravity

A.K.T. Assis (✉)
Institut für Geschichte der Naturwissenschaften, Universität Hamburg, D-20146 Hamburg, Germany; Institute of Physics 'Gleb Wataghin', University of Campinas — UNICAMP, 13083-970 Campinas, SP, Brazil
e-mail: assis@ifi.unicamp.br

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> at the surface of the earth, & found them answer pretty nearly. All this was in the two plague years of 1665–1666. For in those days I was in the prime of my age for invention & minded Mathematicks & Philosophy more then at any time since.

That is, he found direct and inverse methods of fluxions, which are the essence of our differential and integral calculus. From the method of tangents he could calculate derivatives and he could calculate areas by quadratures. His discovery of the fundamental theorem of the calculus linking integration as the inverse of differentiation also comes from his *anni mirabile*, [1, pp. 123–128].

3.3 Inverse Problems in Optics

In his book *Optics* Newton offered several examples of how he tackled opposite problems in physics, [2]. The work is divided into three books. The first book has two parts, dealing with the decomposition of white light into the colours of the spectrum after passing through a prism. The first part begins with eight definitions (of rays of light, their refrangibility and reflexibility etc.), eight axioms (the angle of reflexion is equal to the angle of incidence etc.), six theorems, two problems and sixteen experiments. The second part has five theorems, six problems and seventeen experiments. The second book deals with reflexions, refractions and colours of thin and thick transparent bodies (Newton's rings). The first part contains twenty-four observations. The second part has remarks upon the foregoing observations. The third part deals with the permanent colours of natural bodies and their analogy to colours of thin transparent plates, containing twenty propositions. The fourth part has thirteen observations concerning the reflexions and colours of thick transparent polished plates. The first part of the third book has eleven observations concerning the inflexions (difractions) of the rays of light and the colours made thereby. At the end of the book there are thirty-one Queries dealing with several aspects not only of optics, but also of mechanics, physics and philosophy in general. Although the structure of the book is somewhat similar to Euclid's *Elements*, the demonstrations of the propositions (also called theorems by Newton) are not based on pure logic as a set of constructions and reasonings following from the axioms. In the *Opticks* the proofs of the propositions are, in Newton's words, made "by experiments." This is a remarkable new feature introduced by Newton.

Let us see how the inverse aspects of the problems are handled by Newton in the field of optics. After presenting the definitions and axioms, he introduced a series of propositions, theorems and problems. His fourth proposition (also called the first problem) of the first part of book I runs as follows, [2, p. 64]: "To separate from one another the heterogeneous Rays of compound Light." He let the Sun's light into his darkened chamber through a small hole in his windowshut. About eleven feet from the window he placed a lens and after that a prism which separated the Sun's light into the colours of the spectrum upon a white paper. In the fifth proposition (also called theorem 4) of the second part of book I he explored the opposite effect, [2, p. 134]: "Whiteness and all grey Colours between white and black, may be coumpounded of Colours, and the whiteness of the Sun's Light is compounded of all the primary Colours mix'd in a due Proportion." In this case the proof is presented by six detailed experiments.

In the fifth Query at the end of the *Opticks* we see once more Newton considering both aspects of a problem (Newton, 1979, p. 339):

> *Qu. 5.* Do not Bodies and Light act mutually upon one another; that is to say, Bodies upon Light in emitting, reflecting, refracting and inflecting it, and Light upon Bodies for heating them, and putting their parts into a vibrating motion wherein heat consists?

The last Queries of the *Optics*, 30 and 31, are other examples of this aspect of Newton's way of thinking (Newton, 1979, pp. 374–6):

> *Quest. 30.* Are not gross Bodies and Light convertible into one another, and may not Bodies receive much of their Activity from the Particles of Light which enter their Composition? For all fix'd Bodies being heated emit Light so long as they continue sufficiently hot, and Light mutually stops in Bodies as often as its Rays strike upon their Parts, as we shew'd above. I know no Body less apt to shine than Water; and yet Water by frequent Distillations changes into fix'd Earth, as Mr. *Boyle* has try'd; and then this Earth being enabled to endure a sufficient Heat, shines by Heat like other Bodies.
>
> The changing of Bodies into Light, and Light into Bodies, is very comformable to the Course of Nature, which seems delighted with Transmutations. [. . .]
>
> *Quest. 31.* Have not the small Particles of Bodies certain Powers, Virtues, or Forces, by which they act at a distance, not only upon the Rays of Light for reflecting, refracting, and inflecting them, but also upon one another for producing a great Part of the Phaenomena of Nature? [. . .]

3.4 Inverse Problems in Mechanics

We now consider Newton's masterpiece, the *Principia*, [3]. It begins with eight definitions (quantity of matter etc.), a Scholium about absolute motion, his three laws of motion (which he also called as axioms), six corollaries, followed by another Scholium where he discussed the laws of collision etc. The remainder of the work is divided into three books. The first one deals with the motion of bodies, containing ninety-eight propositions (50 theorems and 48 problems). The second book deals with the motion of bodies in resisting mediums, containing fifty-three propostions (41 theorems and 12 problems). The third book deals with the system of the world in mathematical treatment. It begins with four rules of reasoning in philosophy, followed by six celestial phenomena (the planets describe areas proportional to the times of description etc.). After this there come forty-two propositions (20 theorems and 22 problems). At the end of the book there is a famous General Scholium.

In several places of this work we can observe Newton dealing with opposite aspects of any mechanical problem. This is evident, for instance, already in his third axiom or law of motion (Newton, 1934, p. 13):

> *Law III: To every action there is always opposed an equal reaction: or, the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.*
>
> Whatever draws or presses another is as much drawn or pressed by that other. If you press a stone with your finger, the finger is also pressed by the stone. If a horse draws a

> stone tied to a rope, the horse (if I may so say) will be equally drawn back towards the stone; for the distended rope, by the same endeavor to relax or unbend itself, will draw the horse as much towards the stone as it does the stone towards the horse, and will obstruct the progress of the one as much as it advances that of the other. If a body impinge upon another, and by its force change the motion of the other, that body also (because of the equality of the mutual pressure) will undergo an equal change, in its own motion, towards the contrary part. The changes made by these actions are equal, not in the velocities but in the motions of bodies; that is to say, if the bodies are not hindered by any other impediments. For, because the motions are equally changed, the changes of the velocities made towards contrary parts are inversely proportional to the bodies. This law takes place also in attractions, as will be proved in the next Scholium.

After the three laws of motion there are six corollaries. Then follows a Scholium where Newton demonstrates by pendulum experiments the validity of action and reaction in collisions. He also presents experiments showing that it is obeyed for magnetic attractions at a distance, (3, pp. 25–26):

> I made the experiment on the loadstone and iron. If these, placed apart in proper vessels, are made to float by one another in standing water, neither of them will propel the other; but, by being equally attracted, they will sustain each other's pressure, and rest at last in an equilibrium.

In the third book of the *Principia*, Newton presented six phenomena comprising Kepler's laws (Newton, 1934, pp. 401–405):

> Phenomenon I: *That the circumjovial planets, by radii drawn to Jupiter's centre, describe areas proportional to the times of description; and that their periodic times, the fixed stars being at rest, are as the 3/2th power of their distances from its centre.*
>
> [...]
>
> Phenomenon IV: *That the fixed stars being at rest, the periodic times of the five primary planets, and (whether of the sun about the earth, or) of the earth about the sun, are as the 3/2th power of their mean distances from the sun.*

From these phenomena he derived that the force of gravitation is inversely proportional to the distances (Newton, 1934, p. 406):

> Proposition I. Theorem I. *That the forces by which the circumjovial planets are continually drawn off from rectilinear motions, and retained in their proper orbits, tend to Jupiter's centre; and are inversely as the squares of the distances of the places of those planets from that centre.*
>
> [...]
>
> Proposition II. Theorem II. *That the forces by which the primary planets are continually drawn off from rectilinear motions, and retained in their proper orbits, tend to the sun; and are inversely as the squares of the distances of the places of those planets from the sun's centre.*

After arriving at this result, he begins the opposite process. That is, starting from a force of gravitation falling as $1/r^2$, he derives Kepler's laws. One example (Newton, 1934, p. 420):

> Proposition XIII. Theorem XIII. *The planets move in ellipses which have their common focus in the centre of the sun; and, by radii drawn to that centre, they describe areas proportional to the times of description.*
>
> We have discoursed above on these motion from the Phenomena. Now that we know the principles on which they depend, from those principles we deduce the motions of the heavens *a priori*.

But beyond this, Newton derived a whole set of new results beginning from a force of gravitation proportional to the product of the interacting masses and falling as the inverse square of the distance between them. As an example we have his Prop. XIX, Problem III: "*To find the proportion of the axis of a planet to the diameters perpendicular thereto.*" That is, he calculated the flattening of the planets at their poles. He also derived from his law of gravitation the motion of the Moon around the Earth. In Prop. XXIV, Theorem XIX, he began the explanation of a whole new set of phenomena based on the gravitational attraction, namely, [3, p. 435]: "*That the flux and reflux of the sea arise from the actions of the sun and moon.*" Another result which he could explain appears in Prop. XXXIX, Problem XX: "*To find the precession of the equinoxes.*" In the next few propositions he derived the motion of the comets around the Sun.

In essence, Newton began with Kepler's laws of planetary motion in order to derive his law of universal gravitation. He then applied this law to deduce Kepler's laws and a whole series of new phenomena.

3.5 Inverse Problems in Philosophy

In the last Query of the *Optics* Newton presented his general view on how to proceed in natural philosophy (Newton, 1979, pp. 404–405):

> As in Mathematicks, so in Natural Philosophy, the Investigation of difficult Things by the Method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phaenomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such Exceptions as occur. By this way of Analysis we may proceed from Compounds to Ingredients, and from Motions to the Forces producing them, and from particular Causes to more general ones, till the Argument end in the most general. This is the Method of Analysis: And the Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phaenomena proceeding from them, and proving the Explanations.
>
> In the first two Books of these Opticks, I proceeded by this Analysis to discover and prove the original Differences of the Rays of Light in respect of Refrangibility, Reflexibility, and Colour, and their alternate Fits of easy Reflexion and easy Transmission, and the Properties of Bodies, both opake and pellucid, on which their Reflexions and

> Colours depend. And these Discoveries being proved, may be assumed in the Method of Composition for explaining the Phaenomena arising from them: An Instance of which Method I gave in the End of the first Book.

Newton formalized his general approach in science at the Preface of the first edition of his *Principia* (Newton, 1934, p. xvii):

> I offer this work as the mathematical principles of philosophy, for the whole burden of philosophy seems to consist in this—from the phenomena of motions to investigate the forces of nature, and then from these forces to demonstrate the other phenomena; and to this end the general propositions in the first and second Books are directed. In the third Book I give an example of this in the explication of the System of the World; for by the propositions mathematically demonstrated in the former Books, in the third I derive from the celestial phenomena the forces of gravity with which bodies tend to the sun and the several planets. Then from these forces, by other propositions which are also mathematical, I deduce the motions of the planets, the comets, the moon, and the sea.

At the beginning of the third book of the *Principia* he presented a similar approach, namely (Newton, 1934, p. 397):

> In the preceding books I have laid down the principles of philosophy, principles not philosophical but mathematical; such, namely, as we may build our reasonings upon in philosophical inquiries. These principles are the laws and conditions of certain motions, and powers or forces, which chiefly have respect to philosophy; but, lest they should have appeared of themselves dry and barren, I have illustrated them here and there with some philosophical scholiums, giving an account of such things as are of more general nature, and which philosophy seems chiefly to be founded on; such as the density and resistance of bodies, spaces void of all bodies, and the motion of light and sounds. It remains that, from the same principles, I now demonstrate the frame of the System of the World.

3.6 Conclusion

In this work we have shown how Newton always considered inverse aspects in all branches of knowledge. This includes mathematics, mechanics, optics and philosophy. This certainly was one of the main sources of his powerful creativity in science.

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Chapter 4
Isaac Newton, Robert Hooke and the Mystery of the Orbit

Penha Maria Cardoso Dias and Teresinha J. Stuchi

4.1 Introduction

In the seventeenth century, René Descartes conceived a description of the circular motion that was influential. He recognizes that a rock in a rotating sling has a "tendency" to fly away from the center of the rotation circle, although he never produced an expression for this "tendency". In a modern paraphrasis: Descartes decomposes the inertial motion (abstracting the gravitational field, of course) of the stone after leaving the sling, in a radial component and an angular component;[1] circular motion occurs, when the radial component is "constrained" by the surrounding disposition of matter (*Le Monde*, p.46). According to this description, the contemporary reading of the law of inertia was (George Smith, 2002, p.148–149): circular motion occurs when an already existing "centrifugal tendency" is somehow "constrained", and does not require an external entity (such as the modern force). Christiaan Huygens coined the locution *vis centrifuga*, and found its mathematical expression: he stated a series of theorems published in *Horologium Oscillatorium* (1673), but their proofs were published only posthumously, in *De Vi Centrifuga* (1703). In 1664–1665, Isaac Newton presented a calculation of the *conatus recedendi à centro* (*Waste Book*, in: John Herivel, p.129–132); the calculation was then formulated in a way conceptually different from Huygens's. Later, in 1669, Newton gave a different calculation of the *conatus* (*Waste Book*, in: Herivel, p.192–198); the new calculation has similarities with Huygens's proof, but was conceived independently. Only in the *Philosophiæ Naturalis Principia Mathematica* (1686) did Newton introduce the name "centripetal force", and the expression (modern notation) centripetal force $\propto \frac{v^2}{r}$.

P.M.C. Dias (✉)
Instituto de Física, Universidade Federal do Rio de Janeiro, Rio de Janeiro, Brazil
e-mail: penha@if.ufrj.br

[1] So that $\vec{v}_0 = v_r\,\hat{r} + v_\theta\,\hat{\theta}$; $\vec{v}_0 =$ constant.

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A question that might be asked is: how and why did Newton realize that the deviation from a linear motion required a *centripetal force* instead of a centrifugal *conatus*? Isaac Bernard Cohen proposes (1980, p.249):

> We have seen that both the Huygenian concept and name had been transformed by Newton from "centrifugal" to "centripetal". When did this transformation occur? Was [Robert] Hooke's primary contribution to suggest to Newton that planetary motions should be compounded of a linear inertial component and the effects of a force directed *toward* the sun?

Cohen bases his thesis partly on an exchange of letters between Hooke and Newton that took place between the end of 1679 and the beginning of 1680 (Cohen, 1981, p.178): "[t]he correspondence between [Robert] Hooke and Newton clearly shows that Hooke taught Newton how to analyze curved motion". On November 24, 1679, Hooke wrote to Newton, inviting him to comment on the following method (H. Turnbull, 1960, p.298): "[. . .] compounding the celestiall motions of the planetts of a direct motion by the tangent & an attractive motion towards the centrall body [. . .]"; the case rests on the similarity between Newton's proof of Johann Kepler's "law of areas" (*Principia*, Book I, proposition 1), and Hooke's above mentioned composition of motions. The other part of Cohen's argument consists of historiographic considerations. Commenting on the opinion that Newton discovered the universal gravitation in the 1660s (Cohen, 1980, p.248–250), Cohen argues that there is no evidence that Newton thought in terms of a centripetal attraction between the Earth and the Moon prior to the *Principia*;[2] because Hooke's influence is most conspicuous in the proof of the "law of areas", it is relevant that (Cohen, 1980, p.250) "prior to the correspondence with Hooke in 1679–1680, the second law [of Kepler] was not part of Newton's conscious armory of astronomical principles". Bernard Cohen's thesis seems to be supported by an independent result stated by Derek T. Whiteside (1991, p.20). According to Whiteside, prior to the *Principia*, Newton based the explanation of the motion of the planets on a theory formulated by Giovanni Alfonso Borelli. Borelli explains the motion of a satellite around its planet as the result of the composition of (Alexander Koyré, 1961, p.461–506): the attraction by the planet and the centrifugal motion that originates in the rotation of the satellite.[3] Although Cohen's thesis is supported by convincing historiographic evidences, this is not a subject on which a consensus has been reached. Furthermore,

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[2]We shall not discuss the debate between Hooke and Newton concerning the discovery of universal gravitation. The relevant point to us is that following the "legend of the apple", those who minimize the import of Hooke in the shaping of Newton's later thought in mechanics place Newton's discovery in the 1660s; therefore he would have to have a correct understanding of centripetal forces by the mid 1660s.

[3]To explain the orbit, Borelli makes an analogy with a vertical cylinder that floats in a liquid (Koyré, 1961, p.497–498): if the cylinder is let down from a distance above the surface of the fluid, it sinks, and as it sinks the buoyant force (the *vis centrifuga*, in the analogy) increases till it overcomes the weight (attraction), and the cylinder starts to rise; if there is no loss of motion, the motion goes on forever. The orbit is described by the up and down oscillations of the satellite (cylinder) in a (perhaps cartesian) fluid.

in the introduction to his translation of the *Principia* (henceforth called *A Guide*), Cohen weakens his initial claim (*A Guide*, 1999, p.77):

> What Hooke did for Newton, therefore, was not to tell him how to analyze curved motion into components, but rather to reverse the direction of his concept of displacement in orbital motion, to shift from an outward to an inward displacement.

The change of view has to do with the following (Cohen, *A Guide*, p.75, n.85): "[. . .] research of reconstruction and analysis [of physical orbits] has produced a new interpretation of the development of Newton's methods of dynamics in the years before the *Principia*". The research to which Cohen refers is: Michael Nauenberg (1994) claims that Newton had a process to draw the orbit (in modern words) of a body falling under a constant attractive central force; there are two crucial points in the claim: first, the process demands knowledge that the radius of curvature is proportional to the centripetal force, and, second, the specific orbit on which Nauenberg bases his claim was drawn by Newton in his second letter to Hooke (November 24, 1679), hence before the *Principia*. The conclusion is that Newton had already formulated dynamic principles before he wrote the book, even if the principles remained undisclosed. Although the considerations by Nauenberg do not entirely eliminate Hooke's influence, this influence is weakened in the direction pointed by Cohen. However how Newton drew the above mentioned orbit is controversial: Herman Erlichson (1990) argues that Newton could have drawn the orbit using Hooke's method, already known by him since Hooke's first letter. Furthermore, although Cohen changed his initial opinion, he kept a guarded opinion on the new research (*A Guide*, p.75, n.85): "[t]his reconstruction explains a number of aspects of the development of Newton's thought but does have some gaps. For example, Nauenberg must assume that documents some years apart [1664–1665 and 1679] refer to the identical methods". The focus of the discussion has been thereof shifted, and it has become relevant whether before the *Principia* (preferably in the 1660s) Newton understood the physical role of the curvature in drawing physical orbits; in different words, if Newton knew at that time that centripetal force was (proportional to) the curvature.

Bernard Cohen rests his case on the "law of areas". But it might be asked if also the concept of (centripetal) force is reminiscent of Hooke's method. In section 3, we show that the distinctive characteristic of the concept of force as it stands in the *Principia* is: the centripetal force is (proportional to) the acceleration in a "motion of free fall" from the tangent to the osculating circle along a line through the center of the circle; likewise, the central force is a "motion of free fall" but along the radius vector through the center of force. The consequence is that the centripetal component of a force is inversely proportional to the radius of curvature. The association of a dynamic concept (force) with a geometric concept (curvature) seems reminiscent of Hooke's "attractive motion towards the centrall body": it is a physical motion — a "free fall" motion — that "brings the tangent" to match the osculating circle (the orbit). What is difficult to prove in lack of more historiographic evidences is that Newton did not know it before his interaction with Hooke (November 24, 1679).

In section 4, we argue that if Newton knew the method of compounding the two motions independently and prior to Hooke, he missed a good chance to state the method in his first calculation of the *conatus* (1664–1665): the uniform circular motion is decomposed in the same two motions considered by Hooke; but the *conatus* on the ball is not treated as a "motion of fall", which indicates that there is not an association of the force with the curvature. In the second calculation of the *conatus* (1669), the idea of a "motion of fall" is found in the use of Galileo's theorem for the fall; but again the association with the curvature is missing. The overlook is more interesting insofar Newton generalizes the result to the calculation of the "force" (*conatus*?) on an ellipse (*Waste Book*, in: Herivel, p.130); although this has been taken (Nauenberg, p.227) as an indication that Newton had the concept of centripetal force, the conflict with historiographic evidences (Cohen, *A Guide*; Whiteside, 1991) has been the center of Cohen's guarded opinion, quoted above.

As said above, Nauenberg brought new contributions to the debate on the extent of Hooke's influence on Newton. In section 6, we analyze the specific orbit that has generated the whole debate, but in order to state specific claims, some historical background is needed.

Hooke's method is stated in the very first letter to Newton (November 24, 1679). Instead of commenting on Hooke's method, Newton (November 28, 1679) resuscitated an old problem: to find the trajectory of a body that is let to fall from the top of a tower, if the Earth were permeable and the body could keep falling. The problem was proposed in the antiquity. It is associated with the discussion of whether it is possible or not to assign a diurnal rotation to the Earth. According to a point of view held by greek and medieval authors, if the Earth moves from west to east, a body released from rest from the top of a tower on its west side falls far from the bottom of the tower: while in flight, the body does not partake in the eastward motion of the Earth, so that while it falls the Earth moves to east (Koyré, 1973; Turnbull); however the body is observed to fall at the bottom of the tower, meaning that the Earth is immobile. In order to be able to assign a motion to the Earth, Galileo made the assumption that the body, the tower, and everything on top of the Earth share with the Earth the (instantaneous) linear speed. But the problem continued to be discussed in a different context, within the next hundred or so years, by Galileo, Johann Kepler, Pierre Mersenne, Pierre de Fermat, Giambattista Riccioli, Stefano degli Angeli, Giovanni Alfonso Borelli, James Gregory, etc. (Koyré, 1973); a great difficulty (among many) for those authors was the law of inertia (Koyré, 1973): Galileo and Borelli, for instance, considered the uniform circular motion of the Earth to be an inertial motion. Newton understood that from the solution of the problem it was possible to prove that the Earth moved (Turnbull, p.301): "[. . .] I shall communicate to you a fansy of my own about discovering the earth's diurnal motion"; the (linear) speed at the top of the tower is bigger than the speed at its bottom on the surface of the Earth (because the angular speed is the same), therefore there is a relative speed between top and bottom, and the motion of the Earth is inferred from the fact that the body reaches the ground far from the tower.

Newton's first solution (November 28, 1679) looks like a spiral (Fig. 4.1), a solution that had already been dismissed by philosophers, more than one century before

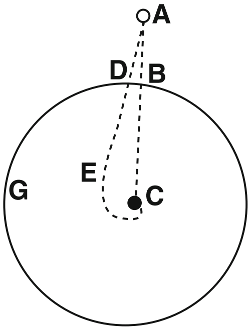

Fig. 4.1 The spiral. Drawing by Newton's hand (Turnbull, p.301). Note that the body falls close to the tower

(Koyré, 1973). In his answer (December 9, 1679), Hooke proposes that the solution would be (Turnbull, p.305; B. Cohen, 1981) "nothing att all akin to a spirall but rather a kind Elleptueid"; he is clearly considering the motion of the planets, as he explains in a later letter (Turnbull, p.309; Cohen, 1981): "[. . .] the Attraction always is in a duplicate proportion to the Distance from the Center Reciprocall [. . .]". Then Newton presents a second solution (December 13, 1679) to his problem.

Newton's second solution (Fig. 4.2) raised a discussion in the history of physics (Jean Pelseneer, 1929; Johs. Lohne, 1960; Koyré, 1968): it looks like the correct solution (Fig. 4.3), but Newton does not explain how he obtained the curve; furthermore the angle of the pericenter in Newton's drawing is much larger than the analytically calculated maximum value for the angle of the pericenter. Pelseneer (p.252) proposes that in spite of Newton's knowledge of central motions, he did

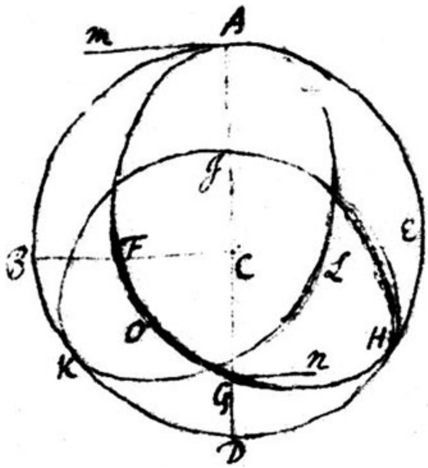

Fig. 4.2 Drawing by Newton's hand (Lohne)

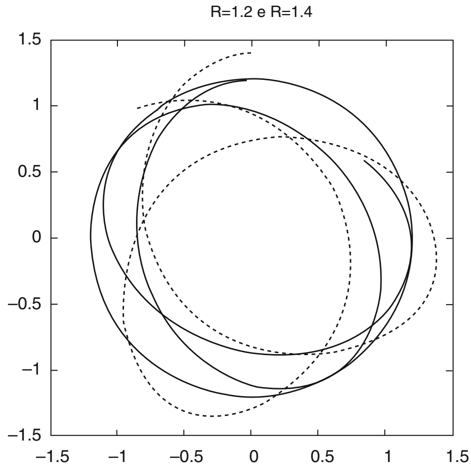

Fig. 4.3 Solution. The curves in full line and in dotted line are found by *RK*4; they correspond to different "heights of fall"

not use an analytical method to draw the curve; contrariwise, Lohne proposes that Newton could have used analytical methods to find the orbit. The discussion has been revived (Erlichson, 1990; Nauenberg, 1994). Erlichson drew the orbit by Hooke's method; the curve looks like Newton's up to some point (*F*, in Fig. 4.2). Nauenberg proposes that when Newton wrote to Hooke (prior to the *Principia*), he already had a method to draw orbits different from Hooke's; the error in the position of the pericenter is due to an error in the drawing, when Newton projected the curve around its symmetry axis, and it is not an error in the method of solution. The point is that the method proposed by Nauenberg depends on the knowledge that the centripetal force is proportional to the curvature.

Hooke's method can be recognized as a straightforward sympletic Euler method (second order) (from now on, called *SE*) of numerical computation of hamiltonian

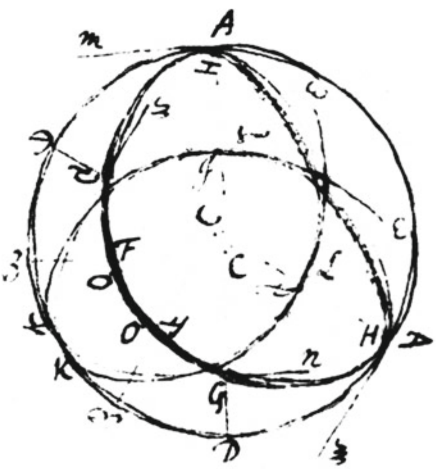

Fig. 4.4 The reflection. Segment *AF* is reflected in the lower hemisphere. The center is moved, because the radii in the upper and in the lower hemisphere are not equal

equations; Nauenberg's is an application of the simple (first order) Euler method (from now on, called *E*), as Nauenberg recognizes (1994, p.242). In section 6, we draw the orbit using the fourth order Runge-Kutta method (from now on, called *RK*4). The orbits obtained by *SE* and *RK*4 are very close to each other (Fig. 4.5, right); the orbits obtained by *E* and *RK*4 (Fig. 4.5, left) agree up to a point a little past the horizontal axis. Therefore as long as the only argument (historiography notwithstanding) is agreement with Newton's orbit up to a supposed point of projection (symmetry), Newton could have used either method, Nauenberg/*E* or Erlichson/Hooke/*SE*. The position of the pericenter is a different problem; we argue that if Newton used a method equivalent to the *E* method to draw the orbit up to the pericenter so that he could reflect it around the line between the center and the pericenter (the symmetry axis), the tendency would be to rise the pericenter, not to lower it, as in Newton's drawing. The agreement of the reflected upper segment of the curve in Newton's hand-made drawing with the segment in the lower hemisphere is a support for the hypothesis that the latter was indeed obtained by reflection of the former; the center is also dislocated in the same direction as in Nauenberg's hypothesis.

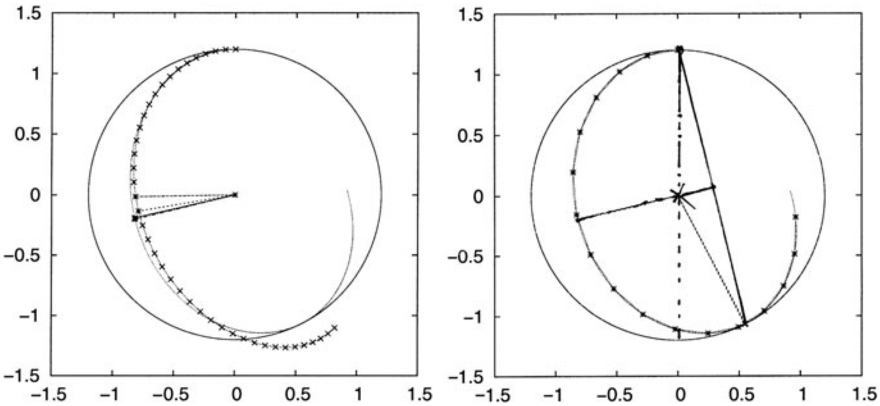

Fig. 4.5 Comparison of methods. In both figures, the curve in full line is obtained by *RK*4. The dotted line is obtained: on the left, by the *E* method; on the right, by the *SE* method. The inscribing circle is also shown. The lines joining the respective pericenters to the center are also shown

4.2 Hooke's Method

Hooke's method is illustred in Fig. 4.6 (middle) below. If a body is at *A*, it has a "direct motion by the tangent", and goes to *B* in an infinitesimal time Δt. By the law of inertia, the body goes to *c* in an equal time Δt, $AB = Bc$. However the body receives at *B* "an attractive motion towards the centrall body" (*S*). The new "motion" is *AV*; in modern vectorial notation, $\vec{AV} = \vec{AB} + \vec{BV}$. Drawing the parallelogram *AVCB*: $BC \parallel AV$, $BC = AV$. Then the body moves in Δt from *B* to *C*, instead of

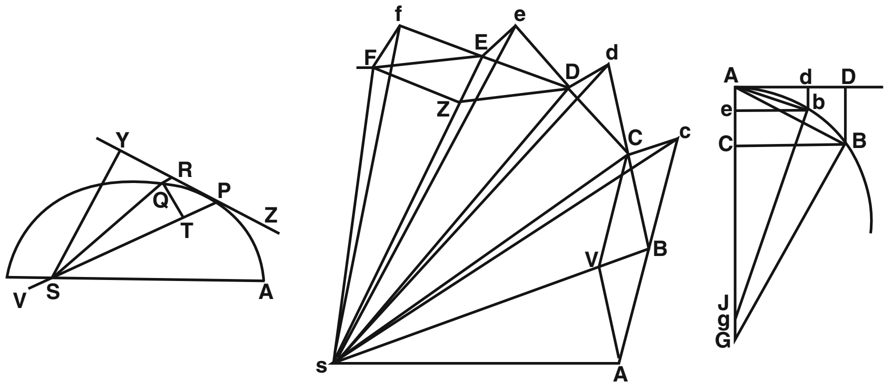

Fig. 4.6 Foundations of dynamics. On the right: the osculating circle. On the middle: proof of the law of areas, and Hooke's method. On the left: the direct problem (find the force, when the orbit is given); $S =$ center of force; $RQ =$ central force

moving to c; in vectorial notation: $\vec{BC} = \vec{AB} + \vec{BV}$. The orbit is drawn out of two "motions", (1) a "motion" AB along the tangent: $\vec{AB} = m\vec{v}_{AB}$, and (2) an "attractive motion" BV from the tangent to the curve along the radius vector: $\vec{BV} = \Delta\,(m\vec{v}_{AB})$. The method indicates that a "pull", BV, towards the center of force is needed to deviate the body from its inertial motion along the tangent to the orbit.

4.3 The Concept of Force in the "Principia"

The foundations of point mass dynamics are found in propositions 1 to 41 in the *Principia*:

1. The circle in Fig. 4.6 (right) is the *osculating circle* (radius R). Some geometric propositions are stated:

   (i) $\overline{BD} \propto \frac{\overline{AB}^2}{2R}$, where $\overline{AB}$ is the chord.
   (ii) In the limit $B \to A$, BD can be taken with any angle with the tangent.
   (iii) *Sagitta* (arrow) (Cohen, *A Guide*, p.307): the word is applied to a particular arrow. In the general case, it is the segment of the line from the *center of force* to the middle of the chord AB (or of the arc $\widetilde{AB}$). In the particular case of the *osculating circle*, the "center of force" is the center of the circle, and the *sagitta* is perpendicular to AB. Then: saggita $\propto \overline{BD} \propto \frac{\overline{AB}^2}{2R}$.

2. In corollary 3, it is given a kinematical definition to the geometric *sagitta*. In the limit, chord $\overline{AB} \approx$ arc $\widetilde{AB} \approx v\Delta t$; then: saggita $\propto \frac{\overline{AB}^2}{2R} \approx \frac{(v\Delta t)^2}{2R} \equiv \frac{1}{2}\frac{v^2}{R}\,(\Delta t)^2$.
3. Δt is given by the area swept by the radius vector (*law of areas* for central forces, proposition 1, Fig. 4.6, middle).

4. By definition, centripetal force $\propto$ sagitta; or, recalling item 1 above, the proportionality is better defined as: centripetal force $\propto \frac{\text{sagitta}}{(\Delta t)^2} \propto \frac{v^2}{R}$.[4]
5. In the general *central* motion case, the *sagitta* is $\frac{BV}{2}$ (it bisects AC) (Fig. 4.6, middle). Lemma 10 or proposition 6 can be understood as a proof that the theorems of "free fall" (in modern notation: $v^2 = 2gh$ and $h = \frac{1}{2}gt^2$) can be applied to each instant separately; they can be paraphrased: area of triangle $ADB = \frac{1}{2} \times AD \times DB$ or $\Delta s = \frac{1}{2}(\Delta v) \times (\Delta t) \equiv \frac{1}{2}\mathcal{G}(\Delta t)^2$. Calling the distance *sagitta*, *central force* is better defined as: central force $\propto \mathcal{G} \propto \frac{\text{sagitta}}{(\Delta t)^2}$, where $\mathcal{G}$ is the acceleration of the fall from the tangent to the curve, along the line to the center of force.
6. Then Newton is ready to solve the direct problem, and the inverse problem. The *direct problem* consists in proving that if the orbit is an ellipse, the central force is $\propto \frac{1}{r^2}$; from geometric properties of the ellipse together with a geometric definition of the *central force* (corollary 1 of proposition 6) (Fig. 4.6, left) it is proved: central force $\propto \left\{\frac{[(SP)\times(QT)]^2}{QR}\right\}^{-1}$, where the numerator is the time squared, given by the square of the area of the triangle SPQ, and the denominator is the *sagitta*. The *inverse problem* consists in finding the orbit for a given *central force*. The integration is done geometrically, not analitically.

In order to build the concept of "force", the orbit is taken at each instant separately. The instantaneous value of the force is measured by the "acceleration in a free fall" from the tangential inertial motion to the center of force (case of central motion) or to the center of the osculating circle (case of centripetal force); in particular, the centripetal component of the force is (proportional to) the curvature. In characterizing the "centripetal force", Newton takes into consideration the "would-be" tangential inertial motion, and a "free fall motion" that deviates the body from the inertial motion, and nothing else; but this is Hooke's decomposition of the motion! This follows from the following structure found in Book I:

Geometric definition of 'force'. It is the arrow BV (proposition 1, corollary 3, corollary 4; proposition 4).

Physical definition of 'force'. It is (proportional to) the acceleration of "free fall" from the tangent to the center of the osculating circle, in the case of the centripetal force (lemma 11; proposition 4), or from the tangent to the center of force along the radius vector centered at the center of force, in the case of central force (lemma 10; proposition 6).

Mathematical calculation of the force. The force is found using the theorem of "free fall" (proposition 4; proposition 6, corollary 1). This needs:

[4] Newton uses the word 'centripetal' for both central and centripetal forces. Because the modern reader is used to two different words, this may be confusing sometimes; clearly, Newton is not confused, whenever he considers one or the other.

1. Geometric theorems that justify the use of the theorems for the "free fall" at each instant taken separately (lemma 10; lemma 11).
2. Calculation of the time of "fall" (proposition 1; proposition 6, corollary 1)

Function of the centripetal component of the force. In the conceptual structure of the *Principia*, the curvature has a physical meaning; the centripetal force causes a "fall" that bends the tangent line into the osculating circle, hence causing the curvature. This is used in the proof (not shown here) of proposition 40.

4.4 The "conatus recedendi à centro"

4.4.1 1664–1665

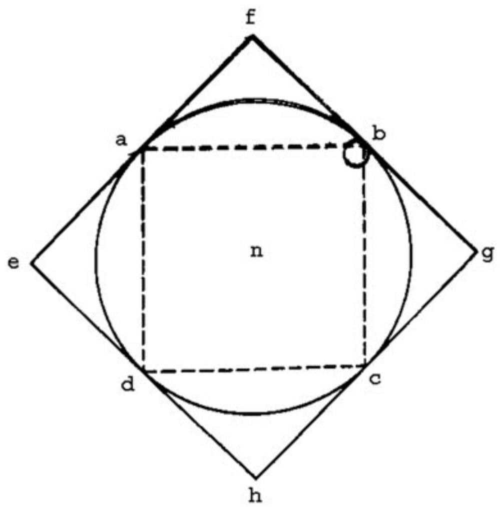

Fig. 4.7 Calculation of the "conatus" (1664–1665). A small sphere hits the circle, describing the square *abcd*

In the above figure, a small sphere (b) moves inside a circle. The sphere collides with the circle, rebounds, collides again, and so forth, so that it describes the inscribed square *abcd*. Then Newton shows that:[5]

$$\frac{\text{“pression” (his words) of the sphere on the circle in a complete turn}}{\text{“force of motion”}} = \frac{\text{perimeter of the circle}}{\text{radius of the circle}}.$$

[5]**Proof**. By definition, "force of motion" $\propto$ ab and "pression" $\propto$ $\frac{db}{2}$ $\perp$ fg. From Fig. 4.7: triangle abd $\sim$ triangle $afb \Longrightarrow \frac{2fa}{ab} = \frac{ab}{fa}$. Or: $\frac{\text{“pression” of the sphere}}{\text{“force of motion”}} = \frac{\text{side } (=ab)}{\text{radius } (=fa)} \xrightarrow{\text{after 4 collisions}} \frac{\text{perimeter}}{\text{radius } (r)} \xrightarrow{\text{number of sides} \to \infty} \frac{C}{r} \equiv \frac{2\pi r}{r} = 2\pi$.

With the benefit of hindsight, this is: $\frac{\Delta(mv)}{mv} = 2\pi$.[6] In modern notation, $mv = ab = bc$ and $\Delta(mv) = bn$; then considering the triangle abd: $ab + bd = ad$ or $m\vec{v}_{ab} + \Delta(m\vec{v}_{ab}) = m\vec{v}_{bd}$. The sides of the triangle abd in Fig. 4.7 have the same meaning of the sides of triangle ABV in Fig. 4.6 (or Fig. 4.6, middle): $AB = m\vec{v}_{AB}$, $BV = \Delta(m\vec{v}_{AB})$ and $AV = BC = m\vec{v}_{BC}$. However the triangle $abcd$ is not oriented in the circle in a way that discloses the composition of motion, as is the triangle ABV. More important, the "pression" $bn \equiv \Delta(mv)$ is not made proportional to the curvature. Furthermore, the "pression" seems to be a centrifugal *conatus*: it is from the inside to the outside, since the ball strikes the circle on its inner side. The similarity between the two methods (Hooke's and Newton's) in this calculation cannot be taken as evidence that Newton knew how to treat physical orbits.

Newton generalizes this calculation (*Waste Book*, in: Herivel, p.130): "If the body b moved in an Ellipsis that its force in each point (if its motion in that point be given) [will?] bee found by a tangent circle of Equall crookednesse with that point of the Ellipsis." In (Nauenberg, 1994, p.227) and in (Nauenberg, in: *A Guide*, p.80) it is claimed that the sentence is mistranslated, and that it must be: "[i]f the body b moves in an Ellipsis, then its force in each point (if its motion in that point be given) may be found by a tangent circle of equall crookedness with that point of the Ellipsis"; therefore (Nauenberg, in: *A Guide*, p.80): "[t]his remark indicates that Newton considered the extension to elliptic orbits of the rule that he (and Huygens) had found for circular motion". The desired conclusion is that Newton considers in this problem a centripetal force; in fact, whatever the correct translation, the context indicates that Newton generalizes the case of the circle to the case of an ellipse; however, the value of the *conatus* is the same, regardless its direction (whether centripetal or centrifugal). Whiteside interprets the calculation as follows (1991, p.20):

> Is the 'centre-fleeing force' [Newton] here posits precisely Huygens's circular *vis centrifuga*? Or is it just a convenient tag for other ill-defined outward 'push', the only demand upon which is that it shall continuously counteract an equally unspecified inwards 'gravitation' to produce the desired elliptical orbits of the planets? If either, was the idea original with him? And, most important of all, what at any [time] up to the early 1680s could he have done to pluck mathematical fruit from it?[7]

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[6] If τ is the period, r is the radius and v is the speed, then $\frac{\Delta(mv)}{\tau} = \frac{2\pi(mv)}{\tau} = m\left(\frac{2\pi r}{\tau}\right)\frac{v}{r} \equiv m\times\left(\frac{v^2}{r}\right)$.

[7] Newton finds another consequence of the "endeavour of rescending" from (not toward) the Sun (*Waste Book*, in: Herivel, p.197): "Finally since in the primary planets the cubes of their distancs from the Sun are reciprocally as the squares of the numbers of revolutions in a given time: the endeavours of receding from the Sun will be reciprocally as the squares of the distances from the Sun" or, in modern language, $g \propto (2r) \times v^2 \propto (2r) \times \frac{1}{r^3} \propto \frac{1}{r^2}$. Some people interpret the calculation as an indication that Newton extends to the moon the gravitation fall on the surface of the Earth, and that by 1669 Newton knew the "universal gravitation". This need not be so, as shown by Cohen (1981).

4.4.2 1669

In the above figure, a body moves on a circle with uniform speed v. In time Δt, the body moves the arc $\widetilde{AD}$. If the body leaves the circle at A, it moves an equal distance on the tangent. For small times, $\overline{AB} \approx \widetilde{AD} = v\Delta t$. The radial distance away from the circle is $\overline{BD}$. In a time equal to the period of the circular motion (τ), the body moves the whole circumference ($\mathcal{C} = 2\pi r$). Hence: $\frac{\overline{AB}}{\mathcal{C}} = \frac{t}{\tau}$. Then Newton poses the problem: find the distance x such that $\frac{\overline{BD}}{x} = \frac{t^2}{\tau^2} = \frac{\left(\overline{AB}\right)^2}{\mathcal{C}^2}$.[8]

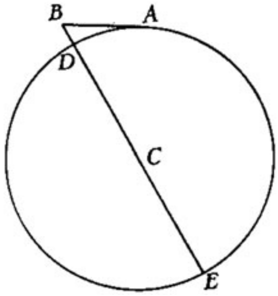

Fig. 4.8 Calculation of the "conatus" (1669). It is (proportional to) the acceleration in moving upward the distance *BD*

As a consequence (*Waste Book*, in: Herivel, p.196):

> Hence the endeavours from the centers of divers circles are as the diameters divided by the squares of the periodic times or as the diameters multiplied by the [squares of the] numbers of revolution made in any given time.[9]

The calculation bears no similarity with Hooke's decomposition of motion, although use of Galileo's theorem indicates that there is a "motion of free fall" along *BD*.[10] However the "fall" is not associated with the radius of curvature of the circle, which indicates that the dynamic structure later found in the *Principia* is not disclosed.

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[8]**Proof**. From the geometry of the circle: $\left(\overline{AB}\right)^2 = \left(\overline{BD}\right) \times \left(\overline{BE}\right)$. For a small arc $\widetilde{AD}$, it can be considered that $\overline{BE} \approx \overline{DE} = 2r \implies \left(\overline{AB}\right)^2 \approx \left(\overline{BD}\right) \times \left(\overline{DE}\right)$. Hence: $\frac{\left(\overline{AB}\right)^2}{\mathcal{C}^2} \approx \frac{\left(\overline{BD}\right)\times\left(\overline{DE}\right)}{\mathcal{C}^2} \equiv \frac{\left(\overline{BD}\right)}{\frac{\mathcal{C}^2}{\left(\overline{DE}\right)}} \implies x \equiv \frac{\mathcal{C}^2}{\left(\overline{DE}\right)}$. In modern terms, $x \equiv \frac{\mathcal{C}^2}{\left(\overline{DE}\right)} \approx \frac{(v\tau)^2}{2r} = \frac{1}{2}\left(\frac{v^2}{r}\right)\tau^2 \implies \overline{BD} \approx x \times \left(\frac{t}{\tau}\right)^2 = \frac{1}{2}\left(\frac{v^2}{r}\right)t^2$.

[9]$x = \frac{\mathcal{C}^2}{2r} = \pi^2\,(2r)$, hence $g \propto \frac{x}{\tau^2} \propto \frac{2r}{\tau^2} \propto (2r) \times \nu^2$, where ν is the frequency.

[10]This calculation has similarities with Huygens's calculation of the *vis centrifuga*. Huygens makes an analogy between the centrifugal force and the weight, based on a physical system: he considers an observer standing on the rim, at the top of a rotating vertical wheel, holding a thread at the bottom of which hangs a small sphere; for this observer the weight of the sphere is balanced by the *vis centrifuga* — which is correct! Huygens uses this result to invoke the expressions for the "free fall", $v^2 = 2gh$ and $h = \frac{1}{2}gt^2$; then he can prove a series of theorems that together mean: $\text{centrifugal force} \propto \frac{v^2}{r}$.

4.5 The Problem Proposed by Newton to Hooke

The problem can be paraphrased, in modern words and concepts, as the problem of a mass that moves attracted to a center of force by a central constant force. The conserved quantities are the mechanical energy (E) and the angular momentum (L); the equations for the polar coordinates (r and ϕ) are:

$$\dot{r} = \sqrt{\frac{2}{m}}\sqrt{E - V_{\text{eff}}} \qquad \dot{\phi} = \frac{L}{mr^2} \qquad V_{\text{eff}} = \frac{L^2}{2mr^2} + mgr \equiv mg\left(\frac{r_0^3}{2r^2} + r\right) = \text{effective potential}$$

where $r_0 = \left(\frac{L^2}{m^2 g}\right)^{\frac{1}{3}}$ = radius of the circular orbit. The orbit in Fig. 4.3 can be obtained by integration: $\int_R^r \frac{dr}{r^2\sqrt{\frac{E}{mg} - \frac{r_0^3}{2r^2} - r}} = \sqrt{\frac{2}{r_0^3}} \times \int_0^\phi d\phi$, where R is the "height of fall" (radius of the inscribing circle). Erlichson numerically solves this integral by the method of Gaussian quadrature (p.732).

4.5.1 The Spiral (November 28, 1679)

Newton's first solution to the problem is the spiral in Fig. 4.1. He could not have found a spiral, had he used a correct method to draw the curve, because the spiral does not even hint at the solution to the constant force problem (Fig. 4.3 and 4.2). However there is an objection against this interpretation. The force $F = -\frac{k}{r^3}$ $(k > 0)$ has two solutions in the form of spiral;[11] Newton (Whiteside, *Papers*, vol. VI, p.153) considered the problem of motion under this force. Therefore, it is claimed that Newton could have obtained a spiral by applying his method to a force $\propto -\frac{1}{r^3}$ (Fig. 9 in: Nauenberg, 2004, p.239); the argument is that Newton could not have known that the curve would reach the center after an infinite number of revolutions, unless he had a process to draw it. In a footnote, Cohen again shows skepticism (*A Guide*, p.76, n.86):

> Nauenberg has made an attractive reconstruction, based on two major assumptions for which there is no direct evidence. One is that Newton knew from his analysis of the logarithmic spiral that the curve would eventually reach the center. The other is that he purposely concealed or did not specify the power of the distance in the expression for the force producing the spiral.

There are other arguments that indicate that Newton did not have a complete concept of centripetal forces. Whiteside calls attention to a feature of the drawing made by Newton (Whiteside, 1964, p.132, n.52):

> Newton's figure, in which *ADEC* is the line of free fall, is drawn from the viewpoint of an observer rotating with the earth, and this has misled several historians. H. W. Turnbull, for example, takes Hooke to task for suggesting in his reply that Newton had proposed

[11] If r and ϕ denote the usual polar coordinates, the solutions in spiral are: (1) $r\phi = \alpha$, $\dot{\phi} \neq$ constant; (2) $\frac{1}{r} = \frac{1}{r_0}\cosh\left[\beta\left(\phi - \phi_0\right)\right]$, where α, β, r_0, ϕ_0 are constants.

> the falling body would spiral round the earth's centre several times before reaching it: 'According to Newton's figure there is one revolution [only]' [...]. [I]t is clear both that the path *ADE* was drawn tangent to *ABC* (as it should be) and that Newton did not continue his spiral quite all the way to the earth's centre. It is worthwhile to notice that Newton rejected implicitly the Fermatian hypothesis of unchanged uniform angular rotation (which would make the path *ADE* fall wholly in the line *ABC*).

This points to a possible conceptual error by Newton, just in case he is considering the force $-\frac{k}{r^3}$. According to Whiteside, the drawing suggests that Newton believes the spiral to be the solution seen by an observer on the Earth, hence on a rotating referential; this interpretation is reinforced by the fact already mentioned in the introduction that Newton's motivation was to probe the motion of the Earth by a person on the Earth, hence he had to consider the motion seen by an observer on the Earth; but the spiral solutions for the force $-\frac{k}{r^3}$ are orbits seen by an "absolute observer" (meaning, outside the Earth). The conflation of referential frames would not remain unnoticed, had Newton already translated curvature in terms of centripetal force: the "absolute" spiral demands a centripetal force acting on the body, while in the motion considered in the rotating frame of the Earth there should be a centrifugal force on the body to make the orbit straight (it is $\propto r\dot{\theta}^2$); in propositions 43 and 44 in Book I of the *Principia*, Newton correctly describes the forces on a body in an orbit on a rotating plane. Finally, in his reply to Hooke (December 13, 1679) Newton reiterates that the problem proposed by him is the problem of a constant force (Turnbull, p.307) (hence, not the problem of a force $\frac{1}{r^3}$): "[a]nd also that if its gravity be supposed uniform it will not descend in a spiral to ye very center [...]".

4.5.2 The Mysterious Orbit (December 13, 1679)

Newton does not explain how he obtained the orbit in Fig. 4.2 above, except for two cryptic remarks. The first remark is (Turnbull, p.307):

> [...] if its gravity be supposed uniform it will not descend in a spiral to ye very center but circulate wth an alternate ascent & descent made by it's *vis centrifuga* & gravity alternately overballancing one another.

This passage has been taken as indication that Newton accepted Borelli's theory (Whiteside, 1991, p.21). The second cryptic remark is Newton's explanation of why the curve cannot be an ellipse (Turnbull, p.308):

> The innumerable & infinitely little motions (for I here consider motion accoding to the method of indivibles) continually generated by gravity in its passage from *A* to *F* incline it to verge from *GN* towards *D*, & ye like motions generated in its passage from *F* to *G* incline it to verge from *GN* towards *C*. But these motions are proportional to ye time they are generated in, & the time of passing from *A* to *F* (by reason of ye longer journey & slower motion) is greater then ye time of passing from *F* to *G*. And therefore ye motions generated in *AF* shall exceed those generated in *FG* & so make ye body verge from *GN* to some coast between *N* & *D*. The nearest approach therefore of ye body to ye center is not at *G* but somewhere between *G* and *F* as at *O*. And indeed the point *O*, according to ye various proportions of gravity to the impetus of ye body at *A* towards *M*, may fall any where in ye

angle BCD in a certain curve wch touches ye line BC at C & passes thence to D. Thus I conceive it would be if gravity were ye same at all distances from ye center.

Erlichson proposes: Newton used Hooke's method to draw the curve; then Erlichson gives an interpretation to this passage, based on his drawing. Without knowing the orbit, it is difficult to imagine on what grounds can Newton claim that AF is a "longer journey" than FG.

Nauenberg draws the curve by what he calls method of curvature; this method can be recognized as the E method. He obtains the curve shown in figure 3 in Nauenberg (1994, p.236); the curve leaves the circle, at the bottom. Nauenberg then draws another curve using symmetry, figure 4 in Nauenberg (1994, p.237): the curve is drawn up to the axis of symmetry ($OC \equiv OF$), and then reflected; the pericenter falls within the bounds of the analytically calculated value. In order to show that Newton made a mistake in the drawing, Nauenberg draws a third figure, figure 5 in Nauenberg (1994, p.237): using the method of curvature, Newton draws the segment AFO, with center of force at C_s (it should be $C_s \equiv C$); instead of applying the method of curvature to draw the remaining of the curve, Newton obtains the segment OGH by reflection of AFO around the symmetry axis OC_s. But Newton mistakenly shifted the center of the curve downward, and rotated the lower part so that the two segments joined smoothly. The claim is that the third figure coincides with the figure made by Newton, which can be verified by superposing the figures. The maximum value for $\widehat{ACO}$ analytically calculated is $\frac{\sqrt{\pi}}{3} \approx 103.9°$ (Whiteside, *Papers*, vol. VI, p.150, n.127); the angle in Newton's drawing is much larger than this value ($\widehat{ACO} \approx 130°$); the error made by Newton causes the angle of the pericenter to increase; without the error, Newton would have found 107° (Nauenberg, 1994, p.226), a value compatible with the maximum value. Nauenberg also drew the figure by Hooke's method (Fig. 10 in Nauenberg 1994, p.243).

4.6 What Numerical Integration of Differential Equations Has to Say on the Mysterious Curve

In Sections 4.3, 4.4 and 4.5, we presented arguments that reinforced Cohen's thesis that Hooke put Newton on the right track to find orbits. The arguments are not entirely decisive, although they strengthen the thesis. It remains to analyze whether Erlichson and Nauenberg respective theses can be upheld face a higher order method, the $RK4$.[12] In Fig. 4.5 above, E and SE are compared with $RK4$. Mere inspection shows that:

1. ***RK*4 × *SE* (right)**. The agreement of **orbits** and **pericenters** is very good, quite perfect.
2. ***RK*4 × *E* (left)**. The **orbits** are close (although not as close as in the SE method), up to (say) point F in Newton's drawing, and diverge significantly afterwards.

[12] Even if we used a constant path of integration.

The **pericenters** are in total disagreement: the angle obtained by the E method is **smaller** than the angle obtained by $RK4$.

There are two consequences:

1. **On the matching of curvatures**. Comparison with $RK4$ (Fig. 4.5) shows that agreement of curvature is not enough to decide between methods. If Newton drew only the first part (the segment AF), and obtained the remaining of the curve by reflection of AF, he could have used either method, given the degree of precision of his hand made drawing.
2. **On the dislocation of the pericenter**. In Fig. 4.5 (left), the pericenter is slightly dislocated in the direction opposite to the dislocation in Newton's drawing: the angle is smaller instead of bigger. This argument makes less plausible the use by Newton of a method equivalent to the E method.

4.7 Some Considerations

4.7.1 Reflection Around the Symmetry Axis

In Fig. 4.4, we superposed two copies of Newton's drawing, one of them taken reflected around the symmetry axis. Within the limits of Newton's hand made drawing, the match between the reflected upper segment and the lower segment is quite good. Therefore it is a possibility that Newton drew the upper segment of the curve until some point below the horizontal axis; considerations in this paper indicate that most probably Newton used Hooke's method. Then he reflected and copied the segment on the lower hemisphere, making segments meet smoothly, and making A ($\equiv H$) touch the circle. The pericenter might have been found by visual inspection. Obviously, Newton understood that the orbit was symmetric, otherwise he could not have composed the curve with a reflected part; but he did not take full advantage of the symmetry: O is not at at the middle of arc AH, where O should be; it seems that O was found by visual inspection, observing where the curve is closer to the center.

4.7.2 So What?

What happend between December 9, 1679 (Hooke's comments on the spiral) and December 13, 1679 (Newton's letter with the polemical drawing)?

For one thing, Newton changed the problem. The problem that motivated the first solution was to probe the diurnal motion of the Earth; then, as observed by Whiteside, Newton draws the external part of the curve close to AB (Fig. 4.1); in modern terms, the observer is on the Earth. Hooke's answer (December 9) shows that he is interested in the motion of the planets; then there should be a rotation of the radius, the analogue of the yearly rotation. In sequence, Newton changed the problem, and introduced a rotation of the radius, acting together with the uniformly accelerated motion along the radius; in modern concepts, the second curve is considered from the point of view of an "absolute observer". Application of (most probably) Hooke's method followed by a reflection in the way suggested by Nauenberg had already shown Newton that the curve does not cross the vertical axis perpendicularly, as does Hooke's "elleptueid". In the first cryptic text quoted above, Newton seems to argue the point that the velocity is not tangent to the curve (line *Gn*); as Erlichson comments, Newton can explain the argument presented in the cryptic remark, based on the drawing he had already obtained.

Newton's hand made drawing has many numerical uncertainties.[13] It is always possible to obtain a curve that looks like the correct solution, if one starts from correct suppositions, and does not care about numerical details.[14]

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[13] It can be verified: (1) the inferior and superior radii of the inscribing circle (Earth) have different values, as do the left and right radii; (2) the vertical (*AD*) is not exactly perpendicular to the horizontal (*BC*); (3) the values of the angles of the pericenter and second apocenter can be measured with a protractor, but only approximately, because points *O* and *H* are themselves not precisely marked; $\widehat{ACO}$ seems to be 130°, and $\widehat{OCH}$ (corresponding to arc $\widetilde{AFOH}$) seems to be 237°; (4) point *O* seems to be at the center of the radius, if one takes *CA* for the true value of the radius (the radius where *C* lays is not exactly equal to *CA*). Given the precision of the drawing, the values for the angles could have been an attempt at naked eye to place *C* and *H* at 120°, and 240°, respectively.

[14] For instance: Let *CO* and *AH* be placed respectively at 120° and 240°, but at naked eyes (this makes the orbit symmetric inside the circle), and draw *CO* and *CH*; because a protractor has not been used, the angles become 130° and 237°. Let *CO* be half the radius *AC* (because, then, the body falls in half the time $\frac{1}{4}$ of the distance, as in an uniformly accelerated motion), but because the circle is drawn by hand, *O* is not exactly at the middle of *CK*. Then the curve is drawn by the Hooke/*SE* method up to, say, a little past point *F*; then symmetry is used to reproduce arc *AF* from *H* in the direction of *G*, using the position of *O* as an orientation of where ends must meet; the rest of the drawing is adjusted by hand (the part of the curve between *F* and *G* has scrawls, indicating that Newton adjusted it). This drawing "looks like" the correct curve, because it starts with a correct method (Hooke's), and a correct principle of symmetry is used (symmetry by reflection around *CO*); everything else is wrong.

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Chapter 5
Sciences in Brazil: An Overview from 1870–1920

Maria Amélia Mascarenhas Dantes, Silvia Figueirôa, and Maria Margaret Lopes

5.1 Introduction

In Brazil, the years from 1870 onwards were perceived as a landmark in terms of scientific activities, both qualitatively and quantitatively. In 1883, the German-born journalist Karl Koseritz acknowledged the advance of Natural Sciences, at the Faculty of Medicine and at the National Museum, whose "great collections" would almost make him "sign a truce" with Rio de Janeiro. He would comment about the excellence of the institutions' laboratories, where experiments on yellow fever, curare and anti-snake venom sera were developed. As an enthusiastic Darwinist, he observed that, at the Faculty of Medicine, several students interested in Natural Sciences were "the most pure Darwinists". A broad understanding of the changes taking place needs to consider some central aspects, as we will discuss in the first part of the text.

But, first of all, it is worth mentioning that the main goal of this paper is to offer a broad panorama of sciences in Brazil in a crucial moment: the transition to the twentieth century is a relevant moment, internationally but also locally, due to the replacement of the Empire by the Republic in 1889. The authors extensively rely on a consistent Brazilian historiography of sciences, unfortunately poorly known outside the country frontiers, without quoting it specifically in the text. Instead, we will take the opportunity to list, at the end, part of what we consider representative, taking into account comprehensiveness, language (texts preferably in English), and availability to international readers. Unfortunately, length limitations sadly obliged us to deep cuts.

Around the last quarter of nineteenth century, a significant economic growth due to coffee production and exportation, which since the 1840s was the main export product of Brazil, had impacted the country on many fronts, and led to a process of modernization – a conservative one, which tried to select from the modern what would help to overcome only part of the problems, keeping untouched privileges

M.A.M. Dantes (✉)
Department of History (retired), University of São Paulo/USP, São Paulo, Brazil
e-mail: madantes@lycos.com

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of the elites. Roughly, there were two central challenges to be faced: what we call "land challenges" and "human challenges" became re1evant. The "land challenges" aimed mostly at the lack of availability of adequate lands for agriculture, despite the vastness of the territory. They implied the conquest of this territory, either by rearrangements of land property, by geographic and natural resources surveys, or by the establishment of communication and transportation networks. Around the middle of the nineteenth century, governmental and private initiatives attempted to solve the problem of transportation building railways. But the period we are analyzing, this issue became urgent again, due to the expansion of coffee plantations to the hinterland of São Paulo State. Meanwhile, more systematic investments in expeditions, in commissions to delimit frontiers, and in cartographical, geographical and geological surveys had begun. The "human challenges" concerned the lack of work force at that time, worsened by the abolition of slavery in 1888. The immigration policies implemented were full of racist bias, thus becoming, also, a policy of "whitening", trying to build up a more "suitable" population for a country aspiring to the levels of the so-called "civilization". The difficulties of Brazilian elites in living with the masses also permeated the policies of urban planning and sanitation of the first years of the Republican period.

The solution for those challenges was sought in scientific ideals and practices of European and, eventually North-American, inspiration. To renew and multiply themselves Brazilian scientific institutions followed international standards, hired foreign specialists, and adapted new institutional models from the Northern hemisphere. Universalistic and progress-oriented philosophical systems such as Positivism, Darwinism, and Spencerism exerted a strong attraction on Brazilian elites, deeply concerned with the integration of the country into the "civilized world". Those were years when the criticism against Romanticism and its idealization of national reality were shaped. And the belief in a "unity of civilization", where national differences were perceived as a "phase" rather then "in nature" differences became predominant. Technical progress was not something Brazilian elites were willing just to hear about, but a step to reach the status of Modernity. The trajectory of Comte's Positivism in Brazil is rather illustrative of the role played by such doctrines. First by means of books on science and philosophy that reached the country, it fascinated teachers and students of Medicine and Engineering. However, close to the end of the century, the Comtean concept of historical development, especially the theory of "three stages", came to the attention of Brazilian intellectuals, giving support to social projects and even political practices, such as the rallying to Republicanism, or the campaign against slavery.

5.2 Professional Scientists

Facing social challenges implied concerns with teaching and research institutions to give professionals the capacity to carry out land surveys, and to work on emerging social problems – in parallel to be a stimulus not only to immigration, but also to the improvement of infra-structure (expansion of the railways, of fluvial transport, rebuilding of ports etc...), and to industrialization in the South East, as consequence of coffee economy expansion, and to the rapid process of urbanization concentrated mainly in the capitals of the provinces. In the years between 1870 and 1890, and particularly in the last decade of the nineteenth century, several actions were undertaken. For instance, this period witnessed the proliferation of professional high schools of engineering. Besides the reform of the *Central School* into the *Polytechnic School* of Rio de Janeiro (1874), with the definitive separation between civil and military teaching, the *Ouro Preto School of Mining* (Minas Gerais) was founded in 1875, inspired by the model of the *Saint-Etienne Mining School*, and not by the *Paris Mining School*; the creation of the *Polytechnic School* in São Paulo (1894) was followed by *Polytechnic Schools* in several states (Bahia, 1897; Pernambuco, 1896; Rio Grande do Sul, 1900).

The new curricula of the Rio de Janeiro *Polytechnic School* gave a specialized education to graduates in physical and natural sciences, in physical sciences and mathematics, and granted the degrees of geographer, civil, mining, crafts and manufacturing engineer. Practical activities and experimentation gained an increasing importance in the education of Brazilian engineers. Teaching programs and libraries acquisitions show that teachers kept their knowledge updated, as they had also introduced innovations into teaching. The *Polytechnic School* was able, on several occasions, to count on its own scientific journals – namely, *Revista da Escola Politecnica*, *Revista dos Cursos da Escola Politecnica*, *Revista Didatica da Escola Politecnica* – to enrich its collections through institutional exchange.

In the case of São Paulo State, the demand for engineers was an explicit need, present in the official speeches of the governor of the State, who emphasized the huge potential of new territories to be occupied by coffee crops:

> we have the greatest river system, and we do not have fluvial navigation; we have plantations but we do not have labor; we have raw material but we do not have factories; we have the mines, but we do not have the miner; it is urgent that we solve all this, accumulating energy that will transform us into the real owners of our land. All this invites us, gentlemen, to outfit ourselves for this struggle that will give us the domain of so many lost forces, of so many abandoned riches and so many natural products that labor has not yet valued.

To attend this demand, in 1893 the *Polytechnic School* of São Paulo was created, following the model of the *Eidgenössische Technische Hochschule* of Zurich, where its first director, Antonio Francisco de Paula Souza, graduated. It was opened with courses of Civil, Industrial and Agricultural Engineering, as well as Mechanical Crafts. Within the same context, the *High School of Agriculture Luiz de Queiroz*, was opened in Piracicaba (São Paulo hinterland) in 1901.

The reform of the faculties of Medicine was also one of the greatest concerns. In the 1850s, an attempt to renew medical teaching through the introduction of more practical disciplines had already failed. In 1874 and in 1882, the Imperial government sent to Europe a commission of physicians, with the aim of getting acquainted, especially with practical teaching, that were absent in the faculties of Medicine in Brazil by that time. The reform of Brazilian medical teaching that occurred in 1884 incorporated those "novelties". Also, at the Faculty of Rio de Janeiro, laboratories of Organic Chemistry, Mineral Chemistry, Physics and Therapeutics were established.

Cabinets of Surgical Medicine and of Histology were founded, as well as one of Experimental Physiology, and an Anatomy room. After 1889, thanks to federalism, the number of high schools, also in the domain of health, increased significantly. In 1897, the government of Rio Grande do Sul State established a Faculty of Medicine in Porto Alegre; and two private Faculties of Pharmacy opened their doors (Porto Alegre, 1896, and São Paulo, 1897).

5.3 Science Conquers the Territory

The creation of commissions and other scientific institutions by the government expressed a scientific choice for the solution of some problems hampering the expansion of agro-exporting economy. Among others, the following commissions were created: the *Carta Geral do Imperio* (General Chart of the Empire) coordinated by Henrique Beaurepaire Rohan, in the 1870s, in which took part Emanuel Liais and Luis Cruls, from the *Imperial Observatory*; the *Carta Itinerária do Império* (Imperial Itinerary Chart), to proceed to the geodesic and topographical survey of the country; the *Comissão Astronômica* (Astronomical Commission) responsible for measuring the longitudes; the *Comissão Científica do Vale do Amazonas* (Amazon Valley Scientific Commission), in which the botanist João Barbosa Rodrigues conducted botanical, geological, and hydrographical studies; the *Comissão Milnor Roberts* (Milnor Roberts Commission), that in 1879 studied the navigability of the São Francisco river. At the beginning of the Republican period, the *Comissão Exploradora do Planalto Central do Brasil* (Brazilian Central Plateau Exploring Commission) coordinated by Luis Cruls, director of the National Observatory at that time, had the main mission of establishing the border of the future capital.

Other commissions were also created for surveying natural resources. The first one was the *Comissão Geológica do Brasil* (Geological Commission of Brazil, 1875), whose initiative, despite the interest expressed by the government, was strongly due to the Canadian geologist Charles Frederic Hartt. It took the model of the *geological surveys*, almost a trade mark in the institutional development of geological sciences around the world in the nineteenth century, which common characteristic was the deep applied research character of the work. In the Brazilian case, it was stressed that through "the stimulus that mining and agriculture alone would receive, the survey would pay with interest the expenses it would make". Its structure was reasonably comprehensive, unfolding attributions from a central trunk constituted by geological sciences. The teams, formed by Brazilian and North-American engineers and naturalists, traveled throughout ten of Brazilian states from North to South, during approximately 18 months of work. The mineralogical, botanical, geological, zoological, archaeological and ethnographical samples collected were donated to the *National Museum* in Rio de Janeiro.

A financial crisis, worsened by the war against Paraguay (1864–1870), provoked the dismissal of several commissions, including the geological one, in January 1878. The Commission model, however, remained in the country, and was reused by the *Comissão Geográfica e Geológica de São Paulo* (Geographical and Geological Commission of São Paulo, 1886–1931), the *Comissão Geográfica e Geológica de Minas Gerais* (Geographical and Geological Commission of Minas Gerais, 1891–1899), and the *Serviço Geológico e Mineralógico do Brasil* (Brazilian Geological and Mineralogical Survey, 1907). The two latter commissions were organized and directed by Orville Derby – the only foreign member of the original team who remained in Brazil for his whole life.

The *Comissão Geográfica e Geológica de São Paulo* was created in response to the claims of coffee producers from São Paulo, aiming at the solution of practical issues such as availability of land, and of means of communication. The CGG, following Derby's orientation, acted under a "naturalist" approach, encompassing geology, botany, geography, topography, meteorology, zoology, archaeology etc., in an attempt to produce the most accurate profile of the physical environment. Its technical team employed mainly engineers educated in Brazil, either from the *Polytechnic of Rio de Janeiro*, or from the *Ouro Preto School of Mines*. As time went by, given the process of specialization of sciences, those different branches broke away from the *Commission*, originating several other institutions still active in the scientific scene. However, Derby's naturalist point of view confronted short term interests of São Paulo elites and government, mainly because after almost 20 years of work, the so-called "sertão" (hinterlands) of São Paulo State – a vast region in the west that encompassed a great deal of fertile land – was not yet chartered to allow occupation and exploration. Disagreeing with the more pragmatist orientation the government tried to impose upon the Commission's work, Derby resigned (and with him some of his auxiliaries staff). Derby's resignation is also linked to the more general issue of "pure science" versus "applied science" that began to surface at that moment.

Immediately after his resignation, Derby was commissioned to direct the *Serviço de Terras e Minas* (Land and Mines Bureau) of Bahia State. Some time later, in January 1907, he was invited to establish the above mentioned *Serviço Geológico e Mineralógico*. The goals of the new *Survey* remained, in general sense, the same as those of the previous Commissions: mineral resources surveys, agriculture, droughts, and road problems. One of the main points of its action was the attempt to tackle the problems of droughts and irrigation in the Northeastern region. Regarding mineral resources, the SGMB played a role of assessment agency to the federal government. The inquiries of the Ministry to the *Survey* asking for advice and analysis about economic value of mineral ores were very often. For instance, the conclusions obtained by the survey of iron and manganese reserves in Minas Gerais supported, in 1909, the main points of a national policy on iron metallurgy. For the first time in Brazilian history, an industrial policy was conceived as a result of a geological survey.

Besides geological and geographical surveys, another important, not overlooked aspect was the increase in agricultural productivity itself, improved by the foundation of institutions devoted to the study of climate, soil quality, acclimation of plant species, and pest control. Following a universal trend set by Germany from the mid-nineteenth century, the *Imperial Estação Agronômica* (Imperial

Agronomic Station, 1887), nowadays *Instituto Agronômico de Campinas* (Campinas Agronomical Institute), was founded in the center of the coffee producing area of São Paulo. This institution was devoted to the close application of chemical studies to agriculture, and the Austrian chemist Franz Dafert, formed in this tradition, was invited to direct it.

Related to the improvement of knowledge on climatic conditions, in the decade of 1890, a Meteorological Service was founded in São Paulo within the *Comissão Geográfica* e *Geológica*. In Manaus (Amazon), a Meteorological Observatory was also created (1893). Beginning with the works carried out by its director Luis Friedman, and the data collected by a meteorological station in the *Museu Paraense Emílio Goeldi* (Emilio Goeldi Museum in Pará), Julius Hann wrote the first essay on equatorial meteorology, until then almost unknown.

5.4 Science Conquers the Public

Related either to the land or human challenges, natural sciences, anthropology, and archaeology found, in the museums of natural history, privileged institutional loci. There was a significantly increase not only in quantity, but also in scientific and social importance of those institutions during the last decades of the nineteenth century in Brazil, especially until the mid-1860s. Despite the attempts to create provincial museums (in Bahia, Alagoas, Ceará), the *National Museum* in Rio de Janeiro, that had been working since 1818, was still practically the sole institution of that kind in the country. For instance, in 1871 the *Museu Paraense Emilio Goeldi* was officially founded. Its origins date back to 1866, when the *Sociedade Philomatica* was established, under strong influence of North American museum concepts, i.e., independence in relation to governmental institutions, and support of entrepreneurs.

In 1876, not only the *Museu Nacional* was reorganized, but the *Museu Paranaense* (Curitiba, Paraná) was founded, following the trend observed in other Latin American countries that resulted from their participation in the Great Universal Exhibitions in the second half of the nineteenth century. From 1883 to 1889, the above mentioned botanist Barboza Rodrigues, directed the *Museu Botanico do Amazonas* (Amazon Botanical Museum) in Manaus. In 1894, thanks to the initiative of Orville Derby, the *Paulista Museum* was organized in the city of São Paulo. The Historical and Geographical Institutes – either the national one in Rio (IGHB) or the local ones in the states of Pernambuco, Bahia and Sergipe – had already been improving their historical collections with many natural, archaeological, and ethnographical products.

The distinctive mark of those Museums would be the scientific investigation and the popularization they carried out, based on their collections. Their directors and naturalists promoted field excursions throughout the country, in search for new and rare botanical, zoological, mineralogical, ethnographical, and archaeological objects. The collections considerably increased during that time, and were made publicly known in exhibitions and scientific publications, namely: *Archivos do Museu Nacional*, *Boletim do Museu Paraense Emilio Goeldi*, and *Revista do Museu Paulista*, which besides the *Revista do Instituto Historico e Geografico Brasileiro,* were the unique Brazilian scientific publications specialized in natural sciences with international extent. These museums also committed themselves to educating a wide public, either by visits to the exhibitions or by free courses, and public conferences related to investigations in zoology, botany, geology, experimental physiology, anthropology, etc. At that time, Museums still were loci of science production not yet apart from the lay public that was needed for the validation and support of these institutions. It is clear that in a country still marked by slavery, lay public meant small, intellectual or economic elite from the capitals of the main regions, who only then began to include "the ladies" in scientific meetings. The presence of women was indeed encouraged, according to the scientific mentality of the time, since only after 1879 was women's attendance at regular higher education courses allowed in Brazil. Examples were the *Conferências da G1ória* (Glória Conferences), created to "educate people", that happened from 1873 to 1880 in public schools of the Gloria District in the city of Rio de Janeiro. The 348 conferences encompassed various and updated themes ranging from Literature, History or Education, to Medicine, Elementary Math, or Darwinism, gathering a wide public, including the Emperor himself accompanied by his daughters.

Brazilian museums of natural history were inserted in the international scene by means of their scientific exchanges, not only with European and North American museums but also with Latin American ones, whose interactions, although on a fair scale, are still less well known. Intensifying their international relations, museums also played an active role in the National and Universal Exhibitions that proliferated from the mid-nineteenth century. The directors of Brazilian museums, believing in the ideals of progress, searching for the paths to modernization, and for international contacts, made every effort to collaborate in these exhibitions, helping the construction of a positive self-image for the country.

Despite their importance to Natural Sciences, and to racial studies in the country, these museums were put aside at the end of the century. And their scientific practices became less important than the laboratory ones – clean, bright, aseptic, housing the scientists in white aprons, with their microscopes and their studies of invisible beings. These labs, since there was nothing to be seen or learnt by the comparative naked eye, would be totally closed to lay public, even if they belonged to the elite. This shift in natural sciences was clearly captured by Batista de Lacerda, director of *Museu Nacional* by the end of the century, who established there the first laboratory of experimental physiology in Brazil, in 1880.

5.5 Science and Health Control in the Cities

The issue of public health also acquired an important status in the projects of social re-organization, placed among the priorities of Brazilian governments. The end of slavery, in 1888, and the increase of immigration generated a radical change within

Brazilian population, also stimulated by the expansion of urban centers that attracted rich land owners, workers, and employees for the services. As a result, the main urban centers, and ports had their sanitary conditions worsened. No doubt, the major concern of Brazilian governments was the capital, the city of Rio de Janeiro, not only the political center, but also the largest port. Since the beginning of the nineteenth century, hygienic measures have been implemented, and in the last years of II Empire, the vaccine against small-pox, for instance, was already produced in the country. The success of microbiology worldwide contributed to its implementation in Brazil, ranging from a special, and unsuccessful invitation, by the emperor Pedro II to Pasteur to direct a microbiological institute in Rio de Janeiro, to the foundation of an *Institute Pasteur* in that city, in 1888. It produced anti-rabies vaccines, under the direction of Brazilian Augusto Ferreira dos Santos, who studied in the Parisian headquarters of that above-mentioned institute. However, it was in the years that followed the instauration of the republican regime that the first sanitation services, based upon microbiology principles, were created (São Paulo, 1892; Rio de Janeiro, 1900). Those measures had political and economical purposes, since hygienic conditions of the cities and the risk of epidemics shook the image of Brazil abroad, diminishing its credibility and bringing more difficulties to the policies of attracting European labor.

The *Serviço Sanitário de São Paulo* (São Paulo Sanitary Service, 1892) had the attributions of controlling and supervising medical practices, and controlling epidemics and endemic diseases that there had already been in the province. Shaped according to the microbiological conception, it encompassed a Bacteriologic Institute, a Laboratory of Chemical, Food and Drugs Analysis, a Vaccine Institute, a quarantine hospital, services of disinfection, and a Section of Medical Statistics. The physician Adolfo Lutz created this Service, where he conducted studies, with a small team, on regional diseases, such as typhoid fever, yellow fever (the one that most worried Brazilian governments and physicians), diphtheria, searching for new means of diagnosis and therapies. Lutz tested theories about yellow fever in fashion at that time, such as the one proposed by the Uruguayan physician Sanarelli, and the theory of transmission of *Aedes Aegypti* proposed by the Cuban Carlos Finlay. Despite negative reactions, some of Lutz works had great repercussion, like the diagnosis of cholera within São Paulo State, in 1894, and of bubonic plague in the port of Santos, in 1899. As to the production of drugs and medicines, during the first years of the Services some continued to be imported, but with the dissemination of bubonic plague, São Paulo government founded a specialized laboratory, the *Instituto Soroterapico do Butantan* (Serum Institute at Butantan, 1901).

In Rio de Janeiro, the implementation of microbiological research was a longer but lasting process. There was a diversified community of practitioners and in the last quarter of the nineteenth century, debates concerning different concepts about medical teaching and practices were intense. Created by the city government, in 1899, to produce sera and anti-plague vaccines, the *Instituto de Manguinhos* (Manguinhos Institute) was directed by the Brazilian physician Oswaldo Cruz, who had been a fellow at the Parisian Institute Pasteur. The new institute of biomedical sciences was quite innovative. The trajectory of Oswaldo Cruz, also director of the Federal Bureau of Public Health, is representative of the role played by microbiologists during this epoch of Brazilian history. Responsible for sanitation campaigns, such as the one against yellow fever (1902) following Finlay's theory, and the campaign against small-pox (1904), this physician was strongly supported by the President of the Republic, the coffee producer and positivist Rodrigues Alves. Those campaigns were part of a broader, quite controversial project of urban reform of the city, and faced resistance to their effectiveness by significant part of Rio de Janeiro population. In 1905, with the diminishing incidence of such diseases, the prestige of Oswaldo Cruz and his team began to be acknowledged, and consolidation came in 1907, with the award of a gold medal in the International Hygienic Exhibition in Berlin.

The trajectory of this institute allied several factors: hygienic measures, governmental support, and the development of researches on bacteriology and other related disciplines. In 1908, *Manguinhos* became a research center in experimental pathology, and was renamed *Instituto Oswaldo Cruz*. Its production of sera and vaccines continued, but the study of infectious and parasitic diseases in humans, and of issues concerning hygienic was also in the agenda. Bacteriology was the main scientific discipline, but other fields like entomology developed. In pathology, activities turned to diseases found in Brazilian territory, and to the assessment of governmental projects such as the construction of the Madeira-Mamoré Railway in the Amazon. The Institute contributed to the prefects carried out in distant regions of the country. Hallmarks were also the expeditions in 1911 and 1912–1913 to the North and Northeastern regions, when the first systematic survey of health conditions of the populations in the hinterland was conducted. In *Manguinhos*, the first generations of Brazilian biologists and microbiologists were graduated, since the new field of experimental medicine did not have space within the faculties. A training course was established in 1909, where students trained laboratory techniques, and came into contact with studies of biological sciences conducted by the institution. The concern with scientific excellence led Oswaldo Cruz to keep exchanges with foreign institutions, and to support the training of the researchers abroad. In 1910 it started the publication of the Institute's journal *Memórias do Instituto Oswaldo Cruz*. The institutional orientation survived after the death of Cruz (1917) until the middle of the twentieth century, with the subsequent directors, such as Carlos Chagas Filho, educated in the first years of the institution, and constituted a model of experimental science that would be disseminated around Brazil.

5.6 Final Remarks

The presence of science in the projects of modernization of Brazilian elites, in the last quarter of the nineteenth century was significant, but not free from contradictions. The work done by Oswaldo Cruz, for instance, was decisive in the process of controlling urban masses; the Exploratory Commissions, although essential to the expansion of the agriculture frontier, accelerated the extermination of Indian nations; and eugenics theories developed in the museums and faculties of Medicine and Law constituted the basis for immigration policies that had, among other goals, the "whitening" of the population. Engineers and physicians were important agents of this process of conservative modernization, aimed at the integration of the country into the "civilized world". They contributed to the justification of, by means of science, even the pitiless aspects of the projects of territorial occupation and social organization. However, the process of implementation of sciences in Brazil did not respond exclusively to short term measures, and interests of Brazilian governors and elites. An emerging scientific community acted under several circumstances to broaden and even to negotiate the research programs proposed by the government. Hartt and Derby, for instance, did not stop investing their archaeological and paleontological interests, while responsible for opening new frontiers in the hinterland. Oswaldo Cruz did not restrict the activities of his team to the study of diseases, and the production of medicines. He built up a center of research in biological sciences. Therefore, many institutions were not strictly linked, nor did they mechanically respond, to economic demands. They were, however, attached to broader, scientific project, which through its "prospective and educational" character tried to anticipate or even shape the future, in medium or long term.

By the end of the period considered here, there was a relatively diversified scientific community, made up of botanists, geologists, microbiologists, astronomers, as well as other professionals. In 1916, the Brazilian Society of Sciences (later Brazilian Academy of Sciences) was founded, an association that intended to organize scientific practices, establishing norms for the action of researchers in the country. It became a very active pole in the 1920s, present in the movements for a reform in the education system, and for the creation of institutions dedicated to study of "pure science". This movement came through in the 1930s, when the first Brazilian universities were created, giving birth to a new phase in the process of institutionalization of sciences in Brazil.

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Chapter 6
Henri Becquerel and Radioactivity: A Critical Revision

Roberto de Andrade Martins

6.1 Introduction

In 1896, Henri Becquerel detected a penetrating radiation emitted by some uranium salts and came across a phenomenon that nowadays we call "radioactivity". Becquerel's discovery of uranium radiation was not casual or blind. It was guided by his acceptance of Poincaré's conjecture that the emission of X-rays could be a phenomenon related to the luminescence of the cathode ray tube, together with his previous knowledge and expectations concerning the properties of uranium compounds (Martins, 1997; 2004).

What Becquerel expected to find was the emission of penetrating electromagnetic radiation (similar to ultraviolet rays) produced by a special kind of fluorescence or phosphorescence that violated Stokes' law. That was, indeed, what he thought he had found. Guided by his preconceptions, Becquerel ascribed to uranium radiation the usual properties of known electromagnetic waves: reflection, refraction and polarisation. Moreover, since he thought the phenomenon was a kind of phosphorescence, he also expected to observe a decrease of the radiation emitted by uranium salts kept in darkness, and an increase when they were stimulated by sunlight – and he reported that he observed all those effects.

Those and several other aspects of Becquerel's experimental work may be described as experimental mistakes. There is nothing new in the observation that scientists sometimes are misled by their theoretical expectations – but it is remarkable how far Becquerel was led by his preconceptions.

Other researchers gradually corrected Becquerel's mistakes. As the study of radioactivity developed, Becquerel reinterpreted his own early work, hiding his mistakes or ascribing to himself their rectification. He was successful, and in a few years his errors were forgotten – and he was awarded the Nobel Prize.

Of course, Becquerel could not succeed in his personal endeavour without the support of colleagues and the French Academy of Sciences. This paper will not try

R. de A. Martins (✉)
Group of History and Theory of Science, Universidade Estadual de Campinas (Unicamp), 13083-970 Campinas, Brazil
e-mail: Rmartins@ifi.unicamp.br

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to disclose the sociological aspects of the episode. It will only analyse the evidence relating to Becquerel's mistakes and his concealment of his own failure.

6.2 Properties of the Radiation

Becquerel began his search for a penetrating radiation emitted by luminescent bodies in January 1896. On the 24th February, he presented to the French Academy of Sciences his first positive results: he succeeded to detect a penetrating radiation (similar to X-rays) emitted by crystals of double sulphate of uranyl and potassium (Becquerel, 1896a). In this and the next communication, on 2nd March 1896 (Becquerel, 1896b), Henri Becquerel did not discuss the nature of the penetrating radiation. He only described that it was able to pass through black paper and thin glass plates, and to affect photographic plates. At this time, he believed that the observed radiation "[. . .] could be invisible radiations emitted by phosphorescence with a persistence infinitely larger than the persistence of luminous radiations emitted by those bodies" (Becquerel, 1896b, p. 503). The properties he described in those two earliest communications are well known and are accepted to this day. From Becquerel's third "radioactivity" paper onwards, he reported several phenomena that we could describe as anomalous. However, he and most of the scientists of his time found nothing strange in those phenomena, since they completely accorded with Becquerel's expectations.

In his third "radioactivity" paper (9th March 1896), Henri Becquerel began the study of the properties of the radiation emitted by the uranium phosphorescent compound he was using (Becquerel, 1896c). He was guided by his expectation that the radiation was an invisible light. One of the known properties of X-rays and ultraviolet light was their ability of discharging an electroscope. Becquerel observed that the uranium rays were also able to produce that effect. Röntgen had tried to observe reflection and refraction of X-rays, with negative results. Becquerel tried similar experiments with uranium radiation – and he apparently succeeded.

One of Becquerel's experiments seemed to show clear evidence of regular *reflection* of the radiation emitted by the uranium salt by a metallic concave mirror – an impossible result. In the same paper presented on 9th March 1896, he described evidence for the existence of *refraction* of the penetrating radiation emitted by phosphorescent compounds. Becquerel first tried to detect refraction of uranium radiation using a prism, and stated that those experiments "gave signs of refraction, but the signs were too weak to be presented today. Moreover, it will be seen from results that will be described below, that some images clearly reveal the fact of refraction and total reflection in glass" (Becquerel, 1896c, p. 561).

The positive evidence referred to by Becquerel was obtained in the study of uranium nitrate. This substance strongly absorbs moisture from the air and its crystals must therefore be protected from the atmosphere. Henri Becquerel put the uranium nitrate in a glass tube, closed with a thin glass plate (0.2 mm thick) sealed with paraffin. This device was put (the glass plate downwards) over a photographic plate wrapped in black paper. After 2 days, the photographic plate was developed and showed a black spot corresponding to the base of the uranium nitrate crystal. This spot was surrounded by a "slightly dark" band, limited by the border of the glass tube. Becquerel concluded:

> This band is due to the action of the radiations obliquely emitted by the vertical faces of the [cylinder of powdered] crystal which is several millimetres thick; the radiations stopped by this tube were refracted and totally reflected inside it, as light rays inside a liquid vein. The action is stronger at the places that are in contact with the uranium nitrate crystal. (Becquerel, 1896c, p. 563)

Besides that, in a later paper, Becquerel claimed that he obtained deflection of the radiation of uranium nitrate using a crown glass prism (Becquerel, 1896d, p. 693). Becquerel *expected* uranium radiation to be refracted and reflected, because he thought it was some kind of penetrating ultraviolet light. What he saw confirmed his expectations. However, uranium radiation is not refracted and reflected by glass. Becquerel never published photographic evidence of those experiments.

The French physicist believed that the radiation he was studying was similar to light. He had already "proved" that it could be refracted and reflected. It was natural to check whether it could be polarised. On the 30th March 1896, he reported positive evidence for the polarisation of uranium radiation (Becquerel, 1896e).

A photographic plate was wrapped in black paper. Over the paper Becquerel placed two pieces of a thin tourmaline plate (0.50 mm), oriented in perpendicular directions. Over them he put a single tourmaline plate (0.88 mm thick), with its axis parallel to that of the small tourmalines and perpendicular to the other. In this condition, light passes through the parallel tourmalines and is stopped by the crossed tourmalines. A flake of double sulphate of uranyl and potassium was placed over this device.

> After 60 h of exposition, the photographic plate was developed; it clearly showed the silhouette of the tourmalines, and the action through the parallel tourmalines was considerably stronger than through the crossed tourmalines. [...]
>
> This experiment therefore shows at the same time, for the invisible rays emitted by uranium salts, the double refraction, the polarisation of both rays and their different absorption through the tourmaline. (Becquerel, 1896e, p. 763)

In this case, as in some others, Becquerel had very scarce experimental evidence: his polarisation experiment was *a single test* (perhaps repeated once, several days later). One of the most crucial pieces of evidence for the interpretation of the nature of uranium rays was the difference between two diffuse dark spots in a photographic plate. In his 1903 book, Henri Becquerel published for the first time his photographic evidence for polarisation of uranium rays (Becquerel, 1903a, plate II, Fig. 6). It is very difficult to recognise the effect described by Becquerel.

At this time, Becquerel's experiments seemed to clearly prove that the radiation emitted by uranium compounds was a kind of electromagnetic transversal wave. Silvanus Thompson discussed the nature of the uranium radiation, and remarked:

> The extraordinary property exhibited by the uranium compounds of emitting a persistent invisible radiation that will pass through aluminium and produce photographic action would suggest that these rays are identical with Röntgen's, were it not that Becquerel's success in

> reflecting, refracting, and polarising them proves that they are more akin to ultraviolet light. (Thompson, 1896b)

6.3 Persistence of Emission of the Invisible Radiations

There was a conflict between Becquerel's expectations and his observations concerning the persistence of the invisible radiation emitted by uranium salts. He observed that those substances emitted the penetrating rays for a long time, when kept in the dark. Nowadays, we believe that this is one of the main characteristics of radioactivity: it is a spontaneous emission of radiation, which cannot be increased or decreased by common physical stimuli (light, heat, etc.). In the case of uranium, the emission decreases very slowly in time – a decrease that cannot be detected in a few years.

In his third "radioactivity" paper (Becquerel, 1896c), Becquerel described the long persistence of the invisible radiation emitted by the phosphorescent crystals of uranium compounds, that he had kept in darkness for 160 h. During this time, there was no perceptible decrease of the penetrating radiation. However, this observation did not led him to the conclusion that this was a new phenomenon:

> Perhaps this fact should be compared to the indefinite conservation of absorbed energy in some bodies, that emit it when one heats them, a fact to which I have already called the attention in a work on the phosphorescence by heat. (Becquerel, 1896c, pp. 562–563)

Henri Becquerel was still being guided by his knowledge of luminescence phenomena. The phenomenon he recalled here had been well studied by his father, Edmond Becquerel (1848). When a phosphorescent substance is exposed to light and brought to a dark room, it will shine during some time, but its luminosity will decrease and after a longer or shorter time it will seem to have lost all its phosphorescence. However, there are several phosphorescent substances that can shine again after losing their glow, if they are heated (Becquerel, 1848). Five years before his "radioactivity" researches, Henri Becquerel had also studied those phenomena (Becquerel, 1891).

6.4 Other Anomalous Properties of Becquerel's Rays

Further experiments made by Becquerel provided new anomalous phenomena: the intensity of the radiation emitted by uranium salts increased when they were stimulated by light, and decreased when they were kept in darkness. Of course, according to present physical knowledge those effects could not exist, but Becquerel reported he had observed them, and this strengthened the belief that the phenomenon was a kind of invisible phosphorescence and that the emitted radiation was invisible electromagnetic radiation (similar to ultraviolet rays).

When Henri Becquerel reported the emission of radiation by his phosphorescent samples kept in darkness, he concluded that it could be due to some kind of invisible, long-lived phosphorescence. In his third "radioactivity" paper, he reported that the effect was still observed when the samples were kept in darkness for 7 days. In his fourth communication presented on the 23rd March 1896, Becquerel presented new evidence:

> If the phenomenon of emission of invisible radiations that we study is a phosphorescence phenomenon, it should be possible to exhibit its excitation by given radiations. That research becomes very difficult because of the prodigious persistence of the emission when those bodies are kept in darkness, protected from all luminous radiations and from invisible radiations of known nature. After more than 15 days, uranium salts still emit radiations almost as intense as on the 1st day. Placing on the same photographic plate, with black paper, a flake kept for a long time in darkness and another that had just been exposed to daylight, the impression of the silhouette of the second is a little bit stronger than the first. Magnesium light, in the same conditions, produces only an imperceptible effect. If the flakes of double sulphate of uranyl and potassium are lively illuminated by an electric arc, or by the bright sparks of the discharge of a Leyden bottle, the impressions are noticeably darker. Therefore the phenomenon seems indeed an invisible phosphorescence phenomenon, but it does not seem intimately related to the visible phosphorescence and fluorescence. (Becquerel, 1896d, p. 691)

Becquerel was not the only one who reported this effect. Silvanus Thompson also stated that stimulation by light increased the emission of penetrating rays by uranium nitrate (Thompson, 1896a).

In those experiments, only a very strong effect could be unambiguously detected. Indeed: the samples used by Becquerel were not exactly equal, but only roughly similar – and any observed small difference in radiation could be ascribed to difference in the samples themselves. Besides that, visual comparison between two dark spots in a photographic plate is highly subjective, except if one is much darker than the other. Becquerel reported that when a flake of the uranium salt was illuminated by an electric arc or by discharge of a Leyden bottle, "the impressions are noticeably darker" (*les impressions sont notablement plus noires*). "Noticeable", of course, can be interpreted either as striking or as merely observable. According to our current physical knowledge, Becquerel could not have observed any strong increase in radiation emission, because uranium radiation is not excited by light. The electric arc could *heat* the uranium salt flake, and that could increase the photographic effect below the illuminated sample – but discharge of a Leyden bottle would not produce the same effect. It seems likely that the spots were very similar to one another, but Becquerel saw one of them darker than the other because he expected the effect to occur. The photographic evidence was never published by Becquerel. This case is comparable to Becquerel's mistaken experiments on reflection and refraction of uranium radiation.

However, there was an independent, objective method that could be tried: the measurement of the effect of the radiation upon the discharge of an electroscope. He also used this second method:

> The electroscope allowed me also to display the weak difference between the emission of a flake of uranium salt kept in darkness for 11 days, and the emission of the same flake vigorously illuminated by magnesium. In the first case, the speed of fall of the [electroscope] leaves was 20.69 [seconds of arc per second] and after luminous excitation it became 23.08. (Becquerel, 1896d)

There was a second series of measurements with similar results (Becquerel, 1896e, p. 765). In both cases, a decrease of the intensity of radiation larger than 10% was observed when the uranium salt was kept in darkness. There seemed to be strong evidence for accepting the increase of radiation intensity under stimulation by light and to interpret the phenomenon as a kind of phosphorescence. In later papers, Becquerel still held the same opinion (Becquerel, 1896f).

Becquerel's electroscopic experiment was, however, mistaken. In this case, it is possible to detect Becquerel's error using information published 7 years later. The table containing the 28th March 1896 measurements was published later (Becquerel, 1903a, p. 20) and allows us to recognise several problems: (a) lack of precision of measurements; (b) lack of reproducibility; (c) only a single series of measurements was made.

6.5 Correction of Becquerel's Mistakes

Before 1898, Becquerel's work was not submitted to systematic duplication or criticism. It was simply reviewed and accepted as a contribution that did not strongly contrast with other known phenomena and therefore called for no deeper thoughts. Up to 1898, only two aspects of Becquerel's work had been criticised: the polarisation of uranium rays and the excitation of this radiation by light.

In his first paper on the radiation emitted by thorium, Schmidt believed that he had found evidence for refraction, but found no sign of polarisation by tourmalines (Schmidt, 1898). In the beginning of 1899, Ernest Rutherford reproduced Becquerel's polarization experiment and could not perceive "the slightest difference in the intensity" of radiation passing through parallel or crossed tourmalines (Rutherford, 1899, p. 112). In the same paper, Rutherford described experiments to test refraction of uranium rays. He used prisms of glass, aluminium and paraffin. The prisms were crossed by uranium radiation emerging from a slit cut in a thick lead plate. He observed no deflection of the radiation.

Elster and Geitel found, in 1897, that the emission of uranium radiation could not be increased by excitation by sunlight (Elster and Geitel, 1897). The intensity was found to be constant (not "slightly decreasing", as Henri Becquerel described it) over several months. Two years later, Marie Curie accepted that Elster and Geitel had proved in this paper that radioactivity cannot be increased by light (Curie, 1899).

In their paper, Elster and Geitel stressed that the radiation from uranium can be distinguished from the effects produced by other substances (aluminium, zinc, phosphorescent paint and fluorspar) because these do not impart electrical conductivity to the air. Notwithstanding the title of their paper, they conclude that the name "hyperphosphorescence" cannot be applied to the observed phenomenon. This seems the first time that the concept of an invisible phosphorescence of uranium was criticised.

In April 1898, thorium was discovered to emit radiations similar to those of uranium (Badash, 1966). This led to an increased interest in the phenomenon. In this same year, polonium and radium were also found. Marie Curie also rejected the name "hyperphosphorescence" and proposed the name "radioactivity":

> Uranium rays have frequently been called *Becquerel rays*. This name can be generalised and applied not only to uranium rays but also to the rays of thorium and to all similar radiations.
>
> I will call *radioactives* the substances that emit Becquerel rays. The name *hyperphosphorescence* that had been proposed for the phenomenon seems to me to convey a wrong idea about its nature. (Curie, 1899, p. 41)

The Curies rejected the old "invisible phosphorescence" concept, but proposed an explanation of radioactivity related to invisible fluorescence. Indeed, for several years they claimed that there existed an unknown, invisible, very penetrating cosmic radiation (similar to extremely hard X-rays), that could be transformed by radioactive bodies into less penetrating, detectable rays.

The Curies concentrated their attention in the substances emitting radiation and not in the radiations themselves. In 1899, Rutherford identified two kinds of radiation (called by him α and β) using as criterion the absorption of radiation by thin aluminium foils (Rutherford, 1899). A few months later, Giesel showed that β radiation could be deflected by a magnet and therefore could not be an electromagnetic radiation (Giesel, 1899; Malley, 1971). After a few years, a completely new view emerged: radioactive bodies emitted three kinds of radiation, two of them (α and β) deviable by magnetic fields (and therefore carrying electrical charges), and the third (γ), non deviable, similar to X-rays. The nature of the radiation emitted by uranium and other radioactive bodies was completely different from what Becquerel had believed and "proved" by his experiments.

The central core of our present theory of radioactivity was proposed by Rutherford and Soddy in 1902–1903. They presented strong evidence for the gradual transformation of radioactive elements, the existence of radioactive series and spontaneous release of internal energy (Rutherford and Soddy, 1902a, b, 1903; Malley, 1979; Trenn, 1975).

6.6 Becquerel's Strategy

In 1899, Henri Becquerel acknowledged for the first time some of his early mistakes, but tried to convey the impression that he had corrected them himself (Becquerel, 1899). From this time onwards, he devoted much of his energy to establish himself as the successful discoverer of radioactivity.

It is remarkable that, at one point of his 1903 book, which presented the state of the art of radioactivity up to that time, Becquerel stated that his only aim was to describe his own researches: "To describe the beautiful work of Mr. and Mrs. Curie is outside the scope of this memoir, that in principle contains only my personal researches" (Becquerel, 1903a, p. 105). Maybe this meant that the researches of other people, described in his book, were secondary to his own work.

Henri Becquerel used a systematic strategy: he turned his old mistakes into as so many successes; he described as his own the discoveries of others; he distorted the whole history of radioactivity and tried to show that he was the central protagonist. Let us show some instances of this strategy.

6.6.1 Spontaneity of Radiation

Before 1898, Becquerel had never described the emission of uranium radiation as "spontaneous". Afterwards, when this was seen to be one of the fundamental aspects of radioactivity, Becquerel reinterpreted his work:

> Among the properties that I have pointed out at the beginning of my researches as characteristic of this radiation that was unknown, there are three fundamental ones that have been afterwards verified by all observers; they are: the spontaneity of radiation, its constancy and the property of imparting electrical conductivity to gases. (Becquerel, 1899, p. 771)

In 1903, after describing his first "radioactivity" paper, Becquerel stated:

> Under those conditions, the phenomenon could be attributed to a transformation of solar energy, of the same kind as phosphorescence, but I soon recognised that emission was independent of any excitation of known nature – luminous, electric or thermal.
>
> We were therefore in face of a spontaneous phenomenon of a new kind. Here I show you the first print which revealed the spontaneity of the radiation emitted by the uranium salt. (Becquerel, 1903b, p. 2)

and at this point, Becquerel refers to the first photograph taken in darkness, described in his second "radioactivity" paper. At other places, Becquerel explicitly states that he recognised at this time the spontaneity of uranium radiation:

> This observation establishes the fundamental new fact of an emission of penetrating rays without apparent exciting cause. (Becquerel, 1903a, p. 13)
>
> [. . .] some days later, from 27th February to 1st March, I recognised that the emission was produced spontaneously, even when the uranium salt was kept protected from luminous excitation [. . .]. On the 2nd March 1896, I reported to the Academy of Sciences the conditions under which I have been led to observe the spontaneity of the radiation, the new fact from which follow all later studies. (Becquerel, 1900, p. 48)

6.6.2 Constancy (in Time) of Emission

Up to 1898, Becquerel described that the emission of radiation by uranium salts decreased with time, after stimulation by light. Afterwards, the story was changed.

According to Becquerel, after noticing that the uranium salt emitted radiation in darkness, he already supposed that the intensity was constant:

> As the uranium salts used had been prepared a long time ago, it was to be supposed that the intensity of the phenomenon was independent of time, and hence that emission should appear constant. All later experiments showed that the activity of uranium presented no appreciable decrease with time.
>
> [. . .] The photographic method was primarily a qualitative one while the electrical method gave numerical data, and the early measurements revealed the constancy of the radiation with time. (Becquerel, 1903b, p. 2)

Notice that in 1899 Becquerel still accepted that the intensity of uranium radiation exhibited a decrease with time:

> It seems that there is a slight decrease of intensity during the 1st months and afterwards the intensity seems unchanged. (Becquerel, 1899, p. 772)

In this same paper, Becquerel stated that uranium radiation cannot be stimulated by physical influences, but did not acknowledge that Schmidt corrected him:

> [. . .] it was impossible to produce any noticeable change of the intensity of this emission by physical influences. (Becquerel, 1899, p. 777)

At other places, Becquerel claimed that his early experiments had shown that the intensity was constant:

> From the beginning of those studies I have checked whether one could observe a progressive weakening of the radiated energy by subtracting those bodies to all known external excitation. A first series of experiments, pursued during 2 months, has initially showed that this energy did not decrease in an appreciable way. (Becquerel, 1900, pp. 14–15)

At some places Becquerel refers to experiments that had shown an increase of the radiation when uranium salts were excited by light, but does not state that it was he who reported those effects:

> None of the attempts to exhibit an excitation by ultraviolet, infrared or light rays produced a [positive] result; the same was the case when uranium salts were excited by X-rays. However, in several experiments, after exposing the double sulphate of uranyl and potassium flakes to the action of sparks and electrical arc, a slight temporary increase of emission was observed, but this very weak effect seems another phenomenon superposed upon the constant and continuous emission by uranium. (Becquerel, 1900, p. 53)

Finally, in his 1903 book Becquerel stated that, by the electroscopic method, he had been able, as early as 14th March 1896, to prove that the intensity of radiation was not increased when the uranium salt was excited by magnesium light:

> In some cases, the photographic impression produced by samples of a salt exposed to light or strongly illuminated by electric sparks seemed stronger than the impression produced by the same bodies carefully kept away from any excitation. [. . .] But it seems that hose facts are accidental, because electrical measurements and experiments made in order to analyse the active rays have not allowed us to detect any action of this kind. This is, for instance, one of the earlier measurements made to detect this effect. (Becquerel, 1903a, p. 21)

6.7 Conclusion

Henri Becquerel's experimental research on the phenomenon we now call "radioactivity" was full of serious mistakes. He ascribed to uranium radiation several properties – such as reflection, refraction, polarisation, increase by light stimulation and decrease in darkness – that were corrected by other researchers. In all cases, Becquerel was strongly influenced by his theoretical preconceptions. His mistakes, however, belong to different kinds.

(a) Photographic evidence for reflection and refraction of the radiation: it is doubtful that he really observed those effects.

(b) Photographic evidence for polarisation and stimulation of radiation by light: the photographic evidence was inconclusive, but Becquerel arrived nevertheless to definite conclusions.
(c) Electroscopic measurement of stimulation of radiation emission by light: Becquerel was unable to follow some well-known rules about measurement and manipulation of quantitative data.

Later, however, he was socially successful in reinterpreting his early work and convincing the scientific community that his research was seldom mistaken, and that he had himself corrected his earlier mistakes.

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Chapter 7
Regeneration as a Difficulty for the Theory of Natural Selection: Morgan's Changing Attitudes, 1897–1932

Lilian Al-Chueyr Pereira Martins

7.1 Introduction

Darwin's proposal, as presented in the sixth edition of the *Origin of species,* included several complementary lines of work. He tried to provide evidence for evolution as a fact; he attempted to explain the causes of organic evolution; he studied the role of those causes in particular cases; he tried to reply to objections to his ideas; he pointed out promising lines of research; and so on. Darwin conceived that natural selection acted on slight, continuous variations that were transmitted to the offspring. Not every individual that is born is able to survive and to produce descendants, because there is a limit (food and space limitations) to the increase of living beings. There is a struggle for existence, and those individuals who have some slight advantage over their competitors will have a higher probability of surviving and producing offspring. Those useful features are hereditary, they will be transmitted to the offspring and this will lead to a gradual change of the population. The species would be transformed slowly and gradually. Darwin also admitted the existence of sudden (discontinuous) variations, both in cultivated plants and in animals, but he did not deem this phenomenon as relevant to the evolutionary process. Natural selection is only able to explain adaptative ("useful") features. Darwin also admitted other natural causes, such as sexual selection (to account for "beauty" and secondary sexual features), the direct action of the environment, and the inheritance of acquired characteristics obtained by use and disuse. He also proposed a mechanism for the transmission of such characteristics (the hypothesis of pangenesis). These are in short what could be deemed the main features of Darwin's theory of evolution.

Not all scientists accepted Darwin's theory of evolution in the early twentieth century and even among the ones who accepted it, some of its features were rejected. The principle of natural selection was being challenged. One of its strongest

L.A.-C.P. Martins (✉)
Programa de Estudos Pós-Graduados em História da Ciência (Pontifícia Universidade Católica de São Paulo); Grupo de História e Teoria da Ciência (Universidade Estadual de Campinas), Brazil
e-mail: Lacpm@uol.com.br

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advocates was August Weismann (1834–1914), who proposed that selection could act in different levels including the microscopic and sub-microscopic ones. Other authors, however, criticized this principle, such as the American zoologist Thomas Hunt Morgan (1866–1945).

Although Darwin did not claim that all features of living beings could be explained by natural selection, some of his followers (such as Alfred Wallace and August Weismann) suggested that this was the case,[1] and that was regarded as a point open to refutation:

> For the opponents of the natural selection theory to show that any fact of nature is inexplicable on the basis of that theory is to shatter the whole hypothesis, since as a comprehensive explanation of the method of evolution, Darwinism must be all or nothing. That there are such inexplicable facts is believed by many biologists, though it does not of necessity follow, as some have thought, that because of the all-sufficiency of the hypothesis may have been controverted, its all-importance as a significant factor in the argument can be left out of consideration. (Abbott, 1912, pp. 25–26)

Morgan, who had been previously a student of William Keith Brooks (1848–1908), did accept from the very beginning of his career that organic evolution does occur. He had first worked in the morphological tradition, trying to find the phylogenetic relationship between invertebrates. His earliest experimental work was on embryology and development – a research that led to the publication of his first book, *The development of the frog's egg*, in 1897. From this time onwards, for several years, his main subject was regeneration. As a graduate student he had already done some work on the regeneration of the earthworm, but now this became his main concern. He investigated the regeneration in some invertebrates such as *Planaria maculata*, earthworms, medusa, and crabs. After several papers on this subject, he published his book *Regeneration*, in 1901 (Allen, 1978, p. 67).

The study of regeneration had been linked to the discussion of natural selection since the publication of Darwin's *Variation of animals and plants under domestication*[2] and had recently been brought to the attention of researchers by the publication of August Weismann's *The germ-plasm – a theory of heredity* (Weismann, 1893a).[3] In the second chapter of this book, Weismann claimed that regeneration was a special adaptation produced by natural selection, and tried to explain its mechanism by his theory of the *germplasm* (Weismann, 1893a, p. 116). Morgan had a special dislike for Weismann's speculations (Allen, 1978, p. 114) and he also opposed the evolutionary explanation of regeneration. He made his point clear in his 1901 book. Morgan's involvement with regeneration triggered a broader concern with adaptation and the limits of Darwin's theory (Allen, 1978, p. 97), and 2 years later he presented a sharp criticism of the role of natural selection in evolution (Morgan, 1903a), stressing again that regeneration could not be explained by this principle.

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[1] Weismann claimed the "all sufficiency of natural selection" in his controversy with Herbert Spencer (Weismann, 1893b).

[2] Darwin attempted to explain regeneration by his hypothesis of pangenesis (Darwin, 1883, v2, pp. 357–359).

[3] The German original, *Das Keimplasma. Eine Theorie der Vererbung*, had been published in 1892.

Morgan did not oppose the theory of evolution as a whole. He accepted organic evolution as a fact, admitting that the theory of evolution was "highly probable taking into account the evidence in favour of it" and "approximately correct" (Morgan, 1903a, p. 454) Besides that, he recognized that "Darwin's theory of natural selection was instrumental in bringing about a general recognition of the older theory of evolution" (Morgan, 1903a, p. 477). However, in spite of this, he believed that the details of the evolutionary process had not been elucidated (Morgan, 1903a, p. 476) and he made several critical remarks concerning Darwin's theory.

From the end of the nineteenth century to 1932 – the year he published *The scientific basis of evolution* – Morgan's evolutionary thought was subject to several changes. Some of them have been carefully studied by Garland Allen (particularly in Allen, 1968, 1978). However, the specific criticism to the theory of natural selection presented by Morgan in his regeneration studies has not been analyzed by Allen or by other historians of biology. The present paper will address this subject, trying to elucidate Morgan's change of attitude regarding this specific critique of the theory of natural selection.

7.2 Weismann's Arguments Concerning Regeneration

In his book *The germ-plasm* Weismann devoted a whole chapter to regeneration. In the first part he deals with its theoretical explanation by the use of his hypothesis of the germplasm, and in the second part he discusses the phylogeny of regeneration.

> It may, I believe, be deduced with certainty from those phenomena of regeneration with which we are acquainted, that the *capacity for regeneration is not a primary quality of the organism, but that it is a phenomenon of adaptation*. (Weismann, 1893a, p. 114; his emphasis)

Of course, Weismann referred here to adaptation as produced by natural selection (Weismann, 1893a, p. 116). He collected evidence supporting this view, and claimed that "those parts which are most frequently exposed to injury or loss must possess the power of regeneration in the highest degree" (Weismann, 1893a, p. 117),[4] and that internal organs do not regenerate because they are not exposed to injury or loss. He also claimed that "A useless or almost useless rudimentary part may often be injured or torn off without causing processes of selection to occur which would produce in it a capacity for regeneration" (Weismann, 1893a, p. 122). The summary of his view was:

> We are therefore led to infer that *the general capacity of all parts for regeneration may have been acquired by selection in the lower and simpler forms, and that it gradually decreased in the course of phylogeny in correspondence with the increase in complexity of organization*; but that is may, on the other hand, *be increased by special selective processes in each stage of its degeneration, in the case of certain parts which are physiologically important*

[4] Darwin had already suggested that "this capacity [regeneration] is generally a localized and special one, serving to replace parts which are eminently liable to be lost in each particular animal" (Darwin, 1883, v. 2, p. 358).

> *and are at the same time frequently exposed to loss.* In all probability this view is the correct one. (Weismann, 1893a, pp. 125–126; author's emphasis)

Weismann's argument is essentially that if regeneration had been produced by natural selection, then we would expect this phenomenon to have such and such properties; those properties are indeed observed; therefore, regeneration can be explained by natural selection. He also attempted to explain what happened in the cells and tissues, according to his own theory. He did not try to analyze, however, how natural selection could produce a gradual increase in the power of regeneration of animals.

7.3 Morgan's Early Researches on Regeneration

Morgan's early involvement with regeneration was related to his embryological studies. However, possibly influenced by Weismann's work, around 1897 he began to analyze the connection between regeneration and natural selection.

In a paper published in 1898 Morgan described his observations and experiments with the hermit crab (*Eupagurus longicarpus*, (see Fig. 7.1)) made at the Marine Biological Laboratory of Woods Hole (Morgan, 1898). He studied whether there was any relation between the power of regeneration of the crab's parts and their liability to injury, as claimed by Weismann. In the case of the hermit crab, "The anterior appendages are exposed, and some of them are not infrequently lost; while the appendages protected by the shell do not seem to be often injured" (Morgan, 1898, p. 287). Accordingly, one should expect that only the anterior appendages would regenerate, when cut off.

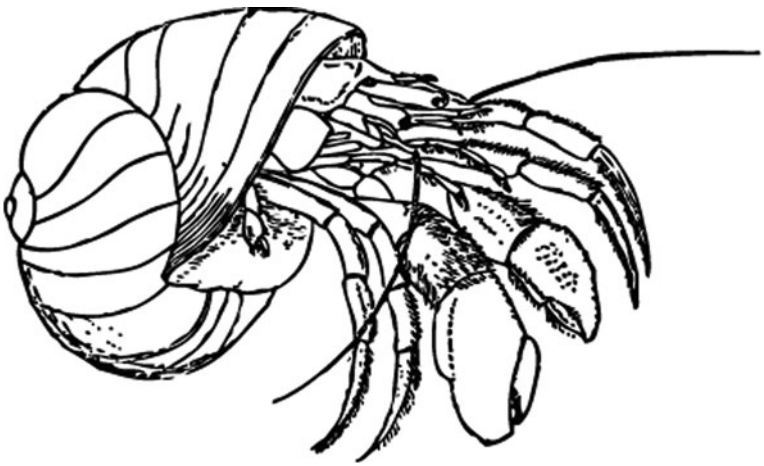

Fig. 7.1 Hermit crab (*Pagurus longicarpus*) (Public domain image available at <http://openclipart.org/media/files/johnny_automatic/11648>, reproduced from Gilman (1894)

Morgan first tried to find out the frequency of appendages lost under natural conditions. The result of examination of 100 hermit crabs showed that the first three pairs of walking legs are most often lost (in about 10% of the cases), and the last two thoracic legs were not absent in a single case; the abdominal appendages were only absent in a single case (Morgan, 1898, p. 292). The eyes were present and uninjured in all the individuals, the antennae and antennules were also present, although in some cases the ends of the antennae were broken off.

Afterwards, Morgan made two series of experiments on the regeneration of the legs of those crabs. In the first one, one or more of the walking legs were removed and the crabs were kept in aquaria with running water for a month. After that, ten crabs were killed and examined. Contrary to the expectation, he noticed that the small fourth and fifth legs possess the power of regeneration. The more anterior appendages regenerate quickly. The first, second and third abdominal appendages could also regenerate but in smaller percentage than in the case of the thoracic appendages (Morgan, 1898, p. 294). In other experiments, he observed that the eyes and antennae of the crabs could also regenerate (*ibid.*, pp. 295–296). There seemed to be no relation between liability to damage and readiness to regeneration:

> The eyes, antennules, maxillipeds, and especially the two last pair of thoracic legs do not seem to be often injured, at least, not in all the individuals that I have examined. Nevertheless, these parts regenerate as quickly and in as large proportion as the three waling legs. Moreover, the last abdominal appendages that are used to hold the abdomen in the spiral shell regenerate as readily as the more exposed anterior appendages. (Morgan, 1898, p. 298)

These and other results led Morgan to think that there is no relation between the frequency of injury of a part and its capability to regenerate (Morgan, 1898, p. 299). He also stated that "there are known already a number of remarkable cases of regeneration of internal organs, and these organs can rarely or never be injured" (*ibid.*, p. 300).

Therefore, the evidence obtained by Morgan was against Weismann's expectations grounded upon the theory of natural selection. Besides that, he stated that even if there were a correlation between regeneration and liability to damage, this could not be explained by natural selection.

> The advocates of such a view overlook a very vital part of the problem. If, for instance it were found, as the result of a large number of observations, that those animals or parts of animals that were most subject to injury had most developed the power to regenerate lost parts, it would by no means follow, as Weismann and other Darwinians claim, that this result must have come by what they call a process of natural selection. They overlook the possibility that unless these animals had from the beginning the power to regenerate they could not continue to live under the adverse circumstance. [. . .]. Many persons confuse this statement with the theory of natural selection, but the two views are as wide as the poles apart. (Morgan, 1898, p. 299)

He also challenged those persons to explain how regeneration could arise by means of variation and the survival of the fittest (Morgan, 1898, p. 287) but he did not discuss the theoretical difficulties in this paper.

7.4 Regeneration (1901)

After several research papers on this subject, Morgan published a synthesis of his results and ideas in his book *Regeneration* (1901). In Chapter 5 he presented a detailed analysis of the relation between this phenomenon and natural selection. First he discussed the view that "those individual parts of an animal that are more exposed to accidental injury, or to attacks of enemies, are the parts in which regeneration are best developed, and conversely, that those parts of the body that are rarely or never injured do not possess the power of regeneration" (Morgan, 1901, p. 92). After presenting the views of several author who supported that opinion (including Weismann), he presents evidence against it, beginning with his own researches of the hermit crab (discussed in the former section of this paper), but also adding evidence obtained by other authors, such as Newport and Schultz. He concluded:

> The results of our examination show that those forms that are liable to have certain parts of their bodies injured are able to regenerate not only these parts, but at the same time other parts of the body that are not subject to injury. The most remarkable instance of this sort is found in those animals having breaking-joints. In these forms, we find that regeneration takes place both proximal and distal to this region. If the power of regeneration is connected with the liability of a part to injury, this fact is inexplicable. (Morgan, 1901, p. 103).

He also referred to evidence of regeneration of internal organs (a fact denied by Weismann), that occurs in higher mammals (such as man) after removing of a bile duct or after the extirpation of a large piece of a nerve by a severe operation (Morgan, 1901, p. 289). This phenomenon could not be explained as the adaptation of organisms to their environment.

> It will be granted without argument that the power of replacement of lost parts is of use to the animal that possesses it, especially if the animal is liable to injury. Cases of usefulness of this sort are generally spoken of as adaptations. The most remarkable fact in connection with these adaptative responses is that they take place, in some cases at least, in parts of the body where they can never, or at most very rarely, have taken place before, and the regeneration is as perfect as when parts liable to injury regenerate. (Morgan, 1901, p. 107)

Other argument presented by Morgan against the adaptative explanation of regeneration was that in many cases this phenomenon did not produce useful parts:

> It is extremely important to observe that some cases, at least, of regeneration are not adaptative. This is shown in the case where a new head regenerates at the posterior end of the old one in *Planaria lugubris,* or where a tail develops at the anterior end of a posterior piece of an earthworm, or when an antenna develops in place of an eye in several crustacean. If we admit that these results are due to some inner laws of the organisms to its surroundings, may we not apply the same principle to other cases of regeneration in which the result is useful? (Morgan, 1901, p. 107).

Other examples included the regeneration of superfluous structures such as the production of two tails in lizards, or two or more lenses in the eyes of newts (Morgan, 1901, pp. 289–290).

Morgan argued in this book that the principle of natural selection could not explain the origin of regeneration. He regarded this case as completely different from those common situations where a useful variation can be selected. In the usual case, in a homogeneous population, a useful variation will increase the chance of survival (and/or reproduction) of an individual, and it is easy to understand that this variation could be selected and the population could change accordingly. However, in the case of regeneration, competition occurs between individuals that have lost some of their parts, and those that have not lost anything. The individuals that have undergone damage have a smaller chance of surviving, and in that case an incipient degree of regeneration could not produce any relevant effect.

> It is assumed that those individuals that regenerate better than those that do not, survive, or at least have more descendants; but it should not be overlooked that the individuals that are not injured (and they will belong to both of the above classes) are in even a better position than are those that have been injured and have only incompletely regenerated. (Morgan, 1901, p. 109)

At the end of the book Morgan concluded:

> Our preceding discussion has led to the conclusion that the phenomena of regeneration are not processes that have been built up by the accumulation of small advances in a useful direction; that they cannot be accounted for by the survival of those forms in which the changes take place better than in their fellows, for it is often not a question of life and death whether or not the process takes place, or even a question of leaving more descendants. On the contrary, it seems highly probable that the regenerative process is one of the fundamental attributes of living things, and that we can find no explanation of it as the outcome of the selective agency of the environment. The phenomena of regeneration appear to belong to the general category of growth-phenomena, and as such are characteristic of organisms. Neither regeneration nor growth can be explained, so far as I can see, as the result of the usefulness of these attributes to the bodies with they are indissolubly associated. The fact that the process of regeneration is useful to the organism cannot to account for its existence in the organism. (Morgan, 1901, p. 292)

7.5 Morgan's "Evolution and Adaptation" (1903)

The discussion concerning natural selection and evolution was just a small part of the book *Regeneration*. However, Morgan devoted much of his energy in the following years to the study of evolution, and in 1903 published *Evolution and adaptation*. In this book, following Hugo de Vries' views, he argued that Darwin's theory could not account for evolution, and that it was necessary to accept the importance of large mutations to explain the origin of species. Among many other arguments against natural selection of small individual variations, Morgan presented again in this book his analysis of regeneration.

The power of replacing lost parts and mend injuries which was of great advantage to many animals was thought as having being acquired through natural selection by Darwin and his followers. It was supposed that the individuals which have the power to replace a lost part better than others, would have a greater probability of survival, and in this way after a time the power to regenerate perfectly would be acquired. Morgan, however, presented several objections to this view (Morgan, *Regeneration,* pp. 380–381):

- It is observed that the individuals of a species are seldom injured in the same part of the body. Thus there will be very little chance for competition of similarly injured individuals in each generation to each other and the effects that are imagined to be gained as a result of it would be entirely lost by crossing with the uninjured animals.
- Since the number of uninjured individuals in each generation will be much greater than the injured ones, the former will have such a great advantage over the others that the injured ones should be exterminated. The slight advantage gained through better powers of regeneration would be of little avail in competition, as compared to the competition with the uninjured individuals.
- The power of regeneration could not have been slowly acquired through selection, since the intermediate steps would be of no use, unless their regeneration was complete; however, in this case, the selection hypothesis would become superfluous.[5]
- There are a few cases known in which the regeneration is of no use to the animal. For instance, when the earthworm (*Allolophora foetida)* is cut in two in the middle, the posterior piece regenerates at its anterior cut end, not a head but a tail. This result cannot be accounted for natural on the natural selection. Or, when the head of *Planaria lugubris* is cut just behind the eyes, it is developed another perfect head, turned in the opposite direction. Or the development of an antenna in place of eye in the shrimp, when his eye stalk is cut near its base.
- In some organisms, regeneration takes place in almost every part of the body; and in some animals, parts that are not injured do also regenerate.

All these reasons led Morgan to conclude that "regeneration cannot be explained by the theory of natural selection" (Morgan, 1903b, p. 381), and this was an important part of his general argument against Darwin's theory of evolution. He did not accept that all useful structures and functions of the organism were the outcome of natural selection since, according to him some of the results of the investigation on experimental embryology and regeneration showed just the opposite (Morgan, 1903a, p. 479).

Should the principle of natural selection be rejected, for that reason? Several authors of that time would answer "yes". A milder attitude was adopted by other authors, such as Glaser:

> No one holds that Newton's laws are invalidated because they do not explain the ultimate attributes of materials that fall, or of the space in which they fall, or why they fall in order that we observe, because every one knows, or has known, that Newton's laws are merely records of events. Natural selection is the series of events which occurs in nature as the outcome of individual differences, the high rate of increase and the environment of living things. The charge, therefore, that this series of events does not explain one of the fundamental attributes of living matter is irrelevant. (Glaser, 1904, pp. 152–153).

[5]This objection is similar to a general criticism made by St George Mivart (1871), in Chapter 2 of his *Genesis of species* and Darwin had answered it in the 6th edition of the *Origin of species* (Allen, 1968, pp. 120–121; Regner, 2007).

7.6 Morgan's Later View on Evolution

For some years, Morgan accepted de Vries' mutation theory as the best theory of evolution, and attempted to find evidence favorable to this view in his early experiments with *Drosophila*. From 1910 onwards, he did find some "mutations" in the fruit fly, but they were not large changes, producing a new species in a single step, as he expected (Allen, 1968, p. 129). However, the study of those "mutations" led to profound changes in Morgan's scientific career. Some historiographical studies show that around 1910–1911 he changed his mind concerning the Mendelian and chromosome theories (Allen, 1966; Martins, 1998). He still had some difficulties in relating those theories to evolution (Allen, 1978, p. 302), but afterwards he was able to combine the Mendelian theory and the study of slight "mutations" to the theory of evolution and natural selection.

Morgan's new views were presented in two books on evolution,[6] published in 1916 and 1925. He presented the evidence got from the experimental breeding of *Drosophila,* claiming that the slight mutations that were inherited according to "Mendel's law" furnished the material upon which natural selection would act. Besides that, "evolution takes place by the incorporation into the race of those mutations that are beneficial to the life and reproduction of the organism" (Morgan, 1916, p. 194). The action of natural selection produced the increase in the number of individuals carrying the result of a beneficial mutation. In 1925, he considered that "While in a way Darwin's theory of Natural Selection is independent of the origin of the new variations that furnish it with materials, yet the scientific formulation of the theory is intimately connected with the origin and inheritance of suitable variations" (Morgan, 1925, p. v). Morgan's main purpose in *Evolution and genetics* was to discuss about the material for natural selection, starting from the evidence got from the experimental work with *Drosophila* developed by him and his collaborators.

Several years later, in his book *The scientific basis of evolution* (1932), he presented his mature view on Darwin's theory:

> In so far as Darwin appealed to minute differences that are inherited, the principle of natural selection holds, even though these small differences are not everywhere present at all times but appear relatively infrequently as mutations. When present as mutants they would fulfill the requirements of Darwin's theory, namely, random variation and inheritance. (Morgan, 1932, p. 111)

Therefore, in his mature view, Morgan did accept the principle of natural selection, acting upon slight mutations, to explain evolution.

Since Morgan had devoted several years to the study of regeneration, and had used them to criticize the principle of natural selection, one would expect that in some of those publications he would return to this subject. However, in those later books he did not discuss regeneration nor mentioned other objections he had raised before, against the action of natural selection in this process.

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[6]The books were *A critique of the theory of evolution* and its revised edition *Evolution and genetics* in which he intended "to review the evidence on which the old theory rested its case, in the light of the newer evidence of the recent years" (Morgan, 1916, p. 7).

7.7 Final Remarks

The present analysis showed that in the beginning of his career, grounded upon the evidence he got from his experimental work with regeneration, Morgan concluded that natural selection could not explain this process. However, in his further works on evolution, he did not refer anymore to this issue. He adopted the principle of natural selection, without answering to some of his own earlier objections against it.

It may be useful to compare this attitude with the one he adopted towards the chromosome theory of heredity in the same period. Morgan had strong objections both to the Mendelian principles and to the chromosome hypothesis until 1910. In the years 1910–1911 he changed his mind, and a few years later he became the main supporter of the chromosome theory of heredity in publications such as *The mechanism of Mendelian heredity* (1915). In this book, together with Sturtevant, Muller and Bridges, he presented evidence for the chromosome theory, mostly grounded upon their studies of *Drosophila.* In this and other later works, he did not mention his previous objections or problems related to the theory, although some of those objections had not been answered by himself or by other researchers. In the case of the chromosome theory, this attitude can be explained as a professional strategy, devoting his effort to a successful line of research, notwithstanding foundational problems (Martins, 1998). It is likely that the same occurred in the case of the theory of evolution: he did not solve the problems posed by himself against the theory of natural selection, but nevertheless adopted it in his later work because it led to fruitful results.

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Chapter 8
Jean Antoine Nollet's Contributions to the Institutionalization of Physics During the 18th Century

Cibelle Celestino Silva

8.1 Introduction

It is a commonplace to regard eighteenth century as the triumph of the Newtonian scientific program. However, in the past few years, historians of science have increasingly acknowledged that eighteenth-century science cannot be resumed as the age of Newtonianism.

In the case of experimental physics, assuming that Newton's world views prevailed throughout the eighteenth century is a naïve historiographic interpretation. At this period, different areas of science were not clearly defined and well developed in the same extension. One cannot deny the influence of Newtonian studies on optics and word view on celestial mechanics studies all over the Europe, however, in order to develop a broader apprehension of modern science development it is necessary to look upon other realms and avoid focusing too intently upon Newtonian celestial mechanics.

Electrical studies developed in Europe along the eighteenth century did not adopted clear methodological programs and it was not rare to find natural philosophers with ambiguous positions using Newtonian and Cartesian ideas. As an example of this statement, the present paper discusses some of the main conceptual and methodological contributions to electrical studies made by the French abbé Jean-Antoine Nollet (1700–1770) dialoguing with the social and cultural contexts of eighteenth century France.

8.2 Restricting the Scope of Physics

There are several excellent works on eighteenth-century history of science with different historiographic perspectives (for instance, Wolf, 1939; Rousseau and Porter, 1980; Hankins, 1985; Frängsmyr et al., 1990; Porter, 2003), but few of them

C.C. Silva (✉)
Institute of Physics of Sao Carlos, University of Sao Paulo, Sao Paulo, Brazil
e-mail: cibelle@ifsc.usp.br

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emphasize Nollet's contributions to the institutionalization and popularization of physics and the relevance of his electrical studies for the period.

In order to grasp what we currently understand as physics, it is essential to take a closer look at the changes and developments in the eighteen century science. The great advance and institutionalization of experimental investigation, as well as the popularization of science in the period, opened new perspectives for science in general and for physics in particular.

At the beginning of eighteenth-century public view of science changed significantly. The social interest for the scientific knowledge and popularization of science was a new social phenomenon which can be explained by several reasons. The utility of science was evident by its putative contributions to social and material development, but it was not the only reason for the increasing interest in science; in addition, its detachment from religion and politics due to its supposed objective and value neutral character fitted very well with the Enlightenment atmosphere.

Along the century, the expansion of the range of scientific investigation and the clear utility of the scientific knowledge, allied to its diffusion, reverberated in all society. The construction of new instruments, the invention of new techniques of measurement and the betterment of already existing technologies related navigators, traders and other financiers with the natural philosophers (Stewart, 1986, p. 52). Achievements like the solution of the problem of longitude, design of reliable maps of colonies, improvement of agricultural, mining, chemical and metallurgical techniques, construction of better ships and better guns contributed to an increment of interest towards scientific wonders and technological developments. Science became less qualitative due to, among other reasons, the considerable development in design and construction of scientific instruments that allowed more accurate experimental verifications of hypotheses.

To some extent, knowledge about natural world had always had a place in courtly and commercial extracts of European society, however, this new social phenomenon is also related to recent and gradual transformations in education in some European countries at the period, with more people attending schools and having access to basic scientific knowledge. Knowing and understanding science was taken as an extra item for one being considered as a polite citizen in eighteenth-century society (Shapin, 2003, p. 167–170). Thus, the publication of books with scientific content towards a broader public, for instance, the *Élemens de la philosophie de Neuton* by Voltarie published in 1738 and *Il newtonianesimo per le dame* by Francesco Algarotti published in 1742, and the increasing number of public lecturers satisfied this new social need.

The public lectures addressed an audience that was no longer limited to aristocracy and science academies members. Thus, public lectures were the vehicle by which difficult mathematical aspects of natural science were made comprehensible to a wider public. Throughout the eighteenth century, the lecture demonstration reached audiences ranging from royalty and nobility to the most humble citizens (Turner, 2003, p. 516). Promoters of lecture demonstrations and public shows expanded the appeal of scientific instruments and experiments and strongly contributed for the popularization of natural sciences.

Nollet, for instance, used in his demonstrations about 350 different instruments in order to entertain a wide audience composed of men and women of all ages, from the capital and countryside. The lectures were very well illustrated with several impressive demonstrations, novelties and hints on how to reproduce them. Nollet's expositions were clear and it was his intention to make the lectures interesting and accessible to as many as possible attendants.

Throughout the late seventeenth century, the scope of physics started to change towards a subject that resembles what we understand today. In some of the more progressive universities, non-Aristotelian ideas were inserted in the curriculum. A change of teaching of physics style came along with new ideas about nature. The change already appears in the popular *Traité de physique* of Jacques Rohault (1620–1672), first published in 1671, in which Cartesian physics was not expounded with methaphysical stress, but as an experimental science. This new emphasis was adopted by several new textbooks in the early eighteenth century used in Oxford, Cambridge, Paris and Leiden.

Together with the Dutch and English disciples of Newton, Nollet was one of the natural philosophers who contributed to the definition of what we understand today as physics and its experimental character. The Dutch texts played a major role in defining the scope of physics by omitting botany, zoology, anatomy and physiology. They strongly influenced Nollet whose *Leçons de physique experimentale* published in six volumes between 1743 and 1748,[1] reprinted and translated many times, confirmed the ingoing new definition of physics. The *Leçons* addressed topic as the laws of motion, simple machines, static, hydrostatic, pneumatics, heat, light, optics, sound, magnetism, electricity and solar system (Home, 2003, pp. 354–58).

In mid-eighteenth century, the study of electrical phenomena was the leading branch of experimental physics. The intensive study – and public demonstration – of shocks and other effects made possible by large machines and by the newly discovered Leyden jar was a highlight of that time. Nollet was one of the leading experimental researchers of the period. He was a successful writer and lecturer, and created several new instruments and demonstrations to exhibit striking electrical effects. In 1745 he published his explanation of electrical phenomena that was widely accepted not only in France but worldwide.

8.3 Jean-Antoine Nollet: A Short Biography

Jean-Antoine was born into a humble family on the 19th of November, 1700 in Pimprez, a village about 95 km to the north of Paris. The priest of the town

[1]There is a controversy over the years of publication of these volumes. Heilbron, following the *Catalogue Général des Livres Imprimés de la Bibliothèque Nationale*, states that the six volumes were published between 1743 and 1748 (Heilbron, 1981). Whilst the leading biographer of Nollet, Jean Torlais, says that the first two volumes were published in 1743, the next two in 1748, the fifth in 1755 and the last in 1764 (Torlais, 1954, p. 257). In his doctoral thesis, Ramez Maluf Bahige consulted original reviews on the *Leçons* and *Académie* approvals of the volumes published in 1755 and 1764 (Maluf, 1985, p. 175). As I have not had the opportunity to consult the original first editions of the volumes, I cannot take sides on this issue.

Fig. 8.1 One of the few portraits of Jean-Antoine Nollet

recognized the talent of the boy and insisted with his father that he was sent to study in Clermont at the age of fourteen, and, then, theology in Paris. The novelties and cultural effervescence of Paris in early eighteenth century enchanted the young Nollet. So that he abandoned the ecclesiastical career in 1728 after finishing the course in theology and becoming a deacon in 1724. He joined the *Société des Arts*, a group of intellectuals dedicated to bringing the arts and sciences to the artisans (Torlais, 1954, pp. 11–19).

It was at this time that Nollet's abilities attracted the attention of important members of the *Académie des Sciences* like Charles François de Cisternay Dufay (1698–1739) and René-Antoine Ferchault Réaumur (1683–1757). Nollet worked with Dufay from 1731 to 1735, learning laboratory techniques and Cartesian approach to physics. Dufay, already a member and contributor of the *Académie,* was involved in experiments with electricity which resulted in his famous six memoirs on electricity of 1733 and 1734 published in *Mémoires de l'Académie Royale des Sciences.*

In 1732 Nollet was hired by Réaumur as the responsible for his prestigious laboratory. Nollet collaborate with Réaumur on several projects including the improvement of the thermometer, camera obscura and a lens griding machine. Nollet was also responsible for the construction of instruments for Réaumur's laboratory (Maluf, 1985, p. 4).

In 1734 and 1736 Nollet traveled to England and the Netherlands accompanying Dufay as his assistant. During the visit to England, Nollet had his introduction to British Newtonian scientific circles and was made a Fellow of the *Royal Society of London* by the famous lecturer John Theophilus Desaguliers (1683–1744). During his trip to the Netherlands he met the brothers Jan (1687–1748) and Pieter (1692–1761) van Musschenbroek and Wilhelm Jacob 'sGravessande (1688–1742) (Torlais, 1954, pp. 31–33).

In 1738 he was called to the Court of Turin where he worked for 6 months offering physics lectures to the Duke of Savoy; in 1739 Nollet joined the *Accademia delle Scienze di Torino.*

When he was back to Paris, his excellent lecturers and his skills as instrument maker established his reputation among French circles. In 1739 he was appointed to a position of *adjoint mécanicien* previously occupied by Georges Louis Leclerc, Comde de Buffon, (1707–1788), at the *Académie des Sciences.* In 1744 Nollet entertained the Dauphin and the Queen of Versailles and was appointed in 1758 by Louis XV the official physics tutor of the royal family. He was the first professor of physics at the Collège de Navarre of University of Paris (1756); lecturer at the military schools of La Fere and Mézières (where Coulomb attended his classes). In 1758 Nollet was appointed *pensionnaire* at the *Académie,* replacing a position vacated by the death of Réaumur. He was elected director of *Académie* in 1762. During this period, he continued to devote time to public lectures, scientific instruments trade and publication of new memoirs and books. Nollet died on April 24, 1970, while not rich, financially comfortable and was buried at Pimprez as he had requested (Maluf, 1985, pp. 15–17).

8.4 Nollet and Experimental Physics

Nollet frequently stressed that the progress of science, particularly experimental physics, should be based on experimental testing of hypotheses. Like many of his contemporaries, he condemned the construction of systems supported only in thought and logical deductions. It does not mean Nollet denied the Cartesian system, however, for him the provided explanations should be tested experimentally.

Up to the time Nollet was admitted as *adjoint mécanicien* at the *Académie*, in 1739, his only publication was his manual of experimental physics, the *Programme ou idée générale d'un cours de physique expérimentale avec um catalogue reaisonné des instruments qui servant aux Expériences* (1738). This book was meant to allow others to repeat his experiments, to guide themselves through further readings, and to promote his instruments among potential buyers. The *Programme* was well received and the interest in Nollet's lectures continued to increase in Europe.

In 1743 the first two out of six volumes of Nollet's *Leçons de physique expérimentale* were published and were well received by the public and the savants. They were translated to several languages and some of its volumes were reissued about ten times. In the *Leçons* Nollet followed the same general outlines of the *Programme*, except for the treatment of electricity.

Besides the *Programme* and *Leçons*, the French abbot published several other books and papers with strong experimental character on different branches of physics. His last work was published in 1770, *L'art des expériences, ou avis aux amateurs de la physique, sur le choix, la construction et l'usage des instruments; sur la préparation et l'emploi des drogues qui servent aux expériences.* The *L'Art des expériences*, published in three volumes, shows clearly his deep knowledge and expertise on construction and use of scientific instruments in all branches of physics.

In this work Nollet discussed in details technical and experimental issues related to conservation and use of the instruments designed and built by him and other major manufacturers of the period.

With the death of Dufay in 1739, Nollet began to be considered the most prominent French electrician. Nollet always included electricity in his lectures; however, until February 1745 he basically repeated the experiments used by Hauksbee, Gray and Dufay. He became more interested in electrical studies after he knew the impressive phenomenon of ignition of sparks by alcohol produced by Georg Matthias Bose (1710–1761) in Wittenberg. Three months later, Nollet read to the *Académie* the paper "Conjectures sur les Causes de l'Electricité des Corps" presenting his theory of affluent and effluent currents of electrical matter to explain electrical phenomena.

The ideas in this work were influenced by basic Cartesian ideas and by the experimental results previously described by German physicist as Bose, Christian August Hausen (1693–1743) and Johann Heinrich Winckler (1703–1770) and by the French pioneers on electrical studies as Dufay. There were an enormous number of electrical phenomena described and several non unified explanations to them. Nollet devoted this paper to describe new phenomena and to find an explanation for all of them. Following Descartes, Nollet defended that attractions and repulsions were caused by direct contact of the electric matter involving the bodies. Contrarily to French Newtonian physicists of the period, Nollet avoided talking about "force" of attraction or repulsion in the *Conjectures*. In order to understand Nollet electrical theory one must keep in mind that, for him, all electrical phenomena should be explained by contact forces:

> *On ne peut pas dire non plus que les effets de l'électricité viennent d'une attraction générale & commune à toutes les parties de la matiére; outre que ce principe n'est adopté que par une partie du monde Physicien, qui n'est pas même la plus grande, ceux que le soûtiennent avec le plus de chaleur sont obligez de convenir qu'on ne peut appliquer avec quelque vrai-semblance les attractions aux phénomènes dont il s'agit, sans faire une violence manifeste aux lois qu'on leur attribue, & selon lesquelles on suppose qu'elles agissent dans le méchanisme ordinaire de la Nature. (*Nollet, 1745, *p. 110)*

Nollet considered that the electrical phenomena were caused by the movement in opposite directions of two currents of electrical fluid, which is present in all bodies, in all circumstances. Part of this fluid escapes through the pores of an electrified body, causing an effluent current, while the loss of electrical matter is compensated by an affluent current:

> *Je conviens donc pour les raison que je viens de rapporter, que la matiére éléctrique s'élance réellement du dedans au dehors des corps éléctrisez, & que ces émanations ont un mouvement progressif & sensible jusqu'à une certaine distance; mais j'ai des raisons tout aussi fortes pour croire qu'une matière semblable se porte de toutes parts au corps éléctrisé, & qu'elle y vient non seulement de l'air environnant, mais aussi de tous les corps, même le plus denses & les plus compactes, que se trouvent dans le voisinage. (*Nollet, 1745, *p. 124)*

Nollet's two current theory was immediately accepted by Bose in Germany and by William Watson (1715–1787) in England. In few months, the French abbot ideas

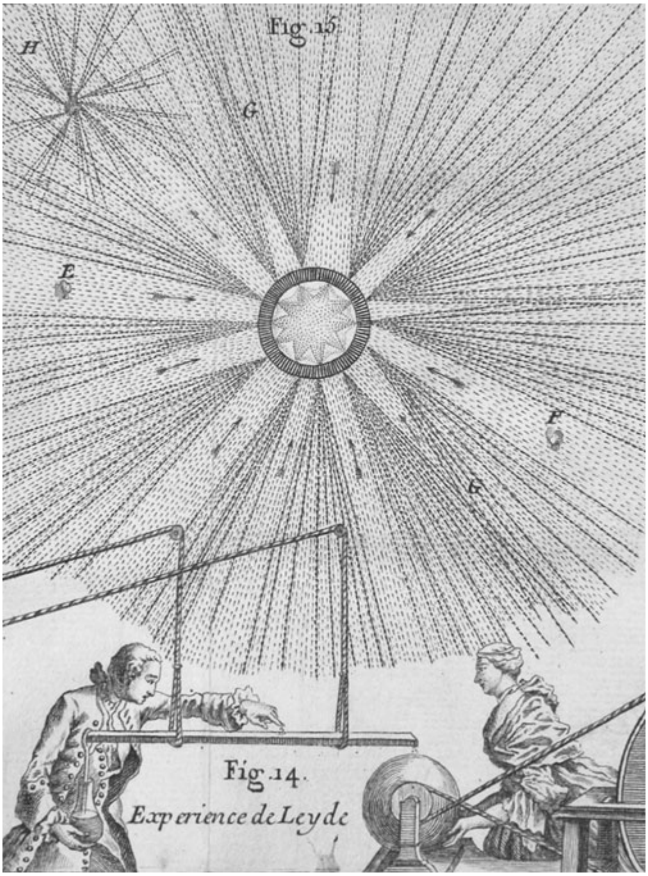

Fig. 8.2 Nollet's effluent and affluent currents capturing light objects towards and outwards an electrified body (Nollet, 1753)

constituted the main theoretical framework used to interpret the electrical phenomena in the decade of 1740. From the second half of the eighteenth century, the ideas of Nollet began to be strongly questioned, mainly by the difficulties in explaining the operation of the newly discovered Leyden jar (Heilbron, 1999). In 1752 Benjamin Franklin (1706–1790) proposed an alternative explanation based on the existence of only one fluid, which became predominant. Furthermore, questions such as what the basic principles of physics are, how to get to them, the relations between these principles and observations and their limits formed the background of the debate between the different theories proposed to explain the operation of this new device.

This kind of discussion was already present in the contention between Newtonians and Cartesians about the movement of the heavenly bodies. Thus, it can be said that the debate between electrical theories is a continuation of one started many years before and that was one of the major philosophical and scientific debates during the eighteenth century.

8.5 Nollet, a Cartesian or Newtonian?

Most historical studies on the eighteenth-century science portray the triumph of Newtonian natural philosophy, with Newtonianism first triumphing in Britain, then in the Netherlands a decade or two later and in France in late 1740s (Brunet, 1931, Priestley, 1966, Cohen, 1966). However, in the case of experimental physics, this interpretation is rather simplistic, particularly regarding the studies on electricity developed in France, where the Cartesian rationalism was still influent in the period. The position of Nollet in this scenario is not consensual among historians of science; for instance, I. Bernard Cohen portrays him as a Newtonian, while Roderick Weir Home as a Cartesian (Cohen, 1966; Home, 1981). In this section, I briefly discuss the epistemological position adopted by Nollet, who was one of the leading savants in France in the period.

Throughout the first half of the eighteenth century, French natural philosophy was marked by a debate over method in general and also specific issues about natural world. Descartes philosophy entered France and parts of Continental Europe not without critics, among them Christian Huygens (1629–1695) and Edmé Mariotte (1620–1684). In spite of criticisms, the mechanical philosophy and its view of nature dominated French scientific thought. When Nollet studied in Paris, Cartesian mechanical philosophy was consolidating its victory over Scholasticism at University of Paris. Whilst in the eighteenth-century Britain, the Cartesianism was replaced by the Newtonian system, which radically differed from Scholasticism and Cartesianism. Differently from Descartes, Newton emphasized the need for a system based not on logical deductions but on mathematical deductions and experimental evidences, clearly differentiating the philosophy of other fields of natural philosophy.

The confrontation between these systems was one of the main issues of European intellectual life in the first half of the eighteenth century. Along with national rivalry, the contention between the two systems reflected distinct conceptions of the scope and extent of natural philosophy. For the Cartesians, their system provided a consistent model based on the mechanical nature of particles in motion applied to explain all natural phenomena. For them, innovations of Newtonian system as gravitation, with no clear mechanical basis, was a return to scholastic occult qualities. For the Newtonians, on the other hand, the Cartesian philosophy was not valid for being based on rational explanations and not on mathematical and experimental statements and sounded as “philosophical romance” due to its reliance on verbal explanations (Gascoigne, 2003, p. 287).

Nollet was familiar with the debate and developed his own views on the adequate methodological approach to be followed by a natural philosopher. Although Newtonianism was not taught at University, there were several publications and courses on the new science available to him. Living and working when French science was characterized by a debate between Cartesianism and Newtonianism, Nollet advocated and practiced a style of physics that was outside this debate, at same time, he accepted ideas from both sides. For instance, he accepted Newton’s optical theory and use of gravitation theory to explain the movement of celestial bodies and was in favor on what he considered as experimental approach to Newton's physics. On the other hand, Nollet's physics was based on mechanics of impulsion and he pursued a style of experimental physics that he believed avoided commitments with either of the two philosophies.

Even in France, many natural philosophers adopted, often implicitly, an intermediate position, especially those engaged in studies of fields with strong connotations. Neither in *Programme* nor in *Leçons* Nollet attempted to contrast his experimental philosophy with Cartesian or Newtonian physics. In fact, he avoided to explicitly take sides on controversial issues like the fall of bodies, movement of planets, cause of tides, among others; he simply stated that he was exposing the most probable opinions based on experimental evidences (Nollet, 1738, p. 81; Nollet, 1753, p. xviii). For him, experience must be consulted and not a particular philosopher:

> *Pénétré de respect, & même de reconnoissance pour les grands hommes qui nous on fait part de leurs pensées, & qui nous ont enrichis de leurs découvertes, de quelque nation qu'ils ayent vécu, j'admire leur génie jusques dans leurs erreurs, & je me fais un devoir de leur rendre l'honneur que leur est dû; mais je n'admets rien sur leurs parole, s'il n'est frappé au coin de l'expérience. En matière de Physique, on ne doit point être esclave de l'autorité; on devroit l'être encore moins de ses propes préjugés, reconnoître la véritté par-tout où elle se montre, & ne point affecter d'être Newtonien à Paris, & Cartésien à Londres.* (Nollet, 1753, p. xx-xix)

In general, Nollet considered Newtonian physics with more sympathy in the *Leçons*, however with reservation. His main criticisms to Newtonian system used to have Cartesian roots because they were related to the attribution of attractive virtues to matter. Thus, labeling eighteenth-century natural philosophers as "Cartesian" and "Newtonian" may be a pitfall. In the case of electrical studies developed in France during the period, both systems were influent and the Newtonian dominance was a much slower process than in rational mechanics and astronomy.

Finally, to define the eighteenth century as the triumph of Newtonianism ignores the significance of other schools of thought and suggests a linear history of science, which is not corroborated by the study of history of electricity in the period, particularly the contributions of Nollet.

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Chapter 9
Natural Kinds as Scientific Models

Luiz Henrique Dutra

The concept of natural kind is center stage in the debates about scientific realism. Champions of scientific realism such as Richard Boyd hold that our most developed scientific theories allow us to "cut the world at its joints" (Boyd, 1981, 1984, 1991). In the long run we can disclose natural kinds as nature made them, though as science progresses improvements in theory allow us to revise the extension of natural kind terms. That is how we discovered that whales and dolphins are not fish. Boyd develops his scientific realism based on Kripke's (1980) and Putnam's (1975) theories about natural kinds. So according to Boyd natural kind terms are rigid designators.

Thomas Kuhn, in his turn, in some of his papers collected in *The Road since Structure*, criticizes the realist view.[1] According to Kuhn natural kinds change with changes in lexicon, i.e. the taxonomic vocabulary scientists accept along with some theory. In other words, the activity of identifying natural kinds is theory dependent; so we don't discover where the "real joints" of nature are. Different lexicons express just different ways of organizing experience. Notwithstanding, Kuhn is not apt to hold a radical relativistic doctrine. He adopts instead a neo-Kantian stance. And I mention this just to stress that Kuhn looks for an alternative view to both relativism and realism.

Kuhn's view of natural kinds is comparable to Quine's, who tried to reconcile realism – in "On What There Is" (Quine, 1953) – with relativism – in "Ontological Relativity" (1969). In addition, in "Natural Kinds" (1969) Quine talks about our ability to identify natural kinds. But the kinds we discover are not immutable collections made once and for all by nature itself, independently of our theories, as Quine makes clear.

In this paper I shall put forward the view that natural kinds are scientific models. In order to develop my alternative view I take some of Quine's and Kuhn's ideas, on the one hand, and a topic discussed by the proponents of the semantic view (namely, the interpretation of scientific theories in terms of models), on the other

L.H. Dutra (✉)
Federal University of Santa Catarina, and CNPq, Florianópolis, Brazil
e-mail: lhdutra@cfh.ufsc.br

[1]Cf. Kuhn, 2002. Cf. also Kuhn, 1990, not included in *The Road since Structure*.

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hand. As to the latter point, however, I argue that scientific models are different from mere *interpretative* (i.e. set-theoretic) models.

I discuss first some general issues concerning natural kinds, such as the very notions of *kind* and *individual*. Then I put forward a complementary criterion to Quine's criterion of ontological commitment. According to my criterion of *ontological density*, given a certain scientific theory we can identify not only individuals and kinds, but events as well. Finally, I comment on scientific models as abstract replicas and on natural kinds as scientific models.

9.1 Kinds and Individuals

Quine adopts in "Natural Kinds" a naturalistic stance and suggests that our ability to identify natural kinds has survival value, and similarity is the central notion to be evoked. Similarity is responsible for our bringing together different (but similar) individuals. So similarity is also to be viewed naturalistically. There must be more (or less) relevant similarities between individuals. From a naturalistic viewpoint, a green parrot is more similar to a red parrot than it is to a green apple. Reasoning this way, we might end up viewing similarity, in its turn, as dependent on our knowledge of natural kinds. We can answer, for instance, why parrots of different colors are more similar to each other than a green parrot is similar to a green apple. The reason is that parrots and apples belong to different kinds; and green things are not a "natural" kind. For Quine we have innate patterns of similarity, which dispose us to identify the relevant aspects to be considered in order to bring together some natural individuals.

The problem put this way presupposes that apparent aspects of things are relevant to bringing them together just in the first stages of the activity of organizing the objects of experience. As our knowledge of natural individuals progresses, apparent similarities give way to (more) "theoretic" sorts of similarity. That is how we came to know that color may not be relevant to identifying kinds of parrots, but that it may still be relevant to identifying kinds of (some) precious stones, say. In the long run, all relevant aspects we point out in order to compare individuals tend to become theoretic in this sense. This is a point also made by Quine in "Natural Kinds."

At this juncture some of Kuhn's ideas come to the fore. The features identified as relevant to comparing natural individuals are paradigm or theory dependent. Although nature itself may initially suggest some common traits of natural individuals as more salient than others (as suggests Quine), rapidly we enter the realm of theory. For Kuhn, who goes farther than Quine in this direction, natural kinds are not essentially different from social and manufactured kinds. Our common view is that in a certain sense social kinds (all sorts of institutions, such as workers unions, social clubs, corporations, nations, etc.) are also sorts of socially "manufactured" kinds (like tables, books, and computers). In this sense, social and manufactured kinds are totally theory dependent; they are implicitly or explicitly defined by ourselves as we organize our environment, social and natural. But for the scientific realist and for the naïve, commonsense realist natural kinds couldn't be that arbitrary.

Kuhn is aware that there are important differences between natural kinds, on the one hand, and social and manufactured kinds, on the other. That is why he tries to find a compromise between realism and relativism. If natural kinds are not arbitrary collections, social and manufactured kinds are not arbitrary either. In this connection, history makes the difference. Social and manufactured objects and their kinds are not created out of the blue. There are social (i.e. culturally and historically) established criteria for such kinds. That is why we can say that Comte's *religion of humanity* is not a real religion, and that an e-book is still a book, though it is not made out of paper, and ink, and glue (my examples). The socially constructed concepts of a book and of a religion have different histories and different criteria of application.

Kuhn's view of natural kinds – more than Quine's – tends to construe natural kinds also as socially constructed and theory dependent concepts. But natural kinds have two sorts of histories. First, as theory dependent concepts, natural kind terms have their histories – an idea developed by Nelson Goodman.[2] But there is a second sort of history of a natural kind. Not its term, but the natural kind itself has its own history in nature. Quine is much more concerned with this point than Kuhn.[3] Evolutionarily speaking, our innate capacity to identify natural kinds developed hand in hand with the evolutionary history of natural kinds themselves. As for Kuhn, at this juncture, it would be necessary to accept that translations between two lexicons would imply at least a partially common history of experiences of natural kinds on the part of scientists belonging to different paradigms. And this wouldn't do in his approach.

These comments suffice to call attention to the *natural* and *social* (i.e. theoretic) aspects of natural kinds. But natural kinds are also made out of individuals. And as to individuals those natural and social aspects of the question could be equally discussed. The problem of identifying individuals seems more complicated than that of identifying kinds, as history of metaphysics witnesses. And Quine is one of those who offer us a new and skilled way of coping with the old philosophical problem, namely his criterion of ontological commitment, put forward in "On What There Is" (1953).

According to such a criterion, as we talk about the individuals we bring together (for instance, blue, red, green, white parrots) we suppose that each parrot is an individual in the world described by our theory of parrots. Suppose color is a salient feature of parrot-hood and that we revise our theory so as to construct new kinds, namely blue parrots, red parrots, etc. Such new kinds contain also individuals, obviously. As we talk about these new different kinds of parrots (sorted according to

[2]Cf. Goodman, 1983. Cf. also Quine's comments in "Natural Kinds" about Goodman's problem and its solution.

[3]Quine has no problem in accepting Darwin's evolutionary view of natural kinds, and, obviously, Kuhn couldn't do it, since such acceptance would amount to viewing things from the point of view of a paradigm, and in the papers collected in *The Road since Structure* Kuhn tries to keep the position of the historian of science, someone who understands and *speaks* the language of a paradigm, but who doesn't belong to it.

color), the terms "blue parrot", "red parrot", etc., stand for those new collections of individuals. Now the question is, "blue parrot", "red parrot", etc., are also names of individuals? According to Quine's criterion, there is a direct, simple answer to that question: if we talk about kinds of *kinds of parrots* (blue, red, green parrots, etc., as separated kinds), then *blue parrot* and *red parrot* are individuals from the point of view of our theory of parrot-hood.

This problem is not alien to the scientific, taxonomic practice. Taxonomic sciences such as chemistry, zoology, and botany use to bring kinds together into genera, and so on, depending on the developments of their theories. So, from the point of view of scientific practice, there is no problem in creating *kinds of kinds*. But the question again is, do the taxonomic sciences take the kinds they talk about as individuals?

As for biology, evolutionarily speaking, this might be the case. Evolution theory focuses on species, and these kinds are the *individuals* which have evolutionary histories. Here the identification of individuals is deeply theory dependent. Biological individuals may be made out of other biological individuals. An organism (a mammal, say) is made out of cells, and according to the cell theory developed since the nineteenth century a cell is an organism of its own – a biological individual, too.

This question is similar to that discussed in "Ontological Relativity." Quine says that from the commonsense viewpoint macroscopic physical objects exist, but according to microphysics it is particles that exist. For microphysics, macroscopic objects are events resulting from the interactions between microscopic particles. As for physiology, the problem is rather different, since according to the same theory both the cell and the macroscopic organism exist; they both are individuals from the point of view of the (same) biological theory. A macroscopic organism is not conceived of as an event resulting from the interactions between its cells.

Thus, the combination of Quine's criterion of ontological commitment with his conception of ontological relativity doesn't solve all cases of scientifically relevant problems concerning individuals and kinds. For the theory that considers particles as individuals a macroscopic body is not an individual. But for the theory that considers cells as individuals our body is also an individual. In addition, evolutionarily speaking, the distinction between an individual and a kind is not that simple. If natural species are individuals, a kind made out of natural biological species – a genus – may still be thought of as an individual, which might evolve as well. And this depends on further developments on the theory.

The borderline between individuals and kinds is not so clearly and easily drawn. First and foremost, kinds seem to be always collections of individuals; but in certain cases kinds seem to be individuals as well. Even if it is apparent traits of similarity that allow us to sort natural individuals into kinds, as theory develops kinds tend to be progressively more *theoretical*. So, natural kinds tend to be identified with a *type*. In order to include new individuals in our kinds of parrots, if we have a well developed theory of parrot-hood, it is not color or other apparent features of individual parrots that will do. A newfound parrot must be compared not with the parrots already included in our known kinds but with our *concept* of parrot. Thus "parrot" is the name of a type. The common parrots of our experience exist in nature; but where does exist this *parrot type*?

9.2 Events and Individuals

By the middle of the nineteenth century the founding father of modern physiology, the French physician Claude Bernard, arguing against the vitalists, put forward the distinction between complex and simple facts.[4] According to Bernard the terms "phenomenon" and "property" stand respectively for a *complex fact* and a *simple fact*. A complex fact is the fact that can be reduced to simpler facts, given the analytic tools of a scientific theory. The simple fact is that fact that can't be reduced to further, simpler facts. Bernard also makes clear that a simple fact in a certain time may be later considered a complex fact, in virtue of the progress of science. For Bernard, in his days the only vital property is the irritability of the cell protoplasm. Irritability was then the only *vital* fact or *property* that couldn't be reduced to simpler facts.

A consequence of Bernard's distinction between phenomena and properties is that whenever we have a property we have also an entity – to which such property is ascribed. Bernard's view is a powerful tool to solve ontological problems stemming from science. It allows us to identify individuals and explain how they can be put in certain relations. Take any occurrence; if the analytic tools of our received theory show that such occurrence results from the relations between certain entities, given their properties, then that occurrence is depicted as a phenomenon. On the contrary, if our analytic tools can't depict such an occurrence as a phenomenon, then we must accept that we are before a simple fact, i.e. a property to be ascribed to an entity. From this viewpoint, entities are sorts of ontological *residues* of analysis, i.e. things that we can't depict as phenomena.

It is this very idea that I'd like to keep in mind in order to put forward my criterion of ontological density. According to such a view, there are two kinds of occurrences in the world described by a given theory:

(1) phenomena, events, or facts, on the one hand; and
(2) entities, individuals, or things (properly speaking), on the other hand.[5]

According to this criterion wherever there are more interesting individuals and less interesting properties from the point of view of a scientific theory, there is an event

[4] Cf. Bernard, 1879. Working on mammals Bernard discovered the glycogenic function of the liver and conceived his theory of the inner medium, i.e. the blood and other liquid media in which the cells and the whole (superior) organism can live. For details cf. Bernard, 1984.

[5] I am aware that there are a lot of philosophical disputes as to the meaning of the terms I use here. For one, Davidson (1980) argues that events are different from facts; for another, Austin (1979 [1961], p. 156), not only identifies facts with phenomena but also phenomena and facts with states of affairs. I take for granted Austin's position and avoid Davidson's.

or phenomenon; wherever there is less interesting individuals and more interesting properties from the point of view of a scientific theory, there is an entity or individual.

A consequence of my criterion is that there are two sorts of *existence*. We can make sense of the received doctrine according to which the verb "to exist" and the noun "existence" are used in two different but related senses. The apparent more general phrasings such as "there is/was an event such and such..." and "there is/was an entity such and such..." are dubious. To say that *there was a parrot on that tree* is different from saying that *there was a flying of a parrot from that tree*. In the first case the term "there was" points to an individual; in the second case "there was" points to an event.

Now, the remaining question is, what have models to do with such a criterion? The answer is that since models are constructed out of the concepts furnished by a scientific theory, models might be ways of distinguishing between the aforementioned two kinds of *existence*. In other words, scientific models allow us to distinguish between individuals and their relations – the facts or events that result from such individuals being in certain relations, given their properties.

Another consequence of my criterion is that it allows us to avoid Davidson's doctrine according to which events are individuals. From my point of view Davidson's problem is solved from the start. There are scientific reasons to distinguish events from individuals, and a simple example from physics makes this point clear.

Suppose two billiard balls, A and B, and that one of them strikes the other. Ball A moves in the direction of ball B, which is at rest from the point of view of a certain point of reference. Then ball A stops and ball B moves. For the Newtonian physicist there are here three occurrences, namely the two balls and their collision. But there are two physical individuals or entities (balls A and B) and an event (their collision). My criterion of ontological density makes sense of such a distinction. Balls A and B are physical individuals from the point of view of the theory, and their collision is an event. Both the balls and the collision exist, but they exist differently. The two balls are described by Newtonian theory as physical individuals; and their collision is described as a physical event.

9.3 Kinds as Models

The example of the billiard balls is a scientific model. It is one of the many models Newtonian physicists used to understand and apply Newtonian mechanics. Such model is a possible physical state of affairs, i.e. one of the possibilities of movement and transmission of energy according to Newtonian mechanics. In addition, other well known Newtonian models are the pendulum, the inclined frictionless plane, our solar system, etc. In all those cases the Newtonian physicist tries to conceive a setting in which physical laws apply exactly. On the one hand, such models result from abstractions we make from real circumstances. For instance, no real plane is frictionless, but we think of a frictionless plane in order to understand some aspects of movement according to Newtonian mechanics. On the other hand, the behavior of real systems – such as an inclined plane with friction which other physical systems act upon – is always taken as an approximation to the *perfect* system depicted in the model.

This concept of a scientific model is common in the literature. It is discussed by philosophers of science such as Max Black, Mary Hesse, Ernest Nagel, Carl Hempel, Frederick Suppe, Ronald Giere, and Nancy Cartwright, among others.[6] I mention here these philosophers because their views of scientific models converge in many aspects.[7] Scientific models (such as the examples aforementioned) are *abstract replicas* of real circumstances.[8] I take the term "replica" to stand for an idealized circumstance, as opposed to a physical *copy*, such as a scale model. A scale model of an airplane, for instance, is a copy of the real airplane. Some of the properties of scientific models are present in scale models, too, such as analogy, same structure, isomorphism, etc. It is in virtue of such properties that a scale model can represent the original object.

Scientific models as abstract replicas don't represent certain real circumstances in the same way as scale models do. Analogy and isomorphism are still important, but what a scientific model is supposed to represent and reproduce is the behavior of the modeled system. Take the model of a frictionless inclined plane. The parts of this system are conceived in analogy with a real inclined plane, but it is the behavior of the system what we are interested in. We construct the abstract replica in order to study the behavior of the real system. It is in this sense that Hempel (1977) talks about nomic models as opposed to iconic models. Here Hempel's view converges with Cartwright's, who talks about scientific models as *blueprints for nomological machines* (Cartwright, 1999a, b).

Scientific models as abstract replicas are not only blueprints for nomological machines; they are themselves abstract nomological machines. Take the aforementioned physical models, such as the pendulum, the frictionless inclined plane, the two billiard balls, etc. All such models are abstract or idealized circumstances conceived from the point of view of Newtonian mechanics. Not only such models represent real physical systems, but they may also be studied on their own. So, here, the idea of a scientific model comes together with the idea of a thought experiment. Many times we can't set things in the way we'd like in order to study their behavior, and then we conceive idealized circumstances in which the behavior of the system is reproduced. And we study the idealized system directly.

Scientific models are essential in the development of a scientific theory and for the understanding of the theory's concepts. But scientific models are different from the models in terms of which a scientific theory is to be interpreted according to the

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[6]Cf. Black, 1962, 1986; Hesse, 1966; Nagel, 1961; Hempel, 1977; Suppe, 1977b, 1989; Giere, 1988, 1992, 2001; and Cartwright, 1983, 1989, 1999a, b.

[7]I discussed the differences and convergences of the views of such philosophers on scientific models in Dutra, 2008. I mention here just the most general convergent aspects of such views. For details in connection with my own view of models as abstract replicas, cf. that same paper (Dutra, 2008).

[8]In this sense, the term "abstract" is also used by Suppe (1977b, 1989) and Giere (1992, 2001).

proponents of the semantic view of theories.[9] Apparently, proponents of the semantic view of theories such as Bas van Fraassen have in mind set-theoretic models – the kind of models Suppe calls *mathematical models*. Scientific models as abstract replicas are another kind of abstract structure. In order to avoid any misunderstandings Suppe (1989, p. 65ff) uses the term "physical systems" to refer to scientific models. The physical systems he talks about are abstract replicas of actual phenomena.

Natural kinds are models or abstract replicas. That is why we can say that a natural kind is represented by a *type individual*. The type individual is a model with which we compare individuals supposed to be included in the natural kind under consideration. The question as to the existence of a natural kind is solved as follows: a natural kind exists as a model – an abstract replica. Thus, we can avoid both realism and anti-realism about natural kinds. It is from the point of view of a given scientific theory that certain aspects of natural individuals are viewed as relevant in order to bring them together. As we include an individual in a certain kind (or exclude it from the kind) we compare such individual with the type individual for that kind – i.e. the model for individuals belonging to that kind. As a scientific theory changes, its models for natural kinds change, too; and we revise the extension of natural kinds, including new individuals and excluding other ones.

A natural kind as a scientific model depends essentially on scientific inquiry. It depends on what is going on in the scientific practice of a research program directed by a certain theory. The taxonomic sciences use the concepts of their theories in order to construct first the type individuals – the models – and then use such models to sort natural individuals into kinds. So, what natural kinds allow us to do is cutting the world at the joints defined by the models of a certain theory we accepted in the first place. In addition, such models for natural kinds ascribe certain properties to the type individuals representing such kinds. Thus, natural kinds as scientific models allow us to distinguish between individuals and events (i.e. relations between such individuals). In other words, each research program applies a criterion of ontological density in order to organize the world of experience as viewed by its theory.

9.4 Concluding Remarks

In what sense can we say that the position sketched in this paper is neither realist nor anti-realist as to natural kinds? Obviously, the existence of scientific models and natural kinds as abstract replicas might be questioned, too. We can ask, to say that a scientific model is an abstract replica doesn't imply that there are abstract entities? And, how could one deal with the possible Platonist solution according to which the type individual which represents a natural kind is a universal? All those questions are metaphysically relevant and might be asked as to the solution to the problem of natural kinds offered in this paper. However, the answers to them can be delayed.

[9] This is a point I discuss at length in Dutra, 2008.

It is not the existence of abstract entities that is in question here, but the existence of natural kinds. What might be the difference then?

The existence of natural kinds is a scientific problem, though it can lead to metaphysical problems. Scientific realism as to natural kinds says that natural kinds exist in nature. A sort of anti-realism (relativism) says that natural kinds are just our conceptual constructions. Viewed as a scientific problem, natural kinds are both our constructions and things existing in nature. Nature and its *joints* are defined by a scientific theory, and its models are used to sort into kinds the individuals belonging to the world described by the theory. The models belonging to such scientific practice are abstract because we can't say that they exist in nature, obviously. They are just *models* or *replicas* of what exist. But we can't deal with what exist in nature without such models.

Consider the case of language and the work of linguists and grammarians. Linguistics and grammar are also taxonomic sciences. Real individual performances are sorted into kinds of sentences, words, etc. The sentences and words linguists and grammarians talk about are abstract entities as well. We can here raise the same metaphysical questions as to the existence of sentences and words as abstract entities. But from the point of view of linguistics and grammar, such questions can be put aside for the time being. In the same way, metaphysical questions about scientific models as abstract replicas can be put aside for the time being. Notwithstanding, the scientific problem of natural kinds is given a direct, interesting solution. To say that natural kinds are scientific models as abstract replicas avoids both realism and anti-realism in what concerns the scientific activity.

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Chapter 10
On the Nature of Mathematical Knowledge

Jairo José da Silva

Philosophies of mathematics often grow around a core of philosophical dogma, and some philosophers seem more eager to squeeze mathematics into ready-made philosophical garment than understand mathematical practice. One example is, of course, Brouwer, a mathematician turned philosopher (or vice-versa) who worked hard to make mathematics fit into the rigid frame of his preconceived (mystical) ideas. As a consequence he committed the capital sin of any philosophy of mathematics, to submit mathematics to a supposedly higher tribunal of reason, with the right to impose restrictions on established mathematical methods. I take for granted that the goal of a truly scientific philosophy of mathematics is to understand the nature of mathematics *as practiced*, not to reform its practices.

Analytic philosophers too are far from free of the temptation to accommodate mathematical knowledge within the limits of cherished pet philosophical doctrines. Naturalism, which preaches that natural sciences must be the model for all sciences, and empiricism, for which the empirical way of being is a privileged way of being seem to lurk in the background of all analytic approaches to the philosophy of mathematics. It is, for instance, well accepted in analytic circles that mathematical existence must be understood on the model of (or contiguously with, whatever this means,) the existence of real objects, and that the causal theory of reference and truth is the theory against which corresponding theories in mathematics must be measured. The extension of causal theories to mathematical realms constitutes, of course, a blatant categorial mistake, but it has nonetheless been suggested, imposing the conclusion that if mathematical objects exist we cannot know them (because they are causally inert). But, obviously, the causal inertness of mathematical objects only shows that they are not *real* (physical or psychical) objects, and that we should grant them another type of existence if we believe they exist.[1] Philosophical *parti*

J.J. da Silva (✉)
Department of Mathematics, Unesp-Rio Claro, Rio Claro SP, Brazil
e-mail: jairomat@linkway.com.br

[1] To be honest, Benacerraf, who brought causal considerations into the philosophy of mathematics (cf., for instance, “Mathematical Truth”, in Benacerraf and Putnam, 1983), seems to be talking of causality in a rather loose way, as some sort of “connection” between the reasons for our belief in

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pris tends to conceal the obvious fact that the insistence on semantic uniformity along Tarskian lines for both mathematical and scientific languages obscures rather than enlightens our understanding of mathematics.

Some have tried to keep mathematics in the vicinity of the empirical sciences by reserving for mathematics, as Aristotle did, the study of the abstract aspects of *empirical* reality. But this approach fails for the same reason it failed for Aristotle: mathematical domains of knowledge are far too rich to be so construed.[2]

The preeminence accorded in empiricist philosophies to objects in detriment of higher-level entities (such as properties, forms, structures, etc.) is also a stringent limiting condition. It imposes the view that mathematics, if a science of *anything* at all, must be a science of *objects*, which – naturalism whispers – must exist somehow, somewhere. Since they cannot exist in our minds (for anti-psychologism is a matter of honor in some philosophical circles) and it is troublesome to locate them in empirical Nature, mathematical objects must exist either as platonic entities (the ontological realist view) or actual or potential concrete symbols of a language (for nominalists).[3] This object-oriented perspective can also be credited for the privilege accorded to first-order logic as the "natural" logic for the formalization of mathematical theories, when even arithmetic already speaks against it.[4]

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the truth of a mathematical assertion and the reasons for it being true. But in general, in mathematics, the reason for our belief in the truth of a mathematical proposition lies in the fact that this proposition follows by acceptable arguments from either conventional stipulations or "intuitive" truths, which coincides with the reason for it being true. The reason for an empirical assertion to be true, on the other hand, is the state of the world, and so the state of the world must, in causal theories of knowledge, be connected in a relevant way to our belief in the truth of empirical assertions. These situations are widely different; why should epistemology treat them uniformly? Such uniformity seems only desirable for those who believe in the existence of an independent world of mathematical entities, on a par with the empirical world, to where we can ascend by means of a form of perception called, *faute de mieux*, "intuition"; that is, for those leaning towards empiricism and naturalism. (Nonetheless, I believe we can still make sense of the notion of mathematical intuition, but construed in an entirely different fashion.)

[2] Aristotelism in the philosophy of mathematics claims that mathematics is only the study of some abstract aspects of empirical reality. Even if developed along these lines a causal theory of mathematical knowledge would face difficulties explaining how we can causally interact with *abstract* aspects of reality. Moreover, although we can argue that *some* mathematical domains are abstracted from our experience (provided we have a good theory of abstraction) the vast majority of them are obviously not. Analytically inspired Aristotelism is particularly troubled by the problem of abstraction. How to handle it after Frege gave it such a bad name? The way out is usually dropping Aristotelism in favor of full-fledged Platonism (which accords mathematical objects an existence just like that of real objects, but in a realm out of this world) or nominalism (symbols are, after all, *real* entities).

[3] The idea that they may be merely intentional objects – see note 5 – is not a viable alternative in analytic circles (probably because this smacks so much of psychologism to analytic sensibility).

[4] Logical formalization, although important in metamathematics, does not play a relevant role in mathematics. The extensive use of formal-logical arguments in the philosophy of mathematics then risk loosing mathematics as practiced by mathematicians from sight, substituting it with reconstructions that are almost never, if ever, adequate. If we want to understand arithmetic, for instance,

The view I want to sketch here is as remote as possible from empiricist or naturalist philosophies. Here is its main thesis: it is irrelevant whether mathematical objects, understood as object proper of the mathematical discourse, exist or not in the strictest sense of the term, i.e., "out there" somewhere, independently of us and our theories (I am convinced they do not); the object-matter of mathematical theories, the focal point of mathematics is not objects, understood as denotata of nominal terms, but the *structures* that underlie domains of objects, whatever their nature may be. These structures, moreover, are entities that in general exist only "intentionally".[5] But even so, the view goes, mathematics can be, and often is applicable; more, it is sometimes indispensable in science and our practical life. In a nutshell, mathematics is a product of rationally constrained *formal creativity*,[6] whose utility depends on its ability to offer adequate formal contexts where domains of our interest can, after being striped to their purely formal framework be immersed for methodological purposes.

Different sort of prejudices also cloud the understanding of one of the most puzzling problems for the philosophy of mathematics: how is it possible that mathematics, an a priori science, can be so relevant for the natural sciences, physics particularly, to the point that physical concepts in general cannot even be formulated independently of it? Amazingly inadequate formulations and "solutions" to this

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its heuristic methods, its ways of validation, the nature of the knowledge it provides, it would be misleading to consider only formal versions of arithmetic, no matter in which logical context.

[5]There is a way in which we can understand mathematical existence which lies somewhere between naturalistic inspired Platonisms and psychologism (which is also, of course, a naturalistic perspective). Analytic philosophers tend to confuse it with psychologism, but it is essentially different from it. I am referring to *intentional* existence. Objects of a certain type (for instance, numbers) exist intentionally to the extent that they are posited, or presupposed, by a theory (in our example, arithmetic), and as long as this theory maintains its logical consistency. Intentional existence of objects is then parasitic on the existence of a logically coherent theory of these objects. The *objectivity* of a theory, i.e. the fact that it is shared by an entire community (the mathematical community in our example) is inherited by the objects the theory posits – numbers, in our case, are objective entities for the community of arithmeticians to the extent that this community agrees that they are talking about "the same thing" when they are doing arithmetic. The moment a theory manifests an inconsistency their objects, in the word of Husserl, "vanish". Mathematical existence is then closely tied to logical consistency, just as Hilbert and Poincaré, among many, wanted. I understand that Frege is not far from this perspective. The so-called context principle, after all, tells us not to ask for the meaning of a term outside a context in which it occurs. Numbers are, for Frege, objectively existing logical objects to the exact measure that they occur as referents of numerical terms in the context of what Frege took for a logical theory, arithmetic. The hypostasis of mathematical objects occurs when intentional existence is taken for theory-independent and self-subsistent existence (the adoption of a naturalistic inspired correspondence theory of truth goes in general hand in hand with this).

[6]By this I mean that mathematical theories and the structures, forms or formal domains they characterize are in general invented by creative mathematical minds rather than imposed by pre-existing "mathematical facts". In fact, not even Euclidian geometry can be said to simply *describe* our experience of physical space or our intuition of pure space, as Kant believed (see Helmholtz 1866, 1870). Our experience of space is too coarse to impose any geometry, and there is no pure intuition of space as Kant believed.

problem have been offered, where more than philosophy one can discern outright mystical prejudices. One example is Steiner's supposedly challenge to what he calls "naturalism". According to him, the "unreasonable effectiveness"[7] of mathematics in natural sciences speaks against the "naturalistic" thesis that man has no privileged place in Nature.[8] I want to suggest here that the effectiveness of mathematics has more do to with its formal character than with pre-established harmony.

One argument that made history in the philosophy of mathematics is the so-called indispensability argument.[9] It goes more or less like this: mathematics is indispensable for science; therefore, it must be true, for otherwise how could it be useful? But mathematics is about mathematical objects, and since mathematics is true mathematical objects must exist, for otherwise how could it be true? Those who do not accept the existence of mathematical objects, but are moved by this argument, like H. Field (1980), worked hard to reconstruct relevant parts of physics without using mathematics in an essential way (or so they claim). My opinion instead is that the argument is unacceptable, for it rests on two false presuppositions. The first is that only a true theory, in some sense of truth with serious ontological consequences, can be useful. The second, that mathematics is about a specific type of objects, such as numbers, sets, etc.

I want to suggest that even theories that are not about anything existing in the "realist" sense (i.e. independently of our theories) can be useful in natural sciences, that the role the so-called mathematical objects play – nothing more than supports of mathematical structures – can be played by any objects, physical or even purely

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[7]The expression is due to Wigner (Wigner, 1960), who thought there was something mysterious and inexplicable in the mundane fact that mathematics is useful in physics ("a wonderful gift which we neither understand nor deserve"). Wigner was the first to raise seriously the question of how to account for the "miracle" of the "appropriateness of the language of mathematics for the formulation of the laws of physics". For him, mathematics is to a large extent done for aesthetic reasons, and the fact that Nature favors the language of mathematics is a wonder we do not understand (nor deserve). The number of times the word "miracle" is used in his article already indicates the frame of mind with which he approaches the problem. Steiner will later stress this mystic undertone.

[8]Steiner (Steiner, 1998) believes that, on top of offering a convenient language and a conceptual apparatus for science, mathematics can also play a heuristic role in it. More specifically, he thinks that purely mechanical manipulations of symbols can lead to findings in physics. One of his favorite examples is Maxwell's discovery of electromagnetic waves (later experimentally confirmed by Hertz). According to Steiner (see Steiner, 1989, p. 458), Maxwell realized that the equations of electromagnetism he received from his predecessors were inconsistent with the preservation of electric charge. Then, by playing with these equations, Steiner says, Maxwell hit on the notion of displacement current, its mathematical expression and the hypothesis that displacement currents also generate magnetic fields. The stage was then set for the discovery of electromagnetic waves. My first reaction to this account is the obvious one: even if it were historically accurate (which it isn't), Maxwell would not be only playing with mathematical symbols, but working out the mathematical consequences of a *physical* hypothesis, namely, that electric charge must be conserved, the truth, however, is that the concept of displacement current and its mathematical expression were natural outcomes of the *physical* model for electromagnetic phenomena Maxwell worked with (mechanical stresses and displacements in an elastic medium transferred from one point to another in finite time by contact).

[9]For a detailed discussion of this argument see Colyvan 2001b.

intentional objects, and that the utility of mathematics lies in its ability to provide formal knowledge, valid in principle in *any* material context.

I then propose to approach mathematics free of prejudices and preconceived notions induced by background philosophical ideas and theories (a sort of *epoché*) in order to answer, basically, the following question: which conception of the nature of mathematics best fits given standard mathematical practices (not reformulations or reinterpretations of them), proving in particular (which is seldom carried out in mathematics within the strict limits of formal systems[10]) and the extensive applicability of mathematics?

Not only mathematical theories, but mathematical objects can also be formal. I will illustrate what I mean with an example. What type of objects real numbers are? They are usually defined as sets of natural numbers (identifying R and 2^{ω}), rational numbers (Dedekind cuts) or Cauchy sequences; so, it seems, sets are the type of objects real numbers are (at least in modern set-theoretical obsessed mathematical foundationalism). But the mere fact that there are different ways of defining them as sets should make us suspicious that sets are what they *really* are.[11]

The concept of real number is intimately connected with the notion of continuity, in particular geometrical continuity. Historically, real numbers were introduced to express *ratios* between straight line segments (for example, $\sqrt{2}$ denotes the ratio between the diagonal of a square and its side); the system of real number standing for the totality of all *conceivable* ratios of this type (the extreme vagueness of this "conceivable" accounts for the vagueness of the concept of real number). Now, from a strictly mathematical perspective, all that interests us are the *formal* properties of these ratios, i.e. the properties they have regardless of *what* they are ratios of. So, clearly, it is no longer appropriate to think of real numbers as *well-determined* objects (a particular kind of "stuff"), but instead as formal entities of a type, namely, ratios between "segments" of no matter which continuous magnitude. This explains the wide applicability of the theory of real numbers in science and practical life. Invariably, the applicability of a mathematical theory, no matter whether we take it as being about a *particular* domain of objects or only purely formal, is related to its formal character, in one or other sense of formal.

Now, suppose for the sake of argumentation, that the arithmetic of the real numbers we actually have, instead of being the theory of the arithmetical properties of an "intuitively given" (let us also concede this for the sake of argumentation) system

[10]To think of mathematical proofs as *formal* proofs is a heritage of the logicist approach to the philosophy of mathematics. But logicism is a reinterpretation of mathematical practice, devised for strictly foundational goals, not an unbiased view of what proving in mathematics is all about.

[11]Of course, I have in mind Benacerraf's famous example (see Benacerraf, 1965). The set-theoretical translation of a mathematical theory should not be understood as an ontological reduction – as if this theory were *really* about certain types of sets –, but only as a different materialization of a collection of formal truths. The fact that mathematical concepts can be translated into set-theoretical concepts does not give sets any privileged ontological status.

of ratios (between line segments, for instance), were freely *invented* as a purely formal theory (in the sense of a non-interpreted theory, whose objects are merely intentional "something") by a creative mathematical mind.[12] Would it make any difference in terms of its applicability? It would certainly not, because this now purely symbolic theory could still be used to represent the same formal facts expressed by the theory of the supposedly intuitively given real numbers. But, let us concede, the arithmetic of the real numbers was not completely freely invented. Is there instead a mathematical theory that was, and even so is applicable?

The answer is affirmative and one example is the arithmetic of so-called imaginary numbers. These numbers constitute a field of purely formal objects obtained by formally extending the field of the real numbers by the adjunction of the imaginary unity i with the stipulation that $i^2 = -1$. The arithmetic of imaginary numbers is then a purely formal extension of the arithmetic of real numbers. This is how Bombelli and other algebraists of the Italian Renaissance who created the imaginary numbers for the exclusive sake of facilitating things in the theory of algebraic equations conceived them, and this is also how we define them in modern algebra (Gauss' model of the arithmetic of imaginary numbers in terms of displacements in the plane is important only insofar as it shows that this theory is consistent relatively to geometry. The fact so often repeated that it gives some substance to the concept of imaginary number is mathematically irrelevant).

But how is this possible, how a purely formal theory, fruit of the mathematical imagination, can be of any help for another theory, with an intuitive content? Husserl puts essentially the same question thus: "how can a mere game with symbols admit of applications?".[13] The answer depends on the fact that "playing with symbols" is not only moving symbols around, but conducting an investigation of the formal properties of formal domains, i.e., domains of maybe only merely intentional

[12] Strictly speaking, this is to a large extent precisely what real numbers are. It is doubtful we can have any clear intuition of any *definite* real number, even in the form of a definite ratio between two segments (our perceptual or intuitive powers would not be able to discern it from another ratio differing only slightly from it). A real number is an idealization, a product of the imagination, not anything "given" to us. The domain of all real numbers, i.e., the system of all conceivable such ratios abstractly considered, is even more obviously a scheme of understanding, which cannot in any clear sense be intuited or perceived. In general, mathematical theories, such as geometry, group theory, set theory, or arithmetic, may be *suggested* by our experience (experience can at beast trigger mathematical imagination), but in the end they are never mere *descriptions* of anything we simply "experience".

[13] This quote occurs in a letter to Carl Stumpf of 1890 or 1891 (Willard, 1994, pp. 12–19). Husserl's own answer to this question of paramount importance, although ingenious, is unsatisfactory. For him, a purely formal consistent extension of a theory, written in a richer language, can be used for deriving results in this theory provided it can do so, but in an inessential way; i.e. provided the theory it extends is logically complete (with respect to its own language). The answer is not satisfactory because it is unnecessarily restrictive, as I will show.

objects indeterminate as to matter, but determinate as to form; the formal properties of a domain being at least partially determinate by the formal stipulations by which it is defined.[14] The essential point to be noticed is that, as far as our interest on given domains, even materially determinate domains, concentrates on their formal properties *only*, as is typically the case when we do mathematics or empirical sciences where mathematics plays an *essential* (as opposed to merely relevant or just important) role (like physics), we will usually find it immensely useful to be able to substitute them by other isomorphic domains, even purely formal domains (where only symbolic manipulations count), where formal truths can be brought to light and then transferred back to the domains of our primary interest. Better than trying to explain what this substitution consists in, let us give an example.

This is due to Husserl (Husserl, 1891). We can think of natural numbers as common formal aspects of equinumerous collections (this idea is behind most definitions of natural numbers, e.g. Frege's) and numerical operations in terms of operations (unions, differences, etc.) with collections of units whose nature is immaterial. If we try to carry out operations with numbers "conceptually", by referring back to operations on collections, we will soon face enormous difficulties. The way out is to substitute numbers by numerals and numerical operations by symbolic operations, our usual algorithms for performing arithmetic operations symbolically. This works because the domain of numbers and conceptual operations is *isomorphic* to that of numerals and symbolic operations. We can calculate symbolically, by "playing with symbols" only because arithmetical truths are preserved under isomorphisms, i.e. they are *formal*.

Another interesting example is the use of algebra in geometry, a method invented by Descartes in the seventeenth century. Even though Descartes believed that usual Euclidian geometry deals with points, lines, planes, figures, solids and like entities, given immediately or constructively to us in geometrical intuition, he found it methodologically convenient to move from this to the numerical domain in order to prove geometrical truths by algebraic means. He did it by simply substituting points with numbers and geometrical constructions with numerical operations in a formally equivalent manner, or, in other words, by putting in place of the geometrical domain another domain isomorphic to it where algebraic methods could be used. The reason it worked is, of course, that since geometry only cares for the formal properties of the domain of geometric entities, we can, for methodological purposes, work in any domain formally equivalent with it. Geometry is only interested in the properties

[14]For Husserl, a formal domain (or formal manifold) is the "objective correlate" of a purely formal theory. In less threatening words, a formal domain is what we get by *stipulating* (a sort of mathematical *fiat*) the existence of a collection of objects (no matter which) where certain operations and relations are defined (no matter which) so that such and such hold – the such-and-such being purely formal (i.e. non-interpreted) expressions involving variables and constants for the objects, relations and operations stipulated, and maybe for higher-order entities also. In short, formal domains are the "abstract structures" of modern algebra. The concept appears, for instance, in the *Prolegomena to Pure Logic*, the first part of his *Logical Investigations* of 1900–1901.

its objects have that are shared by no matter which objects that happen to be identically structured. So, strictly speaking, geometry, like any other mathematical theory, contentual or not, is not a science of objects, but of forms or structures.

Would it matter if we did something analogous to what Descartes did, but moving to a domain that is purely intentional? I claim that it does not, for intentional domains can be created with convenient formal properties (in this resides the art of the mathematician, and also his freedom). But, of course, once a *consistent* set of arbitrary formal stipulations are set characterizing a formal domain, the only properties this domain has are those that follow from these stipulations, or convenient consistent extensions of it (this is what I mean by *rationally constrained* creativity). Complex numbers, as already noticed, are a good example of a very convenient invention. But if we pay close attention, the history of mathematics is the history of good formal inventions, sometimes suggested by the formal properties of domains given somehow intuitively to us, and sometimes produced out of sheer imagination. For example, the formal properties of permutations of numbers suggested the formal notion of group and group theory,[15] but the notion of imaginary number arose from an "irresponsible" and unjustified absolutely free decision to confer numerical dignity to senseless symbolic expressions. Poincaré and Hilbert are then right, it seems; mathematics is a free invention only constrained by the consistency requirement. These inventions can sometimes come from *abstracting* and *extending* observed formal patterns somehow given to us, by Nature herself, for example, but also sometimes out of nothing, born out of our efforts to solve practical and theoretical formal problems. The interesting thing is that it is *because of*, *and not despite* these characters that mathematics is so useful and flexible.

Transferring problems from a context to another isomorphic to it is not the only typically mathematical procedure allowed by the formal character of mathematics; one can also move from a domain to another that *extends* it, provided we can somehow transfer results from the larger to the narrower domain. Our domain of interest can, for example, be a sub-domain of the domain extending it, or the larger domain can have a sub-domain isomorphic to the narrower one, and some relevant result obtainable in the larger domain can be transferred to these sub-domains. It is, for example, a common practice to resort to the field of imaginary numbers in order to show that certain results hold for real numbers. We can, for instance, prove many non-trivial trigonometric identities involving real numbers by means of de Moivre's formula for exponentiation of complex numbers. Complex numbers are so useful in mathematics, science and technique that choosing examples creates *l'embarras du choix*. But a particularly interesting one is how the structure of Minkowski's 4-dimensioanl space-time in the special theory of relativity (in which one of the dimensions is complex) is particularly apt for expressing the condition

[15]The use of group theory in the theory of algebraic equations is a perfect example of the methodological utility of formal equivalences in mathematics. The possibility of solving an equation by radicals is related to a formal property of a group associated with the equation. Galois connections establish ways of "translating" formal properties of a domain into properties of another domain.

of equivalence of referential frames: they must be obtained one from the other by rotations.

A final example: the fundamental theorem of algebra, whose domain is the arithmetic of complex numbers, can be proved by extending this domain, not with new elements, but richer structure. We can endow the field of complex numbers with a topology so that we can talk about continuity of complex functions. By using this notion a relatively simple proof of this theorem can be given. What does this tell us about the nature of mathematics and proofs in mathematics? It tells, I am convinced, that mathematical knowledge is always formal, even when only objects of a determinate type are under consideration, and in order to obtain this knowledge we can move freely from one mathematical context to another, with more elements or richer structure, provided they are formally related in a convenient way. It also tells that proofs in the context of formal-logical systems, where such "mobility" is totally interdicted, are very poor models of mathematical proofs.

Let us say a few final words about the applicability of mathematics in science, physics in particular. An important question is why mathematics is so relevant, even indispensable in physics but not in biology, for instance. The answer involves the difference between formal and material sciences. Biology is evidently a material science, since only *determinate* types of objects are of interest (insects, mammals, etc.). For such sciences mathematics is much less relevant. On the other hand, physics is a to a large extent a formal science, which focuses mostly on formal aspects of Nature, which can, ipso facto, be *mathematically* represented. To modern physics, i.e. physics since Galileo, mostly those aspects of Nature that can be mathematized and correlations that can be mathematically expressed are of interest. This choice may be seen as metaphysically limited, but it opened horizons of technical accomplishment never dreamed before, simply because it allowed mathematical methods for modeling Nature and expressing relevant natural correlations widely available.

Steiner (op. cit.) was puzzled by the fact that formal mathematical analogies can be *heuristically* relevant. This is very hard to understand indeed if one does not fully appreciate the formal nature of mathematical (and physical) knowledge. But as soon as one realizes that these analogies point to *identical* formal properties shared by materially distinct domains, and formal properties are what physics is after, the mystery disappears. To play with symbols can lead to discoveries because playing with symbols is a way of bringing formal properties to light. Once an aspect of Nature has been mathematized, it has been reduced to its formal framework; otherwise it could have not been mathematized. The mathematical theory of any one particular aspect of Nature is a formal milieu where the formal structure of this natural domain is *embedded*; there should be no surprise, much less wonder, that purely symbolic manipulations in the larger context can lead to findings in the narrower. This would indeed be a miracle if Nature and mathematical domains were completely separate realms; but they are not, since they share common formal properties. Moreover, contrary to what Wigner and Steiner believe, mathematics is not the contemplation of beautiful structures devised for aesthetical delight, but the investigation of useful structures created for practical reasons (with aesthetic pleasure as a bonus).

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Chapter 11
The Etiological Approach to the Concept of Biological Function

Karla Chediak

The etiological approach to the concept of biological function, also called teleological or historical, aims to offer an explanation of the function, answering the question of why a trait, organ, biological system or behavior is present in the living organism to which it belongs, performing a functional role.

In general, it is accepted that the etiological conception was originally developed by Larry Wright, in his article Functions (1973). Although much has already been discussed since Wright published his article, some points can be considered common to the different approaches of etiological conception.

Firstly, the etiological approach maintains that the functional explanations which are relevant to biology must explain the presence of a trait (Neander, 1991b, p. 459). What is advocated is that the analysis which considers only the current behavior of a trait cannot explain the important distinction between function and mere effect. For example, the heart has the function to pump the blood, allowing, among other things, the transport of oxygen and the elimination of carbon dioxide, but the heart also produces noise. This is an effect that accompanies the functioning of the heart, but it is not its function. An analysis of the current behavior of a trait also does not distinguish between what is functional and what is a mere accident. For example, the nose has some functions, such as respiratory and olfactory, besides that it can also serve to support glasses, but this is an accidental effect, it is not its function. Moreover, an analysis purely dispositional of a trait neither explains the non-functioning nor the malfunction of a trait that still retains its function. The heart has the function of pumping blood, but if for some reason it does not do it, it does not change or lost its function.

Secondly, the etiological conception maintains that there is a normative aspect of the concept of function and defends that only when the normative character is recognized it is possible to understand the full meaning of a functional explanation in biology. Saying that the heart must pump the blood means that it is expected that it does it, considering its normal operation. However, it does not mean that the normative notion has a prescriptive role. The normative aspect of the function

K. Chediak (✉)
Universidade do Estado do Rio de Janeiro, Rio de Janeiro, Brazil
e-mail: kachediak@gmail.com

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that the etiological conception requires is not prescriptive at all. In fact, there are different ways to understand the notion of norm and some of them do not require that property. For instance, it may be considered only a statistical evaluation or a kind of regularity. Assuming that, in general, thunder and lightning are accompanied by the rain, we may say "it should rain this afternoon", and that sentence is not a prescriptive one, it is only an indication of what is likely to occur. It is true that the etiological conception of function does not work with that kind of normativity either, because it is possible to conceive a trait's behavior that occurs frequently, which does not determine its function. According to the etiological view, the basis required for the application of normativity to biological function is the functional features that have been originated by natural selection to do what they do. They acquired that function thanks to the role they played in the past and that would have made difference in a given population, in terms of survival and reproduction, i.e. in terms of fitness. Thus, there is a close link between the concept of biological function and the concept of adaptation. The function performed by a trait is an adaptation, in the sense that it is explained by the process of evolution by natural selection. It is the selective pressure acting on an evolutionarily significant time that would result in the formation of a biological function. This is the reason why the design provides, for the etiological approach, a teleological explanation of function without appealing to any intentional agent or purpose.

It is said that teleological explanation is a kind of causal explanation that reverses the order of normal cause, in which the causes are previous or simultaneous with the effects, but, in reality, it does not. It explains the presence of a functional trait in a biological system as the result of the action of natural selection, as an effect of evolutionary history, and this history is not teleological. The action of natural selection is not directed to any purpose; it is a blind process that operates in a very simple mechanism, requiring only change in fitness, heredity and differential reproduction. Moreover, the effects of action of natural selection fall on the population and not on the individual items, as Karen Neander explains (1991a, p. 174). The role played by traits of certain individuals in a population contributed causally to their replication and fixation in that population. Because of that it is possible to use this function to explain the presence of the item in the population, because it was a causal process that promoted their replication and fixation.

Contrary to the approach of etiological conception, which correlates function and teleology, Robert Cummins defends the proposal of excluding teleological statements from natural sciences, particularly from biology (1975, 2002). He believes that it should be given an analysis of biological function that is not teleological. The argument put forward by him originated one of the main interpretations of biological function and functional explanations. He proposes an analytical approach to the concept of function and maintains that the functional explanations answer to the question about what is the role of a trait, part of a system, in the activity of the whole system to which he belongs.

A biological system, says Cummins, can be thought of from many points of view, considering the systems it contains, such as the digestive, circulatory or respiratory. Each of them has specific capabilities and is composed of a number of traits with specific behaviors that function and contribute to the achievement of the general activity of the system (Cummins, 1975, p. 761). The ability of the system to conduct its activities can be determined by means of the analysis of the functions performed by its components. This model does not only serve to analyze biological functions, it also can be applied very well to non-living systems, such as production lines of computer systems

According to Cummins, it is not possible to determine at first which system must be considered. In the case of biological functions, it is not even necessary that the system contributes to the maintenance and propagation of their owners. It is only the relationship between the trait that has the function and its contribution to the system it belongs to that is relevant. Then it is required to determine which system is been taking into consideration. Because of that, for Cummins, it is clear that "functional analysis can be conducted in an appropriate manner in biology on an entirely independent of evolutionary considerations" (1975, p. 756).

For instance, he says that in the case of the heart, it would be its function to pump the blood, only if we are considering the circulatory system. Although it would be difficult to draw up a system where the noise produced by the heart would be functional, it would not be impossible. He claims that it had already been suggested that there would be a function for the production of noise considering a system of a psychological nature. In that specific case, it would be right to say that the function of the heart is to produce noise, because the system considered requires that function to the heart (1975, p. 762).

In fact, the analytical approach is an important tool to determine which role certain item plays in the system it belongs to. But one can question whether the analytical approach gives a full account of what means function to the biology, and if it satisfies the requirements of functional explanations in biology.

Moreover, when you take into account only the analytical approach, you can end up getting quite inappropriate conclusions in terms of biological function. Many authors present examples of such distortion, as Neander and Kitcher. The latter, for example, says that one could assign functions to mutant DNA sequences just considering the role they play in contributing to formation of malignant tumors in a human being. However, there would not be any real function in this case, and then no functional explanation (Kitcher, 1998, p. 272). Neander observes that dying of cancer involves chromosome replication and cell reproduction in the growth of tumors. However, she says, this causal role is not tumors' proper function (Neander, 1991a, p. 181). Another example would consider the possibility of a virus attack that spreads in a population. Although the virus does not eliminate that population, it changes the metabolic system of individuals in order to promote the survival and reproduction of its own members. The result is that some organs or biological process of the individuals acquire new roles, representing an important statistic data. But still, from the functional point of view, it would not be correct to say that these new roles of the organs or biological processes are functions, even though they are more frequent in statistical terms.

The etiological conception of function does not only identify the function of a trait in the system to which it belongs, but also gives an account of why that trait is there doing what it does. That explains why, even when it does not work, it maintains his function or even though it still has some effect, this is not its function. Therefore it is not enough to consider the current behavior of the trace, it must be taken into account the origin of this behavior. Only when the source of the function of a trait has been originated from the action of natural selection it is considered functional.

The requirement of a necessary linkage between function and natural selection, defended, in general, by the etiological conception has raised many issues, two of them particularly important. First, it could be questioned whether the selection is really responsible for the origin of a functional trait. Cummins, probably the strongest critic of the etiological concept of function, develops its main arguments seeking to show that it is not correct to consider that natural selection is responsible for generation of a functional trait. Secondly, it could also be questioned whether the biological function is restricted to cases where there was natural selection. That last criticism does not necessarily refuse the etiological view, but points to some difficulties of that theory (Cummins, 2002).

It is possible to dismember the first criticism into three specific issues. The first maintains that a trait could not have been selected for the role it plays, since that before the trait was there, he could not have had this function. No one could explain the presence of a heart and its function of circulating the blood without assuming the presence of the heart that performs that function. If there was selection for a heart because of its function to circulate the blood, it would be necessary that there was some hearts that did not circulate the blood, and some hearts that circulated the blood, but such a hypothesis is very implausible.

Secondly, the ancestor of the trait which is currently present in a given population was not equal to the trait that is being considered. Therefore, it would not be correct to explain the presence of the trait considering its current function, using the selection of a trait that was not him. The ancestor of the heart, for example, was not a heart, but something like the first movement of centralization of blood.

Finally, even considering that the trait had already existed in a given population, its presence could not be explained making use of its function, because natural selection would not have acted because of fact that it had or did not have this function. Selection would have acted on how the trait realized the function, selecting the trait that supposedly had the best performance. One could not appeal to the function to explain the presence of the trait, since all variants would have the same function. For instance, one could assume that, among hearts that already had the task to circulating the blood, it occurred selection for the one which shows best performance.

According to Cummins, the only situation in which could occur what is supported by etiological view would be extremely rare, but not impossible to occur. We would need to find a trait that arose with an entirely new function, which would be beneficial in terms of survival and reproduction, and yet was selected because of this function. Only in these exceptional cases it would have be selection of a trait because of its function and it would be possible to appeal to the function to explain the presence of the trait. According to Cummins, we could not appeal to this kind of explanation when we are considering complex organs such as the heart and the human eye (Cummins, 2002, p. 165). However, it would be exactly that kind of explanation the etiological conception aims to give, appealing to the notions of adaptation, fitness and design.

It is not easy to confront these criticisms; however, some observations can be made. First, the claim that the trait could only perform its function, if it was already present in the system, and because of that it is not right to appeal to function to explain the presence of the trait, does not affect the etiological conception of function. It considers the relationship between the trait and function without taking into account one of the main features of the etiological view, that is, the history of generation and fixation of the trait. It is true that there is a risk of committing a circular reasoning, that is, to explain the presence of a trait appealing to the function and to explain the function appealing to the presence of trace, if one presupposes the other. But the relevant issue for the etiological view is the generation and the fixation of a trait in a population, hence it raises a problem that is not in question here, because it already presupposes the trait and the function as given. That question can only be addressed from a historical perspective and that perspective is not at issue in the first criticism.

More serious for the etiological view is the second question which states that the ancestor of a currently trait was not equal to it and because of that it would not be correct to explain the presence of that currently trait appealing to the selection of a trait that was not him. In fact, it is not possible to determine when the organ that realized the centralization of the circulation of blood has become a heart, but there is no problem in judging as a heart something quite different from the human heart, that does not have the same internal divisions, valves and shape. What classifies, even today, various organs as a heart is the role they play for having been selected to perform this function – circulate the blood. Thus, if it is considered that the explanation for the presence of a trait in a system is given by its function, since it was selected in the past, due to its function, then there is no need that the most distant ancestral of the heart had to be exactly what we currently identify as a heart. It is enough that, from certain point, it could be called that. Natural selection may be responsible for the original presence of a trait in an organism of a population, since we understand for original presence the process which fixes the trait in the members of a given population. As Philip Kitcher says: "In speaking of the origination of an entity in an organism, I do not, of course, mean to refer to the mutational and developmental history [. . .] but in process that culminates in the initial fixation of that entity in members of the population" (Kitcher, 1998a, p. 264, note 8).

However, even if it is accepted that the proliferation and preservation of a trait has occurred due to the function it performs and that it is possible to appeal to that function to explain the presence of the trait, the third criticism remains. It states that it would not be correct to appeal to the function if all the variants of the trait had the same function. This is because in that case natural selection would not act because of the trace had or did not have this function, but it would act based on the performance of the function, selecting only the one that performs better that function. For Cummins, this fact puts in check the statement that natural selection is creative. If the selection is in some sense sensitive to the effect of a function, it is not in the sense relevant to the etiological conception because it would not answer for the generation of trait, but only for its maintenance (2002, p. 163).

According to the etiological view, natural selection is not only responsible for the maintenance of the trait, but it is also responsible for the original fixation of a functional trait, and because of that it is possible to explain the presence of a trait in the system appealing to its function. There is at the heart of this discussion a difference in the way of understanding the process of evolution by natural selection.

However, the third criticism really brings to light a problem for the etiological perspective. It leads us to the second question raised above which asks whether only the natural selection can answer for the generation of biological function. This criticism does not necessarily refuse the etiological view, but denounces some limitations of this theory.

Although in many cases we cannot separate the recent selection of the trait – in general related with the maintenance of the trait – from the selection responsible for its original fixation, because both of them go in the same direction, there are cases where it happens differently, that is, where the recent selection is not in continuity with the original selection. This happens when there was selection of a trait because of its function, but now it does not perform that function anymore or performs a different function. Then it is necessary to determine which process is really relevant to provide function to the trait. The authors who defend the etiological view are not always in agreement with respect to this issue. For instance, Ruth Millikan refers to the concept of "proper function", restricting its application only to cases where there was original selection for a trait" (Millikan, 1989, p. 292; 2002, p. 115). The process of the reproduction of a trait describes a causal-historical process. According to her, history is the most important element which determines the function. She refers to evolutionary history which responds by "reasons of survival" of these items. Thus, according to that definition, something may have a function without having proper function, because the current dispositions of a trait are not sufficient to determine its proper function. From this perspective, only adaptations generate functions.

However, that conception has been criticized because there are many cases where it is accepted that it occurred original selection of a trait for a specific function in the past and it has occurred a more recent selection of the trait for another function. Moreover, there is the problem of the vestigial traits which do have no more function, but continues to be there. The teleological conception of function is based on the evolutionary history that is responsible for the generation of the trait. Then traits which were favored by natural selection in the past thanks to the function they performed will have that function even when it does not perform it anymore, because of its history. That difficulty led Peter Godfrey-Smith to argue that it should be considered the distinction between original selections, with happened in a remote past, and modern or recent selection (1994, p. 556). It would be necessary to taken into account that distinction when considering the concept of biological function. The result of taking into account only the original selection is that it fails to consider some important functions actually performed by the trait, and it fails to assign function to traits that do not performed it anymore, because more recent selection may have acted in order to retain, change or eliminate the role of a trait. Therefore, Godfrey-Smith, taking as its starting point the definition given by Millikan, that the historical process is crucial to characterize the behavior of a trait as functional, proposed to explain the existence of some functional traits among the members of a population appealing to the fact that, in the recent past, these members were successful in the selection process. Then he rebuilt Millikan's conception of function in order to incorporate the recent or modern selection as prior criterion to determine the function instead of the original selection.

The account of the recent selection can be considered a good solution, because it solves some problems presented by the historical approach based only on original selection. However it does not seem sufficient to solve all the problems related to the etiological conception of function. In fact, as Godfrey-Smith recognizes, there is no guarantee that the establishment and maintenance of a trait is the result of the action of natural selection (Godfrey-Smith, 1999, p. 215). Traits can be maintained because phenotypic variations were not produced or even because they were eliminated by fortuitous factors and not by the natural selection. This is a problem for those who argues that biological functions are dispositions and capabilities of a trait generated and maintained by natural selection. According to that conception, if the trait was not generated and maintained by natural selection, it has no function, but the problem is that it is not easy to determine how far natural selection is responsible for the generation and maintenance of a trait. In fact, there are many difficulties to obtain information about the conditions that are required to support the hypothesis of natural selection, and show the hereditary variations and their differences in fitness (Schwartz, 2002, p. 248). However, I think it is possible to deal with these problems, and keep the etiological account of function, since we recognize that there are cases where the biological function was generated and originally fixed by natural selection, without requiring that this is the only way to identify a function. There are many cases where we cannot even decide if there was original selection for the trait or whether the selection was the only process involved in the generation and maintenance of a trait. It is probable that many different kinds of processes work together in the generation and maintenance of a functional trait. Then it is possible to accept the etiological conception of function even if it is not possible to determine the reach of its action because it is not necessary to require the exclusivity of natural selection. Only the denial of the relevant role of natural selection in evolutionary history that generate and fixe the function of a trait could lead to the elimination of the etiological conception, but that view is not considered reasonable by most scientists.

In that sense, both conceptions of function are important and can coexist (Walsh and Ariew, 1999, p. 275). It is not necessary to choose one of them. The analytical conception is important because if we consider that what it claims is, basically, that the function of a trait is given by its causal contribution to the capacity or activity of the system in which it is contained. Then because of its broad nature, it is presupposed by any other type of approach to function, even the etiological one. This is because the function of a trait can only be determined when the relationship between the trait and the system in which it plays or played its role is established. However, it is clear that the analytical conception does not comprise the etiological view, because it does not respond to its specific question, that is, why that trait is or was present in certain biological system doing what it does or did. In that sense, the etiological view has an important explanatory role, particularly in evolutionary explanations, because it aims not only to identify what is the current function of a trait, but why that trait is there playing that role it does, attributing to the notion of function a teleological and normative character.

The analytical conception is not and does not pretend to be normative, and because of that it is usually associated with physiological science which gives explanations about the functioning of parts in relation to the system as a whole. However, as observes Neander, this is a simplification. It is not true that the notion of function employed by physiologists is not normative (Neander, 2007, p. 13). It is the normative character that allows to distinguish between the functional and the dysfunctional behavioral of a trait and between accidental and functional role of a trait. Moreover that distinction between normal function and malfunction are broadly employed by physiologists. Then an adequate analysis of function employed in physiology explanations should take into account the normative character of function. Of course, it is possible to question whether only the etiological conception is able to give an explanation about the normativity of function. It seems reasonable to claim that it is not necessary to consider the evolutionary history of a trait when one perform a physiological analysis of a biological system; probably it is sufficient to employ a concept of function based only on dispositional and statistical criteria. Of course it is possible to question if this kind of criterion can really provide support for a normative concept of function. But if it is true that the concept of normativity is essential to evolutionary and to physiological analysis of a trait, then, although important, the analytical approach cannot, by itself, explain any case of function in biology. Although it is presupposed by any conception of function, biological or not, it is too weak to explain any of them.

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Chapter 12
Human Evolution: Compatibilist Approaches

Paulo C. Abrantes

12.1 Commonsense and the Human Predicament

Philosophy usually takes human beings as *agents*, that is, as systems whose behavior is caused by mental states. Furthermore, we are often recognized as *interpreters*, that is, as systems engaged in explaining and predicting the behavior of other people, by attributing mental states to them. We mindread our fellows all the time in (real or imagined) social interactions. Being both agents and interpreters is considered constitutive of our very nature as *persons*: this is how we *make sense of* ourselves and our fellows. This stance is also in agreement with a commonsense image of ourselves.

My aim in this paper is to look at some attempts to deal with the *evolution* of the human mind which mingle:

(1) a substantive commitment to a commonsense image of ourselves as both agents and interpreters;
(2) a bold stance concerning the role philosophy should play in looking for an integration between a commonsense and a scientific image of ourselves.

Even among philosophers who favor a commonsense image there are, however, conflicting positions concerning its *relationship* to scientific descriptions of ourselves. There are those which argue for an almost complete *autonomy* of folk descriptions *vis-à-vis* scientific ones, and those which attempt an *integration* between both descriptions.

Baker (1995, 2001) falls in the first camp, arguing for an anti-eliminativist, as well as anti-reductionist, slant towards commonsense. She argues for a practical realism: a metaphysics based on our everyday cognitive practices (especially our interpretive practices).

In the next sections, I will scrutinize approaches which fall in the second camp since they embed a commonsense image of ourselves (as agents and interpreters) in a naturalistic-evolutionary framework.

P.C. Abrantes (✉)
University of Brasília, Brasília, Brazil
e-mail: abrantes@unb.br; pabran406@yahoo.com.br

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12.2 Two Kinds of Facts

A central topic related to (1) is the adequacy of *folk psychological* notions for depicting the architecture of the human mind and their application in the prediction of human behavior in everyday situations. The status of folk psychology has been a hot topic in the philosophy of psychology: is it a theory or a craft (Dennett, 1998)? Is it true? Intentional realism is far from being a consensus – instrumentalism and plain eliminativism are options that have prestigious supporters. The various approaches to the mind-body problem (reductionism, functionalism, etc.) make also different commitments towards a folk-psychological conceptual scheme.

A lot of ink has also been spilt by philosophers trying to clarify the notions of representation and interpretation (concerning behavior). These notions relate to the issue of defining different orders of intentionality as a way to deal with metarepresentational abilities. Much debated is also the way these abilities are accomplished.

Godfrey-Smith's strategy (2002a) is to bypass those acute philosophical problems concerning the status of folk psychology as well as those related to agency, representation and the mechanisms underlying mindreading. He argues that, in any case, we have to take for granted two kinds of *facts* – as (empirical) data to be dealt with by any theory addressing the evolution of the human mind:

(1) facts about our "wiring" and how it "connects" with the world, on the one side, and;
(2) facts about our "habits of interpretation", on the other side.

These facts should be taken at face value:

> ...Whether the interpretations made by people are descriptions of the wiring-and-connection facts or not, the world does contain these two sets of facts. Both are empirical phenomena, and in principle there could be complete empirical theories of each (Godfrey-Smith, 2004).

The *natural* sciences tipically focus on the first kind of facts, largely ignoring those related to interpretation. The *social* sciences – more influenced by philosophical and commonsensic concerns – address the latter kind, usually disregarding the facts about wiring-and-connections.

Furthermore, philosophers (and many scientists alike, for that matter) usually don't frame those facts in evolutionary terms. Developmental questions concerning human cognitive capacities and interpretation habits aren't usually raised either (Dennett, 2000: p. 22). These are precisely Godfrey-Smith's theoretical concerns:

> "What kind of description of cognitive mechanisms picks them out in a way that is appropriate for evolutionary explanation?"

In particular, we should also ask for the credentials of folk psychology in framing the typical puzzles that arise in an evolutionary setting:

> "Does folk psychology supply us with concepts that we can use to formulate good evolutionary questions about the mind? Is folk psychology even *trying* to describe real features of cognitive mechanisms?" (Godfrey-Smith, 2005).

An important task is, therefore, that of integrating natural-scientific (including biological) and social-scientific perspectives on those two kinds of *facts*, as a way to coordinate them.

12.3 Coordination as a Philosophical Task

Godfrey-Smith claims that a theoretical coordination between those two kinds of facts should be a *philosophical* endeavour:

> So imagine a future state of scientific knowledge in which we have highly detailed empirical theories of people. One thing this body of empirical knowledge will contain is a description of these two sets of facts. But as well as these two bodies of *empirical knowledge*, we will want a *theory* of how the two sets of facts are inter-connected. Here we find one of the roles for philosophy – to describe the coordination between the facts about interpretations and the facts about wiring-and-connections (. . .) Philosophy would aim to describe the connections between facts about the use of difficult and controversial concepts, and facts about the parts of the world that the concepts are in some sense aimed at dealing with. . . (Godfrey-Smith, 2004; emphasis mine).

He highlights, actually, *two* roles for philosophy:

1. To investigate the relations between different sciences: might these *fragments* of knowledge fit together?
2. To coordinate commonsense and scientific views of the world and ourselves.

It is helpful to refer to the latter role using a more general and traditional label – *compatibilism*. Originally, this is a position in metaphysics concerning the compatibility between free-will – as part of a commonsense image of human agency – and causal determinism, as part of a scientific-mechanistic image of the physical world, including ourselves. More akin to my concerns in this paper, *compatibilism* refers also to those trends in the philosophy of psychology that look for relations between commonsense (or folk) psychology, on the one side, and different kinds of scientific psychology, psychoanalysis and neurophisiology, on the other side.[1]

Sterelny (1990) also pleads for a sort of compatibilism between folk psychology (with its intentional idiom, usually adopted by the social sciences) and a conception of humans as *part of the natural order* (usually presupposed by the physical sciences):

> Philosophy is an integrative discipline. . . There are two very different pictures of what we are. . . Our actions have intentional or belief-desire explanations. We are intentional agents. Our actions reflect our thoughts. This is the picture of folk psychology. There is an alternative physicalist picture which emphasizes our continuity with nature. . . We cannot reject the scientific image of ourselves, so we must try to reconcile it with what we know of ourselves from our common experience (Sterelny, 1990: pp. 1–2; 22).

[1] Hurley (2003b: p. 274) pleads also for a certain kind of compatibilism in the philosophy of mind. One finds an example of a compatibilist stance in the philosophy of science, regarding the topic of scientific realism, in Godfrey-Smith (2003b: pp. 174–6).

The views that are especially relevant to this paper are those pertaining to the coordination of the facts about wiring-and-connections and the facts about our (social) skills of interpreting our fellows (by attributing mental states to them). They illustrate the *second*, compatibilist, role for philosophy pointed out by Godfrey-Smith. This role seems to be relevant to tackle with what looks like a distinctive evolutionary process: human's.

A central issue is also the compatibility between folk psychological depictions of agency and interpretation, on the one side, and reconstructions of our evolutionary past, on the other side.

12.4 The Internal Integrative Project

At least two integrative projects embracing evolutionary biology might be conceived, given the distinction made by Godfrey-Smith between facts about wiring-and-connections and facts about habits of interpretation: an integrative project *internal to the sciences* and another, *external* project.

The *internal integrative project* of evolutionary naturalism is precisely that of giving a purely scientific explanation of how our wiring-and-connections evolved, pretty much in the same terms as one would tell a story about how other organic systems (the immune system, for instance) evolved. In his early work, Godfrey-Smith put forth a set of questions about the function of mind in nature, different from those traditionally asked by the philosophers of mind. In this context, he formulated the *environmental complexity thesis*: "The function of cognition is to enable the agent to deal with environmental complexity" (Godfrey-Smith, 1998: p. 3). The application of this thesis to evolutionary problems illustrates a trend in the internal integrative project[2]: cognitive systems of different kinds are explained as adaptations to the complexity of different types of environments.

The *social intelligence hypothesis*, proposed initially by Humphrey in 1976, can be framed in terms of the environmental complexity thesis: the complexity of the *social* environment (and not just that of the physical environment) was responsible for the chief selection pressures that drove the evolution in the hominid lineage.

The social environment is actually very demanding, cognitively speaking: one might just mention the cognitive load of food and information sharing, of activities like cooperative hunting and collective defense against predators, of grasping social relations and hierarchy, of detecting free riders as a way of stabilizing group behavior (Donald, 1991) etc. These pressures drove the evolution of a particular kind of cognitive arquitecture: intentional systems. Systems of this kind, with capacities for decoupled representation, have a more flexible (or less automatic) behavior,

[2]In his 1998 book, Godfrey-Smith doesn't make explicit, as much as in his more recent work, the relationship between the environmental complexity thesis and an integrative project (Godfrey-Smith, 2002b). See Abrantes 2006.

enhancing their fitness in dealing with the physical and the social environments alike (Sterelny, 2003b: p. 30; Kornblith, 2002: pp. 41–2).[3]

Godfrey-Smith argues, in his most recent work, that it wouldn't be enough, however, to tell a purely *scientific* story about the evolution of human wiring-and-connections along the lines pointed out by the environmental complexity thesis. One has to elaborate more complex evolutionary scenarios that take also into account our interpretive capacities.

12.5 The External Integrative Project

The *external* project attempts, otherwise, to depict evolutionary scenarios in which we are conceived not just as *ecological* agents – (an image commonly associated with human behavioral ecology in which the physical environment plays the central role) -, but also as *social* agents (an image of ourselves which has been central to philosophy and also to the social sciences). The external integrative project of evolutionary naturalism strives to figure out, therefore, how to "weld together evolutionary-scientific and social-scientific conceptions of human agency" (Sterelny, 2003b: p. 5).

In the internal project, the interpretive abilities (that is, mindreading based on a folk psychological conceptual scheme) are not acknowledged as playing any causal role in shaping the evolution of the wiring-and-connections. To get a grip on this project, we have first to distinguish between simple and complex coordination of the two kinds of facts mentioned above.

One way to coordinate these two kinds of facts about ourselves is to assume that folk psychology is a theory that picks out fairly well the wiring-and-connection facts, the inner causes of behavior. This would explain, in a straightforward way, why our interpretive practices are predictively successful.

Sterelny calls this the "simple coordination thesis" (Ibid.: p. 5). Fodor can be read as a philosopher committed to a coordination of this kind, what amounts to see "... folk psychology as science [as], a largely true theory of the overall architecture of the human mind" (Sterelny, 2003a: p. 258). This *simple* (in a sense to be clarified below) coordination presuposes, therefore, the theory–theory and intentional realism.[4] This way of looking at folk psychology is qualified as *scientific* because it has a descriptive focus.

[3]It is controversial whether the social intelligence hypothesis might also be sufficient to account for the evolution of the special mindreading skills of the human mind (eventually supported by a version of folk psychology). I will not tackle this issue here (see Abrantes, 2006).

[4]The expression *theory–theory* comprises the thesis that folk psychology is a theory (with a structure similar to a scientific theory and used to attain the same descriptive and explanation aims). An alternative view is that folk psychology is a craft (Dennett), that is, it has a practical (and not a theoretical) motivation. Sterelny (1998) argues that conflicts might also arise between different crafts and practices, given their metaphysical presuppositions. Interpretation might be grounded on some version of folk psychology (as the theory-theorists presuppose) or, otherwise, on simulation or other mechanisms (Goldman, 2006).

Godfrey-Smith's and Sterelny's *philosophical* integrative project is that of figuring out, in evolutionary scenarios, how to coordinate in more complex ways the wiring-and-connection facts, on the one side, and the interpretive abilities, on the other side. These facts are taken separately as different *traits* – each of them being part of the selective environment of the other trait.

In contrast with a simple coordination like Fodor's, the two kinds of facts here causally shape each other in human evolution. As a consequence, an *arms race* is expected to take place between these traits, bringing forth selective pressures in *both* directions. Godfrey-Smith argues as follows for a complex coordination:

> If folk-psychological interpretation is biologically old, then it has been part of the environment in which human cognitive traits were exposed to natural selection. Folk psychology is not just the tool that we use when first thinking about the mind, it is also a social fact that human agents have had to contend with, for some unknown period of time. It is part of the social context in which thought and action take place. So while it is obvious that folk psychological practices of interpretation will have been affected by the facts about cognitive mechanisms, it is also true that the evolution of cognitive mechanisms might have been affected by the social environment generated by folk psychological interpretive habits (Godfrey-Smith, 2005).

Henceforth, a complex coordination embodied in an evolutionary framework address anew our role as interpreters – a central element of a philosophical image of *personhood* (Dennett, 1986).

This integrative project is *external* to the sciences because it takes seriously the way we conceive ourselves not only as intentional systems but also as interpreters: we have been using, probably for a long period of time, a folk psychological scheme to make sense of the behavior of other people in social environments.

12.6 Nativist and Non-nativist Scenarios

An evolutionary and developmental concern with the human quandary provides a promising field for (thought-) experimentation – by setting up different scenarios in which philosophical and scientific perspectives are taken into account and effectively integrated.

Godfrey-Smith explores some of these possible scenarios, trying to answer questions such as the following: did a folk psychological framework for interpretation *evolve*? Or, else, does this framework just *develop* given certain environmental conditions?[5]

[5] An approach that takes into acccount both folk psychology's philogeny and ontogeny should not be disposed of a priori. One should expect that different descriptions of human cognitive capacities, as well as of the mechanisms that realize them, lead to different accounts not only of the evolution but also of the development of these capacities. And the other way around: evolutionary and/or developmental approaches might lead us to revise the way we ordinarily describe these capacities and underlying mechanisms.

One of the scenarios point to the evolution of a module for our folk psychological interpretation abilities, by means of an orthodox process of natural selection comprising just genetic inheritance.[6]

In another scenario, there is *individual* learning of the interpretation abilities. Interpretation has a non-canalized ontogeny: the individual acquires these abilities in a fact-driven way, by observing other people in the social environment and using general-purpose learning mechanisms.[7]

A third scenario gives a prominent role to *social* learning. The social environment selects for groups that facilitate the learning of the interpretive abilities (a kind of "epistemic engineering"; Sterelny, 2003b: p. 236). Furthermore, learning takes place in niches constructed by several generations.[8]

Sterelny favors the last scenario and explicitly mentions that it is motivated by an external-integrative bias: "A theory of human cognitive evolution needs to integrate the biological and social-scientific perspectives on human nature. Niche construction and its partial transformation into bone fide inheritance is the key to this integration" (Sterelny, 2003b: 171).

He qualifies this "biocultural integrated theory of human agency" as the unique genuinely philosophical project (Sterelny, 2003b: p. 5; cf.: 171). If this external integrative project of an evolutionary naturalism comes to be accepted as a result of its epistemic virtues compared with internal integrative projects, an important dimension of commonsense will have provided fruitful insights for setting up adequate scenarios of how the human species distinctively evolved.

Godfrey-Smith and Sterelny acknowledge, however, that progress in the internal, scientific, project might force a revision of some aspects of the external project or, even, its complete rejection (Sterelny, 2003b: p. 5).

12.7 Is Folk Psychology an Adequate Framework for Describing (Nonhuman) Minds?

There is an old and lasting controversy in the litterature of cognitive ethology, as well as in philosophical reviews of if, about the adequacy of an intentional

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[6]Usually, the following properties are associated with cognitive modules: they are innate, encapsulated and domain-specific. Evolutionary psychologists argue that our interpretive abilities are adaptations to a social life. They exemplify a nativist stance towards mindreading as a social task: one of the modules of our cognitive architecture would be specialized in solving the problem of predicting behavior, by attributing mental states to other people throught the application of a theory of mind – the content of that module (Cosmides and Toody, 2000). In this view, mindreading tasks are solved at a *sub-personal* level (Dennett, 1991).

[7]A genetic takeover process such as the Baldwin effect is not excluded, though, in this scenario.

[8]I discuss in detail the controversy *evolution versus development* concerning the intrepretive capacities in another paper: Abrantes (2010); cf. Abrantes, 2006. The third scenario presupposes that group selection has enough intensity to be taken seriously, given certain conditions prevalent in human-social environments.

vocabulary (taken, basically, from commonsense psychology) to describe and, possibly, to explain the behavior of nonhuman animals.[9]

The prospects of that debate are enlarged by considering the evolution of animal minds. As a precondition, we have to distinguish different kinds of "systems for the adaptive control of behaviour" (Sterelny, 2003a: p. 257). The project of "charting control space" is an attempt "to identify the crucial dimensions of control space (. . .) occupied and occupiable locations in [it] and the potential trajectories between those locations. . ." (Sterelny, ibid.: p. 264). How adequate is a folk psychological conceptual scheme for accomplishing this project? In other words, might folk psychology (and, therefore, a commonsense image of wiring-and-connections) contribute to chart this space and to depict trajectories *from* ancestral nonhuman minds *to* a fully human mind?

Hurley (2003a) is confortable with a wider range of application of our intentional vocabulary. She argues that nonhuman animals may be considered intentional agents in context-bounded situations such as, for instance, competitive contexts over finding food, contrasting with cooperative contexts (Ibid.: p. 21). There would be "islands of practical rationality" out there, even if we shouldn't expect to find *theoretical* rationality, that is, a "conceptually promiscuous" kind of mind (Hurley, ibid.: p. 1). Philosophers might have been "over-intellectualizing" social life, after all (Ratclife, 2005: p. 213).[10] Hurley claims they should emphasize, rather, the space of action: social contexts require often this shift of focus from theoretical to practical rationality, from a *know that* to a *know how*.

Hurley is, however, fully aware of the relevant discontinuities between humans and other animals, as far as mindreading is concerned (Hurley, 2005). We *make sense of* animals by *interpreting* them, but this is just unilateral mindreading. In this context, she distinguishes instrumentally rational agents (which have non-conceptualized reasons in the practical sphere) and mindreaders:

> Even if other animals have minds for us to interpret, most current evidence suggests that they are not mindreaders themselves. Asking what is rational for a creature to do when it plays against nature is very different from asking what is rational for a creature to do when it plays against another rational agent when it is trying to interpret and who is also trying to interpret it. If nonhuman animals are not mindreaders, then game-theoretic problems of mutual interpretation and prediction do not arise in the same way for our relations with them, and strategic rationality does not really get a grip on animals (2003a: p. 278).

Sterelny and Godfrey-Smith blaim Hurley, nonetheless, for having *exapted* PS notions for describing the architecture of nonhuman minds. They work out the implications of taking folk psychology as, rather, a craft. It's primary role is interpretation:

> If we think of folk psychology as a socially-evolved interpretive tool that functions to help us deal with a specific set of social tasks, then when it is used to describe nonhuman animals it is far from its domain of normal use. The framework will be under some stress, and it will

[9]Dennett (1987) and Kornblith (2002) are good examples of philosophical accounts of this topic.

[10]Ratclife claims that folk psychology has "no psychological reality as an autonomous ability"; it is a philosophical abstraction "from a complex of perceptual, affective, expressive, gestural and linguistic interactions, which are scaffolded by a shared cultural context" (2005: p. 231).

> be unclear what conclusions can be drawn from how it behaves (Godfrey-Smith, 2003a: p. 267).

This way of addressing the actual role of folk psychology suggests a further question: shouldn't we be also skeptical concerning the descriptive credentials of folk psychology in the human case? This is what eliminativists, like Stich (2004) and others, have been arguing for. If it is defensible that the primary function of folk psychology is that of (unilateral or mutual, for that matter) *interpretation*, when its conceptual resources are used not only as a craft but *to describe* the wiring-and-connection facts (in a scientific setting, for instance), it might also be "under stress" *even in the case of humans*!

Godfrey-Smith and Sterelny don't go far down this path – they hold a bold skeptical position concerning the *scientific-theoretic* credentials of folk psychology just in the case of nonhuman minds.

It is important, at this point, to make explicit *two* different roles folk psychology might play in integrative projects. First of all, folk psychology may be used as a conceptual framework (e.g. a theory) for describing the human mind (as having, roughly, a belief-desire architecture). This is, traditionally, a scientific task (even if, in this case, a folk psychological conceptual scheme is being applied).

Folk psychology may also be taken as a craft, conveying our everyday interpretive practices. Folk psychology is conceived, in this second role, as a basis for *mindreading*.

Concerning the first role, Sterelny and Godfrey-Smith take a mild realist path: they argue that folk psychology picks out the fundamental wiring-and-connection facts. Taking for granted this conceptual framework, they come up with conjectures about the evolutionary story of intentional systems: why and how have systems with this basic architecture been selected for? Are beliefs (decoupled representations) and preferences "fuels for success" (Sterelny, 2003b: p. 30)?

Even if we are compelled, at the end of the day, to accept a full eliminativism concerning a commonsensic conceptual-psychological framework for describing the wiring-and-connection facts of at least some animals (including us), our interpretation habits might still be acknowledged as facts which presumably played an important role in the evolution of a *human* kind of mind.

I highlighted that in complex coordination scenarios, like those depicted by Godfrey-Smith, the interpretive habits put pressure on the wiring-and-connection facts, shaping their evolution. Hence, one might say that the *use* of folk psychology as an interpretive craft, was an *ultimate cause* of the evolution of a particular kind of mind, with a special wiring and special connections to the world.

12.8 Dual Inheritance Theory

P. Richerson and R. Boyd's dual inheritance theory is one of the main contemporary approaches to human evolution. I want to suggest in the following that their theory embrace elements of a commonsense image of human beings and therefore might be seen as another compatibilist approach to human evolution.

They assume, effectively, that human evolution is anomalous because we are social *and* cultural agents.[11] Culture function, in the human case, as another kind of inheritance system, besides the genetic one, making available a faster way to meet adaptive problems in a wide range of environmental conditions.

Furthermore, they take seriously the human sciences (and its underlying folk image of human agents) to come to grips with an acceptable evolutionary theory. Richerson and Boyd claim that it is not enough to explain the observable variation in human behavior on the basis of just genes and environment (like other approaches to human evolution, as evolutionary psychology and behavioral ecology): "The evidence accords better with the traditional views of cultural anthropologists and kindred thinkers in other disciplines: heritable cultural differences are crucial for understanding human behaviour" (2005b: p. 19).

The traditional nature/nurture dicotomy is forcefully discarded on the basis that culture is not just a *proximate* cause of human behavioral variability but also an *ultimate* cause of our (innate) social psychology. This is one of the levels in which coevolutionary processes involving culture shape human evolution (Ibid.: p. 8).

There is also a compatibilist element in the way Richerson and Boyd model cultural evolution, by assuming *forces* which are *not* analogous to those acting in classical evolution through genetic inheritance. Among these we have several biases in the way we assimilate and transmit culture, as well as a special case of natural selection, acting on cultural variation. Cultural inheritance is not, therefore, strictly analogous to genetic inheritance. This is not an obstacle, of course, for conceiving a coevolution between those two processes.

A definition of *culture* is, of course, crucial to their project: "Culture is information capable of affecting individuals' behavior that they acquire from other members of their species through teaching, imitation, and other forms of social transmission" (Richerson and Boyd, 2005b: p. 5).

Despite the use of the *prima facie* scientific concept of information, in their discussion of that definition they deploy folk psychological concepts (with a disclaimer, though[12]):

> By information we mean any kind of mental state, conscious or not, that is acquired or modified by social learning and affects behavior. We will use everyday words like idea, knowledge, belief, value, skill, and attitude to describe this information, but we do not mean that such socially acquired information is always consciously available, or that it necessarily corresponds to folk-psychological categories (Richerson and Boyd, 2005b: p. 5).

This is an ideational concept of culture which constrats with many other concepts that do other jobs in different theories.[13] Many animals surely have culture, if we

[11] To recognize the exceptionality of human evolution doesn't exclude, of course, the need to find out the relevant homologies between human behavior and psychological capacities, on the one side, and those of other animals, on the other side (Richerson and Boyd, 2005b: p. 104).

[12] They are sometimes rather dismissive about folk psychology (e.g. Richerson and Boyd, 2005b: p. 35).

[13] Culture' should be viewed as a theoretical term.

adopt Richerson and Boyd's definition. However, the evidence presently available is that the *accumulation* of culture is a very rare phenomenon.

We've got an *adaptationist puzzle*: if the advantages of a cumulative culture are so impressive (greater and faster adaptability), why did it not evolve, as far as we know, in other lineages besides our own?

Culture can function as an inheritance system only if there is some mechanism supporting what Tomasello calls the *ratchet effect* (1999). A capacity for social learning through imitation (or observational learning) plays this role in dual inheritance theory.

Learning by imitation incur, however, heavier costs than we might expect at first sight. It requires, effectively, a special psychological capacity: mindreading (theory of mind). The *adaptationist puzzle* led Richerson and Boyd to bring forth our interpretive abilities in their account of human evolution. It is a remarkable result from a compatibilist point of view, since what Sterelny and others call *social agency*, now incorporate also *cultural agency*, that is, the role agents play in cultural transmission with their effects, at a population level, in cultural evolution.

I don't have space here to analyse the mathematical models Richerson, Boyd and others set up which show that there are barriers, however, to the evolution of true imitation. A way to counter these results is to suppose that the psychological precondition for culture accumulation evolved, originally, to meet the complexities of the social environment (an application of the social inteligence hypothesis). Richerson and Boyd suggest, along these lines, that a "roundabout path" might have been traveled by our ancestors: they first evolved an ability to (better) predict the behavior of their fellows – by reading their minds (and not just their behavior). Then, as a byproduct, this psychological capacity could be used for imitation purposes (Richerson and Boyd, 2005b: pp. 138–9). This argument pressuposes that what has been called a *machiavelian intelligence* has the same psychological requirement as true imitation: a mindreading (or theory of mind) capacity.[14]

It still has to be shown, however, why other species – for instance, the great apes – could not have traveled the same path. After all, they were facing physical and social adaptive problems analogous to those of our hominid ancestors. The *adaptationist puzzle* is still there to be solved!

Richerson and Boyd's attempt to meet this enduring puzzle is not very convincing, though. All they have to say is that we got there before other species: "... we have preempted most of the niches requiring culture, inhibiting the evolution of any competitors" (Boyd and Richerson, 2005a: p. 16).

Their commitment to adaptationism and nativism concerning the mind seems to be the problem here. They bite the bullet of the evolutionary psychologists accepting, for instance, that we've got a theory of mind module.[15] This corresponds

[14] See Blackmore (2000).

[15] Richerson and Boyd don't accept massive modularity, though (see note 6). They reject also a thesis evolutionary psychologists are sympathetic with: that culture is evoked by the environment (2005b: p. 44).

to the *first* scenario depicted by Godfrey-Smith, in which a mindreading capacity *evolves* on the basis of just genetic inheritance.

The external integrative project proposed by Sterelny might help fleshing out Richerson and Boyd's roundabout path. The *third* scenario points to niche construction and epistemic engineering as processes underlying the *development* of interpretive abilities. Those processes allow, of course, a much faster pace in spreading these abilities in the population than (classic) evolution through genetic inheritance. Very slight differences in mindreading abilities – due to differences in those constructive processes – might have had big cultural-evolutionary effects in a relatively short period of time, precipitating more differences in niche construction and epistemic engineering, bringing about a virtuous circle. Furthermore, if we admit the full causal power of our interpretive capacities and their bearing in shaping those processes, we can predict that our minds could have changed even after the Pleistocene, what Richerson and Boyd seem ready to accept in their latest Publications (Richerson and Boyd, 2005b: p. 230).

Why isn't there more room for niche construction and epistemic engineering in their theory, despite the importance they attach to cultural evolution? My guess is that Richerson and Boyd don't accept the full implications of their compatibilist stance.

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Chapter 13
Functional Explanations in Biology, Ecology, and Earth System Science: Contributions from Philosophy of Biology

Nei Freitas Nunes-Neto and Charbel Niño El-Hani

13.1 Introduction

Discussions about teleology and function touch in several fundamental aspects of the biological sciences, including many issues closely related to the question of the autonomy of biology (Ariew et al., 2002; Wouters, 2005). Many influent and divergent approaches to teleology are found in the literature (for anthologies or reviews, see, for instance, Allen et al., 1998; Perlman, 2004; Wouters, 2005; McLaughlin, 2001). In this paper, we address one of the debates embedded in controversies about teleology, dealing with functional ascriptions/explanations. We will focus our attention on two very influent approaches to function: Larry Wright's etiological selectionist approach and Robert Cummins' functional analysis.

We will begin by discussing Wright's approach. Then, we will consider objections against the etiological approaches raised by Cummins. We will show, however, that Cummins' critique can be put into question in important ways, making it possible to preserve a role to the etiological approaches to function, namely in explaining the spread of functionally novel biological traits in a population. In the sequence, we will introduce Cummins' theory on functions (i.e., his functional analysis) and apply it to a particular biogeochemical system, described by the CLAW hypothesis (Charlson et al., 1987). This argumentative path will lead us to the conclusion that we need both an etiological and a systemic approach to account for functional explanations in biology and related sciences.

13.2 Wright's Etiological Approach

As Godfrey-Smith (1993) pointed out, Wright's approach to function brought about an important change in the debate over teleological and functional language in biology. For Wright,(1998[1973]), previous philosophers' analyses of function failed

N.F. Nunes-Neto (✉)
Research Group on History, Philosophy, and Biology Teaching, Institute of Biology, Federal University of Bahia, Ondina, Salvador-BA, Brazil 40170-115
e-mail: nunesneto@gmail.com

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because they did not capture the genuinely explanatory power of functional ascriptions. He intended, in turn, to capture the explanatory power of function by means of his well-known formula: "The function of X is Z *means* that (a). X is there because it does Z (b). Z is a consequence (or result) of X being there" (Wright, 1998[1973], p. 71, emphasis in the original).

The first clause shows the etiological form of the functional ascription, while the second clause expresses the convolution that distinguishes functional from non-functional etiologies. For Wright, functional ascriptions must explain in a strong sense, since weaker interpretations of the meaning of function cannot take in due account the function-accident distinction, which is a major concern in his approach. For instance, the question "What is the liver good for?" cannot be translated into "Why do animals have livers?" (Wright, 1998[1973], p. 66). Notice that the second question requires an explanation of a given state of affairs in a particular context. Such an explanation should count as an ascription of function to the liver, which allows us to understand why livers are there in animals. In turn, the other question (what is the liver good for?) allows many different answers, some of them having nothing to do with the function of the liver, but related, rather, to utilities of the item that can be accidents from an evolutionary point of view. From a historical perspective, it is an accident, for instance, that livers are good to eat with onions. Nevertheless, this is not an absurd answer. Then, how can we differentiate functions from accidents? Notice that the fact that livers are good to eat with onions does not illuminate, in any sense, the etiology of livers, since the notion of *utility* (present in expressions like "it serves for" or "it is useful for" or "it is good for"), although playing a role in the explanation of intentional behavior, is not adequate to deal with the uses of function in strictly biological, non-intentional systems. Often, the utility of some biological item is of no use in an account of its etiology precisely because utility is a human-oriented notion. The abovementioned utility cannot be the function of the liver in the intended sense, since it is not the reason that explains why some animals have livers. From Wright's point of view, functional ascriptions/explanations should be etiological, in the sense that they relate to the causal background that gave origin to the trait or behavior under consideration.

Wright also holds that his analysis is highly recommended because it elucidates the concept of natural selection. Indeed, Wright's approach is strongly selectionist, something that is not surprising, since he built it by taking as a starting point an adaptationist understanding of the Darwinist explanation of evolution which was current in the 1970s. The selective advantage of showing a given behavior or trait in the past, related to the realization of its function, is, at least in part, a cause of the instantiation of the behavior or trait by current organisms of a lineage. Therefore, the function – in this conception – is the very reason, the *raison d'etre* of the behavior or trait. In other words, function is, in the etiological selectionist approach, what explains why particular biological traits or behaviors exist or are present in some organisms today.

The etiological selectionist perspective developed by Wright clarified some important issues in the debate, such as the function-accident distinction (Godfrey-Smith, 1993). However, this approach also has a number of problems, which persist in subsequent etiological theories, proposed by philosophers influenced by Wright. These theories are gathered by Cummins in a position he calls “neo-teleology”.

13.3 Neo-Teleology: The Selectionist Etiological Approaches on Trial

In his more recent work on functional ascriptions and explanations in biology, Cummins (2002, p. 157) argues that there are “two subpopulations of functional explanation roaming the earth: the teleological explanation and the functional analysis”. And he intends to – using his own words – “help to select the latter, and nudge the former to a well-deserved extinction”.

Teleology, for Cummins, is the idea that the appeal to function, goal or purpose of some item, say *x*, can explain why *x* exists or is present. For Cummins, teleology survives today in biology, or at least in its philosophy, in the form of neo-teleology, an expression coined by himself to indicate the

> [S]ubstantive thesis that, in some important sorts of cases at least, a thing’s function – the effect we identify as its function – is a clue to its existence. If it is not to degenerate into the trivial thesis that “why is it there?” can sometimes just mean “what is it for?”, neo-teleology must be the idea that, for example, there are eyes because they enable vision, wings because they enable flight, and opposable thumbs because they enable grasping (Cummins, 2002, p. 161).

So, in general terms, the neo-teleological explanation seeks to account for the presence or existence of a biological trait or behavior through an appeal to its respective function. Prominent philosophers of biology are supporters of neo-teleology, such as Peter Godfrey-Smith, Ruth Millikan, Paul Griffiths, and Karen Neander (for more details, see Cummins, 2002). Although Cummins does not mention Wright as a neo-teleologist, he indeed assumes the identity between the selectionist etiological approach and neo-teleology: “a defense of a selectionist etiological account of functions is, in effect, a defense of neo-teleology, since selectionist etiological accounts of functions equate functional attributions with what I am calling neo-teleological explanations” (Cummins, 2002, p. 162). And we can go even further, saying that Wright’s approach is not only a neo-teleological one, because it shares the premises of neo-teleology with other authors’ approaches, but it is the very foundation of the doctrine among philosophers of biology, since it clearly influenced the authors pointed out by Cummins as neo-teleologists.

We can certainly argue that Cummins understands “teleology” in a too restrictive sense, by making it equivalent to the selectionist etiological approach. Arguably, “teleology” has a more general meaning than that ascribed by Cummins, since a teleological explanation can be understood as one in which one says that an event occurs because it is the type of event that produces a given end state, goal, or purpose (Taylor, 1964). Be that as it may, we will pursue here Cummins’ argument as presented by him, in order to critically appraise it in its own grounds. Later, we will come back to the discussion about the meaning of “teleology”, in connection with Cummins’ functional analysis.

According to Cummins, if having a function is what explains why a thing or type of thing exists, then there must be some background story about a mechanism or process that generates the items of the functional ascription. He calls this a *grounding process*. At first, the strong appeal of neo-teleology lies in the fact that it takes natural selection as its grounding process. Neo-teleologists appeal to a selectionist strategy, claiming that traits and behaviors now present in organisms were selected for because of the effects that count as their functions, i.e., they exist in organisms because they have the functions they perform. Natural selection is highly accepted by both biologists and philosophers of biology in part because it eliminates the need of supernatural creators or hidden forces, such as entelechies, to explain the attributes of living beings. Thus, natural selection shows its strength as a grounding process for neo-teleology precisely where grounding processes of previous teleological explanations, committed to supernaturalism or hidden entities, exhibited their limits.

But this selectionist strategy can well be the Achilles heel of neo-teleology. As Cummins puts, "biological traits, mechanisms, organs, etc., are not there because of their functions. They are there because of their developmental histories" (Cummins, 2002, p. 162). For him, the processes that produce the biological traits are insensitive to their functions, since they should precede, as developmental processes, the functional performance of the traits. As a consequence, function cannot be used to explain why the traits are there. But Cummins also argues that neo-teleology lacks justification not only in explaining the origins or existence of traits, but also their spread: "the fundamental problem of neo-teleology is that traits do not spread because of (the effects that count as) their functions" (Cummins, 2002, p. 164).

Cummins splits neo-teleology into two variants: weak and strong. The strong version says that *any* biological trait that has a function has been selected for because it performed that function. The weak version holds a more acceptable thesis, that only *some* traits have been selected for because of their functions. In this section, we address only the strong version, while in the next one we will consider the weak version.

Since it is directed to all biological traits, the presentation of only one counterexample is enough to reject strong neo-teleology. As Cummins puts:

> Strong neo-teleology is refuted if there are legitimate targets of functional characterization that are not targets of selection. Strong neo-teleology must be rejected, since most, perhaps all, complex structures such as hearts, eyes, and wings patently have functions but were not selected because of (the effects that count as) their functions (Cummins, 2002, p. 165).

In other words, the reason to reject the strong version of neo-teleology is its strict selectionism, which identifies any target of functional characterization with a target of selection. As Cummins correctly argues, targets of selection constitute a subset of the targets of functional ascription. This happens because while the function of a trait is something widespread in the population, that is, all organisms possess the function (for instance, we can say that all hearts in a given population have the function of pumping blood), the group of organisms that will be selected for is smaller. Only those that perform better the function will be selected for. This is a different way of saying that the selectionist explanation in evolutionary biology is based on the differential efficacy of items in realizing a given function. It is not an explanation based on the difference between *having* and *not having* a function. Although Cummins, in our view, correctly rules out strong neo-teleology, his arguments are not enough to reject weak neo-teleology, as we shall see in the next section.

13.4 Why Cummins Cannot Rule Out Neo-Teleology from Biology or Its Philosophy

Here, we will present arguments against Cummins' objections to weak neo-teleology. However, we should stress, first of all, that it is not possible, as Cummins correctly argues, to appeal to function to explain the origins or existence of a biological trait, since the function of a given trait is something that it can only perform *ex post facto*, that is, after the structure is formed. As Cummins sums up, traits do not arise because of their functions, but because of their developmental histories (Cummins, 2002, p. 162).

Although Cummins is right in his appraisal that neo-teleology cannot appeal to function to explain the origins or existence of a biological trait, we think he goes one step further than his argument allows when he proposes the complete elimination of weak neo-teleology. He grounds his rejection of weak neo-teleology in arguments that mostly applies to the strong version. Moreover, the objections he raises against weak neo-teleology only make sense if we assume that the evolutionary process is strictly gradualist. Although he says that his arguments are not "merely a defense of gradualism" (Cummins, 2002, p. 166), he also claims that

> Weak neo-teleology comes out true only because of the rare though important cases in which the target of selection is also the bearer of a function that accounts for the selection of that trait. These will be cases where genuine functional novelty is introduced; a trait present in a subpopulation that is just not better at performing some function that is also performed in competing subpopulations (though not as well), but a trait that performs a function that is not performed at all by any counterpart mechanism in competing subpopulations. This unquestionably happens, and the importance of such seeding events should not be underestimated. But complex structures such as sparrow wings and human hearts were not introduced in this way (Cummins, 2002, p. 165).

In Cummins' arguments, it is clear that he himself underestimates the events that he says "should not be underestimated". He takes into account only examples that support his argument (such as eyes and wings) and does not consider those that could refute it, or, at least, show its limits. We think this is partly due to a commitment with a strongly gradualist position with regard to the evolution of traits. He does not recognize, in fact, the existence of several to many cases in evolution where there is a valid identity between the target of selection and the target of functional ascription. These are cases of evolutionary change at faster rates, such as cases of homeosis, i.e., the origin of a structure through a discrete and complete modification of another structure. To put it differently, homeosis amounts to the replacement of one body part by another in a non-gradual manner. It may be caused by either developmental or genetic variations. In these cases, we can appeal to function to explain the spread of a trait if the trait that appears as a consequence of homeosis is also genuinely novel in functional terms. We have here, thus, an important domain in which weak neo-teleology is valid. Needless to say, the extension of this domain depends on empirical rather than theoretical/philosophical grounds.

A case in point where the neo-teleological appeal to function is legitimate is the spread of the morphological and physiological innovations that allowed the arthropods to conquer land environments (Carroll et al., 2005). This is a remarkable example of tinkering, in which a large variety of arthropod appendages evolved through modifications of an ancestral multibranched limb. The last common ancestors of all arthropods were aquatic creatures exhibiting branched appendages. The ventral branches were used mostly for locomotion (e.g., legs), while the dorsal branches were used mostly for respiration and osmoregulation (i.e., as gills). Most arthropods still possess the leg branch. But while gills are preserved in aquatic arthropods, they have been lost (in myriapods) or modified in terrestrial groups, giving origin to structures like wings in insects, and book lungs, tracheae, and spinnerets in spiders. This is indeed an amazing example of tinkering, and illustrates how functional novelties can appear through homeotic changes, spreading through populations due to the advantage of subpopulations possessing the new structure with novel functions.

To give more details about this example, let us consider the origins of insect wings. We can examine two hypotheses: either wings appeared as independent projections of insect thoracic segments or they evolved from the dorsal branches of the multibranched limbs of aquatic ancestors. Averof and Cohen (1997) showed that two regulatory proteins related to wing development (*Apterous* and *Nubbin*) are expressed in the dorsal branches of crustacean limbs. The most parsimonious explanation for this shared regulatory circuit is that crustacean gills and insect wings are homologous, i.e., they evolved from the same ancestral structure. Currently available evidence gives support, thus, to the hypothesis that a gill-like structure of an ancestral aquatic arthropod gave origin to insect wings. Indeed, in the beginnings of the Carboniferous, membranous structures with putative respiratory function were found in all segments of the trunk of fossil insect nymphs. These gill-like structures are not that different from insect wings. The fact that insects only possess wings in the second and third thoracic segments is related to the appearance of *Hox* sites leading to the repression of wing development in the first thoracic and abdominal segments. The origins of wings themselves seem to have resulted from the fusion of the base of the ancestral branched insect limb with the body wall, leading to the evolutionary displacement of the dorsal branch away from the rest of the leg.

In this case of homeosis, a new structure appeared with an entirely novel function and the spread of the trait, the presence of wings, resulted from the advantage of winged subpopulations over non-winged subpopulations. To appeal to natural selection as a grounding process is not an error in this case, and this means that a domain of validity for weak neo-teleology remains, even though its explanatory power is much more limited than neo-teleologists typically assume: it does not explain the origins of traits, since functions can only exist *ex post facto*, and it can explain the spread of traits only when we are dealing with a situation in which functional novelties appear.

13.5 Cummins' Functional Analysis

In this section, we will briefly examine Cummins' (1998[1975]) approach to function. First of all, it is important to take into account that, for him, functional ascriptions and explanations can be offered without reference to evolutionary considerations. This is a non-historical approach which is in stark opposition to the selectionist etiological approaches. As we saw above, etiological approaches are, for Cummins, misoriented because they have insisted on the understanding of function as something that explains the existence or presence of organismal items under consideration. For him, to keep this notion of function, despite all the objections raised against it, is "an act of desperation born of thinking there is no other explanatory use of functional characterization in science" (Cummins, 1998[1975], p. 175). As we argued in the previous section, we think there is a legitimate place for etiological approaches to function in biological thinking, even though their domain of applicability is relatively limited.

Cummins address functional ascriptions from a different perspective, namely, in terms of complex capacities and dispositions. From this perspective, if x functions as a pump in a system s, or if the function of x in a system s is to pump, then, x should be able – or specifically, it should have the disposition – to pump in s. As a consequence, the functional-ascription statements imply dispositional statements; that is, to ascribe a function to something is, at least in part, to ascribe a disposition to it.

For Cummins, to explain a disposition, there are two complementary strategies: (i) the instantiation strategy, and (ii) the analytical strategy. In the instantiation strategy, a particular disposition of a given object is explained through its subsumption to a dispositional regularity. For instance, we can explain in this manner the disposition of a piece of metal to expand with increasing temperature. In this case, the explanation goes on through the application of a regularity about the thermal expansion of bodies (say, the law of linear expansion), in conjunction with initial conditions and propositions about the particular object at stake. Cummins also designates this strategy as a subsumption strategy, since it involves the subsumption of a particular dispositional event to a dispositional regularity. This strategy is often employed in the physical and chemical sciences, while in biology it cannot be applied to all its subdisciplines or domains with the same strength.

In turn, the analytical strategy proceeds in a rather different way. Instead of deriving a dispositional regularity that specifies a disposition d (in a system a) from the facts of the instatiation of d, the analytical strategy proceeds by analyzing d in a system a into a number of other dispositions, $d_1, d_2, \ldots, d_n$, exhibited by a or components of a such that programmed manifestations of d_i results in a manifestation of d. According to Cummins, these two strategies fit together into a unified account of functions if the analyzing dispositions (d_i) can be made to yield to the instantiation strategy.

In his 1975 paper, after presenting the analytical strategy, Cummins proposes a shift in terminology: "when the analytical account is in the offing one is apt to speak of capacities (or abilities) rather than of dispositions" (Cummins, 1998[1975], p. 187). He offers, then, an example of the application of this strategy, taking assembly-line production as a case in point. Production in an assembly-line is broken down into a series of distinct tasks. Each point of the line is responsible for a given task, and it is the function of the components at that point to accomplish the task. If the tasks of the component parts are realized in an organized way, then the final product follows as a result. Thus, for Cummins, the function of a component of a system is whatever it does that contributes to the realization of a given capacity of the system as a whole, more precisely, the capacity that we are trying to explain.

Cummins' proposal can be better understood if contrasted with the neo-teleological view on functions. First, we have to notice that "while teleology seeks to answer a why-is-it-there question by answering a prior what-is-it-for question, functional analysis does not address a why-is-it-there question at all, but a how-does-it-work question" (Cummins, 2002, p. 158). Second, differently from the neo-teleological approaches, in which the functional ascription is taken to be a functional explanation (that is, to ascribe function to something is to explain its existence), in functional analysis, explanation and functional ascription do not coincide, because they are not dealing with the same level of organization. In this last approach, while we ascribe function to a component of the system, the target of explanation is a capacity of the continent system. In other words, for Cummins, the *explanandum* is not the existence or presence of some item (as in neo-teleology), but a systemic capacity we want to understand.

The explanatory interest of a functional analysis is proportional to "(i) the extent to which the analyzing capacities are less sophisticated than the analyzed capacities, (ii) the extent to which the analyzing capacities are different in type from the analyzed capacities, and (iii) the relative sophistication of the program appealed to" (Cummins, 1998[1975], p. 191). These requirements lead us to the point that the appropriateness of the use of function in biology is a matter of degree. The higher the difference in type and sophistication between analyzed and analyzing capacities, the more adequate will be the use of function. But when the above requirements are not fulfilled, that is, when the difference in type and sophistication between analyzed and analyzing capacities is small, the instantiation strategy is more adequate. This also allows us to understand how the two explanatory strategies can be linked. We explain nomologically when there is no use for function. In other words, scientists often explain complex capacities of systems by analyzing them into the component parts' capacities (using the analytical strategy), until the parts' capacities are better explained through a nomological explanation (using the instantiation strategy). In formal terms, a given capacity of a part that explains a capacity of a whole in a functional analysis can be itself nomologically explained, by appealing to some law of nature and initial conditions. In a series of explanations, moving towards lower-level phenomena, a capacity that, in one explanation, is the *explanans*, and in the other is the *explanandum*, can be the linking element between the analytical and the instantiation strategies, according to Cummins' framework. It is interesting to notice how this approach allows us to conceive of an explanatory integration between different fields – each one focusing on some specific level of organization –, such as sociology, organismal physiology, biochemistry, biophysics, etc.

Finally, we have to notice that, if we understand "teleology" as defined by Cummins (see above), we will conclude that his functional analysis amounts to a non-teleological approach. But if we take teleological explanations to have a broader meaning, as signifying "directed to an end", we will reach a different conclusion. After all, in the analytical perspective, the functions of the system's parts are whatever contributes to the realization of a systemic capacity, and we can see this capacity as an organismic end (such as temperature regulation, blood circulation, etc.). In these terms, Cummins' functional analysis can be seen as a systematization of "intra-organic teleology", as understood by Claude Bernard (1966[1878], for more details, see Caponi, 2001, p. 43).

13.6 Cummins' Functional Analysis Applied to a Biogeochemical System

In this section, we will discuss how the theoretical framework developed by Cummins can be applied to a particular biogeochemical system, which links organisms of the marine biota (mainly phytoplankton) with volatile sulphur compounds and clouds over the oceans. This system was proposed as a negative feedback mechanism that contributes to the regulation of the planetary climate on Earth.

We are going to explore this system in more detail, but let us first examine briefly why we cannot use etiological selectionist approaches as epistemological bases for the uses of function in ecology or Earth system science. From the point of view of these approaches, to explain functionally it is necessary to make reference to an etiology, which, in turn, has to appeal to natural selection as a grounding process. Therefore, the object of the functional ascription has to be generated by a selective process, and this means that it has to be a target of selection. However, natural selection is not thought to act at hierarchical levels as high as those with which most ecologists and all Earth system scientists deal with. We can speak – and not without controversy – about group-level or species-level selection, but we certainly cannot speak about community-level, ecosystem-level or Earth system-level selection. This is an arguably insurmountable obstacle to the application of an etiological perspective to ground functional ascription/explanation in ecology or Earth system science.

Cummins' functional analysis is more adequate to deal with ecological and Earth systems, since it avoids the problems that hamper attempts to apply etiological selectionist approaches to them. Nevertheless, we should also advance in showing that this approach is indeed adequate to treat functional explanation in these systems/fields, as we intend to do here.

The system we take as a case study here was put forward in 1987 by Charlson et al., in a hypothesis that became known in the literature as the CLAW hypothesis, an acronym of the names of the authors of the paper in which it was proposed (Charlson, Lovelock, Andreae, and Warren). Thereafter, we will refer to this system as "the CLAW system". The CLAW hypothesis is based on the well-supported observation that marine phytoplanktonic organisms release a sulphur compound that has an impact on global climate, dimethylsulphide (DMS), which derives from dimethylsulphoniopropionate (DMSP), a compound with several biochemical roles in microalgae (for details on these issues, see Ayers and Cainey, 2007; Nunes-Neto, Carmo and El-Hani, 2009).

This hypothesis considers that "the warmest, most saline, and most intensely illuminated regions of the oceans have the highest rate of DMS emission to the atmosphere" (Charlson et al., 1987, p. 656), and the DMS released in the ocean is fastly ventilated to the atmosphere, where it undergoes a series of oxidations, originating *cloud condensation nuclei* (CCN) for water vapor (see Fig. 13.1 for details). The CCN are acidic particles exhibiting properties that make it possible that water vapour molecules condensate, and, thus, they contribute to the formation of clouds over the oceans. Since clouds reflect solar radiation back to space, they tend to cool the planetary surface. As the concentration of clouds over the oceans increases, less solar radiation reaches the surface waters and this tends – according to the hypothesis – to reduce the heat, salinity and luminosity of the oceanic surface. As a consequence, less DMS is released by the marine phytoplankton and this, in turn, reduces the production of clouds, closing a negative feedback mechanism. This is, in sum, the mechanism proposed by the CLAW hypothesis.

Currently, we have a much richer picture of the mechanisms connecting the elements of the CLAW system (see Fig. 13.1). This is not the space to explore the details, but it is importante to notice, for instance, that not only organisms of the phytoplankton are responsible for the release of DMS. Instead, this is a result of marine food web interactions (involving other microorganisms, such as viruses, bacteria, and zooplankton) (For more information, we refer the reader to Simó, 2001; Ayers and Cainey, 2007; Nunes-Neto, Carmo, and El-Hani, 2009). This system participates in the regulation of the Earth climate, because of its contribution to the regulation of the planetary albedo through its influence on the amount of clouds, and for this reason it has attracted more and more attention in recent years.

To apply the functional analysis strategy to this system, we need, first, to define the systemic capacity we want to explain. This capacity is, in our case, the production of clouds over the oceans and it should be explained by capacities of the component parts of the system. The functions are the capacities of the component parts to which we appeal in order to explain (and, consequently, understand) the systemic capacity at stake. The treatment of the operation of this system in terms of Cummins' framework allows us to notice how the explanatory interests of different research areas are interconnected along levels of organization. For instance, the Earth system science is focused on one systemic capacity of the CLAW system (that is, the capacity of clouds to reflect radiation back to space). To explain that capacity, we have to appeal to the capacities of the component parts: the capacity of

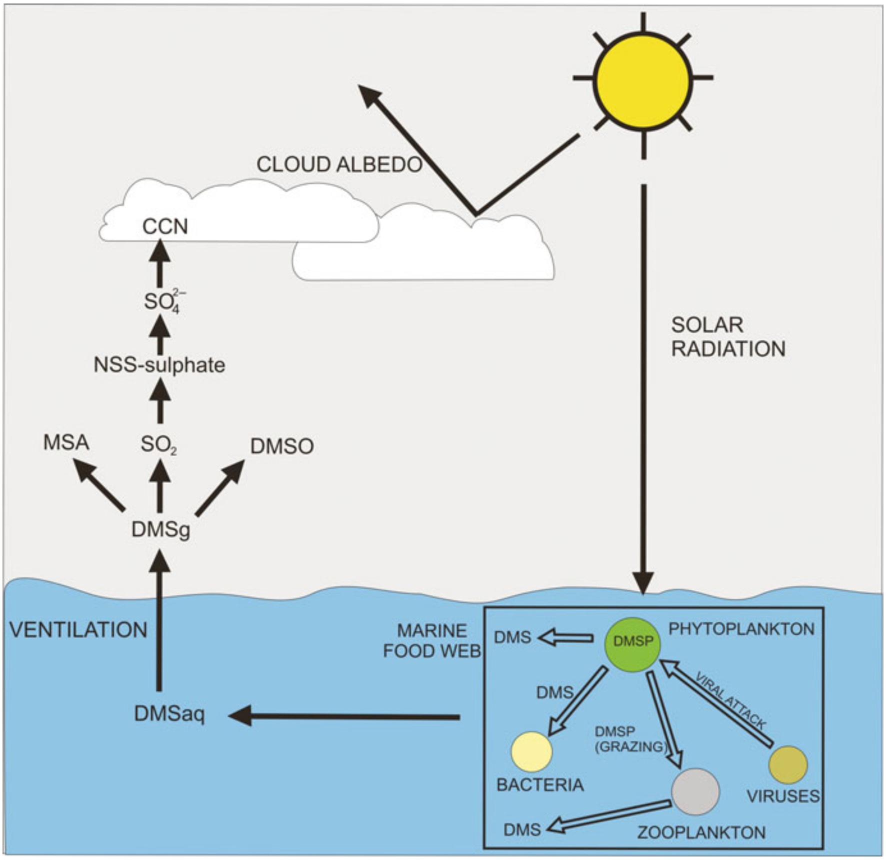

Fig. 13.1 Schematic representation of the main mechanisms involved in the production of DMS by the marine food web. The complex pathways of the sulphur compounds through the biotic and abiotic components of the system lead to the production of clouds over the oceans. DMS_{aq}: dimethylsulphide in the marine water, DMS_g: dimethylsulphide in the atmosphere, DMSP: dimethylsulphoniopropionate, DMSO: dimethylsulphoxide, MSA: methane sulphonic acid, SO_2: sulphur dioxide, $SO^{2-}{}_4$: sulphate ions, NSS-sulphate: non-sea-salt sulphate, CCN: cloud condensation nuclei. For more explanations, see the text (Figure elaborated by the authors)

the marine biota to release DMS, the capacities of the atmosphere that allow the oxidation of sulphur compounds, etc. These are objects of study of marine ecology and atmospheric geochemistry, respectively. If we want to understand, now, the capacity of the marine food web to release DMS, we need to address the capacity of the phytoplankton of synthesizing the precursor of DMS, as well as other physiological capacities of the organisms involved. Now, if we want to understand the capacity of synthesis of the DMS precursor by the microalgae, we are moving to the domain of cell biology. We apply functional analyses until we reach a disposition that cannot be explained through an appeal to function, but, rather, in terms of a dispositional regularity. Here, we are moving into the domain of chemistry and physics, and out of the biological domain. For instance, to explain the phenomena of oxidation of the volatile sulphur compounds we need to appeal to physicochemical laws, i.e., we will explain these phenomena through an instantiation strategy, and it will not be adequate anymore to speak about function. At this point, the functional and instantiation strategies will be integrated in a treatment of the CLAW system according to Cummins' approach.

13.7 Concluding Remarks: A Dualism of Functional Approaches in Biology

In this work, we explored two different philosophical approaches to functional language in biology, giving special emphasis to their domains of application. The etiological selectionist perspectives, such as Wright's, are clearly neo-teleological[1] and have a clear and important domain of application, despite Cummins' arguments, which, albeit recognizing this domain, tend to diminish its relevance. Neo-teleological approaches can explain the spread of traits with novel functions. But we agree with Cummins that they cannot legitimately account for the origins or existence of biological traits. We discussed two reasons for this conclusion. First, biological traits typically come into existence because of their developmental histories, not because they were selected for. Natural selection plays an explanatory role in cases in which the adaptive form of a trait results from a history of accumulating small changes as a consequence of selection, but, even in this case, function itself does not explain the existence of biological traits, since it is the differential efficacy of slightly different states of a trait in performing a function that is selected for. To put it differently, selection does not happen through the differential survival and reproduction of subpopulations showing the function or not, but, rather, of subpopulations all of which exhibit the function at stake, but differ in functional efficacy. Second, the targets of functional ascriptions are not necessarily identified with the targets of selection. In general, these latter targets constitute a subset of the former ones.

However, the fact that an etiological selectionist approach can explain, against Cummins, the spread of biological traits in a population after the emergence of a functional novelty cannot be neglected. This constitutes an important *explanandum* in evolutionary biology, and, as such, it gives room to the legitimacy of application of the etiological selectionist approaches in this scientific field. In other words, our argument saves a significant part of the *explanandum* of weak neo-teleology, even though limiting it more than the advocates of this approach usually propose. The relevance of the etiological selectionist perspective in evolutionary biology is also supported if we notice that it is connected with issues such as the function-accident distinction, which play a key role in evolutionary biology, as we see, for instance,

[1]It is important to remember, however, that Cummins' functional analysis can also be treated as "teleological", depending on how one explains what "teleology" means (see above).

in the distinction between adaptation and exaptation by Gould and Vrba (1982), or between genuine adaptations and fortuitous effects by G. C. Williams (1966).

The etiological selectionist perspective does not apply, however, to the uses of functional language in ecology or Earth system science, because natural (or artificial) selection cannot act at the hierarchical level of the systems addressed by these sciences. To account for functional language in ecology or Earth system science, it is required an approach that deals with complex systems, particularly with part-whole relationships. Cummins' functional analysis is an approach of this kind and we briefly discussed in this paper how it can be applied to the modeling of a biogeochemical system, namely, the system postulated by the CLAW hypothesis. In this model, the functions are the activities of the system's components (solar radiation, marine algae, DMS, etc.) that contribute to the realization of the phenomena explained by the mechanism, i.e., the formation of clouds over the oceans. We would like to suggest that, by extension, we could in principle apply Cummins' functional analysis to other ecological systems, such as plant-animal interacting systems (for a discussion of Cummins' approach applied to ecological systems, see Almeida, 2004).

Our analysis supports the idea that we need a dualism of functional approaches in biology and its philosophy. This is in agreement with Godfrey-Smith's (1993) idea of a "consensus without unity" in the understanding of function, raised against Kitcher's (1998[1993]) unification thesis. For him, Wright's and Cummins' approaches are the two central theories about functions in the philosophy of science, and they cannot be unified into a single approach, because they address different problems and are based on different epistemological grounds. This is in accordance with Cummins' (2002) observation that these two approaches have different *explananda*.

However, we do not think that the "consensus without unity" view should be formulated by appealing to the approaches of particular authors. In our view, we should speak about two different perspectives on function, one etiological, the other, systemic. Wright's and Cummins' approaches have different *explananda*: the former explains the spread of traits with novel functions in a population (and, here, we are also conceiving function differently from Godfrey-Smith, who is committed to the idea that function explains why a given trait is present in a lineage of organisms); the latter explains how the functions of a system's parts contribute to a global capacity shown by the system. But etiological and systemic approaches are not limited to Wright and Cummins, respectively. Other philosophers, such as Ruth Millikan (1998[1989]), Karen Neander (1998[1991]), and Peter Godfrey-Smith (1998[1994]) himself, developed etiological accounts of function. But, even more importantly, there are systemic approaches other than Cummins'. It is more important to lay emphasis on these latter approaches because, while other etiological accounts are often quoted in the literature on functions, systemic approaches are typically limited to Cummins (the very paper by Godfrey-Smith in 1993 is an example). Nevertheless, there are systemic perspectives such as John Collier's (2000a, 2004), which should also be mentioned as alternatives to Cummins' systemic approach to function. This is not the space, however, to discuss the prospects and limitations of these systemic approaches. Our argument here is only that we should take them in due account when discussing function. We should leave a discussion about Cummins' and Collier's approaches to a future paper.

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Chapter 14
On Darwin, Knowledge and Mirroring

Renan Springer de Freitas

When, in the eighteenth century, David Hume proposed that it is crucial for people to believe in what no reasoning or evidence can lead them to believe, he could not surmise that in the twentieth century this proposal would become the embryo of a widely endorsed naturalist project, which has come to express itself in the view that there is nothing to be said about knowledge except what can result from an investigation on the formation of beliefs, whether it be of a sociological, psychological, or biological character. In this paper I will focus on what I deem to be one of the most well known culminations of this project, namely, the Wittgensteinian pragmatism of Richard Rorty, denominated by Rorty himself "epistemological behaviorism" (Rorty, 1980).

As a good naturalist, Rorty could not refrain from radically rejecting epistemology. He focused precisely on the main achievement of the "transcendental philosophy", that which Kant himself called his "Copernican revolution": the thesis according to which knowledge does not derive from the way the world presents itself to the senses but, on the contrary, from the way the human mind "represents" or "constitutes" it when organizing sensorial experience. Rorty rejects that thesis, as he considers it to be the mere culmination of the epistemological project of Descartes-Locke, which, while a tributary of the "platonic principle", that is, of the thesis that some things may be directly known but not others, and its corollary, that the only knowledge that counts as such is the knowledge of what is directly knowledgeable (for only what is directly knowledgeable is real), was doomed to failure from the start. The expression "epistemological behaviorism" refers to the idea that there is nothing else to be said about knowledge except what may result from a socio-historical investigation of the means by which people justify their beliefs – or of the way by which they come to be authorized to believe in what they believe. According to Rorty, there is little to choose from. We either follow "epistemological behaviorism", which ultimately dates back to the sophists (for whom our certainty is a matter of exchanges between people and not of interaction with a non-human reality), or else we follow the "platonic principle". To our misfortune, he goes on

R.S. de Freitas (✉)
Universidade Federal de Minas Gerais, Belo Horizonte, Brazil
e-mail: Springer@netuno.lcc.ufmg.br

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to say, philosophers have chosen the latter, which has resulted in epistemology, the discipline devoted to "the nature, origin and limits of knowledge", as the textbooks define it. The idea that there is such a thing called "the nature of knowledge" subject to being studied by a meta-science, would not make any sense without the notion that knowing is accurately representing what is outside the mind – a notion which is a seventeenth century invention, more specifically one of Descartes and Locke. As to Kant, although his thought is usually seen as a watershed, he (according to Rorty) remained tied to the Cartesian framework and, as much as Locke, strove to solve the problem of how to go from the "inner space" to the "outer space", that is, both thinkers were in search of that which compels the mind to believe immediately upon being brought into its presence. It is because of its effort towards providing an answer to this bad question that the Cartesian epistemological project, of which Kant was but the culmination, was doomed to failure from the start.

Peter Munz (1987) has already take issue with Rorty's overall view of the Cartesian-Kantian epistemological project. Nonetheless, in connection with has been argued thus far, I would suggest that there is still room for further developing the following theses:

(a) admitting that there really is a continuity through Descartes, Locke, and Kant, as Rorty claims there is, and that the epistemological project common to the three of them should indeed be rejected, such a rejection is well deserved in face of the subjectivist and justificationist character of that project, of which Rorty, not by chance, makes no mention whatsoever, and not because the project is intended to "bridge the gap" between the knowing subject and the object of knowledge. Rorty, it must be said, seeks an anchor in Dewey, in Heidegger, and in Wittgenstein – in his opinion, the three greatest philosophers of the twentieth century – so as to decree the impossibility of that transposition.
(b) Rorty has extracted, from the correct idea that the mind does not mirror nature, the equivocal thesis that knowledge does not involve any form of mirroring whatsoever.
(c) Although, in principle, it appears to me that there is nothing wrong with accepting Rorty's invitation to discuss how different patterns of justification of beliefs are established (at the end of the day, "epistemological behaviourism", or what Rorty would later call "edifying philosophy", is indeed an invitation to that discussion), I see the accomplishment of that task as not necessarily incompatible to some epistemological project.
(d) Rorty has not expended any effort to show that Descartes, Locke and Kant exhausted the possibilities of epistemology and that therefore the rejection of the epistemology that is common to the three of them leaves us with "epistemological behaviorism" as the only available alternative.
(e) Rorty's Wittgensteinian pragmatism (and Hume's naturalist project in general) results from a misunderstanding of the philosophical implications of Darwin's theory of evolution.

I shall begin from item "e", as the most important and because it leads, almost automatically, to all the others. With the Cartesian-Kantian view out of the way, Rorty is ready to expose the concept of knowledge that makes him enthusiastic – the concept of Dewey:

> If we have a Deweyan conception of knowledge, as what we are justified in believing, then we will not imagine that there are enduring constraints on what we can count as knowledge, since we will see "justification" as a social phenomenon rather than a transaction between the "knowing subject" and "reality" (Rorty, 1980, p. 9)

The concept of knowledge as that in which "we are justified in believing" is the backbone of Wittgenstein's version of pragmatism recommended by Rorty. Nonetheless, the only reason indicated by Rorty for recommending that concept is that, by accepting it, we can refrain from "imagining that there are enduring constraints on what we can count as knowledge". I agree that the quest for such "enduring constraints," that is, "for those privileged items in the field of consciousness" which can be considered "the touchstone of truth" (Rorty, op. cit., p. 210), is not a promising path. But from this it does not follow that we have to resort to the Deweyan concept of knowledge, as Rorty advises us to. In reality, long before Rorty assailed the project of searching for said "enduring constraints," Popper, whom Rorty never mentions, had already demolished it with an argument that, curiously enough, Rorty himself came to expose in his book when he quoted the following passage from *Science, Perception and Reality*, by Wilfred Sellars:

> science is rational not because it has a *foundation,* but because it is a self-correcting enterprise which can put *any* claim in jeopardy, though not *all* once (Rorty, op. cit, p. 180, italics in the original).

I totally agree with that statement (which Popper would perfect by replacing the expression "put any claim in jeopardy" with "submit any statement to criticism"), but I cannot see how to reconciliate the idea, absolutely correct for me, that science is a self-correcting undertaking, with the idea, recommended by Rorty, that knowledge is that in which we are justified to believe. I think that, if science is a self-correcting undertaking, it is so exactly because what is important in regard to scientific theories is the fact that it is not necessary "to be justified in believing" them. What *is* needed, is that we are able to criticize them, which supposes understanding some of their implications, deriving some of their testable consequences, comparing them to other theories, and explaining why they describe the trajectories they do throughout time. In other words, Popper's idea, which Sellar and Rorty also arrived at, that science is a self-correcting undertaking, demolishes not only the foundational project against which Rorty rises, but also the idea, advocated by Rorty, that knowledge has something to do with belief or justification of a belief.

As I understand it, the thesis according to which science is a self-correcting undertaking results from an application of the Darwinian biological model to a theory of the growth of knowledge. As is known, Darwin proposed that species evolve by means of blind mutations in individual organisms, and of the retention of those few mutations which have some selective value. By "selective value," one should understand the capacity to generate descendants deriving from the ability to mirror regularities that do in fact occur in the environment. In following that track, Popper proposed that knowledge advances by means of conjectures (the epistemological correlate to blind mutations) and refutations (the epistemological correlate to selective retention). "Self-correction", in this perspective, means a gradually progressing capacity to generate new problems whose solutions (that is, conjectures or theories) only deserve such names because they contain some true information about the world. In this sense, theories do mirror, if precariously, the regularities that really take place in the environment. Since, for Rorty, knowledge cannot bear any relation to mirroring, or with containing true information about the world ("truth", Rorty relentlessly repeats, even "the truth" of feeling a sudden pain in the stomach, is only what our peers allow us to say without contesting us), I fail to understand what "self-correction" would mean in his perspective.

If we admit that knowledge involves some sort of mirroring, and that this can only be understood with Darwin's help, so "thinking of human beings in Darwinian terms" (to take Rorty's own terms) involves, above all, and in exact opposition to what Rorty suggests, admitting that the gap between the knowing subject and the object of knowledge may be abridged. If, despite the foregoing, Rorty so emphatically denies the possibility of such abridging, it is because, once having associated knowledge with the justification of beliefs, there was nothing else left for him to discuss but whether the justification is a "social phenomenon" or a "transaction between the knowing subject and reality." Since, obviously, justification is a social phenomenon, for it involves above all obtaining the agreement of our peers, then the path was clear for his "epistemological behaviorism": if justification is not a "transaction between the knowing subject and reality", Rorty argues, and if justification is what matters, then let us forget the "transaction between the knowing subject and reality". Rorty did well to ignore Popper, and would do even better if he had ignored Darwin, for what both invite us to say is exactly the opposite: if justification is not a "transaction between the knowing subject and reality", and if such a "transaction" is what really matters, then let us forget about justification – or, if we cannot forget about it, let us take it for what it is worth: a frustrating booby prize.

Nonetheless, the concern with discussing whether our beliefs derive from our cognitive faculties or from our social experience is so central to Rorty that, ultimately, that which he regards as the principal fault of the "platonic principle" and, as a corollary, of the whole epistemological project of the seventeenth century, which has restated it, is having thought that all our beliefs derive from our cognitive faculties. In other words, if, to Rorty, the Cartesian-Kantian tradition has failed, that was mostly due to its being unable to correctly explain the origin of our beliefs. The passage below shows that with exceptional clarity:

> It is so much a part of "thinking philosophically" to be impressed with the special character of mathematical truth that it is hard to shake off the grip of the Platonic Principle. If, however, we think of "rational certainty" as a matter of victory in argument than of the relation to an object known, we shall look toward our interlocutors rather than to our own faculties for the explanation of the phenomenon. If we think of our certainty about the Pythagorean Theorem as our confidence, based on experience with arguments on such matters, that nobody will find an objection to the premises from which we infer it, then we should not try to explain it by the relation of reason to triangularity. Our certainty will be rather a matter of conversation between persons, rather than a matter of interaction with nonhuman reality (Rorty, op. cit, pp. 156–7)

One is immediately struck by the narrowness of the choices offered by Rorty: we either look at our own faculties or to our interlocutors. Rorty does not envisage a third possibility: of looking at the trajectory described by the products of our thought, that is, our conjectures, trying to understand why that trajectory is as it is. In that sense, the example of Pythagoras' Theorem comes in handy. While Rorty is concerned about what gives us the right to believe that this theorem is true, an author like, say, Whitehead, would strive to show what was made of Pythagoras thought over time (Whitehead, 1953, pp. 42–43). This led him to trace a direct line from Pythagoras to Einstein. Einstein, he explains, is a tributary of the Pythagorean idea that the shape of a figure is an impure mathematical entity, for such an idea is fundamental for the thesis according to which physical facts such as gravity should be reconstructed as revelations of local peculiarities of space-time proprieties.

If we insist, as Rorty does, on linking the question "how to bridge the gap between us and Pythagoras' theorem" to the other question "what determines (or what authorizes) our belief in such a theorem", we will inevitably find that our belief is determined by a peculiar form of conversation between ourselves and our masters and colleagues and, as a consequence, arrive at the conclusion that the gap is insurmountable. But if we link that same question to the question of how it is possible that there is a link between that theorem and a huge set of propositions formulated posteriorly, among which is the Einsteinian proposition that gravitation should be reconstructed as a revelation of local peculiarities of space-time proprieties, then we may entertain some hope of bridging the gap between us and the Pythagorean Theorem. This brings us back to a point that was only mentioned above in passing: Rorty is right when he says that the mind does not mirror the world, but that does not imply that knowledge does not involve some form of mirroring. While the mind does not mirror the world, the objective products of our mind may do it. While Pythagoras' mind does not mirror the world (it only produces conjectures in abundance, just like any other mind), the Phytagorean thought, insofar as it clears the path for a more comprehensive thought in which it can go on living as a particular case, does.

To do him justice, Rorty did not neglect questions regarding the trajectories of ideas or theories. After all, a good deal of his book is an effort to explain how the "Platonic principle" came to culminate in the "Copernican revolution" of Kant – or even in the "neo-Kantian consensus" of the nineteenth century. On the other hand, in view of the fact that what Plato and Kant have in common is the belief in the existence of the ultimate foundations for validating knowledge, Rorty, as a good pragmatist, has striven to explain the origin of such a belief. In both cases, I wish to argue, the result was mutilated Darwinism.

The Darwinian view, as widely known, postulates the existence of two complementary evolutionary mechanisms: mutation, which is accidental, and selective retention of some mutations. In Rorty's Darwinism, however, there is place only for accidents. How, for example, did the "Platonic Principle" come to culminate in the neo-Kantism? Through an accumulation of errors, implies Rorty. The "Platonic Principle" is itself a mistake, to which another mistake was later added: the "invention" of "the mind" by Descartes, to which yet another mistake was added, that of

Locke (who mistakenly assumed as possible that there was a connection between "mind" and the external world), which in turn led to yet one more mistake, that of Kant, who equivocally thought it possible to bridge the gap between mind and world by postulating the existence of concepts capable of organizing, a priori, our sensorial intuition. Finally, the neo-Kantism of the nineteenth century lent new features to this mistake, as it sought in language, as a substitute for the abstract concepts crowding the human mind, the link between the knowing subject and the world. In this sense, neo-Kantism is the final product of "an original wish to substitute *confrontation* for *conversation* as the determinant of our belief" (Rorty, p. 163, original italics).

How could so many mistakes be accumulated without nothing being learned from them (in the framework of a Darwinian view, this would be unconceivable!), and why were precisely these mistakes, and not any others that, so to speak, "made the history" of modern philosophy? If I have understood Rorty's argument correctly, his answer would be something like this: the above mistakes could not only prosper, but also "make" the history of modern philosophy because they are rooted in a metaphor used to talk about knowledge, a Greek metaphor that equivocally associates *knowing* with *visually perceiving*, the ocular (or perceptual) metaphor that, however improper (a proper metaphor would associate knowing with justifying one's beliefs before one's peers, and not with visually perceiving) is endorsed by Western culture. There is, in principle, a whole range of metaphors that can be used to talk about knowledge (we can, for example, associate knowing with crushing something under our feet or to more interesting alternatives, which Rorty's reader can find on page 39 of his mentioned book), but, unfortunately, "the imagination of the founding fathers of Western thought" happened to be "captured" by this ocular or perceptual metaphor. Since the "Western mind" came to be dominated by this unfortunate metaphor, any philosophical notion that is rooted in it, no matter how inappropriate, is a serious candidate for the position of dominant philosophical notion. Well, there is nothing more deeply rooted in the ocular metaphor than Descartes' "mind eyes", or the distinction, crucial to Kant's "Copernican revolution," between intuitions and concepts. Thus, these were the notions that made the history of modern philosophy.

By offering this answer, Rorty ends up answering, by implication, the question that every good pragmatist has a duty of trying to answer: what is it that grants modern philosophers the right to believe in the existence of ultimate foundations for validating knowledge, that is, of some sort of knowledge beyond any need of justification? The answer is that they had their imagination captured by the ocular metaphor. Insofar as Westerners have acquired the habit of taking the visual perception as a model to talk about (or even to conceive of) knowledge, they have gone on to think that in the same way that it is not possible to doubt what the body's eyes see, it is equally impossible to doubt what the "inner eyes" see (from crude sensations to the axioms of geometry) and, therefore, that the truth of what the eyes see (whether the eyes proper or the "inner eyes") imposes itself so absolutely that no additional justification is necessary.

We can now understand why Rorty's effort to explain, on the one hand, how the "Platonic Principle" culminated in the "Copernican revolution" and, on the other, the belief of modern philosophers in the idea of ultimate foundations of knowledge, is a result of a mutilated Darwinism, that is, of a Darwinism that focuses on the accidents but ignores what is more important: the selective process by which some of these accidents are retained. To Rorty, both the successive mistakes that culminated in Kant, and the belief of modern philosophers in the existence of foundations of knowledge, derive exclusively from an accident, namely, that Westerners decided, for "no particular reason" (Rorty, cited, p. 38), on a peculiar way to talk about knowledge – the ocular metaphor. If Rorty's discussion about the ocular metaphor were informed by a genuine Darwinian view of knowledge, he neither would have pointed to that metaphor as the cause of the damaging effects that he has indicated, nor would he have seen it as a mere accident.

According to Rorty, there are two damaging effects of the ocular metaphor. First, it leads us to assume that our beliefs derive from our having been brought directly into the presence of the object of our belief – "the geometrical figure which proves the theorem", as he says on page 163. Second, it leads us to assume that we are capable of apprehending universals, that is, to assume that in the same way the human eye records the presence of singular entities, such as this or that frog, the human mind records that which would be proper to "the frog." I wish to argue that the ocular metaphor is innocent in regard to the first count, and, though it may well be guilty with respect to the second accusation, there is nothing either accidental or damaging in that respect. Let us examine each accusation in turn.

As far as the first one is concerned, Rorty's own example is proof of the innocence of the ocular metaphor: contrary to his assertion, geometrical figures do not prove theorems. If we resort to a geometrical figure to demonstrate a theorem, that is due to a cognitive limitation (which a computer, for example, does not face), perfectly explained in evolutionary terms, and not because we are subservient to the (according to Rorty, arbitrary) cultural prescription that we cannot doubt the truthful character of that which is immediately brought into our presence. In other words, if we resort to vision to make up for our incapacity for abstraction beyond a certain limit – our incapacity, for example, to understand what a rectangular triangle is without "being brought into the presence" of the figure of a rectangular triangle, this is not because the "Western mind" came to be "dominated" by an unfortunate metaphor, as Rorty's mutilated Darwinism suggests, but because the role of vision in human evolution is fundamental, as explained, for example, by Jacob Bronowski (Bronowski, 1978). According to Bronowski, we are indeed captives of the inner eye metaphor, not by accident, but by simple reason that our intellectual activities are enormously conditioned to what the human eye can and cannot do. From this perspective, the important effect on us of being brought face-to-face with a given object is not, as suggested by Rorty, that of believing in this object but, rather, of becoming capable of creating images in our minds, that is, imagining (the very use of this verb shows how captive we are of the ocular metaphor) that which cannot be literally brought into our presence. In a final analysis, Bronowski's book shows that Rorty rejects the "ocular metaphor" by unfairly blaming it for what it is not guilty of. Rorty rejects it because he thinks that it leads us to the mistake of assuming that perception directly accounts for our beliefs. But that is not the effect of this metaphor. The effect of this metaphor is not to lead us to believe in "clear, distinctive ideas," in the style of Descartes, or in "primary qualities", as does Locke, or in necessary truths, as does Kant, but, rather, to establish a nexus between our capacity to perceive visually and our capacity to imagine that which it is not possible to visually perceive. In short, the ocular metaphor helps us understand that what is important in regard to visual perception is not, as Rorty suggests, the fact that it leads us to some sort of equivocal belief but, rather, that it makes our imagination feasible.

The second crime committed by the ocular metaphor is, according to Rorty, that of not subscribing to the Sellarsian nominalism that he recommends, that is, of leading us to suppose that, when, for example, we have a painful sensation, we "recognize" a certain singular entity, "the pain," to which our "inner eye" was previously "introduced" (something analogous to recognizing someone who has been introduced to us before), instead of leading us to suppose, as Sellars nominalism would have us do, that pain is no more than a name to which people resort, without being contested by their peers, to describe a particular state of painful sensation. I think that this accusation can be accepted by the ocular metaphor without any guilty feeling. After the advent of the theory of evolution by natural selection, it is hard to understand how someone can assume that as a result of chance, or of the ignorance of the Greeks, that there is something beyond this or that frog, or this or that painful sensation.

Unless the idea of evolution by natural selection proves untenable, there is nothing wrong with postulating that knowing involves "recognizing" in particular singular entities a "previously known" universal, for such "recognition" is a fundamental selective mechanism. In other words, Darwin showed that Plato was not as mistaken as Rorty supposes: "knowing" does really involve "recognizing" something to which we have been previously "introduced." A chicken "knows" a grain of corn insofar as it is able to "recognize" in a grain of corn an instance of the universal "corn." A chicken incapable of such a "recognition" would eat, if any at all, only the first grain of corn. It would not eat a second one, for it would have no means of "knowing" that that second one is also a grain of corn and so would then starve to death. Thus, a chicken that is unable to apprehend the universal "corn" is not selected for reproduction.

I do not know whether the ocular metaphor is in some way responsible for our assumption that we are able to apprehend universals but, if it is, this is nothing it should be ashamed of. If Rorty condemns it, that is only because, in spite of his compliments to the (alleged) Darwinian naturalism of Dewey, he thinks as if Darwin had never actually existed. The strongest evidence that Darwin never existed lies in Rorty's recommendation that philosophers should confine themselves "to pointing out particular states of affairs" (p. 38), instead of pointing at regularities to which such particular states of affairs are subject. He recommends, for example, that we limit ourselves to talking about people feeling pain, or about people having beliefs, instead of talking about pain and beliefs.

I would like to close this discussion by suggesting that Rorty is right in proposing that we be "naturalist enough to think of human beings in Darwinian terms". The naturalism to which Darwin leads us is not, however, the one that culminated in the naturalism advocated by Rorty. If we understand the philosophical implications of the theory of natural selection, we see that Hume could only propose that it is vital for human beings to always believe in what they have no reason to believe, and from that invite us to join in the development of his naturalist project, because he had a pre-Darwinian view of knowledge. He supposed that the knowledge of particularities was possible ("this glass of water has quenched my thirst") without the previous – *hypothetical* – knowledge of universal laws ("water quenches thirst"). Rorty's proposal that we should limit ourselves to pointing out particular states of affairs is just a regrettable legacy of this pre-Darwinian view of knowledge underlying Hume's naturalist project.

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Chapter 15
Freudian Psychoanalysis as a Model for Overcoming the Duality Between Natural and Human Sciences

Richard Theisen Simanke

15.1 Introduction

The methodological (and, ultimately, ontological) dualism that opposes natural and human (or social) sciences was born out of the German neo-Kantian environment of the late nineteenth century and organized a great deal of the epistemological reflection during the twentieth century. For as long as the logical positivist philosophy of science has prevailed, this dualism has often taken the form of a division between those sciences which had and those which did not have a concrete possibility of fitting into the epistemic model of the received view of science. The philosophical critique of this model, however, was not immediately followed by a systematic challenge of the division of the field of scientific knowledge between natural sciences and the humanities. Freudian psychoanalysis, which arose more or less at the same time as that duality was established, always remained, however, completely impervious to it. While explicitly aligned to the naturalistic standpoint, Freud's psychoanalytic investigations readily entered the field of the humanities and set out to elaborate a social theory encompassing art, religion, language, the social bond and culture as a whole. This paper discusses some of the epistemological commitments presupposed by this approach, especially those that allowed it to ignore that now long-established categorization. Freud's research could thus serve as a model (or, at least, as an exemplary case) for the discussion of these matters in contemporary epistemology.

We are concerned, therefore, with discussing the Freudian positioning, not seeking merely a better understanding of its internal logic and theoretical articulations, but also exploring the possibility of obtaining from it certain useful insights in a broader epistemological reflection. The exposition that follows, then, synthetically presents: (1) some of the historical and philosophical questions involved in the distinction between human and natural sciences; (2) the discussion of the position of Freudian epistemology in this context, illustrated by a fairly exemplary conceptual development as to how Freud seems to overcome this dichotomy; (3) a brief

R.T. Simanke (✉)
Federal University of Sao Carlos, São Carlos, Brazil
e-mail: richardsimanke@uol.com.br

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presentation of some guidelines for a program of epistemological reflection capable of leading to the systematic formulation of an integral and qualified naturalism, such as might be intuited based on the example of Freudian psychoanalysis.

15.2 Epistemological Dualism

The opposition between human and natural sciences was born out of a defensive strategy against the progressive extrapolation of the Galilean-Newtonian model of physics for other fields of science. As we know, this extrapolation was, under various guises, a flag of the Enlightenment of the eighteenth century and of its project for a reformation of society through Reason, and culminated in the naturalist program for social sciences, the principal exponent of which, during the nineteenth century, was Comtean positivism. The anti-naturalist reaction inflamed at the end of the nineteenth century, above all by the neo-Kantian German philosophers (Rickert, Windelband, Dilthey), is characterized, in principle, by the affirmation of the methodological specificity of the *Geisteswissenschaften*, condensed in the celebrated opposition between explanation and understanding. In its own way it recovered the Kantian antinomy between nature and liberty and built upon it a program of investigation for the whole sphere of knowledge which concerns itself with human action and its products, as well as the understanding that the agents have of themselves, embracing disciplines as different as the law, history, grammar, literary criticism, among others. This line of reasoning swiftly drifted from the methodological plane to ontology, and the irreducibility of human sciences came to be justified in terms of the ontological specificity of its objects – the human being and the products of his action – which, in one way or another, would constitute themselves as exceptions to the natural order.

Despite its origin at a fairly precise historical moment and in a fairly precise philosophical context, the distinction between human sciences and natural sciences became such a rooted way of thinking that these categories rarely failed to appear in later epistemological discussion, throughout practically the whole of the twentieth century – and, it may be said, even today. In particular, the ontology presupposed by this distinction came to be, most of the time, assumed in such a spontaneous manner that the attempts to surmount this dichotomy were directed, above all, at its methodological aspects, leaving intact the ontological difference between the human and the non-human, considered since then to be identical to the distinction between non-natural and natural, respectively. Thus, for example, linguistic structuralism and French anthropology of the years 1940–1950 proposed, in a general way, to get beyond the alternative between explanation and understanding, endowing social sciences with strategies of analysis, theorization and formalization comparable in rigor to those of the natural sciences, but completely assuming the ontological fracture between the two domains and, practically, erecting it as a dogma. Everything occurred as though the distinction between the natural and the artificial – between what does not depend and what depends on human action to exist – continued to be contemplated according to a rather simplified version of the Aristotelian distinction

(Physics, II, 192b) and it was possible to ignore the explosive development of the natural sciences in the Modern Age and, more specifically, the life sciences, after the Darwinian revolution of the nineteenth century, with all the more or less evident challenges that these presented to the anthropomorphism and "exemptionalism" (Catton and Dunlap, 1978) implied in that distinction.

The defensive rhetoric that the affirmation of the specificity of the humanities inherited from its origins remained, throughout its historical development, as one of its distinctive characteristics. It encountered its antagonist and, at the same time, a sort of justification in the specific version of scientific naturalism proposed by logical positivism (or neopositivism), the philosophy of science of which enjoyed a certain prevalence between the decades of the 1930s and 1960s of the twentieth century. This philosophy rescued the original positivist program for the purification of the sciences of the vestiges of metaphysics that these might continue to carry built into their theories, establishing a demarcation between science and non-science (or between science and pseudoscience) and the identification of the criteria for this demarcation as its principal objectives. It endorsed, furthermore, a Humean conception of causality as natural contingent regularity, excluded as metaphysical residue any proposition with respect to entities or processes incapable of being observed, and proposed, in consequence, a logical-syntactical conception of scientific theories, as systems of deductively articulated enunciations, in which the functional relations between variables (relating to observable particulars) could be subsumed under progressively more wide-ranging general laws, up to the ideal limit of universality. This vision of science was modeled on the mature sciences – physics, basically – and used, then, as a parameter for the evaluation of the pretensions of scientificity of the other disciplines. As a result, it presented itself as an epistemologically reductive program (all sciences should be reduced to physics) or, in the most drastic versions, eliminative (all sciences should be replaced by physics).

The pretensions of this program make more understandable, to a certain extent, the defensive attitude mentioned above. In the distinction between the disciplines capable or incapable of fitting into the model supplied by the "received view" of science, the human sciences were always at a disadvantage, with no option but the strategy of claiming to belong to another order of scientificity. However, the challenge and the eventual dissolution of the neopositivist program, at the end of the 1960s decade, did not lead, as perhaps might have been expected, to a critical analysis and to a comparable challenge of the epistemic duality which was opposed to its project of the unity of science. Very often, the debate between naturalism and anti-naturalism – in its methodological, epistemological and ontological varieties – occurred, on the part of human sciences, as if the positivist version of scientific naturalism was the only one possible, in such a way that "resistance" to positivism implied, in itself alone, the refusal of naturalism. However, the recent developments within natural sciences appear to have made more urgent the updating of this discussion, the further that these advance upon areas of knowledge traditionally reserved to the humanities – we could cite, as examples, the rise of sociobiology, in its relationship with the social sciences, and the expansion of the neurosciences, in relation to sciences of the mind. When we discuss the emergence of new subdisciplines (or specialties), such as neuro-ethics or human ecology, it is possible to begin to doubt whether the belonging of these subspecialties to the field of human sciences or of natural sciences is still a productive or epistemologically fertile question.

In relation to psychology, in particular, this dichotomy historically presented itself counterproductively. From its origins, the scientific status of psychology – its belonging to one or other of the opposing camps – remained undefined, as in the debate that set Dilthey against the philosophers of the Baden school, for example. Throughout its historic development, this oscillation was no small factor in the fragmentation which affected the field of psychology, leading it to distribute itself through a plurality of concurrent research programs, some of which were inclined towards naturalism (functionalism, the different behaviorisms), while others willing aligned themselves with the humanities (humanist and phenomenological psychologies, cultural psychology, etc.).

In this context, Freudian psychoanalysis presents itself as a notable exception, although the emergence of post-Freudian currents has inevitably been affected by the same dilemmas of psychology in general, being able to find there both anti-naturalist psychoanalyses (North American culturalism, existential psychoanalysis, Lacanian psychoanalysis) as well as naturalist ones (the ego psychology and, more recently, neuropsychoanalysis). For Freud, in contrast, the affinity between psychoanalysis and natural sciences always seemed evident and beyond any doubt. However, certain consequences usually associated with this position do not seem to have been equally assumed by Freud, and his efforts at theorization led him very early on to areas traditionally reserved for history, aesthetics and the sciences of culture in general (the social contract and the social bond, art and religion and, on a lesser scale, education and labor). It is this singularity that makes it epistemologically interesting. In what follows, this Freudian attitude is presented and illustrated, with further discussion of some of its presuppositions and implications.

15.3 Freud and the Unity of Science

One thing strikes the attention about Freud's naturalism: this never seems to have been, for him, a position adopted within an alternative considered as valid. That is to say, everything happens as though Freud had never considered the possibility of another model of science which was not that of the sciences of nature (Assoun, 1983). Thus, in one of his last works, left unfinished and only published posthumously, we can read: "Psychology, too, is a natural science. What else can it be?" (Freud, 1940, p. 282). This is not only a late stand, but the reiteration of an epistemological attitude that dates from his training as a researcher in the areas of neuro-anatomy and clinical neuropathology and that, contrary to what a good deal of the official historiography of psychoanalysis would make believe, was never abandoned nor significantly altered when Freud stamped a more psychological orientation on his research. We can find scattered throughout the whole trajectory of his work, fairly decisive and unequivocal affirmations that psychology and psychoanalysis should, ultimately, find their basis in neurology and biology. Exclusively psychological models for the explanation of mental processes – the whole array of which Freud labeled metapsychology – should be, thus, considered as provisional constructions, in wait for when the advance of knowledge about the brain and the nervous system should make it possible to replace them with a more definitive theory and one closer to the reality it is trying to understand (Freud, 1913, p. 179; 1914, p. 78; 1915, p. 175; 1920, p. 60, among others).

However, this general epistemological orientation did not prevent Freud from extending the application of psychoanalytical concepts to cultural questions, and his range of interests in this field was always fairly wide-ranging, without, however, representing a moving away from his naturalist positions. To give a few instances, Freud understood art as a sublimation, one of the possible destinies of the drives (or instincts); he approached the emergence of culture through a speculative elaboration constructed on the basis of a Darwinian hypothesis concerning the primitive social organization of hominids; he placed religion alongside obsessive neurosis, extending to the former the explanatory model of the latter; he developed an original approach to sociability, explaining the social bond as a result of the transformation of libidinous choices in a complex system of crossed identifications of the members of the group among themselves and their leaders; he formulated an essentially anti-utopian social theory, justifying the irremediable discontent in culture as a vicious circle, in which the repression of sexual and aggressive instincts produces frustration, which generates more aggression and demands more repression, and so on. Despite a certain reductionist risk which results from this approximation between biological and cultural themes, the productions of Freud in this field were always, in general valued and considered to be original. Nevertheless, this same positive evaluation seemed often to require disregard of the naturalist context in which they were elaborated, reducing it to a personal idiosyncrasy or eccentricity of Freud, a sentimental attachment to the epistemic ideal acquired in youth, rendered anachronic by the very originality of the theory. In this way, Freud could be presented as a theoretician of the rupture between nature and culture – for example, in his Lacanian reconstruction, inspired by Lévi-Strauss' structural anthropology. However, there is strong evidence to the contrary. For example, in a late text, dedicated to a historical speculation on the origins of Judaism – and, therefore, supposedly distant from the more biologizing constructions of metapsychology – we can read:

> We are diminishing the gulf which earlier periods of human arrogance had torn too wide apart between mankind and the animals. If any explanation is to be found of what are called the instincts of animals [. . .], it can only be that they bring the experiences of their species with them into their own new existence [. . .]. The position in the human animal would not at bottom be different. His own archaic heritage corresponds to the instincts of animals even though it is different in its compass and contents (Freud, 1939, p. 100).

Nevertheless, this discrepancy might be merely apparent, or else result, as Habermas argued (1972), of a scientist self-misunderstanding on the part of Freud. The standard argument here is that Freud made an original discovery – the immanence of meaning to mental life, the transforming and emancipating role of language and of interpretation – that ought to belong, entirely, to the sphere of the humanities;

however, his attachment to an outdated model of scientificity (naturalist, physicalist, positivist, etc.) led him to fruitless and misleading attempts to translate his discovery into terms accepted by the natural sciences. It was this that led to the bizarre inconsistencies and approximations that were mentioned above (between art and instinct, religion and neurosis, social bond and libido, culture and Darwinism). Hence, to respond to this reading, it would be necessary to argue that these apparently discrepant formulations could be compatible and integrate within a consistent theoretical totality, the principles of which, however, would still need to be specified. It is impossible to do this systematically in the space available here. For this reason, let us examine just one illustrative example, referring to two supposedly different formulations, belonging to two periods of Freudian theorization fairly separated in time, and to texts focused on, in principle, divergent problematics – metapsychology (or neuropsychology) and social theory. In the event it is possible to demonstrate that, beyond their differences, these formulations are compatible or, better still, are mutually dependent, we would then have a point of departure for suggesting the unity of Freudian thinking and the solidarity of this thinking with his conception of science.

Well known and much commented upon is the passage with which Freud opens his work *Group Psychology* (1921) in which he investigates the psychological bases of the social bond starting from a profound reflection on the genesis of the Ego and of its ideal correlates (*Ich-Ideal, Idealich*) through the vicissitudes of object relations and identification. There he enunciates the impossibility of separating completely the psychology of the individual and social psychology, due to the fact that the "Other" is always, in some way, implied in the constitution of the Ego:

> The contrast between individual psychology and social or group psychology, which at a first glance may seem to be full of significance, loses a great deal of its sharpness when it is examined more closely. [. . .] only rarely and under certain conditions is individual psychology in a position to disregard the relations of this individual to others. In the individual's mental life someone else is invariably involved, as a model, as an object, as a helper, as an opponent; and so from the very first individual psychology [. . .] is at the same time social psychology as well (Freud, 1921, p. 69).

It is easy to understand that affirmations like this have been widely exploited, for example, by French psychoanalysis (Lacan, Laplanche, among others), of radically anti-naturalist inclinations. In fact, the French psychoanalysis, philosophy and human sciences of the period are almost saturated with this discourse on otherness, derived from the neo-Hegelianism propagated, in the first decades of the last century, by thinkers such as Wahl and Kojève. The latter, above all, by reinterpreting the phenomenology of the spirit of Hegel as a theory of anthropogenesis, rejects the dialectical character of nature and reserves the negativity exclusively for history, the reason for which the properly human subject would only add to the world through his desiring and negatory action of the natural fact, thus providing a philosophical warrant for the thesis of the rupture between nature and culture which later anthropology would explore at great length. The relationship with the Other moves, then, to the foreground: given that the desire for the natural "thing" is not humanizing, it only remains for the pre-human animal to desire another desire, that is, the desire of the Other, in the double sense of desiring what the Other desires and of desiring to be desired by the Other. Human subjectivity would only take shape within the ambit of this "multiplicity of desired desires" and, therefore, only in a social environment, where the reference to otherness would fulfill an effectively constitutive role. Nevertheless, if we go back to one of the first substantial theoretical works produced by Freud – the neuropsychological manuscript known as *Project for a Scientific Psychology* (1895/1950) –, we can find there something like the metapsychological foundation of this unavoidable participation of the other in the Ego, but in a context impregnated with a psychological naturalism totally alien to the theoreticians of otherness mentioned above.

This psychological naturalism is affirmed at the beginning of the work, as though constituting its program. In Freud's vision, a psychology presented as a natural science implied a clearly materialist and reductionist attitude: "The intention is to furnish a psychology that shall be a natural science: that is, to represent psychical processes as quantitatively determinate states of specifiable material particles [. . .]" (Freud, 1895/1950, p. 295). This reductionism, however, did not entail, for Freud, eliminating from his project the reference to the subjective and qualitative dimension of the mental – in a word, to the consciousness. Much to the contrary, the need to tackle this dimension presents itself as a very clearly formulated demand:

> Hitherto, nothing whatever was said of the fact that every psychological theory, apart from what it achieves from the point of view of natural science, must fulfill yet another major requirement. It should explain to us what we are aware of, in the most puzzling fashion, through our "consciousness" [. . .] (Freud, 1895/1950, p. 307).

Freud is very explicit when distinguishing the qualitative consciousness of unconscious processes which would be defined in an exclusively quantitative manner: "Consciousness gives us what are called qualities" (Freud, 1895/1950, p. 308). In the same passage in which he rejects an exclusively mechanistic vision of the mind, which would exclude consciousness, he also makes it clear that, with consciousness, subjectivity and experience emerge: "Here consciousness is the subjective side of one part of the physical processes in the nervous system" (ibid., p. 311). In the light of these affirmations, it would be no exaggeration to consider Freud as a precursor of the contemporary programs of naturalization of consciousness and of phenomenology (Petitot et al., 1999).

This attention to the phenomenological dimension of the mind reappears in the central role played by the two fundamental experiences (*Erlebnisse*) described in the *Project* – the experiences of satisfaction and pain – in the structuring of the psychism. In the description of the consequences of these experiences, the constitutive role of the relationship with the other – the similar, the fellow being (*Nebensmench*) – in the genesis of the Ego and in the formation of the identity is discussed in detail. Themes familiar to philosophical anthropology, such as the original helplessness (*Hilflösigkeit*) of man, reappear in this context. Thus, in the analysis of the experience of satisfaction, in the course of which the first definition of the psychoanalytical concept of desire is formulated, Freud considers how the emergence of an organic need – hunger, for example – and of the displeasure that accompanies it, initially encounters an organism unprepared to provide it with satisfaction and that attempts, in vain, to discharge the excess of excitement through the reflex path – psychomotor agitation, crying and screaming. However, the removal of the endogenous disturbing stimulus caused by the need requires another form of action, which Freud terms "specific action": a coordinated action appropriate to goals that impinge upon the external world, capable of locating the food and placing it within reach of the organism, presupposing thus, a series of capabilities as yet un-acquired, such as voluntary mobility, rememoration and recognition of objects, judgment, examination of reality, etc. But, although inefficient, the reflex actions of which the new-born child is capable fulfill a "secondary function": they serve as an appeal so that another person will provide the helpless infant with the assistance it needs to survive:

> At first, the human organism is incapable of bringing about the specific action. It takes place by extraneous help when the attention of an experienced person is drawn to the child's state by discharge along the path of internal change [e.g. by the child's screaming]. In this way this path of discharge acquires a secondary function of the highest importance, that of *communication*, and the initial helplessness of human beings is the primal source of all moral motives (Freud, 1895/1950, p. 318, author's italics).

An important series of Freudian concepts is condensed in this passage, such as the origin of language in the prototypical experience of the reflex cry that acquires the secondary function of a calling out. But the affirmation that the initial helplessness becomes the source of all moral motives is what supplies the key to understanding the unavoidable presence of the other in the mental life of the individual, which will be affirmed 26 years later, in *Group Psychology*. The moral naturalism that is deduced from these affirmations is more than evident. Because the very survival of the individual depends absolutely on the existence of another human being who is sufficiently interested in him to give him the assistance, the supreme good – conscious or unconscious – of the whole system of values by which he will guide his conduct, and his mental functioning will be that of being loved or making himself loved by the other. Not for nothing, in *Group Psychology*, does Freud discourse at length on love and passion in his attempts to establish the psychological foundation of the social bond.

Further on, in the *Project* text, Freud introduces a series of notions to describe how, starting from the primordial experience of satisfaction, a primary psychic functioning, geared to the immediate discharge of stimuli, is replaced, for adaptive reasons, by a secondary process, in which the discharge is postponed, in a way that enables the inspection and the exploration of reality, the recognition and the judgment of the objects perceived and rememoried which constitute thought processes. The formation of the Ego as an intrapsychic structure is presented as the result of the initial stages of this process and, consequently, as a condition for its subsequent development. Thought itself is going to be defined as a detour that interpolates itself between the perception of the need and the releasing of the action, although Freud tries to demonstrate how it gradually moves away from its initial practical aim, even though it still retains a genetic relationship with this aim. Here, the progressive construction of the Other, as external object, and of the Ego as psychic instance, mediated by the sensory representations of one's own body and of the other's body, well illustrates how the role of otherness is thought of by Freud, in this theoretical context in which neuronal dynamics and intersubjectivity seem to operate without conflicts in a conception concerning the genesis of the mind's structure and of the psychic subject. Let us quote at greater length this final passage, in order to leave this attitude well documented:

> Let us suppose that the object which furnishes the perception resembles the subject – a fellow human-being. If so, the theoretical interest [. . .] is also explained by the fact that an object like this was simultaneously the [. . .] first satisfying object and further his first hostile object, as well as his sole helping power. For this reason *it is in relation to a fellow human-being that a human being learns to cognize*. Then the perceptual complexes proceeding from this fellow human-being will in part be new and non-comparable – his features, for instance, in the visual sphere; but other visual perceptions – e.g. those of the movements of his hands – will coincide in the subject with memories of quite similar visual impressions of his own, of his own body [. . .], which are associated with memories of movements experienced by himself. [. . .] Thus the complex of the fellow-human being falls apart into two components, of which one makes an impression by its constant structure and stays together as a thing, while the other can be understood by the activity of memory – that is, can be traced back to information from [the subject's] own body (Freud, 1895/1950, p. 331, my italics).

The objective here was merely to illustrate how a typical theme of many humanistic interpretations of psychoanalysis – the role of otherness and of intersubjectivity in the constitution of the identity of the subject – is handled by Freud in the context of his most unequivocally naturalist works, such as the speculative neuropsychology presented in the *Project*. At the same time, these concepts appear to constitute the metapsychological foundation for subsequent developments in the field of social theory and the explanation of culture, precisely those that are the most valued by commentators who seek to bring Freud closer to the human sciences. We should ask what kind of naturalism is it that permits these developments. This question can only be answered in a preliminary manner here. Any more conclusive reply would require a more exhaustive exploration of the Freudian corpus, in addition to discussing systematically the more general epistemological questions formulated at the beginning. Even so, it is perhaps worthwhile advancing some considerations of a more suggestive nature by way of conclusion, as a kind of outline for a program of research with which it may be possible to proceed in the future.

15.4 Preliminary Guidelines for an Integral Naturalism

The unity of the Freudian project, which we sought to demonstrate above, allows for it to be characterized as an integral naturalism, in the sense that it seeks to include both individual as well as the social psychism, both the psychodynamic and the impulsive aspects of the mind, as well as the qualitative, experiential, and subjective dimension, both the emotional and the cognitive. But it is a project that distances itself from positivist naturalism, with which it was frequently identified, for better or for worse. It is another conception of the unity of science that may be perceived there: although it is clear that, to Freud, natural science was synonymous with science tout court, it is not the same model imported from the so-called sciences of matter that he seeks thus to generalize. On the contrary, we need to ask under what conditions Freud promotes the naturalization of meaning that characterizes his work, so that interpreting is no more distinct from explaining and that the meaning of a mental act may be assumed to be its cause. If Freud is intransigently naturalist, we should further ask: what is the concept of nature presupposed by this naturalism, which confers its specificity and which enables its accomplishments? Note that Freud attributes to nature characteristics usually attributed to history: conflict, finality, meaning, competition, temporality, and so on. Although he has inevitably been the inheritor of the philosophy of nature presupposed by the science of his time, with the physicalism and mechanicism which periodically crop up in his texts, it is possible to question whether he merely assumed it passively. The epistemological virtue of Freud, on the contrary, seems to have been his openness to permit his conception of science to be modified as his research advanced, without prejudice to his conviction that it remained within the frontiers of the natural sciences. In a word, it may perhaps be possible to support the need for a qualified naturalism – and for a qualified concept of nature – to do justice to the Freudian epistemological attitude and to fully appreciate his originality and to explore more efficiently the insights that it has to offer. In any event, this seems more productive than forcing psychoanalysis onto the bed of Procustes, whether of humanist anti-naturalism, or of positivist naturalism, which would be to insist on a categorization of the field of scientific activity that presents certain signs of exhaustion and the usefulness of which has become doubtful, since it no longer represents that which effectively is practiced in this field.

This reflection on nature and on the meaning of a renewed concept of scientific naturalism has already been essayed, although these efforts still have not been systematically developed, nor integrated into epistemology and philosophy of science. To give just a few examples, Merleau-Ponty (1995) was a philosopher who rediscovered the reflection on nature, while he was searching for a philosophy of history, a movement in which stumbled across the cosmology of Whitehead and its proposal of a conception of nature as process, and no longer as entity or mechanism. Collingwood (1960) also considered Whitehead one of the representatives of the evolutionary cosmologies that, in his view, from the end of the eighteenth century and throughout the nineteenth century, replaced the metaphor of the machine, organizer of the cosmology of modern science, with the metaphor of history. It is evident that the Darwinian theory of evolution played a key role in the consolidation of a vision of nature as history. Freud, for his part, was perhaps influenced by Darwin to a much greater degree than is, generally, recognized, so that there could be a path there to begin thinking about the peculiarities of the psychological naturalism that he advocated and practiced. Perhaps in the context of a concept of nature as history, the problem of how can a natural being be a subject – crucial for overcoming the duality between human and natural sciences – may be better resolved. More recently, a philosophy of the social sciences based on a realist vision of the sciences (Bhaskar, 1989; Keat, 1981) sought to reclaim a qualified naturalism capable of promoting the methodological integration of the human and natural sciences and of overcoming, ultimately, the ontological fracture which lies at the heart of this duality. The idea that we have endeavored to suggest here is that a global consideration of these developments may succeed in providing a more precise vision and a better understanding of Freudian epistemology. Once grasped, this in turn could provide a model or, at least, a concrete exemplary case from which the cogitation of certain questions of the philosophy of contemporary sciences could be pursued with greater clarity.

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Chapter 16
The Causal Strength of Scientific Advances

Osvaldo Pessoa Jr.

16.1 Units of Scientific Knowledge: Advances

The project of developing a science of science that takes as empirical data the vast work of historians of science, and that takes as theory (or "metatheory") the ingenious accounts of scientific development proposed by philosophers, stumbled on the difficulty of testing the different metatheories (the attempt that went the farthest in this direction was that of Donovan et al., 1988). One possible solution would be to use computers to store the historical information and run programs that could test different metatheoretical theses. But how should the historical information be represented in computer language?

A simple approach is to read the narrative of any historian of science and represent its salient aspects. As an example, consider an excerpt by Daniel Siegel referring to the nineteenth century field of spectroscopy, which is part of the general case study being used to develop our computer model (see footnote[1]). The author writes about certain *problems*, which stimulated the construction of an *instrument*, which was important for the confirmation of a *hypothesis* (that the bright spectroscopic D lines are due to sodium), which in turn was important for the *discoveries* of Robert Bunsen and Gustav Kirchhoff. The historian writes about problems, instruments, discoveries, ideas, theories, laws, etc., and each of these

O. Pessoa Jr. (✉)
Department of Philosophy, FFLCH, University of São Paulo, São Paulo, Brazil; Visiting the Department of History and Philosophy of Science, Indiana University, Bloomington, IN 47405, USA
e-mail: opessoa@usp.br

[1] "The resolution of these problems was greatly facilitated when Robert Bunsen, in the mid-1850s, introduced a lamp which provided a hot flame of low intrinsic luminosity; with the 'Bunsen burner' flame spectra could be observed against a minimum of disturbing background, and spectrum analysis was thereby facilitated in general. In particular, William Swan, using the Bunsen burner, was able to show convincingly in 1856 that the bright D lines could be attributed to sodium, the ubiquity of the D lines being due to general contamination with small amounts of that element. It was against this background that Bunsen and Kirchhoff undertook their collaborative researches of 1859–1860" (Siegel, 1976, pp. 568–9).

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have an influence, in differing degrees, on the appearance and confirmation of other scientific advances.

Let us then single out such "units of scientific knowledge" (ideas, instruments, etc.) and represent each of them in our information basis. Various names have been given to such units (contributions, achievements, manifestations, novelties, cognitive memes), but for brevity we shall call them "advances", even though they might not be a positive contribution to the progress of science. An advance is any scientific knowledge that is explicitly or tacitly passed among scientists. The prototype of an advance is an idea, but there are other types of theoretical advances, such as explanations, laws, problems, theory development, as well as experimental advances, such as data, experiments, and instruments. Other advances include the comparison between theory and experiment, methodological theses, metaphysical assertions, projects, tacit knowledge, etc.

Advances are part of what is usually called "internalist" history of science. The so-called "externalist" conditions (psychological, social, economic factors) are also important for explaining scientific development, but are not included in the definition of advance. The distinction between advances and cultural manifestations is however not always clear-cut, and it is sometimes useful to include the latter as a type of advance, especially when examining the origins of science (Pessoa, 2005).

Also excluded from the definition of advance are the facts in nature (described by the natural sciences). For example, in the context summarized in Siegel's quotation, there was a problem of contamination of all samples, notably by sodium, which made it difficult to identify the spectral lines characterizing each substance. Before there was a general recognition of this fact, around 1856, there was no corresponding advance (which may be called the "problem of spectral background"), even though the fact played a causal role in the development of spectroscopical science.

16.2 Probabilistic Causal Relations Between Advances

A second feature of the historian's discourse is that the advances are connected in certain ways, they influence the *appearance* of other advances, and they also affect the *degree of acceptance* of other advances. In the present approach, such a connection is taken to be a *causal* relation, not a logical one. For example, the construction of the Bunsen burner was essential for William Swan's discovery that the bright D lines are sodium: without the Bunsen burner, Swan would not have confirmed that debated hypothesis. The Bunsen burner may therefore be considered a "cause" of Swan's discovery, in the sense expressed by the so-called counterfactual definition of causality. This definition was given in an isolated passage by David Hume (1748, Section VII, § 29), for the case of a necessary condition: "Or in other words, *where, if the first object had not been, the second never had existed*".

When a scientist derives a new theoretical result, such a result is usually presented as a logical inference based on other advances. Although the connection between these advances is presented as a logical relation, a consideration of the actual circumstances of the derivation will point out which of the advances are the causes (being previously known), and which one is the effect (the new result). When a scientist justifies a result in deductive form, there are at least two possibilities for the causal history of the result: either the premisses are the actual causes of the conclusion (so the scientist actually discovered the conclusion by deductive inference from the premisses), or the conclusion was previously accepted by the scientist and led him to formulate a premiss as an explanatory hypothesis, in an abductive inference. The present approach sees a scientist as a very complex cognitive machine that receives a large number of advances (with changing degrees of acceptance) as causal inputs and generates new advances, which will causally affect himself and other scientists.

Causal relations in social systems are always complicated, and one can rarely single out a necessary and sufficient condition. A cause is better represented as an "INUS condition" (Mackie, 1965), which amounts to saying, in the example quoted from Siegel, that many other causes acted together with the Bunsen burner to lead Swan to his discovery, and that probably another sufficient set of conditions (not including the Bunsen burner) could have led to his discovery.

Another weakening of these causal relations is that a set of conditions can at best increase the *probability* that a scientist will arrive at a certain advance in a certain interval of time. The great number of causal influences that act haphazardly on a scientist, but cannot be accounted for by the model, are considered as "noise" or random fluctuations, the dispersion of which is encompassed by the probability functions.

16.3 The Representation of Causal Connections

How should causal connections and their strengths be encoded in computer language? We will consider another simple example and work with a visual representation of advances as blocks, and of causal connections as arrows.

In 1672, Isaac Newton announced the results of his experiments with sunlight and prisms, which would have a large influence in subsequent research. One of the discoveries that would be later made with his basic experimental setup was the identification of dark lines in the solar spectrum, by William Wollaston, in 1802. Wollaston was interested in the problem of how many colors there are in the solar spectrum, and so he passed sunlight through a long slit (Newton had used such slits, but preferred a round orifice) and through a flint glass prism, and with his unaided eye observed, to his surprise, the presence of seven dark lines, some of which seemed to separate what he took to be the sun's four basic colors.

This simplified causal relation is represented in Fig. 16.1, where other causal factors are ignored.

Assuming that the figure adequately represents the historical relations between the two advances, one question concerns the "strength" of the causal relation: how

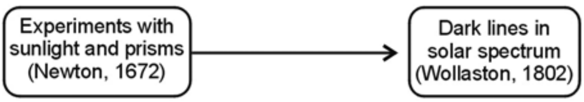

Fig. 16.1 Simple causal relation between two advances

should it be numerically represented in a computer program? An initial consideration is that the time interval between the appearance of the first advance and of the second is an indication of this strength: the shorter the time, the stronger the cause. This suggestion has been examined in more detail in Pessoa (2006), where an ensemble of possible histories of science is considered, and a probability distribution function is associated to each causal relation. Such a function expresses the distribution of times between the two advances, in the set of possible worlds, and the restriction is imposed that the time average is equal to the actual number of years between the appearances of the two advances (in the present example, 130 years).

One may evaluate the causal strength more precisely in the case of independent discoveries. In 1814, without being aware of Wollaston's observation, Joseph von Fraunhofer rediscovered the dark lines in the sun's spectrum, while investigating the problem of dispersion of light in different types of glasses. He also used a long slit but had a superior equipment, using an achromatic refracting telescope to view the spectrum and Pierre Guinand's high quality glass for the optical instruments.

With two independent discoveries, one may estimate not only the time average of the aforementioned probability distribution, but also its dispersion (standard deviation). Composition of causes (A causes B, and B causes C) may be readily represented by summing the time averages ($t_{AC} = t_{AB} + t_{BC}$) and by summing the squares of the dispersions ($\delta_{AC}{}^2 = \delta_{AB}{}^2 + \delta_{BC}{}^2$) (Pessoa, 2009b). The upshot of this discussion is that the actual time interval (called "empirical time") between two advances linked by a causal relation is a first measure of the strength of the causal connection, and should be included in the computational representation of advances. With the actual empirical time between two causally linked advances A and B, one can estimate the probability (for possible worlds), after the occurrence of A, that the effect B will appear in a certain time interval ΔT.

Figure 16.2 represents the two actual paths leading to the independent discoveries of dark lines in the solar spectrum. Both were influenced by Newton's experiments, and both employed a long slit and a flint glass prism. The discovery was unexpected,

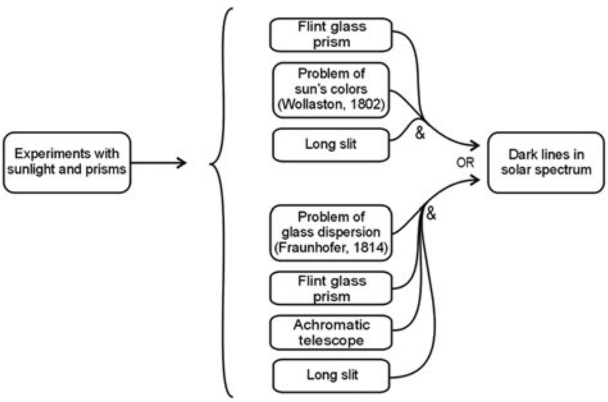

Fig. 16.2 Two actual paths leading to the same advance

and the problems which motivated the experiments were different in each case. In addition, Fraunhofer used a superior equipment, including the achromatic telescope. The causal diagram exemplifies the aforementioned "INUS condition" (weakened to probabilistic causal relations), where either of two sets of sufficient conditions (each of them constituted by a conjunction "&" of necessary conditions) may give rise to the effect.

Independent discoveries are especially interesting for building causal models in the history of science, since they correspond to two possible paths that are actual (not counterfactual). "Almost discoveries" are also of interest, such as the case of Thomas Melvill, who pioneered chemical analysis with flames in 1752, but died the following year at the age of 27. The historian Harry Woolf (1964, p. 628) remarked that Melvill "was clearly on the road to major discovery in science", which would include the discovery of the dark lines in the solar spectrum. Such an advance could therefore have appeared around 1760, in a counterfactual scenario.

When working with causal models, one may choose to include similar counterfactual information or not. It is highly probable that if Melvill hadn't died, he would have arrived at the advance, but one problem with including this "if hadn't died" information in our data base is that one could equally well include "if had died" information. In our example, it could also have happened that the young Fraunhofer died when the glass-making workshop where he worked collapsed in 1801. If one wants to maintain our actual history as the mean of the set of possible worlds being considered (which statistically should be our best guess), then counterfactual scenarios should be introduced in balancing pairs (such as the aforementioned "if had died" and "if hadn't died" pair) (this was *not* done in Pessoa, 2009b).

16.4 Causal Strength of an Advance

The time taken between the appearances of two advances that are causally linked is an indicator of the strength of the cause in producing the specific effect. But the more interesting aspect of such a concept of "causal strength" is that it is measure of the degree of acceptance of the advance, and it varies with time, as scientists discuss its merits. If the advance is an idea, this discussion might involve debating its degree of confirmation, which affects the degree of acceptance of the idea. If the advance is a new instrument, different scientists must investigate its performance, which then affects how trustworthy are its measurements. If the advance is a problem, then its strength reflects how many scientists are concerned with it.

The *causal strength*[2] of an advance may be defined as the potentiality that it may influence the appearance of other advances, or that it may affect the causal strength of other advances (mediated, of course, by the brains and hands of scientists, and by their social and institutional interactions).

[2]The term "causal power" could be used, but it seems to be committed to a realist conception of causes, which I would like to avoid in the present exploratory stage of the project.

A theoretical advance may start out as a simple consideration of an idea, then develop into the proposal of a hypothesis, then be explicitly defended, then it may be considered plausible, and then acquire good evidence, then strong support, and finally wide acceptance. These may be called "degrees of acceptance" of a hypothesis, and the causal strength of an idea grows as its acceptance grows. A hypothesis may also receive negative support, in varying degrees, and this has an effect on its causal strength (which may be nullified, or may cause the downfall of other advances).

Similar considerations may be applied to an experimental advance, such as an instrument. An instrument might be built based on a new principle, but at first its performance might be bad, then its resolution (or other figure of merit) might improve, leading to increasing use of the instrument. The notion of causal strength (the capacity of an advance to give rise to new advances) still applies here. But for instruments, the causal strength is not only dependent on the degree with which it is used or sold (analogous to an idea's degree of acceptance), but also on its figures of merit: a higher resolution allows more precise data, which increase the possibility of discovering new advances (such as new phenomena or laws).

Let us consider the historical example of an explanation (a theoretical advance), the thesis that the dark lines in the solar spectrum originate in the sun's atmosphere. It was first suggested around 1832 by John Herschel and David Brewster, and we may attribute to it a causal strength of 0.3 (out of a maximum value of 1.0). It stimulated further research, and 2 years later Brewster obtained data from the sun that seemed to confirm the hypothesis, so its strength rose to around 0.6. But then, during the eclipse of 1836, James Forbes observed no differences while looking at the spectrum of the sun's corona, and concluded that the dark Fraunhofer lines do *not* arise in the sun's atmosphere. We may thus lower the causal strength of the hypothesis to 0.1, since it was rejected by most spectroscopists, but still attracted attention. Brewster himself, as late as 1859, with J.H. Gladstone, reconfirmed Forbes' negative conclusion. But in that same year, Kirchhoff showed convincingly that the dark lines of the solar spectrum are not caused by the earth's atmosphere, but originate from the presence of chemical elements in the glowing solar atmosphere (McGucken, 1969, pp. 15–33). So now the causal strength rose to around 0.9 (later, it was found that some lines are in fact generated in the earth's atmosphere).

Although the numerical measure for the causal strength is only a rough estimate, it is useful as an input for computations. One should also consider that different scientists or research programs might have different degrees of acceptance for an idea. In the example just given, coming from another field in 1854, William Thomson considered quite plausible the hypothesis that the dark lines originate in the solar atmosphere.

16.5 The Representation of Causal Strengths

We have argued above, when working with causal models in the history of science, that an advance should always be considered together with an estimate of its causal

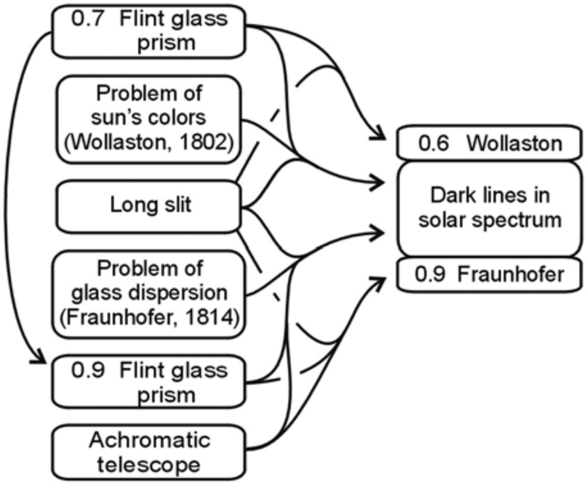

Fig. 16.3 Two paths leading to different causal strengths of the same advance

strength, which usually varies with time. Figure 16.3 is a version of the example given in Fig. 16.2, in which measures of the causal strength are tagged on to two different advances, "flint glass prism" and "dark lines in solar spectrum". All of the causal connections for the *appearance* of the effect in Fig. 16.2 are reproduced in Fig. 16.3; but, in addition, new arrows are drawn pointing to the different *causal strengths* of the effect "dark lines in solar spectrum".

We have seen that Fraunhofer worked with a higher quality prism, so we might represent this higher quality by stipulating that its causal strength is 0.9, instead of the lower quality of Wollaston's prism, which we might fix at 0.7 (One could consider that the two different prisms correspond to two different advances, but for our purposes it is simpler to consider them as the same advance, with different causal strengths).

Consider now the resulting advance discovered by the two scientists, the dark lines in the solar spectrum. Wollaston's discovery did not attract the attention of other scientists, in part because at that time it was still a subtle effect, not so easily reproducible, so we might attribute to his finding a degree of acceptance of 0.6, as represented in Fig. 16.3. Fraunhofer's data, on the other hand, had much higher accuracy, and he was able to map hundreds of lines. His result was unquestionable, so we attribute to his proposal of the advance a degree of acceptance of 0.9.

Our ground rule, before the explicit consideration of causal strengths, has been that "the *appearance* of an advance is causally influenced (in a probabilistic way) by the *presence* of other advances" (rule 1). With causal strengths, one notices that "the *appearance* of an advance is also causally influenced *by the causal strengths* of other advances" (rule 2). Furthermore, "the *causal strength* of an advance is causally influenced by the *presence* of other advances" (rule 3), which may lend support to it.

Let us now return to the causal strengths of the previous advance "flint glass prism". One could argue that it is the lower causal strength of this advance that led to a lower degree of acceptance of the effect "dark lines in solar spectrum". Identifying the latter's degree of acceptance with its causal strength, one may take this to be an example of a general rule (with possible exceptions) for causal models in the history of science: the causal strengths of the effects vary monotonically with the causal strengths of the causes. In other words, "the *causal strength* of an advance is also causally influenced by the *causal strengths* of other advances" (rule 4). Included in these rules is the obvious statement, indicated in Fig. 16.3 by the vertical arrow between the two versions of "flint glass prism", that a new degree of causal strength is causally influenced by the previous degree of the *same* advance.

We have focused on the prisms in order to explore the notion of causal strength, but the greater resolution and accuracy that Fraunhofer had over Wollaston was due to other instruments, especially the achromatic telescope for looking at the spectrum. The Bavarian scientist also used a theodolite for making precise angular measurements. So all of these advances contributed causally for the appearance of the effect, and most of them contributed to its degree of acceptance (and causal strength).

On the other hand, we notice in the figure that Wollaston's advance "problem of sun's colors" and Fraunhofer's "problem of glass dispersion" do *not* contribute to the degree of acceptance of the effect. These two advances were important for making the scientists explore the field (in the context of discovery), but once the discovery was made, these advances became irrelevant for the context of justification, which is involved in the degree of acceptance.

All of these considerations are represented in the diagram of Fig. 16.3, with is a rather complicated network for the simple appearance of an advance by two independent paths. Causal models become quite complicated once causal strengths (degrees of acceptance, qualities of instrument, etc.) are represented, but this complication may be stored in the computer, out of our sights.

16.6 Outlook

The present paper is part of an ongoing project of representing the beginnings of quantum physics by means of causal models in the history of science, with the aid of computers. In a preliminary study of the possible paths leading to the birth of the old quantum theory (Pessoa, 2001), it was suggested that there would be four main paths, the most probable not being the actual one (in the field of thermal radiation), but in the field of optical effects. A simple causal model helped to organize the study, but the conclusion was reached "intuitively", and should be qualified and refined with a more detailed causal model.

Computer programs don't provide actual thinking and intuition, but they allow the storage of detailed information concerning the relations between advances and their causal strengths, and allow simulations to be run, which we hope might help to test metatheoretical theses about the development of science. There are many different types of advances, and the general relations between these types may be investigated with the aid of the computer. One may also imagine attempts to represent (Pessoa, 2009a) and generate counterfactual histories of science (which should however be very "close" to actual history, so that most advances can maintain their identity across possible histories, and basically the order of their appearances is changed), in spite of the controversy surrounding the subject of counterfactuals (see Radick et al., 2008).

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Chapter 17
Contextualizing the Contexts of Discovery and Justification: How to do Science Studies in Brazil

Antonio Videira and André L. de O. Mendonça

17.1 Introduction

In spite of being *démodé* in our times – a situation that is produced by the erroneous and unwise judgement that supposes it is unfashionable – the debate gravitating towards the theme of the context of discovery and the context of justification is crucial in epistemological terms. Besides that, it maintains a very important relevance and actuality about its political and social consequences. Drawing a clear distinction between external factors and internal reasons, or bloting the line which splits up both kinds of factors, the point here is the way how the relation between the two contexts is conceived; an understanding that in its own interior has a conception of science, even the latter is not always explicit about the place should be occupied by the social sphere. Summarizing drastically a process of – more or less – one century of history, it is possible to say that, in the context of British and North-American philosophy of science, there was – in a first moment represented by the logical positivists and Popper – a heterogeneous defense of separation about its objectives and the reasons developed by logic of discovery and psychology and sociology of scientific research. There was also a second moment, in which the historical philosophy of science, or the post-positivist philosophy, partially strengthened by internal disputes among its most bright representatives (Kuhn, Feyerabend, Lakatos and Toulmin), had a very special role mixing the two contexts. There was also a third phase, in which the new sociology of science and the new history of science, precisely the Strong Programme and the Science Studies, finished to surpass the division between rational and social – at least this is the way their supporters evaluate their own work. It is worth to remember that each of those moments presents internal divisions, besides the fact that they were not developed successively as it is implicity suggested: Laudan and Hacking, for example, could be perfectly inserted into the second phase, even if they are nowadays producing thoughts and ideas in the domains of history and philosophy of science.

A. Videira (✉)
Institute of Philosophy and Human Sciences, State University of Rio de Janeiro, Rio de Janeiro, Rua Machado de Assis, 17/101, CEP: 2220-060 Rio de Janeiro (RJ), Brazil
e-mail: guto@cbpf.br

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The question that very often remains out of the present debate, when it happens, concerns the extension of the contexts. Obviously, it is not important to measure where one begins and finishs the other, but the point here is to define, clear as possible, what one understands by “external social factors” or by “social construction of science”, among many different similar expressions frequently employed in our days. Affirming directly this last point, the theme, which this article discusses, is the relation between science and society; our discussion is done from the point of view of a brief (due to the lack of space) historical reconstruction about the context of discovery and the context of justification. We develop the argument that Kuhn and his heirs of the Strong Programme and Science Studies mix the two contexts. They do this, restraining them inside science. This implicates that science is separated from society or that society is colonized by science. Contrary to this position, logical positivists and Popper draw a clear distinction between logic of scientific discovery and their social and political factors, but immediately after this, they argue in favour of the intersection between science and society, even when they think that the former is to be understood as the model for the latter. Playing with words, our point can be stated as if Kuhn and his heirs divide when they mix and the logical positivists and popperians mix when they separate. Our goal here is not to join one of those sides, even because both sides have, simultaneously, strong and weak points. Instead we defend the thesis that salutation, or disapproval, done by some sectors of the academic world and society that have understood Science Studies as if they would be “libertarians” or even “revolutionaries”, is not intrinsically correct. This understanding, even logically possible, does not correspond to the facts, specially when their ideas are jumped over for the contexts of countries like Brazil. In our work, we first go throught very quickly the path opened by logical positivists and Popper and his fellows. After this simultaneously historical and argumentative reconstruction, we elaborate some reflections about the Kuhn’s historical philosophy and the Strong Programme of Bloor. In the sequence, taking Galison and Latour as examples, we sustain the thesis that Science Studies actually are supporters of the *status quo*. At the end of our article, we discuss briefly the possibility to apply the theoretical tools to peripherical countries like Brazil. We do this with and against the Science Studies, looking for less conservative goals on the evaluation of scientific levels accomplished by countries considered as being underdeveloped.

17.2 Science as the Light for the World

The view that defends that the correct, true and acceptable knowledge is this one that separate us from the domain of opinions, supertitions and myths has its origins, in a certain sense, in Plato’s philosophy. In the contemporary philosophical vocabulary, specifically in the context of the philosophy of science of positivist inspiration of the first half of the twentieth century, the expressions created to limit the frontier between the “genuine rational aspects” (or logical) and the ideological factors (or political) and sociological (or psychological), which are inside the scientific knowledge, were "context of discovery" and "context of justification".[1] In very general words, the context of justification is restricted to the space of validation and legitimation of propositions and theories, which aims the truth; the context of discovery is inserted, basically, in the geographical and historical landscapes, which are useful to the elaboration of the final product (results) of the scientific knowledge. The context of discovery concerns the process (means), which is not always explicited, but that conducts to hankered objectives.

It must be remebered that, even the most radical logical positivits and Popper, did not deny the presence of the so-called external factors (or social) in the development of scientific knowledge. With the advantage of extended and retropective sight, it can be said that, contrary to this common view, the logical positivists were not supporters of a sort of rationality, which was strictly defined by logical-empiricist criteria (or by testability) as some of the heirs (or critics) used to claim (Friedman, 1999). By whatever means, even they did not disclaim the role played by the context of discovery, the so-called verificationists believed that philosophy, as a logical reconstruction of the scientific discovery, ought not be occupied with the process of production of the knowledge reportedly true or apparently true, task that has to be done by history, sociology, psychology, among other empirical disciplines. Reacting against the so-called new philosophy of science, specially against the incomensurabilty defended by Kuhn, Popper (1994) denounced what he himself called "the myth of framework".

Taking very seriously the methodological recommendations – one must remember the normativism of the Popperian epistemology – a specific scientist, independently of his/her origin, sex, religion or nationality, would achieve the results, which were desired, or he/she should try to deny them using empirical tests and, after that, should propose hypothesis even more general to occupy its place. This is to be done in order to contribute to the advancement of knowledge by the universal method of conjectures and refutations. The object of the analysis of the philosophical reflection would be precisely placed in the always partial scientific results. Therefore, either in the case of the logical positivists or in the case of Popper, with some reservations, there would be a clear and well established division of work, which could be taken as a point of departure for the philosophical analysis of the well finished scientific results – true theories (verificacionism) or probable hypothesis (falsiabillity). Thus, the rest of researchers (historians, sociologists and psychologists) would be limited to discuss, even as a curiosity, about the "external factors", which play some role during the process of production that outcome results. This is the reason why it is used to be reasonably claimed, that the thoughts of those thinkers were important to consolidate the so-called project of legitimation of science against society: they aimed to reclaim, for the most wide *publicum*, the epistemical superiority

[1] As far as is known, those expressions were used, for the first time, by Reichenbach in his book Experience and Prediction in 1938. In this work, the contex of discovery and the context of jutification signify the process of the origin of knowledge and the public presentation of acquired results, respectively (Reichenbach, 1970).

of science, comparatively to other forms of knowledge, like metaphysics,[2] since scientific theories could not be contaminated by "subjective and irrational factors".

As long as they (Logical Positivism and Popper) have guaranteed, having as their target the rational reconstruction of scientific development, the separation between the context of discovery and the context of justification, it must be emphasized the fact that the logical positivists and the critical rationalists did not conceive science and society as two radically and completely separated spheres. Both philosophical schools vehemently defended the autonomy as a *conditio sine qua non* to scientific progress. They believed that science, by the virtue of its strong democratic qualities (in the sense that, hypothetically, every one can learn to be a scientist and that its products – theoretical and practical – can be, at least in principle, acquired by every one) and by the virtue of its permanent state of criticism should function as a model for society at large, including the necessity to avoid political authoritarism and totalitarism. On the one hand it can be noticed by the Popper's statement (1974: p. 91) that the thesis in his works *The Open Society and its Enemies* and *The Poverty of Historicism* are corolloraries of his *The Logic of Scientific Discovery*, that is, his political theory would be a consequence of his epistemological conception about the nature of science. On the other hand the logical positivits in their famous Manifest of 1929 let us see that their most important project – a confirmation of the original Modern ideal, conceived by the French *philosophes* of the eighteenth century and Kant – consists in shaping all the sectors of the social sphere according the patterns of the scientific conception of the world. It could be justly the question if they did not weave an inversion in the real state of things. Would it be the case that we should have a more general social project, which would allow us to generate a scientific conception that could be successful? Even if the answer is positive, emphasis must be put on the fact that authors like Carnap, Schlick, Neurath and Popper (Popper, 1945, 1957 and 1959), considering their differences, asserted that they were favourable to the approximation between the world of science and the life world, even when they had drawn a border line between the context of discovery and the context of justification. This situation is similar as the following one: if one wants to practice science, it must behave like a super man without any trace of humanity, aside world views, passions, interests, etc. The persistent question is: would it be possible to rationalize at a very high level the society, if not all of us are scientists?

17.3 "Humanizing" Science to Strength It

The revisionism about the legacy of Logical Positivism undertook by Friedman and his colleagues has tried to mitigate, since the 1980 decade,[3] the so-called rupture

[2]As it is well known, Popper did not consider metaphysics as an enemy to be eradicated. Curiouly, two theories, pretentiously presented as scientific at his times, Freud's psychoanalysis and Marxism, became the targets of his criticisms, because they were seen as irrefutable.

[3]Some philosophers of science, as Goerge Reisch (1991), since the beginning of the nineties, has tried to show that Kuhn was much more closer to Carnap than it is normally accepted.

promoted by the historical turning, from the beginnig of the 1960s, which would be implicated in relation to the well known "received view". Although such reconsiderations are, sometimes, able to win more adepts for its own view, they can not reverse the fact that the mentors of the historical philosophy of science stated their difference, during the uncoil of the history of philosophy of science, since they had confirmed the historical and social character of scientific knowledge, in detriment of barely logical components. Among the authors of the emergent philosophy of the sixties, it is not possible to deny the strong influence of Kuhn, for whom the history of science should acquire epistemological status. In his alleged turning back to the history of science,[4] Kuhn identifies that one of the most distinguished characteristic of the discipline is its constitution as a community of researches, which is normaly valid under paradigms that are auspicious.

Leaving apart other polemical points of his work (confusion between the descriptive and the normative levels, assimetrical relation between history and philosophy of science, equivocity of the concept of paradigm and inconsistency of the substitute expressions, conservadorism of normal science, incongruences on the incomensurabilty thesis, etc), Kuhn normally is accused of initiating a movement towards the sociology of studies about science, since he breached the distinction between context of discovery and context of justification. We are not trying here to defend the author of *The Structure of Scientific Revolutions*. Nevertheless, we accept the interpretation that Kuhn should not be taken as if he were suggesting something like a "psychology of mob", to use the Lakatosian defamatory expression, in order to make the latter the supreme queen inside science.[5] It is doubtless, as it was stated by Rorty (1979, 1991), that Kuhn contributed very much for the weakening of the border line between science and other domains of culture, since he took seriously, in his supposed historical-philosophical reconstruction of the scientific knowledge, aspects which are frequently seen as external. It must also be stressed that Kuhn formulated the thesis that, in the dispute among paradigms, it is not possible to employ strictly logical and empirical criteria for their evaluation. Notwithstanding those provocations, which had a huge impact on his own thought and on the later development of philosophy of science, Kuhn tried to reiterate, confronting his critics, that he did not aim to deny either rationality or objectivity. Neither he wanted to refuse the authority of science.[6] He never had the intention to evoke suspicions about science. On the contrary, all that mess was caused by misunderstandings. For those and other reasons, Fuller (2000) begins his controversial book on Kuhn, comparing him to Chance, a personage of *Being There* of Jerzy Kosinski, a simple and modest

[4]In Larvor's judgment (2003), Kuhn's history of science is only a bad use of a historicist philosophical background, since he uses uncounscious the same general methodological principles to observe the historical development as a whole. That is: Kuhn, as positivits, continuously elaborate idealizations of present scientific practice.

[5]This is, for example, the evaluation of Nola (2000), for whom Kuhn should not be included in the group of the Strong Program. Nevertheless, our criticism to Nola is that the latter movement is not anti-science as he supposses.

[6]After *The Struture of Scientific Revolutions*, Kuhn published some philosophical articles, in which he had tried to answer his critics.

ward, who became a candidate to the US presidency, yet he did not want it. But, in those times of Cold War it seems that it was necessary to create the "revolutionary". Whatever the truth is, it seems to be correct that Kuhn, accepting the ideas of some people like Polanyi,[7] was worried, in his innermost conviction, with the autonomous character of science foremost society.[8]

In the beginning of the seventies, it appeared in the British and North-American philosophical world, a new proposal, explicitly influenced by Kuhn's works, but with objectives to give more coherence to his anti-whig historiography. We refer to the Strong Programme. Surely, the most important part of this programme is located in the formulation of four fundamental principles: causality, impartiality, symmetry and reflexivity. Those principles were presented originally in the first edition of *Knowledge and social imagery* of David Bloor (1976).[9] This book is, still today, one of the most important sources of inspiration for the Sociology of Scientific Knowledge (SSK). Set against the so-called sociology of mistake, which is tunned to the persecution of *external factors* that could explain the causes of diversions of *reason*, Bloor and his colleagues proposed a sociological project much more ambitious, whose task consists in explaining *all* scientific theories, *true or false*. Strictly, the strong sociology should serve as a substitute for the tradional philosophy, since it would be more apt to decribe the nature of scientific knowledge. Bloor calls to action the sociologists in order to force them to abandon their lesser position and, consequently, to strengthen their philosophical thoughts in the discussions about the nature of science – or would be all the place? (Bloor, 1991: p. 3).

The principle of symmetry,[10] in spite of very subtle reformulations that it acquired in the last 30 years, postulates that sociological explanation must be equivalent, that is, it must have the same reasons, or causes, to explain the truly and rational scientific theories, as well as the false and irrational ones. This is the fundamental tenet, whereby Bloor and his admirers leave off: if rival theories explain differently the "same set of facts", there is no reason to believe that there is a privileged access to these facts. To the looser side it must be not attributed a lack of correspondance with facts, as well as the winners should not be seen as immune to influences of social order (Barnes and Bloor, 1982: p. 34). The real play here is the statement of a total and complete overcome of dichotomy between the context of discovery and the context of justification. In order to counteract the normal state of affairs, it is fair to emphasize that the most important concern of Bloor is to be able to analyse the nature of scientific knowledge, i.e., sociology must be seen as being able to analyse the *specific cognitive content* of science, and not only its external causes. We must do justice to Bloor, recognizing that he does not deny to science

[7] Polanyi: « I appreciate the generous sentiments which actuate the aspiration of guiding the progress of science into socially beneficent channels, but I hold its aim to be impossible and nonsensical » (2000 [1962]: p. 9). See Kuhn (1970).

[8] For a very interesting discussion about misunderstandings of this expression, see Hacking (1999).

[9] A critical analysis of Galison's ideas is done in Mendonça and Videira (2009).

[10] Latour's ideas are discussed in Mendonça (2008).

its status of rational and true knowledge, he even understands those adjectives in a different manner of traditional epistemology. What he looks for is explicitly to move traditional philosophy of its pretension of describing scientific knowledge, because all it can do would lead to ideologies or social metaphors. It is necessary, in spite of this, to treat the cognitive content of science scientifically (*naturalizing* it). This task could only be done by sociology due to its empirical methods (Bloor, 1991: p. 80). He has never wanted to be a relativist, e.g., one that aimed to disown the cultural authority of science inside society.

The recent opinion of Bloor states that the most direct adversary of relativism is not universalism, but absolutism. For Bloor and his co-workers of Edimburgh, relativism is not a sign of scientific weakness, but only a form of knowledge that is seriously taken by human beings, which are finite and fallible. This form of knowledge is similar to other cultural products. In spite of this similitude, science has a specific characteristic: its objectivity is grounded on consensus. Like Kuhn, for Bloor, the force of science has its origin in his "humanity", which warrants the legitimacy of its autonomy against society.

17.4 Peaceful Times?

Even acknowledging that the ascendancy of the Strong Programme over the constitution of Science and Technology Studies (STS) or, simply, Science Studies, recently sustain the goal that it is important to shake off one very impressive obstacle to overcome definitely the so-called *war of sciences*, which is due to bad use of expressions like "social construction". Its onset at the final years of the seventies and at the beginnings of the eigthies, when authors like Collins, Knorr-Cetina, Shapin, Pickering, among many others scholars, published a lot of new results, made them responsible for a truly renovation in the analysis about science. This renovation could happened because they produced a lot of empirical studies with historical, sociological and ethnographic characteristic, in which science appears as a material and cultural practice. Although they were called accused of disrespect the cognitive status of science, by the "scientific warriors" like the Nobel prize Steven Weinberg, those STS scholars do not like to be seen as "science ennemies". Insomuch that they make much effort to divest what would be misunderstandings. This attitude is clear in two of the most influential representatives of this domain: Galison and Latour. Each one has his own style. Nevertheless, they aim to sustain that scientific facts are *real*, even if they are *socially constructed* (Latour and Woolgar, 1979). Both of them want to overcome the old debate between realism and relativism. Galison does this sustaining that scientific practice is different from the more wide social sphere; Latour proposes a new modernism, which in spirit, but not in words, points towards the direction of the old project of molding society by scientific practice.

Galison deserves his fame, specially because he published the two most relevant books about the particle physics of the twentieth century: *How experiments end* (Galison, 1987) and *Image and Logic* (Galison, 1997). Both books transmit an unusual image of science. Instead of being legitimated only as a domain of testing of theories, experimental science has a certain autonomy. Experiments demand much time, since they need to suffer constant slight modifications in order to give good results; they are not "instantaneous". Experiments demand also powerful instruments and a huge quantity of researchers, that is, they are no more performed as in old days, when scientists worked alone on their workbenchs. The instrumental science, as the theoretical one, is not homogeneous, but it colaborates with one another, by means of a relation which is not due to epistemic hierarchy; it happens in trading zones. It is well known that Galison's model advanced our comprehension about the pratice of science. In spite of these advances, he remains trap to tradition when he is compared to logical positivists or to Popper or even to the post-positivists: his analysis are enveloped by his own principle of always respecting what is established by scientific practice. Affirming that science is a social construction, Galison only wants to say that the process of production of facts, as well its legitimation, is simply due to the collective work of a certain scientific community. In other terms, the expression "social construction" does not keep any kind of relation with the idea that science would be determined by, for example, economical or political interests, which are external, even less that "facts" are fictious because they are simply constructed (fabricated) in society, instead of being *discovered*.

It is certain that Latour does not separate distinctly science from society, at least in a first moment (Latour, 2000b). Actually, trying to keep himself faithful to the slogan "science as it is really done or science in action", he asserts that it is senseless in using the modern and backward vocabulary, because what exists is the collective of humans and not humans (1994 and 2001). After taken distance progressively from his original and more aggressive positions, he has recently, in his book *Politics of Nature*, intended to solve what would be the three challenges for the Western democratic societies: to have a new image of science, to abandon the traditional notion of nature; to redefine politics. In this manner, taking as his ground the discussions of ecological movements, Latour maintains the thesis, sustained in the conception of science supplied by many accounts produced by Science Studies, that the old conceptions of nature and politics are the most important obstacle to democracy (Latour, 2004: p. 59).

Latour suggests a new specimen of modernity, claiming for a relation more equitative among the spheres, by means of an overcome between the so-called powers of consideration and of ordering, which should be considered as substitutes for the classical notions of value and fact. He claims that, since the West always tried, under the cloak of nature, to invent a collective in two chambers, it must do that right now and well. The power of consideration must deal with the question: "how many new propositions should we accept in order to articulate coherently the same and common world? The power of ordering must answer the question: "which order has to be found for this common world, formed by the set of new and old propositions?" In order to do both tasks, which originates four demands (perplexity, consult, hierarchy and institution), Latour supports the works done by scientists, politicians, economists, burocrats, and moralists. In order to ensure the success that enterprise, Latour proposes the use of a third power, which would work as a monitor, a sort of “temporary absolute” that would ensure the ability to govern or to explore new common worlds, since the collective is fed with what remains outside, that has not yet been collected. Only after this long and painful process of assemblage and exploration of collective, in which many battles are waged, it is possible to “speak” of representation of reality that it would socially and collectively constructed (Latour, 2004: p. 294). It is now almost clear that Latour makes a diference in what is the sense fo representation when he is compared to the philosophical tradition about science. For him, differently of representationism, which defends that everything has been already decided at the starting point, the right to talk in the place of something or someone can only be fixed at the destination. In a word, for Latour, representation is always *a posteriori*, never a priori.

Regardless his innovations, Latour remains captive of tradition, in the sense that he sustains the thesis that science represents, with the aid of other domains, facts/artifacts (the non humans), although he explains differently the foundation of representativity. In replacing the binary system science/society for the term “collective”, it seems that to the non-specialists there is no possibility to take part of the construction of the “good common world”, since Latour only describes the work done by scientists, politicians, moralists and administrators. He gives more relevance to the first two, since we, inhabitants of Western and democratic socities, proudly admire the contributions of scientists and politicians. We are conscious that our interpreation of the author of *Laboratory Life*, among other important titles, is very different from the usual one, which considers him to be an iconoclast and critic of science. Suuming up: although trying to show how it would be possible to make science in a democracy, Latour, at the end, emphasizes more science than society, remaining therefore, even against his own desire, assymetrical.

17.5 Conclusion

In the conclusion of our paper, we aim to discuss briefly the impact upon the under development countries, like Brazil, of the methodological, epistemological and historiographical methods adopted by Science Studies. Due to the lack of space, we can only show the structure of our argument. Nevertheless, our main conclusion, which is negative, states that (1) Science Studies has no intrinsic reason to be understood as more libertarian than the epistemological thoughts of Logical Positivism and of Popper and his fellows; and (2) as a consequence of (1), neither philosophy of science, nor history of science plus sociology of science, *that we have at the present moment*, serve as a basis for the development of science in less developed countries like our own.

The structure of the argument runs as follow. Above we discussed that one the weakest points of the philosophy of science of the twentieth century (Science Studies included) is its resistance, or impossibility, to define, clearer as possible, what it considers to be the social and political elements, which can be taken as reasons, or obstacles, to the growth of science. Even if this is true, the post-positivists philosophers and historians of science agree that science can only be correctly described if context of discovery is included in the analysis of science. In other words, if we accept that is useless and has no sense to maintain the distinction between context of justification and context of discovery, how could the so-called less scientifically developed countries improve their conditions?

The relativism, or if one uses a more neutral expression, the theoretical and methodological pluralism of Science Studies, denies the possibility to fix criteria for scientific development, which could be taken as guides showing how science should be done. History of science describes how science is really done, but explicitly avoids to determine why science was done that way and not another, i.e., history of science does not have, and must not have, explanatory goals. We claim that, upon Science Studies' methodology, it seems to be no way out of this situation, since there is not a fixed and well known set of criteria, which determine how to do science and, *at the sime time*, history can not be used as model, because it is contingent, e.g., it can not be repeated or, with other terms, it can not be seen as giving lessons. If we consider to be science what it was obtained through the specific and particular historical process, which are not and can not be repeated, but, in spite of that, actually happened, how could one not share the conclusion that science really results from contigent factors?

Unless we elucidate which are the external factors that actually influence science, it seems that there is no strong reason to believe in the main conclusions of the historical descriptions offered by the post-positivist philosophical thougth. Seen by those epistemological and historiographical lenses, science should be understood as a lucky miracle.

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Chapter 18
Echoes from the Past: The Persisting Shadow of Classical Determinism in Contemporary Health Sciences

Kenneth Rochel de Camargo Jr.

This text deals with the concrete implications of a set of theoretical – more specifically, epistemological – questions stemming from a fact of life in contemporary society: there is a set of interventions, whether in the lives of individuals or in collectivities, operated by professional agents socially perceived as legitimate operators of these interventions, which are presented as the application of reliable knowledge. Scientific, exact, objective, true: multiple adjectives that reinforce the idea of reliability.

Let us think, for instance, of Public Health. As a minimalist definition, one could say this label encompasses a set of bodies of knowledge and practices regarding the health of populations. This means that an important part of the knowledge produced or used in this complex field is at the service of normative practices, which invariably leads to the need of an ethical purpose as a regulating ideal. The acritical use of technical knowledge in interventions in human collectives is a theme already widely approached, both in discussions regarding the technocratic character of any given governmental policy and, in a more micro level, in the exam of power relations between experts and the population, such as in the extensive literature concerning the processes of social medicalization (Conrad, 2007). The fact that the production regarding this theme is extensive, however, does not mean that proper attention has been given to its implications.

Approaches regarding the process of production and validation of scientific knowledge constitute a strategic line of investigation in this sense. These approaches are strategic precisely because that kind of knowledge is what determines the direction and logic of the criticized type of interventions on the *socius*. "It is past the time to recover a central characteristic of the Enlightenment: criticism, even if, as shall be seen, its exercise should now denounce the one-sidedness of the Enlightenment and of the civilization wrought by modernity" (Plastino, 1996: p. 197). This one-sidedness is expressed above all in a Reason that exempts itself form the reflexive exercise of criticism in the Kantian sense of a free and public exam. Asymmetric, as

K.R. de Camargo Jr. (✉)
Instituto de Medicina Social, Universidade do Estado do Rio de Janeiro, Rio de Janeiro, RJ, Brazil and CNPq
e-mail: kenneth@uerj.br

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Latour (1987) might say; inconstant in its partial, scotomized exercise; in placing its own foundations safe from any threat, it becomes an absolute, since unquestionable, Power.

Take, for example, the first assertions made regarding AIDS, in the beginning of the 1980s. The dissemination of the expression "risk group" and concepts associated with it reinforced old prejudices and a false sense of security among those who did not identify with the taxonomic categories of the time, a fact that had negative impacts that are still felt today, more than 20 years later (Camargo, 1994).

These observations do not call into question the usefulness of scientific knowledge in confronting (among other aspects) public health challenges, but seek to demonstrate that, as Boaventura de Souza Santos suggested, "(. . .) only by applying science against science is it possible to get it [science] to say not only what it knows of itself, but all that it has to ignore about itself in order to *know* about society what we expect it to *know*." (Santos, 1988: p. 13) Still according to this author, "The struggle for post-modern science and for the edifying application of scientific knowledge is, simultaneously, the struggle for a society that makes them possible and maximizes their rule" (Santos, 1988: p. 161). In recent years, an almost canonical form of "applying science against science" is represented by the field of science studies, as described in the next section.

There are several available narratives regarding the history of sciences. They range from the presentation of a great revolution which introduces modern science, such as advanced by Hall (1988), to the version that questions the very idea of a scientific revolution (Shapin, 1996). Whether describing multiple starting points or a single origin, it still seems possible to point to a consensus concerning the idea that in the long period that spans from the end of the Middle Ages until the beginning of Modernity a new form of producing knowledge, Modern Science, was developed in Europe. Modern science defined not only a set of techniques and methods, but also a new world view. This view progressively "colonizes" the general culture, becoming hegemonic in western societies. Evidences of this colonization process are found everywhere, including in the current uses of certain words and expressions. As mentioned before, "scientific", "true", "real", "objective" and their cognates are considered in everyday language as part of the same semantic family, used interchangeably, if not as synonyms. It is not difficult to understand the reason behind this: the conception of science which I will temporarily call "popular" (which is encouraged by scientists themselves, it is worth to note) sees science as the activity of faithfully portraying a reality that is preexisting and external, in a simplistic form of realism. This way, the forms of validation of knowledge operated by the scientific production would define the excellency standard for such validation processes.

18.1 Science and Determinism

This conception can be described, in short, as generalistic (only dealing with universal descriptions), mechanicist (the universe can be described, understood – and eventually assimilated – to a gigantic mechanism) and analytical (the whole is expressed by the sum of the parts and, therefore, in order to study it, one must isolate progressively smaller parts for investigation) (Camargo, 2003: p. 107). As a consequence, the process of knowing, the conduct of inquiry, results necessarily in a reduction operation – the creation of a schematic model of the aspects one wishes to study, leaving out details and relations that, allegedly, are not directly related to the studied mechanism (Harré, 1988; Santos, 1988). Reduction, however, often leads to *reductionism*, the projection of the schematic model over the studied situation, assuming the former as the essential truth of the latter (Harré, 1988; Santos, 1988). Therefore, the methodological operations of knowing have as a point of articulation a representation of the world and an epistemology that share a common trait: *determinism*. The triumph and ambition of that mode of knowledge production had their definitive expression in the words of Laplace (1749–1827) in 1886:

"An intelligence that, at a given instant, knew all the forces by which nature is animated and the respective situation of the beings that make it up, and that beyond this were vast enough to submit that data to analysis, would embrace in the same formula the movements of the greatest bodies in the universe and those of the smallest atom: nothing would be uncertain to it, and the future, like the past, would be present before its eyes" (1886: pp. vi–vii).

Determinism, as defined by Laplace, was found to be untenable by Physics itself due to later developments (thermodynamics, quantum mechanics, non-linear dynamics), but its appeal as a world view persists. In terms of what interests us in particular, the strength of deterministic conceptions can be perceived by examining more closely the epistemology associated with the simple realism previously described. Summarily, one could say that, for this epistemology, the reliability of scientific knowledge would be assured, on the one hand, by an exact description of objects and relations in the external reality and, on the other hand, by the rigorous, rational exam of experimental data (Taylor, 1998: p. 114). Each of these terms is centered on deterministic conceptions; on the one hand, the perception of reality is determined by it unidirectionally; on the other hand, the criteria of rationality is conditioned on the inflexible, automatic and even mechanical application (Bates, 2001) of immutable logical rules – an algorithm. This means that the conception of "rationality" in this case implies the exclusion of any human attribute – agency, will, values – from its operation. And, finally, the associated epistemology itself is also algorithmic, that is, deterministic in its operation, assuming the possibility of a sole demarcation criteria that separates, inexorably and automatically, science from pseudoscience, science from metaphysics, or any other opposition that one wishes to emphasize.

This schematic view of science was progressively criticized and even eroded throughout the last four decades, at least. An ever-increasing number of authors called into question this mechanical image of science, suggesting, instead of a process of discovering "things" that had always existed, the idea of a continuous construction of objects and knowledge (for a historical summary of the several positions and trend, see Latour and Callon, 1991). This "constructionism" (even if itself subject to criticism, see for instance Hacking, 1999) calls into question classical science's perspective (objectivity – realism – truth by approximation of "reality") and, as a consequence, the perspective of knowledge validation becomes a problem. In fact, Rorty, for instance, suggests the end of epistemology as a consequence of the pragmatic turn (Rorty, 1979).

Kuhn, in a posthumously published interview, illustrates this dilemma with the following comment regarding an invitation he had received to take part in a trial involving creationism, in Arizona: “Look, that one I declined for I think an excellent reason. [The people who approached me were resisting the creationists. I was sympathetic, but] I didn’t think there was a chance in the world... I mean I was being used by the creationists, for God’s sake! At least to some extent. And I didn’t think there was any way on the world in which somebody who didn’t quite believe in Truth, and getting closer and closer to it, and who thought the essence of the demarcation of science was puzzle solving, was going able to make the point. And I thought I would do more harm than good, and that’s what I told them” (Baltas et al., 2000: pp. 321–322).

There is, therefore, a problem for those of us who adopt to some degree world views that challenge the essentialist views of science and epistemology: how to refuse absolutes and still think of validating knowledge? I hope to signal a possible path in answering this question; initially, I will invoke the contribution of an author considered by many as a pioneer, *avant la lettre*, of contemporary science studies.

18.2 The Currency of Ludwik Fleck’s Contributions

I am referring to Ludwik Fleck (1896–1961), Polish doctor, heir to the Polish school of medical philosophy that flourished in the late nineteenth century (Löwy, 1994). A researcher in immunology, Fleck elaborated an original reflection about knowledge production in his own field of research, taking as a case study the modern definition of syphilis as a disease and the development of a laboratory test then viewed as highly specific for it. The title of his *magnum opus* is itself highly revealing: *Genesis and development of a scientific fact* (Fleck, 1979; see also Cohen and Schnelle, 1986; about the currency and importance of Fleck’s work, see Hacking, 1999 and Kuhn, 1979 and 1996: pp. viii–ix). The publication of his book in German, in Switzerland, on 1935 (a year after Popper’s *Logik* was published) went largely unnoticed. Although Fleck was recognized as a relevant researcher by his peers, his contribution to science studies only resurfaced in the sixties, due to a brief quote by Kuhn, who proclaimed him his predecessor and later stimulated the publication, at the end of the seventies, of an English translation of the “Genesis”.

There are two central concepts in Fleck’s work: the thought collective (*Denkkollektiv*) and the thought style (*Denkstil*). The former is defined as “(...) a community of persons mutually exchanging ideas or maintaining intellectual interaction, we will find by implication that it also provides the special ‘carrier‘ for the historical development of any field of thought, as well as for the given stock of knowledge and level of culture” (Fleck, 1979: p. 39) and the latter as “(...) a definite constraint on thought, and even more; it is the entirety of intellectual preparedness or readiness for one particular way of seeing and acting and no other" (Fleck, 1979: p. 64). It should be noted that the thought style is not an optional characteristic that can be voluntarily adopted, but rather an imposition of the socialization process represented by the inclusion in a thought collective.

Fleck distinguishes between two areas of importance in the interior of a thought collective in modern science (Fleck, 1979: pp. 111–2). One comprises the experts that effectively produce knowledge, named the *esoteric circle* by the author (who details this region even further, describing the innermost circle of specializes experts and the external circle of generalist experts), and another one which comprises the "educated laypeople", the *exoteric circle*. This topography allows for the distinction between different forms of communication (Fleck, 1979: p. 112). The experts' science is characterized by the technical/scientific periodicals and by the reference book, the former representing the intense, fragmented, personal and critical dialog within a given field of knowledge, and the latter its sinoptic organization (Fleck, 1979: p. 118). The exoteric circle is fed by the popular science magazines, which are "(. . .) an artistically attractive, lively and readable exposition with (. . ..) the apodictic valuation simply to accept or reject a certain point of view" (Fleck, 1979: p. 112). Finally, the introduction to the esoteric circle – likened by Fleck to an initiation ritual (Fleck, 1979: p. 54) – is based on the fourth kind of scientific textual medium, the basic manual (Fleck, 1979: p. 112).

Fleck makes one more important contribution to the history of sciences by showing that initial, allegedly "non-scientific", conceptions, which he names *protoideas*, are instrumental in research development and how they remain part of the disciplines' stock of knowledge, calling into question the idea of a science permanently surpassing and rupturing with the past (Fleck, 1979: pp. 23–5). Along these lines, Fleck also describes what he calls the tenacity of thought systems, which actively resist change, a resistance translated in the poetic expression *harmony of illusions* (Fleck, 1979: pp. 27–8). He goes on to list operations in progressive degrees that are adopted by thought collectives in order to protect their thought style from change, ranging from the impossibility of perceiving observations that violate the thought style to creative attempts to adapt the contradiction (Fleck, 1979: pp. 28–33).

Another relevant observation regards that which other authors have called the theory-ladenness of observations, caracterized by Fleck in observations concerning the representation of the human body in anatomical atlases. He describes how at Versalius's time the supposition that male and female genital organs were fundamentally homologous led that author to describe and draw in his *De humani corporis fabrica* a kind of deferent duct that would take the ovaries' "seed" to the uterus. Fleck complements that observation by stating that he tried to find moder, "correct" images tho compare with those, but all he could find, even with photographs, was a stylized representation of the underlying ideas that the images were meant to depict. "It is only theories, not illustrations, that can be compared" (Fleck, 1979: pp. 33–5).

From this summarized presentation of the complex, though clear, ideas of a seminal author, I intend to extract a set of tools fundamental to the task proposed for this text.

18.3 An Epistemology of/in Process

Before moving forward, it is necessary to explicitly present the definition of one of the fundamental objects of the discussion I put forth, knowledge. This text deals with propositional or factual knowledge (Huemer, 2002: p. 435). Usually the discussion of this definition leads to the so-called tripartite analysis (knowledge is (a) a belief; (b) true and (c) justified – see Zagzebski, 1999 and Welbourne, 2000), in itself object of so many other problems, to the point that Hacking includes “knowledg” in his list of “elevator words”, words that are called to work on a superior level that that usually employed to describe facts and ideas (Hacking, 1999: pp. 22–23). Without going into the details of this discussion, I draw attention to the fact that that which is designated “propositional knowledge” is restricted to simple assertions with an assumed factual basis. In this sense, a specific example from medicine would be something like “HIV is the cause of AIDS”, or “pneumococci usually respond to penicillin”. That simplicity, however, is deceptive. Let us consider the former assertion above. Paula Treichler attempted to “unpack” the various other assertions that hide behind an apparently elementary sentence:

“We can construct a set of statements about HIV, varying the points and the degree of transparency to vary the visibility of fabrication and cultural constructedness:

1. HIV causes AIDS.
2. HIV is the name that scientific culture gives the virus widely believed to cause AIDS.
3. HIV is the compromise name proposed by an international commission to resolve the bitter dispute over the “discovery” of a virus judged by many to be a causative factor in the infection and immune deficiency hat can lead to the specific clinical conditions diagnosed as AIDS.
4. HIV is the acronym adopted in 1986 by the international scientific community to name the virus hypothesized to cause immune deficiency in humans and eventually AIDS, another acronym, adopted in 1982 to designate a collection of more than fifty widely diverse clinical conditions believed to be given the opportunity to develop as the result of a severely deficient immune system;
5. HIV is a hypothesized microscopic entity called a virus (from Latin virus, poison) invented by scientists in the nineteenth century as a way to conceptualize the technical cause and consequences of specific types of infectious disease. A virus cannot reproduce outside living cells; it enters into another organism’s host cell and uses that cell’s biochemical machinery to replicate itself (in the case of HIV, often years after initial entry), at which point the cell’s DNA, with which the virus is integrated, is transcribed to RNA, which in turn becomes protein. Our knowledge of this “life story” has been produced by an intense national research effort focused both on HIV and on drugs designed to disrupt its life history at various points; as the major subject of scientific investigation and pharmaceutical research efforts and the major recipient of AIDS research funding, HIV is,

> therefore, as Joseph Sonnabend puts it, "metaphorically representative of other interests'"(Treichler, 1999: pp. 168–169).

I will return to this point later on, when I discuss the logical-cognitive tangles that hide behind each of the contemporary science's assertions.

The following is a short redescription of the tripartite analysis in (hopefully) less problematic terms. First of all, since it is an exam of propositional knowledge, instead of referring to "beliefs" (a term that brings with it additional problems), what is at stake are propositions or *assertions*. Additionally, according to Welbourne (2000), these are communicable, shareable and shared. Instead of "true" and "justified", recognizing the part of both historical contingencies and human agency in their formulation, it would be more adequate to refer to these assertions as *accepted as valid* by specific groups of investigation or research, according to *validation procedures* also accepted by the same groups, that in the end point to the construction of *coherent aggregates* that are, in their turn, nestled in a network of similar, previously validated, assertions – that which Bates (1998a, 1998b) calls unproblematic background knowledge, or UBK.

However, even "assertion" may be a mistake in this context. Although adequate for philosophical exercises, the conception of a validation process that takes isolated assertions one at a time, accepting or rejecting them based on a given set of rules, does not greatly correspond to the way through which investigation or research communities operate. And there is a fundamental flaw in this idea, in the sense that the type of assertion at stake has no meaning in itself (if any type does), but depends on a network of other assertions in order to have meaning. Fleck exemplifies this idea in his discussion about syphilis: "The statement, 'Schaudinn discerned *Spirocaheta pallida* as the causal agent of syphilis,' is equivocal as it stands, because 'syphilis as such' does not exist. There was only the ten-current concept available on the basis of which Schaudinn's contribution occurred, an event that only developed this concept further. Torn apart from this context, 'syphilis'has no specific meaning, and 'discerned' by itself is no more explicit than 'larger' and 'left' in the examples above [*referring to a previous example in his argument*]" (Fleck, 1979: p. 39). We can also turn to the already mentioned example by Treichler.

Another point worthy of note is the emphasis on social interactions. This is in fact a point of convergence for most of this text's theoretical references. Social instances are both the depository and arena where assertion aggregates are accepted and assimilated to the UBK or discarded. But "social instances" is much too diffuse an expression to be useful here. We can think of Fleck's thought collectives or Knorr-Cetina's epistemic communities as a more adequate and precise description. This, in turn, brings another important characteristic to consideration: in complex societies, there is a large number of such communities, and even in the postulated case of a global "scientific community" that shares great portions of a wide and encompassing UBK, there are heterogeneous zones, as the above mentioned Knorr-Cetina, for instance, points out (Knorr-Cetina, 1999). This means that the substitution of a chain of assertions in a given group's local UBK may not have an immediate effect on another, even if intimately related.

Finally, this redefinition is not a mere intellectual exercise. It has very concrete implications for the study of the complex interactions that continuously expand and reshape the UBK, or the collection of UBKs, of contemporary science. At least, this definition increases both the scope and the requirements of this task. Preliminarily, the historical perspective is fundamental. Additionally, the analysis of isolated concepts is, in itself, insufficient; a more encompassing approach is clearly necessary. It is not the case to simply compile a dictionary in which for each term of the science of the past or of exotic knowledge a correspondent term in the present science is produced. It is fundamental to apprehend a different way of thinking, in a work similar to that of the anthropologist who ventures into a culture that is not his own, as suggested by Kuhn: "I have already suggested that the past of science should be approached as an alien culture, one that the historian strives first to enter and then to make accessible to others" (Kuhn, 1978: p. 368). This last quote, finally, opens the possibility of a non-normative epistemology that is focused on understanding and describing how specific groups operate knowledge validation, instead of beginning its task with the prescription of how it ought to be done in general.

18.4 Commonsense About Science

An important element in the thought style or UBK shared by important segments of the so-called "Western society" (numerically important and also due to the power they detain) is founded on the protoidea of deterministic causality originally formulated by modern science. We should bear in mind that, although this is an element of common-sense, its standing as a protoidea makes it possible to find it in action even within the esoteric domains of a discipline – even when it is in conflict with the discipline's predominant methodological approach.

The deterministic causal logic has epistemological implications. On the one hand, it provides a model of a world divided in atomic events that follow one another linearly. On the other, by implication, it sanctions a specific model of knowledge validation, based on empirical data analyzed by an impersonal logic, leading to the formulation of general laws, of which those that can be expressed mathematically are considered most relevant, since Galileo. That model, finally, presupposes one Science unified by its validation model, allegedly applicable to any object, from infinitely small particles to extremely large astronomical objects, including human beings in an individual or collective scale.

This causal logic has pragmatic implications. The model of a unified science leads to the establishment of a hierarchy among different forms of knowledge. That which can be expressed numerically is seen as intrinsically more "scientific" that that which cannot. Designating something as "subjective" stops being a description and becomes an attribution of less worth – naturally, with regards to what is "objective". The social sciences and humanities, which by the intrinsic characteristics of their objects of study necessarily produce knowledge from hermeneutic models (Taylor, 1998), are then seen as "minor" with regards to the explanatory models of the sciences of nature. In the field of health, for example, the hierarchization of different forms of knowledge leads to a relative disqualification of professionals and practices that pay attention to what is “subjective”.

This causal logic has, finally, political implications. The realist epistemology presupposes a single reality of which it is the exclusive spokesperson. Correct knowledge of the causes of problems will inevitably define their correct solution. The bearer of that knowledge has, therefore, the epistemic authority to determine what solutions ought to be implemented. That is the technocratic temptation that is manifested, for example, in the current debate – in fact, it its absence – about this mysterious socio-political entity, the Economy. According to expert academics and specialized columnists, the market has triumphed, there is nothing left to discuss regarding the management of economic exchanges. *Roma locuta, causa finita*. The political debate – how to ensure the best life for all peoples, however that is defined – has been substituted by the reaffirmation of economic disciplinary principles. And these principles do not even reflect the internal wealth of the economic sciences. We are led to believe that neoclassical economy has shown itself to be more “scientific” than competing theories, becoming hegemonic because of this (Fullbrook, 2004).[1]

It is clear, therefore, that critical reflection regarding this common sense conception has a political function as well, following the idea of reformist constructionism such as formulated by Hacking (1999). It is necessary to point out that the emperor is naked. Taking into consideration, however, a note of caution from none other than Bruno Latour. In a recent text, while discussing how the strategies pertaining to the critical approach to science have been co-opted by the conservative-religious-fundamentalist coalition that came into power in the United States in the beginning of the century, Latour signals that the danger in this case comes not from ideological arguments presented as facts, but from an excessive distrust in actually reasonable matters disguised as damnable ideological biases. In his words, “Why does it burn my tongue to say that global warming is a fact whether you like it or not?” (Latour, 2005). As a researcher and professor, I defend the idea that there is an intrinsic value in knowledge. And as a health professional, I also defend the idea that, effectively, there are types of knowledge and practices whose judicious application contributes to a better life. In short, let us criticize Reason, without forgetting that its sleep, as Goya said, produces monsters. . .

18.5 Consequences of the Common Sense View – “Genocentrism”

In order to better illustrate the previous discussion, I suggest we consider a concrete example of the repercussions of the deterministic causality model in a contemporary debate. Let us consider the case of genetics. As a mere illustration of the pervasiveness of this theme, as I opened today’s paper in the science section,

[1] It should be noted that this text was written before the world economical debacle at the end of 2008 which profoundly shook these certainties.

there was a headline reporting the "discovery of the gene that makes people left-handed"...

As milestones of this "genetic turn" in public debate, I would point to two events that were intensely covered by the media: the announcement of the cloning of a mammal (the sheep "Dolly") in July 1996 and of the complete sequencing of the human genome in June 1999, both accompanied by an informational overload that brought with it the implicit suggestion of scientific revolutions and renewed promises of unimaginable diagnostic and therapeutic advancements.

The idea that diverse characteristics of living beings are passed on from one generation to the next is not new; it is even older than modern science. Millennia of experiments with domestication and selective reproduction of plants and animals are in the very origin of what we call civilization (Diamond, 1999). The modern synthesis of the findings of Mendel, Darwin and twentieth century molecular biology, however, is considered, justifiably, one of the great accomplishments of modern science. The processes of biological development, in which genetic material (that is DNA) plays a key role is one of the most notable examples of a complex model, for scientific investigation as well (Kay, 2000; Keller, 2002; Lewontin, 2000): multiple interactions, from the most microscopic possible level (interactions between specific sites in complex molecules) to the most encompassing one (all interactions between organisms and environment, considering that the latter is also a product of the former), an infinity of mutually influencing events, with the appearance at each articulation level of emergent properties, not linearly mappable to the subjacent events. In one word, complexity, in all the concept's extent.

That has not kept that complex dynamic from being captured by the deterministic thought style. The inherent complexity of the field of knowledge related to genetics makes it possible for experts in a given subfield, for example, molecular genetics, even if they are part of the esoteric circle of their subdiscipline, to be part of another's exoteric circle, for instance, population genetics. This makes it even harder for participants in the epistemic community to critically evaluate the area's general ensemble, making them more susceptible to the interference of deterministic protoideas in their thought style. Complex interactions ate turned into a simple model with series of linear causes, tentatively expressed by the following set of assertions:

- each gene determines an elementary, atomistic trait of an organism;
- the collection of genes determines, in a one to one correspondence, the set of characteristics that make up the totality of that organism;
- each organism is therefore an aggregate of these characteristics (the species is defined by a generic set of characters, each individual, by the effective values each character assumes among possible values);
- DNA contains a "program" that codifies the whole organism;
- each of the organism's singular traits is a result of the competitive process of natural selection.

Each of these statements is criticized by one authors already mentioned. Besides Kay, Lewontin and Keller, Eldredge (2004) specifically criticizes the panadaptionism expressed by the last assertion. Despite all this, that model continues to be disseminated, especially through popular scientific magazines, assuring its repercussion in the various exoteric circles, including those, as described earlier, made up of specialists from the several sub-areas in the field. That selective, simplified assimilation of technological development in the genetics field has led to a reinforcement of reductionistic and deterministic conceptions regarding human beings and society, in a revival of 1970s sociobiology that makes clear the political-ideological articulation of these conceptions with the conservative perspective: the selfish gene articulates admirably well with the utility-maximizing agent from neoclassical economy. These conceptions are also reflected on the representations of the health/disease process, being expressed, among other things, in the generic assertion "the gene of the X disease" that has as its corollary (almost invariably explicitly mentioned in news stories about the gene in question) the idea that a radical and definitive cure for X is just around the corner. Thus, recurring patterns in the history of medicine and its relation to society are once again set in motion, in particular the idea of the magic bullet and the reinforcement of cognitive authority, as mentioned earlier. Since almost all aspects of human life are reduces "to our genes" (a frequent expression in public discourse), it follows that biological specialists are the socially legitimate possessors of the ultimate secrets of life and death.

This quick exercise shows, in my opinion, both the consequences of the deterministic conception, even while being superimposed on a logic of investigation that has surpassed it historically, and the capabilities of the described theoretical apparatus to respond to the challenges created by the persistence of this conception.

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Chapter 19
The Metaphysics of Non-individuality

Décio Krause

19.1 Individuals

Sometimes we feel that the better way to delineate what something looks like is by describing what it is not. Due to limitations of space, I shall use this strategy here in order to provide a (even broad) characterization of what I mean by non-individuals. Thus this section on individuals is wide than the next one, where I sketch the notion of non-individuals properly.

Informally speaking, by an individual we usually mean an entity ("object", is sometimes used as synonymous) that, at least in principle, can be distinguished from any other entity, even of a similar species. We shall say that an individual obeys the rules of the theory of identity of classical logic. Even if an individual is mixed with others of similar species, it "retains" its individuality, its "sameness", its "identity". It is *one* and, at least in principle, can be always "separated" from the others, even if not "effectively", say by a formula. A long tradition in Western philosophy has discussed what confers individuality to an individual (see Quinton, 1973), and most of the answers fall within one of the two following lines: (i) *substratum* theories, and (ii) bundle theories, none of them absent of problems. The first one presupposes that an individual is not just the sum of its properties, but it is more. In the recent literature on the philosophy of physics, some concepts (some of them quite old but renewed in this context) reborn: "haecceities", "primitive thisness", and so on, all of them remembering the idea of an underlying *substratum* which would retain the individuality of the individuals, despite the changes in their properties. Bundle theories avoid speaking of any kind of substratum, claiming that an individual is, in a certain sense, just the sum of its properties. This view also encounters difficulties, say with standard assumptions such as Leibniz's identity of indiscernibles which underly classical logic (more on this below). The problem concerning individuation is still alive in the philosophical literature, and the raise of quantum physics poses

D. Krause (✉)
Department of Philosophy, Federal University of Santa Catarina, 88040-900 Florianópolis, SC, Brazil
e-mail: deciokrause@gmail.com

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another cluster of problems due to the (apparent) lack of individuality of quantum objects.

The very important question of distinguishing between individuality and distinguishability has also been considered in the philosophical literature, and it has been claimed that these concepts are in fact distinct, that is, it is not due to the fact that something can be distinguished from others that it is an individual (the case of a possible world with just one object comes to the mind; such a thing would be an individual, although it could not be distinguished from any other object – since there are none). Despite its importance, we shall not revise this history here, to which we report to French and Krause (2006), but keep with the intuitive description given above. Furthermore, in mathematics we can find "individuals" which are really different but such that we cannot point the difference (the excluded middle law $a = b \vee a \neq b$ holds, although a and b cannot be *shown* to be different.

Here, I shall assume a quite related but distinct informal notion. An individual is something that, metaphysically speaking, being part of a whole, once it is exchanged with something else (even with something of similar species), the final result is regarded as distinct from the original one we had before the permutation. In short, a kind of "invariance by permutations principle" should not hold here,[1] so distinct individuals would not obey the substitutivity principle, that is, they cannot be permuted one each other *salva veritate*, thus one of the basic laws of the logic of identity holds (see below). To exemplify, suppose that John and Paul arrive at the same time to buy the only ticket available to a show. Of course we can regard the audience as distinct depending on who buy the ticket – a little bit exaggerating, we can say that this does not happen if they were quantum objects of the same kind.

Thus, a configuration with individual A is distinguishable from a configuration with *another* individual. From a mathematical point of view, these configurations can be taken to be sets subjected to the axiom of extensionality of standard set theories (we shall be speaking of ZFC here, but the same could be said of theories such as NBG, KM, and even of NF and ML).[2] This corresponds to the view that sets of individuals have a cardinal. Individuality in this sense seems to be linked to identity (and difference). In a certain (informal) sense, an individual has a well defined *identity* and it is *different* from any *other* entity, at least by force of logic. Furthermore, it seems that we would regard an individual as identical to itself. How could it be differently? Even when we consider uncertainty and vagueness, it is supposed that these concepts are something related to the languages we use; for instance, "bald" is a vague predicate, but the individuals to which it is supposed to apply are not. John is a well defined "individual", although the sentence "John is bald" might be vague.

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[1] As assumed in standard quantum mechanics – see van Fraassen (1998), (French and Krause, 2006, Chap.4).

[2] For a general approach to the foundations of set theory, we report to Fraenkel et al. (1973). ZFC is the Zermelo-Fraenkel set theory with the axiom of choice.

When we try to formalize this informal concept of individual, in order to make it useful for certain philosophical and technical discussions, we find difficulties. Let us use the symbol "=" for identity, and write "$x = y$" to mean that x is identical to y. What does it mean? Firstly, I think that we may agree with a tradition which goes back at least to Frege's *On sense and reference* (Frege (1948)) and say that identity applies to objects and not to names of objects. Thus, we say that if "$x = y$" is true, then x and y are the very same object, that is, that there are not two objects at all, but only one, which can be named by either x or y, despite the redundancy of this characterization, for it uses concepts similar to that one which is being defined.

If we consider these informal ideas within a formal system (say, of first-order) with "=" as a primitive symbol, the above informal ideas lead us to the standard first-order postulates (or to something equivalent): reflexivity of identity ($\forall x(x = x)$, being x an individual variable), and substitutivity ($\forall x \forall y(x = y \wedge F(x) \rightarrow F(y))$ with the usual restrictions). If we give a standard interpretation to our considered language, x and y are supposed to refer to certain individuals, elements of a certain non-empty set. An already long tradition has kept us with set theory: individuals are collected in sets, which are (informally speaking) collections "of distinct objects of our intuition or of our thought," according to Cantor (informal and imprecise "definition" of course). If the bound variables refer to elements of a set, what would be understood by "="? The answer is that the binary predicate "=" would indicate the diagonal of the domain of the interpretation, namely (being D the domain), the set $\Delta_D = \{\langle x, x\rangle : x \in D\}$. But we also know that the above postulates do not characterize the diagonal up to a congruence relation (Hodges (1983), French and Krause, 2006, § 6.3.1).

Of course there is a sense according to which these semantic details and the involved concepts can be reduced to a syntax of a stronger language in which we can speak about the semantic concepts of our "object language" (Church, 1956, § 09). For theories based on classical logic sketched above, we can suppose that this stronger language is the language of the ZF set theory – this point will be important to our discussion in the next section.

Some authors, such as Quine, prefer to work with first-order languages containing only a finite lexicon, and then "identity" is defined by the "exhaustion of combinations" in all predicates (Quine, 1986, p.63). But this does not define *identity* strictly speaking, but only *indiscernibility* (with respect to the chosen predicates), for distinct individuals can obey exactly the same chosen predicates.[3] Of course we could also mention Peter Geach's ideas involving "relative identity": the most we can say is that x is the same F as y, where F is a sortal concept, like "the same person as" (Geach (1967)). These examples show that we would be aware that there is not *only one* concept of identity. Similarly as we can say that what is a set depends on the

[3] As acknowledged by Quine himself when he says that "[i]t may happen that the objects intended as values of the variables of quantification are not completely distinguishable from one another by the (...) predicates. When this happens, (...) [the exhaustion of combinations] fails to define genuine identity. Still, such failure remains unobservable from within the language." (idem, ibid.).

postulates (the set theory) we are considering,[4] what is *identity* will also depend on the postulates we ascribe to this notion, although we may have an intuitive concept in mind. But apparently classical logic, as an extension of traditional (Aristotelian) logic, was built with an eye in the macroscopic objects of our surroundings, which are thought as individuals in the intuitive sense above. Thus, the proposed postulates (reflexivity and substitutivity) can be taken as *the* postulates of first-order identity – really, they *are* the postulates used in most logic books. But we could thought of higher-order languages instead.

In higher order logic, we can define identity by Leibniz Law, namely, $x = y =_{def} \forall F(F(x) \leftrightarrow F(y))$. This definition, which equals identity and indiscernibility, is inspired in Whitehead and Russell's definition presented at *Principia Mathematica*, but they avoided to accept any predicate in the range of the quantifier for in this case we could suppose the predicate of self-identity being included (namely, being a the name of an object, the self-identity of a can be defined as $I_a(x) =_{def} x = a$).[5] According to Whitehead and Russell, this would entail that identity is being defined presupposing identity itself, hence the definition would be impredicative (Whitehead and Russell, 2008 p.49). But I don't think we need to fear for such an impredicativity, as standard mathematics doesn't. So, Leibniz Law, in my opinion, does not need to discharge self-identity, which is, by the way, a quite "natural" predicate, and to rule it out would be, in my opinion, something quite artificial and ill-justified.

The problem is that, first, if self-identity is allowed, then Leibniz's identity of indiscernibles (the claim that indiscernibility is a sufficient condition for identity in the above Lebniz Law) is a theorem of higher order logic; hence, there are no distinct indiscernible individuals, as it would be expected within a "Leibnizian" way of thinking. But Leibniz Law doesn't characterize the diagonal of the domain of individuals either, for we can always show interpretations where this law is obeyed by a and b although a and b are different individuals. In short, just take a second order language with a and b as individual constants and A, B, C unary constant predicates, and define an interpretation whose domain is the set $D = \{1, 2, 3, 4\}$ and such that a and b are interpreted as 1 and 2 respectively, while the predicates are associated to the subsets $\{1, 2\}$, $\{1, 2, 3\}$ and $\{1, 2, 4\}$ respectively. Then it is easy to see that a and b obey Leibniz Law, yet are distinct natural numbers.

The incapacity of the above semantics to discern between a and b is due to the lack of the "predicates" (really, the extension of the predicates) of self-identity,

[4]For instance, the universal set is not a "set" in ZFC, but "exists" in Quine's NF. The so-called "Russell set" $\mathcal{R} = \{x : x \notin x\}$ is also not a set in ZFC (supposed consistent), but "exists" in some paraconsistent set theories (da Costa et al. (2007)).

[5]More precisely, their definition reads "$*13.01.\ x = y. =: (\phi)(\phi!x. \supset .\phi!y)$ Df." That is (in their notation), x and y are identical when every predicative function satisfied by x is also satisfied by y, and they emend that, due to the axiom of reducibility, "the definition is as powerful as it would be if it could be extended to cover *all* functions of x." (Whitehead and Russell, 2008, p.168). I still recall that Whitehead and Russell regard the predicate $I_a x$ defined above as a function of x (ibid., p.49).

which standardly would be interpreted in the singletons $\{1\}$ and $\{2\}$. With these predicates, 1 and 2 can be discerned, for only 1 has the property of "being identical to 1" (that is, $1 \in \{1\}$, while $2 \notin \{1\}$).

Thus we see that if we are not committed to restrictions such as Quine's in calling identity a defined concept of indistinguishability relative to certain predicates, although we (may) have the informal concept of identity delineated in the beginnings in mind, we shall have troubles in formally characterizing this intuitive concept. But once we join the syntactical aspects, say, either the two above first-order postulates, or Leibniz Law, with semantics involving full models (Church calls those models which consider all the subsets of the domain "principal" (Church, 1956, p.307)), then we get a way to prove by pure logic that an individual can be identical just to itself. (In considering all the subsets of the domain, all the relations on the domain and so on, we are taking all possible properties and relations the individuals can partake, and this entails identity, according to classical logic, as we saw. In other words, within the classical standards, there are not indiscernible things, that is, there are not objects that can be permuted *salva veritate*, except if they are *the very same object*.

Another way to see the same result is by considering a fragment of a set theory such as ZFC (having identity as a primitive symbol) encompassing, say, the axioms (or axiom schema) of extensionality, separation, pair, power set, union, infinite, regularity, and choice. A "model" of such a theory is the von Neumann hierarchy of well-founded sets $\mathcal{V} = \langle V, \in \rangle$, where $V = \bigcup_{\alpha \in On} V_\alpha$, and the V_α are defined by transfinite recursion on the collection On of ordinals as follows: $V_0 = \emptyset$, $V_{n+1} = \mathcal{P}(V_n)$, and $V_\lambda = \bigcup_{\beta<\lambda} V_\beta$ when λ is a limit ordinal. If *Urelemente* are allowed, a slight modification in this definition must be done, but the result to be mentioned below still holds. Every set of ZFC is an element of V_α for some α (and then it is an element of any V_β for $\beta \geq \alpha$, for the hierarchy is "cumulative"). The least α such that $A \in V_\alpha$ is the *rank* of the set A. Any mathematical structure, such as groups, vector spaces, differential manifolds, Hilbert spaces, etc., are sets in this sense. Structures of this kind admit certain mappings that are bijective and "preserve the relations and operations", called the automorphisms of the structure. The collection of such automorphisms, considered with the composition of mappings, form a group, the group of the automorphisms of the structure, or its *Galois group* (da Costa and Rodrigues (2007)). If there is an automorphism h of a structure $\mathfrak{A}$ leading an element a in an element b, then a and b are indiscernible from the point of view of the structure (recall Quine's quotation in footnote 3); in other words, *within* the structure, or by its only resources – predicates and relations – nothing can distinguish between a and b: they are $\mathfrak{A}$-*indiscernible*. For instance, in the field of the complex numbers $\mathfrak{C} = \langle \mathbb{C}, +, \cdot, 0, 1 \rangle$, the complex numbers i and $-i$ are indiscernible, for the only automorphisms of the structure are the identity function and the bijection $h(a + bi) = a - bi$. But i and $-i$ can be discerned *from the outside* of the structure, as we realize when we accept that they are in fact not identical, as we can proof in an extended "rigid" structure. This last information is quite important. Independently of the considered structure, even if the structure has indiscernible elements in this sense, they can always be distinguished from the outside. The "outside" is of course the whole ZFC universe $\mathcal{V} = \langle V, \in \rangle$ which, seen as a structure, is rigid. Really, we can prove that any structure (in ZFC) can be extended (in several ways, say by adding further relations and operations) to a *rigid* structure, that is, a structure whose only automorphism is the identity function (in this structure, the only element indiscernible from a is a itself). Moral: in *the whole* ZFC, every object is an individual, in the sense that the above mentioned excluded middle law always holds. Indiscernibility can be dealt with only *via* restrictions, which may came in the form of an invariance permutation principle.

19.2 Non-individuals

Thus, as we see, standard mathematics (and logic!) is ontologically committed to individuals in the sense just described.[6] If we intend to deal with indiscernible objects within these frameworks, we need to do a mathematical trick, such as to restrict the discussion to a certain structure that has automorphisms other than the identity function. For instance, in a group $\mathfrak{G} = \langle G, * \rangle$, all the elements belonging to the *orbit* of $a \in G$, namely, the set $O(a) =_{def} \{b * a : b \in G\}$, can be supposed to be indiscernible from a, although they are not *identical* with a strictly speaking (when $O(a) \neq \{a\}$, that is, when $O(a)$ has at last two elements). Sometimes we express that by saying that the relevant functions and operations to be considered are invariant under certain symmetric functions. A typical case is in standard quantum mechanics; bosons are "particles" that can be aggregated having all the same quantum numbers. Bosons in certain states (such as Bose-Einstein condensate) would be *absolutely* indiscernible, having all the same quantum numbers. But, can they be discerned "from the outside"? Outside what? To answer that, we need to consider quantum structures, and this leads us to face another cluster of problems. Really, there is not only one formulation of the theory, and even if we assume one of them, say a standard formalism *via* Hilbert spaces (say von Neumann's–see (Redhead (1987), § 1.2)), there are different *interpretations* of this formalism, and depending on which one we choose, we arrive at different ontologies. Anyway, whatever structure we consider, it is a mathematical structure that can be supposed being build within ZFC, so it is subjected to the same restrictions mentioned above (it can be extended to a rigid structure). Even if we go further to quantum field theories (QFT), we ought to recognize that the mathematical structures we use (say, differential manifolds) are "classical", and then any described object is an individual. Hence, fields are individuals, so are the "field quanta" (or "particles" in QFT – see below).

In QFT, we do not deal with "particles" directly. The basic ontology of QFT is composed by fields, insists Tian Cao (Cao (1999), p.4). Particles (of course nothing similar to "classical particles", typical of classical physics) arise as epiphenomena

[6] Thus they are not completely "neutral" as some like Quine himself and Bunge have claimed (Bunge, 1977, p.15).

(Falkenburg (2007)),[7] or field quanta, or the quanta of fields. Falkenburg continues by saying that field quanta of integer spin (bosons) obey B-E statistics (while those of half spin–fermions–obey F-D, exactly as in the non-relativistic case – ibid., p. 224). Really, although we describe then mathematically within mathematical structures (respectively the Klein-Gordon equation and the Dirac equation), there is a sense in speaking of "particles" of a kind (quite different from the "classical" ones).[8] Important to remark that since bosons obey B-E, they cannot be individuals in the standard sense, for no individuals can obey such a statistics.[9]

Quantum objects, if we believe in the story told by quantum physics, seem to behave as non-individuals (this point would be justified in full, but there is no space to to it here – but see (French and Krause, 2006)), and when they can be discerned, they can be discerned, full stop. The right relation to hold among then is to be indiscernibility, and not identity, for the former (in my opinion) suits better with the claims of quantum physics and does not present the above touched problems regarding identity and individuality. This view enters QFT as well as orthodox QM. A long time ago, Heinz Post claimed that quantum objects are not individuals, and that their indiscernibility should be considered *right at the start* (see (French and Krause, 2006), p.318). John Stachel, discussing the "puzzle of individuality" in his Stachel (2005), has emphasized two ways of looking to the *loss of individuality*:[10] the top down direction accepts that some entities, firstly regarded as individuals, should not be considered as such – as in the case of Heisenberg and Schrödinger just mentioned. The another view goes upwards, and assumes the non-individuality at the start, exactly as Post has claimed for. This is the route we have followed, as we shall see below.

Thus we arrive to a situation we can describe as follows. Suppose we have a well developed *quantum language*, $\mathcal{L}_Q$.[11] This language has, by hypothesis, syntactical

[7] A nice comparison among the different concepts of "particle" in the different mechanics is given in (Falkenburg (2007), Chap.6).

[8] Falkenburg recalls that many textbooks on QFT identify these field quanta with particles, yet she doesn't explain in what sense of the word "particle" (idem, ibid.).

[9] Some philosophers, trying to unify the standard statistics, have considered that individuals would obey B-E once we assume some qualifications. Their argumentation runs basically as follows. Suppose two individuals a and b and two possible states A and B. The possible distributions are (1) Aab and $B-$ (this indicates that there are no individuals in B), $A-$ and Bab, (3) Aa and Bb, and (4) Ab and Ba. Then just consider that to the situations (3) and (4) we attribute probability 1/6, while (1) and (2) get 1/3. This is B-E, they would say van Fraassen (1998). I don't agree, for in order to count (3) and (4) as distinct situations, we need to consider that a and b are distinct, thus they cannot be indiscernible.

[10] Heisenberg and Schrödinger, despite their differences in opinion concerning quantum mechanics, also have spoken of the "loss" of individuality in the quantum realm – cf. French and Krause (2006). This is a typical reasoning grounded on standard logic, mathematics, and physics, where the entities are individuals at the start, and them made non-individuals by hand. I my opinion, regarding the metaphysics of non-individuals, there is no individuality to lose.

[11] I recall that Yuri Manin suggested that orthodox quantum mechanics does not have its "own" language, making use of a fragment of standard functional analysis (Manin, 1977, p.84). Sure he was thinking of Hilbert spaces. This claim does not weaken our arguments, for we could keep (in

well defined rules so that the syntax of $\mathcal{L}_Q$ may be supposed to be given. Let us suppose further that we give an interpretation to $\mathcal{L}_Q$ which is compatible with the above discussion on the indiscernibility of quantum objects. That is, in $\mathcal{L}_Q$ we can express, say, that some objects (that can be values of the variables of $\mathcal{L}_Q$) are absolutely indistinguishable, say bosons in a Bose-Einstein condensate (if this is not possible, for sure $\mathcal{L}_Q$ would be not suitable for quantum physics). So, we have a "semantics" for $\mathcal{L}_Q$. This informal semantics can be (in a precise sense) reduced to syntax, as we have mentioned in the previous section. To this reduction, we consider a stronger language $\mathcal{ML}_Q$, in which we can express the syntactical equivalents of the semantic concepts of our delineated semantics.

In the case of standard $\mathcal{L}_Q$, it is reasonable to suppose that $\mathcal{ML}_Q$ may be the language of ZF, in the sense we have remarked above. The question now is: can we reduce to such a syntax the talk of indiscernible quantum objects? It seems to me that, strictly speaking, we can not. Really, as we have seen in the first section, within ZF we are unable to represent *legitimate* indiscernible entities, that is, entities treated as such *right from the start*, from the bottom. The only way to speak of indiscernible quantum objects would be to restrict our discourse to a certain structure (or to a finite lexicon) where we can reason as if the involved entities are indiscernible (the top-down direction mentioned above). But since in ZF any structure can be extended to a rigid one, the individuality of the objects would be soon unrevealed. Of course this strategy can be useful for physics, but I think that it encompasses a philosophical gap, for we would be able to cope with our semantics as conceived, that is, encompassing absolute indiscernible non-individuals.

19.3 A Proposal

The "semantics" (an intuitive interpretation grounded on the lessons taken from quantum theory) mentioned at the end of the last section bases a metaphysics of non-individuals, of entities devoid of identity, entities which (in principle) would be absolutely indiscernible, distinguishable *solo numero*, entities such that whatever of them may enter in a context with equal results, and so on. Quantum objects, under a rather plausible interpretation, would be candidates for exemplifying it (for a discussion of this view, so as of an alternative that treat them as individuals, see (French and Krause, 2006)).[12] Of course we might suppose that there may be other models of non-individual entities, but we shall keep with the quantum one.

Within the analytic tradition, we should not keep restricted to such a general discussion, but try to find a "logical" sense to such claims instead, that is, a way to represent *absolute* indiscernibility. The mathematical treatment that agrees with this

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principle) a certain formulation of a quantum physics sufficiently precise – in the mathematical sense – for making sense what we are saying even if we consider QFT.

[12]Thus we are not claiming that quantum objects *are* non-individuals, for this seems to be a physical problem. To consider them as non-individuals is just one of the possible metaphysics associated to quantum physics (as proposed by S. French – see (French, 2006)).

philosophical point of view would be done *outside* classical frameworks, which are (as we saw) compromised with individuals. This (I think) can be done, at least partially, by using *quasi-set theory*. This theory was built with an eye in the behavior of quantum objects by following some intuitions advanced by Erwin Schrödinger (the whole history is in (French and Krause, 2006)) in saying that the concept of identity, of sameness, would not make sense to these entities. The theory overcomes ancient problems as those mentioned above concerning the impossibility to deal with "legitimate" (made as such right from the start) non-individuals. The theory can still be useful to sustain a view that entities without identity do exist, in the sense that they can be values of the variables of a regimented theory. (I shall not discuss this point here, but see (Krause, 2008)). I will also not revise quasi-set theory in this paper, to which I report to the Chapter 7 of French and Krause (2006). A first approach to a quantum mechanics by considering non-individuals from the start and not assuming them as individuals, as we necessarily do when we work within a standard theory such as ZF, taking quasi-set theory as the mathematical basis, was proposed in Domenech et al. (2008). There, we have built a Hilbert space, the $\mathfrak{Q}$–space, using the non-classical part of quasi-set theory (that part that encompasses objects without identity), to deal with indistinguishable elements. Vectors in $\mathfrak{Q}$–space refer only to occupation numbers and permutation operators act as the identity operator on them, reflecting in the formalism the fact of unobservability of permutations. This is, apparently, the first time where a quantum theory is built within a mathematical formalism (stronger than the propositional logics of Reichenbach, Birkhoff-von Neumann, or Fèvrier – for references, see (French and Krause, 2006)) other than classical mathematics.

Quasi-set theory was used also to approach a view of quantum objects as vague objects (French and Krause (2003)), in the sense that identity conditions cannot be ascribed to them as usual. Since in its underlying logic the notion of identity does not apply to all (pair of) objects of the considered domain, the theory can be classified in the class of *non-reflexive logics*, really as a non-reflexive mathematics. Other indications can be found in (French and Krause, 2006, Chap. 8). Important to say that quasi-set theory does not compromises us with the acceptation of some kind of substance, in particular in seeing the basic non-individual atoms as "particles" in some sense of the word. The dealing with non-individuals can be extended to other objects viewed as *structures*; in a certain sense, pure *forms*, which we could term *non-reflexive structures* (a first account to such structures can be seen in Krause (2005)). Really, if we conceive a metaphysics grounded on non-individuals, we can admit the existence of "structures" which strictly speaking don't have individuality (in the sense put above that individuality entails uniqueness). The mathematical terminology in saying that isomorphic structures are "identical" is clear in its meaning but, in my opinion, it is not quite distinct than saying that two bosons are "identical". I am not proposing to change the mathematical terminology, for all attempts in this direction failed, as we know quite well (take for instance Bourbaki's proposal of naming categoric theories "univalent"). Thus, I think that at least to understand what is going on from a philosophical perspective we should recognize that the abstract idea of a particular structure (say, a group) could be thought of as a general term like "horse". It may have lots of instances, yet in certain cases all indiscernible from one another – when they are isomorphic). This enables us to speak of the structures not as *individuals*, for a structure is not an individual in the sense described in the first section. It is not one, but encompasses an infinity of indiscernible realizations, or instances. Thus, if we ground our claims in that all there is are structures, as the proponents of the ontic structural realism do (French and Ladyman, 2003), then perhaps their basic ontology could be though in terms of non-individual structures (still structures, but not seen as standard individuals); but this is a topic to be further investigated.

The subject of investigating a metaphysics of non-individuals is still in its beginnings, but I guess that if we assume a metaphysics in which identity is not a necessary concept, and that "objects" may be absolutely indiscernible without being identical, quasi-set theory and its underlying *logic of indiscernibility* may result to be in fact useful.

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Chapter 20
Einstein, Gödel, and the Mathematics of Time

Francisco Antonio Doria and Manuel Doria

20.1 Introduction

Discussions on the nature of time are as old as philosophical inquiry itself, and have always riddled scientists and philosophers alike with its many perplexities. Twentieth century physics, entertaining us with theoretically feasible and actual phenomena such as temporal dilation, time travel, timeless singularities – and, as we shall see in this article, the possibility that there is no global arrow of time – has only deepened the mysteries surrounding the concept of time.

Progress has been made in the terrain of cognitive science, particularly in the description of cognitive mechanisms involved in the conceptualization of time (Lakoff and Johnson, 1999). Lakoff et al. suggest that it is "virtually impossible to conceptualize time without metaphor." Even Kant, who in the eighteenth century championed the thesis that time was a pure a priori intuition that necessarily structured all our subjective experience admitted that we reasoned about it in terms of an iterated progression along a geometrical line (just like Galileo in the dawn of kinematics). Contra Kant, Lakoff claims that there is no such thing as a "pure intuition" of time: temporal concepts themselves have an internal structure that is largely assembled by our prior experiences of motion in space. General relativity itself conceptualizes time metaphorically as a space – akin dimension on the spacetime manifold.

We take our cue from the fact that Einstein and Gödel were close friends, and yet the only ground which they eventually shared in scientific terms were Gödel's papers on general relativity. No doubt those are landmark papers: they show that general relativity allows for the existence of an universe with intrinsic rotation; they suggest the possibility of a time machine – and they have a very counterintuitive kind of time, as we do not have a "global" time coordinate in the Gödel universes. It is meaningless to refer to a "beginning of time" in such universes.

F.A. Doria (✉)
Advanced Studies Research Group, Fuzzy Sets Laboratory, PIT, Production Engineering Program, COPPE, Universidade Federal do Rio de Janeiro, 21945–972 Rio de Janeiro, RJ, Brazil
e-mail: fadoria@gmail.com

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Is that an isolated phenomenon? Nonexistence of a global time coordinate is just a property of Gödel's and Gödel – like models of the universe? Or can it be seen as the typical situation? This is the underlying question in the present paper, and in order to deal with it we concoct a potion that mixes up ingredients from differential geometry, from general relativity and from logic. We will argue at the end that:

> Nonexistence of a global time coordinate, from Big Bang to Big Crunch may well be the typical, generic situation in general relativity.

We present a result whose interpretation may support that claim.

The idea that there is no universal direction of time may sound cognitively abhorrent precisely because of the everyday metaphors involved in thinking about time (see (Lakoff and Johnson, 1999), entry on "The Moving Time Metaphor"). A theory being counterintuitive may be a consequence of either taking as literal without sufficient ground metaphorical aspects of a certain concept or as not having apt conceptual metaphors for dealing with novel empirical phenomena.

20.1.1 The Meaning of "Generic" in This Paper

We use the word "generic" in several different senses in this paper:

1. *Topologically generic sets.* Given a topological space X, a subset $Y \subset X$ is *topologically generic* if its complement is a first – category set (a meager set).
2. *Measure – theoretically generic sets.* Given a space X endowed with a measure μ, a subset $Y \subset X$ is *generic for measure* μ if $\mu(X - Y) = 0$.
3. *Set – theoretically generic sets.* Let $\mathbf{L}$ be Gödel's constructive universe of sets, and let $\mathbf{L}^B$ be a forcing extension of $\mathbf{L}$, or a Boolean extension of it. Then a set $x \in \mathbf{L}^B - \mathbf{L}$ is a *generic set*.

Set – theoretically generic sets may be collected in measure – theoretically or in topologically generic sets, given adequate axioms (see below the discussion of Martin's Axiom). We will sometimes speak of generic sets without qualification; context will make clear the intended meaning of the word.

We will sometimes use "typical" as a loose, informal way to describe sets that can be made generic in one of the senses above.

20.1.2 Preliminary Concepts and Results

We summarize here the axiomatics for general relativity that has been introduced in (da Costa et al., 1990) and more recently described in detail in (da Costa and Doria, 2007). That axiomatics is "natural" in the sense that we simply rebuild the usual mathematical background for gravitation theory within Zermelo – Fraenkel set theory with the Axiom of Choice (ZFC). (For details see (da Costa and Doria, 2007).)

Roughly, we take general relativity to be a theory so that:

- Its arena is an arbitrary 4–dimensional noncompact real differentiable manifold, which we identify to spacetime. Therefore we must consider in our characterization of general relativity, the collection of all 4–dimensional real noncompact manifolds with a differentiable structure, a notoriously complicated object.
- To each such 4–dimensional real differentiable manifold we add a smooth pseudo–Riemannian metric of signature +2, and then the Einstein gravitational equations, with or without the interaction of matter fields.
- We also add as much extra structures as required for the description of the fields that appear in the energy – momentum tensor.

General relativity is a theory of gravitation that interpretes this basic force as originated in the pseudo – Riemannian structure of spacetime. That is to say: in general relativity we start from a spacetime manifold (a 4–dimensional, real, adequately smooth manifold) which is endowed with an pseudo – Riemannian metric tensor. Gravitational effects originate in that tensor.

Given any 4–dimensional, noncompact, real, differentiable manifold M, we can endow it with an infinite set of different, nonequivalent pseudo – Riemannian metric tensors with a Lorentzian signature (that is, $- + ++$). That set is uncountable and has the power of the continuum. (By nonequivalent metric tensors we mean the following: form the set of all such metric tensors and factor it by the group of diffeomorphisms of M; we get a set that has the cardinality of the continuum. Each element of the quotient set is a different gravitational field for M.)

Therefore, neither the underlying structure of M as a topological manifold, nor its differentiable structure determines a particular pseudo – Riemannian metric tensor, that is, a specific gravitational field. From the strictly geometrical viewpoint, when we choose a particular metric tensor g of Lorentzian signature, we determine a g–dependent reduction of the general linear tensor bundle over M to one of its pseudo – orthogonal bundles. The relation

$g \mapsto g$–dependent reduction of the linear bundle to a pseudo – orthogonal bundle is 1–1. This is equivalent to endowing spacetime with a smooth 1–foliation.

20.1.3 Spacetimes with Cosmic Time

Definition 1.1 A spacetime M has a *global time coordinate* whenever:

1. M is diffeomorphic to $N \times \mathsf{R}$, where N is a differentiable, real, 3–manifold.
2. M is endowed with a pseudo – Riemannian metric tensor of signature

$$(-1, +1, +1, +1)$$

so that there is a coordinate system where it has the form $g_{00}dx^0 + g_{ij}dx^{ij}$, with coordinate *0* being that of R and i, j roaming over N.

Condition 1 excludes exotic (Gompf and Stipsicz, 1999; Scorpan, 2005) spacetimes, and Condition 2 essentially means that there is a trivial foliation of M "parallel" to R which behaves as the global time coordinate. So, we can reasonably talk about, say, the universe having begun 14 billion years ago, if our universe has a global time coordinate, or global time for short.

We say that a spacetime has the "cosmic time property" if it exhibits a global time coordinate. From here on we suppose that Zermelo – Fraenkel set theory is consistent. Moreover, if required, we suppose that it has a model with standard arithmetic.

20.1.4 The ZFC Set of All Spacetimes

This is a side remark, but how do we make precise the ZFC set of all spacetimes?

- A (topological or differentiable) manifold is described by coordinate domains and transition functions. If the manifold is noncompact, there are denumerable many such domains.
- So, we can code each manifold (in many different ways) by a real number.
- We can therefore define a 1–1 function from the reals to the manifolds (see (da Costa et al., 1990) on that function).
- Use the Axiom of Replacement to define the set of all manifolds out of that function.

20.2 Exoticisms

We are interested in 4–dimensional real differentiable manifolds as those are the arena where the game of general relativity is played. The situation is, however, extremely complicated due to the peculiarities of the geometry of 4–dimensional manifolds.

20.2.1 A Very Brief Introduction to Smooth Exotic 4–Manifolds

Let's start from topological manifolds.

- Consider a topological real n–dimensional manifold, that is, a separable metrizable space endowed with a maximal atlas that makes it locally like R^n.
- If it admits a differentiable maximal atlas, then it can be endowed with a differentiable structure.
- The number of differentiable structures may be > 1 modulo diffeomorphisms.
- In that case, if there is some atlas that may be taken as a standard differentiable structure (say, like the usual structures for R^4 or S^7), we say that the remaining differentiable structures are *exotic* (Scorpan, 2005).

The next summary comes from several sources (Asselmeyer – Maluga and Brans, 2007; Gompf and Stipsicz, 1999; Scorpan, 2005). Below there is a list of concepts and results that we require here:

- Given a smooth manifold, its possible submanifolds determine the manifold. Given a closed differential 1–form α^*, its (local) integral gives a parametric family of submanifolds of our manifold (the family is parametrized by the integration constant).
  That idea can be generalized to encompass higher – order forms.
- The *intersection form* arises out of the possible submanifolds of a given manifold in a way that we are going to specify. Restrict the attention to 2–forms on four manifolds. These forms can be seen to determine submanifolds of the 4–manifold M, as explained above (see also (Scorpan, 2005), p. 115 ff). Then we define the intersection form as:

$$Q_M(\alpha^*, \beta^*) = \int_M \alpha^* \wedge \beta^*.$$

  The intersection form arises out of elements (α^*, β^*) of the second DeRham cohomology group $H^2(M; \mathsf{R})$ for manifold M.

  One usually says that the solutions for the Einstein equations "determine the geometry of spacetime." That's not correct. The fact that one can use DeRham cohomology to handle intersection forms (Scorpan, 2005), together with the fact that mesonic and electromagnetic test fields over spacetime can be used to characterize its DeRham cohomology provides another link between the geometric structure of spacetime and the physics one does over it (Doria and Abrahão, 1978).

  α^* and β^* as above are 2–forms over the manifold M, which can be interpreted as mesonic test fields, or even electromagnetic test fields over spacetime M. So, these fields are the ones whose classes determine the global structure of a spacetime.
- So, we can say that given an intersection form, there is a (topological) manifold that corresponds to that form. And if we classify intersection forms, we get a classification for manifolds.
- More precisely we have *Freedman's Classification Theorem*: for any integral symmetric unimodular form Q there is a closed simply–connected topological 4–manifold that has Q as its intersection form.

  – If Q is even, there is exactly one such manifold.
  – If Q is odd, there are exactly two such manifolds, at least one of which does not admit any smooth structure.

- Follows the very interesting result: the odd intersection form noted [+1] (see the references) represents projective space CP^2. It must also represent "fake CP^2," a nonsmoothable 4–manifold which is homotopy equivalent to CP^2, as both share the same form [+1].
- *Donaldson's Theorem.* Another fundamental result in this domain is due to S. K. Donaldson, who proved it in 1982: The bilinear symmetric unimodular forms $\oplus m[+1]$ and $\oplus m[-1]$ are the only definite forms that can be realized as intersection forms of a smooth 4–manifold.

- Notice that this and similar partial results for indefinite forms give the global topological structure of possible spacetimes, which can very precisely be said to arise out of the spacetime's intersection form.

The result that interests us here is:

Proposition 2.1 *There is an exotic R^4 with a compact set* C *so that no smooth embedded* S^3 *encloses* C. □

For the proof see (Scorpan, 2005), p. 250. It is one of the two main tools required to prove Taubes' Theorem:

Proposition 2.2 *There are uncountably many non – diffeomorphic exotic $E\,R^4$s.* □

We will actually require one of the consequences of Taubes' Theorem:

Proposition 2.3 *If ER^4 is an exotic R^4 and* h *as below is an homeomorphism:*

$$h : R^4 \to ER^4$$

then given an open ball $D(\rho) \subset R^4$ of radius ρ, there is a value ρ_0 so that for a compact set $C \subset ER^4$, for no $\rho > \rho_0$ does a smooth image $h(D(\rho))$ encloses C. □

(It is actually a consequence of the result we gave above.)

20.3 Conjectures, Speculations, More Counterintuitive Results

Recall that ER^4 is an exotic 4–plane. We first state:

Proposition 3.1 *No ER^4 with the property spelled out in Proposition 2.1 has a global time–coordinate.*

Proof: If it had such a coordinate, then it would be diffeomorphic to $\mathrm{R}^3 \times \mathrm{R}$, which is impossible, since no R^3 has an exotic differential structure. □

However it is *homeomorphic* to $\mathrm{R}^3 \times \mathrm{R}$. This means: there is a global, albeit sometimes nondifferentiable global time – coordinate. But we have that *the global time coordinate, if it exists, must be differentiable.*

Corollary 3.2 *For the family $ER^4(\rho)$, absence of a global time structure is generic in the topological and measure–theoretic senses.*

Proof: Immediate: from the map $\rho \in (\rho_0, \infty) \mapsto \mathrm{ER}^4(\rho)$ one can induce the corresponding concepts of genericity' etc. in the space of all those manifolds. Since there is just one standard R^4, the set of all such exotic 4–planes will be generic in the (induced) senses. □

Now, for set – theoretic genericity (we require the axiomarization of general relativity here):

Proposition 3.3 *For* B *an adequate complete Boolean algebra, for* $\mathbf{L} \models$ *ZFC, being Gödel's constructive universe, for* $\rho \in \mathbf{L}$ *a real number so that* $\mathbf{L}^B \models \rho > \hat{\rho}_0$*, then* ρ *can be chosen a set–theoretically generic real number so that* $\mathbf{L}^B \models \mathit{ER}^4(\rho)$. □

That $\mathsf{ER}^4(\rho)$ is a *set – theoretically generic exotic spacetime*. There are other examples of similar beasties. The next result is given rather loosely:

Proposition 3.4 *Set theoretic genericity doesn't imply absence of global time coordinate.*

Sketch of proof: For adequate forcing extensions $\mathbf{V}^B$ there are set – theoretically generic noncompact differentiable 3–manifolds (da Costa et al., 1990), and given one such, noted M, $M \times R$ is a generic differentiable 4–manifold in the same forcing extension. □

20.3.1 Set Theory with Martin's Axiom

For a review of Martin's Axiom see (Kuner, 1983). Roughly speaking, Martin's Axiom acts as a "regularizing tool," that is, the sets that should be of zero measure, or of first category, or both, can be proved to be so given Martin's Axiom.

Proposition 3.5 *If model* $\mathbf{M}_{\mathrm{MA}}$ *is such that it makes true the theory* $ZFC + \neg CH + MA$ *then* $\mathbf{M}_{\mathrm{MA}}$ *makes true the formal version of the sentence "every constructible subset of the reals is a first – category set and a zero – Lebesgue – measure set."* □

CH is the Continuum Hypothesis, and MA is Martin's Axiom. We will use that result in what follows.

20.3.2 Category and Measure

We now go back to the question: which is the typical situation in Nature? Global time or its absence? What can we make out of the fact that there will be spacetimes so that we have no decision procedure to ascertain whether they have local or global time? How frequent is that situation?

20.3.3 Results About the Nongenericity of Global Time

We again deal here with topological and measure – theoretic genericity. Some results that suggest that global time isn't generic in the sense of topology or measure follow from Theorem 9.4.24 and Corollary 9.4.25 in Gompf and Stipsicz (Gompf and Stipsicz, 1999, p. 378 s). Define a topologically cylindrical spacetime to be homeomorphic to $S^3 \times \mathsf{R}$. Then:

Proposition 3.6 *For a reasonable topology and measure, there is a generic set of spacetimes homeomorphic to a cylinder* $C \times R$ *which do not have a global time coordinate.*

Proof: It is again immediate: there are $2^{\aleph_0}$ many non – diffeomorphically – equivalent, diverse, structures which are smooth for those spacetimes. Code each one by a binary irrational in some possible way and induce measure and category from the pullback map. The set of exotic topologically cylindrical spacetimes is of measure 1 and of the second category. □

A second, more general result, goes as follows. Consider the set of all connected topological real 4–manifolds and pick up those that admit a smooth structure; factor them out by homeomorphisms. We then have a set of nonequivalent (modulo homeomorphisms) topological real 4–manifolds which can be given a smooth atlas.

Code them (via the function that maps spacetimes over some set of cardinality $2^{\aleph_0}$ onto, say, the binary irrationals.

Call that lebinary irrational λ; choose a particular smooth structure for it and call the resulting differentiable manifold X_λ.

From the above quoted result (see the reference) we have that $X_\lambda - \{*\}$, where $\{*\}$ is a point, has uncountably many nonequivalent differentiable structures. Then form the set of all pairs $\langle X_\lambda, E_\mu(X_\lambda - \{*\})\rangle$, where $E_\mu(\ldots)$ represents the exotic structure denoted by μ; that set is coded by the λ, μ. In the induced topology and measure the set of exotic spacetimes is both set–theoretically and measure–theoretically generic.

We can picture that construction as follows: over each "point" X_λ there is a "fiber" $E_\mu(X_\lambda - \{*\})$ to which we add (we code) all extra differentiable structures for X_λ, if any.

If Y denotes that space:

Proposition 3.7 *The set* Y *of spacetimes without a global time coordinate is set – theoretically and measure – theoretically generic in the above – described topology and measure.* □

Follows:

Proposition 3.8 *Spacetimes without global time are set – theoretically and measure – theoretically generic in the above described topology and measure.*

Proof: Follows from the fact that spacetimes with global time must have a standard structure. □

20.3.4 Martin's Axiom Again

Follows from Propositions 3.5 and 3.8 that:

Proposition 3.9 *Model* $\mathbf{M}_{\mathrm{MA}}$ *makes true the formal version of the sentence "Given the above topologies and measures, the set of exotic set – theoretically generic spacetimes has measure* 1 *and is of second category."* □

So, if our spacetimes are to be found in a – mathematical – universe where the Continuum Hypothesis doesn't hold and where Martin's Axiom is true, then (loosely speaking) the typical spacetime is a chimaera – like object; it is exotic and set – theoretically generic, and obviously without a global time coordinate.

20.4 Can We Decide Whether an Arbitrary Spacetime Has a Global Time Coordinate?

The answer to that query is, no:

Proposition 4.1 *There is a family g_n of metric tensors for a spacetime M so that:*

1. *There is no algorithm to decide, in the general case, whether g_n, for each n, has the cosmic time property.*
2. *The decision problem for that question may be as difficult as one wishes in the arithmetic hierarchy.* □

Proposition 4.2 *Given any axiomatization for general relativity within ZFC, there is a metric tensor g over R^4 with the usual differential structure so that:*

1. *ZFC $\nvdash$ g has global time. If h is Gödel's metric tensor, then $g = h$ holds of all models for ZFC with standard arithmetic.*
2. *ZFC $\nvdash$ g doesn't have global time. If η is Minkowski's tensor, then $g = \eta$ will hold of some models with nonstandard arithmetic and of no model with standard arithmetic, for ZFC.*
3. *To sum it up: for any model with standard arithmetic **N** for ZFC, **N** $\models$ g doesn't have global time.* □

Proposition 4.3 *Given any axiomatization for general relativity within ZFC, there is a metric tensor g over R^4 with the usual differential structure so that:*

1. *ZFC $\nvdash$ g has global time.*
2. *ZFC $\nvdash$ g doesn't have global time.*
3. ***L** $\models$ g doesn't have global time. Here **L** is Gödel's constructive universe.* □

We can obtain an undecidability result as in the previous results. About the preceding result: there will be models with standard arithmetic for both sentences in the undecidable pair we have considered.

20.5 Conclusion

We may summarize our conclusions as follows:

> Spacetime may well be a cylinder $S^3 \times \mathsf{R}$ with the standard topology and differentiable structure, and with global time. However that very specific geometry doesn't follow from the Einstein gravitational equations, and is in fact very far from what a typical spacetime should look like: an exotic, set–theoretically generic 4–manifold, endowed with a very complicated time structure.

We have here two sorts of results:

- *Category and measure.* We have exhibited results about topological and measure – theoretic genericity of the non – existence of a global time coordinate.
- *Undecidability and incompleteness.* There is no general algorithm to decide whether an arbitrary spacetime exhibits the cosmic time property (whether it has a global time coordinate). And there are formal sentences that translate as "spacetime X has the cosmic time property," which can neither be proved nor disproved in, say, ZFC.

The question is: can we take our arguments here as arguments that give a "natural" zero probability for the existence of global time? How are we to interprete the preceding results? Does our result on the genericity of spacetimes without a global time coordinate reflect the actual situation in the real world? In the world of possible spacetimes? Is our probability evaluation a "physical world" probability? Even if it includes a wide range of conceivable measure attributions?

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Chapter 21
A Contemporary View of Population Genetics in Evolution

João Carlos M. Magalhães and Cedric Gondro

21.1 Introduction

Many authors (Sorber, 1993) highlight two key hypotheses in Darwin's work: (i) that evolution does occur, meaning that organisms descend with modifications from common ancestors; (ii) the main driver of evolution is natural selection. A greater emphasis given to either one or the other of these theories resulted in two different views on evolution: comparative biology and classic population genetics.

It is worthwhile to mention that descent with modifications does not necessarily imply natural selection since other causes for evolution can rather be construed. On the other hand one cannot think of natural selection in the absence of descent with modifications since variation and inheritance are inherent principles of the theory.

Whilst comparative biology (through e.g. morphology, biogeography and palaeontology) disciplines focused mainly on the reconstruction of the history of life, population genetics adopted a hypothetical deductive method to try to understand evolutionary processes and get a handle on the biological *laws* that govern these processes (cf. Rosemberg, 1985).

21.2 The Synthetic Theory of Evolution

Around 1900 evolution was already quite widely accepted. The same cannot be said for natural selection (Bowler, 1985), mainly due to the lack of an adequate theory to explain inheritance in Darwin's work. The rediscovery of Mendel's work on heredity provided the mechanistic framework through which natural selection could be understood. Between the 1920s and 1940s genetics, especially population genetics, and Darwin's theories came together in the so called Synthetic Theory of Evolution, or Neodarwinism.

Population genetics is singular due to its use of formal mathematical approaches and the search for causal explanations, quite distinct from the rest of evolutionary

J.C.M. Magalhães (✉)
Department of Genetics, Federal University of Parana, Curitiba, Brazil
e-mail: jcmm@ufpr.br

D. Krause, A. Videira (eds.), *Brazilian Studies in Philosophy and History of Science*, Boston Studies in the Philosophy of Science 290, DOI 10.1007/978-90-481-9422-3_21,

biology which tends to be more descriptive and, at least in principle, less theoretical. Maybe this is why population genetics is sometimes considered the main evolutionary discipline, but of course this can be rather controversial (e.g. Moya, 1989). A recent appraisal of the field can be found in Stephens (2008).

Population genetics theory deals mainly with the construction of mathematical models that try to explain the distribution and predict the dynamics of biological variation at a population level.[1] They are construed in such a way as to consider only those elements that are deemed relevant in a given context. For example, while studying an evolutionary phenomenon such as the effect of a variable on the distribution of allelic or genotypic frequencies, all other factors must be controlled. The variables related to these other factors are taken as fixed parameters, allowing for linear models that are mathematically more tractable. It should be noted that given a choice between mathematical rigour and approximate but still biologically meaningful solutions, it is common to go down the second path. As Crow and Kimura (1970: p. 3) pointed out "we have to choose some sort of compromise between a model that is so crude as to be unrealistic or misleading and one that is incomprehensive or too complex to handle".

The most basic and well known model in population genetics is the Hardy-Weinberg "law", independently proposed by G. H. Hardy and W. Weinberg in 1908. It describes the relationship between genotypic frequencies and allelic frequencies and how they remain constant across generations (hence also referred to as Hardy-Weinberg equilibrium) in a population of diploid sexually reproducing organisms under the assumptions of random mating, an infinitely large population and other assumptions. This "law" is, in fact, an informal theorem that depicts the mathematical consequences of Mendelian inheritance at a population level. Rigorously the theorem can be proven within a system that specifies the mechanisms of inheritance and all the other causal modulators (for formal approaches and discussion see Lloyd, 1984; Lloyde 1994; Magalhaes and Krause 2001; Lorenzano, 2008).

Naturally real populations will not strictly adhere to the assumptions for Hardy-Weinberg equilibrium, but the model is however quite robust to deviations. When empirical observations are in a statistical sense significantly different from the model's predictions, there is a strong indication that some biologically relevant factor is acting on this population. For these cases new constraints are added and new models are developed to try to explain the underlying mechanisms that triggered the observations.

One such scenario is inbreeding, which arises when matings occur between organisms that are genetically related. This results in a lower frequency of heterozygotes than would be expected under the assumptions of random mating. Naively, inbreeding is a function of the population size but the underlying causes are much

[1] The term model usually means a simplified representation of a system that is being studied whilst trying to capture its key aspects. Population genetics deals primarily with mathematical models – highly abstract systems which, at least in principle, operate in the same manner as biological populations (for a discussion, see Magalhaes and Krause, 2001; Magalhaes and Krause 2006).

harder to tease out of the system, e.g. biological factors such as reproductive mechanisms and dispersal mechanisms or environmental factors such as geographic distances or physical barriers. These deviations of frequencies from equilibrium in conjunction with other parameters can be used to estimate, for example, the level of divergence and/or variability within and between populations.

As the synthetic theory crystallized, evolution began to be viewed as shifts in allelic frequencies caused by *forces* analogous to the notion of force in Newtonian physics (Rosemberg, 1985). These forces, or better, evolution factors, are mutation, migration, drift and selection. According to Wright (see Freire-Maia, 1988), recurring mutation, migration and selection are systematic pressures exerted on populations and as such, at least in principle, quantifiable. Meaning that given the current allelic frequencies and some other population parameters, the future states can be predicted. Drift on the other hand is stochastic and thus unpredictable. Random fluctuations in mutation, migration and selection rates as well as unique events (e.g. drastic environmental changes, formation of geographic barriers) will also impact on the genetic structure of populations in non deterministic ways. Herein we will briefly address drift and selection.

Drift is simply a random fluctuation in the allele frequencies of populations due to sampling of the gametes that will contribute to form the next generation round. It is an important factor for divergence between populations. The intensity with which drift will affect a population is an inverse function of the number of individuals in this population such that the variance of change in allelic frequency is $\sigma^2_{\Delta q} = \frac{q(1-q)}{2N}$ where q is the allelic frequency and N is the number of individuals in the population.

Not all individuals in a population have the same chance to reproduce, so the N in the above equation in reality is a new parameter *effective population size* (N_e) that is the adjusted number of individuals in an idealized population following Wright-Fisher's model,[2] which would have the same genetic drift as is observed in the actual population. This parameter will allow quantification of the intensity of drift and inbreeding since both factors are related.[3]

Selection is still by far the most discussed evolution factor in the literature since it is directly related to the phenomenon of adaptation of organisms to their environment. In population genetics it is commonly thought of as the differential reproduction of individuals as a function of their genotypes. Here the key parameter is *fitness* or adaptive value of a genotype (Backer, 2009 provides a current discussion on this rather controversial concept). In general, for each genotype represented in the population there is an associated adaptive value (fitness), usually represented by

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[2]This model of genetic drift was suggested by Wright in 1930 and Fisher in 1931. It assumes an infinite number of populations of the same size with the same initial allelic frequencies. The distribution of the allelic frequencies across generations can be modeled probabilistically. Kimura in the 1970s studied this stochastic process through the introduction of Komolgorov's diffusion equations (see Crow and Kimura, 1970).

[3]The rate of loss of heterozygosity in finite populations $\Delta F = \frac{1}{2N}$ equals the variance of Δq provided the population is mating at random. Under these conditions, drift and inbreeding are equivalent in terms of their effect on the genotypic frequencies.

w, which *determines* the probability of survival and reproduction of that genotype in relation to the others.

In a rather unrealistic model, given initial allelic frequencies and the values of w are defined and constant, meaning that they do not change from generation to generation, we can estimate the rate of change in allelic frequencies due to selection as $\Delta_q = \frac{q(1-q)}{2\overline{W}} \cdot \frac{d\overline{W}}{dq}$ where q is the initial allelic frequency and $\overline{W}$ is the weighted mean of the adaptive values of the individuals within a population.

With these conditions we can examine the consequences of selection under different scenarios. Consider that one of the homozygous genotypes has a higher adaptive value than the others; the frequency of its allele will increase until all others are removed and this variant becomes fixed. This is an example of directional selection. If instead, the highest adaptive value resides with a heterozygous genotype, its alleles will stabilize at an equilibrium frequency in the population, which is one of the explanations to account for the existence of polymorphisms (allelic variability) in populations. Another possibility is selection against the heterozygote which will eventually lead to fixation of one allele or other depending on the values of w and the initial allelic frequencies. In some cases even minor variations in these frequencies can lead to divergent evolutionary trajectories (Lewontin, 1974).

Of course selection is much more complicated. Genes are physically linked to one another on chromosomes and the adaptive values of the alleles at one locus are neither necessarily constant nor independent of other loci. The highly hypothetic nature of selection theory was emphasized by Wright: "... each selection coefficient is a complicate function of the entire system of gene frequencies and can only be dealt with qualitatively" (Wright, 1931: p. 245). The dynamics of selection can lead to several different final states and the evolution of the system can be over dependent on the initial conditions, virtually leading to indetermination: "this existence of many stable points, even for two loci, means that historical accidents can play a large role in determining the actual genetic configuration of a population, making it all the more difficult to distinguish selective from random events" (Lewontin, 1985: p. 92). Even simple computational simulations can evolve such complex results as to become intractable with just a few non constant parameters (Gondro and Magalhaes, 2005).

21.3 Reconstruction of the Evolutionary Past

Description, classification and ordering of entities from the living world, focus of natural history studies in the classic period, aimed to name and describe living creatures such as they were seen in nature. This kind of classification was supposed to be an objective description of natural facts, consequently there seemed to be no need for an underlying theory. The Darwinian notion of descent with modifications led to a radical shift in paradigms: the set of living entities could be organized hierarchically in tree like structures (dendrograms) with the terminal branches representing breeds, species or higher order groups (a recent discussion is given in Ereshefst, 2008).

Without any details, the basic idea is: evolution is descent with modifications, species and other groups (clades) evolve from an ancestral species due to the accumulation of modifications between geographically isolated groups (for argument sake). This event is called cladogenesis, a historic event; the greater the difference between groups, the longer ago the event occurred and the further away the common ancestor would be.

Classic population genetics applied comparative methods mainly in the simple description of genetic variability within and between populations through the use of allelic, genotypic or haplotypic[4] frequencies or measures of *genetic distances* between groups, which are also based on these frequencies.

21.4 Molecular Biology and Its Impact on Population Genetics

During the first half of the last century genetic variability could almost only be accessed through phenotypic expression. Phenotypes were considered a window to the genotype (Moss, 2003) but they did not allow capturing the complexity of genetic interactions and consequently there was a very weak handle on the evolutionary processes themselves. The truth is that the relative rarity of polymorphisms was mainly due to the lack of tools to detect the genetic variants that were not phenotypically expressed, and not due to selection weeding out lesser adapted forms.

Technical developments, especially protein electrophoresis, opened the door to expose the high levels of variability present in populations. Subsequently molecular (DNA) techniques evolved at vertiginous rates culminating in full sequences of entire genomes. With data sources growing at exponential rates new statistical and computational methods had to be devised to handle them, and more recently an entire new field of research emerged: Bioinformatics.

The newfound variability in proteins led to the question of how to reconcile the high levels of polymorphims being detected with the theory of natural selection. The term genetic load was defined by Muller in 1950 as the difference between the mean adaptive value of a population and the adaptive value of the fittest genotype. Thus, if a high degree of polymorphisms are maintained by selection in natural populations then clearly the mean adaptive value of the population will be low, with a consequently high genetic load. This way, many zygotes or young individuals would be lost in one way or another. So clearly there must be some limit to the genetic load within a population. Beyond this limit selection would not be able to sustain additional variation since in principle the population would show a negative growth trend which would eventually lead to extinction.

The neutral theory of molecular evolution or neutralism combines population genetics with molecular evolution. Originally proposed by Kimura in the 1960s, the

[4]A haplotype is a combination of certain alleles of two or more loci linked on the same chromosome.

theory has been modified and extended by various authors in the following decades (a historical perspective is given in Ohta and Gillespie, 1996). According to the neutral theory, polymorphisms at the molecular level are in general selectively neutral with new variants constantly being introduced by mutation whereas others are lost through drift. The rate of allelic substitution (λ) would depend only on the rate of mutation (μ).[5] The level of variability within a population would be directly proportional to its size (N_e). The average time length between the appearance of an allele and its loss or fixation through drift is also proportional to N_e. Since at the molecular level the number of polymorphisms is extremely large, it was interpreted as evidence for neutralism.

According to Kimura's theory, at a molecular level drift and mutation would be the key agents driving change. In general the role of selection would be to act as a purification agent by removing mutations that compromised functional structures (e.g. a change in nucleotide sequence altering the amino acids in a protein and destroying its function). This led to the notion of non-darwinian evolution (King and Jukes, 1969) and a heated debate between selectionists and neutralists. The quantitative and mathematical natures of the neutral theory made it amenable to experimental testing and for over a decade experimentalists tried to either prove or disprove it.

A key observation that came to support the theory was that protein regions that were functionally critical exhibited a significantly lower rate of evolution than regions functionally less important. The same was later found to be true for genomic regions as well and became known as the principle of molecular evolution (Ohta and Gillespie, 1996). This can be illustrated by looking at the rates of change of microsatellites in non coding regions which show extremely high levels of polymorphism even in relatively small populations. This is in stark contrast to some developmental genes which show high levels of conservation (similarity due to common inheritance) not only within a species but across an entire taxonomic phylum.

In the 1970s work by Ohta (1973), a student of Kimura and eventually Kimura (1981) himself helped bring the two views together by showing that most new mutations under stabilizing selection at a protein level would exhibit such low levels of selection as to be effectively neutral. Thus the compounded effect of thousands of small effects would result in a phenotype which would be selected for or against, while the individual alleles would be changing mainly due to drift and mutation. This became known as the *nearly* neutral theory and became the generally accepted model.

An important consequence of this theory is that the probability of evolution by natural selection no longer depends solely on the adaptive value of the genetic variant but also on the effective population size. A gene can be neutral in small

[5]The probability that a new selectively neutral mutation eventually gets fixed in a population is equivalent to its frequency: $\frac{1}{2N_e}$ and the probability of a new mutation per generation is $2N_e\mu$, hence $\lambda = \frac{1}{2N_e} 2N_e\mu = \mu$

populations but be under selection in large populations, thus becoming *density dependent* (Ohta and Gillespie, 1996).

DNA sequencing and new analytical methods which started to take off in the 1980s led to new approaches to empirically test the theory which in turn led to practical applications of the theory. The level of variation expected under the neutral model can be used to estimate the parameter $\theta = 4N_e\mu$ either based on the rate of homozygotes or the number of segregating sites, that is, genomic locations that show different nucleotides (or different amino acids in the case of proteins) or different numbers of alleles. Thus effective population size, population structure, endogamy, migration and various other phenomena of biological interest can be investigated. Of course different data structures and assumptions might be necessary.

Selection can be studied by comparisons of the frequency of synonymous and non-synonymous nucleotide substitutions or by the extent of polymorphisms in different loci between and within species. These and other statistical methods can expose evidence of previous selection, even though it is not possible to quantify an adaptive value as an inherent property of a given genotype (a review on the topic is given in Banchad and Wooding, 2003).

21.5 Integration of Population Genetics and Phylogenetics

With the advances in molecular biology even the concept of allele changed. A gene began to be viewed in terms of structure; that is a certain sequence of base pairs (ATTGC...) while alleles were variations of this structure arising through mutations. Nonetheless, regardless of how we define a gene, a mutation is still a change to the physical structure of the nucleotides. This can be due to a replacement of one nucleotide by another or through the loss or insertion (indels) of nucleotides in a DNA sequence. The probability that independent mutation events will originate the exact same sequence is very low, to the point that each mutation can be deemed to originate a unique allele, distinct from all others. Under the *infinite alleles* model genetic variation is proportional to the population size. Assuming neutrality, alleles would appear and replace previous ones at a relatively constant rate. If we take two alleles or simply two homologous sequences of DNA that share a common origin, the level of differentiation between them (e.g. number of different bases) is directly proportional to the length of time of divergence. Molecular data allows this to be tested empirically, at least in some DNA regions and in some taxonomic groups. Using this notion, molecular phylogenies can be constructed and even the time length of divergence can be estimated between groups, a *molecular clock* to evolutionary history.[6] In general phylogenies based on molecular data, particularly

[6]To infer timelines the molecular clock needs to be anchored or calibrated. This is achieved by use of independent data e.g. fossil records, geological data. An out group is also important to help anchor the tree, this group is usually distantly related to the groups being compared but still sufficiently close to allow establishing a timeline.

those using numbers of sequences, seem to be more reliable than those based on morphological data.

A new angle being explored through population genetics has emerged from adoption of the coalescent theory developed in the 1980s by Kingman (1982). Herein we will very briefly address a few interesting aspects of the theory, an excellent and approachable overview is given by Nordborg (2001). The coalescent puts together aspects of genetics and phylogenetics, meaning that it adds time as an extra dimension to population genetics and, consequently, a historical perspective. The key concept is to trace genetic genealogies from samples of individuals in a population and from these infer genetic parameters.

Consider two alleles taken from the population's gene pool. The probability that both descend from a common ancestor allele, that is that they *coalesce* in a previous generation t can be estimated based on a simplified population model (Wright-Fisher model). Each pair of alleles coalesces to a single ancestral copy the MRCA (most recent common ancestor) and all alleles coalesce to a single MRCA in the distant past. An elementary model of coalescence assumes selective neutrality, no population structure, no gene flow and no recombination but it can be extended to include the parameters relevant to a specific population study.

Coalescent theory allows testing different population assumptions to try to model a wide range of scenarios to explain the historical evolution of populations across time, for example natural selection, fluctuations in population size or migration patterns. To illustrate, consider that a population is sampled and genotyped. The genotypes are used to reconstruct the genealogy of the sampled population along the lines of a phylogenetic analysis. Over this predicted genealogy neutral mutations are added based on a probabilistic distribution. This is repeated thousands of times generating a distribution of the data. It could be done for various different genetic models, or in other words, using different assumptions about the evolutionary history of the population. The simulated series of data can then be compared to the observed data to make inferences about the current population structure and how this population has evolved, and used to obtain estimates of genetic and demographic parameters.

The theory can also have useful practical applications such as help model the dynamics of fisheries to avoid overexploitation by providing a handle on effective population sizes. Coalescent inspired approaches can also be used in genome wide association studies to infer relatedness between haplotypic blocks.

Recently a new subject has emerged from the integration of molecular genetics with biogeography: phylogeography. According to its main proponent John Avise "a relatively new discipline termed phylogeography has enriched biogeographic analyses and provided an empirical and conceptual bridge between the formerly independent fields of traditional population genetics and phylogenetic biology" (Avise, 2004: pp. 319–320).

21.6 Concluding Remarks

A general trend is underway to change the perspective in population genetics bringing it closer to other branches of evolutionary biology. Moving away from the study of highly idealized and *prospective* models in the classic phase, modern population genetics has become *retrospective*, focussing on reconstruction of the evolutionary history of genetic lines within species, in this way being closer to the tradition of comparative biology. To a great extent this became feasible due to the coalescent theory, which in turn links back to Kimura's neutralism.

As the Roman god Janus, research in molecular evolution and population genetics has two faces, representing the course of history.

When looking into the future, population genetics allows relatively accurate predictions of only some phenomena and only within a rather short time interval. Setting aside the difficulties to realistically determine parameters such as adaptive value (fitness) or effective population size, the theoretical models in general make many assumptions to contextualize a given scenario. Seldom conditionals such as "if and only if A_1 and A_2 and... A_n occur, then B will also occur" can be clearly established. Even in these cases, if B is observable and does not occur in the presence of all the other conditions or if it does occur in the absence of some of the conditions then the assumptions have to be revisited, but seldom can a single factor be sufficiently isolated for conclusive testing. Due to the historical and unique nature of evolutionary phenomena there are no repeatable *experiments,* except in overly idealized laboratory scenarios or through *in silico* simulations.

When examining the past, the search is for singular events that were sufficiently important to leave trails in the DNA registry. In general several theoretic models and different methods are used to tackle the issues of the populations being scrutinized trying to build a coherent story about the origin of the observed patterns. The consistency of the picture that emerges from these different approaches is what matters and not the individual results from each one.

In summary, the research tools currently used in molecular population genetics can yield knowledge which has at best a reasonable degree of confidence, not certainties. This is because the stochastic nature of biological processes and the sequence of events that determine the trajectory of evolution in populations and the genetic lineages within them cannot be re-established except in a hypothetical and approximate form. Regardless of this, the degree of methodological sophistication, the coherence of explanations with known facts and especially the practical outcomes achieved by this field of study are outstanding.

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Chapter 22
Continuity and Change: Charting David Bohm's Evolving Ideas on Quantum Mechanics

Olival Freire Jr.

22.1 Introduction[1]

"It is too bad, very sad indeed, that he did not live to see how his reputation has shot up recently. His interpretation of quantum mechanics is becoming respected not only by philosophers of science but also by 'straight' physicists." The words of the American physicist Melba Phillips, a long-standing friend of David Bohm (1917–1992), demonstrate yet another case of posthumous recognition in science.[2] In fact since the 1990s Bohm's first proposal for an interpretation of quantum mechanics (Bohm, 1952a), now labeled "Bohmian mechanics", has enjoyed a larger audience than his original proposal got in the early 1950s. A sign of the late prestige accorded to Bohm and to the field he mostly worked in is the volume in honor of the centenary edition of *Physical Review,* the most influential American physics journal. It includes commentaries and reprints from the most important papers ever published in this periodical. In the chapter on "Quantum Mechanics", edited by Sheldon Goldstein and Joel Lebowitz, all the papers including Bohm's 1952 paper on the causal interpretation concern foundations of quantum mechanics and a photo of Bohm opens the chapter (Freire, 2005). However, Bohm's current prestige was not totally unexpected. An inspection of the Festschrift honoring his 70th birthday reveals that in life Bohm received tributes from scientist such as Ilya Prigogine, Maurice Wilkins, and Richard Feynman, all Nobel Prizes at the time of this book appeared, Anthony Leggett, who would go on to win the 2003 Physics Nobel Prize, John Bell, Roger Penrose, David Pines, Bernard d'Espagnat, Jean-Pierre Vigier, in addition to a number of Bohm's collaborators (Hiley and Peat, 1987), and the ultimate accolade was to be elected Fellow of the Royal Society in 1990.

David Bohm was a thinker whose influence went well beyond that of the field of "straight" physics. Neurophysiology, biology, and psychology are some of the

O. Freire Jr. (✉)
Instituto de Fisica-UFBa-Brazil
e-mail: freirejr@ufba.br

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[2] Melba Phillips to David Peat, 17 Oct 1994, David Bohm Papers, A22, Birkbeck College, London.

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fields where traces of Bohm's influence can be found. His sphere of influence grew from the 1980s on and he became a cultural icon as a consequence of his contact with eastern thinkers, such as Jiddu Krishnamurti and the Dalai Lama, and his search for a dialogue among science and religion and mysticism. All this influence is claimed to be based on David Bohm's work on the foundations of quantum mechanics. However, Bohm's thoughts on this subject changed meaningfully over the course of the four decades he worked on this and it has been hard to identify which part or stage of his thinking is being considered when his ideas are invoked by his readers. An early example of this was Fritjof Capra and his best seller *The Tao of Physics* (Capra, 1991), where Bohm's ideas on order in quantum theory were presented while Bohm's previous ideas on a causal interpretation of the same theory were ignored. Bohm did not help his readers to make sense of the evolution of his thoughts and in the most widely influential of his books, *Wholeness and the implicate order* (Bohm, 1980), he conflated different stages of his interpretation of quantum mechanics. Even in a paper showing the connections between two of his most important approaches to quantum mechanics, when "asked to explain how [his] ideas of hidden variables tie up with those on the implicate order" he emphasized the continuity more than his change of emphasis (Bohm, 1987).

This paper thus intends to chart the evolution of Bohm's ideas on the interpretation of quantum mechanics dealing with both the elements of continuity and change. Continuity in his thoughts is mainly related to his reflections on realism in physics and attempts to depict the kind of world quantum physics is intended to describe. From the search for a "quantum worldview," a chapter of his 1951 *Quantum Theory* textbook, to the presentation of *The Undivided Universe* as "an ontological interpretation of quantum theory," Bohm kept ontology as the philosophical goal of his investigations. The main changes were related to the role of causality, differences in scientific styles, and the creation of new concepts. Bohm indeed abandoned the quest for a causal interpretation of quantum mechanics moving to give both deterministic and probabilistic laws the same philosophical status. Bohm also moved from the construction of physical models able to reproduce quantum mechanical predictions to attempts to mathematize a few foundational concepts such as order and ultimately to build new physical theories with quantum theory as their limits. It is beyond the scope of this paper to discuss the historical contexts which led him from one stage to another in detail. Instead, I will only review the growing relevant literature. This paper is organized as follows: Section 22.2 is devoted to his early reflections on quantum theory as expressed in his 1951 *Quantum Theory* textbook, but it also deals with Bohm's causal interpretation, including its reception among physicists and its developments. Section 22.3 covers a period beginning in the late 1950s when he abandoned his causal interpretation to the early 1980s, when research to mathematize the insight of implicate and explicate orders matured. Section 22.4 deals with Bohm's thoughts at a later stage, when parts of the causal interpretation were revived, wearing different philosophical clothes, and overlapped with research on the mathematization of the idea of order, eventually leading to the concept of "active information." The fifth and final section is devoted to the legacy of Bohm's ideas, which includes both the research program called "Bohmian mechanics" and the continuing quest for the mathematization of order by Basil Hiley, a longstanding collaborator of Bohm.

22.2 Shifting to a Causal Quantum Mechanics

From the philosophical point of view, Bohm's (1951) *Quantum Theory* is remarkable for its attempt to combine Niels Bohr's complementarity with Bohm's own kind of realism. The former denied quantum theory the ambition of describing a world independent of measurements, while the latter included an ontological description of the quantum world, referred to by Bohm as "an attempt to build a physical picture of the quantum nature of matter." Commitment to an ontology for the quantum phenomena was to be a lasting philosophical feature of Bohm's approach to quantum mechanics. The book is also noteworthy for his conceptual clarity and a few innovations such as the reformulation of the EPR thought experiment using spin instead of position and momentum, which later became the standard formulation for theory and experiments about Bell's theorem due to its mathematical simplicity. Bohm also included a treatment of the measurement process using random phases.

No sooner was the book completed, Bohm was already dissatisfied with it. In a process yet to be well charted by historians, Bohm moved to a causal interpretation of quantum mechanics. Unlike Planck, Lorentz, Einstein, or the early critics to quantum mechanics, he did not express just a hope of going back to a causal description for atomic phenomena. In fact, he built a model for his approach assuming that an object like an electron is a particle with a well defined path, which means it has a simultaneously well defined position and momentum. In this model it suffers the physical influence both from potentials such as electromagnetic potential and a new potential resulting from the mathematical manipulations of Schrödinger equation, which Bohm labeled "quantum potential." These ideas were encapsulated in his 1952 paper titled "A suggested interpretation of the quantum theory in terms of 'hidden' variables." This model was very close to the pilot wave that Louis de Broglie had suggested in 1927 though did not pursue. Bohm was unaware of this but quickly learnt of Pauli's early criticisms to such a model. Bohm further developed his approach, the second part of the paper being a consequence of this. Thus, even a harsh critic like Pauli conceded that the approach was logically consistent while he did not accept it for epistemological reasons (Freire, 2005).

Bohm's 1952 paper had philosophical implications as a consequence of its own physical assumptions. According to Bohm (1952a: p. 166), his interpretation "provides a broader conceptual framework than the usual interpretation, because it makes possible a precise and continuous description of all processes, even at the atomic level." More explicitly, he stated that

> This alternative interpretation permits us to conceive of each individual system as being in a precisely definable state, whose changes with time are determined by definite laws, analogous to (but not identical with) the classical equations of motion. Quantum-mechanical probabilities are regarded (like their counterparts in classical statistical mechanics) as only a practical necessity and not as a manifestation of an inherent lack of complete determination in the properties of matter at the quantum level.

Bohm was so fully aware of the philosophical implications of his proposal that he concluded (pp. 188–9) by associating and criticizing the usual interpretation of quantum mechanics, that of complementarity, as following from the nineteenth century positivism and empiricism preached by Ernst Mach. Such philosophical implications concerned the adoption of a realist point of view toward physical theories and the recovery of determinism as a mode of description of physical phenomena, both discarded by the complementarity view. Later in his career, Bohm (1987: p. 33) emphasized that recovering determinism was not his main motivation and that his major dissatisfaction was that "the theory could not go beyond the phenomena or appearances." The building of an ontology to overcome appearances became a permanent goal in Bohm's research. Later, the priority he gave to determinism was relaxed but in the 1950s the debate triggered by Bohm's proposal did indeed privilege the recovery of determinism. Bohm and his collaborators had supported the emphasis on determinism by choosing "causal interpretation" as the label for their approach. Bohm did not use this term in the title of his initial 1952 papers but he used it in his subsequent paper, while reacting to the first criticisms (Bohm, 1952b). Since then both critics and supporters have emphasized the philosophically minded *causal interpretation* over the philosophically neutral while technically accurate *hidden variable interpretation*. To illustrate how attached to the philosophical priority for causality Bohm and collaborators were we can make reference to the work he and Jean-Pierre Vigier did in 1954 slightly changing Bohm's original model. In this work, they embedded the electron in a fluid undergoing "very irregular and effectively random fluctuation" in its motion (Bohm and Vigier, 1954). While these fluctuations could be explained by either a deterministic or a stochastic description, Bohm and Vigier framed them into the causal interpretation approach, titling their paper "Model of the causal interpretation of quantum theory in terms of a fluid with irregular fluctuations."

Bohm's proposal stirred up a debate and gathered adherents, yet it got a poor reception among physicists (Freire, 2005). In the late 1950s, however, Bohm's research split from that of his collaborators like Vigier and de Broglie. While the latter persevered in their research into the causal interpretation, Bohm gave it up. A number of factors may have played a role in his decision, including discouragement by the limited response to these ideas and "because [he] did not see clearly, at the time, how to proceed further," (Bohm, 1987: p. 40). Another influential factor, not acknowledged by Bohm himself, was his ideological rupture with Marxism in 1956–1957, which may have led him to play down the role he attributed to determinism in science and society (Freire, 2009). As a matter of fact, from 1960 on Bohm gradually began to search for a new approach to the interpretation of quantum mechanics.

22.3 Implicate and Explicate Order

The new approach took 10 years to mature. Indeed, only around 1970 the first papers suggesting "a new mode of description in physics" (Bohm et al., 1970) and taking "quantum theory as an indication of a new order in physics" (Bohm, 1971, 1973) appeared. Bohm drew heavily on analogies and images to convey the content of his new ideas on order, the most well known being the image of a drop of ink falling into a rotating cylinder full of glycerin. When the cylinder rotates in one direction the ink disappears in the glycerin, which Bohm referred to as the implicate order. When it rotates in the opposite direction, the drop reappears, namely the explicate order. Bohm would associate the explicate order with classical or macroscopic phenomena and implicate order with quantum phenomena. As for Bohm the usual interpretation of quantum mechanics was not the final word in quantum physics, he went on to associate the implicate order to a physical theory yet to be worked out that has standard quantum mechanics as a limiting case (Freire, 1999).

Implicate and explicate order would have remained just as philosophical or scientific insights if it had not been the mathematical elaboration it later received. To accomplish this Bohm did not work alone. He counted on the collaboration of Basil Hiley, his assistant at the Birkbeck College since the early 1960s. Their strategy was to analyze the algebraic structures behind quantum mechanics mathematical formalism and subsequently look for more general algebras which could be reduced to the quantum algebras as special cases. This strategy was informed by the fact that they did not want to take any kind of space-time geometry from the beginning of their reasoning. Instead they tried to develop algebraic structures from which space-time could emerge. Here the algebraic primary structure would be the implicate order and the emerging space-time geometry would be the explicate order. With the benefit of hindsight, we can identify Hiley's unique contribution in this sense. A number of different factors also contributed to the development of this mathematical approach, such as new and mathematically talented students including Fabio Frescura, interactions with the mathematician Roger Penrose at Birkbeck Collehe, and inspiration from the Brazilian physicist Mario Schönberg's early works on algebras and geometry. Highly sophisticated from the mathematical point of view, such an approach has however suffered from little contact with experimental results, which could help to inform the mathematical choices to be done.

Before going on to the next stage of Bohm's ideas on quantum mechanics, let us summarize the influences which had led to the ideas of implicate and explicate order. As recalled by Bohm, there was his search for new ideas, his enduring reflection about what was common to his previous approach and standard quantum mechanics (a task that was eased by John Bell's work pointing to non-locality as the irreducible quantum feature), the insight from a TV program in which he saw the demonstration with ink and glycerin, and the fruitful interaction with mathematicians and mathematical physicists. The question remains of how much Bohm was influenced in the early 1960s by his dialogues with the writer Jiddu Krishnamurti. Bohm once acknowledged some influence from Krishnamurti's psychological ideas on the non separability between observer and observed, which reinforced his ideas on the analogous problems in quantum measurement (Bohm, 1982). Later, however, he did not mention such influence again in his research (Bohm, 1987). Basil Hiley thinks that these dialogues were not influential in Bohm's physics, rather they played a role in Bohm's thoughts about society, thoughts, and creativity.[3] A reflection on the relationship between observer and observed had been an essential feature of Bohm's

[3]Basil Hiley Oral History, interviewed by O. Freire, 11 January 2008, American Institute of Physics.

early reflections on the foundations of quantum mechanics, see for instance how he treated measurement both in his 1951 book and 1952 causal interpretation. Thus, it seems that the influence of these dialogues on his physics, if any, was superseded by his enduring reflection on measurement in quantum physics (Freire, 1999).

22.4 Returning to the Quantum Potential

In the late 1970s a new stage in Bohm's quest for a new approach to quantum mechanics began; albeit strongly overlapping the previous one. To a certain extent it meant a return to Bohm's 1952 ideas. This return, almost 30 years later, is vividly described by Basil Hiley[4]:

> We had a couple of research students working for us, Chris Dewdney and Chris Philippidis. They came to me one day with Bohm's '52 paper in their hand. And, they said, "Why don't you and David Bohm talk about this stuff?" And I then started saying, "Oh, because it's all wrong." And then they started asking me some questions about it and I had to admit that I had not read the paper properly. Actually I had not read the paper at all apart from the introduction! And when I took it and, so, you know, I was now faced with embarrassment that our research students [Laugh] were putting me in, in a difficult position, and so I went back home and I spent the weekend working through it. As I read it, I thought, "What on earth is wrong with this? It seems perfectly all right. Whether that's the way nature behaves is another matter." But as far as the logic, the mathematics, and the arguments were concerned, it was sound. I went back again to see the two Chrises again, I said, "Okay, let's now work out what the trajectories are, work out what the quantum potential looks like in various situations."

The students and the surprised Hiley went on to calculate the trajectories allowed by Bohm's quantum potential using the recently arrived desktop computer resources to plot these trajectories creating images of quantum phenomena (Philippidis et al., 1979). Thus, motivated by students and collaborators, Bohm returned to his 1952 approach, but now he had a new problem: how to interpret such an approach and its deterministic trajectories shaped by the nonlocal physical interactions resulting from the quantum potential. Here there is a crucial point to consider while charting Bohm's thoughts on quantum mechanics. While he and his colleagues kept the mathematics and the model used in the 1952 paper they changed many of their philosophical and conceptual assumptions. The quantum potential was no longer considered a new physical potential. Instead it was interpreted as an indication of a new order, in particular a kind of "active information." Emphasis was no longer put on the causality embedded in such an approach. According to Bohm and Hiley (1993) in their synthesis book *The Undivided Universe*, after considering terms such as "causal" and "hidden variable" interpretations "too restrictive" and stating that "nor is this sort of theory necessarily causal," they concluded that "the question of determinism is therefore a secondary one, while the primary question is whether we can have an adequate conception of the reality of a quantum system, be this causal

[4] Basil Hiley Oral History.

or be it stochastic or be it of any other nature." Their main philosophical stance was thus to look for an ontological view of quantum phenomena, while the main scientific challenge remained how to tie such a requirement with the mathematical work related to the idea of an "implicate order." This challenge has survived Bohm and is a task to which Hiley remains focused, as we will see below.

22.5 Bohm's Legacy

Bohm's main legacy for the understanding of quantum physics is his enduring insistence that the foundations of this theory deserves further investigation and that it should be conducted with open minds to see the problems from different perspectives. In addition, his causal interpretation highlighted the non-locality present both in his interpretation and in standard quantum mechanics. The very existence of such an interpretation was the main inspiration for the work that led John Bell to his seminal theorem. Lancelot Whyte once compared Bohm to Kepler (Freire, 2005). As for Bohm's legacy, it is a high accolade for a contemporary physicist to be compared to the great German mathematician and astronomer.

Yet, the meaning of Bohm's quantum potential and implicate order remains controversial. It remains a research program in progress. In fact, subsequent researchers follow one of three lines of research. The first line continues to work on Bohm's original 1952 proposal not only trying to extend the first physical models but also keeping Bohm's early philosophical commitments with determinism and realism. This is, for instance, the path chosen by Peter Holland (1993).

The second line concerns Bohmian mechanics, as coined by Dürr et al. (1992, 1996). They construed Bohm's proposal in a very clean and elegant way. While in his original paper Bohm worked out analogies between Schrödinger equation and classical Hamilton-Jacobi equations, which led to an emphasis on the role of the non-classical potential that Bohm christened quantum potential, Dürr and colleagues adopted just two premises: the state which describes quantum systems evolves according to Schrödinger equation and particles move, that is, they have a speed in the configuration space. With this approach, without quantum potentials, they derived the same results one gets both with standard quantum mechanics and with Bohm's original approach for nonrelativistic phenomena. This approach has been useful for discussing quantum chaos, and for this reason it has received wide acceptance well beyond physicists interested just in foundations of quantum mechanics. One should note that when these physicists define what they understand to be a *Bohmian theory* priority for determinism disappears and they consider that "a Bohmian theory should be based upon a clear ontology", meaning by ontology "what the theory is fundamentally about." While for non-relativistic physics they have adopted a particle ontology, they admitted that they "have no idea what the appropriate ontology for relativistic physics actually is." This way commitment to a quantum ontology comes before an engagement with a causal pattern for physical theories, a position analogous to that has been adopted by David Bohm and Basil Hiley since the 1960s.

The third line of Bohm's scientific legacy is represented by Basil Hiley, who continues to work on research that he and Bohm had been carrying out before Bohm's death. This research tries to connect the insights of implicate order and active information with the quest for algebraic structures able to underpin space-time geometry and standard quantum mechanics. This program has inherited from the causal interpretation the major challenge of obtaining a fully relativistic treatment in order to match the level attained by standard quantum mechanics with Dirac equation. Bohm had once promised that "the day that we defeat the Dirac equation, we are going to have a special victory party, with a case of champagne".[5] Recently Hiley announced that he has "now found a complete description of the Dirac theory in the Bohm tradition, Bohm momentum, Bohm energy and even a quantum potential which reduces to the Pauli QP in the non-relativistic limit".[6] Only time will tell if the case of champagne should be opened.

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Chapter 23
Quasi-truth and Quantum Mechanics

Newton C.A. da Costa and Otávio Bueno

23.1 Introduction

Since its early formulation, non-relativistic quantum mechanics (QM) has been the source of sustained controversy about its foundation. Despite its impressive empirical success, several foundational issues have not been settled by the theory: What exactly happens with the observables when a quantum system is not being measured? And what exactly happens during measurement? What is the nature of quantum particles? In particular, are they individuals or not? And can identity be applied to these particles? Not surprisingly, a variety of interpretations of QM have been developed in the attempt to address these and other foundational questions. Perhaps also not surprisingly, so far there has been no agreement as to which of these interpretations (if any) should be preferred.

In this paper, we examine, in outline, some of these interpretations and argue that, properly understood, they are all quasi-true. That is, they are currently empirically adequate with regard to the available evidence in their domain (roughly speaking, the non-relativistic quantum mechanical domain). This explains why, at least at the moment, there are no empirical grounds to choose between these interpretations. We then offer a tentative framework to assess such interpretations of QM, and indicate that, despite their equal empirical support, there are pragmatic factors to prefer some of them to others.

Due to space constraints, we will need to gloss over several complications that are inevitable in discussions of QM, and will not be able to offer a comprehensive treatment of the issues. In particular, the selection of interpretations we will be able to discuss is limited, and our exposition will be fairly informal. Our goal here is simply to sketch the central ideas, leaving several details for another occasion.

N.C.A. da Costa (✉)
Department of Philosophy, Federal University of Santa Catarina, Florianópolis,
SC 88040-900, Brazil
e-mail: ncacosta@terra.com.br

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23.2 Quantum Mechanics and Some Interpretations

Let us start by discussing a well-known tension that emerges in non-relativistic quantum mechanics, and which is one of the sources for the need for interpreting the theory. Consider a non-relativistic quantum system. In order to describe the system's dynamics, the mathematical formalism of QM offers two distinct kinds of transformations: (a) On the one hand, we have reversible transformations, described by unitary operators on the relevant state space, and which are, generally speaking, obtained from the Schrödinger equation. (b) On the other hand, we have non-reversible and random transformations, described by more complex operators, which emerge in the system, in particular, as the result of measurement. The question is: how exactly are (a) and (b) related? What is so special about measurement? The formalism of QM, on its own, does not settle this issue, since it essentially indicates just how to calculate the relevant probabilities in each case. To address the issue, we need an interpretation of the formalism.

On the Copenhagen interpretation – in its standard formulation (see Bohr, 1987; Heisenberg, 1955) – there is something special about measurement: it leads to the collapse of the wave function (von Neumann, 1932).[1] Central to this interpretation is the idea that, before measurement, typically it cannot be determined which exact state a non-relativistic quantum system is in. For example, is the spin of an electron up or down? For all we know, the system may be evolving in a superposition (a linear combination) of spin up and spin down. After measurement, however, a definite answer is always obtained. It is determined, for instance, that the spin is up. The measurement process leads to the collapse of the wave function, and the system now has a definite, determined state.

The Copenhagen interpretation is often associated with two principles: (A) Heisenberg's uncertainty principle and (B) Bohr's complementarity principle. Roughly speaking, the uncertainty principle states that it is not possible to measure with full certainty both the position and the momentum of a quantum particle. This principle can be read in two different ways: (A.i) one reading takes the principle as offering an *epistemological* constraint on measurement, whereas (A.ii) another takes it as describing an *ontological* feature of quantum systems.

(A.i) On the epistemological reading, that Heisenberg seemed to have favored at least initially, the uncertainty emerges as the result of limitations in the measurement process. On this reading, in order to measure the particle's position, we inevitably disturb its momentum, and in order to measure its momentum, we inescapably disrupt its position. The result is the impossibility of measuring both with full certainty.

(A.ii) Bohr seems to have offered, however, an ontological reading of the uncertainty principle. According to this reading, the uncertainty described in the

[1] What the Copenhagen interpretation exactly is and who is responsible for its formulation turn out to be complex issues, which unfortunately we cannot discuss here (see Howard, 2004).

principle is not a mere epistemological limitation of our measuring devices. The uncertainty is an expression of the ultimate nature of quantum reality: the complementary nature of the phenomena involved. Even if we could devise methods of detecting quantum particles with minimum interference, the uncertainty would still be present as an intrinsic component of the quantum phenomena themselves. On this view, the uncertainty is not something that could be, even in principle, overcome.

(B.i) The reason why Bohr may have favored this reading of the uncertainty principle derives from a particular – also ontological – reading of the complementarity principle itself. According to the latter, quantum phenomena have a complementary nature in that their full description requires that one accounts for, e.g., both their wave-like and their particle-like features. However, it is not possible for the phenomena to exhibit both wave-like and particle-like features simultaneously. We have here the wave-particle duality as an intrinsic, ontological aspect of quantum phenomena. And the point can be extended to other complementary properties in the quantum world, such as position and momentum.

(B.ii) But similarly to the uncertainty principle, the complementarity principle can also be read as an epistemological tenet. On this reading, the principle expresses an epistemological limitation, in that the components of the quantum phenomena under study, such as its wave-like and particle-like features, cannot be detected simultaneously. Clearly, the ontological reading is stronger than the epistemological. After all, if it is part of the nature of quantum phenomena that their complementary features cannot be exhibited together, we could not detect these features simultaneously – as long as our measuring devices are reliable.

Typically, however, the Copenhagen interpretation has been presented in a more anti-realist tone, by emphasizing that QM is fundamentally about the results of measurement, and by insisting that what really goes on between measurements is not something that the theory settles. In this way, roughly speaking, anti-realists will tend to support only the epistemological readings of the uncertainty and the complementarity principles. Realists, however, will tend to favor the corresponding ontological readings.[2] But the point stands that on both realist and anti-realist formulations of the Copenhagen interpretation, measurement is crucial – and special.

However, some interpretations of QM deny that there is anything special about measurement; that is, anything that requires special treatment in the formalism of QM. This is, to some extent, the case of the many-worlds interpretation (see Everett, 1957; De Witt, 1970). On this interpretation, the crucial feature of the dynamics

[2]This is rough since, in principle, realists can adopt *both* the ontological and the epistemological readings of the two principles. In any case, anti-realists are more likely to deny the corresponding ontological versions.

of a non-relativistic quantum system is given by the Schrödinger equation. What happens in measurement – on De Witt's version of the many-worlds interpretation – is that the world splits.[3] A non-relativistic quantum system evolves undisturbed, for instance, in a superposition of states of spin up and of spin down, until it is measured. At this point, the world splits: one world ends up with the spin up measured state, and another with the spin down measured state. In this way, each of the alternative components of the quantum system obtains – although in different worlds.

One of the benefits of this interpretation is that it avoids the introduction of the collapse of the wave function, thus bypassing entirely the need to introduce a genuinely random event to explain what goes on in measurement. Ultimately, all there is on this interpretation are the quantum states described in especial by the Schrödinger equation. It just happens that there are many more worlds than we have initially anticipated. And given that all of these worlds exist, strictly speaking there is no collapse of the wave function: each world exhibits one of the relevant definite quantum states. However, this benefit – of avoiding the introduction of the wave function collapse – can be earned only if we do not invoke the suggestion that worlds split as the result of measurements. Otherwise, there is indeed something special about measurement that needs to be taken into account: the splitting of worlds itself (see Barrett, 1999). In other words, Everett's original formulation of the many-worlds interpretation – free from the splitting worlds assumption – seems better than De Witt's in this respect (see also Vaidman, 1998).

An objection that has often been raised against the many-worlds interpretation is that it is unclear how to make sense of the concept of probability on this view (see, e.g., Albert and Loewer, 1988; Barrett, 1999). After all, given that each component of the superposition obtains, there is no distinction between what is actual and what is possible, and hence it is unclear how exactly to draw the line between what is actual and what is probable.[4]

Moreover, can the world really split in the way postulated by De Witt's version of the many-worlds interpretation without anyone noticing? The many-minds interpretation of QM is offered as an ontologically more parsimonious formulation of the many-worlds conception, since it preserves the assumption that there is only one physical world. Our minds, as it were, suffer the split (see Albert and Loewer, 1988; Barrett, 1999). Given that there is no multiplicity of worlds on the many-minds interpretation, but only of minds, the difficulty of making sense of probability does not emerge. After all, on the many-minds interpretation, there is no difficulty to distinguish what is actual from what is possible.

However, even though the many-minds interpretation does not require the existence of more than one world, it is unclear that there are that many minds – one for each possible measurement outcome, or, more generally, for each potential outcome

[3]Everett's formulation of the many-worlds interpretation is not committed to the splitting of worlds. Roughly speaking, all the worlds exist independently of measurement, and they instantiate the relevant quantum states.

[4]For a response to this objection within the many-worlds framework, by invoking the concept of a "measure of existence of worlds", see Vaidman, 1998.

of a quantum interaction in the whole history of the universe. And even if there were so many minds, the commitment to them is not found in the formalism of QM, which does not even quantify over these things. As a result, it is unclear that the commitment to the many-minds hypothesis is ontologically less problematic than the one to the plurality of worlds. Moreover, given that the outcome of a measurement is supposed to be a physical process in the world, rather than a psychological event in the mind, it is unclear that the many-minds interpretation ultimately offers an adequate account of the measurement process. The latter does not seem to be even properly categorized as a physical event.

This small sample of interpretations of QM clearly indicates the difficulty involved in assessing these views. Each interpretation has clear benefits, providing some understanding of the way the quantum world could be (van Fraassen, 1991). Moreover, each interpretation goes beyond the formalism of QM, and offers an account of what may be going on beyond the phenomena. Some interpretations are fairly minimal in what they add to the description offered by the formalism. For example, on the anti-realist reading of the Copenhagen interpretation, the components added to the formalism emphasize the epistemological limitations that restrict our access to some aspects of the phenomena that QM studies. Other interpretations add a significant amount to the formalism, to the point that it may not even be clear whether we are dealing with just an interpretation of QM or, in fact, with a rival theory, which would yield different empirical results than QM does if we had the required technological devices to test these predictions. For example, the many-worlds interpretation can be seen in this way. On the revised Everett formulation articulated by Vaidman, 1998, the many-worlds interpretation entails the existence of a plurality of worlds. However, this is not a prediction made by either the Copenhagen interpretation or by the formalism of non-relativistic QM alone. In fact, the introduction of the collapse of the wave function can be seen as an attempt to block the commitment to the plurality of worlds (Vaidman, 1998). We are, however, currently unable to test the existence of such a plurality, and thus cannot decide empirically on the merits of the contending interpretations.

It becomes clear that the interpretations involved here also have considerable costs. They are inconsistent with each other – at least in the ontological assumptions they make to describe the quantum world. And their attempts to account for what goes on beyond the phenomena introduce, in some cases, implausible considerations, such as the number of minds required by the many-minds interpretation. What is needed then is a framework to assess these (and other) interpretations in an objective way. We think that one possible framework is given by the partial structures approach (da Costa and French, 2003).

23.3 Quasi-truth and Partial Structures

The partial structures approach has three main concepts: partial relation, partial structure, and quasi-truth (for details, see da Costa and French, 2003). One of the main motivations for introducing this proposal derives from the need for supplying a formal framework in which the openness and incompleteness of the information that is dealt with in scientific practice can be accommodated. This is accomplished, first, by extending the usual notion of structure, in order to accommodate the partialness of information we have about a certain domain (introducing then the notion of a partial structure). Second, the Tarskian characterization of the concept of truth is generalized for partial contexts, which then leads to the introduction of the corresponding concept of quasi-truth.

The first step, then, to characterize partial structures is to formulate a suitable concept of a partial relation. In order to investigate a certain domain of knowledge Δ (say, the physics of particles), researchers formulate a conceptual framework that helps them systematize and interpret the information they obtain about Δ. This domain can be represented by a set D of objects (which includes *real* objects, such as configurations in a Wilson chamber and spectral lines, and *ideal* objects, such as quarks). D is studied by the examination of the relations that hold among its elements. However, it often happens that, given a relation R defined over D, we do not know whether all objects of D (or n-tuples thereof) are related by R, or we need to ignore some of the relations that are known to hold among objects of D, in order to study other relations about that domain in a tractable way. This is part of the incompleteness and partiality of our information about Δ, and is formally accommodated by the concept of a partial relation. The latter can be characterized as follows. Let D be a non-empty set. An n-place *partial relation* R over D is a triple $\langle R_1, R_2, R_3 \rangle$, where R_1, R_2, and R_3 are mutually disjoint sets, with $R_1 \cup R_2 \cup R_3 = D^n$, and such that: R_1 is the set of n-tuples that (we know that) belong to R; R_2 is the set of n-tuples that (we know that) do not belong to R, and R_3 is the set of n-tuples for which it is not known (or, for reasons of simplification, it is ignored that it is known) whether they belong or not to R. (Notice that if R_3 is empty, R is a usual n-place relation that can be identified with R_1.)

But in order to accommodate the information about the domain under study, a concept of structure is needed. The following characterization, spelled out in terms of partial relations and based on the standard concept of structure, offers a concept that is broad enough to accommodate the partiality usually found in scientific practice. A *partial structure* A is an ordered pair $\langle D, R_i \rangle_{i \in I}$, where D is a non-empty set, and $(R_i)_{i \in I}$ is a family of partial relations defined over D.[5]

We have now defined two of the three basic concepts of the partial structures approach. In order to spell out the last one (quasi-truth), we will need an auxiliary notion. The idea here is to use the resources supplied by Tarski's definition of truth. But since the latter is only defined for full structures, we have to introduce an intermediary notion of structure to link partial to full structures. This is the first role of those structures that extend a partial structure A into a full, total structure (which are called A-normal structures). Their second role is model-theoretic, namely to put

[5]The partiality of partial relations and structures is due to the incompleteness of our knowledge about the domain under investigation. With additional information, a partial relation can become a full relation. Thus, the partialness examined here is not ontological, but epistemic.

forward an interpretation of a given language and to characterize semantic notions. Let $A = \langle D, R_i \rangle_{i \in I}$ be a partial structure. We say that the structure $B = \langle D', R'_i \rangle_{i \in I}$ is an *A-normal structure* if (i) $D=D'$, (ii) every constant of the language in question is interpreted by the same object both in A and in B, and (iii) R'_i extends the corresponding relation R_i (in the sense that, each R'_i, supposed of arity n, is defined for all n-tuples of elements of D'). Note that, although each R'_i is *defined* for all n-tuples over D', it holds for some of them (the R'_{i1}-component of R'_i), and it doesn't hold for others (the R'_{i2}-component).

As a result, given a partial structure A, there are several A-normal structures. Suppose that, for a given n-place partial relation R_i, we don't know whether $R_i a_1 \ldots a_n$ holds or not. One of the ways of extending R_i into a full R'_i relation is to look for information to establish that it *does* hold; another way is to look for contrary information. Both are *prima facie* possible ways of extending the partiality of R_i. But the same indeterminacy may be found with other objects of the domain, distinct from $a_1, \ldots, a_n$ (for instance, does $R_i b_1 \ldots b_n$ hold?), and with other relations distinct from R_i (for example, is $R_j b_1 \ldots b_n$ the case, with $j \neq i$?). In this sense, there are *too many* possible extensions of the partial relations that constitute A. Therefore, we need to provide constraints to restrict the acceptable extensions of A.

In order to do that, we need first to formulate a further auxiliary notion (see Mikenberg et al., 1986). A *pragmatic structure* is a partial structure to which a third component has been added: a set of accepted sentences P, which represents the accepted information about the structure's domain (depending on the interpretation of science that is adopted, different kinds of sentences are to be introduced in P: realists will typically include laws and theories, whereas empiricists will add mainly certain regularities and observational statements about the domain in question). A *pragmatic structure* is then a triple $A = \langle D, R_i, P \rangle_{i \in I}$, where D is a non-empty set, $(R_i)_{i \in I}$ is a family of partial relations defined over D, and P is a set of accepted sentences. The idea is that P introduces constraints on the ways that a partial structure can be extended (the sentences of P hold in the A-normal extensions of the partial structure A).

Our problem is: given a *pragmatic* structure A, what are the necessary and sufficient conditions for the existence of A-normal structures? Here is one of these conditions (Mikenberg et al., 1986). Let $A = \langle D, R_i, P \rangle_{i \in I}$ be a pragmatic structure. For each partial relation R_i, we construct a set M_i of atomic sentences and negations of atomic sentences, such that the former correspond to the n-tuples that satisfy R_i, and the latter to those n-tuples that do not satisfy R_i. Let M be $\cup_{i \in I} M_i$. Therefore, a pragmatic structure A admits an A-normal structure if and only if the set $M \cup P$ is *consistent*.

Assuming that such conditions are met, we can now formulate the concept of quasi-truth. A sentence α is *quasi-true* in a pragmatic structure $A = \langle D, R_i, P \rangle_{i \in I}$ if there is an A-normal structure $B = \langle D', R'_i \rangle_{i \in I}$ such that α is true in B (in the Tarskian sense). If α is not quasi-true in A, we say that α is *quasi-false* in A. Moreover, we say that a sentence α is *quasi-true* if there is a pragmatic structure A and a corresponding A-normal structure B such that α is true in B (according to Tarski's account). Otherwise, α is *quasi-false*.

The idea, intuitively speaking, is that a quasi-true sentence α does not describe, in a thorough way, the whole domain that it is concerned with, but only an aspect of it: the one that is delimited by the relevant partial structure *A*. After all, there are several different ways in which *A* can be extended to a full structure, and in some of these extensions α may not be true. Thus, the concept of quasi-truth is strictly weaker than truth: although every true sentence is (trivially) quasi-true, a quasi-true sentence may not be true (since it may well be false in certain extensions of *A*).

To illustrate the use of quasi-truth, let us consider an example. As is well known, Newtonian mechanics is appropriate to explain the behavior of bodies under certain conditions (say, bodies that, roughly speaking, have a low velocity with respect to the speed of light, that are not subject to strong gravitational fields etc.). But with the formulation of special relativity, we know that if these conditions are not satisfied, Newtonian mechanics is false. In this sense, these conditions specify a family of partial relations, which delimit the context in which Newtonian theory holds. Although Newtonian mechanics is not true (and we know under what conditions it is false), it is *quasi-true*; that is, it is true in a given context, determined by a pragmatic structure and a corresponding *A*-normal one (see da Costa and French, 2003).

23.4 A Framework for Interpretations of Quantum Mechanics

The partial structures approach provides a framework in terms of which we can revisit and assess, at least in part, the interpretations of QM discussed above. In this section, we motivate, in outline, this claim.

Despite the significant differences between them, the interpretations discussed above have one common feature: they are all (partially) empirically adequate – in the sense that the empirical evidence *currently available* does not undermine any of these interpretations. However, the evidence at hand also fails to discriminate between the various interpretations, given that the latter are equally supported by the available evidence. There is the possibility though that in the future some new evidence will undermine some of these interpretations without challenging others. But to make sense of this possibility, we need to have a concept of empirical adequacy that is *not* "absolute"; that is, a theory's empirical adequacy is not characterized in terms of all past, present, and future evidence (we do not have access to the latter yet in any case). Rather, the empirical adequacy of a theory is better conceptualized as emerging from, and changing with, the evidence as the latter becomes available. Changes in evidence may change a theory's empirical adequacy as well. For example, van Fraassen (1980, p. 64) offers an account of empirical adequacy that is "absolute" in the relevant sense: a scientific theory is (or is not) empirically adequate with respect to *all* possible evidence – past, present, and future. It seems to us, however, that it is important to develop an account of empirical adequacy that is more fine-grained and responsive to the way evidence changes in the course of the history of a scientific theory. In particular, the account should be sensitive to the way shifts in evidence bears on the empirical adequacy of the theory under consideration (see also Bueno, 1997).

The partial structures approach allows us to characterize a concept of empirical adequacy that is sensitive to shifts in evidence. Consider a partial structure *A* that represents the information generated from various kinds of experiments involving non-relativistic quantum systems and the resulting measurement reports. This structure is clearly partial given that, for instance, there is no information available in the structure regarding the outcomes of future experiments. As more and more information becomes available, more partial relations in the partial structure *A* will shift their R_3-components to either R_1- or R_2-relations. Each of the interpretations of QM discussed above is quasi-true in that partial structure *A*; that is, the evidence currently available in *A* does not rule out the possibility that these interpretations turn out to be true. In this way, the interpretations are (partially) empirically adequate – that is, quasi-true with respect to the available evidence in the partial structure *A*. It is possible, however, that the evidence that becomes available in the future rules out some of the interpretations in question. In this case, there will be a change in the partial structure *A* that represents the available evidence. And with respect to the new partial structure, some of these interpretations will no longer be (partially) empirically adequate – that is, they will no longer be quasi-true.

Although the interpretations of QM discussed above are (partially) empirically adequate given current evidence, it is still possible to assess them in terms of three pragmatic factors:

(F1) *Explanatory power*: How well do these interpretations explain puzzling aspects of non-relativistic QM (such as the measurement problem)?
(F2) *Novel predictions*: Do the interpretations yield novel predictions – even though such predictions cannot be currently tested?
(F3) *Coherence*: Do the interpretations offer a coherent picture of what is going on beyond the observable phenomena?

These three factors are pragmatic in the sense that even if positive answers are given to the questions above, we cannot conclude that the resulting interpretations are thereby more likely to be true. Why is this the case?

Answering explanatory demands, such as the one in (F1), is certainly a useful aspect of an interpretation of QM. But it is much less clear, and far more controversial to decide, whether a successful answer to (F1) increases the likelihood that the interpretations in question are true. A positive answer to (F1) clearly supports the quasi-truth of the interpretations involved by highlighting the partial structures that can be used in the explanation of the phenomena under investigation. But it is not clear that we are entitled to say anything stronger than that. After all, as classical mechanics beautifully illustrates, a theory can explain several aspects of a given domain without thereby being true.

It might be thought that producing novel predictions, such as those suggested in (F2), amounts to more than a pragmatic feature of an interpretation: it should offer an epistemic appraisal of the proposal. But we are considering here novel predictions that *currently cannot be tested*. As such, the predictions do not seem to speak to the truth, or even the approximate truth, of the interpretations in question, since the outcome of the predictions cannot be determined at the moment. Novel, untestable predictions can be counted as having at best a pragmatic role – until the moment in which the predictions can in fact be tested (if we ever reach that point).

Finally, the development of a coherent picture of the quantum world, factor (F3) above, clearly highlights a pragmatic dimension. Having a coherent account of the quantum domain helps us understand such a domain better. But, once again, this understanding underscores a pragmatic, rather than an epistemic, factor. After all, why is it that the fact that a description makes sense to us – by increasing our understanding – should thereby offer us reason to believe that that description is true? Consider, for instance, historical novels. They arguably offer us understanding of nuances, complexities, and significant aspects of life in certain historical periods. But we do not, thereby, take the descriptions provided in these novels to be true. The same point, *mutatis mutandis*, goes for interpretations of QM.

How does the Copenhagen interpretation fare with respect to (F1)–(F3)? The interpretation does not seem to do particularly well with respect to (F1). If we focus on the measurement problem, the introduction of the collapse postulate rather than offering a well-motivated approach to the issue seems basically to reformulate the problem. If we are supposed to understand why measurement is so special that we need to introduce a truly random event at the core of QM, just stating that the wave function collapses does not quite solve the problem. It essentially restates it.

With regard to (F2), the Copenhagen interpretation does not seem to do much better either. After all, the interpretation does not offer any novel predictions – even those that cannot be currently tested.

However, the Copenhagen interpretation does offer a coherent, very deflationary, account of the quantum domain, particularly in its anti-realist version. In this sense (F3) is properly met. This is probably the main reason why this interpretation seems to be so widely accepted among physicists. Given the capricious nature of the quantum domain, it is a virtue of the Copenhagen interpretation – particularly in its anti-realist form – that it does not force one to be committed to significantly more than is strictly needed to use quantum theory.

How does the many-worlds interpretation fare with regard to (F1)–(F3)? If we consider (F1), and focus on the measurement problem, the many-worlds interpretation does not address the issue very well, particularly in its "splitting worlds" formulation. After all, on this formulation, there is still something special about measurement: worlds split! A better account of measurement is offered by the version of the many-worlds interpretation that does *not* invoke the splitting worlds assumption. However, this version needs to introduce a measure of existence of worlds (Vaidman, 1998) in order to accommodate probability in the many-worlds interpretation. The worry here is whether we can really make sense of such a measure of worlds, given that we have no empirical access to these concrete objects.

With regard to (F2), the many-worlds interpretation, particularly in the non-splitting worlds formulation, does offer novel, but currently untestable, predictions: the existence of a plurality of worlds. We may never be able to test this prediction, but it is certainly an interesting and quite unexpected prediction to make!

Finally, if we consider (F3), the non-splitting worlds version of the many-worlds interpretation does offer a coherent account of the quantum domain. It just turns out that, if the interpretation is true, there are many more worlds than we initially thought.[6]

The many-minds interpretation is a variant of the many-worlds conception, and so our discussion here can be brief. With regard to (F1), as discussed in Section 23.2 above, the many-minds interpretation does not seem to offer an adequate solution to the measurement problem, given the need to postulate an incredible number of minds. With respect to (F2), as opposed to the many-worlds proposal, the many-minds interpretation does not yield novel predictions, since it does not entail the existence of a plurality of worlds. However, focusing now on (F3), the interpretation does seem to offer a coherent picture of the quantum domain – particularly if we can make sense of the idea that there are so many minds!

As this brief account of some interpretations of QM indicates, although all of the interpretations examined here are (partially) empirically adequate – that is, quasi-true with respect to current evidence – it is still possible to assess them in terms of three pragmatic factors (explanatory power, novel predictions, and coherence). The considerations above suggest that, among the interpretations we discussed, the non-splitting worlds version of the many-worlds interpretation seems to offer the best account of these factors. Does this mean that this interpretation is true? Not really, given that the factors involved are pragmatic at best. Satisfying these three factors may give us reason to *accept* this interpretation, but *not* to *believe* in its truth – to invoke a well-known distinction used by the constructive empiricist (van Fraassen, 1980).

Now, suppose that we incorporate the satisfaction of factors (F1)–(F3) in the formulation of quasi-truth itself; for instance, we include (F1)–(F3) as part of the set of accepted sentences P that are expected to be satisfied in a pragmatic structure. The idea then is that the more factors an interpretation of QM satisfies, the *more quasi-true* it becomes. In this sense, we can then say that the non-splitting worlds version of the many-worlds interpretation is *more quasi-true* than the Copenhagen or the many-minds interpretations. Moreover, we can also understand how these interpretations offer *rival* accounts of the quantum domain, since they address very differently – from ontological and epistemological points of view – the three pragmatic factors (F1)–(F3) that we discussed.

23.5 Conclusion

In this paper, we sketched how the partial structures approach offers a useful framework to examine interpretations of QM. As we saw, the approach provides an

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[6] It is not clear that the suggestion that worlds literally split in measurement is coherent, since it seems to conflict with several physical assumptions (see Albert and Loewer, 1988; Barrett, 1999). So the coherence point does not seem to apply to the splitting worlds version of the many-worlds interpretation.

account of partial empirical adequacy according to which the interpretations of QM that we examined are partially empirically adequate, that is, quasi-true given current evidence. However, it is still possible to assess the interpretations in question in terms of how well they meet significant pragmatic factors. Despite not giving us reason to believe that the interpretations are true, the satisfaction of these factors allows us to accept some of these interpretations for pragmatic reasons, and explore the understanding they offer of the quantum world.

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Chapter 24
The Qualitative Analysis of Differential Equations and the Development of Dynamical Systems Theory

Tatiana Roque

The first scientific work concerning a qualitative approach to the problem of solving differential equations was published by Henri Poincaré in the end of the nineteenth century (Poincaré, 1881; 1882; 1885; 1886). Before him, the usual methods to treat linear differential equations tried to solve them explicitly, what means to find out a family of functions that satisfy the conditions established by the equation.[1] But a similar procedure is, in general, impossible in the nonlinear case.

Even when the existence theorem affirms there is a solution for any initial condition, in very few cases this solution can be found explicitly. So, in instead of determining the function that actually solves a differential equation, the qualitative approach search a picture of the hole set of possible solutions describing its main geometrical properties. Poincaré justifies the legitimacy and the interest of such a kind of research in two ways:

(1) First of all, the qualitative analysis could help the traditional quantitative methods. The theory of analytical functions developed by Cauchy and Weierstrass gave already the conditions under which a series, that expresses a function, can be prolonged from a neighborhood to another. Qualitative methods could help the quantitative research to find how to go from a neighborhood, were the function is expressed by a series, to another one, where the function is expressed by a different series.[2]
(2) Besides that, qualitative analysis can be interesting by itself, since it can furnish rich information to the traditional problems of Celestial Mechanics, as the three

T. Roque (✉)
Professora do Instituto de Matemática, Universidade Federal do Rio de Janeiro,
Ilha do Fundão – Rio de Janeiro, RJ-Brasil
e-mail: tati@im.ufrj.br

[1] For a history of the methods employed to solve linear differential equations before Poincaré, see (Gray, 1986).

[2] Poincaré uses this same kind of argument in different writings, see (Poincaré, 1881) and (Poincaré, 1921). His argument mentions an analogy with the theory of algebraic equations analyzed in (Gilain, 1991).

D. Krause, A. Videira (eds.), *Brazilian Studies in Philosophy and History of Science*,
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> body problem. These are very difficult problems involving nonlinear equations and the qualitative aspects of the set of possible solutions can be interpreted in terms of relevant physical properties. For instance, if we know that the solutions remain confined in a certain region of the domain, and cannot escape to infinity, we can conclude a certain kind of stability property. If there are periodical solutions in the vicinity of which the other solutions do not escape, we can also conclude for another type of stability. So, qualitative properties of the solutions can be inferred from a geometrical, or topological, study of the set of all possible solutions.

The historical importance of qualitative methods is better expressed by the second argument. These methods showed to be very useful in providing a new point of view to the problems of Celestial Mechanics, as the three body problem. A *solution* to this kind of problem could be no longer identified with the analytical solution but with the topological description of the set of curves defined by the equation.

In the first section we show how Poincaré described these curves for the first time. It implies a change in the meaning of the word "solution" that gave birth to a new domain of mathematics concerned with "dynamical systems", as named by G.D. Birkhoff. This American mathematician was one of the firsts to notice the power of the new qualitative methods introduced by Poincaré. The generalizations and definitions put forward by Birkhoff constitute an effective new theory (Section 24.1.1). A great number of central concepts used up to now in Dynamical Systems Theory were first defined in a precise mathematical language by him. But the history of this theory is not exactly that of a mathematical theory searching for a rigorous basis, independently of specific problems. Stability problems from Celestial Mechanics have always been the main motivations for its conceptual development (Section 24.1.2).

The problem of solving individual differential equations gained a much more general character from the fifties on. In the second section we show the role the concepts of structural stability and genericity played in a general study of dynamical systems, motivated by classification purposes. The dialogue with physics remained of fundamental importance and the exigencies in this general study express the worries about the legitimacy of mathematical models used in physics. During the seventy's the interplay between mathematicians, theoretical physicians and experimentalists working about dynamical systems started to be very intense, as we tried to show in the collective book (Franceschelli et al., 2007), assembling professional mathematicians, physicians and historians of both sciences.

24.1 Trajectories Defined by Solutions of Differential Equations

The seminal work in qualitative analysis of differential equations is Poincaré's memoir "Sur les courbes définies par une équation différentielle". In the first part, published in 1881, Poincaré studies curves defined in the two-dimensional space that are solutions of an equation of first order and first degree: $\frac{dy}{dx} = \frac{Y(x,y)}{X(x,y)}$,

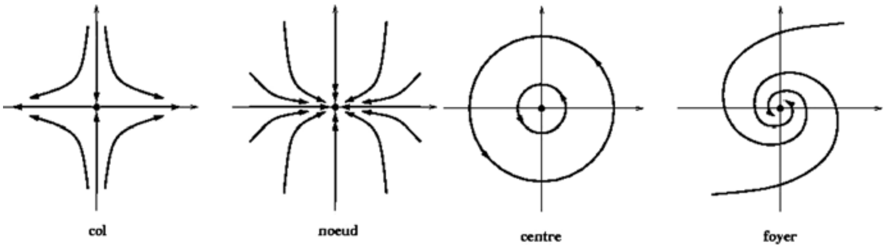

Fig. 24.1 The four types of singularity studied by Poincaré, a saddle, a knot, a center and a focus

where X and Y are polynomials. Generalizing a result of 1879, already treated in his thesis, Poincaré starts analyzing the aspects of these curves in the neighborhood of a singular point. There are four possibilities, represented in the figure above.

These pictures were not traced by Poincaré, but his description is clear enough to enable us to imagine the figures. Poincaré describes these solutions using the values of the roots of an equation depending on the linear part of the development of X and Y. The portraits are obtained by the linear part of the equation since, in the vicinity of singularities, the solution of the nonlinear equation does not change considerably.

The next step was to study the solutions beyond the neighborhoods of singularities, what means to investigate the aspect of the set of solutions when the whole space is considered. For a two dimensional system of differential equations, Poincaré starts separating the domain by what he calls "limit cycles". He concludes that a solution that is not a limit cycle, follow spirals going in the direction of a limit cycle or tend to a singularity. Anticipating the topological character of this kind of analysis, he emphasizes the "topographical" nature of the portrait of solutions obtained in this manner. Extended by the Swedish mathematician I. Bendixson, this result is well known nowadays as "Poincaré-Bendixon Theorem".

In the third part of the memoir, the relationship with Celestial Mechanics becomes more explicit. Poincaré considers solutions in the three dimensional space and redefines the systems of differential equations as to depend on time. He replaces the original equation by an equivalent system of two equations: $\begin{cases} \frac{dy}{dt} = Y(x,y) \\ \frac{dx}{dt} = X(x,y) \end{cases}$.

The independent variable is now always interpreted as being the time and, as a consequence, the solutions will be called "trajectories". As long as Celestial Mechanics is concerned, it is not difficult to imagine the preeminence periodical trajectories acquired. In Poincaré's words, periodical solutions are: "the only breach through which we can try to penetrate into a place so far deemed inaccessible".[3]

[3](Poincaré, 1892, I): p. 82.

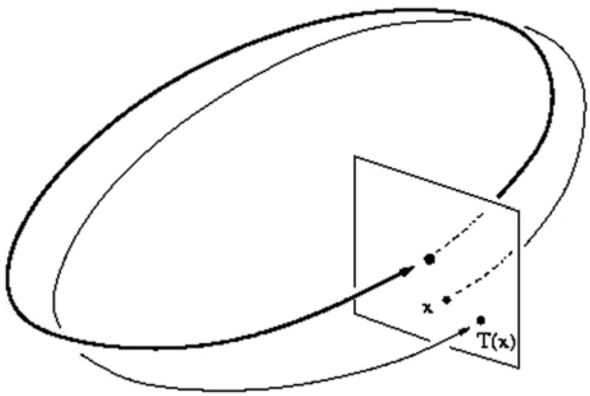

Fig. 24.2 The periodic trajectory cut by a plane at the point x, defining a transformation T(x)

In order to extend his analysis to describe the three-dimensional trajectories of a system of second order, Poincaré starts studying the neighborhood of periodical trajectories. To take profit of the results he had already obtained for two dimensional trajectories, he starts trying to reduce the dimension of the problem. Here appears the most inventive and fruitful methods of the theory: the method of section.

Poincaré cuts the periodical trajectory by an orthogonal plan (now called "Poincaré section") and studies the behavior of the intersections with this plane produced by the trajectories passing through a point in the neighborhood of the periodical point.

A very astonishing result is obtained as a consequence, because the picture of the intersection points on the section is very similar to the picture of two dimensional solutions in the vicinity of singularities (Fig. 24.1): "It's impossible not to be astonished by the analogy presented by the precedent analysis with singularity theory".[4]

The method of sections imply that the dynamical properties of three dimensional trajectories can be studied by means of the dynamics of discrete points, obtained by the iteration of a function in two dimensions. This fundamental idea was explored by Birkhoff to define a dynamical system.

24.1.1 The Definition of a Dynamical System

The first article to name the object of study as a "dynamical system", in instead of a differential equation, was published in French, in 1912, by G. D. Birkhoff:

[4](Poincaré, 1886): p. 204.

"Quelques théorèmes sur les mouvements des systèmes dynamiques". The differential equation, with the independent variable being considered as the time, is now called a "dynamical system".

In a subsequent article, published in 1920, Birkhoff advocates the legitimacy and the interest of qualitative analysis. In the study of a dynamical problem, the type of solution qualitative methods can furnish are fully satisfactory: "The recent advances supplement in an important way the more physical, formal, and computational aspects of the science by providing a rigorous and qualitative background".[5]

A good example of how qualitative information can furnish a "rigorous background" in relation to the more formal aspects of the research appears in another paper Birkhoff published in 1920: "Surface transformations and their dynamical applications".[6] In this work, central for the future of research in this domain, he investigates the properties of the point transformation defined on the Poincaré section of a periodical trajectory. He gives the conditions this transformation may satisfy to reflect all the relevant properties concerning the solutions of a dynamical problem.

So far, a dynamical problem was defined by a differential equation in which the independent variable is interpreted as being the time. The dynamical system is the flux defined by the solutions of the differential equation. Now, a dynamical problem can also be defined by the iteration of a discrete point transformation.

In 1927 Birkhoff publishes a book called *Dynamical Systems*.[7] Here, he explicit enunciates it is equivalent to study the trajectories defined by differential equations and the dynamics of the points obtained iterating a point transformation. This is possible because the properties of trajectories are "mirrored" in the properties of the transformation, specially those we are interested about in qualitative analysis.[8] So, the form of the definition of a dynamical system suffers a "striking" modification.

In (Roque, 2007a) we show that this new definition already implies a qualitative point of view, related to stability problems. The nature of the stability question permit, and legitimate, to neglect the transient behavior of trajectories. Relevant questions, as to know if the trajectories remain confined in some region or if a trajectory returns in the vicinity of its initial point, concern topological properties of trajectories.

24.1.2 The Question of Stability

Some years after his first memoirs on the curves described by differential equations, Poincaré publishes two important works about Celestial Mechanics: the article "Sur les problèmes des trois corps et les équations de la Dynamique" and the book *Les*

[5](Birkhoff, 1920a): p. 5.
[6](Birkhoff, 1920b).
[7](Birkhoff, 1927).
[8](Birkhoff, 1927): p. 143.

méthodes nouvelles de la Mécanique céleste, the first published in 1890 and the second in three parts from 1892 to 1899. Poincaré uses qualitative tools to treat problems of Celestial Mechanics related to the question of stability, as the three body problem.

The stability of the solar system and the stability of the equilibrium figures of rotating fluids are old problems that motivated the development of analytical methods by eighteenth century mathematicians. In (Roque, 2005), we show the evolution of stability concerns after a first polemic discussion involving D'Alembert.

In the eighteenth and nineteenth centuries, stability problems were formulated in terms of differential equations and studied by series developments that express their solutions. But Poincaré noticed these series could not converge, producing a great change in the traditional treatment developed by Lagrange and Laplace.

Using new methods, Poincaré published, in 1890, a memoir in the journal *Acta Mathematica* as the winning work in the international competition about the stability problem, honoring the King of Sweden and Norway, Oscar II. But this paper is different from the one that actually earned the prize and contained a mistake. The history of the erroneous memoir and its correction is analyzed in detail in (Barrow-Green, 1997).

The focus of our research is about the mathematical definitions of stability contained in these works. In the article "Stability of Trajectories from Poincaré to Birkhoff: approaching a qualitative definition", just submitted, we point out how the stability question motivated the introduction of this new approach. Besides that, we try to precise the term "qualitative" often associated to new methods of Poincaré.

We analyze the different stability definitions proposed by Poincaré, Lyapunov, Levi-Civita and Birkhoff, showing that each definition reflect an aspect of the problem under consideration. The definition proposed by Poincaré, saying that a trajectory is stable if it returns as close as we want to the vicinity of its initial point, was still attached to the analytical methods used in the traditional treatment of the stability problem. The new definition proposed by Lyapunov turned out to be the more commonly used until our days, since it fits better with the spirit of qualitative methods and can be generalized to other problems.

Analogous definitions were employed by Levi-Civita and Birkhoff in the study of the three body problem. In 1935, Birkhoff says that Poincaré's definition of stability is not appropriate, his "use of the word 'stability' is, however, unfortunate".[9] The solutions possessing stability in the sense defined by Poincaré must be called "recurrent".

In Birkhoffś view, stability is a flexible concept and its definition must take into account the problem where it is inserted: "The fundamental fact to observe here is that this concept [stability] is not in itself a definite one but is interpreted according to the question under consideration".[10]

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[9](Birkhoff, 1935): p. 310.

[10](Birkhoff and Lewis, 1935).

We often use the adjective "qualitative" to mean that geometry, or topology, is employed to describe the behavior of the solutions of a dynamical problem. But it was just in the beginning of the twentieth that the qualitative nature of the stability problem becomes clearer. Karl Sundman obtains a solution to the three body problem as a series in the variable t that is convergent for all real t. But the problem was not considered to be solved because these series possess a very slow convergence and, above all, does not allow us to infer qualitative information about the behavior of trajectories. Traditional solutions to the problem of stability, first proposed by Lagrange and Laplace, are not useless because they are false, but because they are not interesting enough.[11]

After the works of Poincaré and Birkhoff, but also Lyapunov and Levi-Civita, the properties of the set of solutions considered to be "interesting" are those that permit to obtain qualitative information about the behavior of trajectories. Mathematically speaking, in a first moment, this qualitative information had a topological character.

24.2 The General Study of Dynamical Systems: Structural Stability and Genericity

Since Poincaré's foundational work, the qualitative study of differential equations remained an almost forgotten domain in the field of mathematics, during the first decades of the twentieth century, with few exceptions of isolated mathematicians, as Birkhoff. It was only in the fifties that the theory gained a new impulse.

Despite its fundamental importance, the methods developed by Poincaré only applied for particular types of differential equations. A general study was yet to be done. In 1890, during the International Congress of Mathematicians in Rome, Poincaré announced as follows his perspectives for the future research about differential equations:

> "Much has already been done for linear differential equations, and one only needs to keep the pace with that which is definitely acquired. However, concerning non-linear differential equations, developments have been too modest. Expectations of getting integration on the basis of functions previously known are longtime gone; it is time to study the functions defined by differential equations in themselves, starting with an attempt to systematically classify them. A study of the growth mode in the vicinity of singular points most probably will provide the first elements for such a classification, but we will not be satisfied until a certain group of transformations (such as the Cremona transformations) will be found, playing – vis-à-vis the differential equations – the same role played by the bi-rational transformations group in the case of the algebraic curves".[12]

In fact, the above quotation is a clear demonstration that, in the view of Poincaré, this classification should have the same general character as the algebraic curves

[11] In (Chenciner, 2007) the author explains the solution furnished by Sundman and in which sense it is not considered to solve the problem of stability.

[12] (Poincaré, 1908): p. 180. Our translation.

classification.[13] The fundamental idea if we want to classify algebraic manifolds is that we can view all manifolds that are bi-rationally transformable into each other as a single abstract algebraic manifold. We can interpret Poincaré's assertion as the manifestation of his interest in classifying solutions of differential equations in the same way, but using some other equivalence relation. Yet, the curves that represent solutions of a differential equation are, in the general case, infinitely more complex than algebraic curves. Expectations of classifying solutions of differential equations, as expressed in the above Poincaré's quotation, should be considerably relaxed in order to obtain some generality. So, the equivalence relation must have a topological nature.

In a forthcoming paper about the notion of "genericity", written for the "generality seminar" of the group REHSEIS,[14] we try to show that the tension between the choice of relevant properties and the general characterization goal played a central role in the development of Dynamical Systems Theory in the sixties and seventies. We propose a history of the notion of "genericity" showing how it has developed after the first suggestions of Poincaré.

As long as differential structures are concerned, the Singularity Theory was the first to introduce a notion of genericity (even if in the beginning it was not called like this). The aim was to classify differentiable mappings by their behavior in the vicinity of singularities. A similar classification hope will motivate the development of Dynamical Systems Theory in the fifties and the sixties. In (Roque, 2008) we analyze how Singularity Theory, founded by René Thom, had a decisive influence in the style of dynamical systems classification efforts.[15]

A successful classification program of dynamical systems should be able to define classes of systems with the following characteristics: (1) each class is sufficiently particular to be geometrically well described; and (2) such classifiable systems are sufficiently general to include "almost all" systems in the sense that they constitute an open and dense subspace of the domain of all systems. This last property is the mathematical counterpart of genericity.

The Brazilian mathematician Mauricio Peixoto, influenced by the impact of Set Theory, was convinced that the main goal of mathematics of his times was to classify mathematical objects, with emphasis in their structures and by means of equivalence relations between them.[16] From his point of view, the suggestion given by Poincaré,

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[13]This general character is due to the type of transformations proposed in 1867 by the Italian geometer Luigi Cremona and named after him. Cremona took part in the first generation of the Algebraic Geometry researches made in Italy in the nineteenth century.

[14]Seminar organized in Paris by the REHSEIS (Recherches Épistémologiques et Historiques sur les Sciences Exactes et les Institutions Scientifiques) under the responsibility of Karine Chemla, Renaud Chorlay and David Rabouin.

[15]In his PhD Thesis, David Aubin shows how the interplay between mathematicians working on Singularity Theory and Dynamical Systems, in the Institut des Hautes Études Scientifiques (IHES), influenced the aspect both theories gained (Aubin, 1998).

[16](Peixoto, 2000).

expressed by our initial quotation, could be fulfilled with notions extracted from Set Theory.

Poincaré and Birkhoff's works were certainly the point of departure for such a study, but in order to express their theory in a set-theoretical basis it was still necessary to introduce two new elements[17]:

(1) A space of differential equations, or dynamical systems, possessing a topological structure.
(2) A notion of qualitative equivalence between two differential equations (analogous to Cremona transformations as claimed by Poincaré).

Both problems were solved by Peixoto.[18] He considers a dynamical system as a point of a Banach space and proposes that an equivalence relation between two systems in this space should be a homeomorphism transforming trajectories of one system into trajectories of the other. This last definition is inspired by the work of Andronov and Pontryagin.[19]

In 1937, these two Soviet mathematicians had published a paper called "Systèmes grossiers",[20] in which they studied dynamical systems defined in a two dimensional space and proposed that the trajectories of two systems should be considered to be equivalent if they could be transformed into each other by means of a homeomorphism. A system is called "grossier" (that means "coarse") if its trajectories remain qualitatively similar after a perturbation in the definition of the system. The homeomorphism is precisely the transformation that maintains trajectories "qualitatively similar".

The importance of the coarseness property consists in the role it plays in modeling physical systems. If a system is not coarse, or robust, its fundamental properties are easily lost after a small perturbation. As mathematical models are just idealizations of physical realities, we cannot avoid perturbations in the definition of a mathematical system. Thus, a coarse system is a good candidate to serve as a model for a physical situation.

Around 1950, as Dahan-Dalmedico shows,[21] "grossier" was renamed "structurally stable" by suggestion of Lefschetz. By this time, there were some researchers working on the subject in Princeton, and that attracted Peixoto to join them in 1957. In (Roque, 2007b) we propose a detailed history of structural stability in relation with the development of Dynamical Systems Theory in Brazil.

Andronov and Pontryagin attempted to a mathematical description of two-dimensional structurally stable systems. In his 1959's article,[22] Peixoto showed that

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[17] (Peixoto, 1987).
[18] See (Peixoto, 1959) and (Peixoto, 1962) respectively.
[19] See (Diner, 1992) for a history of dynamical systems in Soviet Union and the role of Andronov.
[20] (Andronov and Pontryagin, 1937).
[21] (Dahan-Dalmedico, 1994).
[22] (Peixoto, 1959).

structurally stable systems, having Andronov and Pontryagin's features, form an open and dense subset in the space of all systems defined on a sphere. It is the first general result in the theory of dynamical systems.[23] Even if it just holds for two-dimensional systems defined on specific surfaces, Peixoto's theorem is a general result: it succeeds in describing the relevant features of *almost all* two-dimensional dynamical systems defined on a sphere.

Peixoto reformulates this result in 1962,[24] using the term "generic" for the first time in the context of Dynamical Systems Theory. Peixoto starts with the following assertion: the fact that structurally stable systems form an open and dense subset of the space of all systems means they are "generic". The question of genericity evolved and gained different mathematical definitions in the subsequent research on dynamical systems. The American mathematician Steve Smale, after knowing Peixoto's works, was the main responsible in trying to generalize them.[25]

Questions involving structural stability and genericity are in strict relation with physical research. This connection is particularly clear in the study of the transition to turbulence states of fluid motions. In (Franceschelli and Roque, 2005) we give an example of how these concepts were used by Ruelle and Takens[26] to furnish a new interpretation of the roads to turbulence, based on strange attractors. The new qualitative point of view introduced by Poincaré can be used, beyond Celestial Mechanics, even in the way physical experiments are constructed.

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[24] (Peixoto, 1962).

[25] See (Smale, 1998) for a historical account of these works, of his relationship with Peixoto and the Instituto de Matemática Pura e Aplicada in Rio de Janeiro (IMPA).

[26] (Ruelle and Takens, 1971).

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Chapter 25
The Problem of Adequacy of Mathematics to Physics: The Relativity Theory Case

Samuel Simon

How can it be that mathematics, being after all a product of human thought which is independent of experience, is so admirably appropriate to the objects of reality? (...) In my opinion the answer to this question is, briefly, this:–As far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality.

Albert Einstein (1921)

It is my conviction that pure mathematical construction enables us to the key to the understanding the phenomena in Nature.

Albert Einstein (1933)

The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve.

Eugene Wigner (1960)

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25.1 Introduction

The relationship between mathematics and nature is a subject that arises with Western philosophy itself. Needless to insist on this relation in Pythagoras', Plato's and Aristotle's thought. However, the relationship between mathematics and physical theories earns a completely new meaning in modern times, particularly with Galileo and Descartes. While they contribute both to mathematics and physics, these two thinkers examine the foundations of the relationship between them. Descartes' mind metaphysics will find an analogue in Galileo's metaphysical realism in the explanation of the use and success of mathematics in describing nature. These two

S. Simon (✉)
Department of Philosophy, University of Brasilia, Brasilia, Brazil
e-mail: samuell@unb.br

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thinkers, more than any other previously, inaugurated a new philosophical problem: the laws of nature are described in mathematical language.

The importance of this formulation has not lost its validity, although the approaches on the foundations of both dominions – laws of nature and mathematical language – are almost as different as the number of thinkers who examined them. However, as it is well known, we can distinguish the philosophers who discussed this subject in two main groups: rationalists and empiricists. While the first group defends certain *a priori* elements (either mental or not) both to the knowing of natural laws and to the constitution of mathematics, the second group champions some kind of *apriorism* only for mathematical knowledge. In the rationalism case, adequacy between the expression of a natural law and the formal language used is, in a sense, easily solved, since the most fundamental contents of the empirical basis and of mathematics are already in the subject's mind (as Descartes thought) and find their correspondent in nature or else there would be a correspondence between the mathematical structure of nature and the equations which account them (as defended by Galileo). Empiricists as Locke and Hume deny the existence of immutable laws and inaugurated a problem that will be retaken only in the nineteenth century through other ways: the possibility of theoretical change. A century before, Kant showed considerable difficulties to the this program, not only because of the immutability of pure intuitions and understanding concepts, but also because Newtonian physics, which Kant intended to ground philosophically, arises, thanks to its large scope and precision, as one of the most consolidated forms of science ever realized by the human spirit.[1]

Non-Euclidean geometries, thermodynamics, electromagnetism – only to mention fields in mathematics and physics – in middle and late nineteenth century, as well as Quantum Mechanics in the following century, will present serious difficulties to the Kantian program and to modern rationalism. Mach's empiricism, followed by logical empiricism and twentieth century's antirealism, show close relationship with the problem of theoretical change. In this sense, the idea of concepts as causality, understood in the sense of classical physics, will be strongly threatened, and will bring some new elements to the empiricists theses. In this context, Relativity Theory, in both its special and general formulations, has a different status, since on some philosophical interpretations it strengthens the realistic thesis, and in consequence some form of realism. With general relativity, as we shall see, this problem acquires a deeper meaning, since the presence of mathematics in the construction of a physical theory becomes central.

25.2 Interactions Between Physics and Mathematics

In a recent study on the interactions between physics and mathematics, Dominique Lambert (1996) presents some noticeable cases of interaction between these

[1] However, Newton recognized the possibility of theoretical change before modern empiricists. See Loseee, 2001, p. 85. In a certain way, in his *Preface* to the *Traité du Vide*, Pascal anticipates the notion of theoretical change by means of what he called "progress" of reason and experience.

two domains.[2] On this we could quote the following examples: spinors theory, connections theory in differential geometry as well as in potential theory and Brownian movement (Lambert, 1996, pp. 30–85). In this context, there are two aspects in the interaction among theories in physics and mathematics: in the first one, mathematical theories are fully developed already and are used by physics (the case that interests us). There is however, the inverse case, which is not less important: in order to establish a physical theory, a new mathematics is needed. Newton's work on differential calculus is certainly the most admirable and fruitful example of this physics.

The relations between mathematics and physics were studied in the twentieth century by numerous philosophers; we shall quote some of them for example. Popper, in *Conjectures and Refutations* dedicates a brief chapter to this question. His conclusion, very similar to Einstein's (see epigraph), is linked, on the one hand, to his attempt of keeping the *falseability* criterion, the same regarding mathematics, and, on the other hand, his commitment to a naïve realism, as he asserts in certain occasions.[3] In an article published in 1980, Zahar analyses the role played by mathematics in scientific discovery and stresses the need of interpreting mathematics.[4] The same problem is examined by Mark Steiner, who emphasizes the use of mathematics as an analogical resource for discovery.[5] Paty underlines the historical character of adequacy, which must be legitimated in each case.[6] On the creative process in mathematics itself and its relation to experience, we cannot forget Poincaré's pioneer works.[7]

[2]Lambert carries out an important and vigorous study on the interactions between physics and mathematics. As regards strictly to the problem of adequacy among the theories of these domains, this author points out to a "platonic naturalism" ("En effet", writes he, "nous considérons que l'activité mathématique ne peut se comprendre en faisant fi de la condition proprement biologique du mathematician. Les "objets mathématiques" sont des "objets mentaux" produits par un cerveau en lien avec un corps mobile et sensible. (...) Il peu paraître paradoxal à première vue de prétendre, ainsi que nous l'avons soutenu (...) que notre position se rapproche d'une certaine manière de Platon" (Lambert, 1995–96, p. 464).

[3]"In so far as a calculus is applied to reality, it loses the character of a *logical* calculus and becomes a descriptive theory *which may be empirically refutable*; and in so far as it is treated as irrefutable, i.e. as a system of *logically true* formulae, rather than a descriptive scientific theory, it is not applied to reality". Popper, 1965, p. 210. Highlighted by Popper. This perspective had already been enounced by Einstein in his article about the relations between physics and geometry. Cf. Einstein, [1921], 1934.

[4]"Through physically interpreting a hitherto uninterpreted – or rather a seemingly uninterpretable – mathematical entity like $t^{\nu}{}_{\mu}$, Einstein was led to a physical discovery" ZAHAR, 1980, p. 39. This aspect is emphasized by various authors, mainly Paty, as we shall see below.

[5]The thesis is this: an indispensable factor in contemporary physical discovery (...) has been the use of mathematical analogies; for example, physicists attempting to discover some physical description restricted their search to descriptions with the same mathematical properties as known, successful description. Steiner, 1989, p. 452.

[6]"L'application des mathématiques à l'étude des phénomènes de la nature s'effectue par un travail patient d'élaboration, qui en prépare les conditions et s'assure de sa légitimité dans chaque cas considère". Paty, 1994, p. 423.

[7]In *La science et l'hypothèse*, Poincaré asserts the following: 'L'esprit a la faculté de créer des symboles, et c'est ainsi qu'il a construit le continu mathématique, qui n'est qu'un système particulier de

Scientists, such as Dirac, Weyl, Wigner and Einstein, come even to consider this relationship as surprising. A known article by Wigner brings in its title part of his conclusions: "The Unreasonable Effectiveness of Mathematics in the Natural Sciences". However, Wigner insists in the "miraculous" character of this adequacy.[8] Wigner raises this adequacy to an "epistemology law".[9] However, according to him, the most fundamental reason is found in the choice of convenient mathematical concepts, having in view covariance.[10] Dirac highlights "mathematical beauty" and establishes it as an epistemological guide to empirical research.[11] By means of emphasizing the presence of symmetries, particularly in their relations to transformation groups, Weyl establishes an identification between objectivity an invariance. This identification will be crucial in the determination of the most adequate transformation group for expressing this objectivity.[12]

Einstein's reflections on the relationship between mathematics and physics will be examined at the end of this work. It is worth noticing, for the moment, that he changes his conceptions about this relationship. By the time of the Special Relativity Theory creation, Einstein seems to have considered mathematics as merely a (fundamental) language for presenting physical theories. Many years later, he reveals his surprise regarding adequacy, and draws a favorable conclusion to the heuristic importance of mathematics on physics.

We shall see that the aspects pointed out by these scientists are important elements in the explanation of this adequacy; in addition, by means of examining the use of mathematics in the development of Relativity Theory, we shall seek to underline aspects that were overlooked by these authors.

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symboles. Sa puissance n'est limitée que par la nécessité d'éviter toute contradiction; mais l'esprit n'en use que si l'expérience lui en fournit une raison' (Poincaré, [1902] 1968, p. 55).

[8] "It is difficult to avoid the impression that a miracle confronts us here, quite comparable in its striking nature to the miracle that the human mind can string a thousand arguments together without getting itself into contradictions, or to the two human mind's capacity to divine them". Wigner, [1960], 1979, p. 229.

[9] "I propose to refer to the observation which these examples illustrate as the empirical 'law of epistemology'". Ibid., p. 237. Wigner refers here to three examples examined by him: planetary motion, matrix mechanics and quantum electrodynamics, or the theory of the Lamb shift.

[10] "The preceding three examples (...) should illustrate the appropriateness and accuracy of the mathematical formulation of the laws of nature in terms of concepts chosen for their manipulability, the 'laws of nature' being of almost fantastic accuracy but of strictly limited scope. (...) Without the laws of invariance the physical theories could have been given no foundation of fact (...)" (Ibid. p. 233).

[11] "We now see that we have to change the principle of simplicity par *principle of mathematical beauty*. The research worker, in his efforts to express the fundamental laws of Nature in mathematical form, should strive mainly for mathematical beauty. He should still take simplicity into consideration in a subordinate way to beauty". ([Dirac, 1938–1939, p. 124).

[12] "We found that objectivity means invariance with respect to the group of automorphisms. Reality may not always give a clear answer to the question what the actual group of automorphisms is, and for the purpose of some investigations it may be quite useful to replace it by a wider group" (Weyl [1952] 1989, p. 132).

25.3 Relativity Theory: Mathematical Foundations and Physical Interpretation

Special Relativity Theory arises along a set of results presented by Einstein in his 1905 article. Although it is implicitly shaped as space-time theory, it is initially a theory that searches the explanation of physical asymmetries, which do not look intrinsic to phenomena, resulting from Maxwell's equations in its application to bodies in motion. From the principle of relativity ("in all coordinates systems in which the mechanics equations are valid, the laws of optics and electromagnetism are also valid"), from the postulate of light speed constancy in vacuum independently of sources motions postulate, from the concept of observer/event, from a redefinition of simultaneity, from the analysis of co-ordinate transformation in the passage from a still system to another one which move relatively to the first in uniform translation, from all this one obtains the explanation of this symmetry – in fact it disappears – founded in these transformations, which had been obtained by Lorentz already, but which were not known to Einstein.

Years later Einstein presents Relativity Theory in a slightly different manner. As he writes right in the beginning of *The Meaning of Relativity*[13] (Einstein, 1950, p. 1), this theory is intimately linked to the conceptions of space and time developed by geometry and classical physics. Quoting Poincaré, Einstein reminds that the foundations of geometry rest on the "position changes", that is, changes we can carry out in our bodies in order to compensate changes we observe in solids. In this context, the space notion corresponding to a body must prevail over notion of space in itself, being the latter a consequence from the fact that we forget we are on earth's surface. The notion of reference space has then to take the place of space in itself. With the former notion, one can then define space tridimensionality: a set of three numbers that is associated to a given point. The continuous motion of this point corresponds to a position change. Considering Euclidian geometry as valid to describe the movement of material points, we can conclude for the inexistence of privileged (or absolute) direction in reference spaces, bringing out only the relationship among the directions, which correspond to the "principle of relativity in relation to motion". As we shall see later, by means of tensor calculus, one can show that the equations describe the laws of nature that conform to this principle. It is only after this that we have the "special relativity principle", which expresses the covariance of natural laws for all inertia systems. At the end, we have the principle of speed light constancy in vacuum and the definition of simultaneity for each *event*.

This new way brings out the importance of invariants and principles for understanding the use of mathematics in Relativity Theory. In fact, the invariant s^2 given by the addition

[13] The original text of this work results from a series of four lectures given by Einstein at Princeton University in 1921, whose first edition was published in 1922. The second edition was published in 1945 and the third one in 1950, both of them with important additions. The 1945 one brings some developments of the theory after 1921, particularly regarding cosmological problems, and the 1950 one brings a general theory of gravitation.

$$s^2 = \Delta x_1^2 + \Delta x_2^2 + \Delta x_3^2, \tag{1}$$

defines a reference space as Euclidian and the coordinates as Cartesian. The extension to Special Relativity, as it is known, includes variable Δx_4 given by $c\Delta t$. The new invariant will be then given by the addition

$$s^2 = \Delta x_1^2 + \Delta x_2^2 + \Delta x_3^2 - c^2\Delta t^2, \tag{2}$$

for, given a light ray that propagates in empty space from a point P1 to a point P2, we have in this referential

$$r^2 = c^2\Delta t^2, \text{ where } r^2 = \Delta x_1^2 + \Delta x_2^2 + \Delta x_3^2, \text{ that is, } r^2 - c^2\Delta t^2 = 0$$

For a new referential, which moves uniformly in view of the first, we have

$$r'^2 = c^2\Delta t'^2,$$

where $r'^2 = \Delta x'^2_1 + \Delta x'^2_2 + \Delta x'^2_3$ and so, $r'^2 - c^2\Delta t'^2 = 0$, in view of constant value c for light speed in vacuum.

The passage from a referential to another will be possible by means of Lorentz' transformations, which will show the mutual dependency of space and time, by which is defined a new concept: the space-time. There is so a denial of the absolute character of space and time separately, and the emergence of a new concept: the absolute space-time.[14] With Lorentz's transformations assuring the invariance of $s^2 = r'^2 - c^2\Delta t'^2$, Einstein attributes a physical meaning to these equations, compatible with former principles. We think important to insist on this point, since, as Einstein himself observes, the character of relativity was already present in Newtonian mechanics (the relativity of reference spaces), since the assertion that two non-simultaneous events take place at the same point does not have an absolute meaning (Einstein, 1950, p. 30). However, this relativity does not matter to the theory construction. In fact, the notion of event was not present in Newtonian physics either. Only with Relativity Theory can we talk of a happening in space-time.

In this way, s^2 shows up as an invariant in the constitution of Relativity Theory given by equation (2), once it is adopted the constant value of light speed in vacuum (and so, also a constant). In the sequence, covariance – that is, the form of equations consolidated by experience – should be kept during the referential changes. In other words, in the wide sense defended by Wigner and Weyl, the invariants play a central role in the edification of this theory. We shall see that these elements will also be constitutive in the edification of General Relativity Theory.

[14]"So from the standpoint of the special theory of relativity we must say, continuum '*spatii et temporis est absolutum*'". Einstein, 1950, p. 55.

It is worth insisting on these two concepts: invariant and covariant. The invariant s^2 (equation 1), present in Euclidian geometry, shows the independency between space and time coordinates when this geometry is used for accounting physical processes. This results from the consideration of ideal solid bodies behavior as regards their motion and their relative position. Einstein insisted that experience shows the validity of these suppositions, as well as the constant value for light speed in vacuum. However, in both cases, the theoretical formulation (equations and concepts) emerge as a safe guide for any theoretical expansion. This is the meaning of Einstein's statement (1950, p. 27), according to which "the consequence of the Maxwell-Lorentz equations that in a vacuum light is propagated with the velocity *c*, at least with respect to a definite inertial system K, must therefore be regarded as proved". In other words, the fact that the equations form has been maintained situates the experience value (physical measures), but establishes the theoretical precedence. Apart from this, as pointed out above, the critique of the simultaneity concept of Newtonian physics, made possible by the postulate of constant light speed in vacuum postulate and the definition of event/observer[15] complete the theoretical structure of Special Relativity. Without these redefinitions and without an "invariant theory"[16] a new space-time concept would not be possible.

Two other consequences of Special Relativity were important to the edification of General Relativity Theory: the concept of proper time, and the equivalence between mass and energy. The equality between mass and energy given by the equation $E = mc^2$ took naturally to the problem of interaction between matter and light, questioning this way the constant value of light in regions next to gravitation fields. In 1907, Einstein presents the first results of this investigation and, from this equality, inquiries on the equality between inert mass and gravitational mass, a question retaken in 1911. Paty (1993, pp. 187–226) presents in a precise and clear way this "long path" that separates the special and the general formulation, and remembers the importance of covariance in the first moments of this investigation. In fact, the covariance of natural laws has constituted, at the same time, both a starting and an arrival point in the elaboration of General Relativity. Einstein starts with this presupposition in 1911, abandons it in the following years and retakes it in 1915, when he incorporates the use of tensor calculus in a fruitful collaboration with his friend and colleague at Federal Polytechnic Institute (today ETH), Marcel Grossmann.[17]

[15]The observer in Relativity Theory has a different status from Quantum Mechanics. The photography of an event (a point at space-time), for example, may be an observer in this theory.

[16]"The whole theory of invariants of the special theory of relativity depends upon the invariant s^2 [given here by equation (2)]" (Einstein, 1950, p. 37). It seems that Einstein hesitated as regards the name given to Relativity Theory, having thought initially in 'Invariance Theory', but 'Relativity Theory' ended up coming into effect, since the first name would emphasize the method but not the content of the theory". Paty, 1993, p. 182.

[17]In a recent study about the so-called Zurich Notebooks, edited by Jürgen Renn (2007), the authors show this difficult path between 1912 and 1915. In this study, it becomes clear that there was not a stagnation period at this moment, as one used to suppose so far, but a time of intense work. See Renn, 2007, p. 5.

Although it looks clear that covariance (and invariance) is the great conducting wire in Einstein's work in this period, other elements arise as determining as regards the mathematics used in General Relativity Theory. As pointed out above, the starting point was the questioning of the light speed constancy postulate. Two other concerns arise at this moment: the dislocation of light spectrum to the red near a gravitation field, and the secular advance of planet Mercury perihelion. Experiments carried out to verify these phenomena presented a surprising agreement, exactly as predicted by General Relativity. In any way, the "equivalence principle", considered by Einstein his most important idea, will be the key to the solution of the covariance problem.[18] Still in *The Meaning of Relativity*, Einstein presents didactically the place of the latter principle in the determination of the relations between the gravitational field and the geometry needed for reaching covariance. Following the Galilean method of "thought experiments", Einstein examines the behavior of a disc in rotation movement (K' system) in relation to a system of inertia K. Placing a series of P rigid bars along the perimeter and being D the number of bars along the diameter of this disc, it is obtained from Special Relativity Theory, the contraction (Lorentz's one) in the direction of the rotation movement. As a result, if in system K

$$\frac{P}{D} = \pi \tag{3}$$

In system K', in rotation movement in relation to K, we will have

$$\frac{P}{D} > \pi \tag{4}$$

We see this way that Euclidian geometry cannot be used to describe the latter system. Bearing in mind that the equivalence principle announces that a referential such as K' may be considered as "at rest" where the gravitational field actuates, we can conclude that this field exerts an influence on the space-time[19] continuum and changes its metric.

This way, for an infinitesimal element, the distance ds of two neighbor points is given by

$$\mathrm{ds}^2 = \mathrm{dx}_1^2 + \mathrm{dx}_2^2 \tag{5}$$

The geometry of surface with continuous curvature may be constructed in an analogous way. An infinitesimal portion of the surface may be considered as flat. As

[18] As it is well known in the literature, Mach's principle also had an important role in the first reflection that took to General Relativity. Other two principles appear in the literature: the minimal gravitational coupling principle and the principle of correspondence, which we call theoretical limit, as we shall see. For the mathematical detail of the role of the five principles – including the principle of covariance – in the edification of General Relativity, see D'Inverno, 1996, pp. 120–132. This author reminds that Einstein did not state explicitly the principle of minimal gravitational coupling. Ibid. p. 132.

[19] The field influence over watches is also clear, since these ones will have their pace changed along the diameter in system K'.

a result, there are, for this portion, coordinates X_1 and X_2 such that the distance between two points will be given by the relation

$$ds^2 = dX_1^2 + dX_2^2. \tag{6}$$

IF we trace on the latter surface any curvilinear coordinates x_1 and x_2, X_1 and X_2 may be expressed linearly as functions of dx_1 and dx_2. Equation (6) will be given then by

$$ds^2 = g_{11}dx_1^2 + 2g_{12}dx_1dx_2 + g_{22}dx_2^2, \tag{7}$$

where the g_{ij} are determined by the surface nature and the coordinates choice.

For a four-dimensional continuum, considering the equivalence principle, we can write for small portions of this continuum

$$ds^2 = dX_1^2 + dX_2^2 + dX_3^2 - dX_4^2. \tag{8}$$

For finite extensions where the gravitational field actuates, we have, from (7) and (8), in general

$$ds^2 = g_{\mu\nu}dx^\mu dx^\nu, \tag{9}$$

where the indexes μ,ν vary from 1 to 4.

In other words, functions $g_{\mu\nu}$ describe, regarding the coordinate system arbitrarily chosen, both the metric conditions in the space-time continuum and the gravitation field.

Equation (9) acquires an important position in General Relativity Theory. As we shall see, it allows describing the movement equations of a body submitted to a gravitational field. This equation will also be crucial to determine the field general equations, that is, the relations between matter-energy and geometry. In order to obtain these equations, Einstein uses Riemannian geometry, tensor calculus and absolute differential calculus in the terms developed by Ricci and Levi-Civita.[20] We will come back to the reasons why we consider important to the adequacy between this calculus and the physical phenomena observed. For now, we shall see, in summary, Einstein's following steps to find the equation of the gravitational field in its relation to geometry.

Having the Euclidian geometry as a guide, the generalization will conduct to the relations between the invariant ds^2 and the metric $g_{\mu\nu}$. As Einstein asserts, magnitudes $g_{\mu\nu}$ determine all metrical properties of the continuum.[21] So they stipulate the invariance of vectors in a surface whatsoever. Following the steps of Levi-Civitta,

[20]As observed by Janssen, in analyzing Einstein's papers in the so-called Zurich Notebooks, the calculus of Mercury perihelion determined the choice of covariance and Ricci's tensor. Janssen, 2007, p. 831.

[21]"The most important point of contact between Gauss's theory of surfaces and the general theory of relativity lies in the metrical properties upon which the concepts of both theories, in the main,

Einstein showed that it is possible to apply to the tensors the differential operations of invariance. This is done in the following way. Given that only quantity $g_{\mu\nu}$ A^{μ} A^{ν} is an invariant, we have

$$0 = \delta(g_{\mu\nu}A^{\mu}A^{\nu}) = \frac{\partial g_{\mu\nu}}{\partial x_{\alpha}}A^{\mu}A^{\nu}dx_{\alpha} + g_{\mu\nu}A^{\mu}\delta A^{\nu} + g_{\mu\nu}A^{\nu}\delta A^{\mu}. \tag{10}$$

On its turn, the notion of parallel transport developed by Levi-Civitta allowed to obtain the coordinates of a vector dislocated from a point P1 to a point P2, along the straight line dx_{ν}.

The value of δA^{ν} will not be null and will be given by

$$\delta A^{\nu} = -\Gamma^{\nu}_{\alpha\beta}A^{\alpha}dx_{\beta}. \tag{11}$$

From the expressions (10) and (11), we have

$$\left(\frac{\partial g_{\mu\alpha}}{\partial x_{\alpha}} - g_{\mu\beta}\Gamma^{\beta}_{\nu\alpha} - g_{\nu\beta}\Gamma^{\beta}_{\mu\alpha}\right)A^{\mu}A^{\nu}dx_{\alpha} = 0. \tag{12}$$

Having in view certain symmetry conditions of the term $\Gamma^{\alpha}{}_{\mu\nu}$, one obtains

$$\begin{bmatrix} \mu & \nu \\ \alpha & \end{bmatrix} = g_{\alpha\beta}\Gamma^{\beta}_{\mu\nu}. \tag{13}$$

Having been adopted Cristoffel's symbol

$$\begin{bmatrix} \mu & \nu \\ \alpha & \end{bmatrix} = \frac{1}{2}\left(\frac{\partial g_{\mu\alpha}}{\partial x_{\nu}} + \frac{\partial g_{\nu\alpha}}{\partial x_{\mu}} + \frac{\partial g_{\mu\nu}}{\partial x_{\alpha}}\right). \tag{14}$$

Multiplying equation (13) by $g^{\sigma\alpha}$ and adding it in relation to $g^{\sigma\alpha}$, one has

$$\Gamma^{\alpha}_{\mu\nu} = \frac{1}{2}g^{\sigma\alpha}\left(\frac{\partial g_{\mu\nu}}{\partial x_{\alpha}} - \frac{\partial g_{\nu\alpha}}{\partial x_{\mu}} - \frac{\partial g_{\mu\nu}}{\partial x_{\alpha}}\right), \tag{15}$$

the second species Cristoffel's symbol.

Once quantities $g_{\mu\nu}$ determine all of the continuum metric properties, also quantities $\Gamma^{\alpha}_{\mu\nu}$ do so.

Cristoffel's symbol makes for the basis of the subsequent developments that will define the fundamental equations of General Relativity Theory.[22] With the concept of parallel transport – which lays down the ways for keeping the direction and the

is based" (Einstein, 1950, p. 61). This idea is basically the same enounced by Einstein in an article of 1914, when the theory was still in faze of conclusion, although in this article he emphasizes the role of invariant ds^2. Einstein, [1914] 2007, p. 612.

[22] Janssen and Renn (2007) consider that the acknowledgment of Christofell's symbol as representing gravitational field was decisive in the constitution of the theory.

norm of a vector along a curve in a given surface or, in more technical terms, how to dislocate a mathematical object from a point to another in a variety – one can construct a geodesic line, whose successive elements result from each other via parallel transport. Being this a generalization of the Euclidian geometry straight line, we have

$$\delta\left(\frac{dx_\mu}{ds}\right) = -\Gamma^\mu_{\alpha\beta}\frac{dx_\alpha}{ds}dx_\beta, \tag{16}$$

which allows obtaining the movement equation of a particle under the effect of inertia and a gravitational field, once the principle of equivalence is admitted:

$$\frac{d^2x_\mu}{ds^2} + \Gamma^\mu_{\alpha\beta}\frac{dx_\alpha}{ds}\frac{dx_\beta}{ds} = 0, \tag{17}$$

where the set of $\Gamma^\mu{}_{\alpha\beta}$ plays the role of intensity of the gravitation field.

These were basically the steps followed by Einstein to obtain equation (17). The question that he presents himself is whether this equation really describes the movement of a particle under the action of inertia and gravitation. The answer is positive if, in a first approximation, it satisfies the same field laws as Newton's theory of gravitation. At this moment, an element was decisive for completing the use and adequacy of non-Riemannian geometry to General Relativity, apart from the classical adequacy: covariance. As Einstein himself asserts: "the unity of inertia and gravitation is formally expressed by the fact that the first member of the equation (17) has a character of a tensor" (Einstein, 1950, pp. 81–82). However, taken separately, it does not have a tensor character ($\Gamma^\mu{}_{\alpha\beta}$ is not a tensor).

Einstein's next steps complete his search. Initially, one obtains the Riemann-Christoffell fourth-order tensor:

$$R^\mu_{\sigma\alpha\beta} = -\frac{\partial\Gamma^\mu_{\sigma\alpha}}{\partial x_\beta} + \frac{\partial\Gamma^\mu_{\sigma\beta}}{\partial x_\alpha} + \Gamma^\mu_{\rho\alpha}\Gamma^\rho_{\sigma\beta} - \Gamma^\mu_{\rho\beta}\Gamma^\rho_{\sigma\alpha}. \tag{18}$$

Next, considering Poisson's equation of the Newtonian theory

$$\Delta^2\Phi = 4\pi\,\rho \tag{19}$$

as a "model",[23] the field equation must be a tensorial equation, which contains the gravitational potential tensor $g_{\nu\mu}$ taking into account the following conditions:

1. It may contain no differential coefficients of the higher as regards to the second.
2. It must be linear and homogeneous in these second differential coefficients.
3. Its divergence must vanish identically,[24]

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[23]Einstein, 1950, p. 82.

[24]Einstein's tensor satisfies the contracted Bianchi identities $\nabla_\mu G^\mu{}_\nu \equiv 0$.

One obtains the differential tensor that will occupy the left hand of the equation. Still in the analogy with Poisson's equation, this tensor must be equal to the density tensor of matter-energy (the energy tensor of matter), which takes to the field equation

$$R_{\nu\mu} - \frac{1}{2} g_{\nu\mu} R = -kT_{\nu\mu}, \tag{20}$$

which satisfies the principles of general relativity and general covariance (the laws of physics must have the same form for any coordinate systems whichever).[25] As Einstein clarifies, "the first two of these conditions are naturally taken from Poisson's equation" (Einstein, 1950, p. 84).

25.4 The Adequacy of Mathematics to Physics: Concepts, Laws and Principles

The results obtained by the Relativity Theory in its both formulations are well known. It is so particularly regarding its generalized formulation, in the determination of phenomena which had so far no acceptable explanation.[26] It seems possible for us to assert that the decisive elements for the success of General Relativity, taking into account the relations between physics and mathematics, were the following. Initially, a long genealogy of concepts, mainly the ones of continuity, relative movement, coordinate system, differential calculus, added to other categories such as observer/event, simultaneity, proper time, and above all added to the principles, especially with the generalization of covariance and the use of invariants. Apart from this we should also consider its theoretical limit (the principle of correspondence). In this case, both Newtonian equations of gravitation and particles movement, and Special Relativity Theory itself are the bases for the edification of General Relativity. Lastly, the presence of mathematical concepts of curve surfaces (manifolds,[27] in a general form) and the mathematical properties of vectors (or mathematical objects on these surfaces), as well as the concept of parallel transport and the application of invariance differential operations to tensors, all this took to the equation for the motion of a particle under the action of inertia and gravitation. One

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[25] As observed by Stachel, although in the 1913 article Einstein and Grosmann abandon covariance, which will be retaken only in 1915, Grosmann had already found that Ricci's tensor is almost only the rank-two generally covariant tensor of differential order, which can be formed from the metric tensor and its derivatives. Stachel, 1989, p. 66. In this article, Stachel points to the reasons that took to the abandon and then the retaken of covariance. Einstein and Grosmann's 1913 work became known as the *Entwurf* theory or the *Entwurf* paper.

[26] The three problems formerly shown – see page 6 above – were adequately solved by General Relativity Theory. There is an extensive literature on the compatibility between this theory and the experimental results found. See, for example, Will 1986.

[27] "Manifold is something which "locally" looks a bit of n-dimensional Euclidean space $\mathbf{R}^n$". D'Inverno, 1992, p. 56.

should not forget on this development Einstein's criticisms to the place occupied by coordinates in the constitution of the field equation, that is, the criticism to the privilege of inertia systems, still present in Special Relativity (Einstein [1949], 1970, p. 67).[28] As is well observed by Paty (2006, p. 9), all this development has as its basis the "physical meaning" of mathematical concepts, which allows this adequacy and, in the General Relativity case, the "drag along of physics by mathematics", takes to the theoretical novelty (ibid. p. 15).

However, it still remains the question: why the mathematics employed in General Relativity Theory bears this power of "dragging along"? We totally agree with Paty that the answer has to take into account the physical interpretation of mathematical objects, but this interpretation must find something, given beforehand, which will be incorporated by physics (nature has to "accept" the proposed mathematics, so to speak). In the case of General Relativity Theory, the equivalence principle allowed the physical interpretation of space, but not another one (as Lobachevsky's, for example). That is, it looks that certain mathematical structures are more convenient to physics than other ones, which seems to evidence the presence of some dissimulated physical content in certain mathematical theories. In the case of General Relativity, these mathematical concepts seem to be the ones of *continuous surface* (or curvature, in a general form) – already foreseen by Gauss and Riemann, who searched for an effective space geometry – and, lastly, *invariant*, which allowed to express general covariance in an non-equivocal way, by means of tensor calculus. The path stepped by physics in the early twentieth century allowed finding the answer to the question made by Gauss and Riemann about space structure.

The discussion above furnishes the arguments that permit to explain the adequacy of Riemannian geometry to General Relativity Theory. Starting from the concept of multiple dimensional magnitude, and the notion of continuous dimensional manifold variety *n*, Riemann carries out a generalization of Gauss's ideas for spherical surfaces.[29] Riemann's second order tensor preserves the notion of continuous variety, which, in Relativity Theory is expressed by the four-dimensionality of space-time and by the equations tensor character, which satisfies the covariance requirement.[30] In other words, theories and concepts already developed (or

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[28] In fact, the resulting conception after General Relativity is of "spatio-temporal coincidences", as Einstein asserts in a letter to Ehrenfest in 1915. "The physical real in what happens in the word (as opposed to what depends on the choice of the reference system) consists of spatio-*temporal coincidences*". *Apud*. Stachel, 1989, p. 86.

[29] Riemann, [1854] 1953, p. 279.

[30] The following Einstein's assertion expresses this idea fundamentally: "I have learned something else from the theory of gravitation: No ever so inclusive collection of empirical facts can ever lead to the setting up of such complicated equations. A theory can be tested by experience, but there is no way from experience to the setting up of a theory. Equations of such complexity as are the equations of the gravitational field can be found only through the discovery of a logically simple mathematical condition which determines the equations completely or [at least] almost completely. Once one has those sufficiently strong formal conditions, one requires only little knowledge of facts for the setting up of theory; in the case of the equation of gravitation it is the four-dimensionality and the symmetric tensor as expression for the structure of space

principles, like the equivalence one) are tested by experience (as the equivalence between inert and gravitational mass, the covariance of natural laws and the concept of field), and are solid and fundamental starting points for a future theory.

However, as Einstein underlines, mathematics has an important role in the edification of Relativity Theory. It is in this sense that the mathematical concepts listed above, according to their expression in Riemannian geometry, permit their use in a physical theory. This was possible because Riemann, starting from Gauss's enquiries, turned to the space structure itself. We can then say that what *we conceive* as surface, curvature and invariant, apprehended and generalized by Riemannian geometry and the differential and tensor calculus, is found again by General Relativity Theory when this one turns to the phenomena structure. Riemann, anticipating Einstein, showed that these concepts are fundamental when we aim to determine space properties, not being a priori defined any more, in the Kantian sense. We have to express them mathematically and the Euclidian geometry will end up being just a limit case. Although the Euclidian space is a particular case of a tridimensional magnitude, it is a kind of guide for generalization, since, for Riemann, the notion of space has a close relation to experience.[31]

As regards the relations between geometry and experience, Einstein's thought gets close to Riemann's: General Theory of Relativity allows finding the elements that determine the nature of space and time, which now depend directly on physics. In his article "Geometry and Experience", Einstein proposes an "equation", which would define this relationship and which would be (G) + (P), where G stands for a geometry and P for the physical laws. This "equation" would define the relation between physics and geometry, and would be submitted to the test of experience.[32]

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which, together with the invariance concerning the continuous transformation-group, determine the equations almost completely". Einstein, [1949] 1970, p. 89.

[31]"I have in the first place, therefore, set myself the task of constructing the notion of a multiply extended magnitude out of general notions of magnitude. It will follow from this that a multiply extended magnitude is capable of different measure-relations, and consequently that space is only a particular case of a triply extended magnitude. But hence flows as a necessary consequence that the propositions of geometry cannot be derived from general notions of magnitude, but that the properties which distinguish space from other conceivable triply extended magnitudes are only to be deduced from experience". Riemann, 1953, pp. 272–273. It is important to remember that the strict relation between space structure and "experience" was acknowledged as relevant in Riemann's time only by his English translator, Willian Kingdon Clifford. Clifford proposed in 1870 that matter and its modifications were manifestations of space curvature. Cf. Jammer, 1957, p. 160.

[32]Einstein, [1921] 1934, p. 6. By employing Einstein's idea of enouncing an equation that represents the link between geometry and physics, we would include the role played by the principles in the constitution of a new theory (**T**). In the case of General Relativity Theory, we would have something like **G(Cm) + Pr(Cm/f) = T(Cf)**, being **G** the geometry employed (in this case the Riemannian one) and **Pr** the set of five (or four) principles that allow the connection between geometry and its concepts and physics (see note 18). In the case of Relativity Theory, (**Cm**) would be the concept of continuous curve surface (in the terms of absolute tensorial and differential calculus, in the context of Riemannian geometry; in this case, $\mathbf{R}_{\mu\nu}$); **Cm/f** the concept (link) of continuous curve surface, which with its physical meaning stands for the gravitational field ($\mathbf{g}_{\mu\nu}$),

In other words, a scientific statement, in Einstein's view, has no *a priori* warrant whatsoever, not only due to the need of experimental check – Einstein goes further in Hume's problem – but, above all, because it is a free creation[33] ("hypotheses" according to Riemann). Relativity Theory is, in great measure, a physical theory of space, which starts from mathematical studies, but acquires its final form only with physics. It is a "practical geometry",[34] which begins with Euclid, carries on with Gauss and Riemann, but can only evolve to a synthesis with the General Theory of Relativity.

Lastly, if space-time is not curve *in itself*[35] – for it must go through our "conceptual system"[36] – the link-concepts (curvature, continuous surface, covariance) which will define the physical structure of space-time, answer for our mental and physical relations with "external reality". These concepts are the starting point of physical discovery or else for the *rediscovery* of nature's structures begun by a mathematical theory and interpreted by a physical theory.

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which would permit the transition from mathematics to physics. Lastly, we would have the **Cf** term, the "generalized" concept of field, that is, the momentum-energy tensor ($\mathbf{T}_{\mu\nu}$).

[33] "The conception here outlined of the purely fictitious character of the basic principles of theory was in the eighteenth and nineteenth centuries still far from being the prevailing one". Einstein, 1933, p. 165.

[34] Einstein, [1921] 1934, p. 5.

[35] Some physicists, however, interpret curvature in an ontological sense; Feynman, for example, writes: "But it is more than an analogy, its means that space-time *is* curved." Underlined by the author. Feynman, 1964, pp. 42–18. However, there is also a trend to deny the ontology of space-time. Weinberg, for example, asserts that "the geometric interpretation of the theory of gravitation has dwindled to a mere analogy. (...) The important thing is to be able to make predictions about images on the astronomer's photographic plates, frequencies of spectral lines, and so on, and it simply doesn't matter whether we ascribe these predictions to the physical effect of gravitational fields on the motion of planets and photons or to a curvature of space and time". Weinberg, 1972, p. 147. In any way, Weinberg shows that Riemann's tensor is useful for calculating what we consider to be space-time curvature, and exemplifies it by easily obtaining curvature for the two-dimensional case. *Ibid*. p. 148.

[36] Einstein, [1949] 1970, p. 13.

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