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Quantum Reprogramming

Ensembles and Single Systems: A Two-Tier Approach to Quantum Mechanics

Evert Jan Post

Springer-Science+Business Media, B.V.

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In science, one expects man to be in charge leading the formalism he has created. In quantum mechanics, the formalism has been leading man.

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Preface

The vein of discussion in this book may be said to be predicated by one overriding major conclusion. It is the conviction that progress in work on quantum fundamentals cannot be forthcoming as long as the Schroedinger equation is taken to be equally applicable to single systems as to ensembles. The removal of this mostly "silent" dichotomy pervading quantum interpretation is a principal objective of this collection of essays.

These essays, therefore, aim at changing the prevailing nonclassical epistemology of quantum uncertainty. This major modification in perceptual philosophy opens up the single-system microphysical domain to a renewed spacetime topological scrutiny. By the same token, it properly reserves standard methods to randomized ensembles. The new situation of a topology-based single-system approach, with standard quantum machinery restricted to ensembles, calls for a two-tier theory of quanta.

The cited topological pursuit directly addresses single system spacetime physical configurations and is therefore radically different from existing explorations that have emerged in the realm of Hilbert's spectral spaces. QED, QCD, Gauge theories, Lie algebras, S-matrix, Regge poles, grand unifications, and super-string theories of "everything" belong in this "Hilbert" category. The present endeavor of treating single systems is much less ambitious; it operates in spacetime itself.

To initiate the necessary steps of paving the way for a two-tier alternative, it was thought to initiate this venture with essays aiming at loosening the iron grip of existing rules and paradigms of quantum mechanics. The conceptual intertwining of needed changes and their effect on existing metaphors made a step-by-step procedure mandatory. Yet individual essay publication became very cumbersome, because each essay had to reiterate radical changes in premises made in earlier essays. All of this evoked too much controversy to come to a workable publication process. Manuscripts either met with unmanageable reviewer responses or outright rejection. If this quantum reprogramming message deserves to come across at all, a suitably indexed essay collection with cross-references seemed the way to go; in fact, it turned out to be the only way available.

A repetition of exposure to conceptual predicaments is apparent in this collection. Yet, in retrospect, a measure of repetition is seen as necessary to loosen hardened convictions. For subject matter in a state of flux, a reduction in size by combing out those redundancies seemed premature.

If reading these chapters generates uneasiness, it may be well to consider an unusual quote, which has been ascribed to Max Planck:

"New scientific truth does not triumph by convincing opponents, but rather because opposition dies and a new generation grows up familiar with it."

Note how Planck is reluctant to credit the new generation with conviction through reason. He cautiously stops at familiarity, which is necessary, but not sufficient for reason. Following Planck's counsel, we now need to explore more this no man's land between familiarity and understanding.

E.J.P., Westchester, California
January '95

Apology and Acknowledgment

None of the criticism in these chapters directed at the "Copenhageners" should be regarded as reflecting adversely on the city of Copenhagen or its good citizens. On the contrary, physics owes an unusual debt of gratitude to Copenhagen, because some of the seeds from which this criticism emerged also first originated in the great city of Copenhagen.

I also owe a debt of gratitude to those who read earlier versions of this monograph. Their mixed feelings, criticism, or the conspicuous absence thereof somehow helped in making the presentation more articulate.

I have gratefully accepted a suggestion by S C Tiwari to reorganize the material in major subdivisions. They are: (1)The Copenhagen Era, (2)The Sommerfeld-de Rham view of Single Systems, (3)Cohomological Synthesis, and (4)Ramifications of the Two-Tier View of Quantum Mechanics. Computer technology made it easy to accomplish this reordering.

I thank Harry Newton for reading most of the earlier manuscript, which resulted in useful advice about language. Michael Berg combed the text for infelicities; those remaining testify to the number that needed to be processed. He suggested lifting out the quote pertaining to formalisms leading man instead of man taking charge of the formalisms. I also acknowledge with pleasure a crucial contribution by Christine Brunak. She challenged (sometimes to her dismay) this unaccomplished typist to become a reasonably accomplished word processor. Tom Boyce extended generous help with some of the more tricky word processing operations and Ruth Jackson gave the manuscript a still very necessary final combing on grammar, syntax and punctuation.

When this book went to press, word came that the mathematician Nicolaas Hendrik Kuiper had passed away. A long time ago, when I decided (at a somewhat later age) to take up physics, Nico always extended unconditional counsel. He first alerted me to the significance of the distinctions between local and global methods in mathematics. This book became in many ways an attempt at transferring those local-global distinctions to the realm of physics. If these efforts have any virtue at all, it was Nico who gave it an early impetus.

E.J.P.

ILLUSTRATIONS

CHAPTER I

INTRODUCTORY REMARKS

1. An Interface of Human Experiences

Man's earliest signs of interaction with Nature in a consciously constructive manner may well have coincided with the dawn of agriculture. It signalled the beginning of an era of partial control of nature, which then, by the same token, would also generate a more acute awareness as to what aspects of nature remained beyond the realm of human influence.

The first discoveries of Nature's rules and laws for the regeneration of the species gave man more of a distinct opportunity for guiding his own destiny through a control of the food supply; provided, of course, he learned to live according to those rules dictated by this newly acquired knowledge. The ensuing agricultural society therefore required a more delicately balanced social organization. This balance could not be well accomplished unless the members of this new society were willing to enter into a new phase of enhanced mutual responsibility.

In addition to learning the practical rules for conducting a successful agriculture, man had to accept a new form of inner discipline to maintain this new society. The more vulnerable structure of this new society was in fact more prone to disturbances by undisciplined characters.

The just depicted portrayal of an emerging agricultural society confronted man with a disciplined study of two sets of rules. They were: first, the technical rules necessary to conduct this new practice of farming, and second: rules dealing with the aspects of a new code of mutual behavior. They were rules of ethics and morality, necessary to protect and consolidate the organization of this early new form of agricultural society.

Contemporary society still reflects this two-sided aspect of development. The founding fathers of the United States of America saw to it that the Agricultural & Mechanical engineering schools provided the knowledge and training in science and technology to help feed this nation, whereas law and theology colleges were to provide the magistrates and preachers who would help make this a society of law-abiding citizens, whose words could be trusted, and whose deeds could be counted upon as contributions supportive of the common good. These two types of schools deal with a vastly different subject matter.

The schools of science and technology pride themselves in maintaining a strong logical thread in their teachings. The rules they promulgate relate and can be reduced to basic natural phenomena. The scientific method is distinguished by its systematics based on strict logic, as applied to the laws of nature.

By contrast, the schools of law and theology deal with rules of human behavior. They cannot, to the same extent as in science, be logically reduced to a common denominator. Rules of human behavior may be established by the simple fact or expectation that following those rules makes for a "kinder and friendlier," and more smoothly functioning society. The schools of law occupy themselves with the practical everyday aspects of such human behavior, whereas schools of theology occupy themselves with the more esoteric aspects of humanity.

Having made this preliminary subdivision between disciplines based on the strict systematics of science versus those calling more on faith, let us assess the viability of that subdivision. To implement a reprogramming of quantum mechanics, it is a major concern whether science strictly follows the rules of the scientific method. Does science sometimes call on the more esoteric qualities of faith and intuition?

This question can best be answered by noting that science would be in very bad shape without faith and intuition. The method of science is a hypothetico-deductive process. It starts out with an intuitive act of faith, by virtue of its making hypotheses that are either found to be valid or invalid. If found to be invalid the courage of conviction requires rejecting a false hypothesis. If found to be valid, on the other hand, the realm of validity of this new hypothesis need careful exploration in order to arrive at a precise statement that includes its limitations.

Not all hypothetico-deductive processes are, however, as black and white as here depicted. Some hypotheses have been found useful, yet their realm of validity has never been established; or, so far, the efforts of doing so have been either incomplete or unsuccessful. Under those circumstances science finds itself in a situation similar to that encountered in theology. One works and keeps working with those principles, because they have been known to be effective.

Finding a deductive chain between some isolated and previously disconnected hypotheses can be a major event in physics. Such experiences have been known to lead to a state of euphoria in which the differences between article of truth and article of faith can become clouded. This collection of essays constitutes an attempt at identifying areas in modern physics where the distinction between truth and faith has become unclear.

Quantum mechanics is a major domain of activity for which it has become common place to operate with unproven premises. Its principal unproven premise rules that the Schroedinger equation may be taken to describe a single isolated quantum mechanical system. It is interesting to note how few textbooks are, or have been, very explicit about this premise. The single-system applicability is largely taken as a foregone conclusion. Yet in view of the basic significance of this hypothesis, a more incisive inquiry is called for into the conceivable consequences of jumping to such a conclusion. In the course of these discussions, we shall find how one questionable premise leads to an array of interdependent questionable premises, all of which carry the same defective gene.

Through the years, a quantum mechanical lifestyle has been adopted that has taught us a series of highly ingenious concepts, enabling us to cope, as best we can, with these genetic inadequacies. Yet, unlike the genes we are dealt by birth, which cannot be changed, the genes of contemporary quantum mechanics can be changed and are susceptible to repair. To do so, however, we have to identify and unlearn questionable teachings of the past.

It is well known how difficult it is to unlearn things we have learned to accept during our early years of indoctrination. It is also known to be harder to unlearn things we have learned exclusively on the basis of faith, than to unlearn things that can be logically identified as "mistaken." In this process of reexamination, we become aware how contemporary quantum mechanics has rather liberally called on faith. Early success was the tempter, which prompted such development, even when a more unbiased path of deduction still seemed to be available and possible.

2. Modern Physics' Logic Demerits

Logical inadequacies in science are much more common than many scientists are willing to acknowledge. They become part and parcel of proceedings whenever the success of a method under consideration has exceeded the quality of understanding that has been gained. It usually means the method in question came into being through an element of serendipity, rather than through a complete process of careful logical deduction, which could have dictated an interpretation from the start.

Quantum mechanics, as we know it today, may be taken as a prime example of the just-depicted state of affairs. Its fundamental tools, *e.g.* the Schroedinger equation, saw the light of day as a result of inspired theoretical experimentation, which then, in retrospect, yielded results well beyond expectations and without the benefit of a relevant interpretation.

It took the better part of a decade before an interpretation could be agreed upon by some sort of a majority consensus. The thus ensuing Copenhagen interpretation emerged as the most widely-accepted method of "rationalizing" the inner workings of the new quantum machinery. Yet the method of rationalizing deviated markedly from what had been customary in scientific reasoning until that day. The change in procedure became a landmark in physics' methodology. It inaugurated a transition between old and new ways of doing things. It is presently known as the transition from *classical* to *nonclassical* physics.

The problem here confronted can best be summarized by the question: How rational was the Copenhagen process of rationalization? There were several telltale signs revealing how the authors of the Copenhagen revolution were well aware of the drastic changes in their logic processing. It was during that time that voices were heard to augment existing logic with something referred to as quantum logic. So what is quantum logic? Is it a valid extension of the logic systems that were known at the time, or should it be identified as a compromise use of existing rules of logic? All of which generates the question: what is a compromise use of logic?

Since logic is defined as a system of valid reasoning for making correct inferences, the methods of logic have the virtue of a self-healing quality. If the use of logic leads to contradictions, the implication is that something could be wrong with the premises that started the chain of logic. This methodology is the very basis of mathematics' famous *reductio ad absurdum.*

Since the protagonists of the great quantum revolution were all well trained and extremely competent individuals, the chances that they might have been making simple reasoning errors must be taken as small indeed. Hence the eventful transition from *classical* to *nonclassical* physics could hardly have been the result of a simple reasoning error. Further inquiry is needed as to whether the decisions instrumental in creating nonclassical physics were sufficiently compelling to risk an inadvertent suspension of logic's self-healing quality.

The logic crisis alluded to here occurred when Schroedinger's equation descended from heaven as a relevant and useful instrument of physics. Physics was confronted with two major options for its interpretation. They were the single system versus the ensemble view. While there was not the slightest doubt that the Schroedinger equation gave relevant information about atomic systems, no decisive answer seemed to be forthcoming as to whether the equation described a single atomic system or an ensemble thereof: *i.e.* a collective of similarly prepared identical atomic systems.

In the euphoria of the moment the world of physics was not prepared to lose itself in nitpicking about a presumed minor distinction between ensembles and single systems. While the Copenhageners opted for the single system, those holding out for the ensemble were calling out in the desert, abandoned by the main stream of physics. The rather subtle distinctions between ensemble and single system behavior were lost in the ensuing wild scramble for results; they remained hidden for the longest time. As a result, the ensemble character was never properly delineated, notwithstanding the irony of Max Planck himself anticipating a major solution feature of that enigmatic Schroedinger equation, before the latter had even been discovered.

Even if contemporary quantum mechanics takes credit for adequately manipulating logic deductions, the basis of premises from which these deductions were operating was wanting. Inadequate effort went into the justification of the choice of the single system as the primary object of description for the Schroedinger equation. In fact there was no justification whatsoever for that decision. It was a premature "jump in the dark," accommodating, at best, some preconceived notions.

No explicit distinction between ensemble versus single system surfaced in the early days of trying to better understand the Schroedinger equation. The ensemble alternative was half tolerated as a conceivable object of description. Yet, also the ensemble did not receive a hard unambiguous justification A presumed closeness of ensemble and constituent single-system responses provided the rationale for accepting this split-personality approach. The ensemble versus single-system asymptotics seemed to make a further pursuit of their distinction a trivial matter. The initial success of the new quantum mechanics thus tempted the Copenhageners to forego a more incisive examination of physical foundations.

The initial inadequacy of practically identifying ensemble and single system became responsible for a string of nonclassical metaphors. The statistical implications of Schroedinger's equation treating particles as point objects would have merely been an act comparable to an assumption instrumental for Maxwell-Boltzmann gas dynamics. Yet the unwarranted Copenhagen-inspired reduction from a many molecules gas to a particle in a single atomic system literally bereaved the Schroedinger-implied statistics from its universe of discourse. This highly *nonclassical* deed unavoidably engendered many nonclassical contingencies. It is the very deed that prompted Einstein (no statistics slough) to take issue with his frequently quoted response: "God does not play dice." There will be ample opportunity to return to this pronouncement later. Compare chapter XVII; 9.

3. The Myth of the Nonclassical Metaphor

There have been more seminars and conferences on the foundations of quantum mechanics than on any other theory in physics. Unlike Maxwell theory, which almost from the start related to first principles, the quantum mechanics of 1925 came to us as a nearly finished product, albeit with an obscured relation to first principles. Quantum mechanics' amazing applicability made physicists accept a seemingly finished structure, knowing that its foundations had not as yet been solidly anchored.

Foundations research for quantum mechanics came much more after the facts than for most other theories. The question arises as to what made it so unique that physics was willing to accept such an unusual situation.

Apart from the amazing applicability, there was the equally amazing equivalence of the double origin of this new discipline in the forms of matrix- and wave mechanics. Add to this the pure magic of the Dirac spin theory, and the stage was set for accepting quantum mechanics as a true gift from heaven. It is this aura of being a gift from heaven that has strongly dominated foundation work in subsequent years.

There is a natural inclination to expect wondrous results from a gift from heaven, and, in fact, there was plenty of reason to be grateful for what could be done with this new tool. Lowering such expectations would be ungrateful and disrespectful to Mother Nature. By the same token, tinkering with a gift from heaven to improve its performance was frowned upon; the theory's record of achievement gave it divine lineage.

Cursory inspection of the programs and contributions to foundations seminars and conferences reveals a general trend of not restricting the realm of applicability and of keeping the general structure intact. The rationale behind this trend is that gifts from heaven should be expected to be perfect or nearly perfect, with an optimum realm of applicability.

Seminars and conferences, therefore, show an almost exclusive trend of fitting foundations to a preexisting structure that has been very successful by manifesting optimum applicability. The more than sixty years of trying to understand the success of this status quo has met with conceptual hurdles that have been a vexation to the spirit. Mindful that understanding and status quo are as incompatible as "having one's cake and eating it too," concessions must be expected, either by changing the theory structure or by delineating more precisely its relevance.

Since changing the structure of a good working formalism would not be wise, the remaining choice is a closer examination of its scope of applicability. A conceivable consequence of that program is the creation of a new realm of physics, not treatable by the standard tools of quantum mechanics as we presently know them. Yet prior to launching into a new endeavor, a profile is needed of the standard tools of quantum mechanics, their customary interpretations and the physical territories presumed to be covered by those tools.

The standard tools at center stage are the Schroedinger equation and its conceptual associate, the Copenhagen interpretation of the solutions of that equation. This equation had already acquired a major track record of applicability prior to the development of an explicit interpretation that had the approval of a majority of the physics community. At this point, one obviously wonders how an equation could be useful without an as-yet-precise knowledge of the physical meaning of its solutions. An important mathematical technicality holds a key to that enigma. A discrete set of characteristic numbers of the solution, known as its *eigenvalues,* takes the central position for the content of physical information being conveyed. The solution itself occupies a secondary place and retains a measure of availability for purposes of interpretation. A growing consensus subsequently converged on the idea that the "wave" function solutions of this equation had undeniably statistical implications.

Once this statistical connotation of its solutions had been established, opinions started to differ as to the nature of that statistics. What was it that could possibly be subject to a statistics in the microphysical domain? One of the ensuing options, which became known as the Copenhagen interpretation, was predicated by the presumed existence of an all-pervasive, *a priori quantum uncertainty* affecting all physical objects. This uncertainty was found to be compatible with the Schroedinger equation by virtue of the fact that it was derivable from that same equation. This concept of universal uncertainty had been earlier suggested by Heisenberg on the basis of a thought experiment of observation in which this uncertainty relates to the observation of isolated single systems. The knowledge about single-system behavior, presumed to be accessible by man, was thus taken to be limited by an observation-based statistics.

Yet this statistics, spawned by Schroedinger's equation, left undecided an issue whether this equation described a single system or an ensemble thereof. Even if experiments at the time indicated ensembles of identical systems as sources of observation, the Copenhageners, this evidence notwithstanding, still opted for the single-system, which they believed to be permissible choice. Heisenberg's thought experiment was taken to be supportive of that decision.

In so doing, the Copenhageners literally sacrificed a classical option for their statistics, because the single-system object lacked what statisticians call a *universe of discourse*. Those who did not approve of this extrapolation from ensemble to single system remained outside this realm of Copenhagen physics and later became identified as supporters of a statistical ensemble interpretation. Retaining the ensemble, they retained more of an option of finding a *universe of discourse* for the statistics implied by the Schroedinger equation. Unfortunately, the ensemble supporters fell short of explicitly identifying a set of parameters that could serve as a universe of discourse. Also, their statistics retained an aura of mystery.

Copenhagen thus took the fateful step of declaring the statistics, pertaining to their chosen option of single-system, to be *nonclassical* in nature. This language stressed the absence of a universe of discourse as an irrevocable inherent feature, which distinguished this new statistics from the *classical* ones that had an identifiable universe of discourse. This ominous initiative created what was believed to be a seemingly unavoidable distinction between *classical* and *nonclassical* physics. The introduction of nonclassical physics thus became a contingency of a universe of discourse that had not been identified.

The word *nonclassical* now became a receptacle for all those situations that did not fit the old mold, now referred to as "the world of classical physics," *i.e.* the methods and concepts that had rendered services prior to 1925. The Copenhagen School transformed the *nonclassical* absence of a universe of discourse into a thesis claiming all human knowledge of microphysical situations to be fundamentally limited by an all-pervading and unavoidable universal statistical uncertainty.

Soon the notion of a nonclassical realm led to situations where certain questions could not be asked, because in the new nonclassical context such questions could not be accepted as meaningful. Young practitioners of the new discipline were admonished not to reveal a lack of knowledge by asking wrong questions. The word *nonclassical* acquired a status comparable to the fifth amendment of the U.S. constitution. This nonclassical interpretation of the theory became known as the Copenhagen interpretation. It licensed the refusal of answering questions that could incriminate the foundational soundness of this new *nonclassical* physics.

This liberal appeal to nonclassical notions gave the potential escape of a partial suspension of reasoning in situations that were deemed to be in the nonclassical realm. Physics underwent a process of formalization into abstract schemes of organizing observations. The abstractness was in part a consequence of a partial and possibly premature rejection of interconnections, which conceivably could still exist from a classical angle. For all practical purposes, such interconnections had all been washed out by an all-pervading nonclassical quantum uncertainty.

The use of an evasive *modus operandi* has been common in many realms of human endeavor other than physics, although recourse to such methods has been rare in the sciences. Assuming it is not given to man to know and understand everything, there is, at times, no alternative. Man takes recourse to conjectures. Faced with the deeper questions of life, religion frequently has taken recourse to conjectures that could not be defended as samples of self-evident truth. Such conjectures, though, were believed to serve a practical purpose.

It now stands to reason how the presumed absence of a universe of discourse in quantum mechanics led to a chain of nonclassical inferences that have become part and parcel of physical thinking since the early Thirties. The attitudes outlined by this school of thinking is now collectively known as the Copenhagen School of interpretation(s). Since it is characteristic of this school of thought to leave certain specifics in the realm of the unknowable, a measure of interpretational plurality is to be accepted as natural within the Copenhagen School.

Instrumental in these nonclassical developments has been the absence of an explicit and complete derivation from first principles of the Schroedinger equation. This equation had come to physics as a gift from heaven. It therefore seemed unavoidable and even fitting to give its interpretation a commensurate religious dogmatic flavor. Not only the Greeks could not be trusted when they came with presents; also heaven was under suspicion. More than anybody else, Schroedinger knew the shortcomings of his equation. It was not he, but his colleagues, who placed his equation on the pedestal as a gift from heaven. Man, not heaven, is to blame for drawing unsubstantiated conclusions that lead to marginal decisions.

What was the major, yet least discussed, "leap of faith" that was hidden in what we now know as the Copenhagen interpretation? *It is the view that holds the mathematical machinery of quantum mechanics to be an instrument describing a physical reality that is identifiable as a single-system.* Quite amazingly, an inspection of the textbook literature reveals little or no justification to substantiate this presumed relation to reality. Starting with Heisenberg, continuing with Schroedinger and Dirac, and the bulk of the textbook literature following suit, the single-system idea became a foregone conclusion as if literally implied by reality. The overriding suggestion emanating from this rather universally adopted extrapolation may be succinctly summarized by the rhetorical question: what else could it have been?

The truth is, however, that through this tacit act of extrapolation, physics brainwashed itself and its students into believing that there were no worthwhile alternatives to the single system option. Having descended upon us with this aura of a gift from heaven, the Schroedinger equation had not quite met with that same intense scrutiny encountered by other theories, which could not boast of such heavenly kinship.

Another compounding circumstance was the mathematics of the Schroedinger process. It was found to be a chapter in the celebrated spectral theory of Hilbert spaces. This aura of being a gift from heaven in conjunction with the perfection of its mathematical structure were now major factors permitting the Copenhagen interpretation to ride in on the coattails of Hilbert's near-perfect spectral theory.

The *nonclassical* modus operandi now developed into a veritable art. Similarly as artists, pioneering new developments, can't wait for the critics to tell them why they do what they do, proponents of the Copenhagen School could not wait for a majority to give them their stamp of approval. Those unresponsive to this new "art form" were painted into a corner of those who failed to recognize limitations of classical procedures. Copenhagen's dichotomy of classical versus nonclassical soon conquered the world of physics by a process of sheer intimidation. The situation settled into a status quo, which has now prevailed for sixty years. There have been new "derivation" alternatives of the Schroedinger equation. Transcriptions from Hamilton-Jacobi particle dynamics have been complemented with transcriptions from diffusion theory and the Euler equations of continuum mechanics. Yet its status as gift from heaven remained undented.

The here-cited potential weakness in the existing relation between Schroedinger process and Copenhagen single-system view, imposes an obligation for reexamining conceivable alternatives of interpretation. It is now useful to recall how early ensemble initiatives were prematurely abandoned, because, at the time of their introduction, they were not, or could not be, specified with adequate statistical detail. Since contemporary foundations research still hammers away mostly at interpretations accepting the single-system thesis, a reexamination of those historical alternatives is now called for.

In the mid-Thirties, Popper[1] supported by Einstein[2] did suggest an ensemble alternative. At the time, the Copenhagen view was already so firmly entrenched that the chances for considering Popper's alternative were very small. The chances for accepting such an alternative were even smaller. In recent times, though, the growing importance of macroscopic quantum phenomena has made quantum mechanics a more tangible discipline. Questions that could not be answered in the past can now be resubmitted and answered on the basis of freshly accumulating evidence.

In macroscopic quantum phenomena, it is the overriding suggestion of total order which specifically justifies questions concerning the whereabouts of this so-called "all-pervasive nonclassical quantum mechanical statistics." If there really is such a nonclassical statistics, why does it not manifest itself clearly and unambiguously in these macroscopic quantum phenomena?

The emerging picture of a Schroedinger equation that might be restricted to ensembles makes the nature of the statistics to be associated with those ensembles a point of major concern. The early ensemble-based work did not quite proceed to the point of unambiguously establishing parameters of this missing universe of discourse. This failing or rather omission on the part of the ensemble proponents is the main reason why Copenhagen's nonclassical notions could rule supreme for so long.

As long as it was not necessary to make commitments about the universe of discourse, one could leave things as they were and had been for the last sixty years. It is the persistent emergence of more macroscopic quantum phenomena which now forces a renewed and more total confrontation between Copenhagen- and ensemble interpretations. This confrontation identifies universal uncertainty for single systems as an impermissible extrapolation. Uncertainty stands reduced to a status of ensemble disorder.

How is this very incisive change in current views going to be implemented in the general framework of physics? It is first necessary to establish the statistical parameters of what could be tentatively referred to as a *Schroedinger ensemble.* This is done by showing that an averaging with the help of these newly found statistical parameters of the ensemble will lead to typical Schroedinger results (chapter III;5). This step complements the austere Popper-Einstein ensemble proposition with hard facts.

In summary, the situation is now as follows: an ensemble interpretation of quantum mechanics leaves Schroedinger's mathematical formulation largely intact. Early ensemble-based book publications by Blokhintsev,[3] Kemble[4] and others testify to this effect. These texts read much like the Copenhagen-oriented texts. The additional detail injected in these essays indicates how the Schroedinger eigenvalue process characteristically applies to unordered ensembles. *The thus ensuing Schroedinger ensemble is found to be randomized in phase and orientation.*

It is relevant to note that the prevailing Copenhagen interpretation of quantum mechanics, true to its noncommittal views, also permits, in principle, the mental construction of a so-called Gibbs-type ensemble of *conceivable* single-system manifestations. The *conceptual plurality* of the Gibbs picture of a single-system view, though, is no substitute for an actual *physical plurality* with interactions between constituent parts.

The restriction of the Schroedinger equation to appropriately randomized ensembles leaves ordered ensembles and single systems without a suitable tool of inquiry. *New instruments of inquiry need to be identified to fill the gap created by restricting the realm of applicability of the Schroedinger equation.* Chapters VI and VIII explore the existence and mathematical nature of such tools. The ensuing subject of *period integrals* is by no means new to physics. Yet, in the perspective of new physical developments pertaining to the mathematical concepts of *local-global*, the physics of single systems can now be approached in an encompassing and more appropriate global way.

The local-global aspect is also reflected in the very definition of the concept of ensemble: *an ensemble is a thing known through its elements.* The dictionary definition of the word ensemble, in fact, says exactly that. A measure of randomness then serves well in making identical elements recognizable as distinct from one another by giving each its very own phase and orientation.

The elements of a wholly ordered ensemble, by contrast, lack this measure of distinguishability as independent entities. *Hence, the wholly ordered ensemble is, for all practical purposes, a single system; while the elements are parts of the whole, they don't have phase and orientation individuality as criteria for distinguishing one from the other.*

It is now not altogether unreasonable to suspect that an all-global description (say without an equivalent local counterpart) is needed for tackling the single system or the wholly ordered ensemble. Since there is, by definition, in the wholly ordered ensemble no change when proceeding from one element to the other, a differential equation process is less suitable for conveying information. The Schroedinger approach is then out, and an all-global approach takes over.

Mathematically, it means that an integral takes the place of the differential equation plus boundary conditions. This integral, though, cannot be of the reflexive type as used in a Hilbert integral equation. The latter typically reflects an ensemble situation, because the local function relates to itself by domain integration of its values elsewhere (chapters VI;8, XVII;7). Such integral equations are equivalent to a differential equation with boundary conditions. An *ab initio* integral formulation, by contrast, has an intrinsic global feature. Period integrals offer a perspective for such *ab initio* global descriptions.

Period integrals have always played a basic role in the development of physics. In these chapters, they are placed on a new level of enhanced physical awareness. Mathematical developments due to de Rham have made period integration a major tool for exploring topological structure. Interestingly, electromagnetic field configurations served de Rham as a source of inspiration in developing his *cohomology* of configurations in abstract mathematical manifolds.

Among the new global quantum tools under consideration as period integrals, some relate to the history of this subject (chapter VI), others aim at new topological perspectives (chapter XIII). The result is a quantum cohomology of physical configurations. Since a quantum cohomology indicates a global approach to the laws of nature, one may wonder: how can a global tool with presumed macro-connotation guide in the predominantly micro-domain of quanta? Consider here that Copenhagen's all-pervasive quantum uncertainty has, in the past, tended to preclude applicability of macro procedures in the micro-domain. How can further insight be gained in this macro-micro applicability of global tools?

As topological probes, period integrals can be expected to be pre-metric (*i.e.* metric-independent). The idea of physical laws having metric-independent connotations, while unnatural at first, now becomes a matter of fundamental physical importance. *Indeed, the counting of quantum states should not be contingent on metric use.* Since only the metric field makes micro/macro distinctions, laws manifesting metric-independence can have equal applicability in micro- and macrodomains. The counting laws of quantum states belong in that category.

It has been a well-hidden secret that a metric-independent branch of physics has been around for some time. The names of reputable mathematicians and physicists have been associated with that endeavor. It took place three-quarters of a century ago. In his account of these matters, Whittaker[5] later remarked: "It must be said, however, that, elegant though the mathematical developments have been, their relevance to fundamental physical theory must for the present be regarded as hypothetical." A few pages later he continues, almost answering his own query: "It is characteristic of theories such as this that differential relations are generally replaced by integral relations."

Whittaker's words acquire prophetic significance in the light of the local→global transition initiated by de Rham's use of period integration. The macro/micro contrast as criterion of physical applicability cannot now be a fundamental issue, because the metric is the one and only means to tell the macro/micro difference. Since counting absolute quanta of nature should not depend on subjective choices of reference frame or metric, a metric-independent status of certain laws of nature should now no longer remain an unexplained mystery.

While these introductory remarks identify and logically justify some of the principal conceptual ingredients used in the following chapters, a hands-on experience is necessary to substantiate their relevance. Words, no matter how eloquent, are merely inducements for getting started. The applicability of these conceptual ingredients provide avenues for exploration, in which experiment must be expected to have a last word.

The given guidelines for widening the quantum mechanical horizon are fortunately not isolated items of the physics literature. Propositions to jointly treat indeterministic ensembles, and deterministic single systems or highly ordered ensembles, have recently been made by Barut[6] and van Fraassen.[7] There is a parallelism in their arguments, in the sense of stressing the need for a two-tier approach to quantum mechanics. The cited studies differ from the present undertaking in the choice of mathematical tooling for treating deterministic situations. They accommodate determinism with the help of the standard Schroedinger formalism. The here chosen approach selects a typically global tool for treating determinism. It is the existence of such deterministic tooling in the form of period integrals that brings the Barut and van Fraassen two-tier approaches further to a logical conclusion.

Finally, a word is in order about a footnote added to the English translation of Popper's book on the logic of scientific discovery.[1] He specifies in some detail the ensembles he considers. Over and above spatial ensembles, he recognizes ensembles distributed in time in the sense of a stream of systems; say particles coming in one at a time. The Debye-Scherrer and Davisson-Germer experiments convey such a picture. Since the latter comes suspiciously close to Gibbs' abstract ensemble of conceivable manifestations of one and the same system, Popper was reaching out to accommodate a Copenhagen view. This conciliatory act may have helped single-systems and ensembles to coexist with indeed minimal undue confrontation.

The spatially distributed example of many systems of random phase and orientation is in the following chapters recognized for its ability to permit a measure of system interaction capable of securing ensemble randomness. In chapter III the existence of zero-point energy is established to be a *sine qua non* for an equilibrium state of ensemble disorder. Without disorder there is no statistics. The alternative then is the deterministic branch of quantum mechanics, which is discussed in chapter VI.

The long time coexistence of Copenhagen and statistical ensemble interpretations has, in the course of time, led to the consensus formulated by Barut and van Fraassen. Yet, notwithstanding their ensemble-nature, Schroedinger and Dirac theories have had an undeniable, albeit qualitative, constructive role for some wholly ordered ensembles such as for instance occur in the theory of ferromagnetism. The *Aufbau* principle of atomic shell structure is another example where single system insight has benefited from ensemble-based methodology, provided, of course, it is used in conjunction with the Pauli principle.

It is a matter of adapting new theory after the facts. Hund's rules of ferromagnetism and the ideas of atomic shell structure, though, were already established prior to the Schroedinger equation. The history of physics over more than a half century has been replete with successful examples testifying to a cross-fertilization of ideas ensuing from the asymptotic relations between ensembles and single systems. It is almost undoable to identify every one of these cases.

Yet, in the long run, a further straddling of the fence between two fundamentally distinct realms of physical experience can only testify to a belief that one can have one's cake and eat it too. How did physics get caught in tolerating an obvious state of schizophrenia between single-system and ensemble?

An explanation, in part, resides in later developments that have been erected on typically nonclassical Copenhagen premises. Quantum electrodynamics (QED) manifests such nonclassical contingencies. *Bluntly abandoning Copenhagen premises would be synonymous with discarding the great triumphs of QED. Hence, a sine qua non for abandoning Copenhagen is finding QED alternatives with adequate promise of achievement.*

Questions and perspectives concerning such QED alternatives, without the use of nonclassical metaphors, are confronted in chapters VIII, IX and XI.

THE COPENHAGEN ERA

CHAPTER II

THE PSYCHOLOGY OF THE 1925 REVOLUTION

1. A Psychological Angle to the History

With the advent of modern quantum mechanics in 1925, physics entered an entirely new phase of conceptual imagery. The words "nonclassical" versus "classical" became the characteristic hallmark, not only of the ensuing developments in the subject matter, but also for the protagonists involved in this real-life drama. More perhaps than at any time prior to that revolution did a younger generation suspect the older generation of being unable to grasp the new trend of things in physics.

Many of the '25 generation have now passed away, and some of them may have themselves experienced a similar feeling of abandonment vis-à-vis the subsequent developments of quantum electrodynamics and quantum chromodynamics, each roughly covering another 25-year generation interval. Perhaps some of these pioneers may now be having second thoughts about the reservations of the oldies of their past. It is the near-inescapable risk of treading new territory. The privilege of exploration knows disappointments as well as triumphs for all ages.

Extracting meaningful predictions from manipulating energy infinities in quantum electrodynamics (QED) is no minor achievement. Yet, the methods to do so grate on the mathematical conscience, and correctly so. Power engineers may wonder about the reality of energy infinities stored in the electrodynamic vacuum. Can they give perspectives on a future where the impossible becomes possible?

Quantum chromodynamics (QCD) has revealed itself to be even more ambitious than QED. QCD has now, in principle, confessed to a possible unobservability of what it professes to be the fundamental constituents of matter. I am speaking of quarks, with gluons holding them together. However, in case the unobservability thesis might be mistaken, there is a consensus not to refrain from building bigger accelerators to make or see quarks. In the meantime, theorists cover a possible reality of unobservability by model simulation with ever more powerful computers. Some chromodynamicists claim quarks will lead to an ultimate understanding of the early universe, say, before it was a few minutes or a tiny fraction-of-a-second old.

These metaphors have not been extracted from science fiction. They can be read in responsible and reputable reports about the developments of modern science. Compare hereto a nontechnical account by Michael Riordan[1] for an insider's view about the search for quarks. The epiloque of that monograph on quarks gives a realistic and fair summary of the way of thinking and methods of working in contemporary particle physics. He says:"...what science does remarkably well, again and again, is to build surprisingly successful images of the universe." Such endeavors remain useful, even if their relation to reality (as we may conceive it) seems tenuous.

Accepting these journalistic accounts as fair and reliable, we see man, in his assessment of Nature, projecting the event of his own birth as a major episode by attributing similar happenings to the universe itself. Since man is part of Nature, the idea is not unreasonable, even if the extrapolation seems outrageous.

Mindful of the transient nature of man's efforts, let us go back to the moment in time when things started to change for the physical sciences. The year which shook the older generation, in a manner more than ever before, was 1925. In fact, not only the older generation, but also the generation that partook in the revolution, responded with a strange undertone of disenchantment. Schroedinger himself did not refrain from expressing discomfort about where his own creations were heading.

More than at any time before, was the scope and realm of validity of new physical theory no longer determined by a theoretical derivation of its new tools, because there was no real derivation. The Schroedinger equation is a tool in question. Its merits were decided by what it could do, and not by its heavenly origin. This seemingly egalitarian view, however, masked an attitude of prejudice in the physics community. This wave equation instrument of modern physics was hailed with a near-religious fervor as a divine gift. Most textbooks on quantum mechanics excel in praise, yet few give an appraisal of limitations. In later life Schroedinger himself turned away from his own brainchild. Its shrouded origin prevented him from arriving at the understanding for which he had hoped.

Yet, notwithstanding this admitted measure of conceptual incompleteness, it did not turn out to be a shortcoming that prevented Schroedinger's equation from becoming the single most important tool of modern physics. Without adequate understanding of what makes it work, we are here confronted with one of the most fertile and effective developments of modern physics. Mindful that understanding can be in the mind of the beholder, the question at this juncture was, and still is: what measure of subjectivity can we afford without really jeopardizing the fundamental quality of physics as an objective science?

There is certainly no argument that Schroedinger's equation is a physically most relevant instrument of modern physics. It is so relevant that many physicists and engineers have developed a very pragmatic attitude with respect to its application. The transmission-line pioneer Oliver

Heaviside, who excelled in unorthodox mathematics, used to say he could enjoy a meal without knowing how it had been prepared. The quantum engineers similarly found out that you don't have to know what the Schroedinger equation means to see the physics of its solutions.

Coherence of subject material is not served if new generations are taught pragmatism with a flavor of unwarranted superiority with respect to those who want to understand more. It can serve as a crutch, but may give false reassurances in the long run. Such methods are not fair to newcomers, whose judgment is bound to be vulnerable, because they are so over-anxious to get ahead in an over-competitive world. While it is true that wisdom does not necessarily coincide with advancing age, keep in mind there is always a chance that it does. By prematurely rejecting experiences of the past, and by letting pragmatism trample the insights of yesterday, we shortchange ourselves and the subject matter. Pragmatism has its limitations, even for getting ahead in the world.

Let us now address those who feel an obligation to make an honest attempt at understanding. This requires more of an inquiry into what the Schroedinger equation could possibly mean. *The two major options have been, and still are today, the Copenhagen interpretation and the slightly more restrictive ensemble interpretation.* While Copenhagen supports applicability of methods to ensembles and single systems alike, the ensemble people have been holding out for a more restricted realm of applicability, namely, an ensemble of identical systems that are deemed to be in a disordered state of interaction. Hence, the ensemble interpretation could be considered, if you will, as being vaguely contained in the Copenhagen interpretation. All we are talking about here is really imposing a restriction on the use of the Schroedinger equation (*i.e.*, backtracking slightly from the 1925+ revolution). As matters stand today, that is still seen as a counterrevolution, or heresy.

At the year of the revolution, 1925, all experimental input came from observations on ensembles of identical systems. Notwithstanding this ensemble reality of facts, the Copenhagen School included single systems, without adequate verification. Copenhagen was seemingly affected by an overriding desire of coming up with a once-and-for-all final solution, a last stone of wisdom, if you will.

The proponents of an ensemble picture unfortunately remained rather inarticulate about the nature of their ensemble. In the light of this seemingly passive attitude of the ensemble proponents, it may become understandable why the Copenhagen view won out over the strict ensemble view. The latent desire of presenting a more encompassing result dominated the thinking of the Copenhagen School. The extrapolation of the Schroedinger methods into the domain of single systems was at best supported by asymptotic relevance of the results. This unjustified step remains a latent shortcoming of the Copenhagen initiative.

The desire to extend the use of methods to single systems led to a string of now-famous specifications which, in turn, have led to a much longer string of logical and epistemological inquiries. It is at this point where modern quantum mechanics acquired its most prominent and notorious nonclassical features. Many of the beloved mysticisms of quantum mechanics have their roots in the desire of wanting to deal with single isolated systems. Since this aspect is a projected "pièce de resistance" of quantum reprogramming, let it suffice to mention here these nonclassical features and how they logically interrelate.

(1) The single particle and Born's probability amplitude triggers a tongue-in-cheek suggestion of point particles.

(2) Soothing the conscience about so bold a step, a finite spatial presence is reintroduced for the point particle with the help of the uncertainty principle, and behold, this *a priori* uncertainty is even derivable from the wave function.

(3) The particle-wave duality becomes a dichotomy of indecision between single particle and ensemble plurality.

(4) Restricting the formalism to ensembles automatically resolves the perennial dispute about completeness of description. The description of a statistical ensemble is incomplete by choice!

(5) Since typical ensemble information is nonlocal, a conflict with Bell's theorem need not be feared. The latter envisions only local hidden variables pertaining to a presumed single system. *All considerations relating to Bell's theorem are made in the perspective of the utterly frail hypothesis of a single-system connotation for the Schroedinger equation.* (compare chapter V; 6)

The willingness to accept the cited leaps of faith demanded by the Copenhagen point of view can only be explained in the light of the tremendous success of the process. The attitude of not arguing with success is no excuse, though, to remain uncritical of those achievements. In fact, relatively little effort has been invested in finding out how much of this success was really contingent on a full acceptance of the Copenhagen point of view, because it is not "wise" to argue with success.

The new picture was emotionally appealing. For the first time in the history of physics, "uncertainty" told man about his limitations. Yet, this apparent humbleness was not perceived as treading on exactly the territory where only the gods were supposed to go. The protagonists of "uncertainty" were telling us that they knew what Nature had in mind to let us know!

One can hardly deny that physics in the late Twenties and early Thirties had taken a religious turn. There is today still that nearly unlimited faith that Schroedinger will bring us there, even though Schroedinger himself expressed his doubts in no uncertain terms. A fair measure of knowing had been obtained largely by a measure of faith, which was not quite commensurate with that gain in knowledge. While intermediate phases of faith versus knowledge are essential in physics, the integrity of the subject requires keeping tabs on them. We need good judgment when we are dealing with faith as well as knowledge.

The position taken in the here-given discussion is that some of the religiously flavored aspects of faith in contemporary quantum mechanics are due for revision. The objective is one of reducing a premature dependence on faith by a more detailed delineation of options. Mindful that changes in a religious-type faith are harder to accomplish than changes in intellectual conviction, a rocky road must be expected. Yet, the reward of a better synthesis between classical and nonclassical concepts and a state of improved harmony with the neighboring disciplines of mathematics and philosophy may be worth it.

To see whether it is all worth the effort, let us sneak in a preview of new perspectives that emerge in the process of abandoning some prejudices based on unverified faith. Looking back, one then wonders why, for so long, certain aspects of faith could have dominated over simple provable truth. Let us check this measure of faith in subjects of great current interest.

Any time quarks are shown as baryon constituents in illustrations, they are depicted as a collection of neat little spheres huddled together in a domain of space that is taken to be a proton or a neutron. To delineate specific quark species, the little spheres may be colored. We all know that those pretty colored pictures are meant to be, at best, mnemonic devices that are not to be taken literally as a gospel truth. Yet, even the choice of mnemonic device reveals a clue as to what is in the mind of those who make such pictures.

A comparison with past attempts at illustrating the submicroscopic world makes us think of the Lorentz electron. The latter, also pictured as a little ball, was said to have a small but finite radius; later, the Copenhagen interpretation surreptitiously converted it into a point. A major difference between Lorentz' electron and the modern quark is that the electron has an observed whole unit of elementary electric charge, whereas the quark is believed to have a not-yet-observed multiple of one-third of elementary charge; a 3-fold symmetry in charge manifestation, though, is feasible.

The comparison between quark and electron reveals how geometric modelling of elementary entities has not since Lorentz made much progress. Both are represented as little spheres. By the time the electron was discovered to have a spin and a magnetic moment, a conversion of the electron ball into an electron ring current had been found to give the wrong gyromagnetic ratio by a factor two. Shortly thereafter, Dirac theory gave a more nearly correct gyroratio, without any geometric model whatsoever. Physics was now ready to accept a point-electron, because it conveniently fitted the requirements of the probability picture of a single electron presence.

The art of living is the art of making concessions. Yet, in doing so, it is important to know when compromise is a temporary rest on the road to further reaching solutions in the future. The electron-quark situation makes us aware of the relatively simplistic models that are being used. In fact there is hardly any model at all. It is either ball or point. Rings had earlier been discarded for their faulty gyro ratio. Conclusion: *Since Lorentz, no efforts have been made to see elementary particles as having a topological presence more sophisticated than a ball or a point.*

Since the geometry of macroscopic objects has been able to invite thoughts about more sophisticated topological structures, one cannot help wondering why Nature would not have used such potentialities in the submicroscopic domain of elementary particles. There are several reasons why contemporary physics has so far refrained from an exploration of these potentialities. Wrong answers at the first attempts created a consensus that in keeping with the successful Dirac approach, abstract procedures were the way to go. It stands to reason to let Nature talk before putting it prematurely into a topological mold. Yet, if ring models were found to be deficient, should not balls and points also be regarded as relics from modelling land?

Closer scrutiny reveals how physics boxed itself into a corner with the single-system extrapolated concept of quantum uncertainty. As it presently stands, it prevents a direct probing of the submicroscopic domain. Then in one of those sweeping classical→nonclassical transitions, it is said that macroscopic notions of space and time are declared to be not applicable in the microdomain (chapterIV, ref.13). Such burning of the bridges to the micro-domain would hardly be justified without first giving the macro→micro extrapolation better scrutiny.

Quantum mechanical uncertainty, as advocated by the Copenhagen School, literally blocks invitations for probing the submicroscopic domain. The thesis of actual physical uncertainty (as distinguished from a more abstract indeterminateness) is equivalent to saying that macroscopic concepts of space simply do not apply in the submicroscopic domain. Physics thus pushed itself into a corner of its own making, from which there is no escape. This has opened the door to many untamed nonclassical conjectures; in panic, anything goes to get out of that corner.

To get out of the corner, it will be necessary to backtrack some on the path that got us into the dead-end situation. Since the inaccessibility of the micro-domain has been at the root of the mounting troubles, let us examine neglected evidence that can help bridge the macro/micro gap of contemporary physics.

2. The Metric Odyssey

The one and only physical entity that can be used to gauge an absolute size of physical objects and an absolute speed of physical manifestations is the spacetime metric. Hence, before launching into untamed speculations about the micro-physical domain, *physics has the responsibility of investigating first the interplay between physical law and the metric.* The metric's physical role and implications were first explicitly brought to the surface of awareness by Minkowski and Einstein. The spatial components of the metric were around, prior to the relativity era. They came already explicitly to the fore in the Lagrangian and Hamilton-Jacobi descriptions of mechanics.

Most physicists are disinclined to accept the metric as a physical field on the same level, say as an electric- or magnetic field. A major reason is that in most physical applications the metric field does not emerge as an explicit physical entity, because the frames of reference, customarily used in physics, have been calibrated beforehand in such a manner that the spatial metric assumes the "invisible" diagonal form $\{1,1,1\}$; the corresponding spacetime form is then $\{c^2,-1,-1,-1\}$.

It was the general theory of relativity that really brought the metric out of the woodwork as having field properties. Its basic postulate holds out for the premise that changes in the metric (field) and its associated calibration, are determined by the distribution of matter. Since this theory has shown a fair measure of experimental relevance, we are well advised not to ignore the metric, even if those changes due to the influence of matter are very small indeed.

Now that the metric is known to be not physically trivial as a field, one may next inquire whether there are physical law statements that do not invoke the metric. After first going out of our way to identify the nature of the metric, it seems odd to subsequently inquire about the existence of metric-independent physical laws. The truth of the matter is: one cannot well establish the independence of something unless that something has first been clearly identified.

The mathematical techniques of establishing metric independence can be rather involved for differential expressions. For integral expressions it can be simpler, because, for integrals, metric independence may frequently be postulated from the start. The differential expressions ensuing from applications of Stokes theorem can be expected to be metric-independent if the cyclic Stokes integral itself is taken metric-independent. **Stokes theorem is a metric-independent diffeo-invariant theorem.**

For differential expressions, metric independence means form-invariance under arbitrary coordinate substitutions, without having to take recourse to metric-related operations such as covariant derivation. Metric independence is to be distinguished from trivial situations in which the metric can be made invisible, such as occurs under the orthogonal groups. Nontrivial metric-independent invariance in physical spacetime is a metric diffeo-4 invariance which invokes neither the metric nor so-called coefficients linear displacement.

To circumvent at this juncture undue mathematical technicalities, it is best to focus on physical laws permitting an integral formulation. In fact, many physical laws do! Many differential formulations are mathematically inferred from integral statements. *The invariant features of physical law-related integrals are found to be conspicuously reflected in their structure from a point of view of physical dimensions.*

Those used to assessing physical entities in terms of physical dimensions may at first be puzzled as to what metric-independence could possibly mean. The physical dimension of almost any physical quantity requires the metric units of time [t] and length [ℓ] for its dimensional characterization. That being the case, how can one ever live in the hope of finding metric-independent quantities and laws involving those quantities? An answer to this question is contingent on the choice of unit systems. More precisely, it depends on a suitable identification and arrangement of basic units.[2]

The old cgs system [m,ℓ,t] only leaves mass [m] as a unit independent of the spatial metric, whereas the MKS system [m,q,ℓ,t] has mass [m] and charge [q] as units independent of the spatial metric. Yet only the unit [q] is a spacetime invariant; mass [m] is not. Consistency about splitting units into invariant units versus those associated with transformation [ℓ,t] invites replacing the noninvariant unit of mass [m] by the spacetime invariant Planck unit [h] of action. The system [h,q,ℓ,t] has the advantage of two invariant units [h,q] versus the two units [l,t] associated with spacetime transformation.[2] Note that Nature provides here the basic elementary units of electric charge [q=e] and of elementary action [h] ; they don't change, regardless of what man chooses to be his preferred units for length or time.

The metric-free laws of physics turn out to relate to one-, two- and three-dimensional cyclic integrals (e.g., Gauss' integral) counting a number of charge units [q] (or [e]), a number of action units [h], or products or ratios thereof. The proven spacetime topological, metric-free, invariance of those cyclic integrals makes their residues totally independent of whimsical human preferences for length and time measures such as inches, meters, noses, seconds or jiffies. Who would like to maintain that the counting of units [e] and [h], whose proven identity is a gift of Nature, could possibly be contingent upon man choosing a spacetime frame with a preordained unit calibration?

While this seems a roundabout way of conveying the existence of metric-free physical laws, the fact is: sometimes only flippancy can shake up certain established opinions. There are several customs in physics pertaining to choices of physical units and several procedures of spacetime invariance which shroud this very issue. The whole thing of metric-independence could be mentioned in two lines, yet experience has taught that communications of such brevity don't register.

In avoiding mathematical technicalities, the counting of universal quantum units is perhaps the most transparent and convincing form in which this message of metric-free physical law can be conveyed. The identification of metric-free physical laws is definitely not a personal invention of this author. The discoveries date back to the early part of this century; some of it took place prior to the general theory of relativity, and later contributions emerged as a by-product thereof. Hargreaves in England and Kottler in Vienna first uncovered features of this peculiarity, then Cartan in Paris and later van Dantzig in Amsterdam; all of them are reviewed by Whittaker.[3] Since nobody quite knew what to do with metric independence, the discovery remained dormant for a long time. Kuessner[4] revived attention for those matters in Germany in 1946. An article by Truesdell and Toupin[5] in the respectable Handbuch der Physik focused on the global formulation of laws of physics and their relation to metric-independence. Yet, physics at large did not heed suggestions that the notion of metric-free could be of any physical consequence.

Since I have been a witness to many responses of pragmatic physicists at the mere suggestion of the existence of metric-free physical laws, I remember some of the commentary. It has ranged from indifference to utter scorn. It elicits remarks as: Metric-free law can have no meaning, **real** physics uses centimeters and seconds; would not the gods do the same?

Metric-free physics indeed sounded rather strange, just at the time when the general theory of relativity had found evidence of the spacetime metric's quantitative role. It is, therefore, not surprising that almost no book on relativity even reports the existence of a metric-free quantum superstructure of relativity. In the context of relativity, the notion of metric-free is not even mentioned as a point of potential mathematical interest!

3. Quantum Mechanics and Relativity (see chapter XV)

Shortly after the emergence of modern quantum mechanics, its compatibility with the premises of relativity started to preoccupy physicists. While Dirac successfully merged quantum mechanics with the special theory, the general theory remained a holdout defying conceptual integration. Efforts to "quantize" the gravitational field remained without physical consequence. The credos of quantization and relativity continued manifesting signs of persistent alienation.

In the meantime no attention had been given to the metric-free nature of at least some quantum laws. Why does Nature reveal metric-free features of quantization (*e.g.*, Aharonov-Bohm and Ampère-Gauss laws), while prevailing efforts in physics have been quantizing metric-related matters such as gravitation, which is exactly the thing that Nature seems to avoid? This brings us to the need for recapitulating the metric-free options on the basis of their physical merits.

The **first** and most important reason is that metric-free laws should be valid in the macro- as well as in the micro-domain, because, *without a metric, laws cannot be restricted by matters pertaining to size or speed.*

The **second** important feature has to do with the nature of the laws that are emerging as metric-free. They are cyclic residue integrals, also called period integrals, counting quantum states of the object under consideration. These period integrals are distinguished by spacetime metric-free general invariance.

Finally the **third** important feature is a natural and *well*-established relation between period integrals and an important branch of topology, which has been pioneered by de Rham. It is known as "de Rham cohomology."

The just stated three principal features largely resolve the presumed incompatibility between the general theory of relativity and the quantum theory.

Why was the general psychological condition in physics so little inclined to give attention to metric-free aspects and the associated notions of topology? Consider hereto that Einstein was not listened to by the pragmatists when he maintained that the principle of general spacetime covariance had played a crucial role for him in the structural make-up of his general theory of relativity. As a result, the subsequent discovery of metric-free spacetime covariance never made the grade in physics, nor did it quite register with Einstein. Correspondence between Cartan and Einstein never touched the topic of metric-free (communication by John Stachel). These are the ironies of history! It was noted earlier that Schroedinger also was not listened to by the pragmatists when he verbalized his disagreement with the Copenhagen interpretation.

Pragmatic physicists have a strange habit of grabbing an item of research, stripping it from what they consider to be redundant detail, and then telling the originator of the research: thank you for what you have just done, but now leave us alone with the version we have given it. This happened to Schroedinger, Einstein and even Maxwell[6] when he pioneered a diversity of vector species. His attempts at explaining to the 19th century physics establishment his electrodynamics in terms of mechanical models backfired, because it was later interpreted as reflecting "classical" limitations in his own understanding.

After Schroedinger's "smeared out" version of the electron had been discarded for valid reasons, the Copenhagen School came out with its probability version, which still remained in the single-system vein of Schroedinger himself. Schroedinger vehemently rejected this alternative. Subsequently, Popper, Einstein and others raised the option of considering an ensemble. By that time few people were listening to Schroedinger, Einstein, or for that matter, the philosopher Popper.

After this had happened, some pragmatists had pangs of conscience and felt something should still be done to reconcile quantum mechanics with the general theory of relativity. As might be expected, the going was not easy, after the principle of general covariance had just been declared void of intrinsic physical content. Several transcriptions of renedering Dirac's special relativistic equations into a generally covariant form were attempted. Except for unbelievably complex equations, nothing came out of it in the sense of new physics.

Yet in the following the principle of general covariance will stand restored, but restricted here to metric-free situations that have a perspective of counting quanta with the help of period integrals. The interpretation of the Schroedinger equation, by contrast, is restricted to ensembles of appropriate disorder--a situation without an obvious bearing on general covariance. Many mysticisms of quantum mechanics and the latter's incompatibility with the general theory of relativity so resolve themselves. Indeed ensemble behavior hardly qualifies as a feature to be given spacetime frame-independence. Yet, if it is to have any meaning at all, counting Nature's natural quanta should better be frame-independent!

Having thus delineated the heretic objective of trimming down Copenhagen applicability to a level of enhanced objective reality, the question still remains how this goal can best be realized. It has been tried many times by people of unquestionable competence and insight. A perusal of Jammer's[7] book on the philosophy of quantum mechanics gives exhaustive evidence of what has been done before. So allow me to state in what respect this issue has now come closer to a state of fruition.

There are now a number of experiments, which more than ever before point to a need for change (*e.g.*, quantum Hall effect, single-particle interferometry and squeezed optical states). Yet, physics remains suspicious of change just for the sake of change, and rightly so. In presenting this subject matter, one finds oneself in a predicament of choice. Presenting the bare physical facts tends to generate a request for more background. Yet if this background comes in part from neighboring disciplines of philosophy and mathematics, people say: give me the physical facts and never mind the philosophy! Let philosophy invent new words and let mathematics worry about proving things that turn out to be perfectly obvious; "we" want physics! These are typical responses to be expected in a science atmosphere that has suffered too long from too much specialization.

An interdisciplinary approach seems the only reasonable answer to resolving these difficulties. Those who want "bare facts" of physics should have no trouble finding them in the table of contents or in references. By the same token, bare facts may lead to conceptual leaps outside the traditional framework of physics. The reader may sense whether they are philosophical or mathematical in nature, or possibly both. A recent philosophically oriented assessment polarizes the issue towards an ensemble aspect of quantum mechanics[8] and the ensuing need of separate tools for ensembles and single systems.

Chapter VI (and following) in this collection give an extensive discussion of period integrals, from their earlier history in physics all the way to their modern use in mathematics as tools for probing topology. Since this procedure is contingent on an abandoning of traditional views of quantum uncertainty, the discussions culminate again and again in a detailed plea of support for an ensemble view of the Schroedinger equation.

Having thus identified ensembles as the selected domain of applicability for Schroedinger's equation, we come full circle by looking for new tools that need to be identified for the treatment of single systems. All of which brings us back to period integrals as the instrument of choice. Their initial use in physical field theory reveals how, quite early in the game, researchers sensed their potential as a bridge between local and global perceptions of Nature. Yet while contemporary physics lost itself in local spinor formalisms, mathematics led the way in exploring global spinorization and a use of period integrals in the topology of manifold structure. Topology is regarded as one of the esoteric branches of modern mathematics, which unfortunately is a quality not helpful for winning popularity in physics.

There is another hurdle! Mathematicians have not always responded in a positive manner to a physics invasion of their territory. Some mathematicians fear injury to their beloved discipline and contamination of the pristine beauty of topology. In the present context they may not approve of impending changes to topology's standard geometric backdrop, if physics becomes a topic of topological concern. The traditionally static geometric backdrop of topology envisioned in mathematics needs in physics an added dynamic aspect of change. Probing into this new territory remains exploratory. It holds promise of giving Feynman diagrams a wider basis in topology. New applications abound, once stumbling blocks are removed (chapters XII, XIII).

Most contemporary inroads made by topology into the realm of physics have not taken place in spacetime itself. Instead superstructures in the realm of multidimensional configuration spaces of quantum states are being probed. In this manner one hopes to learn more about spacetime as the real cradle of those superstructures. The dogma of universal uncertainty has caused this flight from spacetime to a state space of configurations. As long as universal uncertainty rules supreme, it will project its limitations on knowledge about the underlying spacetime.

The multidimensional superstructure predicament is here avoided by probing spacetime directly. Chapter VI first explores what is already there in the form of physical laws that have been or can be cast in terms of residue integrals. These fragments of topological structure are complemented in chapter XIII to form a system in the sense of a de Rham cohomology.

4. Some Milestones in Quantum Physics

In order to see how these matters relate to the past, it will be useful to provide an overview of the conceptual highlights of quantum mechanics between 1900 and 1981. The selection and emphasis of topics has been chosen to serve the purpose of this discussion. (Please consult index for more detailed discussions of topics, names and relevant literature.)

1900: Planck obtains the spectral black body radiation law by interpolating between the Raleigh-Jeans and Wien laws. He achieves this result with the help of the artifact that resonator energy can only change in steps of magnitude $E = \hbar\omega$.

1905: Einstein reaffirms the relevance of the discrete energy exchange $E = \hbar\omega$ in his explanation of the photo-electric effect.

1912: Planck introduces zero-point energy $\hbar\omega/2$ as an ensemble condition for a collective of phase random harmonic oscillators to retain a positive Boltzmann probability. Phase space (plane) discreteness is Planck's favored method of describing quantization: $\iint dp\,dq = h = 2\pi\hbar$.

1913: Bohr extends the Planck-Einstein hypothesis $E = \hbar\omega$ with the hypothesis that angular momentum L changes in discrete steps $L = \hbar$. He thus obtains the spectral formula for hydrogen.

1915: Sommerfeld obtains the fine structure of hydrogen with the help of relativistic considerations. He extends Bohr's angular momentum condition of **1913,** and thus converts Planck's phase space condition of **1912** as a quantization of cyclic phase integrals of analytic dynamics $\oint p\,dq = nh$; n=1,2,... . The latter are now known as the Bohr-Sommerfeld conditions of the "older" quantum theory.

1916: Einstein gives a very elegant and universal derivation of Planck's law of black-body radiation of **1900,** by using concepts of spontaneous and induced emission.

1917: Einstein explores certain topological aspects of the Bohr-Sommerfeld line integral, *e.g.*, how it breaks up into invariant parts characterizing the orbital manifold. Each of the component parts is a multiple of h. Here is, in a way, a physical precursor of de Rham's later mathematical work.

1923: De Broglie converts Bohr's angular momentum condition into a **local** linear momentum condition $\mathbf{p} = \hbar\mathbf{k}$. He postulates, in essence, the proportionality of the four-vectors of energy momentum and the frequency wave vector $(E;\mathbf{p}) = \hbar(\omega;\mathbf{k})$

1923: Duane independently "derives" the relation $\mathbf{p} = \hbar\mathbf{k}$, as restricted to photons, from Bragg's X-ray diffraction formula by using the Bohr-Sommerfeld condition of **1915**.

1925: Heisenberg initiates matrix mechanics. Born and Jordan further mold it into an algorithm for calculating stationary quantum states of "systems" as algebraic eigenvalue process. The method reproduces Planck's zero-point energy of **1912.**

1926: Using the Broglie-Duane relation of **1923,** Schroedinger constructs a wave equation, which also gives quantization as a spectrum of eigenvalues. Schroedinger and Pauli establish the equivalence between the matrix eigenvalue process and the wave eigenvalue process.

1927: Experimental confirmation of de Broglie's relation by Davisson-Germer and Thomson-Reid.

1927: Dirac constructs a wave equation capable of dealing with spin-particles, which amazingly reproduces Sommerfeld's (spinless) fine structure formula of **1915**.

1927: Heisenberg introduces the concept of uncertainty as a general property associated with individual isolated systems. Kennard and Weyl relate this uncertainty to statistical implications of the solutions of the Schroedinger wave equation.

1930: The probability view of quantum mechanics is further codified and emerges as what is now known as the Copenhagen interpretation. Bohr, Born and Heisenberg are regarded as the principal authors of the ensuing descriptional role of the wave function for single systems. This probability picture led to an array of what became later known as *nonclassical* imagery. Here is an overview of the most important and prevalent nonclassical concepts generated by the Copenhagen School:

I Complementarity or wave-particle duality
II The point-particle concept
III Universal uncertainty for single objects
IV The notion of wave function collapse

1932: Von Neumann's completeness suggestion, quantum mechanics as a chapter in the spectral theory of Hilbert spaces.

1934: Popper's ensemble view and criticism of what now had become known as the Copenhagen interpretation of quantum mechanics. The ensemble gave *nonclassical* a subterfuge status. Notwithstanding the consistent appearance, from the Thirties until the present, of a number of reputable quantum texts written in the ensemble vein, the impact of the ensemble view remained very limited.

1935: An attempt at checking the inner consistency of the Copenhagen view by Einstein, Podolsky and Rosen (EPR).

1937-1943: Wheeler-Heisenberg, S-matrix theory, making the wave function independent of the equations of motion.

1951: David Bohm's (local) hidden variables as potential evidence of incompleteness.

1961: The discovery of flux quantization (Doll and Fairbank).

1964: Bell's theorem, as comment on EPR criticism, states consequences of the existence of hidden local variables.

1980: The discovery of the Quantum Hall effect (von Klitzing).

1981: Experiments by Aspect and coworkers rule out hidden local variables by not confirming Bell's inequalities.

A more detailed account of the vacillations concerning hidden variables is given in chapter V. The discussion revolves around the work of von Neumann, Bohm, Bell and Aspect, all of whom view the Schroedinger process as a single-system tool. Also Einstein, Podolsky and Rosen (EPR) took the single-system view, yet with the objective of proving its inconsistency. From a footnote in Einstein's correspondence with Popper we know that Einstein was an ensemble (aggregate) supporter.

Just for the sake of polarizing opinions, let us conclude this milestone overview with a subjective assessment of the measures of surprise generated by some of these major quantum discoveries. The cited measures of surprise are, of course, subject to modification according to the views of the beholder. Here is first an explanation of the abbreviations in the last column of this list:

1: The surprise stroke of genius that led to the correct interpolation between then existing high and low frequency radiation laws. 2: First major application of Planck's new thesis. 3: Surprise payoff of sustained cryogenic program. 4: Bohr's stroke of genius accounting for the hydrogen spectrum is second major application of Planck's thesis. 5: Sommerfeld-Wilson-Epstein extend Bohr's hypothesis. 6: Proposing proportionality of 4-vectors $(E,\mathbf{p})=\hbar(\omega,\mathbf{k})$. 7: Using Bohr-Sommerfeld integral to derive de Broglie's relation $\mathbf{p}=\hbar\mathbf{k}$. 8: Sequence of leads by Compton, Kronig, Goudsmit and Uhlenbeck. 9: First mathematical specifics of a Fermion-Boson distinction. 10: Inductive arguments to transcribe Hamilton-Jacobi commutation relations leading to an algebraic eigenvalue process. 11: Schroedinger's use of de Broglie's relation to obtain a Hilbert-type diffeo-integro eigen-value process. 12: Pauli, Schroedinger, von Neumann relate quantum process with theory of Hilbert spaces. 13: Dirac injects a totally unexpected process for dealing with spin. 14: London predicted in the Thirties flux quanta of twice the size discovered by Doll-Naebauer and Deaver-Fairbank in the Sixties. 15: Hall impedance quanta were totally unexpected by existing theory.

Measure of Surprise →	**0**	**1/4**	**1/2**	**3/4**	**1**	
1 quantum of action					x	Planck
2 photo effect				x		Einstein
3 superconductivity					x	K. Onnes
4 quantum of angul. mom.					x	Bohr
5 quantized action intgr.			x			S-W-E
6 de Broglie's relation					x	relativity
7 Duane's -„-				x		B-S integr.
8 electron spin					x	C-K-G-U
9 excl. principle					x	Pauli
10 matrix mechanics					x	Heisenb.
11 wave equation.					x	Schroed.
12 equivalence of 10 and 11					x	P-S-N.
13 Dirac spin theory					x	spinors
14 flux quanta				x		F. London
15 Hall impedance quanta					x	v. Klitzing

5. Avoiding Interdisciplinary Alienation

Through the centuries, mathematics and physics have been closely related disciplines. Sometimes physical inquiry led to the creation of a new mathematical discipline. The emergence of calculus as a tool needed for dealing with the Newtonian laws of motion is a conspicuous example. At other times there have been instances in which existing mathematics was instrumental for the development of new physical disciplines. Examples are the influence of group theory on the development of crystallography. The existence of Riemannian geometry was crucial for the formulation of the general theory of relativity. Finally, there is the theory of Hilbert spaces as a tool for quantum mechanics.

A cursory glance at these examples already reveals a change in the interdisciplinary relations between mathematics and physics. While, in Newton's and Euler's days, much new mathematics had its origin in physical inquiry, in recent times much basic physics acquired, from the start, strong roots in the available mathematics. This change in interdependence has manifested itself in mathematics as an awareness of having come on its own. Mathematics is no longer the handmaiden of physics. In the perspective of the traditional references to physics and mathematics as the "king and queen" of the sciences, it now appears that the "queen" has become a fully emancipated lady.

There is now ample evidence that shows how mathematical research can help spawn new avenues of inquiry in physics. Whenever such changes in the mutual balance of interdependence are taking place, it is not unusual to see a tendency of going slightly overboard with a newly gained awareness of independence. Similarly as the Aristotelians envisioned a physics that could be guided solely by pure thought, without recourse to experimentation, some mathematicians felt it might be better for their beloved discipline to stay "clean" by keeping away from physics.

There is no denying that there has been growing distance between mathematics and physics in this century. This alienation has not been good for science as a whole. It is the curse of overspecialization, compounded by uncalled-for discipline chauvinism, that is triggering such alienation. It creates a Tower of Babel effect; once languages begin to differ, it becomes more difficult to bridge the ensuing gaps in communications.

A practical result of the here-cited divergence is the following. Mathematicians, these days, get away with studying much less of physics than a century ago, whereas physicists are literally forced to study much more mathematics that has been produced by mathematicians who don't care too much for physics. The added mathematical burden in physics is bound to affect the quality of maturing. Faced with this information explosion, physics is coping with the situation, rather than controling it.

Whenever there is trouble mastering a new field, one can attempt to bolster the damaged ego by playing down the importance and relevance of all that newfangled stuff. Physicists have been heard loudly accusing mathematicians of wasting their time proving perfectly obvious things. Conversely, mathematicians accuse physicists of making uncivilized leaps of faith that are unacceptable in polite mathematical company.

While there is little wisdom or virtue in fostering these festering symptoms of alienation, the fact is, they are existing reality and as such they make constructive cooperation more difficult. Even if the pressure of research funding can force a measure of cooperation, such funding forced cooperation pitches mathematicians (with, at best, a token background in physics) against physicists, who have a necessarily restricted feel and understanding for the sophistication of contemporary mathematical tooling.

The result of this confrontational situation is that the injection of new mathematical tooling in physics tends to occur somewhat ad hoc at intermediate levels. There is no adequate attempt to start from scratch when it comes to mathematizing physical laws. Mathematicians, with little earlier exposure to physics, are hardly inclined to tinker with the formulation of fundamental physical laws as they are handed over from physics. By the same token physicists are too unfamiliar with the new mathematical techniques to feel at ease applying them to their full advantage. It is not hard to identify developments of this kind in physical theory over the past half century. It is all part of the so-called information explosion. There is an increasing flow of information of decreasing coherence.

The following chapters are an attempt at removing obstacles standing, right from the start, in the way of injecting new mathematical methodology. It will therefore be necessary to start at the classical level. In so doing, transitions to the so-called nonclassical levels of quantization assume a more natural character. *In fact, quantization is a much more classical feature than it was made out to be in the past!*

This approach collides with prevailing sentiments in physics. Topology assumes a more prominent place in the following chapters than what has been customary in standard physics discussions. By the same token a physics backdrop for topology militates against major sentiments in mathematics, which has so far preferred a static geometric backdrop. For reasons only mathematicians know, it has been tradition that geometry has been considered as part of mathematics. For reasons only physicists know, physics has easily reconciled itself with the idea of leaving geometry in the hands of the mathematicians.

The reality of the physical world, however, demands a spacetime kinematic backdrop which imposes from the start a topological characterization distinguishing between "static" object structures and physical events, where seemingly "static" structures are converted into other seemingly "static" structures. The truth of the matter is, however, that the apparent static structures have a pronounced inner dynamics disguised by an image of stationarity, which only makes it appear as static. The dynamics becomes even more dynamic, when the stationary dynamics has to make place for a conversion dynamics. It is the familiar scene in particle physics, when particles end their existence either by spontaneous disintegration into other more stable particles or else by a collision-induced disintegration.

In the sequel of these quantum reprogramming exercises, we may expect a confrontation with two major conceptual obstacles standing in the way of an intellectual reorientation in physics. First of all, there is the need to wean ourselves away from the Copenhagen suggestion that the Schroedinger equation is a cure-all for all conceivable quantum ailments. All of us have been brought up with such a not-altogether-justifiable belief that standard quantum mechanics will do the trick, if we only knew how. It is based on man's natural tendency for replacing lack of understanding by a measure of religious fervor; how else are we going to accept in good faith what cannot be quite accepted on the basis of strict logical persuasion?

Last, but not least, let us not close the eyes to the fact that even logical persuasion itself has, from time to time, been known to take recourse to a dogmatic sleight of hand.

So recognizing the realities of life by accepting the possibility that the Copenhagen view of quantum mechanics may not be quite the last word in physics' revelation to mankind, the question is: how can such act of heresy lead us to new and other worthwhile revelations that leave intact those elements that were known to be good in earlier experiences?

Let it be a source of comfort that some of that "new stuff" discussed here has already been around for some time. We just have a look at it from a slightly different angle. Period integrals, as discussed in later chapters, have been an integral part of physics for centuries. All the time we learn new things about them. In the process of weaning ourselves away from the cure-all suggestions of standard quantum methods, it is good to know that Copenhagen can be modified and complemented just by eliminating some uncalled-for interdisciplinary alienation.

The world is full of salesmanship. Everywhere are people who overestimate our resources. They want us to discard everything we have or know. Yet rather than blindly following such advice, it is reasonable to know beforehand a little bit of what we are getting into. It is all part of not rushing into decisions, the consequences of which we cannot fathom.

CHAPTER III

REASSESSING COPENHAGEN

1. Getting There and Being There

In the eternal quest for truth, man has this very natural desire of finally wanting to arrive at some place of destination. It is the urge of trading a state of "getting there" for a state of "being there." We usually settle for a place, without having a clear idea of what the destination might be. After traveling long enough, we are tired, and just about any destination will do. The decision to keep moving or to rest is contingent upon whether we are at ease at the place of temporary residence.

In the process of traveling, it can happen that our ideas about the place of destination change. It is indeed a universal experience that our ideas of a place we have never seen before, but intend to visit, are totally different from the impressions we get once we see the place with our own eyes. This travel metaphor applies not only to places we visit physically, it also applies to people we have heard about, with whom we then later get acquainted. By the same token, a field of studies is another example of something that can turn out to be totally different from what we initially imagined it to be.

It can also happen that we arrive at places where we have never been, and yet we have this uncanny feeling of having been there. Most people like this experience, because it gives them a feeling of familiarity, say, of being at home. We like being at home unless dire experiences in life have made us apprehensive about home. Yet, even if home is not a good place, we tend to defend it in the hope of making it better at some time in the future.

The travel metaphor pervades all of man's pursuits in life. It can be traveling itself or constructing something; it also can be learning a new language or a new trade. Some people like to work on things and never finish; some car and boat owners are that way. They always work and never enjoy the fruits of their labor. There is an immense variety of spiritual and intellectual experiences qualifying for this characterization. Let us scrutinize some, to ease the steps towards topics pertaining to physics.

In theology, man creates a world picture; the guidance it gives may be summarized in holy scriptures. They reflect the wisdom of earlier generations, or they are said to be "divinely inspired." Then, if new knowledge and experiences make it necessary to reinterpret the scriptures on which this world picture is based, it is done with great care and circumspection. This process is, by nature, slow. Too many changes in too short a time cause the spiritual home to lose its aura of comfort and protection. It might come to pass that a home is no longer a home. We feel estranged and move about without much aim, trusting the guidance of serendipity.

To counteract the thus ensuing restlessness, some protectors and preachers of the faith go to great length in reconciling new findings of science with their holy scriptures. They don't rest until they succeed in proving how everything newly discovered was already anticipated in their scriptures; thus confirming their divine and holy sources of inspiration. They attempt replacing faith and belief in the holy scriptures by a conviction that their veracity stands, no matter what. While such techniques offer temporary solace, they harbor a danger of building faith on false convictions.

Since findings of science have been frequent causes of conflict between faith and conviction in theology, one would think science itself to be free of this protecting of the comforts of "being there" versus the uncertainties of "going there." Science is believed to have a measure of self-criticism not to get caught in this trap of complacency about having arrived at a station of knowledge where further exploration would not be necessary.

One would hope the syndrome of having reached a last stone of wisdom is unthinkable for science. Yet, since those who practice science are as imperfect as other mortals, one can be sure of finding the whole gamut of characters all the way from the perennial tinkerers to those who know it all. This can best be illustrated by inspecting one of the more esoteric disciplines of modern physics: quantum mechanics.

2. A Reminder of Not Yet Being There

An account of quantum history reveals some almost unavoidable religious elements invading the territory of science during the 1925-1935 revolution. The already adopted terminology referring to a transition from classical to nonclassical physics assumed, during that time, an altogether new meaning. In a letter to Schroedinger, who was one of the unwilling activators of this movement, Einstein[1] explicitly alluded to the soothing religious quality of the Bohr-Born-Heisenberg (Copenhagen) interpretation of the new quantum formalism. A book on the philosophy of quantum mechanics by Jammer[2] gives ample testimony about this state of affairs. Look for references to Ballentine, Blokhintsev, Bunge, Collins, Einstein, Groenewold, Kemble, Landé, Popper, Slater and many others.

This chapter is an effort at explicitly pinning down some major elements of this religious infusion. There is the understandable tradition of using the methods of quantum mechanics (QM) as a cure-all for too many ailments and problems in physics. Since the tools of QM came as a "gift" from heaven, their universal applicability has been taken too much as an almost foregone conclusion. This frame of mind tends to create a predilection for over-reliance on "gifts." It is our responsibility to probe the application potential of gifts in order to use them wisely.

In cases where reason could no longer guide the path concerning questions of applicability, man had to take recourse to a number of articles of faith. A major article claims, somewhat tongue in cheek, that the new QM methods would apply equally to single systems as to ensembles of such single systems. The expected asymptotic physical behavior of a single system and the observed behavior of an actual ensemble of such single systems encouraged this dichotomous applicability.

This very extrapolation of dual applicability would later come to haunt physics in the form of a proposition holding a single particle to be dual to the essentially plural concept of wave. This led to several related conceptual contortions, all labeled under the heading *nonclassical.* Given the undisputed probability connotation of the wave function, the decision of single system applicability created a predicament as to what this probability was all about. It led to *a priori quantum uncertainty* and a corresponding *zero-point energy affecting also single isolated particles.* Last but not least, it gave birth to the tongue-in-cheek conception of the so-called *point-particle.*

Once the nonclassical road had been chosen, it became compelling to invoke many more nonclassical constructs. In fact it triggers a never-ending need for ideas such as nonclassical statistics, nonclassical logic, hidden variables, including internal consistency checks as embodied in Bell's theorem. In short, a near-unlimited number of things, not understandable in terms of more conventional concepts, had to be collected under the indeterminate heading of nonclassical.

Of course, physics would not be a discipline covering an ever-widening area of unusual phenomena if everything had to be assessed in terms of already known conventional concepts. Yet, whenever the initial steps of change develop into an avalanche of ever-increasing change, such facts are to be heeded as a warning that something got out of hand. This impression is reinforced if one considers that all these perennial Copenhagen problems can be resolved in almost one fell swoop if the QM methods are being restricted to ensembles of identical systems; provided these ensembles obey a measure of disorder regarding mutual system phase and system orientation. It can hardly be denied that all observational evidence underlying the QM procedures of 1925-1935 was, without exception, ensemble-based. Mindful of this factual state of affairs, one should now explicitly ask the question: what prompted the QM extrapolation to single systems, and what was the justification?

Answering the last part of the question first, one finds that no contemporary textbook of QM offers a justification for this extrapolation. All of which brings us to the first part of the question: what prompted it? It is hard to say what prompted a tacit extrapolation without an explicitly verbalized justification. Most likely, it was an overriding desire of having and obtaining a truly fundamental theory. It became an article of faith! The confidence in this article of faith was indirectly reinforced.

The new QM methods, for instance, confirmed the selection rules, the zero-point energy and the quantum number $\sqrt{n(n+1)}$ for angular momentum gave a better spectral fit in the formula of the Landé spin-orbit coupling. Yet almost all, if not all, textbooks fail to mention how Planck,[3] more than a decade earlier, had already introduced zero-point energy as an ensemble property. In section 5 of this chapter, it is shown how the quantum number $\sqrt{n(n+1)}$ also permits an ensemble connotation.[4]

The validity of extrapolating these ensemble features to single systems by the Copenhagen view was unlikely to be contradicted as long as single-system observations remained experimentally rare events. The spectacular experiments of Dehmelt and coworkers finally isolated the single particle. In section 9 of chapter XI, options are discussed for the isolated point-electron with "Zitterbewegung" and the isolated trefoil electron without zero-point energy. On the basis of that testimony, the reader is invited to reassess Copenhagen's thesis of universal zero-point energy, regardless of whether or not a system or particle is part of an ensemble.

The physics community at large had its private reasons for hanging on to the new mystique of QM methodology. It helped to perpetuate the illusion of having an all-encompassing theory. Even if the connotations of wider applicability were purely based on unproven faith, the truth was deceptively masked by supportive asymptotics and the beautiful, rigorous, mathematical structure of Hilbert's spectral theory. So, notwithstanding the absence of a direct proof of single-system applicability, its viability in that realm became a cornerstone of Copenhagen's statistical interpretation.

The subsequent successes of the theory were so absolutely overwhelming that any need for further verification of its basic premises seemed unnecessary. Dirac's identification of particle spin and magnetic moment then enhanced a nearly absolute belief in a magical all around applicability. The epistemological objections of Einstein and Popper soon were forgotten, and so were the claims by Ballentine, Collins, Groenewold and others about ensemble oriented implications of the ψ function.

Yet, more glory was to come. An energy level of hydrogen, which, according to the Dirac theory, had to be degenerate, was, in fact, found to be a doublet. A zero-point energy-based superstructure of QM, known as quantum electrodynamics (QED), has provided unique services for a calculation of this doublet separation (Lamb shift). Measurement and calculation agreed over four decimal places, superimposed on a base level known to about five decimal places. Some preachers of the QED gospel cited the combined nine decimal places as an unparalleled precision in the history of physics. The same theory, for the first time, also accounted for the higher- order differences of the anomalous magnetic moments of the electron and muon.

With such abundance of measured and calculational precision, QED seemed to be an ideal candidate for making very accurate determinations of Planck's fundamental constant of action h and the elementary charge e. Yet dramatic fluctuations in recommended h and e values, published[5] during the decades between 1950 and 1980 and collected in **Table I** of chapter VIII, proved otherwise. Here was testimony that something was not yet right.

When, during the Eighties, combined Josephson- and quantum Hall effect measurements gave reproducible h and e values over 7 to 8 decimal places, the QED data were found to have fluctuated in the fourth decimal place between 1950 and 1980. The selection of numbers in **Table I** is again cited for comparison. Notwithstanding QED's tremendous services in better understanding the Lamb shift and electron muon anomalies, its h and e values still were reminders that QED had not arrived yet.

While the numbers of **Table I** testify to difficulties within QED itself, there has been little inclination to rectify the situation by going back all the way to the premises of QM as the mother discipline of QED. The Popper ensemble option of QM does not relate well to QED, because the latter is strongly dependent on Copenhagen premises. Here is the very reason why it is so difficult to give up Copenhagen, notwithstanding the misgivings that even its most faithful supporters must have about its basic premises. The truth of the matter is that while the Popper alternative has fewer unproven premises of faith, it upsets the apple cart by requiring a totally different QED approach. This challenge has to be met squarely if the Popper initiative is to give at least a perspective on a possibility of competing with standard QED results. Attempts in this direction have been made in chapters VIII;2 and XI;7,9. Without at least this glimpse of a perspective on QED alternatives, the chances for Popper's initiative to win out over Copenhagen would be, if not hopeless, bleak indeed.

Here we have at least some rationale why so many eloquent efforts of the past have failed. The present motivation for reopening these issues to further scrutiny has found much encouragement in some important new experimental results that have recently become available. Mindful that past articles of faith cannot always be lifted to the level of rational truth, let us go to work to see what else needs to be changed.

3. Single Particle Diffraction

For a long time, diffraction phenomena were regarded as typical wave manifestations. By the end of the 19th century, Huygens principle, Young and Fresnel's experiments and last, but not least, Maxwell's theory of the electromagnetic field seemingly made the Newtonian particle picture of light quite unthinkable. In the beginning of the 20th century, the photoelectric effect, and Einstein's explanation thereof, brought an unexpected change in this situation. Here was a resurrection of Newton's particle idea!

The development of X-ray technology subsequently revealed a particle nature of light. The Geiger-Mueller counter made it possible to register and count individual photon absorption events. The persistent reminders of particle connotations in phenomena believed to be wave-related, led to questions as to whether diffraction might also be describable as a particle manifestation by using extended first principles of mechanics.

In 1923, W. Duane[6] confronted this very challenge. He succeeded in deriving a diffraction relation by imposing quantum conditions on Newton's collision treatment of light diffraction. The basic train of thoughts is as follows:

A particle of momentum p hits a reflecting surface under an angle of incidence θ. The Newtonian exchange of momentum is then

$$p = 2p \cos \theta. \qquad 1$$

Following Bragg, the wave description of diffraction can be accomplished without a need to call on quantization. The particle-based description, surprisingly, invokes quantization. It is one of those reminders that classical and nonclassical are much closer than we think. So, let us not exaggerate differences where Nature gives us an inkling of closeness. Duane assumed that particle collision could be governed by the Bohr-Sommerfeld condition:

$$\oint p\cdot dq = nh; \text{ with } n=1,2,3,... \qquad 2$$

The periodicity implied by this loop integral is provided by the lattice structure of the material surface hit by the particle. Let us assume that the surface of reflection coincides with the lattice surface of a single crystal with lattice spacing d, the integral Eq.2 written out with the help of Eq.1 then becomes:

$$\oint p\cdot dq = 2pd \cos\theta = nh, \qquad 3$$

A comparison between the particle diffraction formula Eq.3 and Bragg's wave diffraction formula is now indicated.

For a lattice distance d, irradiation with wave length λ and lattice surface incidence angle θ, Bragg's formula is

$$2d \cos \theta = n \lambda \quad \text{; also with } n=1,2,3,... \qquad 4$$

A comparison between Duane's particle formula Eq.3 and Bragg's wave formula Eq.4 yields as condition for identification:

$$p = h/\lambda \, . \qquad 5$$

Eq.5 is recognized as de Broglie's famous relation between particle momentum p and wavelength λ.

Historically and quite independently of one another, Duane and de Broglie proposed the relation Eq.5 in the same year 1923. Their motivations though were just about diametrically opposite.

De Broglie, intrigued by the relativistic relation between the frequency-wave vector $\{\omega,\mathbf{k}\}$ and the energy-momentum vector $\{E,\mathbf{p}\}$, extended the Planck-Einstein relation $E=\hbar\omega$ to the spatial domain, thus leading to the vector relation $\mathbf{p}=\hbar\mathbf{k}$ with $\hbar = h/2\pi$. This procedure inspired him to look for a wave aspect of particles with rest-mass.

By contrast, Duane sought to establish a zero rest-mass particle aspect of hard X-rays, because that is what X-ray experimentation seemed to call for. Interestingly, de Broglie's proposition for rest-mass particles was strikingly confirmed a few years later by Davisson-Germer. Their work was unrelated to de Broglie's hypothesis; they were investigating the surface structure of crystals by bouncing slow electrons from single-crystal metals. They found, as a side effect, the angle preference according to Eqs.3 and 5. Fresnel's premature wave triumph thus had turned into a veritable wave-particle duality.

Using coincidence counting, it has been possible in recent time to do diffraction experiment with single particles. Aspect[7] *et al* have done so with photons and Rauch[8] *et al* with neutrons. It has been found that a single particle does not seem to divide itself to follow its diffraction options; it does so all by itself and knows exactly where to go once the option choice has been made. A wave analysis cannot quite account for this behavior, yet the particle analysis given by Duane, using Bohr-Sommerfeld conditions, indeed accounts for these findings. He really anticipated this behavior on the grounds of the, at that time, observed behavior with very low intensity X-ray beams. The Aspect-Rauch experiments thus brought home the particle-wave duality with a renewed intensity.

One of the conclusions emerging from the here cited results is certainly that diffraction phenomena are not to be taken as unambiguous evidence for the presence of waves. Conversely, in the spirit of Hamilton's "eikonal" analysis, waves can be expected to have particle-like manifestations, if only in the limiting case of a geometric-optic approximation. Since, in a wave sense, diffraction can only come about through wave interference, waves were always taken to be a *sine qua non* for diffraction.

This point no longer holds if we give Duane's analysis a measure of relevance. It also means the particle-wave duality stops being effective if we try to represent one particle by a wave. The conclusion stands to reason if we consider that waves have, by nature, a plurality connotation; single particles don't because unlike waves they cannot be divided. Hence, *we can at best speak of a wave-many particle duality.*

The birth of the new quantum mechanics a few years later, never gave Duane's position a chance to come out of the woodwork. The particle diffraction exhibited by earlier experimentation, prior to Aspect and Rauch, had always involved a large number of particles (e.g., Davisson-Germer and Debye-Scherrer, Thomson).

The transcription used by Schroedinger to obtain his wave equation was indeed a single-particle→wave transcription. The ensuing full duality conjecture between wave and single particle was understandable; it was not a necessary consequence. Later there have been derivations of the Schroedinger equation using ensembles of many identical, similarly prepared systems. Hence, full duality between wave and many "single particles" became a feasible proposition.

A single system with many particles (and ensembles thereof) leads to a confrontation with the more than two body problem, which, in QM, is as intractable as in celestial mechanics. From the QM angle in the Popper perspective, there is an added complication. Order rules inside the many-particle systems, yet phase and orientation disorder rules on the outside between these systems as constituents of an ensemble. The mystery of the Schroedinger process is certainly its dual capability of effectively dealing simultaneously with internal system order and outside ensemble disorder.

A qualitative understanding of the spectra of more body systems has been enhanced by the consideration of structural symmetry principles. Their group description and their relation to a subsequent lifting of the degeneracy in energy levels has much enhanced spectral understanding. Yet, an alien principle has to enter the scene to create order in the over-abundance of possible solutions of the many-particle Schroedinger equation. The Pauli principle assumes this role; it helps in making the distinctions between fermion and boson particles. Berry[9] *et al* have opened up new horizons for model-based order principles in atomic structure. By promoting a global-oriented idea of collective particle behavior, they have been introducing principles of a kinematic, topological flavor.

Although electron spin has a major role in implementing the Pauli principle, the Dirac procedure, so successful in preparing the one-particle Schroedinger equation for spin description, has sadly failed in the case of the many-particle Schroedinger equation. Even in the single-particle case, we become acquainted with many different species of Ψ functions: instead of one component scalar Ψ functions, there are now vector and spinor type wave functions. The relation of the latter to statistics is much less transparent than in the case of a scalar Ψ.

Suppose that the Schroedinger equation and its Dirac and many particle offshoots, all claiming their characteristic domains of relevance, are really ensemble description tools in disguise. How do their Ψ functions, as all-encompassing local-global tools, manage to selectively perform that double assignment of simultaneously describing the system's inner order and outer ensemble statistics of mutual system interaction?

This, in a nutshell, is a crucial question, with which the Copenhagen proponents might successfully stump their ensemble brethren. A partial yet conspicuous answer appears in section 6 of this chapter. For the two-dimensional harmonic oscillator, the condition of Ψ single-valuedness alone gives $E=n\hbar\omega$, without zero-point energy. Square integrability of Ψ, by contrast, emerges as the key to zero-point energy: $n \to n+1/2$. The latter may, by now, be accepted as a typical ensemble feature.

It really should have been incumbent on those who have elevated the Schroedinger equation and its offshoots to the position of a gift from heaven to identify in more detail what their Ψ function options are all about!

It has been customary to demand from those who are trying out alternatives to reconfirm Copenhagen's preconceived premises, because they have worked so well. Imposing such conditions has become somewhat an automatic establishment reaction. Demands of this nature are not quite fair. Here is evidence of the Ψ function's verified double purpose; Ψ gives information "in the small" as well as "in the large."

Confronted with the here depicted state of affairs, modern physics' attitude has been somewhat less than candid. It has been recommending near-absolute faith in the rules of modern quantum mechanics, knowing full well that the asymptotics between Schroedinger and Bohr-Sommerfeld methods can mask alternatives of interpretation. Specifically in question is the conceptual incongruence of describing a single particle with the plurality tool of waves. Closing the eyes for this incongruence is, at best, a temporarily permissible act of strict pragmatism, until new facts and logic command a reassessment of the situation.

At this point, the choice is ours: either we add to the receptacle of nonclassical enigmas, or we give Duane's wave$\rightarrow$particle alternative a little more of a new lease on life.

4. Dipping below Heisenberg Uncertainty

An interesting series of optical processing experiments has been performed recently with light beams. Using a technique of beam-splitting and trading phase noise against amplitude noise, one eventually ends up with a beam component in which, in terms of the field version of uncertainty, the noise has been "squeezed" below the Heisenberg level.[10]

The originators of these squeezing procedures hasten to mention that for any component below the Heisenberg limit, there is an accompanying component correspondingly above that limit. Hence, *for the combined components, with or without the artifact of squeezing, the Heisenberg limit remains intact.*

The squeezers use a familiar approach in contemporary physics. They have one phrase to catch the attention, intimating some law might be violated. Then, once they have the attention, they follow up with a soothing declaration that all physical laws remain intact. To clarify this capitalizing on an apparent contradiction we need to ask: how does particle uncertainty transcribe into wave language and vice versa?

Since the wave concept has an inherent plurality connotation, the Heisenberg uncertainty for fields has automatically assumed an ensemble connotation. Hence, a conceptually meaningful equivalence between wave-field uncertainty and particle uncertainty necessarily gives particle uncertainty a plurality connotation. The latter conclusion, though, is at variance with the Copenhagen view of quantum mechanical uncertainty. The contradiction is obviously resolved by assigning an ensemble status to the Schroedinger solutions. We then have ensembles on either side of the wave-particle duality.

Having established an ensemble status for the optical wave field and its associated wave uncertainty, we now need to ask: what does the process of squeezing do to the optical wave field and its uncertainty-implied randomness? Squeezing presumably affects the state of randomness of the optical ensembles here considered. *Once an ensemble makes a transition from disorder to order, a dipping below the Heisenberg level of uncertainty does not violate any law, all of which obviates the need to apologize for such behavior.*

In assessing this situation, we fortunately did not need to delve into the details of QED derivations used in the theory of squeezed states. The end result of a component below Heisenberg uncertainty will do. The simple application of **true** wave-particle duality indicates already that Heisenberg uncertainty does not have to be obeyed by single particles. In other words, **Heisenberg uncertainty is at best an ensemble property.** It then follows, the Schroedinger equation from which Heisenberg uncertainty obtains must be an ensemble tool.

5. Zero-Point Statistics of the "Copenhagen" Ensemble

The dictionary definition of the word "ensemble" in chapter I still can guide us with disordered zero-point ensembles, for which I propose the name "Copenhagen" or "Schroedinger" ensemble. Since disordered ensembles are susceptible to Schroedinger treatments, the zero-point energy $\hbar\omega/2$ per harmonic oscillator is then known to emerge as an automatic byproduct of the method. In the spirit of the ensemble interpretation, $\hbar\omega/2$ is the energy *average* per harmonic oscillator of a phase randomized ensemble at absolute zero. There is no implication whatsoever that some oscillators or all oscillators must be assumed to have a mystery level of half a quantum of energy $\hbar\omega/2$.

The individual oscillators, when regarded as isolated entities, have an energy spectrum $n\hbar\omega$ with n as an integer. The latter expression is derivable from the period integral approach, based on the idea that a single system is to be regarded as belonging in the category of ordered ensembles. Using this single-system result, the question may be asked whether zero-point energy is a possible byproduct of ensemble disorder. Planck has shown exactly that. Yet, before we paraphrase his result we need to establish what constitutes ensemble disorder near the zero-point. The parameters need to be identified describing order-disorder transitions near the absolute zero.

If it is true that Schroedinger's equation describes an ensemble of systems instead of a single system, then it follows that we no longer need to go out of our way to find strange and unnatural functions for the statistical implications of the Ψ function. Instead of having a single particle mysteriously hopping around in some sort of a lonely zero-point dance of isolation, the Ψ function statistics now may be associated with the mutual behavior of systems in an ensemble. The next question is: what is it in the mutual system behavior in the ensemble, that may be presumed to be subject to statistical fluctuations?

Since Born's Ψ function statistics has no thermal connotations, the possible association with a zero-point phase change is an option to be considered. Similarly as for phase transitions at given finite temperatures, also near the zero-point phase transitions are known to take place. They usually relate to the creation and/or annihilation of new degrees of freedom, say comparable to order-disorder transitions associated with melting and evaporation at higher temperature. During these order-disorder phase transitions, the temperature is known to remain constant, thus lending a nonthermal quality to the process.

It is reasonable to conceive of similar order/disorder phase transitions taking place for the low temperature cooperative effects, placing them in a category associated with a gradual (ensemble→single system) demise of the Ψ function statistics.

Consider hereto an ensemble of identical dynamical systems, and let us inquire: *what ensemble parameters don't play a role in the thermal performance of such an ensemble?* As possible candidates belonging to this category, *one may consider the earlier-mentioned mutual phases of the dynamical systems and their mutual orientations.* At this point, we should become aware of the *nonlocal quality of mutual phase and orientation.* It means they are not to be considered as local parameters characteristic of individual constituent systems of the ensemble. The importance of this observation is that the nonlocal parameter-quality of mutual phase and orientation permit us to steer clear of local hidden variable theory and its Nemesis known as Bell's inequality.

After this long-winded introduction, which was necessary to delineate the situation with respect to contemporary theory, we may now be willing to go back in time and see how Planck[3] has led the way in exploring this nonthermal or pre-thermal statistics in physics. Let us consider an ensemble of Planck's favorite harmonic oscillators. They are all identical with inertia m and stiffness k. Each has an energy

$$E = (1/2m)p^2 + (1/2)kq^2 \qquad 6$$

where $p = m\dot{q}$ is the momentum and q is the amplitude. Eq.6 gives the energy per oscillator with q and p varying between oscillators. If $\omega = \sqrt{k/m}$ is the frequency, $\hat{q}$ = maximum amplitude and ϕ = phase is, the q,p solutions are:

$$q = \hat{q}\,\sin(\omega t+\phi)\ ;\ p = \omega m\hat{q}\,\cos(\omega t+\phi). \qquad 7$$

Substitution of Eq.7 into Eq.6 yields

$$E = (1/2)\,k\hat{q}^2. \qquad 8$$

A phase space average for the ensemble is given by

$$\langle E\rangle = \frac{\iint E\,dp\,dq}{\iint dp\,dq}\,. \qquad 9$$

The evaluation of Eq.9 is greatly simplified by making the transformation of variables $(p;q)\rightarrow(\hat{q};\phi)$. The Jacobian J of this transformation is:

$$J = m\hat{q}\omega\,. \qquad 10$$

An evaluation of Eq.9 for $\hat{q}$ between the limits $\hat{q}_{II}$ and $\hat{q}_{I}$, and for ϕ the phase averaging from 0 to 2π, now gives for the energy average:

$$\langle E\rangle = (k/4)\,\frac{\hat{q}_{II}^4-\hat{q}_I^4}{\hat{q}_{II}^2-\hat{q}_I^2} = (k/4)\,(\hat{q}_{II}^2+\hat{q}_I^2) \qquad 11$$

No quantization has so far been introduced; following Planck, we shall now assume that the $\hat{q}$ limit II corresponds to a quantum state n+1, and the $\hat{q}$ limit I corresponds to a quantum state n, Eq.8 then gives

$$E_{II} = (1/2)\, k\hat{q}_{II}^2 = (n+1)\hbar\omega, \qquad 12a$$

$$E_I = (1/2)\, k\hat{q}_I^2 = n\hbar\omega\,. \qquad 12b$$

A little bit of algebra between Eqs.11 and 12 gives the familiar relation

$$\langle E\rangle = E_n = (n + 1/2)\,\hbar\omega \qquad 13$$

as an ensemble average for harmonic oscillators for the states n and n+1.

The result of Eq.13 is not all that surprising; one would expect an end result between n and n+1. The interesting part is whether or not the ensemble average can become zero, because n=0 gives a residual energy $\hbar\omega/2$ per oscillator.

At this point, Planck goes a crucial step further. Starting from the premise that a probability is by nature a positive quantity, he shows that without this residual energy there can be no "thermodynamic" or statistical equilibrium for the ensemble.

Here is a condensed version of Planck's argument. Let N_n be the number of oscillators in the statistical energy state E_n denoted by Eq.13. The total energy E of the ensemble is then

$$E = \sum N_n E_n$$

Let N be the total number of oscillators. In a state of equilibrium the number

$$w_n = N_n/N$$

may then be regarded as the probability of an oscillator to be in the state n, and so it obeys the sum rule

$$\sum w_n = 1.$$

Now multiplying Eq.13 by N_n and summing over n gives, after using the above three relations, the result:

$$N\sum n w_n = E - N\,\hbar\omega/2.$$

Since $n>0$ and $w_n>0$, it follows that $E/N \geqslant \hbar\omega/2$, *q.e.d.*. Planck optimizes the entropy and also calculates the "phase" distribution law, he thus proves w_n to be positive (see p.141 of ref.3).

All things being equal, it has to be said that Planck's zero-point energy $\hbar\omega/2$ is clearly cause-related and thoroughly thought through, unlike the "surprise" zero-point energy $\hbar\omega/2$ that comes out of the Schroedinger equation.

The Schroedinger $\hbar\omega/2$ is a surprise gift from nature, which was produced by an equation which, as Schroedinger basically did admit, came as a gift from heaven. We don't know quite what that equation means, unless we are willing to experiment and accept, as potential work hypothesis, that a use of the Schroedinger equation tacitly invokes an ensemble situation.

An inspection of the contemporary textbook literature on quantum mechanics reveals not a word about Planck's zero-point work, which incidentally preceded the 1925 quantum revolution by more than a decade. The mathematical machinery of the Schroedinger process automatically gives the result, just by turning the eigenvalue crank. This process, it needs to be said, does not invite much further thought about the physics.

Let us now examine the ensemble origin of the Schroedinger angular momentum state $\sqrt{n(n+1)}\hbar$ as compared to the $n\hbar$ states of the quantum era prior to 1925; the latter also equates to Schroedinger's fixed axis result. Assuming random orientation of an ensemble in the nth quantum state, the discrete "observable" quantum projections in a given direction x are the 2n+1 states:

$$-n,-(n-1),-(n-2),..\ -3,-2,-1,0,1,2,3,........(n-2),\ (n-1),n \qquad 14$$

For a total of N systems and near-perfect ensemble randomness, each state can be expected to be filled by N/(2n+1) systems. Since the total angular momentum L of a perfectly random ensemble can be expected to vanish, it will be more interesting to inquire about the square of the modulus of L. Hence

$$|L|^2=(L_x)^2+(L_y)^2+(L_z)^2\ . \qquad 15$$

For each of the components one can write

$$(L_x)^2 = 2(1^2+2^2+3^2+.............(n-1)^2+n^2)\hbar^2\ N/(2n+1). \qquad 16$$

The series sum is given by the formula:

$$1^2+2^2+3^2+..........n^2 = (2n+1)n(n+1)/6. \qquad 17$$

Substitution of Eq.17 in Eq.16 and then in turn in Eq.15, when considering that perfect randomness gives isotropy $(L_x)^2=(L_y)^2=(L_z)^2$, one obtains as average angular momentum modulus per system

$$|L|/N = \hbar\sqrt{n(n+1)}, \qquad 18$$

which is the result predicted by the Schroedinger equation.

Unlike the Planck result on zero-point energy, which seems to be missing from all contemporary textbooks on quantum mechanics, the result of Eq.18 is only "almost" missing from the contemporary literature.

There are two exceptions! The Feynman Lectures[4] give a derivation of Eq.18, and so does a book by Kompaneyets.[4] In both cases, Eq.18 is presented as an interesting oddity, as something to think about. There is no attempt at assessing its meaning in the context of the prevailing Copenhagen interpretation. Since an orientational averaging procedure has been used, there has to be some sort of an ensemble. What is that ensemble?

Reading the two presentations, one has to guess as to what type of an ensemble is being implied. The only ensemble compatible with the Copenhagen interpretation would be an ensemble in the sense of Gibbs, which invokes an ensemble of conceivable manifestations of one and the same single system. At this point, one should be made aware of the difference. The actual physical ensemble can accommodate an interaction of its constituents; the Gibbs ensemble cannot, because there is only one physical system that is being considered. The Gibbs ensemble, therefore, is poorly suited to account for a description of order/disorder transitions that can take place in the actual physical ensemble.

An inquiry is now appropriate into the ensemble awareness of the early authors who initiated modern quantum mechanics. True to the epistemological tenor of his thinking, Heisenberg[11] made the use of "observables" a centerpiece of his approach. While, without question Heisenberg's observables were ensemble-based, his subsequent involvement with the Copenhagen view testifies to an omission of epistemological rigor on his part.

By contrast, Schroedinger[12] does not fail to relate the residual $\hbar\omega/2$ to Nernst's law and Planck's zero-point energy. Since, at the time, Schroedinger himself had ventured a single system "smeared out" particle picture, the ensuing dichotomy with Planck's zero-point ensemble may have played a part in his subsequent adamant opposition to the Copenhagen interpretation. Yet it is not known whether Schroedinger ever openly joined the small club of ensemble supporters. Perhaps also in this realm, he knew too well the limitations of his own creation.

6. The Enigma of the Missing Flux Residue

Standard Copenhagen-based folklore has it that zero-point energy may be extrapolated to have meaning as a single particle- or as a single system manifestation. Yet a close examination of the experimental results of flux quantization in superconducting rings does not quite support the legitimacy of that act of extrapolation. In fact, there is no flux residue that could conceivably correspond to a zero-point energy in what should be regarded as an ordered (*i.e.*, single system) electron motion in the superconducting ring. Hence, either the extrapolation is impermissible, or there is still a Schroedinger-based argument to account for this absence of a corresponding zero-point flux.

Initial difficulties in conceiving of a Schroedinger-type harmonic motion without zero-point energy, led to a premature conviction of having stumbled on a full-fledged argument against a single-system extrapolation of Schroedinger results. Arguing along these lines with people, to give up their faith in this Copenhagen article of faith, met with a persistent undertone of opposition. A new insight, though, emerged from a conversation with N. Bloembergen.

Closer scrutiny revealed indeed an harmonic motion in the Schroedinger sense that does not display a zero-point energy. The superconducting ring is a case in point. Hence the question of a generally permissible extrapolation of zero-point energy to single systems still remained unresolved. The inquiry revealed, however, a curious relation between radial variable and zero-point energy. Here are some details of these proceedings (chapterVI;1).

If a single oscillator, or the cooperative equivalent thereof, can be found that permits a check whether or not Schroedinger's zero-point energy state can exist in an isolated system, a long-awaited answer to quantum mechanical interpretation would be in sight. Once matters are examined in the here delineated perspective, it is found that an experiment of just about this kind was "almost" performed, some thirty years ago; yet it was never considered in this light. I refer here to the discovery of flux quantization in superconducting rings.[13] The BCS electron pairs, circulating in the ring, provide an ideal example of cooperative harmonic motion.

The electron pairs are taken to constitute an ensemble with perfect phase and orientation order. Since electrons are moving here in the magnetic field generated by their own motion, one could say: one half of the circulating electrons is performing a cyclotron $\omega = (e/m)B/2$ motion in the field generated by the other half, which is one-half of the actually observed field B. Hence, with respect to the observed field, the cyclotron motion is perceived as a Larmor circulation: $\omega = (e/2m)B$.

Having thus established an apparent cyclotron aspect of the situation, one may now consider applying Landau's[14] solution of the Schroedinger equation for the cyclotron motion. Since this analysis assumes a Schroedinger applicability to this single system situation, it may be considered as a potential test case (*i.e., if the given Larmor→cyclotron transition is acceptable).* Landau's analysis leads to the familiar energy spectrum of the form given by Eq.13, and leaves for $n=0$ an always present zero-point energy $\hbar\omega/2$. It is hard to conceive of such a zero-point motion without exhibiting a corresponding always present flux residue of $h/4e$.

Let us now closely examine the experimental results on the presence of such a flux residue. Fig.1 gives a reproduction of the experimental results of Deaver-Fairbank.[11] It seems fair to say that these data show little or no evidence for the presence of a residual flux $h/4e$. Hence no zero-point level is suggested. Therefore, by calling at this point bluntly on the Landau result as having a finite zero-point level, there would seem to be a contradiction between theory and experiment.

In view of the importance of this apparent contradiction, the earlier mentioned discussion with N. Bloembergen made it well advised to go through the Landau argument to verify whether or not the superconducting ring is truly physically comparable with the external B field situation envisioned by Landau.

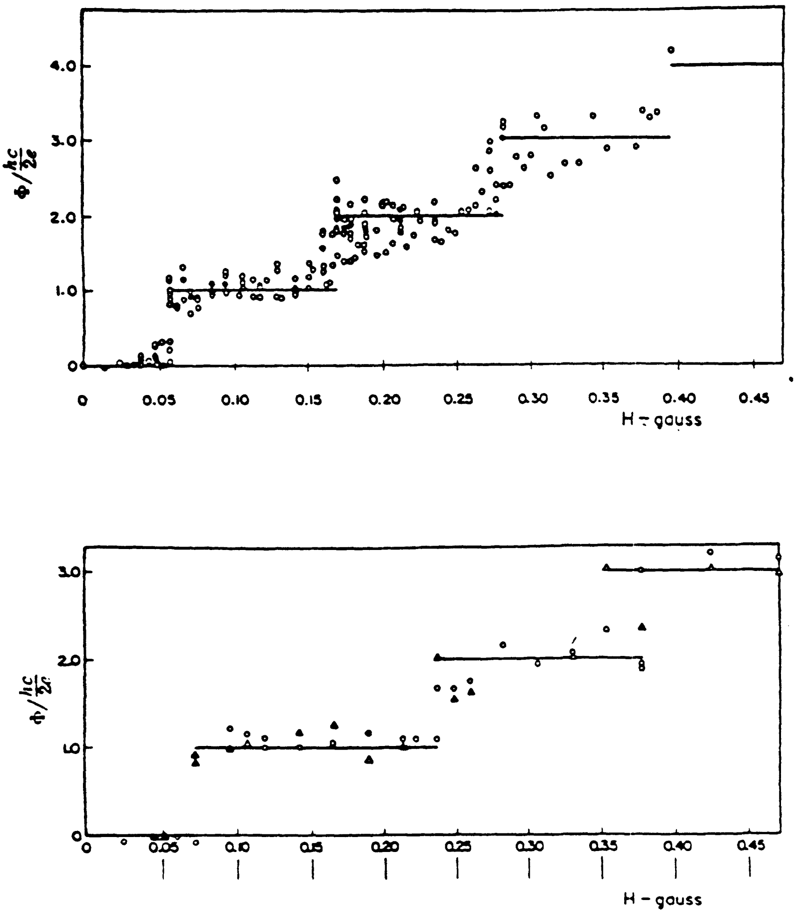


Figure1: Flux quantization in two small tubular superconducting samples according to Deaver and Fairbank (ref.11). The ordinate shows the trapped flux in MKS units $h/2e$ as a function of the external field in which the cylindrical rings have been cooled below the transition temperature. Note absence of residual flux!

Before making that comparison, let us first have a look at the flux measurements of Deaver and Fairbank in Fig.1. They rather convincingly show the absence of a residual flux

It is now necessary (for the first time in these discussions) to write down explicitly the Schroedinger equation for an electron's planar motion in a magnetic field B derivable from a vector potential A. The A may be external, or it may be internally generated. If r and ϕ are the position coordinates of radius and angle, the Schroedinger equation assumes the form:

$$\frac{\partial}{r\partial r} r\frac{\partial}{\partial r}\Psi + \frac{1}{r^2}\frac{\partial^2}{\partial\phi^2}\Psi = \frac{2mE-e^2A^2}{\hbar^2}\Psi. \qquad 19$$

If the ϕ dependence is taken to be physically inconsequential, the angle term cancels and Eq.19 yields the ordinary differential equation:

$$\frac{1d}{rdr} r\frac{d}{dr}\Psi = \frac{2mE-e^2A^2}{\hbar^2}\Psi, \qquad 20$$

Following Landau, Eq.20 can be reduced to an Hermite type equation leading to a spectrum with residual zero-point energy:

$$E=(n+\tfrac{1}{2})\hbar\omega, \quad \text{(Eq.13) with } \omega=eB/m. \qquad 21$$

For the Larmor case of a superconducting ring, however, not only the ϕ changes, also the r changes drop out, because the electrons are restrained to the fixed radius r of the ring. Even if trivial, a nonzero wave function $\Psi \neq 0$ then yields for Eq.19:

$$E= e^2A^2/2m \text{ with } \omega=eB/2m. \qquad 22$$

This perfectly classical relation can be rewritten with the help of $A=Br/2$ (which follows from Stokes' law) and yields

$$E= \frac{1}{2}mr^2\omega^2 = \frac{1}{2}\Phi J; \text{ with } \Phi=\text{flux and } J=e\omega/2\pi. \qquad 23$$

This degeneracy of Schroedinger's process invites here, if you will, an alternative where energy quantization $n\hbar\omega$ is obtained by taken the flux-quantization law as guiding principle. This approach is discussed in chapters VI and VII, but does not permit us to come to an explicit contradiction with Schroedinger's process as a potential tool applicable to ordered "nonensemble" situations (compare chapter XVI;3 for local-global features).

At this point, one may argue that ruling out the ϕ dependence is a highhanded manner of disposing of the Schroedinger process. Let us therefore assume there is ϕ periodicity without r dependence, a situation which reflects the physical reality of the superconducting ring. The condition of single valuedness for Ψ now leads to

$$\frac{2mE-e^2A^2}{\hbar^2 r^2} = (2\pi)^2 n^2 .$$

Using the appropriate transcriptions (compare hereto chapter VII; 4) this expresses a circular motion for which $E= n\hbar\omega$ without zero-point energy. It thus follows: *The Schroedinger zero-point energy specifically relates to the r dependence.*

The constant radius feature of the superconducting ring unfortunately does not lend itself to an acid test to conclude on the ensemble nature of zero-point energy. However, the Schroedinger analysis does indicate how the condition of wave function single valuedness all by itself leaves out zero-point energy for the individual ensemble constituent, whereas wave function square integrability is identified as the actual source of the zero-point phenomenon. It cannot be denied that the $0 \rightarrow \infty$ interval of the radial variable r has a potentially environmental connotation, which could invoke ensemble-associated behavior.

Whether or not the Schroedinger process applies to an isolated cyclotron state would be testable if it were possible to measure the cyclotron radius of a single cyclotron state. The $\hbar\omega/2$ would have to show up in the radius r, because it can only show up in the kinetic energy $(1/2)mr^2\omega^2$ of an electron, which is circulating with the fixed cyclotron frequency $\omega = eB/m$. For a clearly observable distinction between n and $n+1/2$, n would have to be small, preferably 0 or 1. Yet, an explicit observation of the radius r, without changing r, categorizes such an experiment as a quantum nondemolition measurement.

Most quantum measurements are contingent on a transition between quantum states. Direct observation of quantum states is rare, except for the more recent macro-quantum effects such as flux quantization and quantum Hall effects.

7. Uncertainty and Zero-point Energy

The principle of uncertainty has been almost right from the start one of the very conspicuous features of modern quantum mechanics. From the philosophical point of view, it has also been the source of an immense literature about its knowledge-theoretical aspects. Questions converge on limiting the role of causality in physics. Yet from the pragmatic angle, uncertainty is one of the least essential features for the practical application of quantum mechanics.

Uncertainty originally was introduced by Heisenberg[15] with the help of a thought experiment in the microphysical realm. It was only a little later when Kennard[16] and Weyl[17] showed how the Heisenberg result could be derived from the Schroedinger equation. It was this derivation which may have had a crucial role for the inception of the Copenhagen interpretation, because the process is based on premises that hold a strong position in the Copenhagen scheme of things. The basic ingredients of the Kennard and Weyl derivations are the theorems of normal mode expansion and a probability premise for normalized wave-function solutions of the Schroedinger equation.

Since the Schroedinger equation is a generator of normal mode solutions, quantum physics sort of naturally relates to the spectrum of infinite series expansions. Using the pseudo-geometric language of the times, it was said that the spectral theory of Hilbert spaces had become quantum mechanics' major tool of analysis. The theory of infinite series expansions was not merely a field of endeavor of pure mathematics; it also had a fundamental role in applied mathematics. For practical reasons infinite expansions were sometimes truncated to finite expansions; in doing so, a criterion of precision was needed to give an idea how many terms would be adequate for a desired result. For a continuous expansion, an integral truncation would suffice to produce the desired precision.

For many years, such a criterion has been known for the Fourier expansions. It says

$$\delta k \delta q \approx 1, \tag{24}$$

where δq denotes a position uncertainty corresponding to a conjugate uncertainty δk, which is a measure of the inverse wave length (wave number) error introduced by breaking off the expansion.

The origin of this precision criterion may go back to Abbe's investigations on the diffraction limitations of optical instruments. The resolving power of a microscope is essentially based on a relation of this kind. Eq.24 can be given this more physics-oriented appearance by considering the resolution limit of a microscope. The underlying rationale boils down to the fact that (without special measures such as scanning) the precision of an optical instrument is limited by its aperture A (of the numerical order 1) and the wavelength λ of the light being used for the observation, through an expression of the form: $\delta q \geqslant \lambda / A$. Since $\lambda = 2\pi / k$ is the "shortest" wavelength involved in the process of measurement, λ and k assume the role of $\delta\lambda$ and δk in the sense of Eq.24. It is also known that Norbert Wiener discussed this criterion in a Goettingen seminar, prior to Heisenberg's introduction of quantum uncertainty as a universal notion.

It has intrigued generations of physicists that multiplication of Eq.24 by $\hbar$ produces the Heisenberg relation, by virtue of de Broglie's $\hbar \delta k = \delta p$. A precision criterion translates here into an optimum expectation, or worse, the relation thus assumes the familiar Heisenberg form

$$\delta p \delta q \leqslant \hbar, \tag{25}$$

which has now been given a physical significance transcending that of a precision criterion. Eq.25 has been a source of interminable disputes. Since Heisenberg's original approach was not based on the use of the already existing wave function concept, his derivation was for all practical purposes a physical translation of a Fourier-Wiener type precision criterion.

Now what was Heisenberg's ingenious rationale for translating the Fourier-Wiener precision criterion into a momentum position-uncertainty criterion? A matter of mathematical choice of precision was hereby transformed into a dictate of physics. It is normally argued that the process of measurement would be essentially a collision process of a photon and the particle under consideration: a Compton effect if you will. So multiplication of Eq.24 by $\hbar$ to obtain Eq. 25 is calling on de Broglie's relation, and is justified by the Compton effect. The observation thus affects the object being observed, which is a basic premise of measurement theory.

It follows from this overview of physics of the Twenties that the Heisenberg rationale clearly was contingent on the single-system premise of contemporary orthodox quantum mechanics. It was this point of view that later was to be consolidated in what is now known as the Copenhagen interpretation.

The ensuing big question now is: if the Copenhagen single-system interpretation is no longer a permissible picture for the physical situation described by the Schroedinger equation, then what happens to the uncertainty, and what happens to measurement theory? Clearly, some reorganizing will be necessary, if the Copenhagen single-system view is replaced by an ensemble of constituents random in phase and orientation. A reading of ensemble-based quantum texts (*e.g.*, Kemble[18]) reveals how the ensemble point of view merely tends to avoid traditional uncertainty. The issues now can be met head-on, to bring them in a more definitive light.

If the Copenhagen view is no longer an acceptable physical identification for the Schroedinger equation, the Heisenberg picture of an always present primordial single-system physical uncertainty will have to be abandoned. Yet, as shown by Kennard and Weyl, a Heisenberg-type of uncertainty relation is an unavoidable consequence of the Schroedinger equation, in conjunction with a probability connotation of the wave function. Adopting, as before, the basic premise of retaining the Schroedinger equation, while rejecting the Copenhagen single system position, uncertainty, as given by Kennard and Weyl, now has to be related to the phase-and-direction randomized ensemble replacing the Copenhagen single-system view.

Hence, rather than taking the single-system premise of measurement theory, the ensemble invites a rather more natural view of uncertainty (or indeterminacy) as a manifestation of a zero-point randomness of the phase and orientation of the ensemble constituents. The ensemble picture thus pulls the rug from under a leading premise of measurement theory.

The long suspected relation between uncertainty and zero-point motion thus becomes a theoretically more acceptable proposition. The Copenhagen single-system primordial uncertainty and the single-system primordial zero-point motion hereby are given a common ensemble basis.

A most instructive discussion of the generic perspectives of uncertainty, in the context of Popper's ensemble proposition and the Einstein, Rosen, Podolsky-type thought experimentation, can be found in Jammer's book.[2]

8. Planck's Zero-point Ensemble

The sum and total of the here collected diversity of evidence has an inescapable physical message. It is nothing less than a collective testimony inviting an interpretational change of heart vis-à-vis the solutions of the Schroedinger equation. No longer can it be assumed that these solutions describe truly isolated single systems. No longer can it be assumed that these single systems are affected by an ever-present quantum mechanical uncertainty in the sense of Heisenberg. No longer can it be taken for granted that individual harmonic oscillators have a lowest energy state $\hbar\omega/2$ dictated by the Schroedinger equation as well as by the procedure of matrix mechanics.

The new picture taking the place of the Copenhagen interpretation of single systems is, instead, some sort of an ensemble of single systems as first advocated with eloquent conviction by Popper, as early as 1934. Yet this Popper ensemble is not just any old ensemble, there are more surprises in store. They go back even to before the 1925 quantum revolution.

In 1912, Planck introduced what might be called a zero-point ensemble. As summarized in section 5, he averaged an ensemble of phase-randomized harmonic oscillators and showed that thermodynamic equilibrium demanded an average zero-point energy of $\hbar\omega/2$ per oscillator. Now, add to Planck's phase randomness of 1912 an orientational randomness of the ensemble constituents, and one finds how a Planck ensemble, so extended, manifests all the characteristic features that are obtained by solving the Schroedinger equation. Section 5 also shows how the Schroedinger quantum number of angular momentum $\sqrt{n(n+1)}$ can be reproduced by spatial averaging. With all this concerted evidence pointing at a down-to-earth, Popper-type interpretation, physics had to decide on a mysteriously nonclassical picture.

In the preceding sections we have effectively proven the following theorem about the physical implications of Schroedinger's equation.

Theorem: *The Schroedinger equation describes a zero-point ensemble of phase-and-direction randomized single systems, such as were originally introduced by Planck in 1912. The proof of this theorem is contingent on the assumption that the single-system constituents of this ensemble are described by the Bohr-Sommerfeld relations or global equivalents thereof.*

There is an irony in the circumstance that Planck's zero-point ensemble of 1912 anticipated so many of the typical characteristic features that are described by the Schroedinger equation of 1926. It is rather amazing that the fact of Schroedinger's process reproducing Planck's ensemble-based zero-point energy was taken as proof of a single-system connotation of this process. There is no compelling logic for that conclusion. Yet the specific ensemble nature of Planck's zero-point energy is totally ignored in the contemporary textbook literature. Why this reluctance to acknowledge Planck for that contribution?

What can now be said to justify the Copenhagen interpretation, with all its highly nonclassical ramifications? In the early years, just after the quantum revolution of 1925, physics was willing to buy some mystery in exchange for the wonderful tools that had become available. In 1934, physics was still too much in a state of euphoria to accept the down-to-earth criticism of Popper. Even with some measure of justification, physicists could say: we have to work with these new tools, let philosophy adapt itself to these new realities. Adaptations there were, causality and systems of logical inference were questioned. Yet the great eye-openers of new insight were not quite forthcoming.

However, now that more evidence of the past and present is converging on a new reality, we do well keeping in mind that some of the more traditional procedures had not been exhausted. At the same time, awareness is building that some of the nonclassical Copenhagen metaphors were forced upon us without due process. In fact, closer inspection shows the absence of even an attempt at proof in a physical sense. There has been an ever increasing string of abstractions (e.g. the point-particle) losing touch with physical realism. A proof of internal mathematical consistency is the best that Copenhagen can claim. To meet conditions of physical realism, internal consistency is necessary; it is not sufficient!

The here-given arguments lay serious reservations at the doorstep of the Copenhagen interpretation of quantum mechanics. Its tacit extrapolation to single-systems never was and is also presently nowhere based on sound physical evidence. The amazing, and, in part, unbelievable aspect of this state of affairs is that the epistemological inadequacy of the Copenhagen view already was established in the Thirties. Hence, here is a standoff. There is no evidence conclusively justifying the Copenhagen single system extrapolation and neither is there conclusive evidence to rule out the single isolated system as carrier of an absolute always-present zero-point energy. The Schroedinger-based discussion of the superconducting ring in section 6 falls just short of permitting that unambiguous conclusion.

In the meantime many textbooks have appeared suggesting a single-system feature of zero-point energy. Many more instructors have told students to digest what could not be digested. The single-system extrapolation of the Schroedinger process never was truly suspected as wanting, because it gave, almost all the time, asymptotically correct results. Yet, in recent times, there has been more of a half-way articulate undertone of having ensemble and single system both as viable options. To openly admit to having one's cake and eating it is a sympton of schizophrenia. Science may experiment with such dichotomy; it cannot be retained as a lasting feature.

Then, in the perspective of such weaknesses, what is it that has made the Copenhagen interpretation so intolerant of competing interpretations? Since it is certainly not a position of strength, it is weakness itself. The stature of the Copenhagen interpretation has risen above the people who put it together; it became independent of its originators?

Schroedinger himself was living proof of this process of depersonalization. In no uncertain terms did he distance himself from the Copenhagen views. Hence, the people who put it together have not become deified, but a scattered patchwork of their ideas has. This final product then acquires an independent existence by assuming a near-religious stature.

Once ordinary mortals become the mediators of this divine inspiration, the work concerned becomes, in the course of time, to be regarded as a gift from heaven: *e.g.,* the Schroedinger equation and perhaps, by association, also the Copenhagen interpretation. Yet the equation had no compelling derivation relating to experimental observation. Exactly this deficiency carried over into its interpretations. There were, at best, transcriptions from the Hamilton-Jacobi equation of particle dynamics or the Euler equations of fluid dynamics. It hardly can be ignored; the stage was set to need and to accept a fair measure of religious faith to continue from that basis the pursuit of truth.

No matter what we do, faith always remains a critical element. Yet, also in science, there comes a time when faith needs to be tested. This faith needs to be rectified wherever it is not justified, and extended where such is needed to keep on going. The Copenhagen faith has run its course beyond the point where it can be helpful. By overextending its premises about quantum uncertainty, a fear has been created against making realistic spacetime models in the atomic and subatomic domain.

It is this attitude which has created the contemporary abstract pragmatism in physics as a method of last resort. In such theorizing one gropes for abstract calculational models, which tend to become mathematically top-heavy. While there should be no objection against invoking such "abstract" methods, they should not lead to a premature discarding of procedures appealing to our visually oriented intuition.

There is presently an urgency to open up dialogues involving Copenhagen alternatives. It is not a good situation if alternatives, based on fewer and less-questionable premises, have to vie for consideration in opening up new avenues of exploration. The truth is: no matter how much beautiful mathematics we throw at the subject, Copenhagen, as commonly conceived, leads to a never-ending tinkering with logic and common sense.

Misner, Thorne and Wheeler[19] in their comprehensive treatise on gravity, give an extensive treatment of differential geometry, including the de Rham theorems on period integrals. Yet, when it comes to extrapolating these mathematical concepts to the microphysical domain, Copenhagen stands in the way of their intuitive perceptions. They say: "The concept of spacetime is incompatible with the quantum principle." Further reading shows the words "quantum principle" as specifically referring to the "uncertainty principle" as the culprit depriving us of a deterministic assessment. They use the words: "Spacetime does not exist, except in a classical approximation." Yet, the latter phrase would have deprived them of ever using the very de Rham theorems which they have so proudly placed on the pedestal they deserve. Starting with chapter VI, these theorems will be found to hold a physical key to a unique coexistence of discreteness in the realm of continuity.

Clearly, there is here a conflict situation of dramatic proportions. On the one hand, the authors (M,T,W) sense the very striking physical relevance of the de Rham residue theorems, yet dominant Copenhagen philosophy prevented them from taking full advantage of their own intuitive perception. The nonclassical premise of an ever-present absolute quantum uncertainty had blocked the way.

The currently prevailing nonclassical ideas of quantum uncertainty have physics conceptually boxed into a corner from which there is no escape.

CHAPTER IV

COPENHAGEN VERSUS COPENHAGEN

1. Introduction

The mathematical concept of wave is a *grosso modo* idealization of collective behavior of numerous "small" units meeting certain criteria of identity as particles. All wave equations of physics meet this asymptotic characterization, including the free-space d'Alembertian of electromagnetism and the Schroedinger equation of matter-waves. The inherent plurality connotation of wave(s) as a collective manifestation makes the wave-particle duality an epistemological incompatibility, unless the particle is also given an appropriate plurality. This holds for E.M. wave versus photons as well as for matter-wave and leptons.

All so-called nonclassical features of modern physics ensue from an impermissible and **unproven** extrapolation from system plurality to single system. A plurality aspect of Heisenberg uncertainty can be inferred from the plurality nature of zero-point energy. The latter Planck introduced in 1912, in a manner that led him to identify it as an ensemble manifestation, rather than as an *a priori* feature of all single systems.

Seen in this perspective, the Copenhagen interpretation of quantum mechanics thus needs to make place for an ensemble, the single-system constituents of which are described by the period integral extensions of an earlier Copenhagen tool created by Bohr-Sommerfeld.

2. Electrons versus Photons

The Schroedinger equation is a central and major tool of modern quantum mechanics. While it is one of the most productive and frequently used tools of contemporary physics, its origin is, by contrast, somewhat shrouded in mystery. The following is an attempt at separating mystery and fact in the light of some new experiences.

Quantum mechanics owes its current form to two daring leaps of imagination by de Broglie and Schroedinger. On grounds of spacetime invariance, de Broglie postulated an extension of the Planck-Einstein energy-frequency relation $E=\hbar\omega$ into a full-fledged proportionality between energy-momentum four-vector and frequency-wave four-vector:

$$(E;\mathbf{p})=\hbar(\omega;\mathbf{k}). \qquad 1$$

Right from the start, Eq.1 confronts us with a mathematical inconsistency. When taken to describe a wave front, the vector $(\omega,\mathbf{k})$ is bound to have the point association of a field vector. On the other hand, the vector $(E,\mathbf{p})$ relates to the energy and momentum of an object occupying a finite domain of space. Hence, Eq.1 is an invitation to add apples and oranges, a procedure that is known to be frowned upon. Declaring the particle to have a point existence is a way out of this predicament, and, as we know, that is just exactly how the Copenhagen interpretation resolves the inconsistency.

Physics can request dispensation from the apples-and-oranges rule, if, and only if, the results are unusually interesting. Eq.1 has honored that promise beyond expectation. Yet the predicament stands, and sooner or later leads to confrontation. This inconsistency of point versus domain has just been identified as having paved the way for the point-electron. Interestingly, uncertainty reclaims for the point a finite physical presence by hopping around according to the dictates of Heisenberg. Mindful of these undeniable conceptual hurdles of standard theory, we do well by not being too literal about the subject matter and by having the word "wave" retain a metaphoric quality.

As a step following Eq.1 comes Schroedinger's construction of a wave equation for these metaphoric waves implied by de Broglie's postulate. The equation so obtained specifically translates the diffracting influence of electromagnetic fields on the propagation behavior of the here-cited metaphoric waves; the latter are also known as "matter waves." Of course, the cited mathematical limitations of de Broglie's hypothesis unavoidably carry over into limitations for the Schroedinger and Dirac equations. The forced point-domain association invoked by Eq.1 avoids what should have been a domain integration. In fact, as argued earlier in chapter III, Duane *derives* Eq.1 from an integration!

Observational support for de Broglie's hypothesis is found in the specular reflection of electrons bouncing off single-crystal surfaces. Many experimentally confirmed calculations of quantum behavior can testify to an ample support for the wave metaphor, such as is embodied in Schroedinger's equation. They have been so numerous that a general interest in their detailed critical assessment has been waning. In view of these successes it is taken as a foregone conclusion that in borderline cases the theory gets the benefit of the doubt. New experiments invariably are expected to add confirmation to the viability of existing theory.

This remarkable interplay between hypothesis and fact has become the basis for what is known as "particle-wave duality." Since the key to deeper knowledge requires here a transition from particle to wave, there apparently is (in the spirit of the geometric optic approximation) a tacit suggestion of waves being more fundamental than particles. In general, though, physics has been careful in avoiding such value judgments. Particles and waves are said to be complementary, in the sense that sometimes particle methods lead to answers; in other instances, waves do. The recommendation is: use either particle or wave, not both!

This complementarity of the two methods is illustrated by X-ray diffraction results. A wave description of the X-ray specular reflection is given by a formula due to Bragg. Yet, the reality of the photon hypothesis, as manifest in Geiger-counter observations, is so prevalent that it also truly invites an attempt at a particle description. Using, as shown by Duane,[1] the Bohr-Sommerfeld conditions to obtain a discrete momentum exchange, the final result accounts indeed for the specular reflection. Comparison with the Bragg wave description then reproduces de Broglie's relation Eq.1 for E.M. waves and photons. (chapter III;3)

Examining these two results, which simultaneously surfaced in the physics literature, we see how de Broglie *hypothesizes* his conclusion favoring a particle→wave transition for particles with inertial mass. Duane, by contrast, *derives* his result from the Bohr-Sommerfeld conditions and Bragg's relation; thus favoring a wave→particle transition for situations involving photons.

The wave nature of X-rays as an electromagnetic phenomenon, in turn, prompts an inquiry into the wave nature of matter waves. The latter, when taken in the spirit of the Copenhagen interpretation, have no obvious intensity association; only normalized solutions of the Schroedinger equations are considered to relate to the physics. On the other hand, the intensity of electromagnetic waves have a definite physical connotation. Higher intensity conceivably indicates a higher "concentration" of photons. This prompts an inquiry whether an "intensity" of matter waves has been unjustifiably thrown out by normalization. Intensity could conceivably convey information about the concentration of ensemble elements associated with those matter-waves.

Since the normalized wave functions of the Copenhagen interpretation does not permit this option of a variable concentration, *the essence of system plurality has been sacrificed by the physically hard-to-define act of normalization.* The normed ψ function is said to be an "amplitude" for a probability of presence of a single particle as part of a system. The ensemble connotation of many of those systems is hereby negated.

If the Maxwell equations are taken to give a "wave metaphor" for photons, then the Schroedinger equations is taken to give a "wave metaphor" for electrons. While a plurality of photons is accepted as natural in electromagnetic situations, then, on what physically justifiable basis does the Copenhagen interpretation exclude, through wave function normalization, a plurality of electron systems as ensemble elements in the Schroedinger case? This question is at the root of a more precise exploration and specification of the realms of validity of the Schroedinger equation. The boson option of accumulation in the same quantum state naturally invites a notion of spatial boson density. The mutual exclusivity of fermions can only accommodate a spatial density notion if the fermions become parts of boson structures.

3. Electromagnetic Wave (many) Photon Duality

As guidance for dealing with zero rest mass photons and nonzero rest mass leptons, a closer comparison of Maxwell and Schroedinger equations is necessary. Let us first examine how contemporary opinion deals with questions relating to particle-wave duality for zero rest mass situations.

The Maxwell equations proper don't have a wave connotation unless a constitutive behavior is postulated for the medium to which they are applied. The familiar MKS relations $B=\mu_0 H$ and $D=\varepsilon_0 E$ are taken to provide precisely this constitutive information for matter-free space (*i.e.,* when the latter is being examined from an inertial frame). The joint application of Maxwell laws and constitutive assumptions then leads to the familiar d'Alembertian as photon guidance equation. Let us list what is believed to be an accepted consensus for the d'Alembertian wave equation.

The d'Alembertian is primarily designed to be a wave equation, regardless of whether it is used for acoustic or for electromagnetic purposes. The quantized photon and phonon aspects came after the wave connotation already had been firmly established. In fact, the d'Alembertian was created to accommodate waves, not anything else.

Photon and phonon aspects are manifest as soon as waves interact with the energy states of atomic and molecular matter. *The ray trajectories of Hamilton's geometric optic solutions of the wave equation are, so far, the most eloquent testimony in support of a particle-wave duality.* Yet, a connotation conveying an approximate nature of the geometric optic solution suggests waves as an item of greater fundamental importance than particles, without really adequate foundation. The inference of waves as more fundamental than particles is not to be regarded as a foregone conclusion. Its validity is tempered by constitutive postulates that go into the construction of wave equations.

Under these circumstances it is, at best, permissible to conclude that waves represent some sort of collective particle behavior. While geometric optic solutions of the wave equation reveal trajectories for particles, it stops just short of truly identifying particles. The latter are experimentally apparent only in the wave interaction with matter, such as is vividly illustrated by the modern techniques of photon counting.

The quantum electrodynamic (QED) procedure of identifying a quantum structure in an electrodynamic radiation field constitutes a normal mode decomposition of that radiation field. It assigns energy states $(n+1/2)\hbar\omega$ to each individual normal mode. These quanta cannot directly be identified with a particle content of the radiation field, because the physical image of that object is in an *unparticle*-like fashion smeared out over all of space. It is impossible to understand, in this manner, how photons are locally absorbed or emitted.

The energy content of this decomposition diverges for an unlimited normal mode spectrum. The free-space constitutive assumptions leading to the d'Alembertian tacitly assume their validity from zero to infinite frequency. Specifications limiting constitutive validity in the interval $0 \rightarrow \infty$ are not readily available for the not generally recognized electromagnetic medium of matter-free space. In the light of these conceptual hurdles, the QED process can, at best, be regarded as a model for a calculational expedient, not as a model of physical reality.

The here-cited conditions do not support the idea of a d'Alembertian as a sole source of information for photon structure or single-photon behavior. Neither trajectory-based solutions nor solutions of the normal mode type have a clear photon connotation. As solutions of the same d'Alembertian, the wave as a plurality aspect of photon behavior hardly can be denied a measure of physical reality. Conversely, the model of a single particle as a superposition of neighboring trajectory waves frequently is used as a possible image of particle identity in the guise of a wave packet. The latter suggests the epistemological oddity of a many-waves versus single-particle duality.

The moral of this story centers around the identification of an epistemological error in the selection of comparable items in the duality assignment. There is no such thing as a single wave, yet elementary particle physics tells us that single particles are well defined. There is an indivisible particle unit connotation; Fourier analysis is living testimony that there is no such thing as a wave unit. Duality, as it is known in mathematics, requires comparable numbers of descriptive parameters (*e.g.*, point coordinates versus line coordinates).

A Fourier decomposition of waves is a mathematical operation. Its component terms retain a subjective quality defying any reduction to anything resembling a unit of wave. Any wave can be a finite or infinite superposition of other waves. Hence, wave and single particle don't really lend themselves well to a duality comparison. The mathematical abstraction called "wave" is an idealization in the sense of field theory.

As stated from the outset, the concept of wave, as known in physics, can at best be the result of a collective behavior of particles. The acoustic wave best represents the physical reality of that view. For the electromagnetic medium, we need to stretch our imagination, because the particles, being photons, are themselves carriers of visual messages. *In the end, it is not the wave but the particle that is the more fundamental thing in nature.* The term "wave" is, and will always be, a term describing collective behavior. Any other use violates the meaning as defined in the Oxford or Webster dictionaries. If physicists mean something else with their use of the word wave, they must come up with an unambiguous definition.

4. Matter Wave (many) Electron Duality

Let us now, for comparison, direct attention to how the particle-wave duality has developed around the Schroedinger wave equation, which has been developed for describing the behavior of rest-mass particles.

If, for the electromagnetic wave equation, the constitutive law is found to be the weakest link in its derivation, then, in the case of the Schroedinger equation, it should be Eq.1. Even so, there is not quite a derivation comparable to that of the d'Alembertian. There is, therefore, less of a chance of getting to the weak points of the train of thoughts that led to its implementation. If there are limitations on the use of the Schroedinger equation and the prevailing Copenhagen interpretation, they will have to be established in retrospect, because there is not quite a derivation from which these limitations can be clearly inferred. Instead, the whole gamut of ideas that was midwife to the inception of modern quantum mechanics needs to be reassessed.

The Schroedinger equation emerged from an inspired inquiry of what de Broglie's metaphoric matter-waves might do when subjected to an electromagnetic field. Schroedinger arrived at his equation in a two-step process. First, he obtained an equation describing the stationary states of a system as eigenvalues of a multi-resonance system. The mathematical techniques proved to be very similar to what already was known for acoustic and electromagnetic resonance systems. After experiencing some initial difficulties, he then succeeded in constructing a time-dependent wave equation and an equation for multi-particle systems. The latter portrays the system in a Hamilton-Jacobi configuration space rather than in spacetime.

The time-dependent equation differs from the d'Alembertian by having an imaginary first order time derivative, instead of a real second-order time derivative. The cited distinctions between the spatial one-particle systems, and the multi-dimensionality of many particle systems, stress the metaphoric quality of matter-waves even more than for E.M. waves.

The Hamilton-Jacobi equation was indeed instrumental in the approach of Schroedinger, a circumstance which may have prompted a single system view. The Copenhagen interpretation soon consolidated this one-system view of the Schroedinger equation. A factor, supporting the Schroedinger equation in general, is undoubtedly the isomorphism with the matrix mechanics pioneered by Heisenberg, Born and Jordan. While the matrix method had no Ψ function equivalent, the Ψ function proved to be a major tool in calculating these matrix elements. The two methods complement one another without precipitating further interpretational perspectives.

After the Copenhagen single-system view was beginning to be firmly established, Popper[2] and Einstein[3] produced opposing views without much affecting the single-system trend that had already been set in motion. Fuerth[4] focused attention on the similarity with diffusion processes, thus detracting from the single-system implication. Later Fenyes,[5] Weizel,[6] Bohm-Vigier,[7]and Nelson[8] emphasized, either directly or indirectly, a plurality view of the object to which the Schroedinger equation applies by revealing a close association with the Euler equations of fluid mechanics. All of these investigations represented, in some way or another, a handwriting on the wall that remained in part unheeded. An examination of Jammer's[9] Philosophy of Quantum Mechanics reveals that the here given references are not merely isolated cases. There are many more names of people who have expressed opinions about the ensemble view of quantum mechanics: *e.g.*, Kemble,[10] Blokhintsev,[11] Ballentine,[12] Groenewold,[13] Collins.[14]

Comparing the choice between wave-photon versus wave-many-photon ensemble, there is not the slightest doubt that the choice between wave-single rest mass system versus wave-ensemble of rest mass systems needs to be discussed in the light of new observations. Faced with this issue, the one and only way of resolving this problem is by an honest investigation into the question: which of the two situations is being treated by the Schroedinger equation?

The arguments used to settle the wave-photon choice applies equally well to the wave-system (particle with rest mass) choice. Also here the wave-(single) particle duality forces a comparison between epistemologically discordant partners. There is no *unit* of matter-waves to make the wave end of the duality compatible with the discrete realm of particles or systems. To make it compatible, Copenhagen called on the artifact of Ψ function normalization, an act which eliminated any possibility of ensemble density considerations for the future. It also led to the typically noncausal notion of wave function collapse.

Notwithstanding Copenhagen's ingenious attempt at creating dual-partner compatibility between wave and system, it was man rather than nature that erected the modern edifice of *nonclassical* physics.

5. New Evidence Resurrecting Old Copenhagen Tools

If verifiable by evidence, the delineated positions are bound to deeply affect disciplines that have been shaped, either directly or indirectly, by Copenhagen views. Quantum electrodynamics(QED) is a candidate for radical change.

If the QED properties of the electron no longer are deduced from interaction with fluctuations of infinite vacuum energies, they now are to be related to an electromagnetic model instead. This model[15] yields spin and magnetic moment, including a model-based understanding of higher order moment anomalies. In accord with modern mathematical investigations, the spin concept is related to manifold orientability[16] and the enantiomorphism of physical objects therein (chapter XI).

Experimentation with squeezed light[17] offers an interesting option to substantiate an ensemble nature of Heisenberg's uncertainty. The Heisenberg uncertainty relation for fields automatically reinjects an ensemble aspect in view of the earlier discussed wave-many particle duality.

Finally there is the following major question: *If the Schroedinger equation is an ensemble tool, which, strictly speaking, no longer applies to single systems, is there a single-system tool to take its place?*

The answer given here is: period integrals give a metric-independent generally invariant account of quantization,[18] and they are instrumental in determining model topology in spacetime. Moreover, they are the key for reconciling quantum theory and the principle of *general* covariance in relativity (chapters VI and XIII).

These integrals relate, in the sense of the WBK method, asymptotically to results of the Schroedinger equation. This mathematical asymptotics acquires physical meaning as an asymptotics between ensemble properties and the system properties of ensemble constituents. Ensemble results, obtained with the Schroedinger equation can be shown to be phase and directional averages of period integral data pertaining to single systems (chapter III;5). Period integrals can be applied advantageously to macroscopic single systems such as manifest in the quantum Hall effect[19] (chapters VIII and IX).

6. Conceivable Interpretations of ψ

If the single-system interpretation of ψ is out, how does ψ relate to an ensemble description of systems? Even if there is now no longer a need for normalization, the product $\psi\psi^*$ still obeys a continuity condition. Hence, the magnitude of $\psi\psi^*$ seems an extra parameter, the physical meaning of which needs to be assessed. Since normalization does not affect the Schroedinger eigenvalue, change of $\Psi\Psi^*$ rather than ansolute magnitude manifests itself as the crucial physical parameter.

Similar to $\Psi\Psi^*$, the electromagnetic energy density $(\mathbf{E}\bullet\mathbf{D}+\mathbf{H}\bullet\mathbf{B})/2$ is quadratic in the d'Alembertian wave function. Since this energy density relates to a *photon density* in the photon ensemble, it stands to reason to relate $\Psi\Psi^*$ for the ensemble of material systems to the *system density* in the ensemble. Yet, the Ψ function of an ensemble of multi-particle systems exists in the Hamilton-Jacobi configuration space. This circumstance detracts from an analogue to a photon distribution with boson properties. Since the Pauli exclusion principle complicates a density interpretation of $\Psi\Psi^*$ for fermions, the probability assessment was hailed as a rescue from this predicament.

In section 7 of chapter VI, the statistical features of Ψ are shown to be related to an averaging process involving the parameters of a Hamilton-Jacobi family of orbitals. Each orbit is compatible with a set of given Bohr-Sommerfeld conditions. The Schroedinger process thus becomes some sort of a universal substitute for the very specific averaging process that led Planck to the concept of zero-point energy (chapter III;5).

The potential function appearing in the Schroedinger equation is chosen to reflect only the inner potential field of a single isolated system. Under those circumstances, results obtained are relevant only to very dilute ensembles. For increasing ensemble density, the proximity of the constituent systems in the ensemble is bound to play a role and should be accounted for by adding a mutual system potential. The solutions of the Schroedinger equation in crystal field theory testify to the spectral changes due to such system-system interaction.

Planck[20] identified the zero-point energy as a typical ensemble manifestation due to a phase disorder of the systems in the ensemble. This minimal zero-point energy in a phase random ensemble of systems is necessary to avoid a negative ensemble probability in the sense of statistical mechanics.

An example of an ensemble capable of making a transition from phase-disorder to phase order, so that all systems in the ensemble are phase-locked, is the quantum Hall effect. This macroscopic quantum effect gives no evidence of harboring a zero-point energy for the phase-locked cyclotron states of the ensemble.[21] Chapter III; 6 relates the Schroedinger aspects of zero-point energy for circular motion specifically to the dynamics of the radial variable in the interval from zero to infinity. The latter feature strongly suggests for Ψ the role of an ensemble based expedient, this role is identified in chapter VI; 8.

7. Copenhagen Rules out Copenhagen (flow chart)

When carried to its logical conclusion, a consistent interpretation should either affirm itself, or rule itself out of order as not relevant. The Bell theorem[22] has done almost exactly that, except that the answer critically depends on who interprets the implications of the Bell theorem.

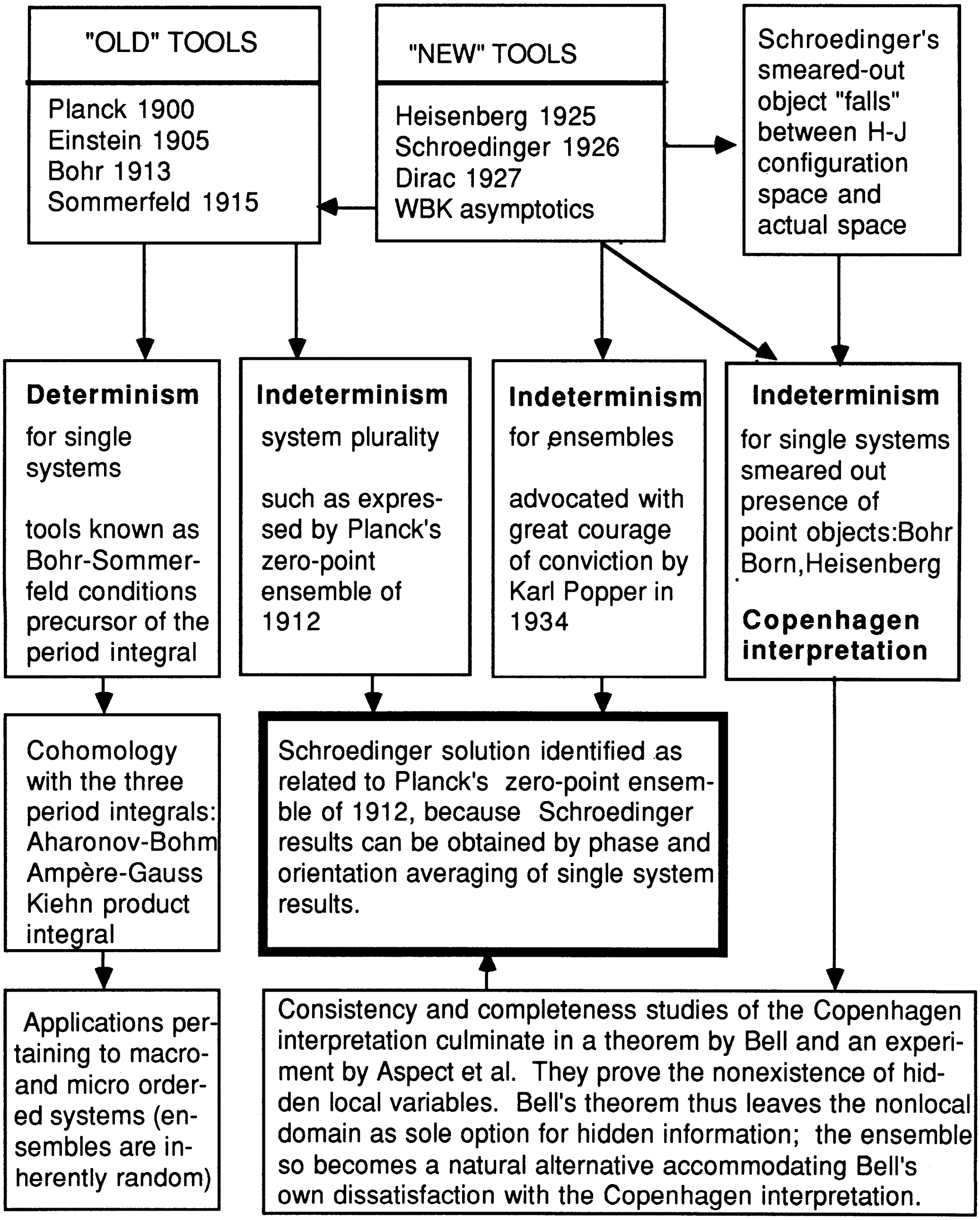
Initially, Bell's theorem and an experiment by Aspect were said to rule out the existence of so-called hidden variables. This statement took the wind out of the sails of those who claimed that quantum mechanics was somehow an incomplete theory. In conjunction with Aspect's experiment, Bell's theorem was read to mean that quantum mechanics (*i.e.,* in its Copenhagen single system connotation) had to be a complete theory.

Accordingly, Bell was hailed as the savior of orthodox quantum mechanics, which comprises the combination of Schroedinger equation and Copenhagen interpretation. It is not clear whether Bell himself agreed with that assessment, because he has been quoted[23] as still referring to the Copenhagen interpretation as "something rotten in the state of Denmark." The conclusions to be drawn from Bell's theorem critically depend on whether the missing information is believed to be of a local system nature or of a nonlocal ensemble nature.

Since the theorem applies to the standard apparatus of quantum mechanics, the missing information is envisioned as hidden in extra variables conceivably pertaining to the Schroedinger equation. Yet the Schroedinger process is local-global in nature. It means the Schroedinger equation itself is a local instrument, the solutions of which are subject to boundary condition that are nonlocal in nature. It thus follows that Bell's test of hidden information can only rule on variables locally hidden in the Schroedinger equation itself.

Hence, by virtue of its locally restricted starting point, Bell's theorem can not rule conclusively on nonlocal information. Examples of nonlocal structural features are, for instance, the structural topology of single systems, and for ensembles the mutual phase and orientation of the constituent systems of the ensemble. Hence the door is still wide open as far as nonlocal information is concerned. Probably against his own better judgment, Bell became the savior of quantum mechanics. Indeed, from the Copenhagen point of view with its point-particle thesis and the Schroedinger differential equation as basic instrument, the chances of injecting nonlocal information had already been successfully eliminated. Within that local framework, Bell's theorem and Aspect's experiment therefore confirmed the logical consistency of the Copenhagen schemeof things. Yet, logical consistency is a necessary, not a sufficient condition for physical realism.

SELF-DESTRUCTION OF THE COPENHAGEN INTEPRETATION



Flow chart I: illustrating the demise of the Copenhagen interpretation by capitalizing on the mere strengfh of its own internal logic.

Since the door is still wide open for nonlocal structural information, the Bell episode can now be considered in the wider context of local and nonlocal information. In view of the overwhelming evidence for the existence of and need for nonlocal information the Bell theorem can be used to effectively rule out the Copenhagen single-system view. The ensuing demise of the good old Copenhagen standby is illustrated in **Flow Chart I.**

The moral of this story is: **Even good mathematics can be misleading if the mathematical identifications of physics are wanting.**

8. Conclusion

If the wave function ψ is taken to describe the state of an ensemble, the associated $\psi\psi^*$ must be expected to describe the density distribution of ensemble constituents. Yet, Copenhagen has accepted the permissible option of wave-function normalization. It then follows that the combination of Schroedinger equation plus wave-function normalization should be only capable of describing ensemble properties that are independent of ensemble density. Only highly diluted ensembles are, at best, sufficiently independent of density changes.

The acceptance of this rather unavoidable conclusion of ensemble behavior, as implied by the custom of thinking in terms of normalized ψ functions, removes the Schroedinger equation method from the imagined goal of an exact instrument of description within the ensemble framework. *By the same token, it diminishes the hope for an exact derivation of Schroedinger's equation as a realistic goal.*

For the many years that the Schroedinger equation has been a major tool of physics, it has been somewhat perceived as a gift from heaven. Yet it is exactly this origin, shrouded in mystery, that also has been a source of liabilities. Its suspected lofty origin has raised the expectations about its wondrous capabilities to, at times, unrealistic levels. Its simultaneous processing of order and disorder still remains a major incongruity of procedure in the Schroedinger-Copenhagen process.

In retrospect, it now almost seems naive to expect a many-particle Schroedinger equation and Pauli's principle to give an exhaustive account of the dynamical performance of a complicated atomic or molecular structure. Yet, for many years, there has been the strong suggestion that such might be the case. The mystique of its origin gave it the benefit of the doubt. On the one hand the aura of being a gift from heaven worked its magic. It made people accept performance claims that might have been refused in other circumstances. The classical many-body hurdles gave the nonclassical many-body intractability a measure of protection, because it stood in the way of a solid verification of exalted claims.

Recognizing ensembles as a plurality of atoms and molecules, any attempt at ensemble description would have to cover at least those physical characteristics of the constituents that are determining for ensemble behavior. The two-fold nature of this assignment, however, engenders an inner conflict of objectives. Consider hereto that the ensemble constituents are ordered single-system structures, their joint behavior in terms of ensemble manifestations are taken to reflect a statistical aspect of disorder.

Physics has perceived as its assignment the dual task of finding tools of description that simultaneously cover aspects of order and disorder. One may have legitimate reservations as to whether any single instrument of description would be able to perform this dichotomous task. Yet, standard quantum mechanics claims it can do exactly that. The hybrid nature of its existing tools is apparent, because the Schroedinger equation is inherently statistical, yet its solutions need additional criteria of order, such as dictated by Pauli's principle. Unlike classical theory, in quantum theory, as presently known, there is no clear-cut separation between quantum mechanics and quantum statistical mechanics, because standard quantum mechanics always retains a "nonclassical statistical" residue.

Faced with physics' self-imposed assignment of straddling the fence between order and disorder, it now becomes understandable why the early Copenhageners opted for a simplifying "leap of faith" by leaving the ensemble out of discussion altogether. This was done by declaring the Schroedinger equation to be a single-system instrument. Nevertheless, the inherent statistical nature of the Schroedinger equation could not be denied. It took revenge for having its ensemble connotation ignored. It dictated the need for at least retaining a residue of ensemble conscience, which now manifests itself under the guise of some nonclassical artifacts, which had to be created for exactly that purpose. They are universal uncertainty and zero-point motion of isolated objects.

All in all, ψ retains the character of a calculational expedient, a probability tool still looking for its universe of discourse. Perhaps the need for an interpretation of ψ has been overemphasized and is out of tune with reality. The counterpart of ψ, in the equivalent of matrix mechanics was later introduced as state vector. The eigenvalue process retains the most central function, not the state vector. The latter is normalizable by virtue of the homogeneity of the eigenvalue process.

The complexity of the situation makes it useful to once more summarize the state of affairs. In order to do so, it is necessary to face the emotional discomfort that is associated with reevaluating a good old friend and finally assessing this friend on a more realistic basis. I am speaking here of the

Schroedinger equation. It had to be taken down from its not very practical ivory-tower position, where enthusiastic followers and admirers had put it, *notwithstanding the strong warnings of its originator.* From a magical tool, serving an indefinite realm of quantum situations, it had to be reduced to a level of plain ensemble pragmatics.

On the other hand, as a result of this reduction in status, new tools are needed, filling the vacuum where the Schroedinger equation can now no longer in good conscience be applied. The field open for new quantum inquiry comprises the single systems and the highly ordered ensembles. The latter behave as single systems, by virtue of the pronounced internal order.

The new tools, which logically become available to fill the void, have a topological connotation. This stands to reason, because a structural exploration of single systems, or ordered ensembles for that matter, will have to start first with the topology of those structures. Only after the topology of those structures has been established is it possible to focus attention on their metric features. Yet once the need for topological exploration is recognized a new complication makes itself known.

Since the study of topology has been the near-exclusive prerogative of mathematicians, the mathematical backdrop they use has been purely geometric. Physics is, however, a discipline in which dynamic behavior in time demands an attention transcending the traditional geometry-based realizations preferred in mathematics. It is therefore unavoidable that physics has to find here its own way.

Notwithstanding the overriding geometric backdrop tradition in mathematics, many topological development in mathematics are found to be already sufficiently abstract to automatically accommodate many of the needs of physics. Poincaré torsion, as reflected in the distinction between space and time, should be cited here as a striking example. It illustrates how the art of abstraction has a capability of paying off (see chapter XII).

CHAPTER V

VON NEUMANN, POPPER-EPR, BOHM, BELL, ASPECT

1. Summary

The following is a conceptual survey of the history of interpretation of the Schroedinger process of quantum mechanics. The presentation is nontechnical in the sense that no explicit mathematics will be used. No disrespect intended, if essentials are discussed by lightly paraphrasing results. I confess to these shortcomings, because I really did not see any other way of disentangling this extremely convoluted and massive amount of historical material. It was a predicament of keeping track of both the trees and the forest. Names mentioned in the title are those of principal protagonists who have been active during an odyssey which now has lasted for well over half a century. An attempt is here made at bringing out an emerging conceptual coherence if the propositions of interpretation about completeness are consistently viewed in an ensemble perspective for the Schroedinger process. The unproven and unjustifiable single-system premise, which is shared by many interpretations of the Schroedinger process, is here identified as the major obstacle preventing a delineation of previous efforts.

2. The Single-System Trap

Starting with Schroedinger, almost all those concerned with the foundations of quantum mechanics took the single-system option as a starting point of their considerations. There are a few exceptions. Popper[1] was one of the first who articulately verbalized his opposition against this almost tacit proposition that the Schroedinger process was believed to describe a single-system situation.

Einstein became only indirectly known as a single-system opponent. He was most concerned with the question whether or not the Schroedinger process could be assumed to give a complete description of single isolated systems. Yet, in a footnote of his correspondence with Popper, Einstein reveals that he shares Popper's opinion about the role of the wave function as describing a statistical "aggregate" of systems, not a single-system.[1]

In the following, the word "ensemble" shall be used for Einstein's aggregate. Unbeknown to many occasional users of quantum mechanics, an ensemble view of quantum mechanics has been able to coexist, until this day, with the far more popular single-system view. The latter is normally referred to as the so-called Copenhagen interpretation. The reader should be aware, though, that Copenhagen views may be subdivided in several sects. In view of their reduced relevance, this overview of interpretational options attempts to avoid an undue involvement in all the differences between those sects.

Two major factors are, in the following, identified as having predicated the convoluted development of contemporary quantum interpretation.

One is a firm popular belief that the mathematical perfection of the Schroedinger process ought to have an equally perfect physical concomitant, even if that perfection is not yet known to us in terms of physical interpretation. This attitude is clearly conveyed by those who speak of wave functions for the world and the universe. Others speak with great conviction about the first few microseconds after the Big Bang. Bell,[2] tongue in cheek, confesses to finding a touch of blasphemy in the pursuit of such unnecessary ambitious goals.

The other factor is an overriding, yet rather unfounded, conviction that the gift from heaven known as the Schroedinger process was meant to describe a single-system. As far as I have been able to establish, here we find Bell[2] siding with the single-system view of the majority. He does not take position against his fellow philosopher Popper, who has supported an ensemble view. I have found in Bell's book one reference to Popper. This reference has unfortunately no bearing on ensembles; it relates instead to an indistinguishability of preferred reference frames.

The trap that has been compounding the development of quantum interpretation is in the asymptotics between the ensemble and its single-system elements. The sometimes deceptive experimental closeness between single-system and ensemble behavior has fostered those beliefs.

The following is, if you will, a running commentary on Bell's book[2] on "speakable and unspeakable," as seen from the angle of an ensemble supporter.

3. Johann von Neumann

Schroedinger and Pauli established the mathematical equivalence of the eigenvalue processes of Heisenberg, Born and Jordan with the Schroedinger wave-equation process. Wigner and Weyl initiated the use of the theory of group representations to establish relations between spatial symmetries and their manifestations in Hilbert space. This outline of the mathematical foundations of the theory was beginning to invite an attempt at an axiomatic formulation.

Since the mathematical apparatus of eigenvalue procedures, as used in the Schroedinger process, had been extensively studied by the Hilbert school of mathematics, it stands to reason that the first attempts at axiomatizing these foundations could be expected to follow the guidelines of Hilbert's formalist school.

In his treatise on the mathematical foundations of quantum mechanics, Johann von Neumann[3] established that the spectral theory of Hilbert spaces turned out to be the ideal instrument for treating quantum mechanical problems. In presenting the subject matter, von Neumann showed himself an extremely competent and faithful student of the Goettingen school of formalists. It is therefore appropriate at this time to contrast this formalist view against a pronouncement of a more intuitional oriented scholar. Herman Weyl later said: the fact that the spectral theory of Hilbert spaces is applicable to quantum mechanics is truly a "favor of fortune."

Seen from this perspective, it also stands to reason that the interest in the theory's physical foundations was very much colored by the near perfection of its mathematical foundations. Any theory of such mathematical perfection was bound to have a concomitant physical perfection. That is how quantum mechanics rode right into the center of the scientific arena on the coattails of the near-perfect spectral theory of Hilbert spaces.

Since the "completeness" of the series expansions, in terms of normalized orthogonal functions, was one of the striking features of the spectral theory of Hilbert spaces, it was now almost unavoidable that a measure of this completeness awareness was expected to somehow carry over into the realm of physics.

A statement and discussions to this effect was indeed first made by von Neumann and later further elaborated by Jauch and Piron. New insight was added by Gleason in an attempt at reducing the axiomatic basis of quantum mechanics. Then Bell[2] entered the stage and with a Bernard Shaw type directness, he claimed that he could restate the position with such clarity that all previous discussions would be eclipsed. In doing so, he still left a door open for global physical incompleteness next to all this blinding mathematical completeness.

At this point, it is necessary to become specific in what respect the physical situation could be considered to be incomplete. It will be necessry to distinguish between local and nonlocal incompleteness.

4. Popper and the Einstein-Podolsky-Rosen Paradox

An inner contradiction between the claim of physical completeness and a wave-function description of a single event was put together by Einstein, Podolsky, and Rosen.[4] In the light of our earlier discussion of the correspondence between Popper and Einstein, it seems strange that Einstein would entertain the discussion of a single event, in view of his expressed belief in a footnote of a letter to Popper.[1] *In this footnote he says to agree with Popper that the Schroedinger equation's wave function should be considered as describing an "aggregate" (i.e., an ensemble) of systems and not a single system.*

In view of the, at that time, dominating Copenhagen conviction that the Schroedinger process had to be a single-system instrument, it must be assumed that Einstein wanted to show that this single-system premise would lead to contradictions, as indeed it did.

Yet, with all this emphasis on a footnote in a letter, one may well wonder: what was the actual content of this letter from Einstein to Popper? The content of this letter has been published in translation in Popper's book[1] together with the German original in facsimile. Perusing the content, one finds the letter first questions the relevance of a physical thought experiment that Popper had brought to bear to bolster his position. The letter then continues with a brief description of a conceivably more promising alternative. It is the EPR thought experiment[4] which, at that time, was in preparation. Though Popper's epistemological position was accepted, if only in a footnote, his thought experiment to bolster his posistion was wanting. The philosopher Popper admits how the consummate physicist Einsten disposed of his thought experiment, briefly and with great clarity. So where does that leave Popper's epistemology?

While Einstein, right from the start, acknowledges the probable correctness of Popper's ensemble idea, in his work with Podolsky and Rosen, he clearly envisions a single event situation, which, when seen in the ensemble perspective, could not be all that relevant. The answer to this paradox reveals that Einstein and co-workers were themselves attempting to produce a paradox, with the apparent intention of invalidating the Copenhagen single-system point of view.

Now looking in retrospect at this convoluted state of affairs, it remains still surprising why the ensemble point of view never emerged above the status of a perhaps half-way acceptable preoccupation of a rather small minority of physicists. Popper was in a very unfortunate position. His concession to Einstein concerning the inadequacy of his own arguments in support of an ensemble view has to be seen in the perspective of the times of the mid-Thirties. Einstein was already regarded as an older statesman of physics. An undisputed champion of the past, yet his famous remark

"God does not play dice" placed him in a category not receptive to the nonclassical gospels emanating from the Copenhagen School.

The Thirties were, however, the heyday of nonclassical physics. Hence Popper, having been knocked out by a former champion of physics (who no longer was believed to be in tune with the nonclassical spirit reverberating from Copenhagen and Goettingen) had not much of a chance to receive a further hearing in physics on his ensemble views. Only the criticism was remembered, and Einstein's footnote was forgotten.

At this point, the single system point of view of Copenhagen had gained a not very deserving victory over the ensemble point of view. **From that moment on, the single system view would dominate the conceptual development in quantum physics for the the next half century.** The very small group of confessed ensemble supporters could not change this course, because they failed to collect truly decisive evidence in support of their position. By contrast, the nonclassical processes of the single-system school became bolder by adding daringly nonclassical energy infinities permeating the whole universe. They could boast some spectacular results in the development of quantum electrodynamics. All of which must be considered as having sealed the verdict on the ensemble as an object of description for the Schroedinger process.

5. David Bohm Reopening the EPR Box

In the Fifties, a new chapter in the interpretational developments was initiated by Bohm.[5] While this was a new development, let it be understood that it carried on in the spirit of the single-system view. Bohr, using the full arsenal of nonclassical aids, had conscientiously responded to the EPR paradox. He had done so retaining the single-system aspect. Bohm, presumably not satisfied with Bohr's nonclassically tainted response, sought another solution to the EPR paradox. Rather than taking von Neumann's word for it that the Schroedinger process would be complete, Bohm initiated an investigation as to what happens if it is taken to be incomplete. He did so by suggesting the existence of new dynamical variables, the function of which was presumed to be hidden, at least for the time being. He was just opening up for those hidden variables a potential place, which might help in a future identification of their as yet hidden dynamical functions.

Bohm's hidden variables were an act of defiance against von Neumann's pronouncements concerning the completeness of the Schroedinger process. Von Neumann's presumed proof had been too much a mathematician's proof. It had too exclusively navigated in the wake of the spectral theory of Hilbert spaces to be accepted and justified as a physical proof of completeness.

With Bohm, we see for the first time a breaking away from the almost axiomatically accepted supremacy of the Schroedinger process as a gift from heaven. By the same token Bohm's initiative also signalled a departure from an almost *a priori* acceptance of the use of nonclassical recipes as a way out of the conceptual predicaments. Bohm had taken position in the Bohr-Einstein dispute by declaring how much nonclassical imagery should be considered as paradoxical.

6. John S. Bell and the Aspect Experiments

Bohm's suspicions about the logical inadequacy of von Neumann's proof were taken up by Bell,[2] with some spectacular and interesting results for physics. To make hidden variables acceptable as a convincing physical argument, Bell argued that they first had to be assumed to explicitly exist, in order to conclude what consequences they conceivably could have on the structure and predictions of the theory. He came up with a result that led to certain inequalities, where, in the strict Copenhagen perspective without hidden variables, only equalities could be expected.

With Bell, physics somehow entered a new phase of existence. Over and above the fact that physics itself went through some incisive changes, it was foremost a change in the type of personality that for the first time began to play a role at the frontiers of that science. With so many eager beavers anxiously making their sales-pitches to extend Copenhagen's lease on life, it was utterly refreshing to see a person who could look at all this in amusement and with a wit, which he also directed at the results of his own endeavors. He not only has an enviable vocabulary to convey his feelings about the subject matter; he also coined new words. Quantum mechanics needed this kind of person in order to come out of the doldrums of nonclassical mania.

So let us now turn to the upshot of Bell's work. The inequalities, contingent on the existence of hidden variables, opened up opportunities for experimental checks, such as performed by Aspect *et al.*[6] The outcome of these experiments confirmed the validity of the equalities of the Schroedinger process. So Bell's inequalities were out and so were Bohm's hidden variables!

The result of these experiments was that Bell and Aspect had restored the integrity of the Schroedinger process as a complete theory. Somewhat against his own wishes Bell was hailed as the savior of quantum mechanics. In private he was heard as referring to the Copenhagen interpretation with Hamlet's famous phrase: "something is rotten in the state of Denmark."

7. An Ensemble Sequel on the EPR Paradox

Let us reiterate that throughout the here-cited Bohm-Bell-Aspect episode, which led to a reinstating of the Schroedinger process as a complete description, the single-system view had been taken as an undisputed starting premise. Any conceivable ensemble aspect had completely vanished from any level of awareness of the protagonists involved in these endeavors. In witness thereof, I refer once again to Bell's book, which has only one reference to Popper, having to do with preferred frames of reference.

The next question to be asked after this observation is: what about the small group of semi-accepted quantum physicists who, through all those years, have supported an ensemble view? It is probably fair to say that they have remained mostly outside the main stream of physics for a variety of reasons.

Through all those years, the ensemble supporters have not reported about a conceivable identification of a universe of discourse for the statistics of their ensemble. Recognizing the existence of at least some asymptotic distinctions between single-system and ensemble performances, the ensemble supporters are not known to have effectively addressed questions as to what type of quantum tools would have to be used in the treatment of single-systems. Neither have they recognized the need for establishing a special mode of treatment for the macroscopic quantum effects that have now taken center stage.

It would appear from this still very preliminary survey that, during all those years, most of the up-and-coming talents in physics were betting on making a career in the nonclassical world of a single-system view of the Schroedinger process. In doing so they received practically no competition from their ensemble brethren. Hence, the single-system people could afford to be tolerant to their ensemble colleagues. It was not necessary to ignore them. They virtually posed no threat to a continuation of their nonclassical pursuits. Let us see whether that situation can be reversed.

First we need to know what hidden variables mean in an ensemble context. We know already, through the Bell criterion, that hidden variables don't have a role in a single-system context. To identify hidden variables that might have a conceivable role in an ensemble context, we need to consider variables that are inconsequential from a single-system angle, yet consequential from the ensemble angle. ***Mutual-system phase and orientation are indeed consequential for ensemble performance; yet they remain inconsequential for single system performance.*** Phase and orientation of a single system only come into play as physically consequential by system interaction within the ensemble.

Phase and orientation can now be considered as nonlocal hidden variables governing ensemble performance. They could not show up in Bell's test, which is geared to local conditions. When phase and orientation are subjected to a random distribution, they can be proven to relate to an average ensemble zero-point energy of $\hbar\omega/2$ per single system in the ensemble, and an average modulus of angular momentum $\hbar\sqrt{n(n+1)}$ per single-system in the ensemble. The proof of the first statement can be found in a Planck classic[7] of 1912; the second statement appears in the Feynman Lectures[8] of 1967 (see also chapter III;5 of this text).

Hence, the just cited results have appeared in the open literature some fifty years apart from one another; one appeared before and the other after the Schroedinger equation had appeared on the scene. They are two very typical Schroedinger results, which played a principal role in the rapid acceptance of the Schroedinger process as a new quantum tool in the mid-Twenties. These typical Schroedinger results are here obtained with the help of a perfectly classical statistics, which involves the now "unhidden" parameters of mutual-system phase and orientation. While the Feynman lectures still present the angular momentum result as a perhaps interesting coincidental curiosity, it is presently the combined impact with the much earlier Planck result which gives this coincidence major status in physics.

The prophets of nonclassical physics will have to deal with this factual reality, because it substantiates a presumed nonclassical result with the help of a perfectly classical process of randomizing phase and orientation of systems that are individually quantized according to the older Bohr-Sommerfeld rules.

It seems highly unlikely that the following admonition can be ignored by further submergence in nonclassical imagery:

> This one example of a classical duplication of results, that were hitherto believed to be typically nonclassical manifestations, imposes a dire obligation for a fundamental reassessment of whether or not nonclassical procedures are an essential and unavoidable ingredient of physics.

It is essential here to take note of the fact that mutual phase and orientation are *nonlocal* ensemble variables. As nonlocal ingredients, they are outside the jurisdiction of Bell's theorem. Locally, phase and orientation are physically inconsequential for system performance, because for an isolated system they are irrelevant positionings in time and space. Hence phase and orientation slip through the mazes of Bell's theorem.

8. Conclusion

These discussions have shown nothing less and nothing more than that the nonclassical imagery of the Copenhgen school can neither be exclusive nor conclusive, because it permits a rather classical counterpart in the form of phase and orientation randomized ensembles. These established facts raise serious questions as to the uniqueness and correctness of those standard quantum mechanical operations which claim a need for nonclassical conceptualization as an unavoidable alternative.

It is furthermore established that the need for nonclassical imagery is a contingency of a rather tacit underlying hypothesis, which permeates, with few exceptions, almost all of modern quantum mechanics. It is the unproven assumption that the Schroedinger process is a single-system description.

Dropping the single-system hypothesis and replacing it by an ensemble hypothesis permits the combining of two statistical calculations discussed in treatises by Planck and Feynman. These calculations, reproduced in chapter III:5, reconfirm two major predictions that were regarded as typical and crucial for the acceptance of the Schroedinger process.

While the Planck calculation was done well before the time when the Schroedinger equation had made its appearance, the Feynman-Kompaneyets discussions appeared some forty years after this event. The Feynman Lectures don't make an attempt at assessing its effect on the prevailing quantum interpretations of the day. It is presumably presented as an interesting oddity, because it appears in volumes II and III. The surviving authors of the Feynman Lectures may well be in a better position of casting more light on the motives that led to the incorporation of these calculations in their text.

In the light of this factual background we presume, it is the combination of the Planck and Feynman-Kompaneyets results that presently makes an assessment of its impact on quantum interpretation an unavoidable subject, which will have to be further addressed.

Perhaps regrettably, there is an anticlimactic quality to the ensuing reversal of the classical→nonclasical transitions of the past. It brings the Schroedinger equation down from the lofty heights, as a gift from heaven, to a product made by man for man. By the same token, we may now better know its limitations, as well as as its amazing potential. Here is a preliminary perusal.

If the Schroedinger equation is indeed to be taken as giving an ensemble description, we need to ask what category of ensembles are admissible and what are the primary features covered by this description. In the light of the preceding discussions, the first question can be answered by mentioning that it should be a category of ensembles of systems that are random in phase and orientation.

In regard to the features covered, we may consider that an ensemble of systems immediately raises the question as to how ensemble performance is to depend on system density. Since the Schroedinger process works with normalized wave functions, there is a strong suggestion it covers a common denominator feature shared by ensembles of varying degrees of density. We play safe by restricting matters to diluted ensembles, as long as there are no explicit system interaction potential in the wave equation. Seen from this angle, the Schroedinger equation without a system interaction potential just provides that minimal system interaction in the ensemble to establish a phase-and-direction randomness and concomitant zero-point effects.

A retrospective view at these endeavors shows how quantum theory through the years has vacillated between some as yet poorly defined procedures pertaining to the concepts of local, nonlocal and global. The Bohr-Sommerfeld single-system approach with quantized cyclic integrals is typically global. The Schroedinger process and the Bell theorem exactly straddle this local-global domain, which is so characteristic of an ensemble situation. The notions local, nonlocal, and global have emerged in mathematics as well as in physics. However, their definitions are, as yet, still in a state of limbo. A further alignment between the mathematical and physical concepts of local and global is undertaken in chapter VI;8.

A SOMMERFELD-DE RHAM VIEW OF SINGLE SYSTEMS

CHAPTER VI

PERIOD INTEGRALS: A UNIVERSAL TOOL OF PHYSICS

1. Introduction

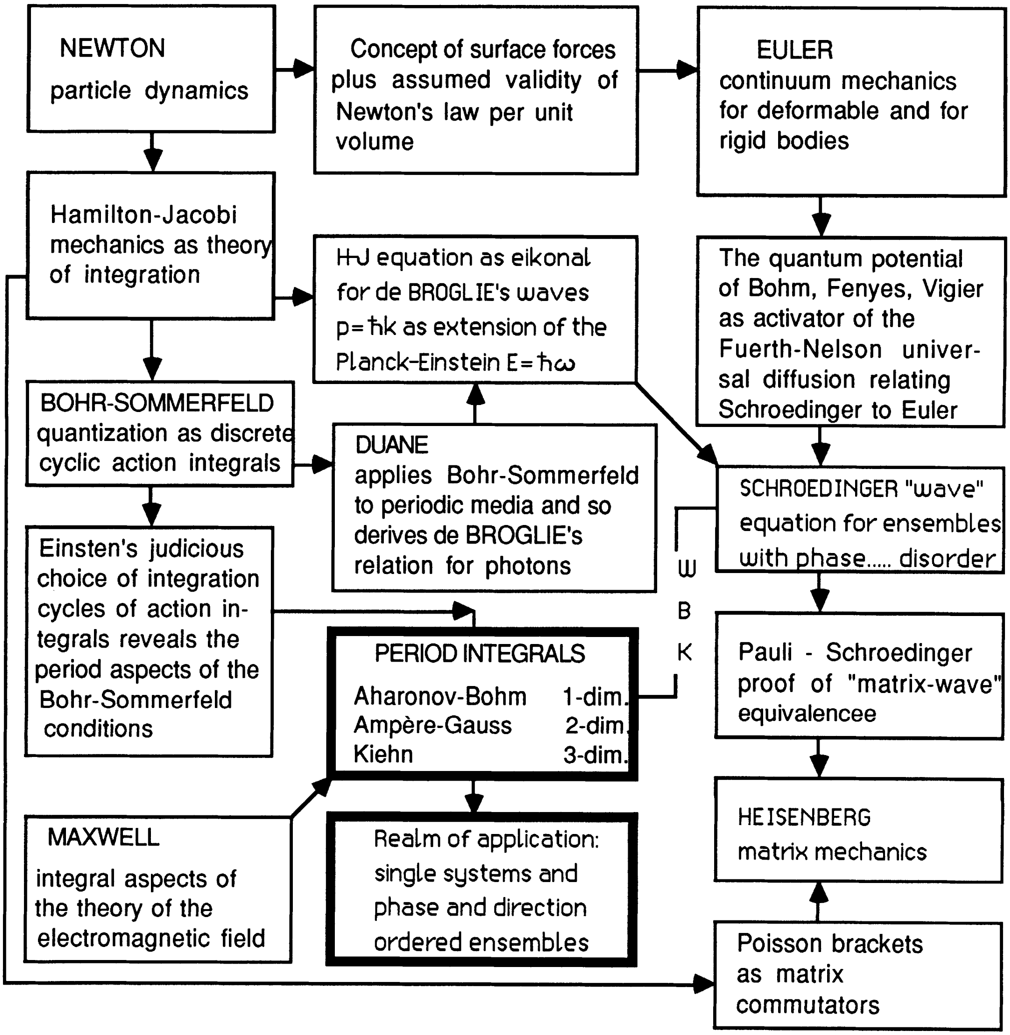
The dictionary definition of *ensemble*, which has served so well in chapter I, still yields further service in tackling the description of highly ordered ensembles. Since the local-global eigenvalue process has manifest limitations for ordered ensembles, the possibility of an all-global process now needs to be considered.

The inner organization of highly ordered ensembles suggests a procedure that is global from the start. Mathematically, it means differential processes make place for integral processes. Such methods have been around for a long time and may be placed under the general heading of period integrals. An historic perspective is useful for getting acquainted with these global mathematical concepts. The ultimate objective aims at putting global methods in context for the purpose of topologically exploring highly ordered situations (*e.g.*, the quantum Hall effect). This leads us to a de Rham-style (quantum) cohomology of physical spacetime. The ensuing perspectives for the dynamics of topological shape and the changes thereof are then further developed in chapters XII and XIII.

The words **local** and **global,** as used in the previous paragraphs, may not as yet signal an immediate mental alert in the context of physics. In the mathematical literature, by contrast, these words have become concepts of considerable consequence since the Twenties and the Thirties. There is a booklet (edited by Chern[1]) published in 1967 by the Mathematical Association of America, which was exactly aimed at familiarizing the world of mathematics more generally with these ideas. Having said this, one would expect a physicist, exploring the importance of these ideas for physics, to look forward to sparklingly clear definitions on the basis of which the incorporation of these concepts in the world of physics might be further pursued. The situation is, however, not quite as simple as all that. Yet, by the same token, this added complication is exactly what makes things more interesting (compare chapter XVI;3).

Marston Morse,[1] one of the pioneers of this branch of mathematics, mentions in his contribution that hard and fast definitions of the notions of **local** *versus* **global** (or equivalently **in the small** *versus* **in the large**)

HOW PERIOD INTEGRALS RELATE TO PHYSICS



Flow chart II: illustrating interconnections between period integrals and various disciplines of physics. WBK denotes Wentzel-Brillouin-Kramers asymptotics. Note also a dominance of a potential reduction of many mechanical phenomena to a common electromagnetic origin.

are highly improbable and not really necessary or even desirable. Instead, we do better to go through a process of osmosis to absorb these ideas in the process of seeking new application. In physics, this realm of local versus global literally acquires a new dimension, by virtue of the transition from geometry to kinematics. We shall find that the kinematic backdrop much enriches the merely static backdrop of geometry. In fact, it gives some food for thought whether restricting geometry as a branch of mathematics may not be too confining for a free emergence of new mathematical concepts. Inspired by Morse's wise counsel, let us explore this new realm by using the added dimensions of kinematics and physics. In the process of doing so we may recognize many ideas that from time to time have already surfaced in physics as isolated fragments.

The phrase "period integral" is one of those things, which may not now ring a bell of recognition for many physicists. In fact, depending on specialization, the word may only have a ring of vague familiarity to many mathematicians. In recognition of these factual realities, a discussion aimed at delineating their fundamental role in physical description should do well by starting from scratch.

An idea of what a departure from a position of "scratch" amounts to is best obtained by establishing where and when period integrals have first been used in physical theory. If this intimates that period integrals are perhaps items of a bygone era, the answer is: yes! Therefore, using the old names has disadvantages, because period integrals have been around in different disguises and under a variety of name identifications. To avoid a premature identification with earlier more narrow connotations, a new name may well be desirable. Since our mathematical brethren have, in recent years, much extended the concept and use of these structures under the name *period integrals*, let us adopt here this mathematical identification, because it does not conjure up undue bias of earlier physical applications.

The projected treatment therefore starts out with a reasonably encompassing overview of period integrals related to procedures in physics. Where the interrelation seems tenuous at first encounter, a preliminary attempt is made to call attention to common characteristics, justifying a lumping together in the same category. After having done so, an overview is given of period integration from an angle of the mathematics of differential forms. The definitions of differential forms have to be explicitly adapted to needs pertaining to parity and time-reversal operations of physics. Subsequently, the use of forms and their period integrals is sketched in their exploration of manifold topology. Finally, a transition to physics is accomplished by identifying the topology creating "substance" or "substances" of physics.

2. Period Integrals in Contemporary Physics

One of the earliest and still most useful period integrals in present-day physics undoubtedly is Gauss' integral of electrostatics:

$$\oint_{c_2} \mathbf{D}\cdot d\mathbf{S} = \Sigma_k q_k \; . \qquad 1$$

In Eq.1 **D** is the field of dielectric displacement integrated over a cyclic (*i.e.,* a closed) 2-dimensional domain c_2. The right hand member of Eq.1 represents the algebraic sum of electric charges q_k enclosed by c_2. Every textbook on the fundamentals of electromagnetism explains how the integration domain c_2 of this integral can be dented or otherwise deformed without affecting its value, provided these deformations take place in domains of zero charge $q_k=0$. According to Gauss' theorem, the deformations of c_2 can only take place in domains where div**D**=0. *It is this invariance property of the right-hand member of Eq.1 under deformations of c_2 in the realm* div**D**=0, *which is normally regarded as the most characteristic property of a period integral.* The period integral acts, so to say, as a perfect sensor of what is inside the enclosure c_2 ; it might be said to be a mathematical analogue of the human sense of touch by just making sure what it is we are dealing with. In fact, it does not matter whether the electric charge inside is of a macroscopic or of a microscopic physical size. Since it registers all charges inside c_2, Eq.1 is universally valid in the macro- and microphysical domain. Add to these observations that all charge is additive and reducible to an algebraic sum of multiples of elementary charges $\pm e$. *Since charge counting is not expected to depend on metric specifications, a metric-free rendition of Eq.1 is implied.*

Once Gauss' law has been accepted as the prototype of a period law, an immediate analogue presents itself pertaining to gravity. It says a closed surface integral of the Newtonian gravitational "displacement" **g** equals the sum of the gravitational masses enclosed by a cyclic integration domain c_2:

$$\oint_{c_2} \mathbf{g}\cdot d\mathbf{S} = \Sigma_k m_k \; . \qquad 2$$

The integration cycle c_2 is here deformable where $\mathrm{div}\,\mathbf{g} = 0$. *Eq.2 is not as perfect a period integral as Eq.1, because, unlike electric charge, mass is not a perfectly additive quantity.* The energy associated with mass interaction leads to a small mass defect in the sum mass, whereas electric charge interaction leaves the sum charge unaffected.

The just-given statement may at first seem only weakly confirmed. In the course of time, however, it has now become a factual truth born out by longstanding observation. It is a consequence of established experience, which indicates that Coulomb interaction energies leave electric-charge unaffected. By contrast, gravitational and other interaction energies (say Coulomb) do affect mass distributions by virtue of the mass-energy theorem of relativity. *This theorem prevents mass from being a purely additive quantity.*

The next period integral to be considered is the Ampère integral. It is a one-dimensional loop integral of the magnetic field **H** taken over a cyclic domain, say c_1. This integral equals the algebraic sum of currents J_k linked by c_1 :

$$\oint_{c_1} \mathbf{H}\cdot d\mathbf{l} = \Sigma_k J_k \,. \qquad 3$$

The cycle c_1 is deformable where the current vanishes (*i.e.,*where curl**H**=0, as follows from Stokes' theorem).

A comparison between Eqs.1 and 3 gives the following items of informational interest. Eq.1 is about charge at rest, Eq.3 is about charge in motion. Eq.1 can only assume values that are multiples of the charge quantum, Eq.3 can assume a continuum of values depending on how fast charges move through the loop c_1 .

Since the fields **D** and **H** jointly occur in the same Maxwell equation, it is tempting to give also their period integrals a joint connotation. This can be done by extending the physical period integral concept from space to spacetime.

Instead of *enclosing* charge at rest, in a spacetime context, Gauss' integral must be regarded as *linking* world lines of charges that are progressing solely in the time direction. Tilting the worldlines in a spatial direction transforms the electrons at rest into electrons in uniform, cooperative motion. The c_2 , which was a closed spatial surface before the tilt, now is a closed two-dimensional c_2 imbedded in spacetime.

The next question is whether a spacetime cycle c_2 can, in general, be used for the purpose of selectively linking the worldlines of charges that are in a cooperative state of motion. The answer is yes! To see this, one may consider a time integration of the Ampère integral Eq.3, its right-hand member then assumes the dimension of charge. A spacetime unification of Eqs.1 and 3 can now be written as follows:

$$\int_T dt \int_L \mathbf{H}\cdot d\mathbf{l} - \int_S \mathbf{D}\cdot d\mathbf{S} = \Sigma_k q_k \,, \qquad 4$$

in which the integration domains denoted by T,L, and S are understood to join up to form a true 2-dimensional cycle c_2 in spacetime linking the worldlines of q_k for all k.

Mindful of the traditional, inertial frame-based spatial identification of **H** and **D**, as commonly encountered in textbooks, the evaluation directives for Eq.4 seem easier said than accomplished. Whenever we see an integral, we are so conditioned to look for an evaluation that we may not see the obvious answer. To eliminate any conceivable concern, let it be said that the evaluation is already given in Eq.4, provided the c_2={T,L;S} truly resides in a charge- and current-free domain (div**D**=0; curl**H**=0). The major task is making sure the integration cycle meets the period integral specifications. *As long as the c_2 deformation invariance is met, the complete potential of a Cauchy-like procedure of integral evaluation is at our disposal.* Of course, this fact is not too surprising, because the Cauchy integral in the complex plane is itself a period or residue integral.

While the Ampère and Gauss integrals were early, and perhaps the earliest, examples of period integrals (not only in physics but also in mathematics) a relative newcomer to this family of period integrals is the cyclic version of the line integral of the vector potential **A**. One may wonder: why did not the first pursuit of period integration by Ampère and Gauss trigger an all-out search for other examples of period integration in physics? A little detour in history provides some added perspective.

Earlier this century, a state of mind has been prevalent among mathematicians, insinuating that knowledge of physics unduly burdens the mathematical mind. Conversely, many physicists hold that too much mathematics obscures the true spirit of physics. To those mathematicians who have chosen to remain ignorant of physics, it may well be a surprise that Gauss, who introduced the early concept of period integration in physics, was himself a mathematician.

Even if it is true that in science, as well as elsewhere, the master does well to know his limitations, the Gauss example shows how easily one could be shortchanged by undue cognitive restriction. All of which goes to show that words of wisdom, when pulled out of context, can cause a severe case of myopia.

So, returning to the subject matter, one finds that the key to these developments has been, without question, the integral theorems which convert cyclic integrals into integrals over the interior of the cycle. The two-dimensional case has been named after Gauss; the one-dimensional case has been named after Stokes. Work of Poincaré and Brouwer has led to higher dimensional mathematical extensions of these integral theorems. They are now referred to as the generalized Stokes theorem. Only earlier this century became the application of period integration a major tool in exploring manifold topology (de Rham).

A new physical perspective on the integral $\int \mathbf{A}\cdot d\mathbf{l}$ came to the fore, when it was found to occur as a phase factor of the Schroedinger wave function. The uniqueness requirement for the wave function then became associated with a cyclic integration of $\oint \mathbf{A}\cdot d\mathbf{l}$. In this manner, F. London[2] first inferred the possible existence of flux quantization, and Aharonov-Bohm subsequently elevated this integral to a neat, independent tool in theoretical physics. Its period integral features are beautifully illustrated in what is now known as the Aharonov-Bohm effect.[3] Also London's application to the super-conducting ring illustrates, at least in principle, how the integration path (as a result of the Meissner effect) is deformable in the field-free interior of the superconducting ring. *Singular cases will come into the focus of special attention later, when it will be necessary to consider the possibility of field-free domains that can shrink to the integration cycle itself* (chapter VIII; 2; the electron's field-free interior).

Since the vector potential $\mathbf{A}$ is part of a four-vector with the scalar potential V as fourth component; also this component needs to be included in the complete Aharonov-Bohm integral:

$$\oint_L \mathbf{A}\cdot d\mathbf{l} - \oint_T V dt = \Sigma_k \Phi_k \text{ , integrated over a cycle } c_1 = \{L;T\} \qquad 5$$

The right-hand member of Eq.5 gives the sum of fluxes Φ_k linked by c_1. While the period integral properties of Eq.5 seem to be beyond question, the fact is that its applications are not as clear and straightforward as for the Ampère-Gauss integral. In praxis, however, most cycles of Eq.5 are either purely spatial or purely time-like; cyclic in time is a return to an initial situation.

Another difficulty associated with Eq.5 has to do with the choice of elementary flux unit Φ_0 . When flux quantization was first discovered, the expected flux unit was h/e; the observed unit turned out to be $h/2e$. *The latter unit* $h/2e$ *typically occurs when the electrons are in cooperative fashion moving in each other's magnetic field.* Such is the case in the superconducting ring; notice that this situation requires at least the presence of two electrons as participants in the process (see chapter VII).

Eq.5 could be regarded as a by-product of the Bohr-Sommerfeld integral $\oint \mathbf{p}\cdot d\mathbf{l} = nh$, because $\mathbf{p} = m\mathbf{v} - e\mathbf{A}$. Inside the field-free region of a superconductor, there is no momentum $m\mathbf{v}$, which means $\oint \mathbf{p}\cdot d\mathbf{l} \rightarrow e\oint \mathbf{A}\cdot d\mathbf{l}$. Hence, the Bohr-Sommerfeld condition assumes the appearance of an Aharonov-Bohm integral.

Since the asymptotic relation between the Schroedinger equation and the Bohr-Sommerfeld conditions is known to lead to a corrective modification of the latter from $\oint \mathbf{p}\cdot d\mathbf{l} = nh$ to $\oint \mathbf{p}\cdot d\mathbf{l} = (n+1/2)h$, one may well wonder whether or not this correction could imply the existence of some sort of a zero-point flux, say in the sense of $e\oint \mathbf{A}\cdot d\mathbf{l} = (n+1/2)h$ for n=0. Taking into account the half-valued flux $h/2e$ in superconducting rings, this would be $\oint \mathbf{A}\cdot d\mathbf{l} = (n+1/2)(h/2e)$.

An inspection of the experimental data of Deaver-Fairbank and Doll-Naebauer[4] does not give any support whatsoever for the existence of such a zero-point flux state (chapterIII; 6). Here again is a subtle reminder that zero-point phenomena are ensemble-related, not single-system related. *Hence, the mathematical asymptotics between the methods of Schroedinger* *and Bohr-Sommerfeld reflects the difference in the physical situations associated with an ensemble of single-systems versus an isolated representative of those single-systems.*

Recently, magneto-resistance experiments, performed by several experimentors[5] on extremely small-sized rings of normally conducting metals, have confirmed a resistance periodicity indicative of the London flux unit h/e. To observe the h/e periods, it is essential to work with rings that are flat. Experimentation with rings in the form of tubes has led to the observation of periods $h/2e$, notwithstanding the presumed absence of superconductive pairing. Interestingly, an explanation of the half-period $h/2e$ was found to be related to an ensemble formation of phase-randomized parallel current sections in the tubular ring. One can hardly avoid noticing a similarity with Planck's introduction of the zero-point energy as an ensemble-based feature (chapter III).

The fact that many applications of Eq.5 are, strictly speaking, outside the realm of period integration proper may well have been a major reason why the parallelism between Eqs.4 and 5 has not been stressed in the physical literature. The integral Eq.5 retains, of course, physical meaning, even if the integration cycle is not in a field-free domain. For example, an electron in an external magnetic field circulates in a quantized cyclotron orbit. Its so-called discrete Landau orbits are found to link flux increases in steps of h/e not $h/2e$. Hence, in the pure period integral case the right-hand member of Eq.5 is measured in multiples of $h/2e$, whereas the "nonperiod" case leads to a right-hand member measured in multiples h/e. Since discrete residues in the right-hand member are a trademark of period integration, it seems as if a period feature retains; even if the period condition does not seem to be met (chapter VII). Let us consider a somewhat similar state of affairs that has to do with the familiar Bohr-Sommerfeld integrals of the earlier quantum mechanics.

A convenient starting point is the equation of motion, say of an electron, in a central Coulomb field k/r^2. In polar coordinates, one has, for the simple case of a planar motion:

$$m\ddot{r} - mr\dot{\phi}^2 = k/r^2 \quad . \qquad 6$$

Multiplication of Eq.6 with r gives in the right-hand member of Eq.6, the Coulomb potential times the electronic charge: $eV = -k/r$. Hence, a subsequent time integration over one period of the motion gives, on the right-hand side, the Aharonov-Bohm integral times e and on the left, after partial integration, two Bohr-Sommerfeld integrals. The end result is:

$$\oint p_r dr + \oint p_\phi d\phi = e \oint_T V dt. \qquad 7$$

Since the left of Eq.7 is known to equal a multiple of h, then Eq.7 implies

$$\oint_T V dt = \text{multiple of } h/e, \text{ not } h/2e. \qquad 8$$

Questions present themselves if we like to come to a well delineated understanding of the Eqs.7 and 8. First of all: are the Bohr-Sommerfeld integrals period integrals, and, if so, does that make Aharonov-Bohm, as applied in Eqs. 7 and 8, a period integral?

The Bohr-Sommerfeld integrals are indeed period integrals if the Pfaffian expression defined by the four-vector of energy-momentum {H;-p} is integrable, because the integration path can then be deformed in the domain where the local integrability holds. Here is a brief proof that shows how the Hamilton equations of motion are the very conditions that substantiate this integrability. Consider the Pfaffian

$$dW = H(p_l, q^k, t)dt - \sum_l p_l(q^k, t)dq^l \quad ; k,l = 1,2,3.$$

The expression dW is a total differential (*i.e.*, closed in terms of differential form language; see next section) if the coefficients of dW satisfies the following relations of crosswise differentiation:

$$\sum_l \frac{\partial H}{\partial p_l}\frac{\partial p_l}{\partial q_k} + \frac{\partial H}{\partial q_k} = -\frac{\partial p_k}{\partial t} \qquad 9$$

$$\frac{\partial p_l}{\partial q_k} = \frac{\partial p_k}{\partial q_l}. \qquad 10$$

Since $\partial H/\partial p_l = \dot{q}^l$, substitution in Eq.9, while using Eq.10, gives after rearrangement the other Hamilton equation $\dot{p}_k = -\partial H/\partial q_k$, because $\dot{p}_k = \partial p_k/\partial t + \sum_l \dot{q}^l \partial p_k/\partial q^l$. It thus follows that the left-hand side of Eq.7 is a period integral. If that is so why is the right-hand side of Eq.7 not a period integral? We know it can be a period integral, but, as it is, the cyclic time path of integration resides in a realm of spacetime where the electric field E is definitely not zero. There are here a number of unusual features requiring a conceptual clarification.

First of all dW is a total differential, indicating the existence of a scalar field W from which H and p derive by a gradient process, yet there are loop integrals of p that don't vanish. It means that either W is not single-valued, or the integration loop links certain forbidden domains. This situation is similar as for the Gauss integral, its cyclic integrals vanish if the cycles don't enclose net electric charge.

After this comparison, the next question naturally is: what is the physical nature of the "obstruction" that is being enclosed or linked by the cycles of the Bohr-Sommerfeld integrals? An answer to this question is not readily found in the existing textbook literature on analytical mechanics. The first explicit reference to this question that has come to my attention occurs in a little-known paper by Einstein[6] in which he attempts to find more of a justification for the Bohr-Sommerfeld recipe of loop integration. Why does this recipe so surprisingly lead to very sensible answers? What happens if the integration loops are chosen in a different manner?

In the cited investigation, Einstein arrives at the conclusion that the loop integrations are only nonzero if they link with what may be called the orbital manifold. For a central force motion, the orbital manifold is a "flat" torus (opens up by orbital precession). The nonzero Bohr-Sommerfeld loop integrals link with the torus either as an internal azimuthal cycle for $\oint p_\phi d\phi$ or/and as an external meridional cycle for $\oint p_r dr$.

Note how the Einstein construction of replacing a flat annular region by a "flat" torus makes the two-valued p field single-valued. In so doing, he made the orbital manifold into a topological object and established the period character of the Bohr-Sommerfeld integrals as a means to explore the topology of that object. Einstein's method was a precursor of a general mathematical procedure, which was, in particular, developed by de Rham[7] for exploring manifold topologies by period integrals.

The several originators of the Bohr-Sommerfeld integrals had all made a choice of intuition. Yet, Einstein's topological features never reached stages of maturity and fruition in physics, because, soon afterwards, Schroedinger's method replaced the Bohr-Sommerfeld conditions. The latter were then declared to be an approximation of the Schroedinger method, which was now believed to be the more exact answer.

Returning to the main theme of this discussion, the conclusion has now been reached that the left-hand side of Eq.7 indeed constitutes a set of period integrals. What are the implications of this conclusion for the right-hand side of Eq.7 ? Here is an Aharonov-Bohm integral, which is known to be capable of displaying period features, yet in this case, the time integration path "resides" in an interval of time for which $E \neq 0$. Even if there is no law that says period properties are transmitted by the equal sign of Eq.7 to the right-hand member, the possibility of such an occurrence should not be ignored.

Consider Einstein's orbital manifold for a Bohr circular orbit. The torus manifold now collapses to a circle. The integral $\oint p_r dr$ vanishes, only $\oint p_\phi d\phi \neq 0$. Is the latter no longer a period integral, because its deformation domain has collapsed? It would seem an injustice to the procedure to deny the period property if the deformation domain has collapsed to the path of integration itself. In a similar fashion, a possible rescue operation for the period character of the right-hand member of Eq.7 ought to be considered, so as to make sure no valuable opportunities are here unduly discarded.

A closer inspection of Eqs.6 and 7 raises questions about the hybrid character of these equations. On the right-hand side are the potential and an Aharonov-Bohm integral, which are honest-to-goodness field structures. Yet, on the left, there is, by contrast, a mathematical expression which definitely is not a field structure. It is based on abstractions such as particle mass m, which unlike particle charge e cannot, as earlier mentioned, be reduced to the same simple basis in field theory. What is being done here (and what everybody has done since Newton, Lagrange and Hamilton-Jacobi) is really an attempt at using a judicious mixture of field theory and particle abstractions.

Strictly speaking, the period properties of Bohr-Sommerfeld integrals are to be thought of as residing in a Hamilton-Jacobi configuration space, whereas the period integrals of field theory are presumed to reside in spacetime. The overlap of experiences is in their sharing of a common physical space of three dimensions. Since there are Bohr-Sommerfeld period integrals with regions of cycle deformation collapsing to the cycle itself, then why not have Aharonov-Bohm integrals with regions of cycle deformation collapsing to the cycle itself?

For a particle cruising in an external field, it is difficult to see, however, how, similarly to the Bohr-Sommerfeld case, the permissible deformation domain could be said to have collapsed into the cycle itself. Yet there is still another possibility: *it is the option of having particles with a field-free interior*. Such particles trace their own quantized field-free orbit.

The next question is whether some of the known particles may be said to have a field-free interior, with the conclusion that those that don't have a field-free interior would be recognizable by a selectively distinct behavior. For instance, the discrete Landau cyclotron states of an electron in a magnetic field could be taken to be indicative of a field-free interior of the electron. All of which prompts the question: have other particles ever been systematically checked as to whether or not they occupy discrete quantized cyclotron orbits? A transition from point-particle concept to particles with field-free interiors may at first be a somewhat mind-blowing proposition in the light of modern methods of approach. Yet, since models with a field-free interior are actively considered in chapters VIII and IX, it may be well to become already mentally prepared at this stage. *The fact is that many applications are contingent on this concept of particles with field-free interiors.*

The hybrid situation between particle abstraction and field theory generates, however, numerous other questions. For instance, up to this point there are the one- and two-dimensional period integrals for fields: Aharonov-Bohm and Ampère-Gauss. They assess flux and electric charge, respectively. Then there are the one-dimensional period integrals for particles of Bohr-Sommerfeld, which, according to Eq.7, relate to Aharonov-Bohm. The Bohr-Sommerfeld integrals, unlike the Aharonov-Bohm integrals, assess an integrated form of angular momentum or action: not flux! It is the existence of discrete electric charge that enables this flux-action transition. Yet, charge itself is the residue (period) of a two-dimensional period integral. Kiehn[8] has therefore concluded that, when assessed in terms of fields, angular momentum, be it orbital or in the form of spin, should really be regarded as the residue of a three-dimensional period integral. Since three-dimensional cycles can only be imbedded in a four-dimensional manifold, a spacetime assessment of physical law now is unavoidable.

At this juncture, the presented overview of contemporary physical theory indicates the existence of one-, two-, and three-dimensional period integrals for sizing up field configurations in spacetime. The residues of these integrals are flux (magnetic as well as electric), electric charge, and spin-orbital angular momentum. These residues are measured in terms of multiples of h/e or $h/2e$, e and h. They are constants of nature, and, to the best of present knowledge, they are known to be good spacetime invariants under general spacetime substitutions. All these features happen to be natural topological prerequisites.

In this day and age, practitioners of non-Abelian gauge theories would most likely not tolerate an absence of the many times hypothesized magnetic charge. Mathematically this hypothesis claims that not all cyclic integrals of the magnetic induction $\mathbf{B}$ should vanish:

$$\oint_{c_2} \mathbf{B}\cdot d\mathbf{S} \neq 0; \text{ at least for some } c_2. \qquad 11$$

The original idea, that Eq.11 should vanish for all c_2 , may well go back to Maxwell. Off and on, there have been suggestions, though, that Eq.11 might nevertheless be true. Yet, most of the time, such rumors have been followed by a withdrawal of the suggestion by the author or a rejection by the knowledgeable authorities in the field.

An exception was seemingly "Dirac's magnetic monopole."It lingered on, it was modified, and until this day it has remained an item of interest to theoretical physicists. In an act of justice and reverence to Dirac, let it be known that Dirac's integration cycle c_2 was **never** a cycle. He specified a pinhole in c_2 to let through a "singularity line" connecting the monopole to infinity or a monopole of opposite polarity. In other words: *Dirac was careful not to violate the iron rule of Maxwell theory which demands Eq.11 to vanish for all honest-to-goodness true cycles* c_2 (*i.e.*, perfectly closed without any pinholes whatsoever).

From a point of view of modern mathematical-physical theory, the existence of a magnetic monopole now is as impossible as it was in the past.

The fields $\{E;B\}$ derive from a globally defined $\{V;A\}$, which has its own (experimentally observed) residues. *De Rham's theorem then demands that the 2-form* $\{E;B\}$, *which derives from the one-form* $\{V;A\}$, *is exact (i.e., no periods or residues, meaning no magnetic charge).*

Notwithstanding an exhaustive search over decades, all known efforts to experimentally identify magnetic charges have had a negative result. **In this excercise in quantum reprogramming, the nonexistence of magnetic charge shall be accepted as Nature's near-unambiguous answer to this most encompassing inquiry into its existence.**

3. Mathematical Tooling

In the preceding section at least three good period integrals of physics have been confirmed to exist. Two of them are in current use; they are the 2-dimensional Ampère-Gauss integral and the 1-dimensional integral of Aharonov-Bohm. The third one, a 3-dimensional period integral, is of recent vintage. It has now been applied in situations of potential consequence. Yet, before such applications come to the fore, it is important to agree on some convenient mathematical language.

Since period integrals, as they are here used, don't really demand a detailed evaluation in terms of coordinates and field components, a notation which does not require coordinate details is to be preferred. Cartan's method of differential forms (see ref.7) is the ideal instrument for such purposes. The method applies to so-called "pair" and "impair" scalar-valued integrals. These integrals remain invariant under general spacetime coordinate changes, except that *the impair integrals change sign under an orientation-changing spacetime transformation; the pair integrals do not!*

Since the coordinatization is inconsequential, the notation leaves out all coordinate reminders. Hence, instead of $\Sigma_l \oint A_l dq^l$, one simply writes $\oint A$. For the double integral $\Sigma_{lk} \oint\oint B_{lk} dq^l \wedge dq^k$ one similarly writes $\oint B$, or possibly $\oint\oint B$, if the double integration needs emphasis. Antisymmetry of the element of area $dq^l \wedge dq^k$ engenders, through the summation l,k, the corresponding antisymmetry $B_{lk}=-B_{kl}$. The symbols A and B, so defined through the indicated summations, are said to represent differential forms. They invariantly combine integrand and integration element. The differential form A is said to be a 1-form and the differential form B is said to be a 2-form, definitions of 3-forms, or p-forms, for that matter, follow the same recipe. The number p is, of course, not allowed to exceed the dimension of the manifold n, because a form which has p>n is identically zero.

The differential operation allowed in this context has to preserve the cited invariance under general coordinate substitutions. It is known as the exterior derivative d. The exterior derivative d operating on a p-form converts this into a (p+1)-form. In three dimensions dA means curl**A** and dB means div**B**. In fact the exterior derivative d replaces all standard vector operations grad, div, curl, in three dimensions. Moreover, unlike these vector operations, d is valid in manifolds of arbitrary dimensions under arbitrary coordinate transformations. Hence, the mathematical tooling does not merely cover translations and rotations, also orientation changes and nonlinear transformations are also covered.

Since the invariance of the differential form operations does not depend explicitly on metric structure, its applicability becomes of special importance for those realms of spacetime physics that are not related to gravity. *The basic quantization rules of nature manifest a perhaps surprising metric-independent general invariance.* The latter fact may well be considered as indicative of a possible misalignment between physical reality and the many efforts of quantizing gravity.

The reader will note how higher manifold dimension and extension of the group of invariance give surprisingly added simplicity of invariant operations, albeit in exchange for a more discerning assessment of the objects on which the exterior derivative "d" operates. The simplicity of vector analysis in three dimensions is bought by a reduction to **one** vector species, which is possible only under the group of proper rotations. The differential form equivalent, by contrast, works with **four** distinct differential forms. They are the pair one-form, impair one-form, pair two-form and impair two-form, corresponding to **four** vector species.

There exists, of course, an equivalent version of vector analysis, which is not "watered-down" to a single vector species. A detailed discussion of how the vector species reduce as a result of group restriction has been highlighted in a text by Schouten.[9] He makes an interesting comparison with Faraday's lines and tubes of force. So far, a differential form equivalent accounting for orientability features can only be found in the already mentioned text by de Rham.[7] Since physical description reaches further with all four vector species, Faraday was far ahead of the late 19th century mathematical simplifiers who gave us standard vector analysis.

While differential forms are these days standard fare in mathematics curricula for physicists, the applications to physics have not gone much further than lip service to Cartan's "new" methodology. The simplifications of the past, once hailed as milestones in undergraduate instruction, have gone too deep to expect sudden change. The objective of this section therefore, can, at best, be a reminder of what has been done in the past and what can be found in the literature.

The task of reassigning all distinct physical vectors to four species, which in the past were assigned to the one and only species of vector analysis, is an undertaking that does not mix well with writing a monograph on quantum reprogramming. An international forum would be necessary, and, even then, one may still end up with a perennial fight similar to the one between the proponents of cgs- and MKS units.

If, in the following readers, are confronted with vector-assignment choices pertaining to the above, decisions to this effect are not always accompanied by complete explanations. Relevant sources should be consulted for more detailed information. Yet, of all the questions that can be expected to surface in this context, there is one that merits further elaboration. It has to do with the physical role of what is known as the spatial and spacetime metric structure.

Almost all discussions in physics are predicated by the existence of a spatial- and/or a spacetime metric structure. The metric's function is one of giving us a comparison of size in space and a comparison of how fast things evolve in spacetime. Most physicists regard the spatial metric as something inherited from the neighboring discipline of mathematics. As such, it is not normally regarded as a typical subject for physical scrutiny.

The spacetime metric has more of an explicit physical connotation, because its major metric coefficient c^2 invokes the vacuum speed of light c. The latter has been the subject of extensive physical investigations. Yet, a full-fledged involvement of the spacetime metric in physical theory has occurred for the first time with the event of the general theory of relativity. Its principal implications seem to be macroscopic in nature.

In the light of the above observations, it will be clear that the metric has now become much more of a physical entity than in the past. Yet, so far, its major physical impact may be said to be in the macro realm. On a microscopic scale we normally extrapolate, "when necessary," the existence of the macroscopically observed metric. Yet, the phrase when necessary should really be replaced by the word "necessarily," because the mathematical tools that are being used in contemporary physics don't give us any choice at all. The standard tool of vector analysis, and just about any other tool of mathematical communication in physics, only exist by virtue of a built-in, mostly hidden, metric structure. In fact, it is built-in in a manner so as not to be seen, which is as well, because most of the time it is not needed; only the general theory of relativity invokes explicit metric use.

One could bring to bear a pragmatic argument to leave the metric where it is (*i.e.*, invisible). One does not expect gravity to have a very explicit role in microphysical considerations anyway. It just happens to be that Cartan's formalism of differential forms is based on metric-independent invariant operations. It holds in differentiable manifolds devoid of metric structure. The injection of a metric structure (say, in the form of a metric field tensor) does not affect the here-delineated Cartan formalism.

Hence, even at this point one could still leave well enough alone and proceed as usual, except that one might display at least some curiosity as to whether or not the metric-free austerity of Cartan's formalism has possible physical ramifications. The question has been asked by, among others, Cartan himself. A brief account with references can be found in Whittaker's treatise (chapter II, ref.2) on "the history of the theories of electricity and aether." The answer is: there are several laws of physics that can be written in a manner that does not explicitly depend on the spacetime metric structure. What does that mean?

Whittaker's account of this subject matter is very brief. He explains: "....discoveries (concerning a metric-independent physics) which have great potentialities, but the significance of which at present appears to lie in pure mathematics rather than in physics, and which therefore are not described in detail here." So the question remains: why is part of physics metric-independent, whereas other parts remain metric-dependent? It is one of the most engaging puzzles of Nature. Does Nature present us with such puzzles just for the sake of some intriguing mathematical exercises, or is some real physics hidden in these conspicuous properties?

An answer to these questions can hardly be forthcoming unless these distinctions are made explicit in the everyday dealings with the subject matter. Without the use of a discerning mathematical tool, the awareness is simply not stimulated to more incisive perception. After the earlier diagnosis about contemporary mathematical communication in physics as completely tied down by metric contingencies, it now should not be too surprising that little progress has been made since these discoveries were made in the early Twenties. There has been little change, since Whittaker wrote those lines in the Fifties.

Now, more than sixty years later, the Cartan method has come more in the focus of attention, yet its metric-independent character still is falling by the wayside. Users may, at best, have noticed that this newfangled thing, which mathematics is trying to sell to physics, works well only in certain parts but not so well (or not at all) in other parts of physics. A natural reaction is: why learn a discipline that is less versatile than what is presently being used?

The fact is that form-language does not go so well with a contemporary physics, which is completely cast in metric-based language. Here we have the upshot of modern trends of "prematurely selling" form-language programs. The result is a number of ad hoc applications and some temporary lip service to a new and exciting methodology, and then little or nothing happens thereafter.

The only way of making these developments less dependent on fashions and temporary trends is by first pinpointing a potential physical significance of the existence of metric-free laws. Since the spatial metric is the one and only basis for comparisons of size, the validity of a metric-free law should be independent of whether one is confronted with macro- or microscopic situations. In other words, metric-free laws have a more universal validity. *Any metric-free law that has been established in a macro environment thus has a fair chance of retaining its validity in a micro environment.*

It has not been customary to make comparable metric versus metric-free distinctions for the time domain. Birss[10] notes how spatial metric or metric-free does not always go hand-in-hand with spacetime metric or metric-free. Here the perhaps radical objective is pursued of identifying laws that are *spacetime metric-free*. This endeavor has interesting ramifications. For instance, in black holes metric notions are believed to change dramatically, yet metric-free laws can be taken to be unaffected. While contemporary physics is not in a position of making observations in black holes, there is solace in the thought that there are things in nature that might not even be affected by black holes.

4. The Integral Theorems of Stokes and de Rham

In the course of these discussions the name "de Rham" has been mentioned several times. For purposes which are being pursued here, two theorems in mathematics known as "de Rham's theorems" are of crucial importance and shall be referred to frequently. For physics, the understanding and objective of these theorems is much facilitated by a comparison with Gauss' theorem of electrostatics.

Gauss' theorem of electrostatics says (Eq.1, section 2 of this chapter): the cyclic integral of the dielectric displacement **D** equals the algebraic sum of electric charges enclosed by the cyclic domain c_2 of integration. The cycle c_2 is hereby understood to reside where div**D**=0.

For purposes of topological applicability, de Rham casts the Gauss statements in the form of an existence theorem. He says, given a set of scalars (charges) distributed in space, it is always possible to construct a 2-form (here defined by **D**) such that its cyclic integrals equal the chosen set of scalars, or algebraic sums thereof, depending on the choice of c_2.

When visualizing this situation from an angle of Gauss' original theorem, we are inclined to think of a "corner" of physical space in which the "few charges" that we had in mind reside. We rule out the rest of the universe with all its charges and merely examine the few charges under consideration. This choice is a mathematically permissible act of convenience, yet by the same token, in doing so, the option of making assertions about the totality of the whole physical universe has been forfeited. In mathematics the universe can be chosen; in physics, it is given, hence we need to settle for step-by-step exploration.

We now need to consider dimensional generalizations of the de Rham's "existence" version of Gauss' theorem of electrostatics. Where Gauss considers 2-forms in a 3-dimensional space, de Rham considers p-forms in an n-dimensional space with $p \leq n$.

At this juncture, we can appreciate the physical merits of de Rham's generalized existence version of Gauss' theorem of electrostatics. Its proof calls on a generalized version of Gauss' integral theorem. also known as Stokes' generalized theorem. Using the Cartan notation, the generalized Stokes' theorem for the p-form f_p can now be succinctly written as:

Generalized Stokes'- (Gauss) theorem
$\oint_{c_p} f_p = \int_{d_{p+1}} df_p$; with $\partial d_{p+1} = c_p$ = bounding cycle of domain d_{p+1}

The theorems of de Rham are in essence corollaries of the generalized Stokes' theorem. They come about by "removing" from the embedding manifold the domains where df_p is nonzero. The thus remaining manifold has received a topological structure by virtue of its "holes." This reduced manifold has everywhere $df_p = 0$, yet not every one of its cyclic integrals of f_p vanishes. The differential form f_p in this reduced manifold is said to be closed. Only those cyclic integrals vanish whose c_p does not enclose or link a hole that has resulted from removing those domains where $df_p \neq 0$. It is general procedure in geometry and in differential topology to speak of "holes," where physics prefers to endow the hole with an analytic continuation of f_p where $df_p \neq 0$. In either case, whether one speaks of holes or of domains where $df_p \neq 0$, both have the function of assigning a topological structure. In the case of $df_p \neq 0$, the topology is dictated by physics.

The second de Rham theorem is a special case of the first theorem. It represents a case for which all the periods vanish. An earlier form of this reduced version of de Rham's first existence theorem has also been known as Poincaré's lemma.

Using the terminology and notations explained in the previous sections de Rham's theorems can now be briefly formulated. An inspection of the literature will yield a number of equivalent renditions. One may, of course, consult de Rham[7] himself, his articles and his monograph. His articles testify to the fact that electromagnetic situations have been a source of major inspiration. Flanders[13] has made further attempts at making these matters available to physics and engineering. A beautiful discussion of the de Rham theorems has been given by Hodge in a monograph on the subject of harmonic integrals.[7] The reader be advised that harmonic integrals are a metric-based follow-up of the metric-free period integrals of de Rham's theorems. The following is somewhat a common denominator version, which follows the theorem sequence (first, second) given by Hodge.

De Rham's first theorem:
Given a "basis" of p-cycles $(c_p)_s$ on an n-dimensional manifold M, where p<n and s=1,2,3,4,...........enumerates the basis of independent cycles on M. Let to each p-cycle be assigned an arbitrary real number $(\alpha_p)_s$, then there exists on M a regular closed p-form ϕ with the assigned periods $(\alpha_p)_s$, *i.e.*

$$\oint_{(c_p)_s} \phi = (\alpha_p)_s \quad .$$

De Rham's second theorem
If ϕ is a closed p-form with **all** zero periods, *i.e.* $(\alpha_p)_s = 0$ for **all** s , then ϕ is exact and there exists a (p-1) form η such that

$$d\eta = \phi \quad .$$

The key to the applicability of these theorems is in the concept of *the basis of independent cycles* that is being considered. Looking at the first theorem in the perspective of Gauss' theorem of electrostatics, the complete basis of independent cycles identifying all the charges of the physical universe is so enormous that it is impractical to handle. For isolated physical structures, however, imbedded in this universe, the complete collection of independent cycles may be expected to be a more manageable number. Under those circumstances, the first theorem already can have an important structure-determining function.

The applicability of the second theorem on the other hand is contingent on the condition that **all** periods vanish, which is not an easily verifiable task if the totality of the whole physical universe needs to be considered. Hence, for applications of the second theorems in the context of physics, the reader should be keenly aware that physics can only speak in good confidence of its human "corners" of the universe, with a few isolated structures. Unlike physics, mathematics can more easily consider the global nature of the manifolds it cares to consider. It is this very fact that has permitted physics to avoid a face-to-face confrontation with the distinction of closed-versus-exact for differential forms. The experimentally unconfirmed, yet forever recurring contemporary concept of the magnetic monopole testifies to the fact that physics is firmly determined to have its cake and eat it too. Mainstream physics has now, for over half a century, ignored the closed-versus-exact distinction as an issue of fundamental physical relevance!

In these reprogramming discussions of quantum theory, it is argued that so many different people have now been exploring so many different "corners" of the universe that the chances of finding exceptions to the here-assigned closed-exact distinctions becomes very small indeed. Since the existence of magnetic monopoles would reduce the definition Eq.16 of the vector potential A to an, at best, local role, any global applicability of the Aharonov-Bohm integral would be in question. Faced with the experimentally verified global consequences of this integral, as in the case of flux quantization, unverified hypotheses restricting that global applicability cannot claim a basis in physical reality. In this light, we should no longer curtail a prominent role of de Rham's theorems in the realm of physics.

5. Gauss-Ampère, Aharonov-Bohm and Kiehn Integrals

This new instrumentation can now be used to give an overview of a number of physical laws that can be lifted out as permitting a metric-free period integral formulation. At this point, no attempts are made to substantiate or prove explicitly this metric-independent spacetime invariance. There is a good reason here not to obscure the main issues with lengthy mathematical proofs.

There was a time in physics when it was thought to be necessary to prove how everything under the sun in physics could be written in a generally invariant form. Soon afterwards it became clear that just about everything in physics could indeed be forced into a generally invariant form.

The now following Eqs. 12 through 21, which express familiar physical laws or natural ramifications thereof, are distinguished by the spacetime metric-free property. Metric-free extensions of those laws in spacetime have been established for the one-dimensional flux integral of Aharonov-Bohm integral, the two-dimensional charge integral of Ampère-Gauss integral and a three dimensional spin angular-momentum integral proposed by Kiehn,[8] for a joint discussion see reference.[11]

Period Integral Laws

$$\oint_{c_1} \mathcal{A} = n\, h/2e; \text{ in mutual particle } \mathbf{B}\text{-field, } A=\{V,\mathbf{A}\} \qquad \text{(pair)} \qquad 12$$

$$\oint_{c_1} \mathcal{A} = n\, h/e \ ; \text{ external } \mathbf{B}\text{-field (single charge)} \qquad \text{(pair)} \qquad 13$$

$$\oint_{c_2} \mathfrak{G} = s\,(\pm e) \ ; \ \mathfrak{G} = \{\mathbf{H},\mathbf{D}\} \text{ Ampère-Gauss} \qquad \text{(impair)} \qquad 14$$

$$\oint_{c_3} \mathfrak{S} = k\,(\pm h) \ ; \text{ for E-M field } \mathfrak{S} = \mathcal{A}\wedge\mathfrak{G} \qquad \text{(impair)} \qquad 15$$

n, s and $k = 0,1,2,3,4,.......$

In the integrals of Eqs.12-15, the symbols n, s and k are integers, the symbol $\pm$ reflects on the "impair-polarity" of such residues as $\pm e$ and $\pm h$.

Impair forms[7] are here denoted by using a Venice font of script symbols: $\mathfrak{G}$, $\mathfrak{S}$, $\mathfrak{C}$.

It is important to point out how the exterior derivatives of the differential forms A, $\mathfrak{G}$ and $\mathfrak{S}$ give a set of 2, 3, and 4-forms respectively, which have familiar roles in physics:

The Gaugeable Fields $A, \mathfrak{G}, \mathfrak{S}$		
$dA = F$;	the 2-form of the electric field **E** and magnetic induction **B**.	16
$d\mathfrak{G} = \mathfrak{C}$;	the 3-form of charge density ρ and current density **j**.	17
$d\mathfrak{S} = \mathfrak{L}$;	this 4-form $\mathfrak{L}$ is a pure divergence Lagrangian density.	18

Eq.16 is the familiar definition property of the vector potential, Eq.17 are the matter related Maxwell equations, Eq.18 may have only a latent familiarity, yet in practice many or most of the Lagrangians used in physics are pure divergences.[12]

When taken in a global sense, the Eqs.16-18 imply, according to the theorem by de Rham,[7] that the new differential forms $F, \mathfrak{C}$, $\mathfrak{L}$ are "exact," which means, they have only zero periods:

Global Conservation Laws

$\oint_{c_2} F = 0$;	for all c_2 ; global flux conservation	(pair)	19
$\oint_{c_3} \mathfrak{C} = 0$;	for all c_3 ; global charge conservation	(impair)	20
$\oint_{c_4} \mathfrak{L} = 0$;	for "all" $c_4 = M_4$; *i.e.*, if spacetime is cyclic.	(impair)	21

The properties of Eqs.19-21 are to be distinguished from the properties of the differential forms A, $\mathfrak{G}$, $\mathfrak{S}$, as displayed by Eqs.12-15. The latter are called "closed," because their exterior derivative only vanishes in specific domains with the result that not all their cyclic integrals vanish.

The nonzero periods (*i.e.*, residues of period integrals) versus the zero periods convey information about the topology of the specific domains where the exterior d vanishes. This method of probing topology is called "de Rham cohomology." In mathematics, it is not customary for closed differential forms to distinguish between domains of zero and nonzero exterior derivative; nonzero domains are a "holes" in the manifold!

In physics, by contrast, it is essential to give attention to the physical causes that make the hole act as a topological obstruction. Here we arrive at one of the major challenges of a period integral use in physics. *One cannot* *expect an investigation of the topology of physical structures to be a carbon copy of investigations of the topology of abstract mathematical manifolds.*

From this point on, it will be necessary to carefully probe physical situations so as to get an intuitive feeling of how to proceed, and then, from time to time, to call attention to the mathematical-physical parallelism. In so doing, it is well to keep in mind that the mathematical concept of topology in its most fundamental form, derives, after all, from the perceived physical existence of boundary separations, enabling us to speak of inside and outside domains (*i.e.,* the Jordan curve separates in two dimensions; in higher dimensions it is the Jordan-Brouwer hyper-surface). Through the years, mathematics has had a preference for using static geometric metaphors to illustrating topology. In physics, kinematic illustrations are needed to account for the dynamic nature of spacetime.

The point of primary interest is now to identify a physical "entity," which separates domains in what is believed to be the arena of physical experiences. Is it three dimensional physical space, or is it the celebrated spacetime of Minkowski? In the light of the preceding discussions, a spacetime basis is favored, because all the invariant residues of the period integrals with which we have become acquainted, *i.e.*, charge e, flux h/e, and (action integrated) spin-angular momentum h are known to be proven, metric-independent, general invariants in spacetime.

What are the spacetime entities eligible as domain separators? In spacetime, it would have to have a three-dimensional connotation. Electric charge is a major candidate. The Faraday cage effect and the Meissner effect of superconductivity both testify already to the macro-ability of charge and current to function as spatial domain-separators.

To be a domain separator in the submicroscopic domain elementary charge would have to be a dynamic 3-cycle in spacetime. *The word "dynamic" refers here to the inescapable spacetime connotation of a three-form of charge.* It is difficult to avoid here an explicit involvement with time. A cycle c_3 can only close in spacetime. A static "ball" or "sphere" of charge, unsupported by Faraday's metallic cage, will be discussed a little later. It is a rare manifestation, but perhaps possible in ball-lightning.

The elevation of elementary electric charge as a basic cause and generator of physical topological structure in Nature is, of course, a far-reaching step, the viability of which can only be judged on the basis of its consequences. Charge seems to be either an explicit or an implicit constituent of almost all rest-mass carrying elementary particles. The neutral pion, which falls apart in photons only, seems, at this point, an exception, unless one notes that photons can create electron-positron pairs. The electron trefoil, to be discussed later, is, so far, the only explicit example which reveals how particle properties can be tied together with the help of a topological model. The large number of mostly unstable elementary particles thus provides a real challenge for topology-based conceptual experimentation in this arena. Let us examine in more detail some known situations.

At this point, we have three period integrals that have all the appearances of being choice instruments for topological probing. What exactly is it they probe? The geometric realizations of topological models in three dimensions can be conveniently simulated with appropriately carved pieces of wood, steel wire and paper. Yet when dealing with configurations created by Mother Nature herself, the initiative for providing the configuration is Hers, not ours! Since physical objects exist in spacetime, visualization is a step more difficult than in three dimensions. In fact, just visualization is not enough. Real-world things have a way of evolving in time; they may be stationary for certain and sometimes very long intervals of time, but then they change. In the elementary particle domain, these changes can take place very fast (*i.e.*, these events are very localized in spacetime). *This kinematic backdrop for topological probing is related, yet, in principle, very different from the static geometrically oriented backdrop that has been customary in mathematics.*

In the next section, these matters will be somewhat intuitively approached by using physical examples. There are, however, some general directives that can be helpful to keep in mind. A major function of the model used for backdrop is one of visualizing how the object in question occupies space and, as frequently happens, how it separates space in inner and outer domains. For the geometric model, this separation usually is accomplished with the help of paper. For physical objects, governed by the three period integrals, the "separation" must presumably have something to do with the physical fields that happen to define the differential forms of these period integrals. So what is it that could conceivably separate these definition domains of physical fields?

An indication of how these separations physically come about can be obtained from the already-mentioned familiar and traditional macroscopic three-dimensional configurations. A classical static example is the Faraday cage. Inside the cage a, nonzero, although constant potential field can exist; yet, since the gradient of this constant vanishes, the electric field inside is zero, whereas outside it is definitely nonzero. Hence, more abstractly, by making things independent of the cage material, a charge sheath acts as separator of inner and outer domains. In fact, this picture may not even be all that abstract, if we realize that ball-lightning could conceivably be envisioned as a cage, consisting of a plasma sheath held together by Casimir forces.

A dynamic example of a domain separator is the "skin" of a superconductor. Consider a superconducting ring with circulating current. The current sheath of the ring is known to be a perfect separator of inner and outer domains. The London equations show the vector potential inside the ring to be different from zero, yet its curl vanishes; so inside the electric field and the magnetic induction are zero. However, in the outer domain, one finds a nonzero magnetic induction and, if the ring has an excess electric charge, a nonzero electric field.

These concepts are now to be tested on macro- as well as on microphysical examples. Microphysically, one postulates the existence of charge-sheath separators, even for elementary charge. Interestingly, applications of period integrals hinge on the micro-physical field-free interiors of the particles involved.

6. Physical Gauge and de Rham Theory

The distinction between closed and exact forms critically relates to several aspects of what is known as "gauge theory in physics. A few words are in order to delineate the situation. The outcome of this comparative discussion will be found to strongly favor a physics-based de Rham approach as a natural successor to the more traditional physics-based gauge discussions. It should be mentioned, though, right from the start, that this de Rham-based development remains incompatible with the more recent non-Abelian gauge theory. This is exactly the reason why a discussion of this aspect is here unavoidable.

An awareness of gauge in physics first surfaced with the introduction of the vector potential as a mathematical expedient in electromagnetic theory. Locally the vector potential could only be defined modulo a pure gradient field. The family of arbitrary gradient additives was referred to as a set of permissible gauge changes of the vector potential. Since the set has closure properties, it is customary to speak of a gauge group. The gauge groups here referred to are strictly Abelian.

The local indefiniteness of these gaugeable quantities led to speculations as to whether one gauge would be physically more important than others. All this local indefiniteness, though, remains globally inconsequential for cyclic integrals of those gaugeable quantities, because cyclic integrals of exact parts vanish. In the course of time, this local indefiniteness of the vector potential was extended by acts of theoretical experimentation, by enlarging the Abelian gauge group to an non-Abelian group.

Prior, though, to the emergence of the gaugeable vector potential, the Gauss and Ampère laws of electromagnetic theory already had manifested earlier evidence of local indefiniteness similar to that of the vector potential. The integrands of these integral laws permitted "additives" that were divergence-free and curl-free respectively. Here the traditional free-space constitutive field identification $\mathsf{D} \to \mathsf{E}$ and $\mathsf{H} \to \mathsf{B}$, though, provided an opportunity to fix the gauge in a physically useful way. The fields D and H

so appeared more tangible as measurable local quantities than the more gauge-evasive vector potential **A**.

The expression for a charged particle's field momentum e**A** holds some promise to similarly fix a gauge for **A**. Yet, uncertainty as to whether particles can be purely electromagnetic in origin has, so far, been standing in the way of an equally conclusive gauging of the vector potential.

It is well known that the questions as to whether the fields **A,D,H** are locally measurable quantities have perennially plagued physics. Since their local measurability is contingent on the choice of a physically meaningful gauge, this question for a unique gauge remains the key to this predicament. The situation has, so far, not much of a prospect for a resolution.

A prime reason for this seemingly unsatisfactory state of affairs has to do with a predilection in physics for placing undue emphasis on locally based interpretations, even when an unbiased observation of experiment suggest otherwise. The truth of the matter is that some fields, say, **E, B** and the associated Lorentz force, permit an unambiguous local assessment, whereas others, such as **A,D,H,** definitely don't! For the latter, only cyclic integrals acquire a well defined meaning in terms of quanta of flux and electric charge.

Having gone through some of the mathematical tooling associated with de Rham theory in the previous sections, it should now not come as a surprise that the distinction between **exact and closed** is, or should be, at the conceptual root of **physical gauge.** The overemphasized and unduly heralded distinctions between classical and nonclassical also find an unexpected common ground in the concept of period integration that emerges from the de Rham theorems. Period integration naturally accommodates quantization, yet, in view of the purely additive features invoked by de Rham theory, it stops short of accommodating any form of non-Abelian gauge. Here are some major mathematical and physical reasons justifying reservations with respect to non-Abelian gauge theory.

Mathematically, de Rham's theorems become inoperative in the non-Abelian context; they simply cease to exist. It is not at all clear whether it would be meaningful to look for a non-Abelian analogue of these theorems. These mathematical reservations indicate a forfeiting of the exclusive global perspectives associated with Abelian gauge. These points are conveniently ignored in most discussions of non-Abelian gauge.

Physically, non-Abelian gauge arguments frequently have been used in conjunction with an hypothesized concept of magnetic charge; they are also known as magnetic monopoles of such charge. Yet, from an Abelian point of view, magnetic charge has to be ruled out as a realistic physical concept, because the 2-form F, defined by the fields **E** and **B**, can only be exact. The reasons for this exactness follow directly from de Rham's second theorem, because this 2-form F is the exterior derivative of the one-form A

defined by scalar- and vector potential. Since a use of de Rham's theorem is contingent on a global connotation of A, it should now be mentioned that a global definition domain of A is physically extremely well supported by the many applications of the Aharonov-Bohm law. Conclusion: here we run into a contradiction between Abelian and non-Abelian gauge.

Some of the magnetic monopole extremists are quite willing to deny the vector potential a global domain of definition, in which case de Rham's second theorem would become inoperative. It is necessary, however, to weigh the implication of such a desperate act of rescuing the magnetic monopole hypothesis. As already mentioned, the Aharonov-Bohm effects vividly emphasize a global relevance of the one-form A. In addition, the most exhaustive searches for the hypothesized monopole residues of the 2-form F have all been negative. In the face of all this incontrovertible evidence, it would not be wise to sacrifice a globally exact F for a nonexact (closed) F? The mutual exclusiveness of the here-depicted alternative does not come to the fore, neither in the traditional classical Abelian treatment nor in the nonclassical non-Abelian treatment, because both neglect the local-global distinctions that are the conceptual hallmark of de Rham theory.

Let us summarize these conclusions:

> The fields **E** and **B** (which define the exact spacetime pair 2-form F) can be given precise local meaning as measurable quantities through the Lorentz force. The fields **A** and {**D**; **H**}(which define the closed spacetime pair one-form A, and the closed spacetime impair 2-form $\mathfrak{G}$) are, by contrast, not in general locally measurable; they are defined modulo exact parts. Yet, the cyclic integrals of $\mathsf{A} = (V, \mathbf{A})$ and $\mathfrak{G} = (\mathbf{D}, \mathbf{H})$ are globally measurable quantities that are expressible in terms of Nature's natural quanta of flux, electric charge, and action. The latter includes orbital and spinorial integrated angular momentum.

7. Action Extrema and Action Conservation

The odyssey in the realm of period integrals has made us acquainted with two major aspects of conservation. They are the local and the global aspects of conservation. The local conservation is mathematically conveyed by a vanishing exterior derivative, which is merely an invariant transcription of the familiar continuity equations of physics, which usually invoke divergence expressions. Global conservation, by contrast, is conveyed by everywhere vanishing cyclic integrals. Global and local conservation tend to go hand in hand, in the sense that global conservation indeed implies local conservation. Yet, local conservation cannot, in general, be called upon to infer global conservation. Hence, global conservation is the stronger of the two statements. A closed form may have domains with everywhere vanishing exterior derivative; yet this fact does not permit an inference of universal global conservation. The nonzero residues testify to a conditional conservation associated with closed forms. Exactness, by contrast, testifies to a universal absence of field sources, everywhere in the spacetime universe. Hence, exactness, as an expression of global conservation, is the more encompassing criterion of conservation.

The here-given criteria for universal conservation work fine for the familiar conservation of electric charge, which is expressed by the exactness of the 3-form C. It is slightly less common to speak of the conservation of flux, because it requires lumping together magnetic and electric flux. Yet seen in this context, Faraday's law of induction is to be regarded as an expression of universal flux conservation, as expressed by the exactness of the 2-form F. The proposition of magnetic monopoles clearly would make F a closed form, which is no longer globally derivable from a one-form A. While flux and charge relate to the exactness of 2 and 3-forms, one may now ask whether any meaningful physical conservation is known relating to 1- and 4-forms.

An exact one-form is everywhere derivable from a spacetime scalar permeating all of the universe. However, this spacetime scalar is only defined modulo an exact part. This exact part has a vanishing exterior derivative (4-dimensional gradient); its gauge thus becomes a constant of integration. An inadequate example of an exact one-form is the energy-momentum vector, which in Hamilton-Jacobi theory is taken to be derivable from a spacetime scalar. Physical limitations of this H-J process are discussed in the next section.

The exact 4-form in spacetime has other features, which reveal a new aspect of global conservation without a local counter part. An exact 4-form would have to be the exterior derivative of a 3-form. The Lagrangian 4-form of action, as given by Eq.18 of section 3, is a striking example. This 4-form

$$\mathcal{L} = d(\mathsf{A}\wedge\mathsf{G}) = \mathsf{F}\wedge\mathsf{G} + \mathsf{A}\wedge\mathsf{C} \qquad 22$$

meets the requirements of exactness, which according to the general rules would translate into global conservation. Yet, the corresponding integral expression of this conservation can only be made explicit for the totality of a spacetime M_4, which for this purpose would have to be cyclic. A conservation statement, which is necessarily extended over the totality of a cyclic spacetime, is indeed of limited local usefulness.

A complication for local action conservation is that the exterior derivative of a 4-form defined in a four-dimensional space is trivially zero: $d\mathcal{L} \equiv 0$. The reason is that permutations of four variables over five positions necessarily invoke a *repeat* of at least one variable. This *repeat* reduces an antisymmetric quantity identically to zero. Hence, *there is no realistic prospect of any usable form of local action conservation*, whereas *a universal formulation of a global action conservation remains very impractical.*

The just-stated conclusion reflects on the absence of a consensus in physics on whether or not there is such a thing as action conservation. There are meaningful expressions for flux and charge conservation. Mindful of the impossibility of local-action conservation, the question is whether conditional statements of global conservation are more practical than statements invoking the whole universe. Here is a possibility:

The period integrals of $\mathfrak{S}$ open up a possibility of singling out so-called action isolated (or perhaps we should say action-adiabatic) global systems. If $\mathfrak{S}$ is of the form given by

$$\mathfrak{S}=A\wedge\mathfrak{G}, \tag{23}$$

the period integral Eq. 15 of this chapter can be optionally regarded as an isolated case of global action conservation that is "locally" specified through the cycle c_3:

A Theorem of Action Adiabaticity

The action residues of the integral

$$\oint_{c_3}(A\wedge\mathfrak{G}) = kh, \text{ with } k=1,2\dots, \tag{24}$$

selected by the cycles c_3, represent globally conserved action.

Eq. 24 is, of course, identical with Eq.15 of section 3 of this chapter. They constitute, by virtue of this fact, a legitimate statement of a semi-globally conserved action, such as selected by the cyclic-integration domain c_3 pertaining to the physical configuration that is being considered. Through the specifics of the selection of the residue cycle c_3, action conservation in the form of Eq.23 has acquired a pronounced system related connotation. There is no longer a universal global conservation that holds for all cycles, such as in the cases of flux and charge; instead there is a system-related conditional global conservation. A realistic physical example of such a system-based semi-global conservation is the current carrying superconducting ring discussed in the next chapter.

If $\mathfrak{L}$ is not of the form of Eq.22, no statements about a system-related action conservation can be made. Instead, the system is then in an action exchange with its environment, and may be said to seek an action extremum adjustment. The ensuing *variational Lagrangian*, written in the now more familiar tensor form, is:

$$\mathfrak{L}=\frac{1}{4}F_{\lambda\nu}\mathfrak{G}^{\lambda\nu} + A_\nu\mathfrak{C}^\nu . \tag{25}$$

The Lagrangians of Eqs. 22 and 25 may look alike, yet the factor 1/2 makes a significant difference. Unlike Eq.22, the Lagrangian of Eq.25 is not the exterior derivative of a 3-form. In tensor language it says the Lagrangian of Eq.25 is not the divergence of the contravariant 4-vector density $A_\nu \mathfrak{G}^{\lambda\nu}$, which defines the 3-form $A \wedge \mathfrak{G}$. Lagrangians of the form of Eq.25 are relevant for obtaining equations of motion of charges moving in an external **B** field, instead of in an internally produced mutual **B** field of a superconducting ring.

If $\mathfrak{L}$ is a pure divergence, Stokes' theorem would make the variational process trivial, such as is the case for the $\mathfrak{L}$ defined by Eq.22. In fact many Lagrangians used in physics are of the trivial type. The Lagrangian of the free-space electromagnetic field is an example; even so the variational derivative still retains its usefulness for obtaining the d'Alembert wave equation. We are thus confronted with two types of Lagrangians, which should be given different names. They are:

I The action conservation Lagrangian defined by Eq.22.

II The action variational Lagrangian defined by Eq.25.

Cases I and II mutually exclude one another. They lead to the following alternative:[12]

Alternative of Action Variation versus Conservation

If case **I** (Eq.22) prevails, the variational process becomes trivial and action is semi-globally conserved. If case **II** (Eq.25) prevails, the variational process is not trivial. Action now seeks an extremum adjustment. Hence, action balance takes the place of action conservation in case **II.**

Case I can occur only if the sources of all (external) fields can be accounted for in terms of residues of S, so as to make them internal fields. The choice between between cases I and II is in some ways a matter of practicality. Recall hereto the example in chapter VII of Larmor circulation, which can be seen as one half of the electrons performing cyclotron orbits in the field of the other half.

8. A de Rham Perspective on Schroediger's Equation

More than most other pioneers of quantum mechanics, Schroedinger remained skeptical about the interpretations that were beginning to emerge in the early thirties. While he was quite forthcoming and verbal about his reservations, he did not succeed in convincing his fellow physicists to share his doubts. Schroedinger was concerned about an unreasonable aura of miracle with which the Copenhagen School was beginning to surround the newly founded formalism. Today, several decades later, it can be said that de Rham's theorems on closed and exact forms hold some good promise to substantiate and clarify Schroedinger's own doubts about his own miracle.

At the time of the exchange between Schroedinger and the Copenhagen School, the theorems of de Rham were just beginning to establish a position of importance in the world of mathematics. There is a subtlety of concept in these theorems, because they initiate a new phase of mathematical awareness concerning the use of local and global aspects.

De Rham's theorems are, if you will, a natural dimensional extension of the Cauchy theorems of complex analysis. From the two-dimensional complex domain, the concepts of analyticity are extended to real manifolds of arbitrary dimension with the help of the concepts of closed and exact. If complex variable theory initiated a concern about the topology of singularities, de Rham theory similarly initiated a concern about the topology of the domains of support for the closed and exact properties of differential forms. Note that the point-singularities of complex variable theory can, in the de Rham case, assume finite manifestations covering extended domains.

Even half a century after their inception, de Rham's theorems have not yet been incorporated as an integral part of mathematical physics. The stumbling block has been an uncertainty in conclusively identifying the physical nature of spacetime's topological obstructions separating analytically distinct domains. Yet some of the very ideas that led to these theorems derive from physics. Let us examine here how de Rham's theorems directly affect Schroedinger's transcription that led him to his wave equation by starting from Hamilton-Jacobi theory.

Schroedinger used the transcription of (Hamilton) de Broglie

$$e^{iW(q^\lambda,t)/\hbar} \rightarrow \psi(q^\lambda,t)\,, \tag{26}$$

in which ψ is the Schroedinger wave function and W is the Hamilton-Jacobi action, both as functions of space coordinates q^λ $(\lambda=1,2,3..)$ and time t. Schroedinger arrived at his wave equation by taking the variational derivative of the Hamiltonian written as a function of ψ and its derivatives.

The essence of the now following arguments centers around the precise specifications concerning the global nature of the action function W, which in turn affects the global properties of the wave function ψ. The de Rham theorems are essential for reassessing the mathematical conscience about W and ψ. For this purpose, it suffices to take $\hbar=1$.

In general, neither W nor ψ can be expected to be single-valued. Schroedinger's favor of fortune, as Weyl called it, is the miracle of a procedure, which by imposing global conditions of square integrability and single-valuedness on ψ yields important physical results.

Let us examine from an angle of de Rham theory the (one-particle) Hamilton-Jacobi equation of analytic dynamics:

$$\frac{\partial W}{\partial t} = H(p,q,t)\,,\ \text{with}\ p=-\operatorname{grad}W\,, \tag{27}$$

the wave function ψ is predicated by the existence of a spacetime scalar function W satisfying Eq.27. An indication of the local existence of W is provided by the Hamilton equations of motion, because they ascertain the local integrability of the Pfaffian defined by the spacetime energy-momentum one-form (compare Eqs. 9 and 10 of section 3 of this chapter) :

$$R = dW = Hdt - p_\lambda dq^\lambda \,. \qquad 28$$

Eq.28 depicts this differential form as the exterior derivative of the spacetime scalar function W.

It suffices, for the sake of this argument, if a single particle situation is considered, with summation over $\lambda=1,2,3.$, because it facilitates a spacetime physical identification. For *point-particles,* H and p_λ appear physically as components of a spacetime four-vector "field"; they are not just defined on a given trajectory. This "field" can be strung together into families of world-line trajectories. Since distinct trajectories intersect at a single the field W must be multi-valued and so are p_λ.

If Eq.28 is taken in the global sense, which is a prerequisite for applying de Rham's second theorem, it follows that the energy-momentum one-form would have to be exact. This exactness, in the de Rham sense, implies according to the same theorem that all cyclic integrals of R vanish:

$$\oint_{c_1} R = 0 \text{ for all } c_1 \,. \qquad 29$$

Yet physical experiences from the classical, as well as quantum aspects, tell us that Eq.29 cannot be unconditionally met. The Bohr-Sommerfeld integrals

$$\oint R \rightarrow \int p dq = nh \;;\; n=1,2,.... \qquad 30$$

provide tangible and familiar evidence that Eq.29 can't possibly vanish for all cycles c_1. The contradiction between Eqs.29 and 30 translates into a conclusion that the action 1-form is closed but not exact (*i.e.,* dR=0, but Eq.29 does not hold for all c_1). The latter fact automatically activates de Rham's first theorem for those cycles for which Eq.29 does not vanish. By the same token it deactivates de Rham's second theorem.

It follows that the W function of Hamilton-Jacobi does not meet the condition of single-valuedness required by de Rham's second theorem. Hence there is no single-valued W, such that the energy-momentum one-form is everywhere defined by dW. From a global point of view, one should also consider that **a closed form, unlike an exact form, is only determined modulo an exact part!**

Without a single-valued W, the chances of having a single-valued ψ are not very good. The question is whether imposing conditions of single-valuedness on ψ is possible at all. The saving grace for rescue from this predicament is truly in the "wave" nature of the de Broglie-inspired transcription recipe of Eq.26, which was used by Schroedinger. This transcription permits the conversion of an inherently multi-valued W into an optionally single-valued Ψ. The recipe of square integrability and single-valuedness indeed singles out the optionally single-valued solutions corresponding to unique quantum states.

Seen in this perspective, it is an amazing but pleasant surprise that the Schroedinger process is capable of retrieving earlier results resembling the Bohr-Sommerfeld process. First we saw that the Bohr-Sommerfeld integrals were incompatible with a globally defined single-valued action function W. Then we attempt to conceal this multi-valuedness with the help of the transcription of Eq.26, and subsequently we end up finding an optionally selectable single-valuedness of Ψ, which, amazingly, reproduces Bohr-Sommerfeld results, at least in an asymptotic sense (compare hereto the WBK approximation).

What is the physical implication of that Schroedinger detour for obtaining those slightly different quantum states? Before an answer to this question is attempted, let us just rephrase the procedures that have been here described from an unconventional angle. It is necessary to give the convolutedness of the scheme a chance for sinking in.

From the Hamilton-Jacobi angle, an assigning of nonzero values to certain action integrals $\oint \mathsf{A}$ corresponds to selecting families of solutions with specific physical properties. An example is the family of elliptical orbits for planetary motion covering the annular-shaped orbital manifold by giving the integrals $\oint \mathsf{A}$ fixed values. A continuous change of the values assigned to these integrals $\oint \mathsf{A}$ selects a continuum of new orbital families. When, instead, the Bohr-Sommerfeld quantum conditions are imposed on these integrals, the continuum of orbital families now shrinks to a discrete set of orbital families.

Yet each of these discrete quantum families still has a continuum of orbital options. For the planetary motion it is the choice of orientation of orbital plane and the position of orbital perihelion. For the harmonic oscillator it is orientation and time of zero amplitude or phase, which was exactly the object of Planck's averaging process that led him to the introduction of zero-point energy (see chapter III; 5).

The next point of concern involves the transcription of Eq.26. It brings the multi-valued nature of the action function W in a latent state. Schroedinger's recipe of single-valuedness for the wave function Ψ retrieves asymptotically the Bohr-Sommerfeld quantum discreteness. ***The statistical implications of Ψ now indicate a family averaging over the still-available phases and orientations of each family of orbits compatible with the given quantum state.*** Schroedinger's "detour" is thus shown to bring about a phase averaging, which is exactly what led Planck to the need for an ensemble-based zero-point energy.

The presented delineation between Bohr-Sommerfeld and Schroedinger procedures becomes, in fact, an attempt at a synthesis-oriented counterpart of the more traditional analytic wave→ray asymptotics that goes back to Hamilton. While the wave→ray procedure favors the Schroedinger process as more fundamental, the de Rham-based synthesis, by contrast, gives no reason to support that conjecture. Instead, it reinstates fundamental qualities of the Bohr-Sommerfeld process.

It is now possible to gain new insight into the physical implications of the Schroedinger process. Since the the Hamilton-Jacobi action function $\mathfrak{w}$, as specified by the B-S integrals, represents a family of equivalent quantum orbits. By virtue of the transcription of Eq.1, the wave function ψ, also represents that family of selected orbits, which leads to a

Proposition of Equivalence

The variational process used by Schroedinger on a Hamilton-Jacobi-type Lagrangian translates into a statistical equilibrium in a family of Hamilton-Jacobi orbits that are equivalent from a Bohr-Sommerfeld angle.

In other words: ***the Copenhagen suggestion of fuzzy orbits is herewith replaced by a statistical ensemble of actually conceivable orbits.*** Hence, the next question has to be: What are the options of physical interpretation for those Hamilton-Jacobi families? Since each element of these orbital families is known to represent a physically possible dynamic situation from a so-called classical angle, there really is no longer an adequate reason to hide behind an indeterminate nonclassical picture. The one and only remaining nonclassical quantum feature is the discreteness of orbital families. So abandoning the nonclassical fuzzy orbit escape, we now have only two major possibilities of interpretation:

I: The "Classical" Copenhagen Option

The B-S discrete families of H-J solutions can be thought of as ensembles of conceivable manifestations of one-and-the-same physical system in the sense of Gibbs. This would be the single-system option of the Copenhagen interpretation, yet, without a need for calling on a nonclassical statistics.

II: The Ensemble Option

The other possibility is to consider each B-S family of H-J solutions as an actual physical ensemble, whereby each orbit of the family represents the performance of a distinct physical element of an ensemble of identical physical elements.

Interpretation II was suggested by Popper in 1934; it is known as the statistical or ensemble interpretation. Seen from this common Hamilton-Jacobi origin of I and II, the Copenhagen interpretation visualizes the H-J orbital manifold in the vein of Gibbs' abstract ensemble of conceivable manifestations of one and the same single system. It explains, if you will, why many contemporary discussions indeed vacillate between physical ensemble and single-system views.

Since this quanta reprogramming has accumulated evidence, supporting an ensemble view of the Schroedinger process, it is now opportune to mention that Gibbs' single system abstraction was an artifact substituting *system manifestation plurality* for *actual system plurality*. The statistics for cases I and II are the same and perfectly classical, because their universe of discourse is the same; they are the phase-and-orientation parameters in the orbital manifold determined by the Hamilton-Jacobi process. The Gibbs artifact of conceivable manifestations is now responsible for Copenhagen's single-system extrapolation with the single system as a carrier of a universal always-present zero-point energy. The physical viability of the Copenhagen view has reached here its most vulnerable position.

Schroedinger never denied that his brainchild was a favor of fortune. He maintained the Copenhagen School was rushing into premature judgment. Copenhagen inspired action thus won out by default, because Schroedinger's denunciation was not followed by a counter action that could sway the public opinion in physics away from the Copenhagen path of nonclassical mystique. When initiated by Popper, a small minority propagated ensemble views in the Thirties. Most of this minority silently adhered to many of the Copenhagen nonclassical precepts. It meant the Copenhagen point of view was really never in great danger of being overruled by facts or logic.

Now, however, a de Rham view on the Schroedinger process unambiguously identifies phase and orientation as ensemble parameters. Since these parameters have no intrinsic significance for the dynamical performance of isolated single systems (except for the purpose of establishing initial conditions), it follows that the phase and orientation of a single system can acquire a physical role if, and only if, they partake in nonlocal environmental dynamical exchange; say, with neighbor systems. This evidence shows mutual-phase and mutual-orientation as nonlocal parameters of system-interaction in the ensemble. These findings conclusively rule out any possibility of accepting the Schroedinger process as a single system tool, such as is intimated by the Copenhagen interpretation (option I).

CHAPTER VII

LARMOR AND CYCLOTRON ASPECTS OF FLUX QUANTA

1. Summary

Flux quanta $h/2e$ relate to a system with electrons circulating at the Larmor rate in their self-field. Quanta h/e are linked by an orbital electron motion circulating at the cyclotron rate in an external magnetic field. The Larmor situation corresponds to a seemingly exact 1-form $p=mv-eA$ of energy-momentum with perfect angular momentum balance between mechanical and field angular momentum. The cyclotron motion, by contrast, relates to a closed 1-form p, and invokes an explicit action exchange with the external field environment.

2. Larmor Circulation in Superconducting Rings

When in the early Sixties German and American teams[1] proved the existence of flux quantization in superconducting rings, the size of the observed quanta turned out, surprisingly, to be half of what was expected on the basis of Fritz London's earlier prediction of this phenomenon. A Letter by Onsager,[2] which appeared simultaneously with the letters reporting the experimental verification, related the factor "two" to the electron pairing demanded by the theory of superconductivity.

A vivid account of these events can be found in the last part of the third volume of the Feynman Lectures.[3] The reader will note that Onsager's dictum of that Factor Two elicits a Feynman footnote explaining that only Onsager understood the given argument for justification.

Since neither Onsager nor Feynman are with us any longer to help us out of this controversy, let us proceed by paraphrasing a Shakespearean admonition that perhaps, also in physics, the beauty of understanding can be dominated by the eye of the beholder. The objective of this chapter aims at some further insight into this Onsager-Feynman controversy by looking at the issue as part of a generation gap in physics. Onsager came of age prior to the quantum revolution of the late Twenties, whereas Feynman grew up with modern quantum mechanics as the new gospel of physics.

Let us attempt here to resolve these questions in the spirit of a Bohr-Sommerfeld approach to quantization. Such a semiclassical argument may have been closer to the thinking of Onsager. Over the past half century, an appeal to the earlier tools of quantum mechanics had to be argued with the utmost of care so as not to create a credibility gap. A circumspect treatment will be desirable to avoid opening up a can of worms.

In the light of the preceding chapter, the following simple thoughts may be able to cast some light on this situation. Consider a reasonably slender superconducting ring of radius r. Let s be the number of electrons circulating in this ring, creating a flux through the interior. The cooperative nature of superconductivity suggests that all s electrons circulate in the ring at the same rate. Although theory indicates s to be an even number, let us see, for the sake of argument, whether this conclusion can follow here directly from observation.

The total energy E associated with the superconducting ring situation consists of the sum of the kinetic energy E_k of s circulating electrons and the magnetic energy E_m created by the circulating current

$$E = E_k + E_m \,. \qquad 1$$

If m is the electronic mass and ω the common rate of circulation

$$E_k = (1/2)smr^2\omega^2 \,, \qquad 2$$

and if J is the total ring current and Φ the ring-enclosed flux

$$E_m = (1/2)\Phi J \,. \qquad 3$$

A comparison of the two energy components requires further knowledge of ω. Consider hereto the electron's momentum expression $\mathbf{p}=m\mathbf{v}-e\mathbf{A}$, in which $\mathbf{v} = \boldsymbol{\omega} \times \mathbf{r}$ and $\mathbf{A}$ is the vector potential. Using London's recipe of $\mathrm{curl}\,\mathbf{p} = 0$ as characterizing the superconducting state, and noting that $\mathrm{curl}\,\mathbf{v} = 2\boldsymbol{\omega}$, one obtains for $\boldsymbol{\omega}$ the Larmor frequency:

$$\boldsymbol{\omega} = (e/2m)\mathbf{B} \,, \qquad 4$$

$\mathbf{B} = \mathrm{curl}\,\mathbf{A}$ is the magnetic induction, e is the electronic charge.

An application of Stokes' theorem to the ring configuration leads quite generally to an effective magnitude $B = |\mathbf{B}|$ governed by the relation

$$A = (1/2)\, r\, B \,, \qquad 5$$

A curl-free $\mathbf{p}$ can be gauged to vanish. This leads to $m\mathbf{v} = e\mathbf{A}$, which with the help of Eq.5 and $v = \omega r$ reconfirms Eq.4.

It is now possible to prove that

$$E_k = E_m \,. \qquad 6$$

Let us hereto transcribe the kinetic energy using the flux expression $\Phi = \pi r^2 B$;

$$E_k = s(1/2)mr^2\omega^2 = s(1/2)\pi r^2 B m\omega^2/\pi B = (1/2)\Phi s m\omega^2/\pi B,$$

now using Eq.4 the last part transcribes as

$$E_k = (1/2)\Phi\, se\omega/2\pi = (1/2)\Phi J, \qquad 7$$

because the ring current J is by definition:

$$J = se\omega/2\pi\ . \qquad 8$$

This proves Eq.6, and the total energy may thus be written as

$$E = \Phi J\ . \qquad 9$$

Quantization can now be introduced through the experimentally established formula

$\Phi = n\ h/2e$; (flux quantum h/2e is unit for self-field), 10

where n=1,2,.. is the flux quantum number. Substitution in Eq.9 gives

$$E = (s/2)nh\omega/2\pi = (s/2)\, n\hbar\omega\ . \qquad 11$$

Eq.11 indicates that electrons assume pair-wise the energy $n\hbar\omega$. This conclusion is a direct consequence of the experimentally observed quantization of Eq.10 and therefore confirms Onsager's dictum.

Starting from the angular-momentum expression $L = smr^2\omega$ one now easily finds, with the help of a similar transcription, that

$$L = (s/2)n\hbar\ , \qquad 12$$

again confirming as a consequence of Eqs.4 and 10 that electrons assume pair-wise the value $n\hbar$.

The magnetic moment μ associated with the ring current can now be obtained either by using the experimentally established orbital gyro-magnetic ratio e/2m, or by a transcription of the formula $\mu = \pi r^2 J$ with the help Eqs.4,8,10 and 12, both leading to

$$\mu = (s/2)n(e/2m)\hbar, \qquad 13$$

i.e., a magnetic moment that is a multiple n of the magneton unit $(e/2m)\hbar$ per electron pair.

Hence, as a consequence of the experimentally observed flux quanta $h/2e$, as given by Eq.10, both ring energy and magnetic moment give unified testimony of the electron pairing in the superconducting process.

Having established the basic feature of the electron pairing as a consequence of experimental observation and the Bohr-Sommerfeld conditions, one could, in retrospect, say that one-half of the electrons is circulating in the field generated by the other half. Since one-half of the electrons generate only half the B field, the situation can now be summarized as follows: ***with both halves of the electrons circulating at the Larmor rate in the total B field, each half may be said to perform a cyclotron circulation in the B field generated by the other half.*** The Onsager pairing thus provides added insight into the relation between Larmor and cyclotron frequencies.

3. Angular Momentum Balance in Superconducting Rings

How is angular momentum conserved in superconducting rings? If an increase in temperature kills the supercurrent in a ring, where does the momentum of the charge carriers go? A tempting answer is that the electrons start colliding with the lattice of the ring material, thus causing a transfer of a conceivably observable momentum to the material of the ring.

There is, however, another mechanism that could well prevail over the just-mentioned collision process. As soon as the charge carrier motion slows down, the ensuing change in magnetic flux produces an electromotive force, counteracting the retardation of the motion. Moreover, an opposite torque acts on the opposite charges of the lattice. It thus follows that the angular motion of the charge carriers exchanges momentum with the magnetic field it generates. It is reasonable to endow the surrounding magnetic field with a magnitude of angular momentum. It also seems reasonable to assume that this field momentum will vanish simultaneously with the mechanical momentum if the charge carriers come to rest. *So, how much of the momentum is exchanged with the lattice of the superconductor? and how much is exchanged with the self-field of the ring?*

Here is a good opportunity to test Eq.15 of chapter VI in a macroscopic situation. Eq.7 of chapter VIII gives an application in the submicroscopic domain, let us try here an application in the macro- or, better, the mesoscopic domain. Since the field formula is supposed to apply for orbital as well as spin angular-momentum, let us write L_f for this "orbitally"-related field momentum and use the flux unit h/2e instead of h/e for its evaluation. One thus obtains via Eqs.12,14 and 15 of chapter VI and Kuennetz rule:

$$L_f = \oint_{c_1} A \oint_{c_2} \mathfrak{G} = n(h/2e)se = (s/2)nh, \qquad 12a$$

which is to be compared with the L of Eq.12 for the mechanical of the charge carriers. It follows that $L_f = 2\pi L$. The factor 2π is characteristic of orbital integration over c_1, as distinguished from a spinorial 4π double loop circulation encountered for the trefoil electron model in chapter VIII.

The 2π thus stems from the customary differential definition of orbital angular momentum, $p_\phi = mr^2\omega$, *which is an action prior to the loop integration.* Hence,

$$L_f = \int_0^{2\pi} p_\phi d\phi$$

equals the angle-integrated mechanical angular momentum.

Eqs.12 and 12a say that the action integral of the mechanical angular momentum equals the field angular momentum, because the latter is already in the action-integrated form. So going through the transition temperature energy is not conserved, but action is globally conserved between charge carriers and the field they generate. The lattice has no role as a buffer.

4. Flux Quanta Linked by Cyclotron Orbits

Now, by contrast, a single electron circulating in an external magnetic field is, strictly speaking, not a closed system, even if the situation, as depicted, is conservative. According to Landau, its quantized energy states change in discrete orbital steps $\hbar\omega$ per single electron. Higher quantum states thus correspond to increased orbital radii r, with accordingly increased kinetic energy. The question can be asked: how much flux of the external magnetic field is linked by a given cyclotron orbit?

The following Bohr-Sommerfeld-based argument can provide an answer to this question. If, in the previous case, the cyclic integral of the one-form defined by the momentum p was taken to vanish: $\oint p = 0$, this conclusion no longer holds when the electron is circulating in an external field. Instead, one has for the circular orbit:

$$\oint p_\phi d\phi = \oint m\omega r^2 d\phi - \oint eArd\phi = nh, \quad n=1,2,\dots . \qquad 14$$

Since the equation of motion in a field B now yields for ω a circulation at the rate of the cyclotron frequency:

$$\omega = (e/m)\,B\,, \qquad 15$$

an evaluation of Eq.14, with the help of Eqs.5 and 15, gives

$$2\pi r^2 Be - \pi r^2 Be = \pi r^2 Be = nh.$$

In other words, the flux intercepted by an electron orbit circulating in an external field B is quantized according to:

$$\Phi = nh/e \text{ ; } (h/e \text{ flux unit for external field) } n=1,2,\,.. \qquad 16$$

The result of Eq.16 is obtainable from the Landau states, provided the zero-point energy is taken not to contribute to the orbital radius. The orbital flux interception in the quantum Hall effect obeys Eq.16. There is, as shown in chapter III; 6, conclusive experimental evidence to justify the absence of a zero-point term in Eqs.10 and 16. Eq.16 is the key to an extremely simple description of the fractional quantum Hall effect.[4]

What is the message of these simple considerations? It seems that the Bohr-Sommerfeld procedure has, in the present context, a potentially unique feature, which cannot quite be duplicated by quantum mechanics in its current form. The Bohr-Sommerfeld method, in conjunction with Hamilton-Jacobi theory, imposes on us greater awareness of a physical delineation between *closed* and *exact* differential forms. Only in ad hoc situations has gauge theory so far awakened to this distinction. There is as yet in physics no systematic incorporation of these concepts.

Mathematically, closed and exact forms are locally indistinguishable. They only differ globally in that an exact form has always-vanishing cyclic integrals, whereas the closed form can have nonzero cyclic integrals. The postulated existence of an action function W from which p is derivable, with W obeying the Hamilton-Jacobi partial differential equation, makes p exact according to a theorem by de Rham.[5] The self-field situation of the superconducting ring meets this condition of exactness for p by virtue of London's equations for superconductivity. Yet the electron orbiting in an external field does not meet that requirement, because now there is a $\oint p \neq 0$; yet a W still locally exists. Within those constraints, the Hamilton-Jacobi process and the Bohr-Sommerfeld quantization have proven to still retain a measure of relevance (*e.g.,* Sommerfeld's analysis of the hydrogen fine-structure in chapter X).

The standard "partial" rationale leading to the Schroedinger equation is, of course, affected by this unresolved status of the one form p between closed and exact. It is not obvious whether this state of indecision between closed and exact can be helpful in maintaining, for the Schroedinger equation, the status of being a fundamental tool of physics.

Offhand, it is unlikely that a wave-equation approach, which is local in nature, would be able to accommodate, at all times, the intrinsically nonlocal characteristics revealed in mesoscopic quamtum situations. The conditions of wave function uniqueness and square integrability, though global in nature, can't be expected to make up for all the lacunar global structure of standard theory.

5. Energy Considerations

It is of interest to note that the quantization $n\hbar\omega$ for the harmonic oscillators in a superconducting ring applies to electron pairs including their magnetic self-energy. The energy E for N electron pairs in the ring is the sum of kinetic energy of the N pairs and the magnetic field energy

$$E = N\, mr^2\omega^2 + (1/2)\Phi J = N\, n\hbar\omega;\ n=1,2,3,\ldots\ldots\ldots \qquad 17$$

Substitution of the current for N pairs $J = N2e\omega/2\pi$ gives

$$E = N\,(mr^2\omega^2 + \Phi e\omega/2\pi).$$

Using the expressions for the Larmor frequency and the quantization of the self-generated flux, there is no problem in confirming Eqs. 1 and 6. Note also that the result is compatible with an equal magnitude mechanical- and field energy per pair of $2mr^2\omega = 2e\Phi/2\pi = n\,\hbar$ (spin terms cancel).

The cyclotron situation can be treated in a similar manner. The kinetic energy for s electrons in cyclotron orbit is

$$E_k = (1/2) s m r^2 \omega^2$$

with an associated magnetic *interaction* energy which does not get the factor 1/2:

$$E_m = \Phi J.$$

For s electrons in the cyclotron orbit $J = se\omega/2\pi$, and now using the expression for the cyclotron frequency it is not hard to show that

$$E_k = E_m = s\, n\hbar\omega.$$

Even s is here demanded by the Pauli principle. Hence, also here the spin terms cancel.

CHAPTER VIII

FITTING PERIOD INTEGRALS TO PHYSICS

1. Summary

The following further illustrates how period integrals produce answers in nonstatistical situations. Since new methods, when they are compared with traditional approaches, always appear apocryphal at first, a measure of reconditioning must be expected. More leisurely discussions follow in chapters IX and XI to show the relation to familiar treatments in physics.

2. The Electron's Anomalous Moment

Mathematics enjoys a privileged position insofar as some of its developments seem to be purely cerebral without an obligatory confrontation with physical reality. Of course, there remain requirements of meeting the reality of logical consistency, which somehow is part of physical reality. The fact is, however, that mathematics can keep coasting on abstractions and their inner consistency much longer than physics.

Many developments in modern topology indeed are living testimony to the veracity of such typical differences between physics and mathematics. Mindful that a topological assessment of physics cannot be just an exercise in the creation of freewheeling abstractions, a frequent confrontation with observed facts is necessary to guide the conceptualization into realistic avenues.

Many of the now-known macroscopic quantum effects have been found to provide a useful realm of investigation, because they have the advantage of something that comes closest to what may be called a visualization. Examples are: flux quantization in superconducting rings, the Josephson effects, different versions of the Aharonov-Bohm effect, the integer and fractional quantized Hall effect, the neutron diffraction experiments in gravitational fields and, more recently, the single particle diffraction experiments.

To the extent that these effects have still been somewhat describable by standard quantum procedures, wave-function uniqueness is found to assume a central role. Since uniqueness relates to wave-function phase, as described by the Aharonov-Bohm integral, questions of how to decide between flux units $h/2e$ and h/e take center stage.

In the previous chapter, self-generated fields in conjunction with electron pairing were found to relate to the period integral status of Eq.12 of chapter VI, whereas a period integral status of Eq.13 of chapter VI was taken to relate to external magnetic fields. An application of the latter though is directly contingent on whether or not the participating particles have a field-free interior. For particles to accumulate in the same quantum state, boson pairing is also here essential.

Since it is not easily possible to probe directly into particle interiors, an indirect way of checking the feasibility of the field-free interior proposition is by testing models that are postulated to have a field-free interior. Hence, the applications of chapter VI's Eqs.12-15 are contingent on particle constitution. All this is to remind us that the macroscopic manifestations of matter are inseparable from its microscopic nature. Since the electron is a major protagonist in almost all situations, an electron model with a field-free interio permitting application of the Aharonov-Bohm period integrals, is next on the agenda. The model should, over and above, account for gyro-magnetic anomaly, and the half-integer differential spin.

Since the electron has a magnetic moment, it is taken to have a flux quantum h/e for the following reason. For the "trefoil" model, to be explained later, the charge $e/2$ moves in the field generated by the other $e/2$, which in the end justifies Eq.13 of chapter VI with n=1 (*i.e.*, chapter VI, Eq.12 for the charge e/2). Furthermore, its electric charge is governed by chapter VI's Eq.14 for s=1, and its (integrated) spin by chapter VI's Eq.15 for k=1 (please note k=1 not k=1/2, as will be discussed shortly).

To produce a flux and corresponding magnetic moment, the model has to be some sort of a ring of circulating charge. A simple ring, which has been tried many times, must be rejected, because it has a spatial center of symmetry, which is not acceptable for a charged particle. To avoid the spatial center of symmetry, and to account for charge polarity as a manifestation of spatial enantiomorphism, the electron ring shall be thought of as a knotted ring, also known as a "trefoil." Similarly, as electron and positron, left and right trefoil are adjoined under orientation-changing spatial transformations. This double ring is visualized in Fig.2 as a tube with a field-free interior; the result of a submicroscopic Meissner effect, if you will. The internal cycle c_1 of the double ring is the integration loop for chapter VI's Eqs.12 and13; it closes after a 4π circulation and not after a 2π circulation. From a spacetime point of view, the "trefoil" is a constant time slice of a Jordan-Brouwer hypersurface of charge.

These are the model specifications that suffice to make at least a first- and second-order calculation of the electron properties. The mass m of the electron is taken to be determined by the volume integrals of the field energies $E\cdot D/2$ and $H\cdot B/2$ and the matter related contributions $A\cdot J/2$ and $\rho V/2$. Standard theory yields (see Eq.9 of chapterVI, or Eq.2, p.66 in ref.2 of chapterVI of this text):

$$mc^2 = \Phi J + eV. \qquad 1$$

The last term in Eq.1 is the electrostatic energy of the familiar Lorentz model with e determined by chapter VI's Eq.14 for s=1. In accordance with the necessarily dynamic nature of elementary charge, the Lorentz term is here augmented with the (much bigger) magnetic energy term ΦJ, in which Φ is the elementary flux linked by the trefoil and determined by chapter VI's Eq.13 for n=1, whereas J is the total ring current.

Recalling the predicament of stabilizing the Lorentz electron with Poincaré stresses, this dynamically extended Lorentz model, by contrast, provides those stresses naturally in the form of the Ampère forces acting between the two halves of the trefoil ring (see chapter XI; 5). There is an asymptotic equilibrium between the repelling Coulomb forces and the attracting Ampère forces if the charge of the tubular charge sheath of the trefoil approaches a circulation speed equal to the vacuum speed of light. Since electric charge belongs in Cartan's pre-metric Maxwellian realm, charge moving at the speed of light does not violate metric-based restrictions, for which the speed of light is an upper bound. Compare this argument with the speed of light argument used for the *Zitterbewegung* in chapter XI; 8. Note also that charge is a primary concept, whereas mass has a derived status via the concept of energy. Mass, unlike charge, is not of a distributed nature, its system-based definition gives it a global property, subject to metric restrictions of the special theory.

Further metric properties can now be injected. For an effective trefoil radius r, the magnetic moment μ of the electron may be represented by the familiar expression:

$$\mu = \pi r^2 J \qquad 2$$

Since, for reasons of model stability, the vacuum speed of light c is favored for the azimuthal motion of the charge sheath circulating around the trefoil ring (chapter XI; 5), the total ring current J and ring radius r are then related by

$$J = e/(2\pi r/c). \qquad 3$$

Elimination of r between Eqs.2 and 3 gives

$$\mu = e^2c^2/(4\pi J). \qquad 4$$

Now eliminating J between Eqs.1 and 4 yields a familiar looking expression for the magnetic moment

$$\mu = \frac{(e/m)(h/4\pi)}{(1-eV/mc^2)}, \qquad 5$$

which for $eV \ll mc^2$ approaches the magnetic moment of a Dirac electron.

It thus follows $\Phi J >> eV$, which means the electron (according to this model) consists mostly of magnetic energy. The electrostatic energy merely gives rise to a small anomaly in the moment of the Dirac electron. This correction term can be related to an impedance ratio.

The ratio V/J has the dimension of an impedance, which might be called the surface impedance Z_S of the trefoil. Solving Eq.1 for the current J then gives

$$J = mc^2/(\Phi + eZ_S). \qquad 6$$

Substitution of Eq.6 in Eq.4 yields for the moment μ the alternate expression

$$\mu = (e/m)(h/4\pi)\left[1 + Z_S/(h/e^2)\right]. \qquad 5a$$

The correction term assumes the familiar first order term $\alpha/2\pi$ of QED if Z_S equals the omni-directional vacuum impedance: $Z_S = (1/4\pi)\sqrt{\mu_0/\varepsilon_0}$. Such a matching of impedances is, if you will, a stability enhancing feature. Note how the fine structure constant α manifests itself here as a ratio of two by now very familiar impedances: $\alpha = e^2/2h)\sqrt{\mu_0/\varepsilon_0}$. The impedance h/e^2 has assumed a prominent role in the quantum Hall effect, whereas the vacuum impedance has now had a role for some time in important radar "engineering" applications.

Except for the spin, the period integral description of the electron is now almost complete, at least in outline. A model-based description of spin, though, is not exactly a subject matter that has received much encouragement in the physics curricula of the past sixty years. Ever since Dirac's introduction of spin as a by-product of a wave equation linearization, the spin concept has been regarded as an essentially abstract affair, not conducive to model-based speculations. While there have been attempts at modelling, most of them have not been able to tie together the known properties of spin particles. The exploration of the mathematical ramifications of spin have been more successful in homing in on spin's fundamental nature as a thing related to orientability and enantiomorphism.

Mathematically, spin was found to relate to a covering group for the rotation and Lorentz groups, or any linear orientation *preserving group,* for that matter. In fact, prior to their use in physics, Cartan had introduced spinorial-type considerations for the discussion of group structure. When seen from that angle, spin is found to relate to the orientability of manifolds and the existence of orientable objects therein. Haefliger[1] has stated the necessary and sufficient conditions for a manifold to support a "spin structure" (see chapter XV; 5). From a physical point of view, spin structure now relates to enantiomorphic pairing in spacetime. In a spatial sense, an enantiomorphic pairing has been known for a long time in crystallography. Physical spin adds a dynamic aspect to crystallography's static enantiomorphism. Note that the enantiomorphism of imbedded structures can have a global meaning if, and only if, the imbedding manifold is orientable. It means there is no circuitous path in spacetime that can carry one member of an enantiomorphic pair over into its companion. The individuality of such *unordered* pairs is therefore not negotiable.

This little detour in mathematics is necessary to appreciate the now following model-based discussion of electron spin as more than a possibly ad hoc procedure. Let us take chapter VI's integral Eq.15 as the period integral governing spin, thus accepting Kiehn's electromagnetic definition of the spin 3-form. There is a theorem by Kuenneth (chapter XIII; ref.1) that permits the decomposition of the period integral of this 3-form (the exterior product of the one-form A and the 2-form $\mathfrak{G}$) into the product of two period integrals:

$$\oint \mathfrak{S} = \oint_{c_3} \mathsf{A}\wedge\mathfrak{G} = \oint_{c_1} \mathsf{A} \oint_{c_2} \mathfrak{G} = n(h/e)se = h\ ;\ \text{for } n=s=k=1. \qquad 7$$

Eq.7 gives an integrated spin of the electron equal to Planck's constant h. Standard quantum mechanical procedures, by contrast, must be understood as operating with the differential notion of orbital $h/2\pi$ (prior to a 2π loop integration) and a corresponding differential spin angular-momentum notion $h/4\pi$ (prior to a 4π loop integration). The factors 2π and 4π are traditionally related to a rotational-unit operation and a spin unit operation. The latter, sometimes visualized by the so-called "spinor-spanner," can, for the present purpose, be more appropriately illustrated with the help of Fig.2.

How does a nontrivial unit operation 4π enter the integration process indicated in Eq.7? The answer is obviously not through the integral $\oint\mathfrak{G}$, but through the integral $\oint\mathsf{A}$, because c_1 is forced to remain in the field-free interior of the trefoil. Since c_1 is forced to take a knotted path 4π inside the field-free trefoil tube, the transition from integrated to differential spin is accordingly $h/4\pi = \hbar/2$, compare hereto Fig.2.

These arguments are not designed to please afficionados of abstract spin formalisms. They illustrate how a too-restricted mathematical approach generates mysterious nonclassical complexity later. "Nonclassical" is such an easy word to say. The irony is, if current mathematical tools of physics were already ill-equipped for dealing with the perfectly classical matters of crystal symmetry and crystal physics, why should we expect them to do better for particle enantiomorphism? The result was that physics could not avoid falling victim to the perfectly irrelevant nonclassical obscurism of standard spin theory. Orientation changing operations have been absent in physics, except as *ad hoc* reminders.

ENANTIOMORPHIC PAIR OF TREFOIL KNOTS

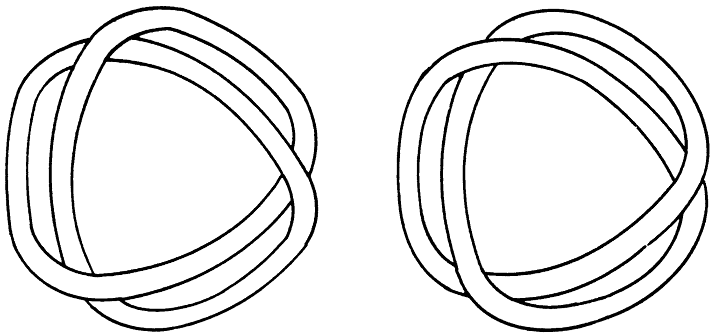


Figure 2: Here depicted are two enantiomorphic modifications of the spatial trefoil knot. They can neither be identified by motions nor by deformations in three-dimensional space. The two modifications of this knot are a nontrivial topological mirror pair in an orientable space. The characterization nontrivial refers here to the exclusive role of identification by improper (*i.e.* mirror-type) transformation. "Topological" refers to the invariance of the pair property, even under continuous deformations.

The obtruseness of PCT physics bears ample witness to this fact. The SO-3 bound spinor calculus of physics could be said to be an ad hoc local substitute for properly dealing with the global character of orientability. Ironically, it is all an aftermath of the undue SO-3 restriction precipitated by the vehicle of standard vector analysis!

3. Josephson and Quantum Hall Effects

In the process of having to make convoluted conceptual detours, one would almost forget what this discussion of electron structure is all about. The objective was to show that it is relevant to speak of field-free interiors for electrons and other particles. *A particle with a field-free interior permits period integral applications of chapter VI's Eqs.12 and 13 in environments that are not field-free,* because now the orbit itself traces a permissible integration path. Once this fact is established, a wealth of applications opens up pertaining to situations that are not otherwise transparent. Here is a selection.

The Josephson ac effect connects a voltage step V at a superconducting Josephson junction with the observation of a junction-related frequency ν. The discussions in chapters VI and VII, pertaining to factors 2 of Cooper pairing and cyclotron versus Larmor frequencies, can now help in guiding the way to this integral taken over the period $T = 1/\nu$:

$$\int_0^T V dt = n\, h/2e \, . \qquad 8$$

For n=1 Eq.8 gives h/e in terms of the voltage-frequency ratio

$$h/e = 2V/\nu \, . \qquad 9$$

Eq.9 gives very reproducible measurements of the h/e ratio.

Now, consider the flux quantization in a superconducting ring. The tubular ring, in principle, provides a field-free interior inside the ring tube for the integration cycle c_1. Electron pairing is essential in order to move in one another's magnetic field. Yet pairing indicates the possibility of many electron pairs in the same quantum state. Eq.12 of chapter VI gives directly the observed result. Superconducting rings cut in halves and sandwiched together with Josephson junctions can be made to give interesting demonstrations of the Aharonov-Bohm effect when they link an adjustable external magnetic field. Except for the pairing argument, the two examples given so far can, and have been, treated by the wave-function uniqueness approach, which as mentioned earlier, is very close to a period integral argument.

A different situation prevails in the case of the quantum Hall effect observed in the two-dimensional interface of MOSFETs (metal oxide semiconductor field-effect transistor). This situation requires a simultaneous application of two period integrals (chapter VI's Eq.13 and Eq.14). At low temperatures, this device has been found to manifest plateaus of constant Hall impedance at which, for all practical purposes, the forward resistance goes to zero. In the plateau states, the two-dimensional charge carriers assume a macroscopic state of cooperative order, which can be visualized as a translating lattice of cyclotron states with the common cyclotron period T. The Hall impedance Z_H is determined by the ratio of the Hall voltage V_H and the forward current J_F. This ratio can be converted into a ratio of period integrals by multiplying numerator and denominator by T:

$$Z_H = \frac{V_H}{J_F} = \frac{TV_H}{TJ_F} = \frac{\int_0^T V_H dt}{\int_0^T J_F dt} = \frac{\text{Eq.13}}{\text{Eq.14}}\left(\text{chapter VI}\right) = \frac{n}{s}\,\frac{h}{e^2} ;$$

n=1,2.. ; s=2,4,6...... . 10

Eq.10 describes both the integer and fractional quantum Hall effects. The first observations of this effect were taken to correspond to ground states of flux n=1. The number s has been called the Landau filling factor of the cyclotron states, because Landau first gave a Schroedinger description of quantized cyclotron states. *Since there is here an accumulation of electrons in the same quantum states, there should also exist some mechanism of pair forming to provide the necessary boson characteristics to do so. This pairing implies even s values.*

Shortly after the first observations, new data became available indicating that, contingent upon the initial ground state premise n=1, the filling factor s could also assume the value of rational fractions. At that point, all bets on a simple theory in terms of standard quantum methods were off. Subsequent attempts at explaining these phenomena have called on ground state solutions of the many particle Schroedinger equation. The picture that thus transpires is one of an apparent fractional charge.[2] This state of affairs has also been visualized as a cyclotron lattice with a sublattice of partially empty orbits.

The complexity of the Schroedinger approach was further compounded by experimental observations that the fractional s seemed to have only "odd" denominators. A little later, "even" denominator fractions s were observed and somehow, also, they were accounted for by Schroedinger-based arguments.

Looking at this brief overview of traditional approaches to the quantum Hall effects, one can hardly help being tempted by the simplicity of the period integral procedure summarized in the transition of equations, as illustrated by Eq.10. A word is in order to show Eq.10 to be more than just a coincidental transcription towards a known end result. A more leisurely discussion, justifying in large part the viability of this transition by semi-classical arguments, can be found in section 2 of chapter IX. Comparison shows as a major predicament encountered in the Schroedinger-based approach, that it is not so much the Schroedinger method itself that is to blame, but rather the tacit insistence on ground state conditions.

The origin of the ground state premise goes back to the early understanding of superfluidity, because the loss mechanisms come to a halt for anything residing in its lowest quantum state. Yet a comparison with the quantized flux states in a superconducting ring shows the existence of a series of quantized flux states well above the ground state. The question is whether these higher flux states are generated by more participating electron pairs, all circulating in a kinematic ground state of motion, or can the participating electron pairs be jointly elevated into a higher kinematic state? The cited difficulties would be closer to a resolution if the quantum numbers n and s were independently observable. The Schroedinger equation, all by itself, cannot give an unambiguous answer, because the fermion-boson choice needs to be injected as an alien additional element.

The ensuing many particle Schroedinger equation has retained a much more academic status than the single-particle Schroedinger equation.

The procedure illustrated in Eq.10 calls on two period integrals. The period integral of Aharonov-Bohm conveys the Schroedinger phase information; the integral of Ampère-Gauss conveys information about a cooperative motion of the charge carriers. The ability of the Schroedinger process to cover cooperative behavior needs to be questioned.

Any adaptation of the Schroedinger process to accommodate wider ranges of applications would most likely have to depend upon the injection of extraneous criteria fitting the situation at hand. However, that also requires a reassessment of quantum mechanics' statistical implications. The preceding considerations reveal a measure of complementarity between Schroedinger process and period integrals. The crucial distinction: period integrals are not statistical; however, the Schroedinger process is!

The conflict situation here alluded to was resolved in the past by pointing at mathematical asymptotics between Schroedinger and Bohr-Sommerfeld. The latter were regarded as a geometric-optic approximation of the Schroedinger result, thus placing the Bohr-Sommerfeld integrals in the category of an inexact procedure in physics. Leaving this statement for what it is, at least for the time being, a question arises whether or not the period integrals Eqs.12-15 of chapter VI are also to be regarded as approximate tools of physics. After all, in witness of chapter VI's Eq.7, the Bohr-Sommerfeld process can be looked at as derivable from Aharonov-Bohm.

Since it is hard to maintain that the Ampère-Gauss integral (Eq.14, chapter VI) would be of an approximate nature, it is for exactly the same reasons hard to maintain that the Aharonov-Bohm and Kiehn integrals (Eqs.12,13 and 15, chapter VI) are to be regarded as having an approximate status in physics. *While there is no question that results obtained with Bohr-Sommerfeld relate asymptotically to results obtained with the Schroedinger equation, this fact alone does not justify a relegation of Bohr-Sommerfeld to the realm of inexact physical tools.* There is an option, not considered in the past: *i.e.,* to replace the mathematical asymptotics by a physical asymptotics. In other words: *Bohr-Sommerfeld and Schroedinger address different physical situations.* Hence, more cautiously, the two physical situations, instead, are asymptotically related.

It cannot be denied that the here-presented option of giving the Bohr-Sommerfeld conditions the benefit of the doubt is really what should have happened to begin with. There is no logical ground for ruling one method as approximate, unless all avenues have been explored, making sure they address exactly the same physical situation. Since that has not been done in the past, this premature conclusion now stands corrected.

Yet, even if theory has no obvious last word in these matters, the practical results of Josephson effect (h/e) and quantum Hall effect (h/e^2) have been dramatic for metrology. These two experiments now provide measurements reproducible to within seven decimal places for the quanta of action h and charge e. **Table I** shows measurements based on a mixed data input from quantum electrodynamics and other sources. Fluctuations affecting the fourth decimal places for e and h are apparent. ***If this contradicts reports that quantum electrodynamics has been credited with predictions covering ten decimal places, an explanation lies in a mix-up of absolute and relative precisions.*** The numbers are displayed in **Table I**. They speak their own irrefutable language.

Table I : The metrology revolution of Josephson and Hall effects

h 10^{-34} Joule sec.	α^{-1}	e.10^{-19}Coulomb	year
6.6252	137.0377	1.60207	1953
6.62559	137.0388	1.60210	1963
6.626186	137.03608	1.6021901	1969
6.626196	137,03602	1.6021917	1973
6.626176	137.03609	1.6021892	1984
6.6260755	137.0359895	1.60217733	1986

Recommended data for fundamental physical constants: Planck's action h; fine structure constant α ; elementary charge e. Sources are Revs. Mod. Phys. for the years cited. The number of decimal places reflects the confidence level at the time of publication. Note how some changes go well beyond preceding confidence levels. Cohen and Dumond (Revs Mod. Phys.**37**,593;1965) report how, without a quantum electrodynamics (QED) related input, the '63 value for α^{-1} would have been 137.0367 instead of the value 137.0388 cited for the year 1963.

The just-presented experiences provide some material that might be interpreted as inviting a reassessment of some standard notions of quantum theory. Further pursuit of these matters should not avoid the mentioning of some recent experimentation that has given evidence of single-particle diffraction. Since diffraction has been so exclusively identified with wave motion, the manifestation of such a phenomenon almost sounds like a contradiction in terms.

The descriptions of this unexpected particle performance still tend to seek predominantly a basis in the Schroedinger method. A procedure, initiated much earlier by Bohm and Vigier,[3] reveals a trajectory feature of the Schroedinger solutions. This method has proven effective in predicting the trajectories of diffracting single particles. The old alternative of wave or particle now called for a wave and particle proposition. The old Copenhagen dichotomy of indecision between ensemble and single particle had once again reared its ugly head.

It is of relevance to mention briefly that Fenyes[3] and others have used a Bohm-Vigier-type argument to "derive" the Schroedinger equation from the Euler equations of fluid dynamics. This step can only be accomplished by injecting a so-called quantum potential in the Lagrangian. The Bohm-Vigier reverse process of identifying this quantum potential by means of the Schroedinger equation is less pretentious.

Modern methods of coincidence counting have made the single particle experiments very convincing. However, hard X-ray photons had already, much earlier exhibited a semblance of one particle diffraction. They have been the subject of investigations by Duane[4] in the early Twenties, prior to the discovery of the Schroedinger equation. The tools available to Duane were the Bohr-Sommerfeld conditions. Using photon momentum and the periodicity of the diffracting crystal lattice, Duane showed how the Bragg formula of wave diffraction if obtained, provided photon momentum $\mathbf{p}$ and wave vector $\mathbf{k}$ obey the relation (chapter III; 3):

$$\mathbf{p} = (h/2\pi)\mathbf{k} \ . \qquad 11$$

Note how Eq.11 harbors the hidden ingredients of Copenhagen's later point-particle thesis, because *it relates a domain-based momentum* ***p*** *to a point-based field quantity* ***k***. Yet, notwithstanding this mathematically forbidden identification of "apples and oranges," the physical compromise it entails has been most rewarding, although not without limitations. For comparison, the reader should also note that the Bohr-Sommerfeld integral and the period integrals Eqs.12-15 of chapter VI don't invoke a similar mathematical incompatibility, because e, h/e, and h have a well-recognized domain status.

Using relativity-based arguments, Eq.11 was simultaneously obtained by de Broglie. He instead assigned waves to material particles. Duane's objective clearly was directed towards associating a particle identity with the photon impact that is so prevalently observed in Geiger-Mueller counters used for the detection of hard X-rays. Yet, ironically, it was de Broglie's inverse procedure that led to the discovery of Schroedinger's equation.

An alternative to squeezing Eq.11 into the straitjacket of a wave description is the option of looking at $\{\omega,\mathbf{k}\}$ directly as particle attributes. This option has remained underdeveloped in contemporary physics, because it invokes a need for particle modelling. Models have remained submerged in the Copenhagen sea of universal uncertainty.

This juxtaposition of the Bohm-Vigier wave trajectory analysis of the Schroedinger equation and Duane's application of Bohr-Sommerfeld methods underlines, in an entirely different setting, the complementary functional positions of Schroedinger equation and period integration. Here, once more, is reason to abandon the old prejudice of relegating period integration to a position inferior to solving the Schroedinger equation. The truth is they have a domain of overlap, the details of which should be the subject of further investigation. It is probably the best way of learning to understand the limitations of the Schroedinger process.

4. Preliminary Overview of Results

The objective of this whole endeavor has been merely an attempt at constructing a more coherently organized picture of some physical laws that have been around for some time. Yet, in presentations and subsequent discussions of period integral techniques, one finds an uneasiness of attitude which seems to act as a hurdle on a possible path to greater acceptance. This uneasiness tends to translate into a nonverbal state of suspicion. In the light of such incomplete communication, let us call on the old devil's advocate by bringing hidden suspicions to greater awareness. Here are the results of piecing together sundry remarks generated by this subject matter.

First of all, there is the suspicion of the approximate and asymptotic nature of the period integrals. This topic has been extensively treated in the preceding sections. So, at this point, *we shall assume that the reader is now willing to accept the preliminary conclusion that period integrals and Schroedinger equation address different physical realities,* which are sufficiently close to account for a certain overlap in results.

The next item on the agenda is concerned with the question of when and how to apply the Eqs.12-15 of chapter VI. Approaching quantum mechanical situations with the Schroedinger equation, one is prepared for some hard work of solving eigenvalue problems. As mentioned earlier, the period integrals Eqs. 12-15 of chapter VI are ready and solved. The where and when of their application, though, should ultimately become a matter of judicious physical decision-making. Yet, in the process of finding a way between Schroedinger equation and period integrals, there is no harm in trying things out to see what works best. Rather than dogmatically insisting on the one-and-only way, there should be room for some theoretical adventure. It is just another way, other than by physical experiment, of asking nature questions by doing "theory experiments" instead.

There is also a psychological hurdle to overcome, because we have all been conditioned to believe that good results can only be obtained after much hard work. The period integral method seems too easy and generates suspicion, which, in turn, brings about a frame of mind to reject the extremely simple transcriptions of Eqs.9 and 10.

Let us now examine more closely the judicious decision-making involved in the where-and-when of applying period integrals. In many ways, the decision-making is purely a matter of finding a cogent physical rationale as to when to use the Schroedinger equation and when to use period integrals. Here we enter potentially dangerous territory. The essence can be summarized in a more than sixty-year-old question: Where does the Schroedinger equation come from and what are its limits of applicability? Alternatively, one may ask: what is the nature of the wave-function's statistics? Answers for a judicious choice between period integrals and wave equation can hardly be forthcoming unless we understand more precisely each of the protagonists. What better way of getting to know them than by having them compete for relevant results.

A great help in the now-following endeavor is the scholarly book by Jammer[5] on the history and philosophy of quantum mechanics. Most texts on quantum mechanics exhibit bias versus what presently may be known as the Copenhagen interpretation. Yet, more so than any text I have seen so far, Jammer's book makes it clear how essentially two major interpretational options are compatible with the standard quantum formalism. We can now build on conclusions summarized in chapter VI; 8.

The probability amplitude defined by the Schroedinger wave function describes either (1) the statistics of a Gibbsian-type ensemble of conceivable manifestation of a single-quantum mechanical system, or (2) the statistics of a true ensemble of an actual plurality of systems that are identical and prepared in a similar quantum state.

An interpretation (1), or variations thereof, are usually referred to as a Copenhagen interpretation. Interpretation (2) has been known as a statistical or ensemble interpretation, though neither name very clearly delineates the plurality of actual physical systems involved. After all, the Copenhagen interpretation also deals with an ensemble, an abstract Gibbs-type ensemble is here associated with the statistics.

A major distinction between the two interpretations manifests itself in a concept that has become known as quantum mechanical uncertainty. The original Gibbsian concept of conceivable manifestations of one and the same system now undergoes an extrapolated specialization, in the sense that no single system can ever be expected to be in a state of absolute rest. This premise does not sit too well with the premises of relativity, because it creates a hidden premise of an absolute reference with respect to which this state of unrest is to be established. Since a state of unrest is kinetic in nature, the uncertainty dogma comes full circle by a synonymity with the concept of single-system zero-point energy.

Interpretation (2), by contrast, deals with finite ensembles of real physical systems. Its associated statistics relates to a mutual behavior of member systems in the ensemble, and thus avoids the relativity predicament. Since this statistics has no thermal connotation, the question is now: what mutual system parameters are eligible for that purpose? Here it is appropriate to call on Planck,[6] who first introduced the concept of zero-point energy as a typical manifestation of a real physical ensemble (chapter III; 5).

The parameter subject to statistical randomization is the mutual phase of the constituent systems of the ensemble. Additional evidence is available in a calculation that has surfaced in the Russian literature.[7] It shows how a randomly oriented ensemble of systems, all of which reside in the individual state n, will assume an ensemble average of $\sqrt{n(n+1)}$. Also the details of the statistical randomization orientation are more extensively discussed in chapter III;5.

Since solving the Schroedinger equation gives "full-automatically" the ensemble flavored features of Planck's zero-point energy and the quantum average $\sqrt{n(n+1)}$, the question is justified whether the Schroedinger equation can truly be expected to describe a single quantum mechanical system, instead, of a statistically randomized ensemble of such systems.

Similar questions now need to be asked about the nature of quantum mechanical uncertainty and zero-point energy. Yet, even these two cases have not sufficed to establish without ambiguity the physical ensemble nature of Schroedinger's method. Textbooks have even presented these derivations as conceivable support for the Copenhagen Gibbsian-type ensemble. Let us therefore examine some recent observational evidence that can be related to the here given arguments.

The so-called optically "squeezed states" (chapter III; 4) have given experimental proof that systems can drop below the Heisenberg uncertainty level, provided a companion system correspondingly ends up above the Heisenberg level.[8] Here Heisenberg uncertainty is only obeyed as a system average. The system plurality implied by these observations suggests a quantum mechanical interpretation in category (2): *i.e.*, of ensembles consisting of real physical systems. Neither logically nor observationally is it indicated to accommodate these phenomena with the help of the Copenhagen single-system interpretation (1) or variations thereof.

Once the Schroedinger equation is accepted as a tool applicable to true ensembles randomized in phase and orientation, then the period integrals can be accepted as single-system tools. The physical difference of the respective fields of applicability of Schroedinger equation and period integrals has now been delineated. Asymptotic behavior between an isolated system and an ensemble of those systems is to be expected. Yet those differences now are no longer a valid cause for questioning the precise validity of the period integrals versus the Schroedinger equation. Each has its own domain, with a measure of overlap.

The next upcoming question is what we ought to do about ensembles that are no longer randomized in phase and orientation? Many of the macroscopic quantum effects are in this category. The cooperative order in such ensembles makes the whole ensemble behave as if it were a single system. The easier assessment of these ordered systems in terms of period integrals, as compared to the Schroedinger method, is living testimony supportive of the given delineation of a two-tier approach.

Yet, the evidence supportive of interpretation (2) has been known for the most part and still has not been able to swing physics' publicly stated opinion away from the Copenhagen interpretation. Some of the fundamental causes of this inertia have to do with the relation between the Copenhagen interpretation and quantum electrodynamics. A transition from interpretation (1) to (2) would undermine quantum electrodynamics' (QED) use of the zero-point energy of vacuum. Supporters of interpretation (2) have remained tongue-in-cheek about this important aspect. There is an understandable reluctance to abandon the amazing calculational potential of QED for matters of merely philosophical and conceptual virtue. The main reason why the issue may now be resubmitted for further scrutiny is the period integral calculation of at least one of the major results of QED (*i.e.*, the electron's anomalous magnetic moment as presented in the previous section). *This microphysical result, which is contingent on the electron's field-free interior, extends the applicability of the Aharonov-Bohm law to many practical cases of macroscopic and microscopic quantum effects.*

While not much more than a sketch, the given calculation of the electron's magnetic moment and spin does not invoke the infinities of QED. The latter (QED) is contingent on the extension of the concept of zero-point energy to the somewhat hypothetical family of harmonic oscillators of the electromagnetic field in vacuum. Standing waves of this type have been used to calculate (the experimentally verified) Casimir effect of attraction between two plates. Conventional quantum mechanical methods though have been shown to yield the same result.[9] All of which goes to show that the zero-point energies of vacuum, whether real or not, are not to be underestimated as calculational expedients. It is one of those frustrating enigmas of physical theory, if what we believe to be the more logical and consistent method has trouble competing with methods that don't measure up to that test.

It would be unfair to dwell merely on the problems of the Schroedinger method, without giving the period integrals more of a preliminary probing. Since the three integrals, as here used, are interdependent, one wonders whether Eqs.14 and 15 of chapter VI should be taken as primary choices. The decision between chapter VI's Eqs.12 and 13, when seen as a ratio of its Eq.15/Eq.14, then can be made to depend on what Eq.14 counts. This is, at least, a half-way justification for Onsager's electron-pairing argument. The pairing, which was used by Onsager as an explanation of the unexpected factor 2 in chapter VI's Eq.12, is discussed in chapter VII.

Anyone claiming that the Schroedinger equation describes phase and direction randomized ensembles, instead, of nondescript nonclassical single system situations can expect major opposition. For one, it is bound to renew the pressure for deriving Schroedinger's gift from heaven from first principles, say from the period integrals, if the latter truly have first principle status. It is the traditional punishment handed out to those who claim the existence of a true ensemble basis of quantum mechanics. So far, the period integrals, here considered, relate solely to stationary states. The Schroedinger equation, by contrast, describes stationary as well as transition states. Additional period integral information about quantum transitions is needed (Chapter XIII). Here we refrain from finding a complete "derivation" for an equation that is seen as a gift from heaven. Respect for giver and gift demands that we first probe the realm where the gift may be used without abusing its potentialities.

Bohm and Vigier[3] have shown that a "derivation" of the Schroedinger equation from the equations of continuum mechanics is contingent on the injection of a Bohm-Vigier-type quantum potential. In the Fifties and Sixties there were a number of papers on this theme by Nelson,[3] Fenyes[3] and Weizel.[3] The latter, in particular, suggests a zero-point connotation for the quantum potential.

A major reason why truly convincing derivations have not been forthcoming points at deeper distinctions between period- and Schroedinger methods. The latter is capable of dealing with so-called transitional states, whereas the period method seems so far restricted to the description of stationary states of excitations only. It is well known that not all transitions between those stationary states of excitation are equally probable. The great improvement of the Schroedinger method over the Bohr-Sommerfeld method was its ability of reliably predicting transitional selection rules.

In the light of this typical distinction, one is inclined to infer that the selection rules are at least, in part, a consequence of the "ensemble awareness" of the individual systems. Yet, one can hardly expect conclusions about ensemble behavior without at least an insight in the primary disposition of the isolated system to respond to the ensemble. What is the criterion for a change of state of the isolated single system?

An emission or absorption event is a transitional state of a finite size system over a finite interval of time. Such an event should be recordable in terms of a spacetime integral extended over the spatial extent of the system and over the interval of time during which the event takes place. This finite event domain D_4 of M_4 may be expected to be simply connected. Its boundary $c_3 = \partial D_4$ should then be the topological equivalent of a 3-sphere. It is now tempting to equate an emission or absorption event with "something" that comes out or goes into D_4. Since the events are known to be quantized, the question now is what 3-form has quantized period residues that could account for such events?

The 3-form of field $A \wedge \mathcal{G}$ conceivably could be regarded as a possible candidate. Yet the earlier applications made of this 3-form reveal it as a tool depicting stationary states, not transitional states. Note that the stationary state option corresponds to a decomposition of its integral into a product of 1- and 2-dimensional period integrals of a more familiar vintage.

R.M. Kiehn (chapter XIII, ref.2) considers the 3-form $A \wedge F$, which is the exterior product of a closed 1-form A and an exact 2-form $F = dA$. Any decomposable situation, comparable to $A \wedge \mathcal{G}$, should now give identically zero, because F has no nonzero residues. Yet $A \wedge F$ can have nonzero residues for a cycle c_3 topologically equivalent to a 3-sphere. This leads to a spacetime integral over an interior where $F \wedge F \rightarrow \mathbf{E} \cdot \mathbf{B} \neq 0$. This inequality is known as a criterion of nonintegrability of A, leading to a doubly bladed bivector F. Hence, events of emission and absorption are interestingly concurrent with a temporary state of nonintegrability of A. The residues of $\oint A \wedge F$ are found to be multiples of $(h/e)^2$.

In order not to overload an investigation of period integrals too early with too many ramifications, let it suffice, for the present, that at least two crucial Schroedinger results[6,7] can be verified by a statistical processing of period integral results.

What is the upshot of this plea for reinstating period integrals as a more nearly exact tool of physics? It opens up new avenues for particle modeling and possibilities for macroscopic quantum systems. Zero-point energy, uncertainty, and Schroedinger equation now reflect a pre-thermal statistics of mutual-system phase and orientation in an ensemble.

Specifically, it can be said: period integrals are the exploratory tools for establishing the nature and shape of physical objects, regardless of whether these objects are large or small. For the specification of integration domains of period integrals, electric charge is to be seen as a primary "substance" of physics. It determines topology by separating distinct field domains. Faraday cage and Meissner effect in superconductors provide macroscopic prototypes of such separation. The absence of metric parameters in these descriptional tools permits the extrapolation of such properties into the micro-domain, including elementary charge itself.

The integration loop of the one-dimensional period integral is like a thread strung through the field-free holes of the physical object under consideration. The loop can be shrunk onto the object until the latter's "substance" acts as an obstruction preventing further shrinking. The cycle of a two-dimensional period integral is like a plastic sheath wrapped around and shrunk onto the object, until its substance acts as an obstruction preventing further shrinking. Three-dimensional period integrals are, or can be, cyclic product combinations of what might be called string and plastic wrap. Taking the Pfaffian integral $P = \oint A \wedge dA$ as a stability criterion, one finds P=0 for stable particles drawing worldlines in spacetime. These worldlines originate and terminate in simply connected event domains where P≠0. This picture provides, for all practical purposes, the basis for a Feynman diagram. In chapter XII and XIII, the potential of these methods are investigated in much more extensive detail.

In addition the reader should also be made aware of existing variations on the theme of model-based calculations of the electron's anomalous moment. Recent discussions have been given by Barut[10] and Elbaz.[11]

CHAPTER IX

IMPLICATIONS OF COOPERATIVE BEHAVIOR

1. Cooperative Order and Zero-Point Motion

A reexamination of experimental findings about flux quantization places in question the effective applicability of the Schroedinger process to cooperative phenomena. The apparent suspension of wave-equation procedures assumes dramatic proportions for the quantum Hall effects.

Few equations in physics have a wider and more versatile applicability than the Schroedinger equation. In fact its success as a major tool of modern physics has been so great that, in the course of time, physicists have developed a nearly religious conviction about its almost unlimited potential. Short of the realms of QED, QCD, and Dirac's relativistic spinor equation, the relevance of the Schroedinger equation has virtually remained unquestioned except by Schroedinger himself and a few others.

The situation conveys the ironic picture of a younger generation believing that Schroedinger himself perhaps did not fully understand the extraordinary potential of his own equation. It is not unusual in music that young performers frequently know better what to do with a composition than the composer himself. The analogue of a difference between creating and recreating performers may be said to manifest itself also in the realm of the physical and engineering sciences.

Yet, unlike the musical composition, which is regarded as having reached a final form once it is published, efforts to improve and extend the machinery of quantum mechanics remain an ongoing endeavor. Few theories can be said to have reached a form that does not invite further tinkering. While the Dirac equation is to be regarded as a further development of the Schroedinger equation, attempts at extending the Dirac scheme into a workable method for dealing with many particles with spin ran into difficulties in retaining full relativistic invariance. Yet none of these hurdles has been big enough to shake the convictions of those who believed in the Schroedinger single-sytem supremacy.

Mindful that religious denominations are known to take directions for which the founders cannot be held responsible, the conclusion following from this comparison indicates that a fair measure of religious argument has also assumed a place in physics. Any awareness of a law of nature starts out with a hunch or a belief that something might be true. The difference between science and religion is that science demands a substantiation of the stated beliefs. In religion, such substantiation may be beyond what is deemed to be possible.

In quantum mechanics, physics somehow is involved in situations halfway between science and religion. The many useful applications of the Schroedinger equation indicate that the belief works. In fact, it works so often and so well that the sheer statistical evidence of applicability is taken as proof and substantiation of the stated beliefs. Whenever we run into an exception we are inclined to suspect first an error in the implementation of our beliefs and not the belief itself. We have not yet reached the courage of our convictions to test our quantum mechanical beliefs beyond the point where we keep overwhelming ourselves with the brute force of statistics.

In the following, two examples will be examined which give direct and near-incontrovertible evidence of the inapplicability of the Schroedinger equation to highly ordered systems. The experimental evidence for one of these examples has, quite surprisingly, been around for almost three decades. The argument goes back to the discovery of flux quantization and it is based on the experimentally confirmed absence of a residual zero-point flux in superconducting rings.

The second example is of more recent vintage and relates to the quantum Hall effect. The latter is pictured as a lattice of identical cyclotron states drifting past the voltage probe of a Hall sample. The cyclotron state and the superconducting ring state have in common that electrons circulate in a magnetic field. For the ring, the magnetic B field is generated by the electrons themselves circulating at the rate ω of the Larmor frequency, where $\omega=(e/2m)B$. For the cyclotron, they are circulating in an external B field, at the cyclotron rate $\omega=(e/m)B$.

A Schroedinger analysis of the quantum states of electrons circulating in magnetic fields has been given by Landau.[1] The result of his calculations is given by the following energy expression:

$$E_n=(n+1/2)\hbar\omega \pm \xi\mu B; \; n=0,1,2,3,..... \qquad 1$$

with $\xi=1$ for external B and $\xi=1/2$ for internal self-B. The magnetic moment μ of the electron is

$$\mu=(e/m)(\hbar/2)(1+ \alpha/2\pi), \qquad 2$$

including the first order anomaly term $(\alpha/2\pi)=0.00116...$.

It is now easily verified that the individual spin terms in Eq.1 have the values $\pm(\hbar\omega/2)(1+\alpha/2\pi)$, provided $\xi=1$ combines with the cyclotron frequency and $\xi=1/2$ is combined with the Larmor frequency. Except for the small anomaly term $\alpha/2\pi$, the zero-point contribution $\hbar\omega/2$ in the first right hand term of Eq.1 would be almost completely masked by a spin contribution of the second term.

For a superconducting situation, such as prevails in the rings used for the determination of flux quantization, the BCS anti-parallel pairing of electrons may be expected to lead to a cancellation of the spin contributions, including the anomaly term. It then follows from the Landau energy spectrum given by Eq.1 that an observable zero-point contribution is retained, because it is not now masked by spin terms.

Given the situation at hand, it is hard to understand how else the zero-point motion could manifest itself than by a residual circulatory motion of electrons; it is, after all, the degree of kinematic freedom considered in the Landau analysis. Such a residual circulatory motion at half the quantum level, even if hard to understand from a point of view of dynamics, would unavoidably lead to an associated residual half-flux quant h/4e.

A careful examination of the thirty-year-old experimental flux data of Deaver-Fairbank[2] and Doll-Naebauer[3] does not show any evidence supporting the thesis of a residual zero-point flux. Compare hereto chapter III; 6; Fig.2. There is little question that we are here confronted with a conflict situation between theory and observation with the inescapable conclusion that theory would have to yield to the reality of observation. Here is "almost" incontrovertible evidence raising the question: how far should one go in always giving Copenhagen the benefit of the doubt? Yet, mindful of man's propensity for finding arguments to rescue prevailing teachings, let us examine another angle.

In the just-given discussion the Landau analysis has been adapted to a situation with an internally generated B field by inserting the factor 1/2 in front of the spin terms of Eq.1; which is typical of the difference between a self-energy term versus a mutual energy term. This step is here intended to leave the Schroedinger analysis in the capable hands of Landau.

The following rationale is one way of justifying this extrapolation of Landau results. In the self-field situation, one half of the electrons can be considered as performing a cyclotron motion in the field generated by the other half. This picture is compatible with the Larmor frequency being half the cyclotron frequency. The half in front of the spin term then simply becomes a factor accounting for the active B field. It is incumbent upon those who see this argument as inadequate to give an independent Schroedinger analysis for the harmonic electron motion in the superconducting ring, by showing explicitly how the zero-point term vanishes.

At this point, we note how the theoretical expectation of a zero-point energy appear at variance with the experimental observations cited in refs.2 and 3. This state of affairs, in turn, places in question the applicability of the Schroedinger process to physical situations that are distinguished by a high degree of cooperative behavior.

2. The Quantum Hall Effect

The quantum Hall effect is experimentally rather unique in that it permits a direct observation of quantum states. These days, measurement theory refers to such situations as a "nondemolition" measurement. The Hall probe examines a transport of a lattice of identical cyclotron states. The measurement does not require a changing of quantum states. Only at the beginning and the end of the Hall sample exists a balanced creation and annihilation of cyclotron states. Table I of the previous chapter has already illustrated the reproducibility of data obtained by experiments based on the direct observation of quantum states. In chapter VIII, a striking relationship between quantum Hall effect and period integration has demonstrated the potential superiority of the global method for treating such macroscopic quantum phenomena.

Examples of attempted theoretical analyses in terms of the Schroedinger equation can be found in a recent monograph.[4] The cited method of analysis centers around ground-state solutions of multiple particle Schroedinger equations. The number of particles considered (*e.g.,* 2,4 or 6 particles) for a Landau state does not account for the whole MOSFET sample.

The cited approach mostly starts from ground-state Schroedinger solutions. They may be said to correspond to cyclotron orbits intercepting only a single flux unit h/e of the applied **B** field. This restriction is not really called for by the physical situation. In fact, it is this silent assumption that has led to a dichotomy between what are believed to be two effects: the so-called integer and fractional effects. *To rectify the consequences of this physically unnecessary imposition, an agreement with experimental reality then forces the introduction of either partially filled lattices of cyclotron states or some kind of hypothetical fractional charge.*

These comparisons suggest that a Schroedinger approach seems anathema in cooperative situations. This raises questions as to what kind of quantum tools can be used instead of the Schroedinger equation.

Acting on the need for describing a phenomenon manifesting a high degree of order with the exclusion of any identifiable statistical behavior, it is only logical and natural to ban for its description all tools that have statistical implications. The Schroedinger equation is therefore ruled out as a suitable tool. The following arguments, based on a straightforward model reflecting the cited order, then leads, in conjunction with the period integral quantization recipes of chapter VI, to a dramatically simplified description of the quantum Hall effects, in fact, no less than a unification of integer and fractional effects.

Consider the two-dimensional interaction space of a Hall effect sample of length $\mathfrak{l}$ and width $\mathfrak{w}$. This interaction space is assumed to be filled with a lattice of orbiting electrons in the same cyclotron quantum state. The surface density of cyclotron states be denoted by the symbol σ.

Each cyclotron orbit is assumed to have s orbiting electrons, thus giving a total charge per orbit of

$$q = s\ e, \tag{3}$$

where e is the elementary charge of the electron and s is called the orbital filling factor.

Each orbit intercepts a flux F of the applied field of magnetic induction perpendicular to the surface of interaction. The orbitally linked flux Φ is taken to consist of n elementary flux units Φ_0, where n is called the flux quantum number:

$$\Phi = n\ \Phi_0\ . \tag{4}$$

The total orbitally intercepted flux F through the sample is now

$$F = \sigma\ w\ l\ n\ \Phi_0\ , \tag{5}$$

and the total orbital charge in the sample is

$$Q = \sigma\ w\ l\ s\ e\ . \tag{6}$$

The Hall impedance Z_H is defined as the ratio of the Hall voltage V_H and the forward current J_f in the sample

$$Z_H = V_H / J_f\ . \tag{7}$$

The Hall voltage and forward current are presumed to come about as a result of a rigid motion of the whole lattice of cyclotron states. Let $v = dl/dt$ be the lattice drift velocity in the lengthwise direction of the sample; one then obtains V_H from Eq.5 through Faraday's law of induction by taking dF/dt and J_f follows from its definition equation dQ/dt. Hence

$$V_H = \sigma\ w\ n\ \Phi_0\ v\ , \tag{8}$$

$$J_f = \sigma\ w\ s\ e\ v. \tag{9}$$

From Eqs.7,8 and 9 one finds for the Hall impedance;

$$Z_H = (n/s)(\Phi_0 / e). \tag{10}$$

The Eq.8 is an unusual yet rigorous application of the Faraday induction law. Only the orbitally linked flux travels past the Hall probe circuit. There is no criterion of travel for the flux outside the orbitally linked components.

The expression Eq.10 is not only independent of the sample dimensions w and l, it is also independent of the lattice drift velocity v. For given values of the quantum numbers n and s, the experiments show Eq.10 to be met for an interval of applied field values B. For a MOSFET sample it is also possible to change the density of available electrons by changing the gate voltage. The corresponding changes in J_f for fixed n and s values point at an interval of permissible changes in lattice drift velocity v. These intervals of constant Z_H are called "Hall effect plateaus."

A comparison with experiment (or Eq.13 of chapter VI) yields for Φ_0 the preferred value

$$\Phi_0 = h/e, \qquad 11$$

(rather than $h/2e$) as the flux unit intercepted in an external B field. The $h/2e$ value prevails for electrons circulating in a self-generated internal B field. By calling on the electron's field-free interior, it is possible to present more elaborate theoretical arguments supporting Eq.11. However, rather than opening up a can of worms in a no man's land where Schroedinger's equation does not apply, an empirical acceptance of Eq.11 may, at this point, be preferred.

Since the Pauli principle (a Schroedinger independent premise) demands boson formation to have electrons accumulating in the same cyclotron quantum state, pair formation of electrons must be expected to be a fact of nature also in the case of the Hall effect. The extremely accurate rational number ratios n/s observed in experimentation present an emphatic plea favoring an electron pair formation resulting in a cancellation of the spin energy terms in Eq.1 and their anomaly contributions.

It thus follows that the quantum number s can only assume even values. Since n can assume odd and even values, the experimentally observed simplified fractions corresponding to n/s are bound to lead to a cancellation of *factors two* due to an always present factor two in s.

The given description jointly accounts in a simple manner for most of the quantum Hall effect characteristics. It unifies integer and fractional effects. It shows the needed measure of sample independence. The forward current insensitivity, *i.e.*, independence of the lattice drift velocity v, accounts for the plateau formation. *An anti-parallel spin pairing is established by the total absence of deviations due to the electron's magnetic moment anomaly.*

The even properties of the quantum number s finally can be called upon to account for an observed preference for "odd denominator" fractions for the fractional Hall effect. Please note that the literature gives preference to the inverse of the n/s factor. In the meantime, and as is to be expected from the here presented point of view, even valued denominators of s/n have now also been found.

An insight into why von Klitzing's procedure tends to favor integer s/n factors and why odd denominator s/n fractions dominate the scene in the observations of the fractional effect, can be obtained by making an $s \rightarrow n$ diagram.

An inspection of **diagram** I clearly reveals the dominance of entrances with odd denominator such as observed by Tsui *et al* and Willet *et al.*[5] Since the observations by von Klitzing were performed by constant magnetic field, while varying the electron density, it is the parameter s that

Diagram I: Ordering of Quantum Hall Effect Observations

s ↑										
10	10	5	10/3	5/2	2	5/3	10/7	5/4	10/9	1
8	8	4	8/3	2	8/5	4/3	8/7	1	8/9	4/5
6	6	3	2	3/2	6/5	1	6/7	3/4	2/3	3/5
4	4	2	4/3	1	2/3	4/7	4/7	1/2	4/9	2/5
2	2	1	2/3	1/2	2/5	1/3	2/7	1/4	2/9	1/5
s /n→	1	2	3	4	5	6	7	8	9	10

The given ordering of s/n data comes about by presupposing even s values combined with odd as well as even n values. Of the 50 entrances $\nu = s/n$ shown in the diagram, 17 are integers. Of the remaining 33 **reduced** fractions, only 7 have even denominators. The observed dominance of odd denominator fractions is thus shown to be purely a consequence of an even s hypothesis, *i.e.*, electron pairing.

changes. This explains why, for constant B (and n), a changing s has a greater probability of hitting the integer entrances on the left of the diagram. On the other hand changing B and correspondingly n, while keeping the electron density constant, will clearly lead to the fractional entrances on the right of **diagram I.**

Frantic experimental and theoretical activity is presently going on to better understand the nature of the two-dimensional lattice of Landau cyclotron states that is presumed to be responsible for Hall effect quantum behavior. The prevailing methods of analysis expect to be able to find a key to the manifest order displayed in the quantum Hall effect with the help of a Schroedinger-based process, which is confessed to be inherently statistical in nature. In the context of the here-chosen period integral approach, the ratio (n/s) of the quantum numbers n and s is provided by experiment with great precision. The chances of individually measuring with precision (ns), or n and s, have so far looked slim. The two methods of theoretical assessment amount to a conceptual trade-off. The transition from Schroedinger process to period integral method dispenses with a number of ad hoc assumptions that are necessary to make the Schroedinger approach work (*e.g.*, fractional charges or empty orbital sites).

CHAPTER X

A TALE OF FINE STRUCTURE COINCIDENCES

1. Summary

Here is an odyssey in the realm of spectral secrets of Nature's simplest atom. A modification of Weber's force function, recently proposed by Phipps as an alternative to the Coulomb potential gives a fine structure of twice the observed amount. Yet the two relativistic approaches by Sommerfeld and Dirac, with or without this Weber-Phipps modification, both give the observed fine structure splitting, except for higher order differences in even powers of α. Pauli's approximation, however, in conjunction with the electron's magnetic moment anomaly, gives, by far, the best agreement with experimental observation. The major reason may well be that it permits, in addition to a correction for the comoving nucleus, also odd powers of α such as introduced by the accurately known magnetic moment anomaly of the electron. An inspection of the traditional QED calculations of the Lamb shift reveals for s-states a *residual distance* of effective proximity between electron and nucleus. Model-based interpretations of this residual distance leave many open questions.

2. Introduction

One would think that a particle moving in a central field might be one of the most exhaustively treated topics in the physics literature. In taking this conclusion for granted, one would therefore hardly expect modifications that could contribute much in terms of deeper physical insight. Nevertheless, even this early and great breakthrough of modern science is by no means a closed chapter in the contemporary annals of physics. It has retained a multitude of questions that today are of as much interest as in the days of Newton.

In the course of time, the extension from one- and two-body situations to three- and more body situations have presented very serious hurdles in obtaining closed-form solutions that could give an immediate insight in the stability of such compound structures. Over and above, the formidable mathematical obstacles presented by more bodies carried over into the modern realms of relativity and quantum theory. Hence, notwithstanding these latter day revolutions, some problems have not changed.

After Newton's fundamental solution for the case of the ideal inverse square law, the most commonly considered variations of the classic planetary problem have to do with changes in the force law, say due to deviations in the ideal inverse square law, changes due to proximity effects or perturbations due to other gravitating bodies, and, last but not least, interactions with rigid body dynamics and continuum mechanics due to the finite size of the interacting objects. In dealing with these matters, Euler, Lagrange, Hamilton and Jacobi have given Newtonian theory its mathematically and physically most-sophisticated form, a form which subsequently was found to be almost ideally suited for accommodating the subsequent developments of relativity and quantum theories.

The great Newtonian breakthrough in mechanics ironically was made possible by the introduction of a hybrid conceptual feature. The mixed use of field and particle notions provided a major key to solving problems in celestial mechanics. The kinematic aspects of the theory are particle-based, yet the force interaction between particles or bodies is based on the use of what has become known as a field concept. This situation prevails for gravitational as well as for electrical interactions of the Coulomb type.

In one of his later publications, Brillouin[1] called attention to this dichotomy associated with the notions of kinetic and potential energy. While the kinetic energy of a particle can be clearly associated with an isolated particle, the potential energy, by contrast, is always a mutual affair of the particle and objects creating a "field" at the location of the particle. Yet Newtonian treatments have, in the course of time, led to a tacit convention in which kinetic and potential energy are uniquely related to one and the same particle. Kinetic energy relates to a given inertial frame, and could be said to be defined with respect to the rest of the universe.

When the field concept first was introduced by Newton for dealing with his universal law of gravitation, it was the first mathematical coding of what then was called an "action at a distance," as distinguished from the more familiar contact forces. Newton concluded this action at a distance to be a fact of nature, without extensive considerations as to how this action at a distance might come about physically. Except, perhaps, for an added presumption of instantaneous interaction between two physically separated locations in space, there were no further specifications as to what might be taking place in the space between the objects. In the course of time, Coulomb and Ampère were destined to add new examples testifying to nature's ability to act over distance without the need for an explicitly observable physical contact.

When the development of Maxwell theory taught us to identify the Coulomb and Ampère forces as near-field manifestations of a general, time-delayed, radiation field, it was bound to affect the earlier thesis of action at a distance. For the Coulomb and Ampère forces, at least, new properties were identified, yet no mechanism of how an action at a distance might come about. Maxwell added the feature that action at a distance might not be instantaneous, but instead takes time to propagate from one place to another.

Later, Einstein's suggestion for the existence of gravitational waves indicated that a similar situation might also prevail for the action at a distance associated with gravity. Hence from a thesis of pure instantaneous action at a distance, without further concern of how it comes about, the presently prevailing opinions hold that also these near-field manifestations take time to get from one place to the other.

At this point, no further questions shall be pursued as to how an instantaneous or delayed action at a distance could conceivably be understood. Instead of speculating in terms of more explicit concepts of a pseudo contact interaction, say, in terms of invisible or imponderable exchange objects, attempts at trying to know more about the force properties seem more appropriate. Hence, prior to lifting such ideas out of the realm of speculation into the realm of physical reality, it is better to stay closer to more realizable objectives by first optimizing assessable knowledge about force behavior.

Man's tendency to escape the realities of the present, by taking flight into a realm of wild, though, interesting imagination, is naturally restricted if there is a lack of tools for navigating those uncharted waters. As a case in point, questions could be asked whether or not the object-field interaction depends on the mutual motion of the object and field source. If an orbiting charged particle is taken to create a magnetic moment, does that particle interact with the magnetic field creating that moment? Normally, it is presumed to have no effect, unless the particle itself is taken to have a magnetic moment.

It is possible to address the situation in a different manner and rephrase the concomitant question. The Coulomb law is originally viewed as an interaction between two stationary charges. On what basis are we allowed to assume that this force interaction is not affected by a mutual motion of those charges? It can indeed hardly be denied that such extrapolations are in part untested. At best, conclusions obtained therefrom can, in retrospect, establish a viability of such an extrapolation.

In the following, the pseudo-planetary situation envisioned by the structure of hydrogen or hydrogen-like atom is to be analyzed for a number of variations of the assumed Coulomb interaction potential. Subsequently, these variations are subjected to the dictates of relativity, leading to, all told, four major cases. Two of these cases lead to nearly indistinguishable copies of the famous hydrogen fine structure, leaving some open questions as to what theoretical approach is most favored by the available experimental observations.

The mathematical machinery used for the analysis is that of Hamilton-Jacobi. This procedure remains user-friendly in relativistic as well as in nonrelativistic cases, because, as essentially shown by Einstein,[2] the Hamilton equations of motion express the fact that the spacetime differential one-form of energy-momentum is **closed.** The fact that this one-form is **closed and not exact** leaves open the possibility of nonzero periods (*e.g.*, residues) for its period integrals (chapter 6; 2)

In the just mentioned paper, Einstein[2] also points out how the Bohr-Sommerfeld choice of azimuthal and radial cyclic quantized phase integrals coincides with the choice of two period integrals as basis elements characterizing the topological features of a toroidal-type orbital manifold. The spacetime general invariance of the Hamilton-Jacobi process is hereby secured, because the property of a spacetime one-form of being closed is a spacetime topologically invariant feature.

From this point on, let us theoretically experiment with different types of potential functions, which are then combined with relativistic and nonrelativistic kinematic potentials. It does not matter too much if the Lagrangian combinations of the two transformationally incompatible potentials cannot all be integrated into a coherent spacetime, generally invariant process. The generally invariant process merely reduces to a subgroup invariance that may still appear to be of practical relevance.

For the sake of easy reference and for retaining a convenient overview, it is appropriate to start out with the classic case initiated by Bohr for circular orbits and later extended to elliptic orbits by Sommerfeld, Wilson, Epstein and others.[3] Since this procedure is based on an assumption, which extrapolates the static Coulomb potential unchanged to orbital situations, let it be referred to as the kinematically uncorrected Coulomb process.

3. Case I, Coulomb Potential Nonrelativistic

While this example is elaborately treated in many textbooks, here are some appropriate highlights for the purpose of subsequent reference. The Lagrangian function L is here defined as:

$$L = \frac{1}{2}mv^2 - U, \tag{1}$$

whereby m=mass of electron; v=velocity

$$v^2 = \dot{r}^2 + r^2\dot{\phi}^2 \quad \text{with r and } \phi \text{ polar coordinates,} \tag{2}$$

$$U = -k/r, \quad \text{the static Coulomb potential,} \tag{3}$$

$k = e^2$ the electronic charge squared in cgs units and $k = e^2/4\pi\epsilon_0$ for MKS units, possibly multiplied by the nuclear charge number Z. The explicit evaluations are here done for the hydrogen case; the nuclear charge number is therefore taken as Z=1.

The generalized momenta are obtained by using the lagrangian definition $p_\lambda = \partial L/\partial q^\lambda$. They thus lead to the following explicit expressions:

$$p_r = m\dot{r}\,, \qquad 4$$

$$p_\phi = mr^2\dot{\phi}\,, \qquad 5$$

with the help of the Legendre transformation H= pv - L, one finds for the Hamilton energy expression:

$$H = \frac{1}{2}mv^2 + U, \qquad 6$$

which, after eliminating v with the help of Eqs.4 and 5, can be converted into the Hamilton-Jacobi form :

$$p_r^{\,2} + r^{-2}p_\phi^{\,2} = 2m(H - U). \qquad 7$$

Since L does not depend on ϕ, the momentum p_ϕ is a constant of the motion and is taken to obey the Bohr-Sommerfeld residue condition

$$\oint p_\phi d\phi = lh, \qquad 8$$

so for a 2π orbit $\quad p_\phi = l\hbar$, with $l = 0,1,2,3.......$ 9

The momentum p_r is not a constant of the motion, hence here the Bohr-Sommerfeld integral has to be explicitly evaluated. Substituting Eqs.3 and 9 into 7 and solving for p_r yields the other Bohr-Sommerfeld integral

$$\oint p_r dr = \oint \pm\sqrt{2mH + \frac{2mk}{r} - \frac{(k\hbar)^2}{r^2}}\,dr = nh;\ n = 1,2,..... \qquad 10$$

For this cyclic integral the + sign of the integrand is taken for the out going loop and the - sign of the integrand for the return part of the loop, thus closing the cycle. This integral, and the following we shall encounter for the other three cases, are all of the following type, for which the magic of complex integration[4] gives the solution given by Eq.11:

$$\oint \pm\sqrt{A + \frac{2B}{r} - \frac{C}{r^2}}\;dr = 2\pi\left(-\sqrt{C} + \frac{B}{\sqrt{-A}}\right);\ A<0, B>0, \text{and } C>0;\ 11$$

For case I, presently under consideration, comparison with Eq.10 gives

$$A = 2mH;\ B = mk;\ C = (l\hbar)^2 \qquad 12$$

Substitution of Eq. 12 in Eq.11 gives for Eq.10

$$n\hbar = -l\hbar + \frac{mk}{\sqrt{-2mH}}\,, \qquad 13$$

and, after solving for H and writing $\alpha = k/\hbar c$ as the fine structure constant, one obtains the familiar energy expression given by Eq.14.

$$H = -\frac{1}{2}(mc^2)\frac{\alpha^2}{n^2+l^2}\ , \qquad 14$$

in which n and l are the radial and azimuthal quantum numbers, m is the electron mass, c the vacuum speed of light and α is defined by:

$$\alpha = e^2/\hbar c\ (cgs) = \alpha = e^2/2h\sqrt{\mu_0/\epsilon_0}\ (MKS) = 1/137{,}036) \qquad 15$$

There is no fine structure!

4. Case II, Coulomb-Weber Potential Nonrelativistic

Phipps[5] recently has called attention to a kinematic correction of the Coulomb potential suggested by Weber as early as in 1834. Weber also is known for discovering that the vacuum speed of light is related to the ratio of the electrostatic and magnetostatic units of charge. Writing down the Lagrangian for the hydrogen atom and taking into account a Weber correction we may consider the modification

$$L = \frac{1}{2}mv^2 - U\left[1 - \frac{1}{2}(v/c)^2\right], \qquad 16$$

in which U is again the static coulomb potential.

Following the standard rules of Hamiltonian mechanics the generalized momenta are now

$$p_r = (m + U/c^2)\,\dot{r}\ , \qquad 17$$

$$p_\phi = (m + U/c^2)\,r^2\dot{\phi}\ . \qquad 18$$

With the help of the Legendre transformation one obtains from Eqs.16, 17 and 18 the Hamiltonian energy expression

$$H = \frac{1}{2}(m + U/c^2) + U\ ,$$

which, after eliminating v between Eqs.16,17 and 18, yields the Hamilton-Jacobi form:

$$p_r^{\,2} + r^{-2}p_\phi^{\,2} = 2(m + U/c^2)(H - U) \qquad 19$$

Since p_ϕ is a constant of the motion, the Bohr-Sommerfeld condition permits us to use Eq.8. For the radial Bohr-Sommerfeld condition we now have to solve the integral

$$\oint p_r dr = \oint \pm\sqrt{2(m + U/c^2)(H - U) - (l\hbar)^2 r^{-2}}\,dr = nh\ . \qquad 20$$

Now substituting for U the electrostatic value given by Eq.3, the expression under the root sign of Eq.20 is again of the form given by Eq.11, but now with the different A,B,C values

$$A = 2mH;\ B = k(m - H/c^2);\ \ C = (l\hbar)^2 + 2(k/c)^2 = (\hbar)^2\,(l^2 + 2\alpha^2) \qquad 21$$

Eq.11 now gives for H the equation

$$n + \sqrt{l^2+2\alpha^2} = \frac{k(m-H/c^2)}{\sqrt{-2mH}} . \qquad 22$$

In the the previous case, by squaring this analogue of Eq.13, a linear equation in H was obtained. This time, because of the H term in the numerator, one obtains, when writing N for the left hand side of Eq.22, the following quadratic equation in H:

$$H^2 - 2mc^2(1 - N^2/\alpha^2)H + m^2c^4 = 0 \qquad 23$$

with roots:

$$H_{1,2} = mc^2(1 - N^2/\alpha^2) \pm mc^2(1 - N^2/\alpha^2)\sqrt{1-(1-N^2/\alpha^2)^{-2}}. \qquad 24$$

Rejecting the root associated with the plus sign as injected by the process of squaring, the remaining physically significant root for $m \gg H/c^2$ already is evident from Eq.22:

$$H \approx -\frac{1}{2}mc^2 \frac{\alpha^2}{(n+\sqrt{l^2+2\alpha^2})^2} . \qquad 25$$

Comparison with Eq.14 of case I shows that n and l now have slightly different contributions to the energy states, indicating the existence of a spectroscopic fine structure. A subsequent comparison with the now following relativistic cases will show that ***the fine-structure separation given by the term formula Eq.25 is twice as big as the one spectroscopically observed.***

5. Case III, Sommerfeld's Calculation

This calculation is one of the first applications of relativistic mechanics to atomic structure. In Sommerfeld's procedure a static Coulomb potential is combined with a relativistic correction of the kinetic energy part. One thus obtains for the Lagrangian:

$$L = -mc^2\sqrt{1-(v/c)^2} - U . \qquad 26$$

The corresponding generalized momenta are found to be

$$p_r = \frac{m\dot{r}}{\sqrt{1-(v/c)^2}}, \qquad 27$$

$$p_\phi = \frac{mr^2\dot{\phi}}{\sqrt{1-(v/c)^2}} . \qquad 28$$

The same Legendre tranformation gives now for the Hamiltonian energy expression:

$$H = \frac{mc^2}{\sqrt{1-(v/c)^2}} + U. \qquad 29$$

Eliminating v, the Hamilton-Jacobi equation becomes

$$p_r^2 + r^{-2} p_\phi^2 = \frac{(H-U)^2 - m^2c^4}{c^2} \qquad 30$$

The azimuthal Bohr-Sommerfeld condition yields $p_\phi = l\hbar$, which gives for the radial Bohr-Sommerfeld condition the integral

$$\oint p_r dr = \oint \pm \sqrt{\frac{(H-U)^2 - m^2c^4}{c^2} - \frac{(l\hbar)^2}{r^2}}\, dr = nh\,.$$

31

Taking for the Coulomb potential -k/r, the A,B,C expressions for this integral are

$$A = (H/c)^2 - (mc)^2\ ;\ B = kH/c^2\ ;\ C = \hbar^2\,(l^2 - \alpha)^2\,. \qquad 32$$

Substitution in Eq.11 and solving for H gives ***the presently standard fine structure formula***

$$H = \frac{mc^2}{\sqrt{1 + \dfrac{\alpha^2}{(n + \sqrt{l^2 - \alpha^2})^2}}}\,.$$

33 Note that Eq.33 does not permit so called s orbits "through" the proton, because l=0 leads to complex values for H.

6. Case IV, Combining Weber Correction and Relativity

In the same paper in which Phipps[5] calls attention to the 1834 Weber potential-energy correction, he proposes to rewrite the combination of Coulomb and Weber term as

$$U[1 - \tfrac{1}{2}(v/c)^2] \rightarrow U\sqrt{1-(v/c)^2}\ . \qquad 34$$

Together with a relativistic kinetic-energy term, the Lagrangian now assumes the form

$$L = -(mc^2 + U)\sqrt{1-(v/c)^2}\,, \qquad 35$$

which leads to the following energy-momentum expressions:

$$H = \frac{mc^2 + U}{\sqrt{1-(v/c)^2}};\ p_r = (H/c^2)\,\dot{r};\ p_\phi = (H/c^2)\,r^2\dot{\phi}\,. \qquad 36$$

Unlike the Eqs.27-29, the Eqs. 36 indicate by contrast that the particle and field components of energy-momentum are equally affected by the Lorentz-type term $\sqrt{1-(v/c)^2}$. This removes a transformational dichotomy remaining in the Sommerfeld-Dirac choice of the Lagrangian Eq.26.

Eliminating v between Eqs.36, the Hamilton-Jacobi form becomes:

$$p_r^2 + r^{-2} p_\phi^2 = \frac{H^2 - (mc^2 + U)^2}{c^2}\,, \qquad 37$$

with $p_\phi = l\hbar$ a constant, the radial Bohr-Sommerfeld condition is now

$$\oint p_r dr = \oint \pm\sqrt{\frac{H^2-(mc^2+U)^2}{c^2}-\frac{(l\hbar)^2}{r^2}}\,dr = nh\,. \qquad 38$$

Substituting for U the electrostatic value - k/r gives here the corresponding A,B,C coefficients:

$$A = (H/c)^2 - (mc)^2\,;\ B = mk;\ C = \hbar^2\,(l^2 + \alpha^2) \qquad 39$$

Substitution in Eq.11 and solving for H gives the ***alternate term formula for fine structure*** Eq.40

$$H = mc^2\sqrt{1-\frac{\alpha^2}{(n+\sqrt{l^2+\alpha^2})^2}}$$

40 Unlike Eq.33, Eq.40 permits the so-called s orbits "through" the nucleus if the azimuthal quantum number l is taken to be zero.

Case IV has not previously been the subject of a Dirac equation approach. The following may suffice to dispel lingering doubts about the relevance of this spinless semi-classical treatment. A Dirac analysis based on Eq.37 indeed reproduces also the revised term formula Eq.40. (Similarly as in the original Sommerfeld-Dirac transition, spin and orbital moment are taken to be additive. Spin is introduced by the quantum number transition $l \rightarrow j = l \pm \frac{1}{2}$. Details of the Dirac procedure, as modified by the Weber-Phipps Lagrangian, are given in section 8 of this chapter.

7. Discussion

The here-undertaken project of reconsidering motion-induced force corrections did in fact emerge as a prerequisite, or as a byproduct if you will, of an intended investigation of force corrections due to proximity effects between interacting objects. Since object motion is enhanced by object proximity, it would appear essential to acquire, if at all possible, independent information of these two effects. On the grounds that motion-induced force corrections constitute a common element of classical and nonclassical theory, a classical approach not only is appropriate, but even necessary. Methodological convenience then prompted the use of Bohr-Sommerfeld conditions for obtaining quantum information, which in case IV, has been reconfirmed by Dirac methods (compare section 8).

The major point submitted to reconsideration in this investigation is the question: should particle and field energy-momentum be taken to follow the same transformation rules? While offhand many of us might be inclined to confirm this step, the fact is that, by the same token, many of us have, in the course of time, accepted exemptions to this rule. Earlier this century, nobody less than Sommerfeld and Dirac have tacitly exempted field energy-momentum from motion-induced changes. Subsequently, the textbook literature consolidated this exemption without much further justification.

Even when taking into account a possibly hidden undisclosed logic justifying said exemption, after having been alerted to this situation, a reconsideration is presently called for and should be one of respectfully submitting this matter to further scrutiny. Mindful that the recognized hybrid nature of the energy-momentum one-form might limit the solidity of a very hard-nosed transformational argument, the method of discussion here adopted deemphasizes spacetime transformation and uses instead a form of theoretical experimentation. Let us consider, in this spirit, the four cases treated here by weighing virtues against inadequacies.

A comparison of the cases I and III has led in the literature to a diversity of lore, or convention, if you will. For $n \neq 0$, an azimuthal quantum number $l=0$ yields in case I, a well-defined energy state for Eq.14, even though $U \rightarrow \infty$. Yet. the fact is that Eq.33 of case III yields, under the same circumstances, a complex energy state. Some may even regard this behavior difference for the term formula Eqs.14 and 33 as a sign of inner wisdom of the relativistic approach; it knows how to avoid *pendle* orbits going through the nuclear center.

However, while the just-given rationale might suit the relativists, the fact is that nonrelativistic Schroedinger treatments welcomed the nonclassical equivalent of a classical *pendle* orbit as a rather feasible proposition. *Pendle* orbits were said to be typically "nonclassical" characterizations identified as "s-states." Spectroscopic observations subsequently revealed slight energy shifts of these suspected *pendle* orbits. Using radar technology, the shifts were then measured with great accuracy by Lamb *et al.* The first successful calculations of this Lamb shift associated this phenomenon with perturbations in the Coulomb potential due to electron-proton proximity. The ψ functions of these so-called s-states, at the origin, have been crucial for calculating those energy perturbations!

The conceptual relation between fine structure and Lamb shift, although still obscure, thus might be said to be slightly improved if motion-induced corrections are taken into account according to Eq.40. In addition the *pendle* orbit $l=0$ now again is a physically feasible proposition. Yet it raises new questions. The absence of an orbital moment for the s-state requires a proximity shift offsetting the vanishing spectral shift due to spin. Whether a remaining net effect accounts for the Lamb shift needs investigation. An empirical proximity proposition can, in principle, always meet such objective!

When seen in a classical context, the fine structure relates to a precession of elliptical orbits. Since case III dictates an advancing precession, it follows that the precession for case II is retrograde at twice the rate at which case III precesses in forward direction. For case IV, the relativity correction of the kinetic energy and the motion-induced potential energy corrections are both taken into account. Since the potential energy correction gives a precession at twice the rate of the relativity correction, the remaining net precession is retrograde at the same rate at which the relativity precession of case III advances in opposite direction. The fine-structure separation presumably gives only information about the magnitude, but not about sign of the orbital precession.

Where do we now stand? Further conclusions ought to wait for Lamb shift evaluations based on proximity corrections, possibly in conjunction with omitting spin-orbit level shifts for the s-states. For a more complete picture of the fine structure of hydrogen-like atoms, we discuss in section 8 whether the Dirac approach for the Weber-Phipps Lagrangian indeed confirms Eq.40. In section 10, the Pauli approximation is reviewed. Its agreement with experimental observation is numerically superior by considering the co-moving nucleus and the electron's anomalous moment. Finally, a possible lifting of the degeneracy between s and p states according to Eq.40 is reexamined in section 11 from the point of view of the Pauli approximation, because, unlike the Sommerfeld and Dirac procedures, the Pauli approximation permits us to deal explicitly with the spin-orbit level shifts due to the electron's magnetic moment anomaly and their expected effect on s and p states.

8. Dirac Approach for Weber-Phipps Lagrangian

Since the Dirac and Schroedinger equations are transcripts of the Hamilton-Jacobi equations, it follows that the Dirac equation, on the basis of the Sommerfeld-Dirac Lagrangian, should tie in with Eq.30 (*i.e.*, case III in the previous section 6).

$$p_r^2 + r^{-2} p_\phi^2 = \frac{(H-U)^2 - m^2c^4}{c^2} \qquad \text{(Dirac).} \tag{30}$$

It then follows that the Dirac equation modified by the Weber-Phipps Lagrangian would have to correspond with case IV treated in section 7, thus leading us to Eq.37:

$$p_r^2 + r^{-2} p_\phi^2 = \frac{H^2 - (mc^2+U)^2}{c^2} \qquad \text{(modified)} \tag{37}$$

A comparison of Eqs.(30) and (37) shows that the structural differences between the two are solely in the right-hand members; in Eq.(30), U combines with H; in Eq.(37) U combines with mc^2. Yet both right hand members are differences of squares, which is important for the Dirac decomposition.

The wave equations for the radial components of the two component spinor functions F and G are, according to Dirac:[6]

$$\left(\frac{d}{dr}+\frac{j+1}{r}\right)F=\frac{H-U-mc^2}{\hbar c}G \qquad (30a)$$

$$-\left(\frac{d}{dr}+\frac{j-1}{r}\right)G=\frac{H-U+mc^2}{\hbar c}F \qquad (30b)$$

In Eqs. 30a&b, j is the azimuthal quantum number, which is here taken to account for orbital and spin moment. The eigenvalues H of these two equations are given in reference 5. They are found to reproduce **Eq.33** if the azimuthal quantum number l is replaced by the spin orbit number j, which is taken to assume values $l\pm1/2$.

Making the transcription from Eq.37 instead of Eq.30, we need to account for the change in the right-hand member, which leads to a pair of radial differential equations slightly different from the previous pair:

$$\left(\frac{d}{dr}+\frac{j+1}{r}\right)F=\frac{H-U-mc^2}{\hbar c}G\,, \qquad (37a)$$

$$-\left(\frac{d}{dr}+\frac{j-1}{r}\right)G=\frac{H+U+mc^2}{\hbar c}F\,. \qquad (37b)$$

Now we need to check whether or not the eigenvalues H of Eqs. 37a &b indeed reproduce the term formula Eq.40. Following the same procedure as outlined by Dirac[6] for the previous case, we make the substitutions:

$$F(r)=r^{-1}e^{-r/a}f(r) \text{ and } G(r)=r^{-1}e^{-r/a}g(r)\,, \qquad 41$$

in which the parameter a in the exponentials can be chosen for later algebraic convenience. For the functions f and g, we obtain the differ-erential equations:

$$\left(\frac{d}{dr}-\frac{1}{a}+\frac{j}{r}\right)f=\left(A+\frac{\alpha}{r}\right)g \qquad 42a$$

$$\left(\frac{d}{dr}-\frac{1}{a}-\frac{j}{r}\right)g=\left(B+\frac{\alpha}{r}\right)f\,, \qquad 42b$$

in which $$A=\frac{mc^2-H}{\hbar c} \text{ and } B=-\frac{mc^2+H}{\hbar c}\,, \qquad 43$$

and for U, the central field value for electron and proton charges -e and +e have been substituted. They are here taken in cgs units, so as to retain alignment with reference 6. Hence

$$U = -e^2/r \tag{44}$$

and the cgs version of the fine structure constant then is

$$\alpha = e^2/\hbar c. \tag{45}$$

Power series solutions of Eqs. 42

$$f(r) = \sum c_s r^s \quad \text{and} \quad g(r) = \sum c'_s r^s \tag{46}$$

lead to the recursive relations:

$$(s+j)c_s + c_{s-1}/a = Ac'_{s-1} + \alpha c'_s \tag{47a}$$

$$(s-j)c'_s + c'_{s-1}/a = Bc_{s-1} + \alpha c_s \tag{47b}$$

Standard regularity conditions for wave functions require f and g to be polynomials, thus implying for s a lowest number s_0 where the series start and a highest value where they terminate. For c_{s_0} and c'_{s_0}, the lowest order nonzero terms in the power series 46, c_{s_0-1} and c'_{s_0-1} are zero. Hence Eqs. 47 reduce to

$$\alpha c'_{s_0} - (s_0+j)c_{s_0} = 0 \tag{48a}$$

$$(s_0-j)c'_{s_0} - \alpha c_{s_0} = 0, \tag{48b}$$

the compatibility of which yields:

$$s_0^2 = j^2 + \alpha^2 . \tag{49}$$

Let the power series Eq.46 terminate for some power, say s-1, the coefficients c_s and c'_s then vanish and the Eqs.47 yield a relation which determines the parameter α as:

$$AB = \alpha^{-2} . \tag{50}$$

Eliminating from Eqs.47 the coefficients completely with the help of these relations, one finally ends up after some algebra with the expression:

$$2s = \alpha \frac{A+B}{\sqrt{AB}} \tag{51}$$

Substituting the expressions Eq.43 defining A and B one finds, after some further algebra, the expression, where s (instead of s-1) has now been taken to be the number for which the series terminate

$$H = mc^2 \sqrt{1 - \frac{\alpha^2}{s^2}} .$$

52 Since s has to equal s_0 (defined by Eq.49) plus an integer, say n, we have

$$s = n + \sqrt{j^2 + \alpha^2}\,, \tag{53}$$

substitution of which in Eq.52 leads to a the term formula Eq.40a, equal to Eq.40 after replacing orbital number l by j.

$$H = mc^2 \sqrt{1 - \frac{\alpha^2}{(n + \sqrt{j^2 + \alpha^2})^2}} \tag{40a}$$

In consideration of their complexity, one should never cease to be amazed that two methods as different as the Bohr-Sommerfeld and Dirac procedures, end up giving identical end results. For some reason, one feels they should have a simpler and physically more transparent common denominator that somehow has not yet reached our awareness.

9. The Pauli Approximation

It is a near-contradictory feature of the Dirac procedure that its identification of the electron spin does not lead to an explicit electron magnetic moment in the result for the energy levels as given by the fine structure formula Eq.40a. Spin, in this final result, has become a rather latent feature. The question now arises how to account in the Dirac process for the observed and well established anomaly in the electron's magnetic moment $(e/m)\hbar/2 \rightarrow (e/m)(1 + \alpha/2\pi + ...)\hbar/2$. Since Eq.40a yields only even powers of α, it must be judged to be at a loss to account for these matters, because the first and biggest electron moment anomaly term already gives an odd power of α.

In the Dirac procedure an explicit magnetic moment term only appears when an external magnetic induction is imposed. Seen in this light, the reason for this seemingly contradictory feature has to do with the fact that, in the normal fine structure, the electron's magnetic moment interacts with the magnetic induction generated by its own orbital motion. The Weber-Phipps potential can be regarded as a striking example, producing a kinematically generated magnetic induction. Yet, as detailed in the previous sections, an analysis of its fine structure revealed twice the observed splitting unless the calculation is combined with a relativity induced splitting.

The history of fine structure is replete with factors "2." A very ingenious method for tracking down such factors has been discussed by Thomas.[7] By dynamically composing two Lorentz transformations, which is "slightly" outside the domain of the Lorentz group, he could identify a (Thomas) precession and an associated 1/2 presently known as the Thomas factor. Instead of using relativity and/or the Dirac process for trimming down the double Weber-Phipps splitting, one could use the slightly more apocryphal Thomas factor for obtaining the same result; after all, all three options (Bohr-Sommerfeld, Dirac and Thomas) have a relativity origin.

However, instead of chasing factors 1/2, which are hereby identified as more or less of relativistic origin, we should now be chasing the much smaller factor $\alpha/2\pi$, due to the moment anomaly. To do so, we first need to identify the role of the electron magnetic moment and then we make it explicit by rewriting the Hamiltonian and rederiving the fine structure. Pauli[8] has done exactly this by injecting a magnetic dipole term in the Schroedinger equation and calculating its impact with the help of perturbation theory. The results of these calculations are consistent with the relativity based calculations for powers of α^2 and α^4. Without going through the details of a perturbation calculation one can fairly easily verify these matters for the principal quantum number n=2.

If the energy contribution due to the magnetic moment is taken to be μB (μ is the magnetic moment and B is the effective magnetic induction generated by the orbital motion), the Dirac value for μ is $(e/m)\hbar/2$ and $B=-(v/c^2)E$ if E is the local value of the electric field $E=k/er^2$, with k defined as e^2 cgs units or $e^2/4\pi\epsilon_0$ in MKS. Using the familiar data of a Bohr circle orbit the level shift can now be written as

$$\mu B=(e/m)(\hbar/2)\,(v/c^2)(k/e)r^{-2} = (\hbar/2m)\omega m\,(\alpha/n)^2 \quad .$$

Bohr orbit calculations give $\omega=mk^2/(n\hbar)^3$; hence substitution gives as energy jump associated with the spin flip

$$W=2\mu B= \; mc^2\,\alpha^4\,n^{-5} \; . \qquad 54$$

Eq.40 gives for n the principal quantum number and l the azimuthal quantum number, when keeping n constant, as energy jump W' associated with the transition $l\rightarrow l-1$ the expression

$$W' = \frac{1}{2}\,mc^2\,\alpha^4\,\frac{1}{n^3 l(l-1)} \qquad 55$$

Eqs.54 and 55 give equal energy jumps $W'=W$ for n=2, a fact which may here be taken as supporting evidence for a spin flip mechanism underlying the Dirac term formula. Using recent data for the fundamental constants, its numerical value is:

$$\nu= W/h = 10949.27 \text{ Mc/s}. \qquad 56$$

The observed values for hydrogen and deuterium are:[9]

$\nu_H= 10968.52$ Mc/s observed

$\nu_D= 10971.59$ Mc/s observed 57

The electron's anomalous moment and the comoving nucleus can in first order be accounted for by multiplying Eq.56 with the corrective factor $(1+\alpha/\pi - m/M)$, one thus obtains:

$\nu_H= 10968.73$ Mc/s calculated

$\nu_D = 10971.72$ Mc/s calculated 58

A comparison of the observed and calculated data shows fair agreement despite the diversity of theoretical input which is a patch work of Dirac theory, QED, relativity and the classical mechanical correction m/M; the two body nature of the latter cannot easily be made to fit well the principles of relativity.

In fact, closer agreement covering the seventh decimal place still is possible by injecting the electron's measured anomaly data listed on p.111. Additional corrections due to higher powers of α^2 may be expected to be slightly different for the Dirac term formula and the Weber-Phipps-modified Dirac term formula. Hence, an even more incisive comparison of theory and observation would be necessary to determine whether or not the Weber-Phipps modification gives a closer fit. At this point, another fine-structure feature needs to be considered. It has become known as the Lamb shift. This phenomenon testifies about new seemingly never-ending complexity into the workings of the simplest atom.

10. Remaining Lamb Shift Problems

In the early Thirties, the spectroscopic exploration of the hydrogen fine structure revealed some very small discrepancies, casting doubts on a complete agreement with Dirac theory. The mutual positions of the s and p levels became the object of suspicion that led ultimately to a further scrutiny. The question was asked whether transitions between the fine-structure levels could be observed directly without changing the principal quantum number. After the development of radar technology made oscillators available in the microwave range, Lamb[10] and co-workers succeeded in measuring directly a finite frequency interval of 1062 Mc/s between the $2s_{\frac{1}{2}}$ and $2p_{\frac{1}{2}}$ levels of hydrogen.

From a point of view of the standard Dirac theory, without the Weber-Phipps modification, the two cited levels were believed to be degenerate. An inspection of Eq.33 shows why. The quantum number l cannot be allowed to vanish, because it would lead to complex energy states. The so-called s-states of the Schroedinger equation were somewhat tacitly taken to correspond to $j = l \pm s$ with $l=0$ and $s=+1/2$ versus $l=1$ and $s=-1/2$. Hence, the $s=+1/2$ value for $l=0$ rescues the energy from becoming complex.

The just-presented argument is no longer compelling in the light of the Dirac theory, as modified by the Weber-Phipps Lagrangian. Eqs.40 and 40a no longer need the $s=+1/2$ to rescue the energy from becoming complex. It now is possible to consider s-states in the sense as originally envisioned for the Schroedinger equation by taking $l=j=0$. If, for s-states, the orbital magnetic moment reduces to zero, how, in the perspective of the Pauli approximation, could a finite spin term be justified?

Yet, another feature of the Dirac theory, as modified by the Weber-Phipps Lagrangian, needs to be mentioned. For Eq.40, the $l=j=0$ option lifts the earlier cited degeneracy for the original Dirac theory governed by Eq.37. The s-states now exhibit substantial shifts.

$$H = mc^2\sqrt{1-\frac{\alpha^2}{(n+\alpha)^2}}\ ,$$

as compared to the nearest p-state

$$H = mc^2\sqrt{1-\frac{\alpha^2}{((n-1/2)+\sqrt{(1/2)^2+\alpha^2})^2}}\ ,$$

the corresponding Bohr circular orbit without spin $l \rightarrow n$ is also lifted

$$H = mc^2\sqrt{1-\frac{\alpha^2}{n^2+\alpha^2}}\ .$$

In the light of the Dirac theory with the modified Lagrangian, the question may be asked whether the Lamb shift can now be regarded as a difference between a p-state shifted by spin and an s-state without spin. A numerical evaluation does not confirm this option and keeps pointing at a Coulomb proximity effect.

The standard QED process for obtaining the Lamb shift is, in essence, based on the calculation of an effective Coulomb distance of proximity δr by having the electron hop around as a result of interaction with the zero-point energy of vacuum. Since this energy would be infinite for an unlimited spectrum, QED provides a rationale for a finite spectrum interval $\omega_1 \rightarrow \omega_2$ to obtain a finite δr. The expression for its square average so obtained is given as

$$\langle \delta r\rangle^2 = (2\alpha/\pi)\,(\hbar/mc)^2 \ln(\omega_2/\omega_1). \qquad 59$$

Hence, instead of going to zero, the minimum distance between electron and proton is ∂r. This will affect the potential energy between the two. The ensuing energy level change δW is then calculated by perturbation theory, using the s-state wave function for the unperturbed state. This calculation then gives the observed shift of 1062 Mc/s.

These QED calculations have been a target of much criticism for many reasons. There remains a measure of arbitrariness in the selection of the effective spectrum interval. Furthermore, the perturbation calculation is performed near a singularity of the Coulomb field and uses the wave function of the Schroedinger s-state at the origin. This proximity-shifted Schroedinger s-state, presumably without spin-shift, is ironically then compared with a spin-shifted Dirac p-state so as to give the observed Lamb result.

Kramers,[11] who, in part, suggested some of the QED methods that have been used over the past decades, is known to have expressed an opinion that it would be very difficult indeed to make these procedures rigorous. So, after all this time, the hydrogen atom still is giving us troubles from the QED angle as well as from the wave-equation angle. The Lamb shift proves to be a much more complicated situation than the electron's anomalous moment. A model-based analysis of the latter, accounting for its principal features, invokes the modeling of only one particle; the Lamb-shift, by contrast, requires a modeling of two distinct particles.

A model-based Lamb-shift calculation needs to combine spin-orbital fine structure with a Coulomb proximity effect, plus a spin-spin electron-nucleus interaction for the s-levels. Hence the Lamb-shift, in principle, demands model specifications of two particles as well as a specification of the proximity behavior ensuing from the chosen models. While the idea that standard QED can do all this in a model-independent manner has a pleasing simplicity, the fact that this unusual result can be accomplished only by sacrificing a good measure of mathematical rigor and explicit physical transparency and precision remains a matter of concern.

CHAPTER XI

CLASSICAL NONCLASSICAL ASYMPTOTICS

1. Summary

Model-based experimentations with globally oriented classical and semi-classical methods have retained a viability in the microphysical realm, despite the prevalence of the abstract methods of contemporary quantum physics. In chapter VIII, these model-based methods were found to emphasize the electron's purely electromagnetic origin. While the Lorentz electron was purely electrostatic, the energy of the Dirac electron was purely magnetic, yet nature's electron, by contrast, appears to be a blending of the two. The joint presence of electric and magnetic energy was found to relate to the magnetic moment anomaly.

The electron's half spin, normally identified as a consequence of abstract local spin description, becomes, when seen from a global angle, a consequence of a forced 4π integration path inherent to the electron model, which is taken to be a trefoil.

From the angle of the trefoil model, the magnetic moment anomaly (not covered by Dirac theory) is found to consist of two contributions of opposite sign. In chapter VIII a positive component was already identified as due to reinstating the now residual role of its electrostatic energy. A much smaller, not earlier mentioned, negative anomaly component is due to a small flux leakage into an internally closed path, which detracts from the full flux unit h/e required by the Aharonov-Bohm integral. The trefoil's orbital knot here amounts to a meridional current component generating a toroidal flux. The latter closes on itself and detracts from exterior manifestation of the experimentally observable flux.

2. Introduction

To many people, the almost century-old transition from "classical" to "nonclassical" physics was felt as a gradual sacrifice of visualizable contact with the subject matter. Relativity brought a measure of discomfort of having to depict things in spacetime. Uncertainty in quantum mechanics deprived us of obtaining visualizable images of the microphysical world. Compounding the hurdles on the traditional road to cognizance is a widely spread sentiment that the cited restrictions are in the nature of things.

Man's innate limitations and his general position in the order of things are more and more accepted as frontiers beyond which advancement is bound to become difficult, if not impossible.

The increasing preference for formal descriptions over the seemingly futile attempts at visualization may be regarded as public testimony confirming the cited trend of giving up geometric modelling, where the human eye, and even its modern experimental extensions, can no longer see. During the time when such developments are in progress, it is always difficult to judge where the hurdles are real, and where they are mainly psychological. Hence in the process of educating a new generation in the fundamentals of physics, it can hardly be avoided that along with sound and solid knowledge, a few prejudices are also passed along from generation to generation. It is the prerogative of the new generation to identify those prejudices and replace them with better and more circumspect premises leaving more of a horizon for further explorations.

The here-presented material attempts (1) to identify prejudices and (2) to open up possible pathways that become available as a result. Preparing an audience to make the classical-nonclassical transition, instructors are frequently tempted to play up the classical versus nonclassical distinction beyond the point of necessity. In the following, we pinpoint some of these instances by determining the seemingly extended viability of some classical relations in the realm normally reserved for nonclassical methodology.

The exploration of this subject matter is done by examining the interplay between classical and nonclassical, covering relations in both domains. While, without question, many nonclassical methods deserve credit for first explorations in the microphysical realm, attempts at reproducing these results, after the facts, by simpler, more tangible procedures has its virtues, even if they are only taken as mnemonic devices.

3. An Historic Perspective on the Electron

More than any other particle, the electron has been the subject of classical and nonclassical theorizing. Lorentz tentatively saw the electron as a charged sphere of purely electrostatic energy. The Einstein mass-energy theorem then gave it its Lorentz radius of $r = e^2\mu_0/4\pi m$ in MKS units. To secure the stability of this system, Poincaré postulated the existence of stresses preventing such charge structures from flying apart.

The existence of a fundamental quantum of electronic charge had become more and more apparent in the course of the 19th century. Faraday's law of electrolytic deposit as being proportional to current, chemical valence and atomic weight was a first tell-tale sign. Later, findings with cathode ray tubes and Millikan's oildrop experiment further secured the knowledge about charge and mass of the electron.

More on grounds of history than on the basis of logical need has the existence of charge quantization been treated as an item of information belonging mostly in the realm of classical physics. Formally speaking, the notion of nonclassical physics started at the turn of the century, with Planck's introduction of a quantum of action. Once the first step had been made on the path of unconventional conceptualization, others followed and reached a culmination point in the birth of modern quantum mechanics in the late Twenties.

Bohr's theory of the hydrogen atom first related Planck's quantum of action to electron behavior. Normal and later unexpected anomalous electron behavior in magnetic fields led then to a decision to endow electrons with spin and magnetic moment, over-and-above the earlier identified charge and mass.

A few years later, these new electron attributes became the subject of a wave-equation description by Dirac. If anything in particular had to be mentioned as having further consolidated the classical/nonclassical distinction, it was the ensuing theory of spinors, which contributed a major share. In the Dirac theory spin and magnetic moment of the electron were magically produced by the imposition of utterly formal requirements, which seemed to have no bearing at all on any visualizable model of the electron. Insistence on Lorentz invariance and linearity in the energy-momentum parameters were the principal conceptual ingredients that led to this most amazing achievement in theoretical physics.

Dirac's theory was instrumental in bringing about, and in consolidating a philosophical change in the approach to physics that had been earlier promoted by Heisenberg. Rather than modelling geometrically what we could not see, the new attitude became more pragmatic and settled for describing in a logically consistent manner what could be observed. In the light of those experiences, it seemed wise to restrict theory to experimental observables in the sense of Heisenberg and then find rules Nature is willing to disclose for working with these observables. Dirac's theory remains one of the amazing monuments which demonstrates how effective such an approach can be.

Yet, depending on the situation at hand, also logical consistency has its limitations for getting to the truth. *In the past the electron had been identified by two characteristic dimensions: the Lorentz radius and the 137-times-bigger Compton radius. The new radicalism, which had been emerging with the Copenhagen interpretation, had the audacity of favoring a point-electron.*

No name is specifically associated with this plea for a point-electron. Everybody knew, deep in his conscience, that a bit of common sense had gone overboard. The idea sort of emerged anonymously as a tongue-in-cheek consequence of the Copenhagen statistical interpretation.

Even the radicals refrained from boldly claiming responsibility for this conceptual outrage. After more incisive inquiry, one finds a consensus to the effect that a point-electron is something one ought to be able to live with, at least for the time being. Where do we go from here?

Let us keep in mind that it may be better not to put all of our eggs into one basket. Granted, for more than half a century, the formal abstract method of approach held an edge over the more explicit modelling approach. Does that mean that the old-fashioned type of modelling is now beyond redemption and should be abandoned altogether? We shall find that some minor, or, depending on the mind of the beholder, not-so-minor changes in modelling can restore a mutually beneficial complementary exchange between modelling and abstraction.

4. An Assessment of Formal Equivalences

Let us compare, in this section, a number of familiar expressions from classical electromagnetic theory with the corresponding counterparts of nonclassical theory. They are expressions for magnetic moment, energy, magnetic flux and last, but not least, the Larmor theorem calling on a most important equivalence between rotation and magnetic induction. The relevant expressions are here given in terms of an MKS type unit rendition.

The MKS choice is not without reason, because, in the course of discussions, one finds that MKS renditions make some subtle distinctions that are missing for cgs. One major point is that MKS renditions don't have the vacuum speed of light c popping up in places where it is merely used as a factor serving the electrostatic and magnetostatic unit dichotomy within the cgs system. This type of factor c has no real functional role relating to the physics of the subject matter. Since, in the following, we like to see the forest as well as the trees, we do well taking advantage of MKS renditions, which have eliminated this c redundancy.

Let us consider the standard formula for the magnetic moment μ of a ring current J of radius r:

$$\mu = \pi r^2 J \, . \qquad 1$$

If e is the amount charge, all of which is circulating at the same rate ω, the current J can be defined as

$$J = e \, \omega / 2\pi \, . \qquad 2$$

Eq.2 inserted in Eq.1 gives an equivalent magnetic moment expression

$$\mu = (1/2) \, e \, r^2 \, \omega \, . \qquad 3$$

Eq.3 gives a magnetic moment rendition which is analogous to the definition of angular momentum L in mechanics

$$L = m \, r^2 \, \omega \, . \qquad 4$$

Eqs.3,4 give the familiar orbital gyromagnetic ratio $\mu/L = e/2m$.

All of the above is true for reasonably "slender" ring currents, which means the r is adequately defined so that e resides on the orbit; e does not have to be a point charge, though; it may be smeared out around the orbit.

Now let us do some experimenting by multiplying both sides of Eq.1 with an effective magnetic induction B generated by the current J:

$$\mu\ B = \pi r^2 B\ J\ . \qquad 5$$

Since $\pi r^2 B$ is by definition the flux Φ passing through the ring, Eq.5 simply illustrates a familiar equivalence:

$$\mu\ B = \Phi\ J\ . \qquad 6$$

On the left, Eq.6 could represent an interaction energy of μ with an external field B. However, since B is here supposed to be the B field created by μ itself, a factor 1/2 would be in order to give the electromagnetic field energy of the system. Eq.6 may be said to give the sum of field- and mechanical energy, which is in accordance with F. London's analysis of the superconducting ring; the kinetic energy of the charge carriers equals the magnetic field energy (see p.66 of ref.2 cited in chapter VI of this text).

Now let us experiment with the expression Eq.3 for the magnetic moment by using the Larmor theorem $\omega = (e/2m)\ B$. Substitution leads to

$$\mu = (1/2)\ e\ r^2\ (e/2m)\ B = (e^2/m)\ \pi r^2 B/4\pi\ . \qquad 7$$

Since $\pi r^2 B$ is the earlier-cited flux Φ through the ring, and knowing that flux to be quantized, one is now very much tempted to try this out on Eq. 7 and see where this leads to. Taking the flux unit to be h/e, we have

$$\Phi = \pi r^2 B = h/e\ . \qquad 8$$

Note that a unit $h/2e$, such as observed in superconducting rings, would be out of order, because the initial charge e has no companian. Yet the trefoil gives a pairing of charges $e/2$, replacing $e \rightarrow e/2$ in Eq.12 of chapter VI thus justifies Eq.13, chapter VI. So, substituting Eq.8 into Eq.7 yields

$$\mu = (e/2m)\ h/2\pi, \qquad 9$$

which is the familiar expression for the Bohr magneton and also the magnetic moment value ascribed to the electron by the Dirac theory. The experimentally observed values of the electron's magnetic moment have been found to be slightly bigger, a discrepancy that was later accounted for by quantum electrodynamics (QED).

The overall result, so far, is that flux quantization generates a familiar form of magnetic moment quantization. In other words, if an electron has a magnetic moment $(e/m)\hbar/2$, where we write $\hbar = h/2\pi$, then that same electron also carries a flux unit h/e.

There has always been a suspicion that something of this sort might be true. These very elementary considerations give us some added suspicion that such an assumption might be based on fact. Fluxes, unlike magnetic moments, must be examined in proximity; so chances for observation are slim. In evaluating the saturation magnetism of ferromagnetic materials, Chamberlain[1] *et al* have found some supportive evidence for the order of magnitude of these elementary fluxes.

Perhaps encouraged by these preliminaries let us go a step further and involve quantum and relativity concepts both. Consider hereto the energy relation

$$mc^2 = \hbar\omega \ . \qquad 10$$

Charge moving in a self-field invites using Larmor's theorem for ω. After comparison with Eq.9 one finds mc^2 equalling twice the magnetic field energy (see Eq.9 of chapter VI and Eq.1 chapter VIII)

$$mc^2 = \hbar\,(e/2m)\,B = \mu\,B. \qquad 11$$

According to Eq.6 , Eq.11 could be rewritten as

$$mc^2 = (1/2)(\mu B + \Phi J) = \Phi\,J. \qquad 12$$

Eqs.11 and 12 represent some sort of an invitation to resurrect Lorentz' electromagnetic electron, except that now the energy is all magnetic in origin. Lorentz' early model was purely electrostatic; after all, at that time, the magnetic spin properties of the electron had not yet been discovered.

If it is indeed permissible to consider, at least tentatively, the electron as an electrodynamically based structure, then there is the legitimate question: what happened to the electrostatic energy? Presumably one has to assume that the electron's energy is mostly magnetic in nature, with a tiny bit of electrostatic energy. As shown in the previous chapter, the electron's magnetic moment anomaly indeed relates to this very small component of electrostatic energy.

In all the experimentation with well-known formula that we have done so far, nothing has been said about the magnitude of r and the actual velocity ωr of charge circulation around the ring. It is, all by itself, interesting how much can be said about such electron-like ring currents without making any commitment at all about the r or ωr. Since ω is committed by Eq.10 a commitment about r is a commitment about ωr.

It would indeed be unusual if the very specific physical structure, which is called an electron, would have a completely indeterminate radius r. The velocity of an electron in the first Bohr orbit is known to be αc, where α= 1/137: the fine-structure constant. Should the peripheral charge velocity for the electron ring be bigger or smaller? Since the Compton wavelength $\hbar/mc$ was mentioned a long time ago as a possible candidate for r, let us see where that goes if we evaluate ωr for $r = \hbar/mc$. One finds, by using Eq.10,

$$\omega r = \omega\,\hbar/mc = mc^2/mc = c. \qquad 13$$

Our standard training in relativity makes the result of Eq.13 unacceptable.

Even so, there are here some good reasons for a reconsideration of this unorthodox result.

To bring this point in focus, it will be necessary to argue for the domain of the particle itself the existence of different kinematic rules for mass and charge. Mass is subject to the speed-of-light restriction of relativity. For charge "riding" on mass, the same restriction must apply. However, this conclusion no longer is valid for a particle's interior.

Charge is a primary concept and mass is a derived, secondary concept, because mass relates to, and follows from, the energy configuration of the fields associated with charge. This distinction acquires a fundamental importance when seen in the perspective of the metric-free (*i.e.*, no speed-of-light restriction) versus the metric-based aspects of electrodynamics.

It has remained a rather well-preserved secret that there exists a pre-metric form of electrodynamics. It was discovered shortly after the emergence of the general theory of relativity by Kottler and Cartan. Later the subject was much further developed by van Dantzig and the Dutch school of Schouten. While the special- and the general theory of relativity are both inextricably related to the existence of a spacetime metric field, pre-metric electrodynamics, by contrast, is metric-independent .

One wonders first what kind of physics could possibly relate to a metric-free spacetime; is not all physics inextricably tied up with metric concepts? The answer is that much, but not all, physics is inseparable from the metric. Many counting procedures of identical physical objects should, in principle, be free of metric considerations. There are many situations in which charge, flux and angular momentum states are, in principle, countable multiples of universal quantum units: *e.g.*, e, h/e, and h. Pre-metric spacetime, therefore, is a natural backdrop for the description of fundamental quantum phenomena.[2] Such counting with the help of period integrals is essentially a topological process (chapter VI).

Since the metric-free counting procedures referred to here are topological in nature, pre-metric general spacetime invariance is to be regarded as a sine qua non for topological implementation. When, in the early Twenties, the first discoveries were made about the metric-free invariance aspects of electrodynamics, the general theory of relativity was still fighting for a measure of experimental acceptance. Mindful of the mathematical complexity of the general theory, in which the metric held a most essential role, there was at that time little inclination to consider further metric-independent mathematical subtleties, which, at the time, had no immediate bearing on physical reality.

A descriptive account of this situation was given by Whittaker[3] some three decades later. His description clearly depicts the sentiment of that time, which still prevails today if one reads the commentaries on Einstein's principle of (metric-dependent) general covariance in the contemporary textbook literature on the general theory of relativity.

Yet Whittaker's account reaches a near prophetic level when he emphasizes the importance of global-type integral formulations in physics over the prevailing local methods based on solving differential equations. The frequently mentioned, but rarely incisively discussed, incompatibility between relativity and quantum mechanics can hardly be resolved without a balanced discussion of local and global methods in physics. By recalling these past contributions we aim at restoring in this manner a measure of balance between classical and nonclassical epithets.

5. Magnetic Forces as Poincaré Stresses

In the preceding historic perspective on the classical electron, Poincaré stresses were mentioned as the postulated agent preventing the Lorentz electron from blowing apart. When hardly a quarter of a century later, the electron was found to have magnetic properties, the then prevailing consensus showed little inclination to consider whether those added magnetic properties might yield a substantiation for the physical reality of the Poincaré stresses. A situation is discussed in this section, which indicates how electrostatic and magnetostatic forces can approach a balance.

The essence of the idea can be demonstrated with the help of two parallel electron beams; the electrostatic forces between the beams cause a repulsion and the magnetic forces generated by the beam currents are counteracting with an opposing attraction. The cited effect can be expected to play a role in the focusing designs for electron-beam devices. In the present context, the condition of balance between these forces is a point of major concern.

Consider a section of length l for the two parallel beams at distance r with $l >> r$. Let q be the charge per unit length, and let the charge move at the velocity v. The electric field E at a distance r becomes, according to Gauss' theorem, $q/2\pi\epsilon_0 r$ and the repelling Coulomb force F_c on the charge lq of the neighbor beam section becomes

$$F_c = l(q)^2/2\pi\epsilon_0 r \ . \qquad 14$$

The magnetic field H generated by the beam current J = qv at the position of the neighbor beam at distance r is, according to Stokes' law $qv/2\pi r$. The corresponding magnetic induction $B = \mu_0 H$. The mutual magnetic force F_m according to Ampère's law becomes $l\,J\,B$ or

$$F_m = \mu_0 l(qv)^2/2\pi r \ . \qquad 15$$

Equating the F_c of Eq.14 and the F_m of Eq.15 yields the not altogether surprising result

$$v^2 = (\epsilon_0\mu_0)^{-1}, \qquad 16$$

indicating that the balance between Coulomb and Ampère forces can only occur if v approaches the speed of light c.

The traditional relativity conscience is inclined to militate against the unorthodox result of Eq.16. How can charge reach the speed of light, without causing divergencies of the kind: $\lim. 1/[1-(v/c)^2] \to \infty$, for $v \to c$? Let it be said that in many applications, v comes very close to the speed of light. In such applications pertaining to particle behavior, mass is the inseparable companion of charge by virtue of the energy associated with any charge configuration. *Yet when we consider the mechanism of electromagnetic mass formation in the particle itself, it is unavoidable to consider differences in charge and mass kinematics.*

In reconciling these matters, it is necessary to consider the metric differences between mass and charge. Charge is a metric-independent quantity not subject to the limitations of relativity. In premetric electrodynamics, there is no basis, nor reason, to limit charge to sublight velocities. Mass, on the other hand, is a typically metric-based concept and is, by virtue of this fact, subject to metric restriction. It takes time before the different perspectives, brought about by premetric aspects, acquire a fair measure of maturity and acceptability in our thinking.

6. What Is A Slender Ring?

In the course of these discussions, a ring structure has been casually mentioned as accounting for the electron's observable magnetic moment. In an even more casual manner, the ring was taken to be a slender ring. Let us now test whether this assumption is reasonable by making a calculational estimate.

Consider hereto a "slender" ring of "effective" azimuthal dimension R and of "effective" meridional dimension r with r << R. Let the elementary charge e be circulating on the ring periphery in an azimuthal direction with the velocity v. The ring current is then

$$J = ev/2\pi R\,. \qquad 17$$

Now let the *effective* r be so defined that it determines an effective H in the ring interior with the help of the Ampère formula, because most of the flux is close to the tube radius r

$$H = J/2\pi r\,. \qquad 18$$

The flux through the ring may then be evaluated as

$$\Phi = \pi R^2 \mu_0 H. \qquad 19$$

Assuming the ground state of flux $\Phi = h/e$, one finds from Eqs. 17,18 and 19 the remarkable relation

$$(v/c) = (2\pi/\alpha)\ (R/r)\ , \qquad 20$$

where α is the fine structure constant defined by the impedance ratio

$$\alpha = \sqrt{\mu_0/\epsilon_0}\ /\ (2h/e^2). \qquad 21$$

If, as argued in the previous section, we make the extrapolation $v \rightarrow c$, the ratio of meridional and azimuthal radii becomes

$$r/R = \alpha/2\pi = 0.00116....\ , \qquad 22$$

which is very slender indeed.

7. Trefoil and Multi-foil as Lepton Models

The trefoil has been explored in chapter VIII as a potential model for the electron. The preliminary results showed a possibility of accounting for a positive correction $1+\alpha/2\pi$ of the anomaly of the magnetic moment. The measured value of the electron moment anomaly is slightly less. The measured muon anomaly, on the other hand, is slightly more than that calculated value $\alpha/2\pi$. Let us explore the chances of accounting for these higher order corrections with the help of a trefoil-type model.

Thus far the experimentation with formula in the preceding sections has shown how classical expressions retain a measure of relevance, even in domains for which our thinking has been conditioned not to expect their applicability. The Dirac electron so identified itself as an almost purely magnetic structure. It is then natural to restore at least some of Lorentz' electrostatic energy. If this is to be done, it should be done in a manner to secure a balance between electrostatic and magnetic forces. The first step to this effect is a generalization of Eq.12 by adding the electrostatic energy eV:

$$mc^2 = \Phi J + eV\ , \qquad 23$$

in which V is taken to be a surface potential of the ring and e is again the elementary charge.

The next assignment is finding a ring model that can account for a conceivable equilibrium between electric and magnetic stresses; a tubular ring already has a built-in feature to this effect. Yet, the ring has a disturbing center of symmetry. The existence of such a center must be regarded as incompatible with the principle of electron-positron pairing, because parity transformations (spatial inversion) are supposed to change electrons into positrons and vice versa.

The remaining option now is to physically endow the tubular ring surface with a property that its charge changes sign under inversion. This assumption is, however, not adequate to account for the so-called "local" half spin properties of the electron. A possible solution compatible with the electron's half-spin is a knotted double ring, which, in the mathematical literature, is known as a trefoil. In the context of particle theory, the trefoil has been introduced, in a related yet somewhat different manner, by the late Herbert Jehle.[4]

Since the trefoil has an azimuthal winding number 2 and a meridional winding number 3, the current J through the trefoil tube encircles twice the central hole and goes around three times in the meridional direction. The current J thus links the ring periphery three times and must be expected to generate a small closed flux ring Φ_r, which was neglected in the calculations of chapter VII. Since this flux closes on itself, it has no outward magnetic moment manifestation; it consequently subtracts from the principal flux component Φ_p going through the hole of the trefoil. The two flux components together add up to the fundamental flux unit linked by the Aharonov-Bohm integral.

Once topological features can be admitted as an element of particle structure, there are several options of accounting for anomalies with respect to the model-independent Dirac process of magnetic moment evaluation. Here is a survey of some possibilities that conceivably could have a wider bearing on reality if the trefoil model is extended into a "multi-foil" model.

Let J be the current in the single multi-foil tube circulating n times azimuthally and k times meridionally. The number pair (n;k) are the "multi-foil" winding numbers. Let Φ_p be the external flux component generated by the current J of this trefoil-like configuration manifesting a magnetic moment μ. Let Φ_r be the flux component that closes on itself encircled by the meridional windings of the multi-foil knot. Ignoring the small electro-flux component and assuming a total flux ground state h/e, the flux quantization laws would give:

$$\Phi_p + \Phi_r = h/e \qquad 24$$

Taking into account the multi-foil winding numbers (n;k), the energy balance assumes the form

$$mc^2 = \Phi_p nJ + \Phi_r kJ + eV, \qquad 25$$

in which (as before) the left-hand side represents the particle mass energy and the last term on the right-hand side represents the electrostatic energy associated with the multi-foil surface. Solving for J, one obtains:

$$J = \frac{mc^2 - eV}{n\Phi_p + k\Phi_r}\,, \qquad 26$$

or writing in accordance with the condition of Eq.24 and x<1

$$\Phi_r = xh/e \quad \text{and} \quad \Phi_p = (1-x)h/e \tag{27}$$

Eq. 26 becomes

$$J = \frac{e(mc^2-eV)}{h[n+(k-n)x]} \tag{28}$$

For n azimuthal windings nJ is now the current that generates the externally observable magnetic moment μ. Eliminating the geometric references from the magnetic moment formula, as done earlier in chapter VIII, Eq.4, gives the expression $e^2c^2/4\pi J$ in which J is the total circulating current. This expression should now be corrected for the azimuthal flux leakage by the factor (1-x). Hence replacing J by nJ and inserting the flux reduction factor (1-x), the magnetic moment is now

$$\mu = \frac{e^2c^2(1-x)}{4\pi nJ} \; . \tag{29}$$

Substitution Eq.28 for J gives as magnetic moment for the multi-foil of winding numbers (n;k) the expression:

$$\mu = \frac{e^2c^2(1-x)}{4\pi n} \frac{[n+(k-n)x]h}{e(mc^2-eV)}$$

or

$$\mu = \frac{e}{m} \frac{\hbar}{2} \frac{(1-x)[1-(1-k/n)x]}{1-eV/mc^2} \; , \tag{30}$$

which immediately assumes a familiar form for the electron, when $eV/mc^2 = \alpha/2\pi << 1$ and also $x<<1$. Taking into account these inequalities, the multi-foil magnetic moment for the general winding numbers n and k becomes

$$\mu = \frac{e}{m} \frac{\hbar}{2} \left\{1 + \alpha/2\pi + \left(\frac{k}{n} - 2\right) x\right\}. \tag{31}$$

Note how the anomaly term with x changes sign if, instead of the winding numbers (2;3), the numbers (2;5), or (2;7) are chosen.

Let us stop short of declaring that the cited change of sign distinguishes the muon from the electron. Other things are needed for continuing along those lines. For one, it is necessary to make a more convincing evaluation of the number x then presently suggested by Eq.22. At this point, let it suffice to say that the flux ratio Φ_r/Φ_p would have to be of the order $(r/R)^2$, which, according to Eq.22, makes Φ_r/Φ_p indeed a higher order anomaly of the order α^2, which is at least of the order of magnitude as experimentally observed.

A quick comparison with experimental electron and muon data[5] in comparison with their common electrostatic anomaly component $(1+\alpha/2\pi)$

shows a negative correction for the electron and a positive correction for the muon

$1+\delta_e \quad = \quad 1.001159652; \quad$ - correction 0.000001757

$1+\alpha/2\pi = \quad 1.001161409\ ; \quad$ common term for electron and muon

$1+\delta_\mu \quad = \quad 1.001165924\ ; \quad$ + correction 0.000004515

Since the electrostatic correction is positive, and the flux leakage correction is negative, the muon might be thought of as having a relatively higher electrostatic energy. The latter fact could contribute to its inherent instability, if the condition for impedance matching can no longer be accurately met.

Another option of accounting for the change of sign of the higher-order anomaly correction is revealed through Eq.31 by assuming electron and muon have different winding numbers. As indicated in the next section, an azimuthal winding number n=2 seems dictated by the half-integral spin property electron and muon have in common. This still leaves an option of different meridional winding numbers k. The number pair (2;3) yields according to Eq.31, a negative correction -x/2. The number pairs (2;5) and (2;7), say, as conceivable muon candidates, give +x/2 and +3x/2. According to the just cited experimental data the observed ratio of higher order muon versus electron corrections is of the order 4515/1757=|2,57|. For comparison, the ratio associated with the number pairs (2;7) and (2;3) gives |3|. All this remains a crude form of experimentation, because Eq.24 still ignores a conveivable electro-flux contribution; the latter is, so far, only accounted for in the energy balance.

By the same token, the adventure of open experimentation with particle models also invites reexamining the completeness of the flux balance of Eq.24. Taking into account the electro-flux we have:

$$h/e = \Phi_p + \Phi_r - \int_0^T V dt = \Phi_p + \Phi_r - V/\nu; \text{ the period } T = 1/\nu. \qquad 24a$$

Since Φ_p is the component generating the magnetic moment μ, Eqs.2 or 4 of chapter VIII might be replaced by the nonmetric relation:

$$\mu = \frac{e^2}{4\pi m}\Phi_p\ . \qquad 32$$

Solving Eq.24a for Φ_p and substitution in Eq.32 yields

$$\mu = \frac{e}{m}\frac{\hbar}{2}\left[1 + \frac{V/\nu}{h/e} - \frac{\Phi_r}{h/e}\right]. \qquad 33$$

Independent of the energy balance Eq.25, the flux balance Eq.33 seems to yield all the essential anomaly features. The first term in the bracket gives the Dirac part, followed by the Schwinger term and the higher-order correction for flux leakage. Since $(V/\nu)/(h/e) = eV/mc^2$, there is a measure of, yet no full, compatibility between energy and flux balances.

These are just samples of model-based explorations into some conceivable structural features of nature's fundamental building blocks. Looking at these efforts in the perspective of the very abstract QED approach which avoids making explicit models, one cannot deny the latter's inner wisdom of steering clear from the potential pitfalls invited by model making. Yet, also QED is not altogether model-free. The point-particle emerges in QED as a model of sorts, which (see section 9) requires rather more unusual properties to account for the world we observe. Perhaps a measure of more realistic model-making should be kept alive, provided we remain aware of distinctions between model and reality.

The upshot of this theoretical exeprimentation indicates that a cautiously extended model-making can help by complementing the very abstract QED approaches. It injects new adventure, where earlier only a dead-end-street could be envisioned. The new adventure yields numerous questions worthy of further consideration, *e.g.,* why can bound muons, neutrons, and pions become stable? Could the muon still have a pivotal role for the structure of protons and neutrons, now believed to be compound structures of quarks and gluons?

8. The Enigma of Half-Integer Spin

Finally let us reexamine the question whether or not the electron model can account for the half integer properties $\hbar/2$ of electron spin. It is this property that has marked a major milestone in the almost exclusive acceptance of the contemporary abstract methods of physics. The subtle connection between spinors and the two-valuedness of the group spaces of rotation and Lorentz groups has given iron-clad justifications for the prevailing abstract approaches. Arguments favoring abstractness still prevail today on the instructional level. While two-valuedness is now much better understood from a mathematical[6] angle, so far, contemporary physics has not incorporated these mathematical improvements.

Abstract "spinorization" is a global mathematical concept intimately related to the much more visualizable notion of nontrivial mirror symmetry. Two objects are said to be nontrivially mirror symmetric if they can't be identified by continuous deformations.

The crystallographers discovered this phenomenon in crystal morphology for the rotation subgroups. They gave it the name "enantiomorphism." Spinorization, in the physical sense, must be regarded as an expression of spatial and spacetime enantiomorphism for particles in the microphysical domain. Mathematically, the global nature of spinorial pairing becomes an explicit pair feature if the objects so adjoined are imbedded in an orientable space or spacetime. Physically it means that an electron can't come back as a positron by carrying it around a cyclic path.

The mathematical methods of physics at the time of the emerging abstract procedures culminated, quite exclusively, in the use of differential equations, which are tools that are strictly local in nature. Yet, space and spacetime enantiomorphism (or spinorization, if you will) are undeniably global ingredients, even in the microphysical building blocks of nature. It seems reasonable to expect that these global characteristics leave a nonlocal trace in the differential equation approach. *Dirac first picked up this thread of global conscience by injecting his spinors in local description.*

After this lengthy introduction on the delineation of local and global procedures, the identification of half-integer quantum numbers now appears anticlimactic. It is no coincidence that Dirac is the father of the action unit $\hbar = h/2\pi$. The $\hbar$ simplifies matters for local differential descriptions.

Planck's original action quantum h, by contrast, prevails in the global integral conditions of the earlier forms of quantum mechanics practiced by Planck, Bohr and Sommerfeld. The factor 2π is then readily identified as the result of a single-loop integration. Planck's h is the globally secured quantum of action; the $\hbar$ appears as local counterpart in a 2π single loop integration, if, and only if, orbital momentum is also locally conserved. The half- integer unit $\hbar/2$ appears as a locally conserved counterpart of a globally conserved h that is obtained in a double-loop integration of 4π, such as is required by the trefoil. Dirac theory produces both the $\hbar$ and $\hbar/2$ by locally reestablishing a conscience for global requirements.

9. *Zitterbewegung* and Trefoil Model

At center-stage, we consider here a famous paper by van Dyck, Schwinberg and Dehmelt.[7] This publication established milestones, not only in terms of precision in measuring the electron's gyromagnetic ratio, it also claims these measurements to be the result of observations on single electrons, isolated and nearly at rest in Penning traps.

In presenting their results the authors delineate their views in a theoretical perspective that in part goes back to Schroedinger, Dirac, Pauli, Huang, and Schwinger. The Dirac theory of the electron plays a role, because it locally associates the velocity of light with the electron. Schroedinger is involved for the famous concept of the *Zitterbewegung*, which is, in essence, an expression of a universal zero-point disturbance by viewing an isolated single free electron as subject to Heisenberg uncertainty. Pauli is involved for general pronouncements concerning electron modelling and limits of experimental observability. Huang is instrumental in these discussions for his reexamination of Schroedinger's concept of *Zitterbewegung*. Schwinger is involved for his QED work that led to the precision calculations of the electron's anomalous moment

Huang[8] has reexamined Schroedinger's *Zitterbewegung* for the Dirac electron. This reexamination sheds an interesting light on contemporary thinking in physics. For the purpose of this discussion, the following statement, taken from the abstract, is cited, because these views, expressed in the Fifties, still hold a central position more than thirty years later for the discussions in ref.7, which go back to the Eighties:

"The well-known *Zitterbewegung* may be looked upon as a circular motion about the direction of the electron spin with radius equal to the Compton wavelength×(1/2π) of the electron. The intrinsic spin of the electron may be looked upon as the orbital angular momentum of this motion. The current produced by the *Zitterbewegung* is seen to give rise to the intrinsic magnetic moment of the electron." Huang[8]

In the last paragraphs of his article, Huang points out familiar shortcomings of classical electron models, which give either a correct magnetic moment or a correct spin, but not both. He mentions that he cross-checked his results with leading authorities in the field of quantum mechanics and QED, which, in a sense, qualifies his conclusions as a third-generation Schroedinger result.

Now returning to ref. 7, let us quote here how van Dyck, Schwinberg and Dehmelt summarize the electron picture they associate with their Penning trap experiment:

" ..... What remains is a soft quasi-orbital structure of radius about one Compton wavelength×(1/2π) formed by the circular Zitterbewegung of the hard "point" electron of dimension smaller than 10^{-16} cm. It is this structure on which measurements of the intrinsic magnetism of the electron provide information. While the above constitutes a certain justification of Pauli's initial rejection of the spinning electron model as *Neue Irrelehre*, Pauli, on the other hand, overshot the mark when he attempted to prove that spin and magnetism of the free electron could not be measured by a suitable variant of the Stern-Gerlach experiment." Dehmelt[7] *et al*

By surreptitiously removing a nonclassical statistics from their model, Huang, Dehmelt, *et al*, ***silently*** opened a way to becoming even more explicit about further pursuing a classical vein of argument.

The Trefoil Electron Model

Pauli the originator of the neutrino, the exclusion principle, and many other contributions to modern physics, not only went too far in rejecting free electron measurements, he also went too far in rejecting the spinning electron model. It can hardly be denied that the quasi-orbital model pictured by van Dyck, Schwinberg, and Dehmelt comes amazingly close to a classical model of a ring current. The words "soft quasi-orbital" are clearly injected to cover a retraction from the taken position and to make apparent that the model is not to be taken too literally in a classical manner.

What prevents a model development in an even more classical vein? Huang indicates how this difficulty relates to meeting simultaneously physical reality for spin and magnetic moment both. A factor 2 keeps haunting these models; either the magnetic moment is off or the spin is off.

If the Huang[8]-Dehmelt[7] *et al* model of the electron has already a close resemblance to a classical model, the here presented trefoil model may well be classified as classical all the way. Why does the here chosen model manage to reconcile spin and magnetic moment calculations simultaneously, whereas earlier classical models have only been able to meet either one or the other, but not both?

An inspection of the previous section permits the identification of two crucial distinctions that make this reconciliation possible. They are: I. Jehle's[4] trefoil model, which is here applied to the electron, in conjunction with: II. Kiehn's definition of field angular momentum as a three-dimensional period integral permitting an orbital as well as a spin adaptation (see chapter VI).

The Dehmelt *et al* experimentation shows that those adhering to Pauli's nonclassical radicalism about measurability, may have to swallow some of their pronouncements. Yet combining the Jehle-Kiehn propositions (*i.e.* applying spin integral to trefoil) reveals a need for even greater tolerance than Dehmelt *et al* were willing to accept for a classical model. The trefoil is in the category that Pauli might have denounced as *neue Irrelehre*. (new misleading teachings).

Last, but not least, an inquiry from QED circles must be expected whether the trefoil model can offer also perspectives for obtaining higher order anomaly terms that presently permit a detailed distinction between electron and muon properties. Let us recall that the first higher order QED calculations showed discrepancies that were resolved later. The trefoil model should be given equal consideration to prove itself. The following statement deleneates the present situation; it casts a frightening harsh light on the conceptualizations that have been permitted in physics for more than three decades:

The fact that Huang, Dehmelt et al ***need to take recourse to transforming the "Zitterbewegung's (nonclassical) statistical disorder" into the "perfect (classical) order" of a point-charge in circular orbit is, for all practical purposes, a resounding experimental rejection of Copenhagen's a priori single-system uncertainty and zero-point disturbance. Over and above, this QED inspired model of a point-charge in circular orbit does not***

compare well with the trefoil model from a point of view of providing model-based calculational potential.

10. Conclusion

The presented excursion in the land of abstract modern physics, with the help of a vehicle that is widely believed to be unsuitable for such endeavors, is bound to raise eyebrows. The mere simplicity of some of the procedures raises suspicion, because we have been psychologically conditioned to suspect intrusions of classical methods in the nonclassical realm. The end product can be taken as a mnemonic device to make a difficult matter more transparent or, a reminder to seek new options. Sometimes both paths, if used in a complementary fashion, can give a perspective for synthesis.

In the process of pursuing the latter course of action, the interface of conceptual confrontation can present us already with some pointers about different angles of assessing the classical and nonclassical realms. First of all, we need to realize that the classical/nonclassical distinction is a very subjective notion which has been far too much emphasized for purposes of instruction. Contemporary teaching of physics prematurely abandons visualization, in fact, visualization has, at times, been portrayed as an obstacle on the path of understanding modern physics.

For comparison, let us also assess some conceivable shortcomings at the classical end. Classical and semiclassical physics have retained an undue emphasis on the methods of analytic descriptions with the help of differential equations. Many laws, originally given by Nature in some sort of integral form, have been religiously converted into a local form, permitting the use of differential equations. The Maxwell equations are an example. Whenever the phrase "Maxwell equations" is mentioned, people think of a set of differential equations, not the integral global counterparts from which they derive. Yet, the latter relate directly to the experiments that initiated Maxwell theory.

Mathematically, a restoration of a global point of view in physics is bound to inject greater awareness for matters pertaining to topology. The principle of single system quantum uncertainty has unfortunately prevented a topological view of microphysics from coming to fruition. The immersion of physical objects into a sea of an, always present, *a priori* quantum uncertainty does not invite topological inquiry. Traditional views of quantum uncertainty affecting isolated particles, as if bouncing around on an "infinite" zero-point energy of vacuum are in dire need of revision.

In the spirit of an interpretation in the sense of Einstein and Popper, quantum mechanical uncertainty becomes a manifestation of real physical ensembles. Here the additional requirement is stipulated that the ensemble needs to be randomized in mutual phase and mutual orientation of the ensemble constituents. For many years, Planck's zero-point energy $\hbar\omega/2$ and the calculations of Feynman and Kompaneyetsthe of the quantum number $\sqrt{n(n+1)}$ of chapter III; 5 have, for many years, been silent testimony to a classical statistics underlying modern quantum mechanics. With all those classical alternatives hitting us in the face, it is hard to conceive how the nonclassical myth ever got off the ground. It seemed that, at the time, the world of physics had a need for mystery and magic.

AN ATTEMPT AT COHOMOLOGICAL SYNTHESIS

CHAPTER XII

ARROWED TIME AND CYCLIC TIME

1. Topological Torsion

In the preceding chapters, cyclic integrals in the time domain have been used for periodic systems. The justification for this course of action was somewhat glibly based on the conclusion that the consecutive periods of a purely periodic system are truly indistinguishable. At the end of a period, the system is exactly in the same state as at the beginning of the period, so, for all practical purposes, it looks like a closed time loop. In addition to this strictly formal argument, there was, in retrospect, a justification with which it is difficult to argue, because the results so obtained made sense. For the quantum Hall, the Josephson a.c. and other effects, the identification of time periodicity as a cyclic feature leads to experimentally verifiable results. Furthermore, discussing the relation between Aharonov-Bohm and Bohr-Sommerfeld integrals, a cyclic time integral is found to relate to cyclic spatial integrals (chapter VI, section 2, Eq.7).

Yet, even if many physical laws happen to be invariant under time reversal, the truth is that other physical laws (*e.g.*, the entropy law) give us an arrow of time. It is this unilateral property of the time coordinate, as contrasted with the bilateral properties of spatial coordinates, that has remained a shadow standing in the way of a wholehearted acceptance of Minkowski's spacetime as a true four-dimesional arena of physical reality. Somehow and sometimes, physics and physicists feel they has been carried away by Minkowski's famous and eloquent Cologne address, in which he pulled off an epistemological masterpiece by claiming that "the difference between space and time would sink away into a mere shadow."

Then, when physics had a chance of collecting its wits after the mind boggling Minkowski-Einstein avalanche of spacetime physics, pragmatists had second thoughts about the matter. Some said, wait a moment, what was that again? There is no difference between space and time? You can't sell me that, I can't accept it! Of course, the pragmatist is right; yet, by the same token, Minkowski was right. A resolution of this apparent controversy is in Minkowski's own words. He never said there would be no difference; he said "the difference would sink away into a mere shadow."

This chapter aims at getting to know more about Minkowski's shadow hanging between space and time.

The presented predicament can also be looked at from a more purely mathematical angle. The identification of algebraic counterparts of geometric as well as topological features has opened a door to an abstract assessment of higher dimensional configurations, the ensuing disciplines of algebraic geometry and algebraic topology testify to this. Here for a change, it was the escape in abstraction that reshaped perception. At first, the conceptual step from three- to higher-dimensional spaces does not invoke essential differences in the nature of added dimensions. The pseudo-visualization so obtained is traditionally geometry-inspired. The added dimensions are thought of as having geometric connotations.

An opportunity occurs for making dimensional distinctions that retain an invariant quality, when the multidimensional configuration is taken to be a metric space. The invariance of the signature of the metric under *real* transformation adds such an invariant feature. The Minkowski signature $\{+,+,+,-\}$ testifies to an undeniable distinction between time- and spatial coordinates. All of which goes to show that *those who unduly favor the substitution* $u=ct\sqrt{-1}$ *in transforming the indefinite metric* $x^2+y^2+z^2-c^2t^2$ *into a positive definite metric* $x^2+y^2+z^2+u^2$ *can be blamed for wiping out a space-time distinction given to us by Nature.*

After having thus established that the metric is an active reminder of space-time distinctions, the question arises as to what happens in premetric situations. In the previous chapters, the period integrals were, after all, hailed for their metric-independent qualities, permitting an extension of their applicability from macro- to micro domains. While metric-independent does not imply there is no metric, the question remains: if we can't call on a metric, how do we keep track of space-time distinction?

The answer suggests a potential topological criterion for space-time distinction. The concept of topological torsion can be assigned to assume this role of providing a premetric distinction between space and time dimensions. Let us briefly summarize here the principal points of algebraic topology that have led to the concept of topological torsion.

When, towards the end of the last century Poincaré and others extended the so-called "Betti groups" to multidimensional configurations, the question arose whether these additive groups should also include in their basis of generators cyclic (torsion) elements. Since the Betti (Homology) groups belong to the category of finitely generated additive groups, there was no obvious reason to exclude these torsion elements. The ensuing purely algebraic extensions to the notion of manifold topology were accepted, in the hope that future developments would identify a more explicit role for Poincaré's new concept of torsion.

Subsequent efforts to come up with geometric illustrations of torsion led to the Projective Plane and the Klein Bottle. Both examples stretch the ability at visualization, because they can only be imbedded in four dimensions. Alexandroff and Hopf[1] then proved that torsion cannot really occur in less than four dimensions; in fact in four dimensions it is only possible to have one dimensional torsion. In five dimensions, the torsion dimensions can be 1 and 2 dimensional and in general for dimension $n>3$, they run from 1 to $n-3$. In other words, only four dimensions can accommodate a unique case of one-dimensional torsion. It is now tempting to relate the cyclic group characteristics of torsion with the uniquely cyclic features given to time by the purely periodic systems. This conjecture only flies, however, if time periodicity has an intrinsic feature, which distinguishes it from spatial periodicity as displayed in crystals. Let us note that the spatial lattice periodicity requires an object repetition in each lattice point; the time periodicity, by contrast, acquires a unique fourth dimension character, because it is generated by one and the same object.

This at first surprising restriction on the possibilities of torsion structure in manifolds is geometrically not an easily visualizable matter. Perhaps it becomes more acceptable, if we realize that there are other typical topological features that are contingent on manifold dimension. Linking is a familiar and visual example; it is not possible in two dimensions. Linking becomes a realistic possibility from three dimensions upward.

Similarly, one-dimensional torsion becomes a realistic possibility in four dimensions. It is the here-cited property of topological torsion that will accommodate both the cyclic as well as the unilateral nature of time.

2. Physical Examples of Torsion

Let us now see whether elementary particle processes can give a reasonably fundamental illustration of how time differs from spatial coordinates. Consider hereto the cloud-chamber pictures of some familiar elementary particle processes. The trajectories of electrically charged particles become visible by their condensation tracks, and the trajectories of the neutral particles, such as neutrino and photon, are inferred on the basis of known conservation laws. The completed world-line trajectories of these elementary particle events have become known as Feynman diagrams.

The general picture so obtained is a spatial network of world-lines, each provided with an arrow of time. These world-lines represent stable, or almost stable, particles. They originate (creation) in nodal points of world-line intersection, with the arrow of time pointing away from the nodal point. They terminate (*i.e.*, physically annihilate) where the arrow of time on the world-line points towards the nodal point.

The nodal points are **event centers** where particles are transformed into other particles. These events can come about spontaneously (*e.g.*, as in the weak interaction of β-decay); other interactions (*e.g.*, electro-magnetic, and strong interaction) are contingent on outside influences such as particle collisions.

How do these diagrams of elementary particle processes relate to a corresponding spacetime language reflecting the topological overtones alluded to in the cohomology of forms and the associated homology of their domains of integration? The world-lines of the diagrams may be taken to have a three-dimensional lateral extend. The spacetime existence of a particle then is characterized by a four-dimensional domain which is very elongated in the direction of the world-line. It is, so to say, a four-dimensional tube or wire.

In view of the varying distances between nodal points, the length of these tubes or wires cannot be very critical for identifying the nature of the particle. *Stable and semi-stable particles thus have physical characteristics that are largely independent of this rather coincidental worldline length. One therefore would expect a (periodic) section of the worldline tube to suffice as being fully descriptive of the particle under consideration.* Away from a nodal point (event domain), it does not really matter where the *section* is made; any section on the replicating worldline tube will do.

We now have a particle picture that says we should expect a periodicity in the direction of the worldline that is characteristic of the particle in question. A characteristic periodic time interval T on the worldline tube is indeed given by the familiar relation $T=h/mc^2$, where m is the particle mass and h is Planck's constant of action The period of a photon hardly needs commentary. Yet the neutrino period may need more thought.

The overall picture of the worldline tubes representing particles is now that of different types of periodic, beaded strings. The periodicity of the beads simulates, so to say, the self-replicating nature of the particle in the course of time.

A one-dimensional torsion cycle can now be identified as characterizing a particle's cyclic time. This cyclic time functions as a measuring tape intertwined with the direction of the arrowed time on the particle's worldline.

The torsion number counts the number of cyclic elements on a given worldline, and so becomes a measure for particle lifetime in terms of its own characteristic period. The torsion numbers for stable and semi-stable particles are, of course, enormous. For the life time τ of a muon say, the ratio τ/T is of the order $5.6\ 10^{16}$, and, in general, near-limitless for an electron.

Worldlines originate and terminate in four-dimensional event domains of transitional action. The latter have no obvious periodicity properties. All that is known about interaction events is that they take a small interval of time and a small spatial domain to come about. They are spacetime domains topologically equivalent to a four-dimensional ball.

Seen from a particle point of view, the process of physical evolution thus resembles a neuron net of event domains interconnected by world-line tubes with arrows of time and beaded in a near-infinitely fine manner.

3. Period Integral Assessment of Feynman Diagrams

To make sure that a period integral cleanly links or encloses an object, say an obstruction, it is necessary that its integration cycles reside everywhere in a region where the exterior derivative of the integrated form vanishes. The Gauss integral always serves as a useful reminder of this fact; the surface of integration resides where the divergence (*i.e.*, exterior derivative) of the integrand is zero. The surface is, so to say, not allowed to go through a charge. Electric charges are either inside or or outside the surface enclosure.

The just-depicted situation differs slightly from what is usually envisioned from the point of view of a geometry-based mathematical interpretation. In that case, the integration cycle is thought of as enclosing a hole. Since there is nothing assumed to be in the hole, for a geometric substratum, the idea of only enclosing part of a hole is here not given any relevance. The integration cycle takes everything or nothing, because it cannot be anywhere else! The discreteness of elementary charge imposes a similar alternative, provided the indivisibility of elementary charge stands firm. So far, experiment confirms the indivisibility of electric charge.

The physics-based interpretation of period integrals yields an additional bonus of evaluation opportunities if the hole is physically alive; meaning it is not a void as in the geometry-based interpretation. An application of Stokes theorem now permits an evaluation of the period integral by integrating over the content of the hole. In physics, it can happen that the content of the hole is more accessible than the field surrounding the hole. By having the hole interior a void, the geometry-based interpretation bereaves itself of this option, because the differential form is not defined in the void.

The task now before us is one of designing an operational process that takes full advantage of the complementary duality of the cohomology of period integrals and the homology of their integration cycles. The stated physical purpose is an evaluation of period integrals governing the worldlines or, rather, one cycle thereof. Then, last but not least, central to the dynamics of the here considered physical topology is the question: is there a period integral characterization of what happens in the nodal event of transition of the Feynman diagrams?

Following a study by Kiehn[2] on the onset of instability in fluid dynamics, the Pfaffian integrability classes of a one-form, say, A, are taken to be indicative for the transition. Relevant to these considerations is the analogue of a Pfaffian integral of the type

$$\oint\oint\oint A\wedge dA = \text{multiple of } (h/e)^2 \,, \qquad 1$$

which, on the basis of dimensional consideration, should be expected to have residue units $(h/e)^2$. Unlike the earlier discussed period integrals, which characterize the structure of worldline objects, this integral can be shown to be capable only of giving an event description. Here is an example of topological dynamics, in contrast to traditional static topology.

We should not invoke, at this point, details of individual particle structure, because the vanishing of this integral Eq.1 for particle-like spacetime domains would have to be something that all stable or semi-stable particles have in common. Such a structure-independent criterion is instrumental in emphasizing its exclusive relation to *events*. This requirement is indeed met by noting that the worldline tube domain of a stable object is a cyclic integration domain c_3 which is a product cycle:

$$c_3 = c_1 \times c_2 \,. \qquad 2$$

The Pfaffian integral Eq.1 can now be rewritten according to a formula by Kuenneth leading to the reduction:

$$\oint\oint\oint_{c_3} A\wedge dA = \oint_{c_1} A \oint\oint_{c_2} dA = 0. \qquad 3$$

Since dA is exact the integral over c_2 vanishes, hence Eq.3 vanishes.

The transition domains, where world-line tubes originate and terminate, is a center of change where incoming particles cease to exist and new particles are being formed. Using Stokes law, the cyclic Pfaffian integral of Eq.1 can be converted into a domain integral over the interior d_4 of the boundary ∂d_4. This integral, however, can be different from zero:

$$\oint\oint\oint_{\partial d_4} A\wedge dA = \iiiint_{d_4} dA\wedge dA \rightarrow \iiiint_{d_4} \mathbf{E}\cdot\mathbf{B}\; dV dt \neq 0 \qquad 4$$

if, and only if, $dA\wedge dA \neq 0$, which means, in d_4, the one-form A belongs to the lowest Pfaffian integrability class in four-dimension. The conversion $dA\wedge dA \rightarrow \mathbf{E}\cdot\mathbf{B} \neq 0$ is the standard translation into the Maxwellian fields of electric field strength **E** and magnetic induction **B**; $dVdt$ is the spacetime integration element. The situation represented by Eq.4 clearly indicates that ∂d_4 cannot be a product cycle

$$\partial d_4 \neq c_1 \times c_2 \ , \tag{5}$$

because the argument presented for Eq.3 holds, regardless of the Pfaffian integrability class of A.

At this point, it is necessary to be aware of a difference with respect to standard textbook discussions. Since mathematical discussions tend to assume a geometry-based epistemology, the analytic continuations of A in the hole interior d_4 are not normally considered. There is, therefore, no A in the hole and the Pfaffian integrability cannot come into play. In fact whenever the hole is taken to be empty and void, the product of an exact form and a closed form is deduced to be exact. Here, an earlier-mentioned ambiguity reoccurs: the cycle identified by a hole becomes a boundary as soon as the hole has an interior. For the homology classification though the boundary of a hole with interior is taken to be a cycle.

To avoid counting abstract geometric holes as physical holes with interior, it is essential that all topology considered is of physical origin.

Note that the (physical) interior of a "homological" hole supports forms that are neither closed nor exact; their exterior derivatives are nonzero (*e.g.*, $d\mathbf{G} = \mathbf{C} \neq 0$, and $dA \wedge dA \neq 0$). They are analytic continuations of forms that have vanishing exterior derivatives outside the hole.

Standard cohomology chooses to ignore those analytic continuations, because the geometry-restricted epistemology permits the hole to be empty without an analytic continuation for differential forms that are closed outside the hole. A physics-based epistemology is incompatible with this practice, because the hole is the source of the closed field outside the hole. *Here is one of those rare cases where physics reminds mathematics not to oversimplify; more often mathematics acts as the conscience of physics.*

4. The Physical "Substance" of Topological Obstructions

The "substance" of topology in geometry-oriented procedures comes about by definition, or, let us say, by the playful moods and imagination of practicing mathematicians. They can place their empty holes wherever they wish, say, just to make the configuration topologically interesting. In physics, nature determines what is interesting.

Gauss' law of electrostatics and its Ampère extension remind us again how electric charge somehow separates the domains where $\mathbf{G}$ is closed, $d\mathbf{G}=0$, from the "hole domains" where $d\mathbf{G}\neq 0$. In the purely mathematical approach, the $d\mathbf{G}\neq 0$ domain is replaced by a void, because electric charge has no place in a geometry-oriented topology.

To obtain a coherent picture of physical topological structure, the next question ought to focus on how the separation between $d\mathbf{G}=0$ and $d\mathbf{G}\neq 0$ domains agrees and relates to the separation of the $dA=0$ versus the $dA\neq 0$

domains. This information is needed for assessing the topological role of the cyclic integral of the vector potential. The compatibility of the one- and two-connectedness of charge-current structures is a crucial viability test for a spacetime topological assessment of physical structures.

A refocusing of interest on this innocuous integral of the vector potential A has largely been due to Aharonov and Bohm. The vector potential A in electromagnetic theory has traditionally been treated as some sort of calculational expedient without an explicit and direct observable physical interpretation; only curl**A**=**B** had meaning as a direct physical observable. Aharonov and Bohm showed this traditional point of view to be wrong, at least in part. While A itself has no unique local meaning, by virtue of its being defined modulo an exact part, its cyclic integral has a direct physical meaning. This integral effect was verified by quantum interference between two electron paths linking with an adjustable magnetic flux ring. The interference clearly changes with the linked flux, although the electrons cannot locally experience the change in **B**, because their paths are nowhere close to the **B**≠0 area.

Soon it became evident that superconductors were ideal devices for creating field domains where **A**≠0, yet curl**A**=**B**=0, by virtue of the Meissner effect. A vanishing electric field **E**=0 was, of course, already recognized as a *sine qua non* for superconductivity. With **E**=**B**=0, the superconducting state acquired a status of spacetime invariance, which is appropriately expressed by the invariant statement $d\mathcal{A}=\mathcal{F}=0$; this neatly covers London's condition of superconductivity and the Meissner effect.

At this point, it may be said that electric charge and current not only have a role in separating $d\mathcal{G}=0$ and $d\mathcal{G}\neq 0$ domains, but simultaneously, they also separate $d\mathcal{A}=0$ and $d\mathcal{A}\neq o$ domains. While these inferences are based on macroscopic situations, the metric-free nature of the associated integrals and their residues make it tempting to extrapolate these findings to the micro- and even the submicroscopic domains.

Going to the submicroscopic extreme of elementary particles with electric charge and nonvanishing flux, the topology of their charge-current distributions is given by the exact, impair 3-form $\mathcal{C}=d\mathcal{G}$. The particle is then, in part, describable by the period integrals delineating its 1- and 2-connectedness with 3-connectedness determined by their product integral.

Evidence of relevance has been presented in the earlier description of the electron and muon properties (chapters VI, VIII and XI). Starting with charge and mass, the spin and magnetic moment can be calculated, including the anomaly term, $\alpha/2\pi$, which electron and muon have in common. Their mass difference still remains a shrouded secret of nature, yet a qualitative picture may become available in terms of their higher order anomaly differences.

The charged pion is not known to have a magnetic moment. Here the question is whether or not this implies a zero flux residue, or could it possibly imply that the magnetic flux is internal, without an external manifestation of magnetic moment? A topological identification of photon and neutrino without the 3-form $\mathbb{C}$ of charge presents, however, new questions about domain separation.

Since neither neutrino nor photon have an overt charge manifestation, the role of domain separation would presumably have to depend on how the 2-form F relates to the lightcone. This would have to lead to an enantiomorphic adjoint configuration for the neutrino and a self-adjoint configuration for the photon. *It is not necessary, though, to resolve at this moment all matters of particle topology; the presently intended objective is only one of indicating the possibilities of perspectives. Let the earlier discussed electron-muon model suffice for the moment.*

While these earlier electron-muon considerations attempt to account for criteria characterizing stable and semi-stable particle configurations, the new topological feature introduced in this chapter explicitly relates to the dynamics of physical topology. It is the field characterization of the event domain. A finite value of Eq.1 as period integral would have to indicate an event domain, the limits of which are given by shrinking the cycle c_3 until Eq.1 begins to change its value.

An analogue of Eq.4, although not simply related to a period integral, is the spacetime event integral: $\iiiint \mathfrak{G}\wedge\mathfrak{G} \rightarrow \iiiint D\cdot H\,d^4x \rightarrow e^2$, which provides a measure for charge creation or annihilation in event domains.

CHAPTER XIII

QUANTUM COHOMOLOGY

1. Introduction

A choice of strategy for an exposition of "quantum cohomology," such as defined and outlined in this chapter, becomes a matter of coping with two major hurdles standing in the way of this proposed objective. Quantum cohomology not only goes against major contemporary traditions in our attempts at understanding physics, it also demands a major readjustment in the prevailing geometry-based traditions of interpreting topological structure in mathematics. Mindful of these presently prevailing traditions in these two domains of science with which communication needs to be established, there are some real questions concerning an appropriate strategy of approach.

If the here-anticipated predicament of communication already requires walking a tightrope between two disciplines, there is, in addition, the marginal familiarity in physics circles with the principles algebraic topology. While such lack of familiarity may compound the situation, the lack of accompanying prejudice could be an asset. The following thus becomes an attempt at outlining principles of algebraic topology in the perspective of metaphors that still have to earn their status of relevance.

This chapter gives first a mathematically more detailed picture of the discussions in chapter XII. This amounts, in fact, to an outline of what can be suitably called a physics-based quantum cohomology. As in chapter XII, the customary geometry-oriented realizations of abstract topology are now no longer adequate in dynamic physical situations involving the time. A switch from a geometric to a kinematic backdrop realization is necessary.

The concept of topological torsion is of central importance in these developments. The difficulties encountered in visualizing torsion in three dimensions has long ago been identified as a contingency of the essentially four-dimensional connotations of torsion. In four dimensions, only one-dimensional torsion can occur. *In spacetime, one dimension can be topologically distinguished from the other three dimensions by virtue of its option to assume torsion features.* It stands to reason to test whether topological torsion can portray the exceptional position of the time-like direction of physical spacetime.

This topological time- and space-like separation of spacetime is compatible with, yet exists over and above and independent of, the spacetime distinction implied by the metric's signature. The topological invariance of the torsion distinction not only leaves intact Einstein's principle of general spacetime covariance, it, in fact, calls, by virtue of its topological invariance, for a metric-independent extension of said principle. The abstract topological feature of torsion thus can be taken to correspond to three-dimensional physical space without torsion and a time coordinate directly relating to torsion. Mindful, though, of the exclusive four-dimensional condition for torsion to exist, only combined space and time bring torsion's one-dimensional nature to an explicit and physically significant fruition.

The impact of torsion notions is assessed and delineated for the contemporary structure of physical theory. While torsion, as here envisioned, is a purely topological concept, an exploration implies an implementing of homology with torsion in the context of physics. Since contemporary physics is not exactly replete with a tightly knit structure of topological notions, an exploration of this kind must be combined with finding isolated instances where topology has already made its presence felt.

The next step is integrating these isolated instances into an encompassing structure, consisting of a de Rham cohomology of the differential forms of physics. Torsion then relates to the homology of integration cycles of the period integrals of those differential forms of physics. The periodicity associated with torsion permits cyclic closure in time, in the sense of weak homology. This picture of cyclic time periodicity is needed to define a period integration in time, which is compatible with time's unrelenting arrow of progression.

2. Spacetime Characterization of Physical Objects

Any physical object with which can be associated any measure of permanency in time can, in principle, be visualized as tracing its history in spacetime as a tube-like worldline configuration. This time-like elongated appearance is characteristic of any entity that is not subject to a change destroying the individuality of the object. The worldline tube gets longer and longer the bigger the time interval of unchanging appearance.

A constant time-slice of the worldline tube gives a bounded cross-section in three dimensions, which is taken to be finite in size. The two-dimensional boundary of this cross-section domain thus gives its momentary location and confinement in space. The interior of this two-dimensional boundary reflects the interior structure of the object under consideration at the moment when the constant time-slice was taken.

For physical objects with an internal dynamic structure, such instantaneous identification would lead to a loss of information about the internal dynamics of the object. The question is how information about the interior can be retrieved by viewing the 3-dimensional object in a time extended sense?

For an object that is a compound structure, consisting of many different molecules, atoms and particles, such a characterization is, in principle, possible but would ultimately lead to a confusing compilation of time-like data. Every individual atom, molecule, or particle would have to be taken, one at a time, until a complete picture of the object is obtained. Seen from that angle, it seems simpler and more practical, from an information gathering point of view, to start with the simplest possible components of such objects, say, the elementary particle. In fact, this approach leads, if you will, to a practical definition of what an elementary particle is. One may consider the following definition option:

Definition

In the category of microscopic particles, those completely determined by a single time interval shall be considered as elementary in nature.

Since all known particles have an associated energy E, the time interval T needed for a complete characterization of its dynamic nature might be defined by a relation of the type:

$$T = h/E, \qquad 1$$

where h is Planck's constant.

Returning now to the spacetime characterization of physical objects by four-dimensional worldline tubes, and the three-dimensional slices thereof, two consecutive constant time-slices of its worldline, separated by the interval T of Eq.1, now suffice for the description of an elementary particle. A compound physical object, by contrast, would need a multitude of well labeled time-slices for its complete characterization. Its worldline would look like a multiple beaded string. By contrast, *elementary particles resemble singly beaded strings.*

The manifestation of time so acquires an arrowed marker given by the direction of the worldline. Its cyclic components are particle determined systems of equidistant time-slices on that worldline. Only for the elementary particle does the worldline assume the appearance of a singly beaded string.

3. Elementary Particles and Topological torsion

In the spirit of the picture of the worldline tube with equidistant time slices, the elementary particle may be said to replicate itself in the direction of the arrow of time. For a stable particle this process of replication in time is taken to be perfect without "aging." It could go on indefinitely, unless there is some collision event terminating the particle's existence.

For a semi-stable particle, time replication goes on until it terminates its existence either through spontaneous or collision-induced disintegration.

The self-renewal aspect of time replication is crucial. It adds a fundamentally dynamic feature to the particle's spatial existence. In space the particle is an isolated object. If there are several identical particles, each may be regarded as a replica of the others, yet all of them are taken to be spatially isolated nonadjacent objects. In the time direction, by contrast, replication is part of a particle's individual existence. The time replica replaces the object from which it came a period earlier. This adjacency is characteristic of its time behavior as distinguished from its, in essence, isolated spatial aspects.

The following overall picture of elementary particle description now transpires. Based on the nature of the elementary particle, as perceived through presently available experimentation, the particle manifests itself as a worldline tube with, for that particle, a unique time periodicity according to Eq.1. Further structural detail of the particle now must be expected to be related to a configurational structure of a constant time-slice of its worldline tube.

At first, the separateness of time and spatial features seems to defy any chances for a spacetime invariant description. Knowing the proven physical virtues of the invariance properties of spacetime descriptions, a major conceptual hurdle still has remained in reconciling the vast epistemological differences implied by time and space descriptions with the equalizer connotations of spacetime invariance. The latter unquestionably seems to wash out those very distinctions and remains for many a psychological stumbling block in accepting spacetime invariant description as a constructive asset.

The cited predicament of reconciling epistemological distinctions with the great equalizer of spacetime invariance has plagued physics for many decades. Right from the start, after Einstein had enunciated the principle of general spacetime covariance as an axiomatic cornerstone of his theoretical developments, the principle was attacked. In fact, it was declared to be physically void, and many modern renditions of the general theory of relativity reflect in varying degrees, a withdrawal from Einstein's originally more radical position.

The subsequent emergence of quantum mechanics as an exercise in solving eigenvalue problems, further compounded the situation. The many attempts at creating generally invariant Schroedinger and Dirac wave equations remained without physical perspectives. There was no gain that could be said to be commensurate with the efforts that went into these endeavors.

The issue presently confronting us, therefore, is one of weighing Einstein's early radicalism against the somewhat negative pragmatism unleashed by the criticism of the principle of general covariance. For this purpose, it is now instructive to take a good look at some purely topological developments which also have led to analogous distinctive asymmetries in making the step from three to four dimensions. Even if there may, at this moment, be no immediate ground for interrelating the two manifestations of dimensional asymmetry, the similarities are sufficiently striking to justify further cognizance. The mathematical subject matter referred to here has a reputation of abstractness and abstruseness, an intuitive approach to the torsion concept in manifold topology, therefore, is called for.

Since the word "torsion" has a variety of connotations ranging from differential geometry to the theory of deforming elastic bodies, it should be made clear, from the onset, that *topological torsion, as referred to here, is related neither to the torsion component of the linear connections of differential geometry, nor to the torsion concept of elastic deformation.* Topological torsion represents an entirely different conceptual species. In the present context, the reasons for directing attention to this concept of topological torsion are threefold:

(1) Topological Torsion has a distinct periodicity feature, thus permitting a possible accommodation of the previously given definition of the elementary particle as a "time"-replicating entity.

(2) Topological Torsion first becomes manifest as a distinct one-dimensional feature after making the transition from three to four dimensions. It thus opens a rare option for describing the dynamics of physical experiences.

(3) This concept of torsion is a topologically invariant notion and therefore is perfectly compatible with a principle of general four-dimensional (spacetime) covariance. These topology-related matters invite an extension to a metric-free rendition of the principle of general covariance.

An inspection of the here-given criteria of recognition for topological torsion and a comparison with the previously given worldline tube description of elementary particles reveals a measure of similarity which is far too striking to be passed up for further scrutiny. Hence, in judging its physical potential, we need to explore the possibility of bringing physical heuristics closer to the mathematical idea of topological torsion.

4. Dimensional Restrictions on Torsion

While topology has, for some time, made inroads on the teaching of modern calculus courses attended by physicists, the subject, as taught, is normally restricted to the topology of point sets without many ramifications dealing with the topology of manifolds. The discipline dealing with the latter is referred to as "combinatorial topology." It has striking applications in three as well as higher dimensions, except that the visual contact is lost in the higher-dimensional realm. At this juncture, manifold topology becomes as abstract as point-set topology. The first attempts at bridging the conceptual gap between these two branches of topology started, in fact, with the search for a set theoretical foundation of the geometric notion of dimension.

Analogous to the transition from visual geometry to algebraic geometry, the higher-dimensional counterpart in topology has become known as algebraic topology. In an exploratory investigation of the interrelations between physics and topology, a cursory overview of some modern-day tools of algebraic topology is in order. Since a feature resembling topological torsion already has been identified in the previous section and in chapter XII, the conceptual ingredients that have historically led to the notion of topological torsion, and the restrictions on its manifestation in manifolds, need to be discussed now.

The problem is to go about this assignment with sufficient mathematical precision so as not to risk immediate rejection from its professionals, while still retaining contact with a backdrop of reality to hold an interest for physics. For better or for worse, here is an attempt at such a heuristic report of the salient points of the homology branch of algebraic topology. Tedious constructional phases of "simplicial" subdivisions of topological "configurations" are simply left out. No proofs are given in a traditional mathematical sense; just major concepts are mentioned and placed in a physically plausible perspective.

The word "configuration" is meant to cover here a diversity of terms used in the professional literature (*e.g.*, "vertices domain" or "(curved) polyhedra" etc.). In the practice of physical applications, "configuration" refers here to domains of integration of physically relevant differential forms. The properties of differential forms of being *closed* forms or *exact* forms have been used by de Rham to create a dual instrument of topological probing, called "cohomology." Physics invites a synthesis, between the homology of integration domains and the cohomology of forms that are being integrated. Here are preliminary installments of this scheme!

In the last part of the nineteenth century, the Italian mathematician, Betti, pioneered a topological characterization of geometric configurations in space, n=3, with the help of Abelian groups. For arbitrary n we have as elements of the groups in n-dimensional manifolds the, in principle, infinite sets of p-cycles (*i.e.*, zero boundary or closed, p-dimensional hypersurfaces: p=1,...n) associated with a given geometric configuration. In Betti's visual spatial context, the cycles can be regarded as one-dimensional loops and two-dimensional closed surfaces. A general n-dimensional configuration can have cycles of dimensions 1,2, ...n.

The next step is subdividing this amorphous infinity of 1-cycles (or p-cycles) into subsets of bounding cycles, as distinct from the nonbounding cycles. This criterion for subdivision has been called "homology." The resulting equivalence classes of homologous p-cycles form a finitely generated Abelian group, which is the difference (quotient) group of all cycles versus the subgroup of bounding cycles. The structure of this group of equivalence classes relates directly to the topological structure of the configuration under consideration; it is called the "p-dimensional homology group" of that configuration. The topology of an n-dimensional configuration is probed by (n+1) homology groups H_p with p=0,1,2,.....n. The meaning for p=0 is still to be specified.

The groups H_p are Abelian and are considered to have a finite basis. It means H_p can be regarded as a module or a finite dimensional linear vector space if you will. The dimension r_p of that vector space is called the rank of H_p and depends solely on the configuration being examined. The number r_p (basis) is, in fact, a measure for the topological complexity of the configuration. For a given p, the number r_p can be smaller, equal, or much, much bigger than p.

As a physical example, one may think of Gauss' law of electrostatics as a tool describing the topology of a distribution of elementary electrical charges in three-dimensional space. In that case, p=2 corresponds to the two-dimensional integration cycles for Gauss' law, and r_2 relates to the number of electrons under consideration. The Gauss integral is zero if no charges, or no net charge, are enclosed. In establishing the *basis* dimension r_2, charge polarity needs to be taken into account. This basis dimension, or Betti number r_2, can be enormously large for 2-cycles enclosing an electron cloud.

The torus gives a simple geometric example: for p=1, the basis $r_1=2$ corresponding to the azimuthal and meridional equivalence classes of cycles on the torus surface, for p=2 one has $r_2=1$ the torus surface itself.

The numbers r_p, which give the rank of the basis of the H_p, say that H_p is the direct sum of r_p Abelian subgroups of rank one. They are the generators of H_p; each generator group has its own unit elements e_i with $i=1,2,....r_p$. An arbitrary element of H_p is an equivalence class of cycles defined by an array of "multiples" of the unit elements e_i (*i.e.*, a vector in the vector space spanned by the basis of H_p). For the simple case of the torus, an arbitrary cycle with vector components $[\![a,b]\!]$ is a combination of "a" meridional windings and "b" azimuthal windings. In geometric cases the coefficient domain of the vector space are integers; in physics they can be multiples of units e, h and h/e.

An inspection of the torus example shows that the infinity of meridional-azimuthal combination cycles contributes no essentially new information about the group structure of H_p , or rather H_2 , in this case. The most important structural element of H_p is the basis number r_p; which is 2 for the independent 1-cycles on the torus. The number r_p is not affected, whether one takes as coefficient domains the integers or the rational numbers. The group is, after all, merely a tool for assessing the topological structure of the configurations being investigated.

The vector space picture for the cycle basis of H_p , so far, suggests that no rank-one subgroup of H_p might be cyclic of finite order. Unit elements of vector spaces usually are taken to be cyclic of infinite order, meaning one can never return to the origin after a finite number of operations.

Mindful of the dominant role of algebraic methodology in geometry and in topology when going to higher dimensions, Poincaré first indicated that it might be meaningful to include the possibility of basis elements of finite cyclic order. The number r_p thus includes rank-one groups of finite and infinite order. The generating groups of finite order form a subgroup of H_p, called the torsion group T_p associated with the dimension p.

From a purely algebraic point of view, there would be no dimensional restrictions on whether or not a given generating group can be a torsion group. However, there are restrictions as soon as the additive group is declared to be an homology group pertaining to a given topological configuration.

A specification still needs to be made for p=0. The elements of the homology group H_0 are taken to denote the number of components of the configuration. A nontrivial torsion group is ruled out here (*i.e.,* $T_0=0$). A single component configuration is thus denoted by a one-element group H_0. A similar specification holds for the top dimension. So torsion is ruled out for the group H_n (*i.e.,* $T_n=0$). This specification is compatible with the possible occurrence of configurations with Poincaré duality $H_p=H_{n-p}$. Hence, in general:

$$T_0=T_n\equiv 0. \qquad 2$$

Further restrictions on torsion are imposed by recurrence relations between the homology and torsion groups as a result of Alexander duality between the homology groups of any given configuration C and its complement with respect to the manifold M_n in which C exists:

$$T_{r-1}(C)\approx T_{n-r-1}(M_n\text{-}C). \qquad 3$$

On the basis of Eqs.2 and 3, one easily establishes that torsion cannot exist in three dimensions. Torsion groups T_0 and T_3 vanish in view of Eq.2, still leaving the option of one- and two-dimensional torsion. For n=3 and r=1, Eq.3 gives $T_1(M_n\text{-}C)\approx T_0(C)$; hence, T_1 vanishes in view of Eq.2.

The result is general, because C and therefore M_n-C are arbitrary. Similarly the existence of T_2 can be ruled out because Eq.3 does not permit negative dimension numbers.

Since $T_o=T_1=T_2=T_3=0$, it follows: **torsion is not possible in three dimensions or less.** The geometric examples of torsion of the projective plane and the Klein bottle cited in the literature are somewhat contrived pseudo-visualizations which have their real origins in four dimensions.

Let us now check the situation for n=4. Starting with Eq.2, we have $T_0=T_4=0$. Now, applying Eq.3 for r=2, one obtains $T_1(C)$—$T_1(M_n$-C), which indeed can be different from zero. For r=3, one obtains similarly, $T_2=0$. The result $T_3=0$ follows for r=4, because T_{-1} is ruled out. Hence. for n=4; $T_0=T_2=T_3=T_4=0$, yet $T_1\neq0$, so, **one-dimensional torsion can exist in four dimensions because $T_1\neq0$.**

Modern textbooks on algebraic topology normally do not present proofs of torsion options in manifolds. At least two independent proofs of the torsion options in manifolds can be found in the classic topology text by Alexandroff and Hopf.[1] One proof calls on a decomposition theorem relating the Betti number r_{n-1} of C to the components of the complement configuration (M_n-C). The other proof is based on the Alexander duality between C and (M_n-C). The here-given account is a plausible version of the latter and is referred to in a footnote of ref.1. It makes this unusual and little-known result understandable, by calling directly on a fairly standard duality of algebraic topology.

Since the details of at least two full proofs have been given elsewhere by leading pioneers in the subject, the present arguments are only meant to serve as a frame of reference to show where these ideas come from and how they have evolved.

5. Torsion and Kuenneth Product Rules

The integration domains of multiple integrals frequently are configurations that come about by product forming between configurations of lower dimension. For instance, the two-dimensional torus is a configuration that may be regarded as the product of two one-dimensional cycles. In the light of the preceding discussions, it thus stands to reason to inquire how the homology groups of a product configuration relate to the homology groups of the product components.

An interest in the topology of product configuration is invited by their possible function as integration domains of period integrals. If the product structure of integration domains can be examined in the context of a corresponding product structure of the integrands, it is possible to come to some interesting conclusions for the evaluation of those integrals.

If differential forms happen to be products of differential forms of lower order, a multiple integral can simplify to a product of integrals. If the latter are, moreover, period integrals equatable to multiples of quanta, the evaluation of those multiple integrals then is immediate; they become products of quantum units and their quantum numbers.

This type of evaluations parallels the Cauchy method in the complex plane. It is even simpler, because the Cauchy process demands an evaluation of residues; here nature's quanta provide the residues. These options of product reduction point at a need to become informed about product homologies.

Let us now quote a major result, due to Kuennetz, as to how product homology relates to the homology of the components. Let C_{p+q} be a product configuration of the component configurations C_p and C_q in a manifold of n dimensions. For the index n going from 0 to n, the C_{p+q} homology groups then relate to the homology groups of C_p and C_q according to the relation:

$$H_n(C_{p+q}) \approx \sum_r [H_r(C_p),H_{n-r}(C_q)] + \sum_r [T_r(C_p),T_{n-r-1}(C_q)] \,. \qquad 4$$

Eq.4 simplifies considerably in the light of the earlier-discussed restrictions on torsion groups. A four -dimensional space gives n the range n=0,1...4, and assuming for C_p and C_q cycles of dimensions p=1 and q=2 respectively, the second summation vanishes, because one of the torsion groups always vanishes.

The following relations are thus obtained when taking into consideration that C_1 has only two nontrivial homology groups: $H_0(C_1)$, $H_1(C_1)$ and C_2 can have three nontrivial homology groups: $H_0(C_2)$, $H_1(C_2)$, $H_2(C_2)$. They give four nontrivial homology groups for $C_3=C_1\times C_2$, the product cycle. Writing out the summations of Eq.4, they are:

$$H_0(C_3)=H_0(C_1),H_0(C_2)$$
$$H_1(C_3)=H_0(C_1),H_1(C_2)+H_1(C_1),H_0(C_2)$$
$$H_2(C_3)=H_0(C_1),H_2(C_2)+H_1(C_1),H_1(C_2)$$
$$H_3(C_3)=H_1(C_1),H_2C_2) \,. \qquad 5$$

The commas of the bracket expressions in Eq.4, still appearing in Eq.5, are to be understood as additive connectors.

The Eq.5 become a more transparent once we become more specific about the homology groups of the components. While the homology groups of the lower-dimensional configurations can be verified by visual inspection, Eq.4 has the virtue of helping to obtain an insight into the potential homology structures of higher-dimensional configurations where visual inspection fails us. In four dimensions though, kinematic behavior gives an additional realization potential for topological configurations, over and beyond the traditional three-dimensional geometric realizations used in standard topology.

Instead of dealing with the homology group elements themselves denoting equivalence classes of cycles, it is more elucidating to cite the rank $r_p(d)$ or Betti number, which gives the group basis. The compound symbol $r_p(d)$ thus enumerates the group basis of the p-th homology group of a d-dimensional configuration. They identify the number of independent equivalence classes of boundaries and cycles that are the elements of the homology groups. Here are some specifics:

Let C_1 be the topological equivalent of a circle; its Betti numbers are (1,1). The first Betti number 1 signifies that there is only one (circle) component, the second Betti number 1 says the circle itself is a single equivalence class indicating a one-element group. The topological equivalent of the sphere has the Betti numbers (1,0,1). The first and the last Betti numbers "1" are also here similarly identified as for the circle. The zero in the middle says all one dimensional circles on the sphere are boundaries separating the surface, in the sense of a Jordan curve into two parts. The sphere is said to be simply connected or more precisely, the sphere is one-connected, because the one-cycles on the sphere are all boundaries constituting an equivalence class identified by the zero-th element of a trivial homology group of zero basis (*i.e.*, $r_1(2)=0$).

A three-dimensional sphere, or the topological equivalent thereof, has Betti numbers (1,0,0,1). It is also said to be simply connected or more specifically it is 1- and 2-connected. The 4-sphere has Betti numbers (1,0,0,0,1), simple-connectedness here means 1, 2, and 3-connectedness.

Definition: The Betti numbers $r_p(n)=1$ for p=0 and p=n and $r_p(n)=0$ for 0>p>n are taken as **criterion for simple-connectedness** of cyclic n-dimensional configurations.

A torus surface is the product of two circles. Visual inspection reveals its Betti numbers as (1,2,1). One also easily verifies this by writing out the terms of Eq.4. Let us now consider the 3-dimensional product configuration of a circle (1,1) and a sphere (1,0,1). Eq.5 can now be used to write down immediately the Betti numbers for the three-dimensional product configuration:

$$C_1 \times C_2 = (1,1,1,1). \qquad 7$$

For the product of a circle and a three-sphere one obtains from Eq.4 for the Betti numbers of the four dimensional product configuration

$$C_1 \times C_3 = (1,1,0,1,1). \qquad 8$$

The product of two 2-spheres (1,0,1) gives, by contrast,

$$C_2 \times C_2 = (1,0,1,0,1). \qquad 9$$

A comparison of the Betti numbers for configurations Eqs.7,8,9 shows that Eqs.7, 8 and 9 satisfy the symmetry of the so-called "Poincaré duality" relation

$$r_p = r_{n-p} \,. \qquad 10$$

The same can be said for the Betti numbers of the circle, sphere and torus. It means Poincaré duality (only) holds for closed configurations (*i.e.*, configurations with zero boundary). The products of the closed configurations, here considered, are again closed.

There is another remarkable feature that product configurations have in common; they are not simply connected, even if the factors of the product are. An inspection of all the given examples verifies the given statement. Since this property is needed for applications to be discussed in the next section, let us state this as a lemma for future reference:

Lemma I: Product configurations are not simply connected.

As a second Lemma, one can verify the converse statement:

Lemma II: A simply connected configuration cannot be represented as a product of lower-dimensional configurations.

As mentioned earlier, torsion does not play any role in the given product discussions. While torsion can have a one-dimensional manifestation in four dimensions, it cannot have an independent role in the lower dimensional factor configurations. The torsion feature can thus be injected as an option, according to imposed epistemological needs, only after the product configuration requires a four-dimensional imbedding..

6. Homology of Integration Cycles, Cohomology of Forms

The present concern with homology theory was predicated by the need for an improved understanding of the topology of integration domains of multiple integrals. As mentioned earlier, homology started in the 19th century and developed further in the beginning of this century. The Kuennetz product rules, for instance, go back to the Twenties. In the course of time, other methods for probing topological structure were developed.

A method that was in part dual to homology theory emerged under the name of contra-homology or briefly "cohomology." This procedure has been given, for physics, an especially poignant form by de Rham. The mathematical development took place in the Thirties. Starting in the early part of the 19th century, physics had been using isolated fragments of what has now been integrated by de Rham into a full-fledged theory of co-homology. Let us delineate the principal mathematical features of cohomology as a structural dual to homology, then after this is done physics-based connotations can be injected.

If homology is based on the rather subtle conceptual difference between cycles that are boundaries versus cycles that are not boundaries, cohomology is based on the conceptual distinction between differential forms that are *closed* versus those that are *exact*. While all boundaries are cycles, not all cycles are boundaries. Similarly all exact forms are closed; yet not all closed forms are exact. The exact forms are a subset in the set of closed forms, as boundaries similarly are a subset in the set of cycles. Since these sets are also Abelian groups one may consider their subdivision in terms of equivalence classes having the group property. The difference (quotient) group of closed p-forms versus exact p-forms is called the cohomology group H^p, similarly as the difference group of p-cycles versus p-boundaries is called a homology group H_p. The importance of these dual constructs is their conditional isomorphism

$$H^p \approx H_p \,, \qquad 11$$

which is contingent on the absence of torsion properties in H_p , because H^p has no torsion equivalent. This means T^p is an asymmetry feature between homology and cohomology.

Let us reiterate here these and other symmetries and asymmetries in homology and cohomology. A differential form A is said to be *closed* if its exterior derivative vanishes: $dA=0$. The condition is not sufficient for *exactness*. The symbol d stands for the exterior derivative operation (alternating differentiation); similarly, the symbol ∂ is taken to stand for the boundary operation on a given configuration (chain). While the boundary of a boundary is zero: $\partial\partial\equiv 0$, also the exterior derivative applied twice vanishes: $dd\equiv 0$. The relevance of these general identities to familiar mathematical operations commonly used in physics becomes clear by noting how the identities in three dimensions **curl grad** $\equiv 0$ and **div curl** $\equiv 0$ are special cases of dd=0. Details are found in texts dealing with the subject matter of differential forms (compare chapter VI).

A differential p-form B is *exact* if it derives from a (p-1) form A (*e.g.*, B=dA). Since $dd\equiv 0$, operating again with d gives dB=0.

Warning: a local global distinction related to closed and exact is reflected in the information that the knowledge dB=0 does not permit a conclusion (standard for a long time in physics) that a form A (globally) exists such that dA=B.

In different wording: the property of an exact form of being closed does not permit an inversion by concluding that closed forms are derivable from forms of lower order (*e.g.*, a so-called "potential"). In making such unjustified inversions of conclusions, one throws out precisely the possibility of making a cohomology probe of the (field) configurations that are being assessed. While pioneering 19th century physicists seemed mostly aware that this inversion was subject to restrictions, the subtlety of the cited distinction has escaped later generations. Half a century of off-and-on searches for magnetic monopoles has been largely precipitated by a tacit belief in this inversion viability. It testifies to the relevance of these mathematical subtleties.

Whether a field or form is "closed" or "exact" is fundamentally an issue to be decided by experimental inquiry. The magnetic monopole inquiry testifies to this fact. Yet, questions are justified as to how many times physical research should be allowed to ignore Nature's resounding "NO" to the magnetic monopole inquiry. The conceptual inertia displayed in these forever recurring investigations is the more questionable now that the physical relevance of a globally existing potential **A** for the field of magnetic induction **B** has proven its global significance through the many physical applications of the Aharonov-Bohm integral.

Not only in economics, but also in contemporary physics has a consensus abounded favoring the idea of having one's cake and eating it too. A delineation of priorities is therefore necessary to make it clear that the following selection of differential forms and their physical implications don't permit easy compromises jeopardizing cohomological principles.

As fundamental 1,2 and 3 forms that are "closed," we consider:

1. The pair 1-form A defined by the vector and scalar potentials $\{\mathbf{A},\phi\}$ explored by London, Aharonov and Bohm.

2. The impair 2-form $\mathfrak{G}$ defined by the displacement **D** and the magnetic field **H,** which gives a spacetime substantiation of the laws of Ampère and Gauss.

3. The impair product form $\mathsf{A}\wedge\mathfrak{G}$ suggested by Kiehn.[2]

The residues or periods of these differential forms are respectively in terms of units of **flux** h/e, elementary units of **charge** e and the elementary units of **action** h (*i.e.*, integrated spin or angular momentum):

1. $\oint_{c_1} \mathsf{A}$ = **flux** linked by a c_1 residing where $d\mathsf{A}=0$. 12

2. $\oint\oint_{c_2} \mathfrak{G}$ = **charge** world lines linked by a c_2 residing where $d\mathfrak{G}=0$. 13

3. $\oint\oint\oint_{c_3} \mathsf{A}\wedge G$ = **action** "enclosed" by a c_3 residing where $d(\mathsf{A}\wedge\mathfrak{G})=0$. 14

The completion of a physics-based cohomology now is contingent on finding exact 1, 2, and 3 forms. An exact 2-form is defined by the electric field **E** and the magnetic induction **B**: $F=dA$. An exact 3-form $\mathfrak{C}$ is defined by the charge density ρ and the current density **j**. It obeys the relation $\mathfrak{C}=d\mathfrak{G}$ (the matter-based Maxwell equations), because the 2-form $\mathfrak{G}$ is defined by displacement **D** and magnetic field **H**.

The Pfaffian integrability classes of A give additional options for exact 1,2 and 3 forms according to the following scheme, based on a theorem by Darboux. This theorem reduces the one-form A to bilinear expressions of four independent spacetime scalars $\alpha, \beta, \gamma, \delta$. They represent, in the following manner, the four distinct cases of integrability:

1. $A = d\beta$	$A \neq 0;\ dA = 0$	15
2. $A = \gamma d\delta$	$dA \neq 0;\ A \wedge dA = 0$	16
3. $A = \gamma d\delta + d\beta$	$A \wedge dA \neq 0;\ dA \wedge dA = 0$	17
4. $A = \gamma d\delta + \alpha d\beta$	$dA \wedge dA \neq 0.$	18

Case 1, Eq.15 yields, by definition, an exact one-form A, because it derives from a scalar β.

Case 2, Eq.16 yields an already-mentioned exact 2-form $dA=F$, which is physically defined by the electric field **E** and the magnetic induction **B**.

Case 3, Eq.17 is governed by a 3-form $A \wedge dA$. This 3-form is at least closed, because its exterior derivative is given as $dA \wedge dA = 0$. There is a rough-and-ready rule for differential forms claiming **closed form$\wedge$exact form=exact form**, which needs to be applied with caution. The 3-form $A \wedge dA$ does not always have to be be exact, because the integral criterion for exactness

$$\oint\!\!\oint\!\!\oint_{c_3} A \wedge dA = 0; \text{ for all cycles } c_3 \qquad 19$$

holds if dA is of rank 2, as is true for case 3. Applying Stokes' Law to Eq.19, the integral over the 4-dimensional interior of c_3 has a vanishing integrand in view of the accessory requirement $dA \wedge dA = 0$ for case 3.

Case 4, Eq.18 reflects a situation of minimal integrability, dA is of rank 4. It is described by a nonzero 4-form: $dA \wedge dA \neq 0$. This nonvanishing 4-form is physically equivalent to a nonzero spatial scalar product $\mathbf{E} \cdot \mathbf{B} \neq 0$ of electric and magnetic fields. Stokes' theorem now says the integral Eq.19 is nonzero. The rough rule **closed$\wedge$exact=exact** is now invalid.

Case 4 generates the following sequence of cyclic integrals of 1, 2 and 3 dimensions:

$\oint A \neq 0$ nonzero periods of flux, 20

$\oint\oint dA \equiv 0$ zero periods: no magnetic monopoles, 21

$\oint\oint\oint A \wedge dA \neq$ nonzero periods of $(\text{flux})^2$; see ref.3. 22

Eqs.20,21,22 formally wrap up the possible integrability situations pertaining to A. Note for future reference that, even for integrability case 4, Eq.19 can still be met, provided the cycles c_3 are restricted to products of 1 and 2-cycles

$$c_3 = c_1 \times c_2 \,. \tag{23}$$

The Kuennetz product rule gives

$$\oint\oint\oint_{c_3} A \wedge dA = \oint_{c_1} A \oint_{c_2} dA \equiv 0, \tag{24}$$

implying, in view of Lemma II, that Eq.22 can only be nonzero for simply connected c_3. Eq.24 follows from Stokes' theorem:

$$\oint\oint_{c_2} dA = \oint_{\partial c_2} A \equiv 0. \tag{25}$$

Integral Eq.25 vanishes, because the boundary of a cycle is zero: $\partial c_2 = 0$. Conversely, if all cyclic integrals of a form vanish, the form is exact.

Eq.25 says that $F = dA$, all by itself, can never establish a 2-connectedness in spacetime. Yet what F can't do, another form can! The 2-form $\mathfrak{G}$ of Eq.13, which is defined by the displacement **D** and the magnetic field **H,** assumes the role of establishing 2-connectedness in spacetime. The Gauss theorem of electrostatics joins here with Ampère's law, and together, they assume a dynamic spacetime role.

In contrast to the integral given by Eq.24, an integral of the product form $A \wedge \mathfrak{G}$ of Eq.14, for a corresponding product configuration of integration $c_3 = c_1 \times c_2$, now translates into a finite 3-connectednes that is coupled to what can be a nonzero 2-connectedness:

$$\oint\oint\oint_{c_3} A \wedge \mathfrak{G} = \oint_{c_1} A \oint\oint_{c_2} \mathfrak{G} = \text{action-integrated "spin"}; \; c_3 = c_1 \times c_2 \,. \tag{26}$$

Until this point, the discussion has centered around integrations over cyclic configurations. Integrability case 4 invites a look at the interior of cycles (*e.g.*, integrals of the 4-forms $dA \wedge dA = F \wedge F$ and $\mathfrak{G} \wedge \mathfrak{G}$ and $F \wedge \mathfrak{G}$ taken over spacetime interiors). Using the equivalent field quantities of standard Maxwell theory they appear in more familiar attire as integrals over time element dt and space elements dV respectively :

$$\int dt \iiint \mathbf{E} \cdot \mathbf{B} \, dV = \oint dA \wedge dA \qquad \text{phys. dim. } (h/e)^2 \tag{27}$$

$$\int dt \iiint \mathbf{D}\cdot\mathbf{H}\,dV = \int \mathfrak{G}\wedge\mathfrak{G} \qquad \text{phys. dim } (e)^2 \qquad 28$$

$$\int dt \iiint (\mathbf{E}\cdot\mathbf{D} - \mathbf{H}\cdot\mathbf{B})\,dV = \int F\wedge\mathfrak{G} \qquad \text{phys. dim. } h \qquad 29$$

Unlike the integrand of Eq.27, the 4-forms of the integrands of Eqs.28 and 29 are not in general derivatives of 3-forms. Yet, as is too frequently done in physical applications, in charge-free space one can *locally* take exception to this restriction by locally defining the existence of such a 3-form. In charge free space, the Lagrangian integrand of Eq.29, for instance, is a pure divergence of the vector $\phi\mathbf{D} - \mathbf{A}\times\mathbf{H}$. It is not, in general, possible, however, to globally extend such definitions, because they violate basic cohomology distinctions by taking closed to be exact.

It is even less permissible to make an exact form closed, as shown by the inverse predicament of the absent magnetic monopole. In the first case, one takes advantage, for the purpose of expediency, of the fact that things don't exist locally that in principle could exist there. In the monopole case, one assumes things to exist that, according to cohomology, can't exist. Expediency may accept a risk, yet magnetic charge violates a truth.

A conscientious distinction between the notions closed and exact, in conjunction with Stokes' theorem, is therefore at the heart of any attempt at a cohomological restructuring of physical theory. The cycle-boundary distinction, in conjunction with the separation feature of a Jordan-Brouwer cycle, is similarly at the heart of homology.

7. Cohomology Characterization of Physical Objects

Having thus outlined the ingredients for an homology of integration domains of differential forms, and a corresponding cohomology of forms with conspicuous physical connotations, new questions arise whether or not they can be synthesized into a tool aiding existing theories of elementary particle structure. All there is at this point are fragments of past and present knowledge that are indicative of such possibilities. In reporting about them, one finds there is too little to speak of a full-fledged theory; yet, there is enough so as to be reluctant to discard the whole effort as a hopeless endeavor.

Since we are trying here to classify particles with the equivalent of Betti numbers identifying the bases of pairs of homology-cohomology groups, the possibilities offered by this scheme seem, in principle, ample to accommodate the relatively small number of stable and semi-stable elementary particles.

An important question in this context is whether the classification can be expected to be unique. The answer is "no." The homology classification of geometric objects is already known not to be unique, in the sense that topologically distinct configurations can have the same Betti numbers.

The same is true, and even more so, for a cohomology classification. For the purposes of physics, there is the added distinction between pair and impair forms. This distinction is not normally acknowledged in mathematical discussions. The pair/impair distinction in physics relates to matters of orientability and the associated particle pairing principles of physics. A synthesis of homology and cohomology is needed to optimize results.

Notwithstanding reported negatives, to be taken as warnings against undue expectations, an abundance of differential forms can be identified as relating to fundamental physical fields. They leave little doubt about a relevance, in some form or another, for the purpose of ordering manifestations in the sense of a physical cohomology. While an inspection of the literature shows a fair number of publications dealing with the role of differential forms in physics, closer scrutiny reveals a great reluctance to develop these matters into, even an outline of, a cohomology for physics. Reasons for this reluctance need to be identified.

From a practical point of view, one would like to start with what topologically seem to be the simplest physical objects of nature (*e.g.,* elementary particles). For reasons that hardly need much elaboration, the physical laws associated with the differential forms discussed in the previous section normally are considered as macrophysical in nature, without a true applicability in the micro- and submicroscopic domains. Modern physics has shrouded these microscopic domains, and, even more so, the submicroscopic domains, in a sea of fuzziness created by what is known as quantum mechanical uncertainty. A total and unmitigated subscribing to the uncertainty notions of contemporary physics stops any microphysical cohomology investigation in its tracks, and that is exactly what has happened.

The present investigation was not started, though, just to withdraw from this endeavor prior to testing at least some of its potential for particle physics. Essential for this follow-up is a position which takes the viability of the contemporary "readings" of the axiom of uncertainty as unacceptable. Quantum mechanical uncertainty, as presently known, stands in the way of a systematic cohomological exploration of the particle realm. The *metric-free structure* of the thus far mentioned differential expressions is taken as a supporting inducement for taking this course of action. The metric gives us the macro-micro distinction; hence, the metric absence then takes away a major hurdle preventing macro$\rightarrow$micro extrapolation.

Let us now explore how elementary particle properties interact with the cohomological connotations of flux, electric charge, and spin as expressed by period integrals Eqs.12,13,and 14. Charge and spin are properties that can be directly attributed to elementary particles through experimentation.

The integral of flux, however, gives us some trouble. Here are some of the reasons.

Elementary particles are known to have magnetic moments, which can be evaluated experimentally with great precision. In macroscopic situations, we are used to associating a magnetic flux with a magnetic moment. If the flux is not closed on itself, such as in the case of flux linked by a torus-ring, it will have an external magnetic-moment manifestation. The mutual relations between magnetic moment and magnetic flux, therefore, are not unique. Flux can, in part, be torus-enclosed, whereas another part leaks out as an experimentally observable dipole moment. The flux-moment relations present us with questions regarding how to obtain independent knowledge of each. Such information could be indicative of specifics concerning the elementary particle's interior structure. In the present cohomological context, flux shall be regarded as a more fundamental concept than magnetic moment. The latter is seen as a secondary entity derivable from flux.

Tables of elementary particle properties give us listings of their masses in terms of energy units **mev**, thus covering finite restmass as well as zero restmass particles. These data are experimentally obtained from energy-momentum exchanges in collision processes. The cohomology classification, here attempted, still does not give us information about ratios of particle masses or energies. The period integrals Eqs.12,13, and 14 give us, at best, indirect clues as to the nature of the energies involved.

Table II gives a preliminary idea how Betti numbers could be assigned to some familiar elementary particles. While the Betti numbers for electron and muon are easily identified, the photon, by contrast, remains the least descript. Photon action, according to Eq. 29, should vanish, yet circularly polarized photons are known to carry angular momentum ±h. Electro-flux $\int dt \int \mathbf{E} \cdot d\mathbf{r}$ and magnetic flux $\int\int \mathbf{B} \cdot d\mathbf{S}$, when taken in a cyclic context, cancel on account of flux conservation, yet their individual magnitudes remain available as entities characteristic of photons.

As far as cohomology is concerned, it still remains true that electron and muon could be considered as perfectly identical entities. Hence the electron/muon enigma still stands out as largely unaffected by a cohomological probing. At this point, there is a reasonable cohomology-based argument accounting for the magnetic moment anomaly, which electron and muon have in common. For the higher order difference, there is at best a qualitative argument in terms of a small, yet inescapable, closed flux leakage associated with a trefoil model for electron and muon.

Quantum cohomology yields a very preliminary qualitative insight into the stationary structures of at least some prominent elementary particles. If we now include the stability versus instability features offered by the integral Eqs.22,27, also transitions between particles can be seen in a quantum cohomological perspective. It offers an elucidating qualitative insight into a possible understanding of Feynman diagrams by looking at them as four-dimensional neuron nets. The neuron centers are the event domains, where particles are taken apart to be reassembled into new particles. Particle properties of **Table II** are exchanged in event-domains. Charge and spin are conserved, flux conservation and topology are now available to be related to some of the known empirical number conservation rules.

TABLE II: A Preliminary Cohomology Classification of Particles

P		F	C	A	C←Betti number r_4			
A		L	H	C	L			
R		U	A	T	O	external and		
T		X	R	I	S	internal (E,B)field		
I		electro	G	O	U	manifestations		
C		magnetic	E	N	R	|	|	
L		|	|	"spin"	E	↓	↓	
E Betti number	r_0	r_1	r_2	r_3	r_4	B	E	$L=B^2-E^2/c^2$
↓	↓	↓	↓	↓	↓	↓	↓	↓
electron ±e	1	h/e	±e	±h	0	ext.	ext.	>0
muon ±μ	1	h/e	±e	±h	0	ext.	ext.	>0
and spectrum?		-	-	-	-	-	-	-
± e-neutrino	1	h/e	0	±h	0	masked by		<0
± μ-neutrino	1	h/e	0	±h	0	light cone?		<0
and spectrum?		-	-	-	-	-	-	-
pion	1	h/e	±e	±h	0	int.	ext.	>0
neutr. pion π_0	1	h/e	0	0	0	int.	int.	=0
photon	1	h/e	0	0	0	ext.	ext.	=0
	H_0	H_1	H_2	H_3	H_4	←Homology groups		

Table II is an attempt at a cohomological classification of elementary particles of the stable and semi-stable category. The Betti number r_0 secures the single component structure. The Betti number r_4 conveys the worldline manifestation of the particle object in question. The combination action≠0 and B internal implies spin=0.

The stable worldline tubes can only terminate in an event domain, if a collision induces such an event to take place. The semi-stable worldline tubes can terminate spontaneously in an event domain. The length of a worldline tube that branches spontaneously (*i.e.*,without entering into a collision) measures the intrinsic lifetime of the particle in question. The worldline "length" represents a measure for the progression of arrowed time. The inner dynamics of the particle establishes through Eq.1 a cyclic time characteristic of the particle. The particle worldline tube thus appears as an arrowed time string, which is beaded in terms of its own cyclic time.

The depicted Feynman diagram situation can further be specified in an homological sense. The event domain is taken to be the topological equivalent of a four-dimensional ball and is, by virtue of this fact, simply connected. The worldline tube, by contrast, is a four-dimensional product of a one-dimensional arc of arrowed time displacement and a three-dimensional configuration pertaining to the spatial particle structure. This worldline tube configuration is not simply connected by virtue of **Lemma I.**

The one-dimensional torsion number of a worldline tube configuration in spacetime is the lifetime of the particle in terms of its own characteristic cyclic time.

A crucial point for physical applicability is a cohomological distinction between event domains of instability and worldline tube domains of sustained stable particle life. The following theorem helps out in finding a distinguishing criterion between entities known as physical stable or semi-stable particles and physical events were particles undergo transitions:

Theorem: The Pfaffian integral (Eqs.22,27) vanishes, at all times, for the product domains of worldline tubes (Eq.24) of particles, whereas, by virtue of **Lemma II,** it can be different from zero if, and only if, the event domain is simply connected. This property makes Eq.24 a suitable candidate for assuming the role of a particle stability criterion.

8. Conclusion

The presented mathematical outline of a quantum cohomology can, at this point, hardly be regarded as a logically complete structure that can be developed from a few well-chosen axioms. Such axiomatic procedures are rare in mathematics, and even more so in physics. Confrontation with the reality of physical observation is bound to restrain and guide human imagination from what we believe things to be, towards what nature is trying to tell us how things really are.

A measure of justification for the here-chosen course of action in the direction of a quantum cohomology has been given by some quantitative applications ranging from macroscopic quantum effects to applications to the electron itself. In the light of the prominent role of the methods of period integration in the cited examples, we would have forsaken our obligation to the subject by not carrying matters further along the lines of a development that was, in part, already available in mathematics.

The conflict situation that has arisen with respect to the standard methods of quantum mechanics makes it mandatory to realign Copenhagen views. An epistemological assessment[4] strongly reaffirms early pronouncements by Einstein[5] and Popper[6] about the ensemble connotation of the wave function of quantum mechanics (compare chapters III-V).

Since homology and cohomology are not exactly household words in everyday physics, let us mention some mathematical sources that have well served this reluctant neophyte in these subjects. Apart from an earlier mentioned classical textbook by Alexandroff and Hopf[1] on homology, de Rham's early papers and textbook,[7] together with a monograph by Hodge[8] have served as primary sources for cohomology theory. Many newer texts are available; they may be cast in less accessible language. The just-mentioned classics have the advantage of starting from scratch and using simpler language. De Rham's text is the only one which makes the, for physics, so essential distinction between pair and impair differential forms. Relevant information about the pair/impair distinction, and its relation to tensor species compatible with applying Neumann's principle to media symmetries, have been compiled by Schouten,[9] a survey of essentials is given in Chapter VI of this text.

In the course of these discussions, the metric-free aspects of certain physical-law statements have been mentioned time and again to justify an extrapolation of macrophysical laws into the microphysical domain. The first discoveries of metric-free aspects emerged in the Twenties and Thirties through the work of Kottler, Cartan and van Dantzig. An easily accessible discussion of the mathematical facts can be found in a book by Whittaker.[10] A discussion of some physical implications of metric independence, in connection with period integrals as counting laws, has been given by the present author:[11] an overview in chapter VI. Applications of period laws to the quantum Hall effect[12] and the electron's anomalous magnetic moment[13] have led to experimentation with quantum cohomology where standard methods show diminishing returns (chapters VIII, IX).

There has been a conspicuous reluctance in either accepting, or even considering the period integral methods of quantum cohomology. Also attempts at taking issue with them have been conspicuously absent. The origin of this evasiveness can only be traced back to what is believed to be a conflict situation with standard quantum methods and an ensuing overpreoccupation to seek solutions within the existing realm. More radical reassessment of prevailing views of quantum mechanics is still on hold.

In extolling mathematical virtues against physical virtues, and vice versa, let us make a comparison of the conceptual ingredients and quantitative results for a quantum electrodynamics (QED) approach and a quantum cohomology (QC) approach to the anomalous moment of the electron (see chapter VIII):

	QED	QC
1.	Fundamental premises are metric-based.	Fundamental premises are metric-free.
2.	Electron-muon topology is reduced to a point.	Electron-muon topology is related to a trefoil.
3.	Energy infinities are trimmed by normalization recipes.	Charge kinematics permits speed of light.
4.	First order moment anomaly shared by electron and muon: $\alpha/2\pi$.	First order moment anomaly shared by electron and muon: $\alpha/2\pi$.
5.	Quantitative account of higher-order anomalies.	Qualitative account of higher-order anomalies.
6.	Leads to discrepancies with α data from other sources for higher order predictions: see ref.14	No quantitative comparison in view of unavailable quantitative higher order predictions.

The reader should note that the comparison under point 3 lists, in either case, propositions that may be considered weird. The energy infinities of **QED** need to be weighed against the **QC** proposition of permitting the speed of light for the substance of charge. The absence of metric-based restrictions for the 3-form of charge and current density brings the latter proposition within the realm of formal acceptability. Supporters of **QED** procedures, naturally, can make a similar claim; their methods for trimming out infinities make infinities more acceptable.

A qualitative understanding of the electron's negative higher order corrections in the **QC** is due to the trefoil's inherent azimuthal leakage of flux closing on itself which, for that reason, can't contribute to the electron's magnetic moment. The muon's positive higher-order correction can then be attributed only to an increased electrostatic energy outweighing the decrease due to azimuthal flux leakage (compare chapter XI).

A comparison of the Dirac equation's spinorial justification for the electron's half-integer spin $\hbar/2$ versus the trefoil's quantum cohomological argument, reveals a relation between spinorization and orientability. This aspect has been discussed by Haefliger.[15] It lends uniqueness to the enantiomorhism of the trefoil's double-loop knot, which imposes a 4π integration loop on the integral $\oint A$ of Eq.12. Since this integral enters into the expression of Eq.14 for integrated spin, the ensuing *differential* spin in the Dirac equation is the value of Eq.14 prior to the integration over 4π: *i.e.*, $h/4\pi = \hbar/2$. The **Dirac-QC** comparison for justifying the electron's half integer *differential* spin gives:

Dirac Eq.	**QC**
half integer spin corresponds to unit operation 4π of the spin group, which, in turn, reflects orientability and the possibility of enantiomorphic pairing.[15]	half integer spin corresponds to 4π trefoil loop integration, based on the prerequisite of enantiomorphic pairing, which needs an orientable spacetime.[15]

For the quantum Hall effect, **QC** methods hardly compare with the elsewhere-proposed applications of the many particle Schroedinger equation. Proponents of the Schroedinger approach expect to obtain, in this fashion, evidence for the mesoscopic order manifest in the quantum Hall observations. This expectation creates the predicament of how to accommodate the statistics inherent to this method in a situation of near-perfect order. *Only a nonclassical statistics can be made to perform such acts of disappearance.* The **QC**, by contrast, starts out accepting the cyclotron lattice order, which reopens the field to yesteryear's simplicity of period integration, while rejecting, by the same token, the applicability of Schroedinger methods to such highly ordered situations.

The conceptual ingredients of cohomology have been prepared in the workshop of mathematics, some of it, perhaps, inspired by physics, but then made independent to stand on its own. Others have lifted the idea of metric-free aspects of physical law out of anonymity. It is de Rham's cohomology of differential forms in conjunction with aspects of metric independence that call for resurrecting period integrals as a major tool of global methodology in physics.

Contemporary quantum mechanics, with its uncertainty thesis and its multidimensional configuration spaces, has prevented physics from fully embracing developments in spacetime cohomology. It is hard, and perhaps literally the long way around, to find physically relevant and cohomologically complete sets of closed and exact differential forms in higher-dimensional configuration spaces, in the expectation there will somehow exist a reduction that gives physical meaning in spacetime.

In recent times, "string" theory related topological explorations of higher dimensionalities by E. Witten[16] and others remind us that all of this is, after all, pursued in order to account for spacetime physical experiences. Witten calls attention to metric-free aspects of these developments. This "new" metric independence has, so far, not shown an awareness of the earlier metric independent work of the Twenties and Thirties (compare index references to metric-independence). Yet, these recent reports can be taken as an encouragement to support Witten in his call for a metric-free (extended) principle of general covariance for spacetime physics, because this extended covariance permits the one and only invariant reconciliation between quantum principles and general theory of relativity (chapter XV).

Yet similar as Whittaker's[10] account of the metric-free work of Cartan and others, also this new wave of awareness of "metric independence" remains in a physical state of indecision. There is no answer as to why nature would make part of physics independent of the existence of a Riemannian metric structure. Neither is there a mentioning of the macro$\rightarrow$micro implications of metric-free, nor an identification of metric-independence as a prerequisite for an invariant counting of quanta.

The epistemological assessment of quantum mechanics by Einstein and Popper in the early Thirties remains a *sine qua non* for physics to escape from the intimidation of Copenhagen's "painted corner" of those who were incapable of understanding. The Popper move initiated the liberation of physics from the dominant rule of the single-system wave functions. In so doing, he opened up a possibility of erecting a global spacetime quantum extension on Maxwell theory, which, in turn, makes possible a physically relevant spacetime quantum cohomology.

Since spacetime's *algebraic* geometry now has an undeniable physics connotation through the general theory of relativity, a similar association must be expected for a spacetime *algebraic* topology. That is what this chapter aims at doing, on an exploratory basis. As the relevance of this association dawns at the horizon of awareness, Poincaré torsion emerges as an asymmetry between homology and cohomology.

The kinematics of periodic phenomena naturally invites a torsion association. Periodicity in space can somehow be visualized only as object-multiplicity. Periodicity in time, by contrast, is a self-replicating feature of a single object in spacetime. The exclusiveness of an inherent self-replicating cyclic property of a single physical object manifests itself, for the first time, in the four-dimensional context of a kinematic periodic phenomenon. The same is true for the abstract torsion concept of algebraic topology. The possibility of whether time periodicity is a valid physical realization of Poincaré's torsion is not to be ignored.

A principal objective of this chapter aims at establishing the feasibility of a model-based visual counterpart of the very abstract methods of quantum electrodynamics QED and quantum chromodynamics QCD. In the spirit of earlier considerations about topological torsion, the word "visual" is here to be taken in the static as well as in the dynamic, four-dimensional sense of kinematic observation. An existence and possible use of model-based alternatives to the otherwise exclusively forbidding abstractions of QED and QCD is something whose time may have come, provided a reduced role of quantum uncertainty can be accepted. In mathematics, the possibility of torsion at the homology end, and its absence at the cohomology end has, at times, been cited as a disturbing asymmetry. In spacetime assessment, this asymmetry is found to have an essential function for the interpretation of cyclic and arrowed time. In the spirit of *weak* homology, it permits a closing of time-integration loops of period integrals. It makes possible a synthesis of homology and cohomology to accommodate a topology of objects and a topology of events.

The content of this chapter owes an important input from many early discussions with the late David Bourgin and with Robert M. Kiehn during my stay at the University of Houston in Houston, Texas.

CHAPTER XIV

OPTIMIZING REDUCTION TO FAMILIAR CONCEPTS

1. The Art of Explaining

Most attempts at explaining something unfamiliar are based on a reduction to something else with which an audience has already a measure of familiarity. In fact, it is said to be a mathematician's dream to erect all of mathematics on a set of axioms that can be regarded as self-evident truth. This characterization indicates an interrelational quality inherent to the art of explaining and, by the same token, it reflects somewhat a cynic's view, in the sense that understanding is in no small degree a matter of becoming acustomed to things. The battle between formalism and intuitionism is living proof, though, that there is no such thing as a "last truth," even in mathematics. If it is already dangerous to navigate blindly on "isms" in mathematics, one may certainlyexpect the same to be the case in physics.

Whatever "explaining" does to us, its effectiveness depends on the knowledge and images with which the recipients of those explanations are already familiar. That is why an explanation, to some, can be a convolution of reason to others. In a variation on the Shakespearean admonition that beauty is in the eye of the beholder, it can be said that whatever constitutes an explanation is in the mind of the beholder.

Mindful that standard explanations are of the interrelational type of reduction to familiars, more rare are explanations requiring new additions to the arsenal of fundamental concepts that constitute an existing body of knowledge. An event, which most of us would consider as belonging in this category, is the discovery of the calculus. It introduced such brand-new notions as "infinitesimals" and the infinite summations thereof. Here the reduction to, at that time, familiar concepts suddenly failed. The mind, then, must be conditioned to accept those new fundamentals. Yet, before welcoming new items as worthy new members of a Hall of Fame of Fundamentals, criteria for admission need to be established. The arsenal of fundamentals should retain its inner logical consistency, which means compatibility with existing fundamentals should be checked.

Obviously, reluctance is called for in introducing new fundamental concepts, lest we run the risk of building houses of cards that are liable to collapse in the wake of only minor disturbances. Mathematics traditionally has been more conscientious then physics in the handling of fundamentals. Yet, in defense of physics, let it be said that the subject matter of mathematics better lends itself to rigorous practices. A central point of this investigation has been one of questioning physics' introduction of too many counter-intuitive notions, which have been ranging from wave-function collapse, a priori single-system uncertainty, nonclassical statistics, all the way to particle-wave duality. With the sincerest of respect for services rendered, some of these enigmas are due for a reevaluation.

Since much explaining depends on reduction procedures through which new truths are related to older truths, such new truth is expected to be subject to the same restrictions as the old truth to which it has been reduced. Mathematicians are more skilled than many practitioners of other disciplines in specifying the conditions for which their results hold. They also are in a better position to do so. Yet, by the same token, as sticklers for precision, mathematicians frequently are the targets of scorn for "proving" things that seem perfectly obvious to others. Let the Jordan curve theorem be regarded as a case in point: it claims that a closed curve in two dimensions separates an inside and outside domain. A first reaction of the uninitiated is: but of course, this is obvious; what is there to prove?

Such premature convictions on the part of beginners rapidly evaporate as soon as they are confronted with the possibility of closed curves that don't separate inner and outer domains. Examples are the azimuthally and meridionally closed curves on a torus surface; it is possible to get to either side without crossing those curves. They are cycles that don't have the separation properties of boundaries. After we have said "but of course," we realize the Jordan theorem's importance for the topology of the two-dimensional configuration in which the closed curves have been embedded.

So, what first appeared as a pedantic exercise in futility suddenly manifests itself as a potential tool for topological exploration. This distinction between *cycles* and *boundaries* extends from one-dimensional curves in two dimensions to (n-1) cycles in n dimensions. This scheme (used in the previous chapters) is at the very heart of contemporary homology theory.

In other disciplines, it frequently happens that restrictions, say, comparable to the just-mentioned topology contingencies, are not precisely known or cannot be precisely known. It is not always possible to decide whether certain restrictions are due to our lack of perceptiveness, or caused by natural limitations. After all, lack of perceptiveness is by itself a very natural limitation. What is meant here is that even if the final statements of a physical theory appear in an austere and purely mathematical form (the mathematical limitations of which are precisely known) it can still be true that, notwithstanding this mathematical precision, the physical limitations can still remain totally obscure.

In the light of discussions in preceding chapters, the Schroedinger equation of quantum mechanics is recognized as a case in point; its realm of applicability is quite unclear and not agreed upon by all afficionados of that discipline. Yet, the very precise mathematical language in which quantum mechanics has been cast has made it tempting for some to elevate also the physical implications of its results to a level of axiomatic impeccability, that are normally reserved for results in pure mathematics. The risk in making such extrapolations may be acceptable, if caution prevails in the extrapolation process. Experience is here the best teacher in calling the alert when to distrust and reexamine given results.

While topological probing of physical configurations may start with some well-defined topological concepts, their standard realm of mathematical realization is restricted to abstract geometry. Boundaries as domain separators in geometry are abstractions that can be introduced at will. The epistemological distinctions between mathematics and physics becomes here quite unambiguous. Whereas, in mathematics, topological structure can be created at will, in physics, it is necessary to make physically relevant identifications for domain separation. These identifications go to the heart of that what creates the physical manifestations with which we are concerned.

So the art of explaining is very much contingent on taking cognizance of one's audience realm of familiarity. Applied to a Schroedinger-based quantum mechanics, in which mathematical and physical epistemology are intertwined beyond recognition, a delineation has become next to impossible. Let us, therefore, briefly recall physical identifications made in the preceding chapters for the sole purpose of getting a better feel for topologically assessing physical structures.

2. Elementary Charge and Weak Interaction

An important topology contact in physics comes about through the cohomology connection. The differential 1,2 and 3 forms of physics are assessed according to whether they are *closed* or *exact*. The homology connection comes about as a result of selecting integration domains for these differential forms. A striking example of an homology choice is the trefoil-shaped electron to secure a 4π integral loop for the Aharonov-Bohm flux integral.

Choice of differential forms, their properties, and integration domains identify steps leading from mathematics to physics. In the process of doing so, we home in more and more on what may be called the "substance" or "substances" that generate the physics we perceive. The unavoidable yet age-old question being asked here: what is the nature of matter?

At the beginning of this century, matter had already become reduced to atoms that could be visualized, if you will, as miniature planetary systems of electrically charged particles interacting through Coulomb's law instead of Newton's gravity. The next phase of experimental and theoretical research then homed in on the nature of the particles themselves. While the nature of the atom had been successfully reduced to smaller entities, the process of reduction for these smaller entities, known as elementary particles, presented some fundamental hurdles. Is it reasonable to keep subdividing ad libitum?

While the quarks and gluons of quantum chromodynamics (QCD) proved useful as reduction entities in interrelating properties of larger compound objects, these entities are found to violate the spirit of explanation by not reducing to something observable or already familiar. Attempts at experimentally isolating and identifying quarks and gluons have remained unsuccessful. Yet many experiments have been very suggestive of the relevance of the quark concept as a descriptional tool. It then becomes, in the sense of Riordan[1] a matter of definition of reality whether or not this relevance should be accepted as proof of quark existence.

The smaller entities that make up atoms, such as electrons, protons, α-particles etc., had, at the time, already manifested themselves in experiments permitting an isolation and individual evaluation. Here, the beloved process of explaining by reduction to something already familiar worked fine, yet it comes to a grinding halt in assessing subatomic entities. The item of reduction simply refuses to make itself known for the purpose of becoming better acquainted. At this juncture, the process of reduction fails. The ensuing alternative is either to elevate the quark as a legitimate object of familiarity to justify reduction, as is presently done, or to consider the possibility that the situation still permits a different less radical path of reduction. Let us consider the latter option.

A quantum cohomological assessment of subatomic matter may be regarded as a process which attempts more of a reduction to familiar concepts. Yet the procedure is contingent on a postulated extended applicability of certain differential forms of physics in the subatomic domain. Since a judicious extension to micro-applicability of existing laws (based on proven metric-independence) is less radical than introducing the brand new concepts such as quarks and gluons, let us argue in support of a balance between radically new and not so radically new.

Quantum uncertainty of single particles has been a stumbling block standing in the way of such cohomological procedures. Earlier discussions of the properties of the electron in chapters VIII, IX, and XI have revealed that a subatomic physical reality of the 3-form of charge is bound to have a major role in rendering services for the development of ideas about the substance and nature of subatomic physical obstruction.

The association of charge with an exact 3-form in spacetime is related to its property of playing the role of a boundary separating inner and outer domains. Faraday cage and Meissner effects are macro examples of charge's role as a field separator in three-dimensional physical space. In spacetime, charge would have to be a closed hypersurface and since a closed three-dimensional cycle can only be embedded in four dimensions, the inherent dynamic nature of elementary charge must be regarded as an inescapable conclusion.

It then follows from this essential dynamics of charge that all elementary charge must have simultaneous magnetic, as well as electric field manifestations. Yet, charged pions testify to the fact that not all elementary charge has an observable magnetic-field manifestation. When speaking in the electromagnetic vein, this fact can be reconciled for the pion if the magnetic flux is taken to be internally closed, without displaying an externally observable dipole moment. A *qualitative* measure for the ratio of meridional and azimuthal radii of a ring charge in azimuthal motion is, according to Eq.22 of chapter XI, of the order of the fine-structure constant α.

A transition from azimuthal to meridional current, while retaining c as dictated by lightcone intersection, would increase the meridional current by a factor of the order α^{-1}. For a conserved flux unit h/e, and the energy being almost purely magnetic, the associated pion mass would have to be of order α^{-1}. The pion-electron mass ratio is, in fact, of the order $2\alpha^{-1}$.

These are, of course, highly speculative considerations. They are presented for the sake of argument, to show that a reduction of particle structure to classical and semiclassical concepts should not be prematurely discarded. They are, if you will, a follow-up on the considerations concerning electron structure given in chapter XI.

Seen from this angle of internal and external flux, weak interaction decay resembles an internally trapped pion flux seeking an exterior magnetic moment manifestation. Yet, where does the excess energy go, and how do we account for spin and lepton number conservation? All of this brings us to Pauli's neutrino to account for an energy, charge, flux and angular-momentum balance.

3. Neutrinos as Manifestations of Electroflux

The transformation of an enclosed magnetic flux into a flux with an externally observable magnetic moment must be expected to invoke the induction of an electro-flux $\int dt \int \mathbf{E} \cdot \mathbf{dl}$ in the sense of Faraday's law. It is now tempting to associate the neutrino with a manifestation of electro-flux. Electro-flux has the dimension (h/e) and, as shown by the Josephson effect, is seemingly quantized in units of that dimension, (chapter VIII).

Neutrinos and anti-neutrinos would thus correspond to exchanges of magnetic flux and electro-flux. Conservation of lepton number can then be tentatively thought of as relating to this exchange between internal and external fluxes, with an electro-flux of appropriate sign in the balance for flux conservation.

Since, according to Eq.15 of chapter VI, flux and angular momentum behavior are interrelated by virtue of the integrand's dependence on the 1-form A, we need to look at spin angular momentum behavior in conjunction with flux behavior. Spin and angular momentum have the same dimension as action, yet action is a scalar, whereas spin and angular momentum are not. Spin and angular momentum assume a scalar character of action after integration over relevant angle variables.

The following picture now transpires. Action can, in part, be regarded as integrated spin or angular momentum. Not all action is necessarily of that nature. There is a conservation of angular momentum and spin. There is not, in general, a conservation of total action.

Since action is a 4-form L over the top of the spacetime dimension, its local exterior derivative trivially vanishes and integration over "all" conceivable 4-cycles has meaning only if the universe of spacetime can be considered to be closed. In this sense, conservation of action could, at best, have a global meaning as a statement for the universe as a whole. This global statement is without a local counterpart, because the exterior derivative of the 4-form L vanishes identically, without making locally a physically meaningful conservation statement.

If, however, the 4-form of action (Eq.18, chapter VI) is a pure divergence, an application of Stokes' theorem reveals that the action in a given domain of spacetime is then given by a cyclic integral that contains this domain of spacetime. In that case, all action changes in that domain are determined by what goes through its three-dimensional boundary.

If the Lagrangian density is not a pure divergence (*i.e.*, not the exterior derivative of a 3-form), action creation and annihilation can take place in the given domain of spacetime itself. The angular momentum-spin component of action can be conserved, wherever it derives from a 3-form according to Eq.18 of chapter VI.

Similarly, as different kinds of action can be distinguished, so can different kinds of flux. Unlike action, flux conservation, involving magnetic and electro-flux, has a local and global meaning. While magnetic and electro-flux are not individually conserved, they can have an invariant association with specific particles (*e.g.*, the electron and external magnetic flux, the neutrino and electro-flux).

Particle-associated magnetic flux can be subdivided again in enclosed internal flux without magnetic moment manifestation, such as may be the case for the pion, or an external flux, as in the case of electron and muon. While not all flux has a magnetic moment, a magnetic moment always has an associated flux. In the weak interaction process of pion decay, internal magnetic flux presumably trades places with external electron or muon flux. To identify the particle nature associated with the resulting electro-flux in the balance, the spin structure of the objects under consideration have to be taken into account.

For configurations without spin, changes in magnetic flux states result, according to Faraday's induction law, in an equivalent opposite change of electro-flux $\int_T dt \int_{c_1} \mathbf{E}\cdot d\mathbf{l}$, which, in principle, relates to a photon of electromagnetic interaction.

The weak interactions though don't emit photons; they instead emit or absorb neutrinos. If it is valid to consider these matters in an electromagnetic context, the question is: what determines the difference between photon and neutrino exchange?

In the here-assumed perspective, the Faraday law should also remain intact for spin situations. Neutrino emission should then relate to the self-knotting of the associated emitting structure. It then stands to reason that the topology of integration path c_1 of the flux integral should be able to testify about these matters: a 2π path c_1 relates to a photon, a 4π path c_1, by contrast, relates to a neutrino.* In other words: different and mutually exclusive mechanisms of Faraday induction are taken to be responsible for photon versus neutrino exchange. **Table III** summarizes the situation. The, perhaps, naive rationale underlying this premise is based on the consideration that the single loop has a center of symmetry, whereas the knotted loop has not!

It may also be useful to construct a similar overview of the action components generated by the Lagrangian 4-form $\mathfrak{L}$. The classification of action situations dictated by integrals of the 4-form $\mathfrak{L}$ differs from the classification of the 2-form F in one very important respect. The F is a totally metric-independent structure, the $\mathfrak{L}$ by contrast depends on the metric. Hence, along with the 3-form $\mathfrak{C}$ as creator of topology, one must

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* The idea of knotting in relation to elementary particle structure has been pioneered by the late Herbert Jehle (Phys.Rev.**D11**,2147(1975)). During a conversation I had with Jehle in the early seventies, we discussed the possible flux states of electron and muon. When I suggested the muon might be a higher flux state of the electron, his reaction indicated disagreement. While I do not remember him qualifying his point of view at the time, later I realized a higher flux state would be incompatible with the anomalous moment calculations for the muon given in chapters VIII and IX.

Table III: Breakdown of Flux components for the 2-Form F

magnetic: $\iint_{D_2} \mathbf{B}\cdot d\mathbf{S}$	example	symbol
enclosed flux, magnetic moment zero	$\pm\pi$ (pions)	Φ_π
linked flux, magnetic moment nonzero	$\pm e$ (electron)	Φ_e
	$\pm\mu$ (muon)	Φ_μ
electro-flux: $\int_T dt \oint_{c_1} \mathbf{E}\cdot d\mathbf{l}$		
Faraday flux, c_1 is unknotted	γ (photon)	Φ_γ
spin-based flux, c_1 is knotted	ν (neutrinos)	Φ_ν

The mechanisms of flux conservation are contingent on the topological structure of the cycle $c_2 = \{(D_2)_{t=0}; (D_2)_{t=T}; c_1, T\}$.

also expect the lightcone to play a role as a topology determining factor. **Table IV** gives a possible classification for $\mathbf{L}$. It is not quite clear, though, how the space versus time distinction of metric structure relates to the more subtle torsion distinction and the nonmetric principal blades decomposition of the Maxwell field tensor, to be discussed later in this section.

Table IV: Breakdown of Action Components for the 4-form $\mathbf{L}$

Let $\mathbf{L}=d(A\wedge G)$ be a pure divergence; which implies the variational condition is trivially met. The action integral $W = \oint\oint\oint_{c_3} A\wedge G$ expresses spin-angular momentum conservation if W=constant, there is no action exchange with the environment. Cycle c_3 residing where $\mathbf{L}$=0.

For the period integral W one has the cases:

I: Let $c_3 = c_1 \times c_2$; *i.e.*, c_3 is not simply connected.

Ia: c_1 is not knotted, residues of $\mathbf{L}$ are orbit-based.

Ib: c_1 is knotted, residues of $\mathbf{L}$ are spin-based.

II: c_3 is simply connected.

IIa: $\oint\oint\oint_{c_3} A\wedge dA = 0$; dA is of rank 2; W=constant→ case I.

IIb: $\oint\oint\oint_{c_3} A\wedge dA \neq 0$; dA is of rank 4; event possibility.*

*The 4-form $\mathbf{L}$ now has an interaction component $A\wedge C$ preventing it from being a pure divergence. Variational process is no longer trivial. Action exchange with the environment establishes extremum

An inspection of the combined Tables II, III and IV gives a preliminary idea how the 3-form of charge and the lightcone together could be instrumental in a topology-based classification of elementary particles. It is by no means an automatic "turn the crank" procedure. The potential interaction with physical reality is, however, sufficiently compelling to ask new questions which could complement some of the more formal existing descriptions.

We now get a little closer to questions as to why some configurations emit photons, and others emit neutrinos. To go a step further, it is important to distinguish between different types of spacetime periodicity.

In nature, the most frequently encountered notions of periodicity are the spatial periodicity of waves and the purely spatial periodicity of crystal lattices for example. These observations on spatial periodicity invite an inquiry into the subject of pure-time periodicities. There are many macro- and microphysical examples of those in nature, yet our reluctance to get into subatomic modelling has left the elementary particle option largely unused as a candidate for pure-time periodicity.

For the trefoil electron model with circulating charge, the period $\tau = h/mc^2$ is easily identifiable as charge circulation, because the predominantly magnetic nature of the electron permits us to write for its energy $h/\tau = \phi J$; with $\phi = h/e$, it follows $J = e/\tau$. The latter identifies a physical meaning of τ.

As reflected in the notion of "matter waves," *a dominant paradigm of physical thinking has been one that all periodicity implies waves.* Since we would not say that an atomic or molecular lattice implies waves, neither should we claim that every atomic or subatomic time periodicity implies waves.

It is quite another thing if, for mathematical purposes, say, a lattice is represented as a *standing* "Fourier wave." For the standing wave, spatial periodicity λ and time periodicity τ have been separated as independent epistemological entities. There is not a necessary implication that they are physically tied together by a velocity of propagation $u=\lambda/\tau$, which is characteristic of some sort of a dynamic medium.

Crucial for recognizing waves is the occurrence of an interrelated spacetime periodicity as characteristic. Spatial periodicity and time periodicity, each taken as isolated physical facts, can have independent physical meanings without implying a wave existence. For their mathematical description, this raises the crucial question as to whether these separate epistemological manifestations of spacetime wave versus space periodicity and time periodicity can be recognized as meaningful invariant characterizations.

For the spacetime wave, the spacetime phase invoking the four-vector $(\omega,\mathbf{k})$ is the most conspicuous invariant attribute. The Bohr-Sommerfeld integral carries a related semi-invariant connotation for periodic orbits invoking the energy-momentum vector $(E,\mathbf{p})$, except that we conveniently leave out the E part. The time periodicity is here viewed as due to an orbital periodicity not a wave periodicity. In chapter VI (Eq.7), the Bohr-Sommerfeld integral was shown to be derivable from the Aharonov-Bohm integral, illustrating an interchange between orbital and time periodicity. In this transition from Aharonov-Bohm to Bohr-Sommerfeld, the spatial part **A** of the vector potential is conveniently inactive, which is not quite true anymore in relativity.

An understanding of these convenient expediencies, which are so characteristic and sometimes so very necessary for the developments of physics, is contingent on the invariant characterization of the space and time features of spacetime. The invariance of the spacetime signature under real spacetime substitutions is, so far, the best we have, provided this distinction has not already been obliterated by a geometric overzealousness, which is reflected in the notorious substitution $u=ct\sqrt{-1}$. Let us see how the space versus time distinction manifests itself invariantly in the metric-independent context of a Maxwell field.

Consider the Maxwell field, which for the sake of familiarity is here expressed in its "cartesian" components, although we keep in mind that its rank is a general metric-independent invariant:

$$F_{\lambda\nu}=\begin{Bmatrix} 0 & E_x & E_y & E_z \\ -E_x & 0 & B_z & -B_y \\ -E_y & -B_z & 0 & B_x \\ -E_z & B_y & -B_x & 0 \end{Bmatrix}.$$

The principal blades theorem for skew symmetric matrices claims that a real spacetime transformation always can be found transforming this matrix into a skew symmetric diagonalized form:

$$F_{\lambda\nu}=\begin{Bmatrix} 0 & E_x & 0 & 0 \\ -E_x & 0 & 0 & 0 \\ 0 & 0 & 0 & B_x \\ 0 & 0 & -B_x & 0 \end{Bmatrix}$$

For the Pfaffian stability condition $\mathbf{E}\cdot\mathbf{B}=0$, the matrix has rank 2, hence only one of the diagonal 2 by 2 submatrices can be of rank 2, which leaves either an exclusive magnetic or an exclusive electric manifestation.

While this diagonal reduction had no relation to the metric, let us now diagonalize these 2 by 2 submatrices with respect to the metric.The electric part gives the *real* characteristic values:

$\lambda_{1,2} = \pm E_x/c$ (the electric option).

The magnetic part yields purely *imaginary* characteristic values:

$\lambda_{1,2} = \pm i\, B_x$, with $i=\sqrt{-1}$ (the magnetic option).

In other words, when F is examined in the perspective of a spacetime metric structure, it reveals an invariant distinction between magnetic and electric features. Under real spacetime transformations, the electric part can be diagonalized; the magnetic part cannot! This distinction relates to the essentially 2-dimensional "blade" structure of magnetism versus the "arrowed" physical nature of the electric field. Note also how the two distinct real roots for the electric field component reveal two physically distinct manifestations.

For elementary particles, the real characteristic values of the electric option can be taken to reflect a pairing without an explicit reference to electric charge. Neutrino pairing is a possible candidate that could be taken as a manifestation of this primary distinction.

Magnetic flux, by contrast, does not manifest pairing, when taken all by itself; here, pairing becomes manifest only in conjunction with the charge and current generating the flux.

In general, since $E \cdot B = 0$ places the object under consideration on a worldline living out its natural stable life, the characteristic roots of the Maxwellian field are given by $\lambda = \pm\sqrt{E^2/c^2 - B^2} = \sqrt{\mathcal{L}}$. The sign and values of $\mathcal{L}$, as stipulated in Table II give: $\mathcal{L}>0$ two neutrinos; $\mathcal{L}<0$ magnetic flux, pion internal, electron, muon external; $\mathcal{L}=0$ photon.

The characteristic root possibilities for the rank-2 Maxwellian field are instrumental for complementing the cohomological particle classification of chapter XIII. It reveals a potential feature of distinct elementary structures: *i.e.*, 2 real roots for the neutrinos, a zero root for the photon, and a complex root for a dominant magnetic flux manifestation (*e.g.*, external for electron and muon, with an internal option for the pion).

4. Conclusion

There is neither claim nor warranty that the given arguments permit a complete and unambiguous reduction of elementary particle structure to known or partly-extended forms of known laws of nature. The preliminary picture here obtained presents; at best, a fair inducement for work to make the reduction to familiar devices more complete, or, in the worst case, to discard it as unsatisfactory. Either way, this process of reduction to familiar old concepts is bound to give new information and perspectives about the realm of validity of those older devices.

Even if the procedure is not more than merely a mnemonic device, it serves as a useful counterweight against a too-liberal creation of imagined building blocks, which later have a suspicious preference for remaining experimentally incognito.

Approaching the end of this odyssey in the land of explanation simply confirms that science continually vacillates between methods of reduction and innovation. When confronted with the world of observation, most of us first attempt to reduce seemingly new facts of observation to something already known. If, and only if, this process of reduction fails to give satisfaction, should we be prepared to go the path of innovation outside the realm of known things. It means injecting fundamentally new principles concerning ideas that have not been around before. More cautiously, we should perhaps say: ideas that may have been around before, but not in the framework of a newly given context of reality.

With apologies for citing an already much used example in this reprogramming endeavor, the reader is invited to consider once more the case of metric-independence. The awareness of metric-independent laws of physics, which has been around since the early Twenties, acquires an entirely new physical perspective when it is related to the premise of simultaneous macro- and micro-applicability. Metric-free period integrals don't *a priori* discriminate between macro- and micro-sized residues. Only shrinkability of integration domains, by permissible deformation, gives qualitative information of size. Finally, metric-independence is a *sine qua non* for the counting of identical objects.

Seen in this perspective of metric-free, the quality of science becomes enhanced by optimizing the balance between logical reduction to familiar concepts, on the one hand, and innovation, on the other. Science that never steps outside a preordained realm of logical reduction is in danger of losing momentum, notwithstanding the inherent beauty of some of its logical endeavors. On the other hand, science thriving solely on innovation, without adequate cross-checks of logical interconnections, runs the risk of going out of control.

Innovation that summarily requires a suspension of criteria of observation and logic is treading dangerous territory. The Copenhagen view of quantum mechanics is dangerously close to fitting this mold. It limits observability through innovation of *a priori* uncertainty and replaces the logic of causal inference by new rules, tentatively referred to as quantum logic.

It goes without saying in detailed specifics that whatever applies to a Copenhagen view of quantum mechanics is bound to affect disciplines that have been erected using those same premises (*e.g.*, QED, QCD and perhaps Strings). Even if it threatens to become boring, it is necessary to reiterate those contingencies, because they reflect decades of established patterns of thought. While these descriptions have been erected to have definite measures of physical relevance, the physical identifications of some of their innovative creations, instrumental for their implementation, have remained elusive. There is a good chance they may remain so forever (*e.g.,* the energy infinities of QED, the quarks, magnetic charge of QCD, the excess dimensions of Strings).

Confronted with these measures of reality, questions arise about the virtues of those innovations. Then, going back in time, we find that most of this, if not all, was precipitated by the early dispute between Copenhagen and the lonely proponents of an ensemble point of view in quantum mechanics.

If the ensemble point of view is so much more reasonable than the Copenhagen interpretation, how was it possible that a vast majority of the physics community has supported Copenhagen and still continues building new theories on its premises? It would be understandable if the ensemble point of view had received support only from obscure sources that deserve to be viewed with great suspicion. The truth is, however, that an examination of Jammer's[2] book on the philosophy of quantum mechanics reveals names of very reputable proponents of the ensemble view. In the realm of textbooks, there have been respectable publications. All of this, ironically, suggests a near irrelevance of interpretation when it come to using quantum mechanics.

There are multiple articles and essays extolling the virtues of an ensemble view. Combing the contents of Jammer's treatise, one also finds names of people whose very reputation has been questioned, solely because they supported an unpopular point of view. Is it possible to come up with a consensus as to what, in particular, has dissuaded the majority from giving the ensemble a better chance?

Without consulting all the sources that perhaps should have been consulted, I am inclined to come up with a two-pronged conclusion. Unlike the Copenhagen trio (Bohr, Born, and Heisenberg), ensemble proponents never made a convincing plea as to how their ensemble could be physically specified, and what, in particular, in the ensemble had to be regarded as subject to statistics.

There are many clever investigations homing in on a conclusion that the statistics implied by the Schroedinger wave-function should be "classical" in nature. They unwittingly support Planck,[3] who, in 1913, had given an early indication how phase randomness of ensemble constituents could be regarded as a conceivable source of zero-point energy and associated uncertainty. The 1925-1927 revolutionaries, with the exception of few, were so enraptured by the possibilities of their own new tools, that they were little inclined to give the father of the quantum of action the consideration he so amply deserved. From that point on, many potential supporters of the ensemble point of view were swept off their feet by the mystique of the subsequent Copenhagen developments.

Life is perhaps unbearable without mystique. Science would be dull and stale without it. Mystique is essential for the growth of science, provided some of it resolves itself later, possibly to be replaced by new and deeper mysticisms. Mystique is a phase in the process of understanding, because it is essential in fetching human attention. Yet, mystique is an inducement not a goal for better understanding.

However, in the past half-century, none of the old quantum mysticisms have resolved themselves. The changes have been minimal. Physics still preaches the same wave-particle duality, ignoring the possibility that not all periodicity has to be of wave origin. Physics still wants to have its cake and eat it, too, by having wave-function collapse in the process of salvaging causality. Last, but not least, physics still accepts a point-presence for particles which have mass, charge, spin and magnetic moment. How has the formalism responded to this amazing collection of contradictions?

The formalism has responded by being forgiving. It substitutes for a point-particle a finite spatial presence by subjecting the point to uncertainty, thus creating an equivalent domain space that would have been occupied by a finite particle. Somehow, the quantum formalism knows more than we; it kindly covers up for our interpretational transgressions.

Yet, even if we are privileged in having a user-friendly formalism at our disposal, the standard objective of science is to find out why nature does what it does. Instead, modern science attempts to learn why a formalism does what it does. The situation depicts an unusual shift in who is holding the initiative.

In science, one expects man to be in charge leading the formalism he has created. In quantum mechanics, the formalism has been leading man.

Nobody knew better than Schroedinger himself that we had to know more about his "gift from heaven" if we were to become responsible users, lest we find ourselves in the not-so-enviable position of the sorcerer's apprentice who did not know how to undo the avalanche unleashed by his magic words. Schroedinger eloquently verbalized his opinions about this. His observations, though, were of little avail!

Is there anything that presently can be done in easing the predicament of the many contemporary apprentice magicians? In the spirit of considerations, cited in this monograph, we might ease up a little in trying out new magic words. We might listen more to some of the counsel of wise men like Einstein, Planck, Popper, and several others, who, many years ago, gave the ensemble the consideration it logically deserves. At this time, the ensemble evidence they gathered can be augmented to build a case that logically supersedes, by far, the quality of argument that can be used in support of Copenhagen views.

If an opinion poll were taken as to what the Copenhagen interpretation means to whom, one would get many different answers. The reason is that the Copenhagen interpretation really emerged as a work compromise. Different people accept compromises for reasons that are different, according to their personal relation to the compromise.

Only recently has it been possible to be more precise about the ensemble option. It shows more clearly the limitations of the Schroedinger formalism in that it should be restricted to ensembles of random phase and orientation. Ironically, prior to the 1925 revolution, Planck pioneered exactly such ensembles in his investigations of the zero-point energy.

What is the moral of physics' reluctance to acknowledge those earlier results of one of the immortals of contemporary research? Perhaps, taking cognizance of Planck's own reluctance to introduce new ideas might help. Physics truly needs fewer fundamentally new ideas that cannot be reduced to what we already have. There are reasons to consider quantum uncertainty as a zero-point energy associated with a state of primordial ensemble disorder, not as an *a priori* attribute of any single system. Are uncertainty, zero-point energy and *Zitterbewegung* all necessary? or are they more of the same? We need to call more sparingly on fundamental innovations.

Planck's [very reluctant] introduction of a quantum of action emerges as one of the truly irreducible fundamental propositions of quantum theory. Perhaps the later discovery of $E = mc^2$, in conjunction with the atomicity of matter, could have given an inkling of energy atomicity.

Copenhagen's earlier glory is action's identification as a residue of a period integral by Bohr, later extended by Epstein, Sommerfeld and Wilson. Kiehn's recasting of this line integral in H-J space into a 3-dimensional field integral opens up new perspectives for finite particles with topological structure. This 3-form integral is a field analogue interrelating quanta of electricity, flux, and action. The fairly recent discovery of the quantum of flux by Deaver-Fairbank and Doll-Naebauer, and its representation in terms of a residue integral by Aharonov, Bohm and London further supplements a picture of identifying fundamental physical quanta as building blocks of physical theory.

In the light of such experimental testimony, it would be sinful neglect not to try out de Rham's period integral methods as a major tool in the exploration of topological physical structure in space and time. These tools are already used in *ad hoc* fashion; it is now time to recognize the need for their more organized integrated use, unhampered by nonclassical myth.

RAMIFICATIONS OF THE TWO-TIER VIEW OF Q.M.

CHAPTER XV

COMPATIBILITY OF QUANTUM MECHANICS AND RELATIVITY

1. Summary

The objective of this chapter is to gather evidence that the long-abided reconciliation of quantum laws and relativity is met by the period integral recipes of quantization. This compatibility, then, automatically extends to any quantum cohomology based on those period integral laws. Some of the obstacles that stood in the way of such a reconciliation are here identified. It is primarily the unsubstantiated expectation that Schroedinger's equation and its Dirac relativistic version had to be viewed as intermediate steps on the road to an encompassing quantum theory meeting the requirements of the general theory of relativity. By restoring the ensemble as their object of description, the wave equations of quantum mechanics become released from the unreasonable imposition of having to satisfy the principle of general covariance. The treatment of single systems is, by contrast, based on the restoration of generally invariant integral laws of physics. They can be adapted to serve as period integrals in the sense of a de Rham-type cohomology for assessing topology. Since Diffeo-4 invariance is a prerequisite for assuming the role of spacetime topological probe, compatibility of these integrals with the principle of general covariance is now no longer a problem. The presented investigation compares mathematical-physical techniques. Since notations in this overworked area of physics easily call up unwanted associations, language, rather than formalism, is given priority to delineate fundamentals.

2. Introduction

An inspection of physics literature over the last six decades reveals a recurrence of so-called "difficulties" associated with the conceptual compatibility of two principal disciplines: quantum mechanics and the general theory of relativity. The references tend to be somewhat tongue in cheek, because rarely does one find major foundational investigations devoted to the subject of their compatibility. In the familiar Dirac equations, there is already a measure of successful blending of quantum mechanics and the special theory of relativity. The *general* theory of relativity, however, has placed obstacles on the path of a more complete conceptual unity.

The objective is to explore the mathematical-physical nature of these difficulties. The conclusion obtained from those explorations is a radical one. No compatibility can be expected, as long as we keep adhering to the Copenhagen view of quantum mechanics. It is specifically the Copenhagen dictum of a ψ function which is said to be the probability amplitude indicative for single-particle position and momentum. This single particle is then given a physical presence equivalent to a mathematical point singularity. An ever-present Heisenberg uncertainty subsequently reinstates for this point-object a de facto finite physical presence.

A substitution of this Copenhagen view with that of the statistical ensemble interpretation, such as advocated by Einstein,[1] Popper[2] and many others,[3] takes away the need for having to seek a general relativistic version of what is commonly referred to as a quantum mechanical-wave equation. Seen in this context, the compatibility demand loses its relevance. From the ensemble point of view, the standard tools of contemporary quantum mechanics are no longer under the unreasonable obligation of having to provide a final and unequivocal answer to all microphysical fundamental situations.

In the light of the ensemble interpretation, quantum mechanics has to be denied the Copenhagen-permitted formal applicability to truly isolated single systems that are not part of an ensemble. Even if such applications have been known to yield meaningful answers, their relevance is to be taken in an asymptotic sense. Ensemble properties undoubtedly are governed by the individual properties of their constituents as isolated systems. On this basis one would expect an asymptotics of the properties of the ensemble and its constituents. This asymptotics, however, does not justify the Copenhagen extrapolation, which claims that quantum mechanics, as is, covers the single system.

Once it is agreed that the contemporary tools of quantum mechanics are instruments of ensemble description, a compatibility of these tools, with the dictates of the general theory, needs to be identified as a pursuit without a clearly defined objective. Instead, we ought to be looking for the truly more fundamental tools that are applicable to isolated single-quantum systems. The latter are now no longer excluded from exploration by a misinterpretation of quantum uncertainty, such as ensues from the standard tools when viewed in the Copenhagen perspective. In recent times, the excessive difficulties encountered in applying wave equations to macroscopic quantum phenomena, such as the quantum Hall effect, testify to the diminishing returns when ensemble tools are extrapolated to single macro systems.

In the following, we first assess why contemporary quantum concepts don't gibe well with the dictates of the general theory of relativity. We proceed to make a preliminary exploration of period integrals as tools that do indeed comply with the invariance prerequisites imposed in the context of the general theory. Then, after viewing the evidence linking the ensemble thesis with the use of the standard tools of quantum mechanics, the suitability of period integrals are examined as a substitute for dealing with single isolated systems, be it microscopic or macroscopic. Finally, after delineating the need for this two-pronged mathematical-physical picture of ensemble versus single system, an epistemological review of other evidence clinches the case against Copenhagen to desist from an unjustifiable identification of ensemble and single system. The Copenhagen interpretation thus emerges as, at best, a temporary work hypothesis retaining viability only as long as the ensemble single-system identification is asymptotically permissible.

3. General Theory of Relativity and Dirac Equation

A major hallmark of the Dirac equation is the near-automatic emergence of the concept of spin. A mathematical equivalent to the Dirac concept of spin had earlier been introduced by Cartan[4] in his investigations of the group spaces of orientation preserving transformations. The seemingly arbitrary mathematical act of excluding orientation changing characteristics from a group was found to leave still a local reminder of a possible extension to orientation changing operations. Later, work by Haefliger,[5] Milnor[6] and others have revealed how spin properties bear local testimony to global orientability structure of manifolds, and the geometry and topology of the physical objects therein.

Seen from this more encompassing picture, "spinorization" not only is relevant to the orientation preserving rotation and Lorentz groups, it is also relevant to the orientation preserving real general linear group. The emergence of a group feature leading to the two-unit operations of orbit and spin 2π and 4π, is a direct consequence of the physically imposed restriction to **real** transformations.

It is an irony of fate that a condition of mathematical "reality" for the proper rotations leads to a two-dimensional spin representation replete with complex variables. Only in the case of the orthogonal group is it possible to find such a simple two-dimensional complex representation. Finding a representation for the spinorization of the general linear group (if possible at all) is more complicated. An assessment of a fundamental feature as spin, though, should not be contingent on such mathematical coincidence.

It follows, from this brief glance into the realm of group representations, that the spin formalism as quantum mechanics knows it, is rather coincidentally linked to the orthogonal orientation preserving groups in three and four dimensions. Yet, a hallmark of the general theory of relativity is the act of invoking an extension of the orthogonal invariance groups of physics to general real spacetime substitutions. This principle is known as "Einstein's principle of general covariance." Presently in the mathematical literature, this set of transformations is also referred to as a diffeomorphism (*i.e.,* spacetime Diffeo-4).

Einstein's imposition of general covariance created an extended hierarchy of invariance principles in physical theory. It confronts us with an extended classification calling for an identification of physical quantities belonging in the general diffeomorphic category. Einstein's principle of general covariance thus demands scrutiny of the distinct physical roles of Diffeo-4, all the way down to the discrete subgroups of crystallography.

Soon after the introduction of the principle of general covariance, Kretschmann, and later, more forcefully, Bridgman,[8] argued that the principle of general covariance could have no physical implications. Today, many texts on relativity pay rather unthinking lipservice to this point of view, without really attempting an independent assessment. It is said that Einstein conceded to Kretschmann. Yet a careful reading of the papers that triggered the controversy reveals how Einstein upheld the view that his principle was essential for the structure of spacetime physical theory. I do believe we owe the creator of the general theory, and the principle he claimed as instrumental for that theory, a token of respect by fully reporting his position!

Now, faced with Dirac's magic of creating spin by blending quantum mechanics and the special theory of relativity, the late Twenties and the early Thirties had to lead, almost unavoidably, to a frantic search for also reconciling Dirac theory with the general theory of relativity. After Kretschmann had just succeeded in physically emasculating Einstein's principle of general covariance, one might well ask why?

Apparently not everybody shared the finality of the Kretschman-Bridgman verdict. Among those who gave the project of reconciling the brainchilds of Dirac and Einstein an all-out attempt were impressive names combining unique talent and expertise in physics and mathematics (*e.g.,* Schroedinger,[9] van der Waerden,[10] and Einstein[11] himself). Let us take a brief look at the formidable obstacles they encountered in the process of accomplishing their task.

Since the existing spinor formalism dictates the use of local orthogonal inertial frames, and since a Riemann space does not permit such frames to be extended over finite domains, one is faced with the alternative either of dropping the spinor formalism as is, or accepting a situation where the local frames differ from spacetime point to spacetime point.

Clearly, for a general relativistic transcription of the Dirac equations one has to accept the latter (*i.e.,* the multiple frame solution). This procedure is known as the method of *anholonomic* frames. For vectors and the like, it had already been spelled out by Ricci at the turn of the last century. The coefficients of transport recording the orientation changes between adjacent frames are known as "Ricci coefficients of rotation." They are the anholonomic counterparts of the holonomic Christoffel symbols that give the parallel displacement between adjacent points in the holonomic frames originally envisioned by Einstein. Spinors thus forced an anholonomic transcription of Einstein's GR theory.

To those not accustomed to dealing with such seemingly outlandish concepts, let it be known that physicists have been using these concepts all along from the last century onwards. In three dimensions, the familiar procedure of "curvilinear coordinates" is based exactly on the same principles. When introducing polar or cylindrical coordinates, one locally erects orthogonal frames aligned with the coordinate directions. The procedure is used for transcribing the vector-differential operations of gradient, curl and divergence into noncartesian coordinates.

The Reference Alternative of Mathematical Physics

For comparison, keep in mind that the holonomic process of arbitrary references imposes the need to distinguish between a diversity of vector species, yet with a single differential operation (exterior derivative). By contrast, restriction to orthogonal anholonomic frames instead yields a diversity of differential operators (grads, curls and divergences), operating on the one and only vector species permitted by standard vector analysis.

In the present context, an interesting historical question is why 19th century physicists chose to use anholonomic references? The choice seems strange, because they did not have to worry about the spinors which had not yet been discovered? Contemporary textbooks reveal no clue as to the motivation for doing so. The anholonomic procedure is just given without asking questions about alternatives. An explanation is now necessary for later reference.

In the last century, and even today, many physical quantities say velocity, momentum, electric field and magnetic field had been defined only with respect to orthogonal frames. Oblique frames made it necessary to distinguish between many more vector species, such as covariant and contravariant vectors, vector densities and last, but not least, polar vectors and axial vectors.[12] The choice had to do with decisions whether or not an essential physical purpose could be served with all those different vector species.

The wise men of that era opted for the seemingly simpler solution of retaining the one-vector species under the orthogonal group. The metric field tensor in its diagonal form (1,1,1) could be used for mapping of the different species into a single species. Except for crystal physics, there were, at that time, few physical phenomena at the horizon opting for a plurality of vector species. In the general theory of relativity, the metric was, however, identified as a physically active field. The metric-based transcription between vector species could no longer be taken to be physically trivial. The new situation really called for a difference in the historically grown single-vector tradition; yet, somehow, it never led to a radical redrafting from scratch. That is where we were then, and still are today.

The general relativistic transcribers of the Dirac equations had no choice in the matter. Once again, they were forced into a confrontation with these decisions and difficulties of the past. The transcribers were forced to use anholonomic references, because spinors don't admit an holonomic process under Diffeo-4. On the other hand, the 19th century transcribers of classical physical methods to general coordinates did have a choice. They opted for anholonomic references as the simplest solution. The general theory of relativity was never a sufficiently integral part of physics to give holonomic alternatives a chance. In praise of tradition, let it be known that even the modern textbook's way of doing (general) relativity is now anholonomic.

What has become the upshot of the here-cited anholonomity blues of physics for the general relativistic transcription of the Dirac equation? Terrible complications! In fact, it may by now be said that these complicated equations have not led to any new physical insights, except one: the transcriptions of the Dirac equations into a general-covariant garb were not based on a physically well-defined course of action. No useful purpose is served in reproducing here those complicated procedures.

It is not surprising that, the negative outcome of this endeavor had cast new dispersions on the viability of the principle of general covariance (*i.e.,* over and above that which had already been said in the earlier critiques of Kretschmann and Bridgman). Contemporary workers in these fields rarely express opinions about these contradictory matters.

Let it suffice to cite here an observation made in the context of reconciling relativity and quantum mechanics by Misner, Thorne and Wheeler in their tome on gravity.[13] It may be quite representative of attitudes of many workers in this area: "*Spacetime does not exist, except in a classical approximation.*" The authors' inner conflict about this subject matter becomes dramatically apparent, when in the same tome, de Rham's[14] theorems of differential topology are discussed. Statement and theorem reflect a conflict of purpose. In the present volume, de Rham's options are pursued and extended well into the microscopic physical domain.

4. The Physical Motivation for Diffeo-Extension

In a discussion about matters concerning the role of invariances in physics, I remember one participant who argued the physical irrelevance of the principle of general covariance, with the following remark: "If you set your mind to it, just about anything can be written in a general covariant manner." There is indeed an unfortunate truth in this observation, because it is reminiscent of an era when attempts were made to give a "general relativistic" formulation of almost everything under the sun. While the cited observation may be very effective in stopping mindless applications of invariance principles, by the same token it does not exactly contribute to a spirit of utilizing the powerful classification potential associated with an hierarchy of invariance groups and their subgroups.

Just from a purely mathematical point of view, one has to admit that integrability theory better be Diffeo-invariant to have any meaning at all. Hamilton-Jacobi theory, Poisson brackets and Pfaffian integrability are living testimonies proving an indispensable mathematical need for Diffeo invariance.

The physics of crystals illustrates a typical example of mathematical neglect. The point-group symmetries of crystals are known to number 32 in total. They are all subgroups of the rotation group, including reflections and inversions. The pioneers of crystal physics in the last century had to extend the hierarchy of groups from orientation-preserving rotations to those permitting orientation changes. This group extension, and the associated extension of the number of vector species (polar, axial), was absolutely essential, because, without it, we would not have gained an understanding of many phenomena typical for crystals (*e.g.*, piezo-electricity, enantiomorphism and others). Yet, the standard methods of mathematical communication in physics have remained quite unable to accommodate those polar-axial distinctions. At the time, they were introduced ***ad hoc***, when needed; they still are today.

By extending the group description with the element of time-reversal symmetry, attempts have been made at classifying crystals with magnetic properties. However, if somebody now came along by calling for a "general relativistic" formulation of crystallography, the natural response truly ought to be: Let us not exaggerate!

The central question is: What could possibly be a physical motivation for a Diffeo-extension of the hierarchy of invariance groups? Since Kretschmann and Bridgman could not accept the general theory of relativity as a sufficient motivation for a Diffeo-4 extension of the group hierarchy, what other reason exists of which they, and perhaps also Einstein, were unaware? A comparison with crystal physics may again be helpful in elucidating the situation for spacetime.

Consider the study of EM waves in birefringent media. Standard procedure starts out with the Maxwell equations and the constitutive relations for the medium under consideration. The standard renditions of the Maxwell equations are invariant under the group of rotations R(3). The constitutive equations, on the other hand, are invariant only under the symmetry group of the medium under consideration, say, the dihedral group D(4,2) for a tetragonal crystal. There is no question that D(4,2) has to be a subgroup of R(3), because otherwise the Maxwell equations could prejudicially affect the end result. Yet, we expect the Maxwell equations to be applicable to any crystal medium; for all 32 crystal point-groups to be subgroups of R(3), the orientation changes need to be joined to R(3). The tacit, not usually explicitly mentioned, condition is:

Principle of Invariance Hierarchy

The invariance group of the field equation (*i.e.*, fundamental law) should be an overgroup of the constitutive symmetry groups to which the field equations are being applied.

Now, extending these consideration to EM wave propagation in matter-free space, can we apply the same principle? The answer is yes, but prevailing traditions in physics have made it difficult. The first question here is: Where are the constitutive equations of matter-free space? Tradition has had it to build the free-space constitutive equations into the field equations themselves. The invariance group of the free-space equations was first believed to be the Lorentz group; later, through the work of Cunningham[15] and Bateman,[16] the free-space equations were found to be invariant under an overgroup of the Lorentz group, known as the "conformal spacetime group."

All this still leaves unanswered questions about the identification and whereabouts of the free-space constitutive equations. Presumably, the spacetime metric should have something to do with the free-space constitutive equations, because the invariance of the metric defines the Lorentz group in the manner as introduced by Einstein. While the metric itself is not invariant under the conformal group, an algebraic concomitant of the metric is indeed invariant. This concomitant, together with a universal constant known as the impedance of free space, determines the free-space constitutive relations, which map the six vector $\mathsf{F}(\mathsf{E},\mathsf{B})$ into the six vector $\mathfrak{G}(\mathsf{H},\mathsf{D})$. These relations are conformally invariant.

What are the field equations that are to be combined with these free-space constitutive equations? Presumably, they have to be written in a manner using all four field vectors $\mathsf{E},\mathsf{B},\mathsf{H},\mathsf{D}$, such as is customary when using the MKS system of units. The invariance group of these equations has to be either the conformal group or an overgroup thereof.

The next step invokes the dictates of the general theory of relativity. Due to spacetime curvature, the metric cannot be taken as constant in a finite domain of spacetime. A standard, though not universally recommended, recipe of creating relativistically invariant equations, is by replacing partial derivatives by covariant derivatives. Unfortunately, covariant derivatives invoke the metric through the Christoffel symbols. This act of creating general covariance contaminates the field equations with elements of constitutive description. How can this be avoided?

A functional separation of field- and constitutive-laws is possible if, and only if, the Maxwell equations can be rendered in a general covariant form without the help of covariant derivatives. In the Twenties, Kottler[17] and Cartan[18] discovered that such a metric-independent invariant form of the Maxwell equations indeed exists. Notwithstanding an independent and later rediscovery by van Dantzig[19] in the early Thirties, and an overview of these matters by Whittaker,[20] as well as two monographs in English[21] and one in German,[22] textbooks published in the subsequent six decades have been repeating the same half-truths or falsehoods about this subject matter.

A popular half-truth is: Maxwell's equations are invariant under Lorentz transformations, but not under Galilei transformations. *The truth is the metric independent rendition of the Maxwell equations is Lorentz- and Galilei-invariant, because both are subgroups of Diffeo-4.* The unified treatment of Micheson-Morley and Sagnac experiments in chapter XVIII testifies to the practical usefulness of a slightly wider conceptual horizon. The dominant neglect of mathematically precise language in physics has led to a bereavement of useful new perspectives. It should therefore not be surprising if reconciliation between relativity and quantum mechanics has not been forthcoming in an atmosphere of pragmatically reducing mathematical perceptiveness.

Since detail of technical notation frequently triggers unwanted associations, let us verbally summarize the secret of the metric-independent generally covariant form of the Maxwell equations. Since standard textbook renditions of Maxwell theory are presented in a rotation invariant form, excluding reflections, the use of one-vector species suffices. Extending the invariance group creates new vector species; they reduce to one species, once the group is restricted to rotations. *The Diffeo-4 invariance of the Maxwell equation is contingent on the physical identification of four spatial vector species.*

Physics has been disinclined to differentiate between those vector species, because it involves decisions of physically identifying first the distinct Diffeo-3 vector species to be grouped together as two spacetime

Diffeo-4 six-vector species. Instead, physics consensus has opted to continue the *one-vector gospel* by erecting anholonomic local orthogonal inertial frames. This "Myth of Simplicity" is now backfiring, creating a hurdle in understanding the compatibility of two major disciplines.

While Whittaker was still undecided about the implications of these mathematical physical elaborations, Cartan probably came closest to a physical message by emphasizing the integral origin of the Maxwellian laws. The implied invariance of those integral statements then automatically determines the transformation of those integrands as the four distinct Diffeo-3 and corresponding Diffeo-4 fields. Cartan, in his method, preferred to refer to the bilinear expression of field components and integration elements as differential forms, thus reducing undue preoccupation with the subjective choices of coordinates.

To cover the options demanded by physics, we need to distinguish between scalar-valued integrals and pseudo-scalar-valued integrals. The first are absolute invariants under arbitrary changes of coordinates; the second only change sign under orientation changes of coordinates. De Rham calls the differential forms associated with scalar-valued integrals "pair"; the others associated with pseudo scalars are called "impair." The assignment of pair and impair, when combined with the dimension of the integration domains of the integrals, gives an idea of the field species necessary in holonomic description.

There are already four distinct vector species needed to define the differential forms for the generally invariant one- and two-dimensional scalar-valued integrals of standard Maxwell theory. The fields **E** and **H** relate to one-dimensional integrals, the first integral is pair (scalar-valued); the second integral is impair (pseudo-scalar-valued). The fields **D** and **B** relate to two-dimensional integrals, the first integral is impair and the second integral is pair. The one-dimensional integration element changes sign under spatial inversion, and the two-dimensional integration element does not; it follows that **E** and **D** are polar vectors and **H** and **B** are axial vectors, which, indeed, is the assignment demanded by crystal physics.

Without going through all the details of identification, it should be mentioned that de Rham's differential form assignment of pair and impair is in one-one correspondence with the vectorial and tensorial transformation assignments given by Schouten. Since the Schouten-de Rham distinctions are absolutely necessary for coping with crystal physics, the latter-day prophets of modernizing mathematical communication in physics have, in their fervor of eliminating coordinates from physics, made the mistake of also leaving out de Rham's pair-impair distinction! Ironically, what was meant save work ends up causing more work. Unfortunately, to some, such developments have a sunny side; they provide material to propose funding for "new" research.

The just-mentioned integrals of the differential forms of electro-dynamics have been found to count quanta. They have been extensively discussed in the previous chapters. These facts were known long ago. For Gauss' law of electrostatics, it became apparent, early in the 19th century, through Faraday's discovery of discrete charge (his law of electrolytic solutions). This integral counts an algebraic sum of elementary quanta $\pm e$ of electric charge inside a closed surface of integration. An extension to include Ampère's law covers dynamic situations of cooperative motion of charge.[23] Another example is the integral of Aharonov and Bohm.[24] If its path of integration resides in a field-free domain, it is known to count units of flux. R.M. Kiehn[25] has added to these one-and two-dimensional integral counting devices a three-dimensional device counting units of integrated spin and angular momentum.

Since counting quanta does not depend on choice of coordinates or units of measurement, those integrals exhibit metric-free Diffeo-4 invariance. In the absence of a metric, no references can be made to macro- or micro-physics, thus indicating a potential macro and micro applicability of these integral laws.

An overview of these period integral methods is given in ref.26, and their applicability to a diversity of physical macro and micro situations is presented in ref.27 and in chapters VI-XI.

Those who study the metric structure of the universe may be interested in the possibility that metric-free laws could, in principle, retain physical relevance even in domains of unusual metric behavior, such as are expected to be manifest in black holes.

5. The Diffeo Equivalent of Spinorization

In the sense of Haefliger[5] and Milnor,[6] spinors give local testimony to spacetime's global ability to accommodate spatial and spacetime enantiomorphic objects. Analogous to the reference alternative of section 3, we may now consider a

Spin-Orientability Alternative.

Recognizing spin as a local artifact for dealing with the structure of spacetime orientability and the enantiomorphic objects embedded therein, an appropriate use of Diffeo-4 global description should be able to replace the standard use of spin formalisms.

Hence, avoiding the hurdle of being tied down by a spin-restricted choice of local frames, the local description needs to be abandoned altogether to make place, from the start, for a global description. The Diffeo-4 invariant integrals of previous chapters and sections are ideally suited for global description. A subsequent point of concern is then finding field configurations that have a potential of accounting for spacetime pairing such as is manifest in the enantiomorphism of charge-and spin-pairing.

The local image of point-electron is clearly no longer adequate; instead a ring-type configuration is needed, which accounts for pairing and magnetic moment. In chapters VIII and XI, the knotted ring configuration in space, also known as trefoil, was selected as having left and right modifications. Subsequent application of the cited period integrals to this trefoil configuration indeed accounts for many observed data associated with electron and muon pairs, including their half-integer spin and the first order anomaly of their magnetic moments.[27,28]

The cited procedure gives a finite calculational alternative compared to standard QED calculations for point-electron and muon, which require the processing of calculational infinities. Also, a qualitative understanding of higher-order anomalies is possible; yet, for a quantitative evaluation of the higher-order terms, the QED process has an edge over the trefoil model.

Fundamental in the distinction between wave-equation approach and period-integral approach is the evolution from point-model to a finite model, with a specific topology. Unlike the metric-related predicament of Diffeo-4 invariance encountered with the wave-equation, the period integral procedure, by contrast, presents no problem. In fact, Diffeo-4 invariance has emerged as essential to make those gains possible.

There is reluctance to accept spacetime on a Diffeo-4 basis. The unilateral properties of time play a role in these inhibitions. It is, therefore, relevant to draw attention to the little-known fact that so-called one-dimensional topological torsion manifests itself for the first time when making the transition from three to four dimensions.[29] Nature gives us perfect cyclic time in the form of stable elementary particles, and arrowed time through worldlines and entropy-related phenomena. Torsion aspects are discussed in chapters XII and XIII.

6. Conclusion

Looking back at the array of arguments which have been brought to bear on the compatibility between relativity and quantum mechanics, the most compelling one undoubtedly is that all those years, we have been addressing the wrong situation. *The quantum mechanical wave equations have mistakenly been elevated to a fundamental position that could not be theirs.*

The cause of this dubious assessment is a consequence of Copenhagen's failing to make adequate distinction between ensemble, and ensemble constituent. While this omission was acceptable in the days of early development, because of the asymptotics between ensemble and constituent, the time has come either to prove this identification as a viable proposition, or to abandon the Copenhagen interpretation altogether.

In absence of a proof of viability, a farewell to Copenhagen views is now indicated, with thanks for the many services rendered. Much useful information was gained by pushing the methods just over the edge of applicability. In fact, a farewell to the Copenhagen of 1930 is, in part, a reinstatement of principles, which, a decade earlier, had also emerged in Copenhagen.

Today, textbooks on quantum mechanics provide details of solving the Schroedinger equation for the harmonic oscillator. It shows how this solution leads to an eigenvalue spectrum for the energy of $(n+1/2)\hbar\omega$, with n=1,2,3..... replacing Planck's earlier spectral formula $n\hbar\omega$, without the 1/2. The spectrum without the 1/2 was said to be a relic of a theory of the past, referred to as incomplete and inadequate. The term 1/2 had been created by the magic of the Hermite polynomial solutions of the Schroedinger equation.

Yet, when it so magically appeared, the 1/2 term was rather immediately identified as a contribution due to an always-present zero-point energy. How did they know? The identification is usually made without much of an explicit rationale and so it is passed on from textbook to textbook. The unsuspecting reader may well think that zero-point energy was first discovered with the help of Schroedinger's equation. Nothing is further from the truth, though, than this suggestion!

More than a decade prior to the emergence of the Schroedinger equation, Max Planck[30] identified zero-point energy as a property that would be manifest in a phase-random ensemble of harmonic oscillators. He was guided to this conclusion by his rejection of negative probabilities.

Suffice it to say that Planck's derivation of zero-point energy was, of course, contingent on the assumption of energy states $n\hbar\omega$ for the individual harmonic oscillators. Now we know how the textbook writers knew why the $\hbar\omega/2$ had to be a zero-point energy. And so it was passed on from one to the other.

Although I have seen quite a few textbooks on quantum mechanics, I have not seen them all. Of the ones that I have seen, I remember hardly a single one referring to Planck for the idea of zero-point energy; much less do they refer to Planck's ensemble-based derivation thereof. All of this seems so contrary to the best science traditions. We need to ask how this distortion of history came to pass?

Had this distortion been intentional, it would have been an act of simple dishonesty. If it was unintentional, it should, perhaps, be referred to as "disingenuous." Whichever name applies, the fact of the matter is that a younger generation has been so imbued with a nonclassical Catechism that the interest in why, and where it all came from, has simply subsided.

As proof that these omissions of referring to Planck were most likely made in good faith, consider that also the much fewer "ensemble authors" about quantum mechanics have conspicuously failed to get Planck on their side. Also, any reference here to Planck's zero-point energy is, amazingly, missing. What more evidence do we need that, all those years, nonclassical dogma has prevailed over reason? Is contemporary quantum mechanics going the path of religious fundamentalism?

Here is one more example of failing to draw classical conclusion from a nonclassical predicament. The Schroedinger equation is known to lead to an angular momentum quantum number $\sqrt{n(n+1)}$, which for large n becomes n+1/2, thus suggesting a perhaps distant relation to the 1/2 of the zero-point energy. There are two textbooks[31,32] that give a *classical statistical derivation* of the number $\sqrt{n(n+1)}$ by an evaluation of the average modulus of angular momentum for an ensemble of directionally randomized objects in angular momentum states $n\hbar$. Both texts present this derivation as a perhaps interesting mathematical oddity; they stop just short of considering the physical-ensemble option as an essential feature of the wave-equation approach. Why does their classical counter-example not rule out the nonclassical escape? or would the use of classical logic perhaps violate the premises of nonclassical procedures?

Mindful of the massive amount of physical research that is either directly or indirectly contingent on an explicit or an implicit use of a nonclassical Catechism of quantum mechanics, let us note that the time has come for a reconsideration of its epistemological foundations. Many conferences have been devoted to this objective. The emphasis tends to be more on what is new now and less so on what was. Since the here-presented thoughts relate so closely to the work of Einstein and Planck both, it seems fitting to cite Einstein in a tribute to Planck:

....."Some men come to the temple of science, because it offers opportunity to display their particular talents. Science is a sport in which they excel. Others come to the temple offering their contribution in the hope of profitable return. These men are scientists by circumstance; the opportunity that happened to present itself when making their choice of a career. Had the attending circumstances been different, they might have become politicians or captains of business. Should God's angel descend from heaven and drive from the temple those who belong to those categories, I fear the temple would be an emptier place. Few worshippers would remain, some from days gone by and some from our generation. Our Planck belongs to the latter, and that is why we love him." (translation)

These words give heartwarming testimony as to how charisma and professional excellence can go together. Those who know the contemporary atmosphere in science and the pivotal role of funding may wonder whether even an angel from heaven could remove the financial mire from the doors of the contemporary "temple of science." Finding elegant solutions to unavoidable predicaments, though, tests man's capacity for civilized behavior. Coming generations have to find a way to live up to that challenge.

CHAPTER XVI

QUANTUM UNDERSTANDING IN GLOBAL PERSPECTIVE

1. Frame of Mind

Since all that is said here has been said before, I ask indulgence for this additional account. Let it be taken as a legal writ to set the stage for freeing the spirit from what has been said; it is here attempted by repeating in different order that what has been said before.

Quantum mechanics, as manifested in the Schroedinger eigenvalue procedures, has become a sophisticated and finely honed calculational process. Articles using the process, and textbooks explaining it, reveal how physicists have become extremely clever in calculating experimentally verifiable results without really telling why they do what they do.

The modern way of presenting is by emphasis on relevance. The steps taken must be justified by results. This way of working is fairly universal and may not necessarily stop short of a disposition that sanctifies the means if they leads to a desired goal.

Those who have been engaged in the sport of calculus know that solving integrals can be critically dependent on making the right substitutions. The end result, or lack thereof, then justifies or rejects the chosen substitution.

Yet, for some cases, there is no general key. Somebody in the past had a stroke of genius and just did it. These are instances where science can become an art. However, of all the conceivable functions that exist, the arsenal of known algebraic and transcendental functions occupies only a tiny sector. This humbling thought can help us realize that in a wider science context, finding the right substitutions is a game we play to serve a more encompassing objective.

Mindful of the delicate balance between science and artistry, let us now turn to quantum mechanics, and the Schroedinger equation in particular. It takes artistry to deal with the Schroedinger equation. While artistry does not always serve pragmatism, in quantum mechanics, pragmatism is served by artistry. One does not need to know why it works to make it work. Those of us who operate computers know this to be true. The famous self-made English mathematician, Oliver Heaviside, told us a long time ago:

"To enjoy a good meal, one does not need to know how it has been prepared." He was referring to his unconventional operational procedures that were later confirmed and substantiated by his academic colleagues.

The Heaviside episode took place in the last part of the Nineteenth Century. Not to prematurely denounce unconventional genius was a lesson for academia. Now, moving forward in time to the second quartile of the Twentieth century, a quirk of fate placed academia in the very same position Heaviside had found himself half-a-century earlier. The Schroedinger equation was born. It was a gift from heaven, somewhat akin to Heaviside's operational calculus. It did a marvelous job, by giving numerous correct and useful results.

It was shown to duplicate an earlier procedure, initiated by Heisenberg. Subsequently, it appeared that the ready-made spectral theory of Hilbert spaces emerged as the ideal instrument to serve this new physics discipline, which was now called "quantum mechanics." Finally Dirac's relativistic version was hailed as the synthetic penultimate which was taken to replace, in one fell swoop, both Newtonian and relativistic mechanics.

Yet, notwithstanding these lofty perspectives, science willingly acknowledged that questions persisted. Where was the complete physical rationale leading to this amazing instrument of science? Schroedinger had used an interesting transcription. He never claimed it to be a "proof." The proof of the pudding was wholly in the eating, which, of course, was much aided by having this near perfect Spectral Theory of Hilbert spaces ready to serve the good cause. Hermann Weyl had called it "a favor of fortune" that so much mathematical perfection could fit like a glove on a physics procedure that had still retained so many conceptual obscurities.

It is one of those ironies of fate, that half a century after Heaviside had his trouble with academia, academia herself adopted the Heaviside technique of getting useful results with a tool that was not understood; and they had a very good time with it. The artists, who knew how to make music with this wonderful new instrument called the "Schroedinger equation," preferred not to over-analyze its physical meaning. After all, unlike the case of Heaviside, everything now seemed mathematically well above-board, protected, as it were, by the towering authority of Hilbert. Should not that fact alone sanctify the physical implications? Then, last, but not least, there was perhaps a little bit of that underlying fear, that probing unduly into the origins of a gift from heaven might in the end kill the magic of the method's wonderful potential.

Yet, the quest for knowing more about the physical angle will ultimately transcend the special talents that have so diligently and competently exhausted a set of almost conspiring coincidental circumstances. Perhaps their urge to excel in this wonderfully pragmatic artistry of Schroedinger-generated mathematics may now be sublimated and redirected in the opening up new horizons. Let us probe some of the ideas of what people believe or imagine is going on behind the scenes in that equation.

2. Existing Understanding

The efforts at physically understanding the usefulness of Hilbert's spectral theory for quantum mechanics are here best examined in the perspective of the following constructive highlights that had a determining influence on the structure of the theory:

1. *Schroedinger's smeared-out particle model* was a first attempt at replacing the Newtoniam point-particle by a finite particle extension. There was an underlying assumption that the wave equation described a single system. Schroedinger's scheme was abandoned for more particle systems, because it led to the uncomfortable image of smeared-out particles interpenetrating in actual physical space.

2. *The uncertainty relation* entered quantum mechanics in essentially two steps. Heisenberg first presented the principle as a consequence of measurement consideration. The process shows a distant kinship with Abbe's famous diffraction criterion limiting the resolving power of an optical instrument, which, in turn, can be seen as a precision criterion for truncated Fourier expansions. The subsequent derivation of the uncertainty relation from the Schroedinger equation by Kennard then did much to consolidate its position as a navigational marker for future courses of action.

3. *Copenhagen Interpretations:* Schroedinger's smeared-out particle, and his equivalence proof of matrix and wave mechanics, as well as Kennard's derivation of uncertainty, carried suggestions that had an inductive role for Born's subsequent interpretation of the wave function as a probability amplitude. From that point on, dating back to the early Thirties, *the Copenhagen interpretation emerged as a statistical description of single-system behavior.*

4. *Ensemble Interpretations:* The Copenhagen interpretation could, in a sense, be regarded as a Gibbs-type ensemble of conceivable manifestations of one and the same single system. *Popper* argued that the epistemological basis for that sort of a supposition was not very convincing, because all, at that time, available observations could either be traced to real physical ensembles of many systems in a domain of space (spectra) or to a stream of particles (Davisson-Germer experiment) representing an ensemble randomly stretched out in time. The ensemble observation "stretched out in time" came suspiciously close to Copenhagen's Gibb's-type ensemble of conceivable manifestations of one single system. The *Copenhagen single-system* and *real physical ensemble* interpretations have been able to coexist without major confrontations for more than half a century.

5. *Hidden Variables:* In the course of time, the Copenhagen point of view generated questions as to whether or not the Schroedinger description could be considered as complete. The absence of an explicit universe of discourse for the statistics of the Copenhagen description led to a terminology referring to it as "nonclassical." By the same token, this led to suggestions that there were variables to which this nonclassical statistics applied, but perhaps they had to be regarded as hidden variables. In the early Fifties, David Bohm[1] became an explicit spokesman for this way of thinking. These developments became known as *"hidden variable theory."*

6. *Bell's Theorem:* In 1964, John S. Bell[2] succeeded in proving a theorem indicating that the existence of local hidden variables would lead to certain inequalities, which would constitute deviations from the predictions of standard theory. Experiments by Aspect[3] and coworkers in 1982 did not support the inequalities. Instead, they confirmed standard quantum mechanical predictions. This event had to be regarded as taking away the physical basis for a *local hidden variable theory.*

The last episode places Bell and Aspect *et al* in the attention center as saviors of the pragmatist view of the quantum formalism, as it presently exists. It did not resolve the ensemble-versus-single-system issue. Hence worries about interpretation have persisted. In informal encounters, Bell himself has been reported as saying about the Copenhagen views: something is rotten in the state of Denmark. A remark that was, of course, not meant to reflect on the denizens of Denmark.

3. Local and Global Concepts in Mathematics and Physics

In the discussions of Bell's theorem, a new concept called "local" is found to play a key role in the assessment of physical situations. In view of the importance of this seemingly innocuous restriction, a more incisive discussion is in order. The concept *local* and its counterpart, the concept of *global,* have gained a prominent role in mathematics since the Twenties. This increased awareness was generated by developments in topology and in differential geometry. The integral counterpart of the latter made these distinctions more necessary now than at any time earlier.

Two other words, or rather two other expressions, have been, and still are, in circulation, conveying that same distinction. They are *in the small* and *in the large.* Here, we shall prefer the words *local* and *global,* because *in the small* and *in the large* have a metric connotation implying size. In physics, an elementary particle or an isolated atom or molecule, are global entities, even if they are conceived of as being very small.

Marston Morse,[4] who has been a pioneer in pursuing the mathematical implications of these concepts, mentions that it is not easy to grasp the essence of these notions in a set of satisfactory definitions. Hence, if it is already difficult to do so within the framework of mathematics, one may expect added complications in the process of extending these concepts to the realm of physics. Before any definition should be given, it is necessary to develop first a level of greater awareness for these distinctions. For the time being, we do well to follow Morse's counsel by assuming that it does not seem necessary, or even desirable, that hard-and-fast definitions be given. (compare chapter VI;1 and chapter III;6)

Without hard definitions, the process of getting acquainted with the concepts "local" and "global" in physics is by delineating parts of existing physics according to known local and global perspectives. Electromagnetic theory is an almost ideal example for bringing out the physical relevance of these concepts in their full stature.

Local and Global in Electromagnetism

Electromagnetism is governed by two major global laws. They are (1) charge conservation expressed by the *exactness* of the 3-form of charge density and current density $\mathsf{C}=[\rho,\mathbf{j}]$ and (2) flux conservation expressed by the *exactness* of the 2-form $\mathsf{F}=\lfloor \mathsf{E},\mathsf{B} \rfloor$. Global conservation is mathematically expressed as a *global criterion of exactness* by the fact that their cyclic integrals vanish for all cycles:

global charge conservation: $\oint\oint\oint_{c_3} \mathsf{C} = 0$ for all cycles c_3

global flux conservation: $\oint\oint_{c_2} \mathsf{F} = 0$ for all cycles c_2

The local counterpart of these laws are obtained by calling on Stokes law; one thus obtains the more familiar differential laws:

$d\mathsf{C}=0$; local conservation of charge.

$d\mathsf{F}=0$; local conservation of flux (first set of Maxwell equations).

The exactness of C and F has a further global consequence, according to a theorem by de Rham;[5] **modulo an exact part, there exist globally defined 1-forms A and 2-forms $\mathfrak{G}$ such that**

$\mathsf{F}= d\mathsf{A}$; definition equation of the vector potential.

$\mathsf{C}= d\mathfrak{G}$; the second set of source related Maxwell equations.

An *extended* global interpretation of these relations is now dictated by existing knowledge of physics. All sources of charge are known to be discrete and multiples of an elementary charge e their worldlines can be "linked" by cycles z_2 residing where $\mathsf{C}=0$. For flux, instances of discrete units are manifest, provided some specific conditions are obeyed. *It thus goes a step further by stating that all flux can only be a multiple of an elementary discrete unit* $h/2$ *if, and only if, it can be viewed as linked by* *cycles* z_1 *residing where* F=0. Another de Rham[5] theorem now permits, on the basis of those premises, the claim:

$\oint\oint_{z_2} \mathfrak{G}$ = the Ampère-Gauss integral counts units e "linked" by z_2

$\oint_{z_1} \mathfrak{A}$ = Aharonov-Bohm integral counts units h/2e linked by z_1

This one-page breakdown of electromagnetic fundamentals is an only slightly idealized local-global reorganization of existing well-established knowledge. The principal difference with respect to standard versions is a changed emphasis of awareness. This modified presentation recognizes as an integral part of electromagnetic theory a quantum superstructure of period integrals, enabling us to count units of charge and flux.

Local and Global in Quantum Mechanics

Let us now see if we can make a similar examination of the existing tools of quantum mechanics on the basis of this newly acquired awareness of local and global connotations.

The differential relation intimated by the Schroedinger equation has to be identified as a statement of local conditions with global ramifications as imposed by the boundary conditions. Let us see whether we can pinpoint in some detail the global implications of these boundary conditions, by examining a specific case.

An application of the Schroedinger equation to cyclotron motion (Landau) reveals that the zero-point energy component in $(n+\frac{1}{2})\hbar\omega$ is due to the boundary condition of square integrability. A Larmor-type situation, such as prevails for the superconducting ring, having fixed radius orbitals, by contrast, reveals an absence of zero-point energy. The boundary condition of single-valuedness now gives quantum states $n\hbar\omega$ with n=1,2,.... .

We learn from this comparison that the boundary condition of single valuedness governs a global feature, which can apply in the small on a very local level, whereas the condition of square integrability governs behavior at infinity which, to say the least, has definitely a more environmental connotation. Here, we have an interesting reminder of Marston Morse's warning not to rush into fast and hard definitions of local and global. We also have a reminder that, unless specified to the contrary, the terminology *local-global* is to be preferred over the terminology of *in the large* versus *in the small,* because single-valuedness is a global condition, which can have a very pronounced connotation "in the small." In fact, in the next paragraphs, a new use is proposed of the terms "in the small" and "in the large," which is in better keeping with their metric connotation of size.

Let us ask hereto what this cyclotron-Larmor comparison tell us about the interpretational aspects of the Schroedinger process? The fact is: *The Schroedinger process imposes global conditions in the small and in the large.* Unless very outlandish schemes of interpretation are admitted, it will not be possible to accommodate this local-global dichotomy in the Copenhagen perspective of a single system existing in the small. Nonclassical conceptual excursions are the prize to pay for accommodation.

By contrast, an ensemble view of the Schroedinger process very naturally accommodates this dichotomy of boundary conditions. The new state of affairs can now be succinctly summarized as follows:

The Micro- and Macro Roles of Global Conditions

The global boundary conditions can govern "in the small" the individual systems in an ensemble. Other global boundary conditions can govern "in the large" the behavior of system elements in the context of the totality of the ensemble (compare chapter III; 6).

The next section gives a realignment of an avalanche of already existing evidence that further corroborates an ensemble connotation for the mixed local-global Schroedinger process. This realignment also leads to an identification of a universe of discourse of the presumed nonclassical statistics of the Schroedinger process.

4. A New Perspective on Quantum Understanding

Our next step is an identification of a set of presumed hidden variables that, interestingly, can be given both a local as well as a global identification. They are the initial conditions characterizing the individual dynamic performances of the elements of an ensemble. They are the phase of a dynamic system and its orientation in space. Just looking at the performance of isolated individual systems, phase and orientation are rather inconsequential parameters. They only seem to serve a purpose of mathematical pedantry, say, to meet the requirements of a demanding teacher who wants us to be aware of the existence of constants of integration.

Now, look at these same hidden variables of phase and orientation from an ensemble point of view. It would be most unlikely if the mutual phase and orientation between the elements were taken to be inconsequential for the ensemble behavior. Clearly, we need to distinguish between phase- and orientation-ordered ensembles, such as do occur in meso- and macroscopic quantum behavior, versus phase and orientation randomized ensembles.

Nobody less than Max Planck[6] felt that we had an obligation to look into these matters. He indeed did so in 1912, and showed that an average zero-point energy of $\hbar\omega/2$ would be necessary to secure a positive

Boltzmann probability for an ensemble of elements that is randomized in mutual phase.

Calculations of an average modulus of angular momentum for an ensemble of randomly oriented elements gives $\hbar\sqrt{n(n+1)}$. This result is reported in a text by Kompaneyets and also in the Feynman Lectures.[7] In both texts, it is mentioned as an interesting mathematical oddity. There are neither attempts at assessing the impact of this result on aspects of interpretation, nor is a comparison made with the earlier reported Planck zero-point energy. A mutual relation for large n is hinted by the asymptotics $\sqrt{n(n+1)} \approx n + \frac{1}{2}$.

Summarizing these findings, two typical results with an historic role in the rapid acceptance of the Schroedinger process are reproducible by a statistical processing of real physical ensembles. The individual single systems are hereto quantized by the earlier asymptotically related Bohr-Sommerfeld process.

The Schroedinger quantization is of a hybrid local-global type. The Bohr-Sommerfeld process, by contrast, is purely global. The asymptotic relation between Schroedinger and Bohr-Sommerfeld methods is not merely a mathematical approximation. It reflects physical asymptotics in that one addresses single systems; the other addresses ensembles thereof.

5. Conclusion

We can now be very brief about how all this relates to Bohm and Bell. Phase and orientation can simply be taken as examples of David Bohm's hidden variables. They deviously slipped through the mazes of Bell's theorem, because locally, they are inconsequential, and globally, they are outside the realm of jurisdiction of Bell's theorem.

Local and global in quantum mechanics can now be further examined in the context of relativity. The Kretschmann[8] and Bridgman[9] objections against Diffeo-4 invariance are now to be delineated for aspects of numerical- and form-invariance. Numerical invariance for the global residues of period integrals is fine, yet, locally, in the sense of crystal physics' Neumann principle, it could eliminate just about all local constitutive behavior. Hence, we can repeat after Pierre Curie: "C'est l'assymetrie qui fait l'effect." Meaning: "the effect is in the asymmetry." Hence, numerical local Diffeo-4 invariance could only convey a **trivial** amorphous state of affairs, leaving no effects whatsoever. Seen in this perspective, Kretschmann and Bridgman would have had a point afer all.

Nontrivial numerical Diffeo-4 invariant objects are global in nature. Examples are the integrals of chapter VI, which either identify quanta as periods or express conservation. Examples of Diffeo-4 form-invariance are the Stokes theorems and the exterior derivatives of forms. When carried to its logical conclusion, the Kretschmann-Bridgman episode of assault on the principle of general covariance ironically justifies this principle in the end. In addition, it yields, in local-global perspective, a distinction between constitutive law and media-independent fundamental law. Discerning application can make the principle of general covariance a powerful ally in theorizing, whereas indiscriminate rejection of the principle is surely an open invitation to create confusion and disorder.

After taking a more discerning view of the principle of general covariance, by including notions of local versus global and matters of metric-dependence versus metric-independence, it would seem that Einstein's[10] intuitive defense of covariance is vindicated at last.

CHAPTER XVII

ABSOLUTE VERSUS RELATIVE INDETERMINISM

1. Absolute Indeterminism

Contemporary physics may be said to adhere, somewhat hesitantly, to a principle that could be called "absolute indeterminism." In practice, the hesitation means that perhaps a majority of physicists vaguely confess to subscribe to such a point of view for what they perceive as practical reasons. This portrayal of attitudes, with respect to quantum fundamentals, is in many ways characteristic of Copenhagen-type interpretations.

This Copenhagen scheme culminates in the Heisenberg uncertainty relation. The latter, when taken in the Copenhagen spirit, supports a point of view that might be called an expression of nonclassical pragmatism. This unorthodox pragmatism has a measure of inner consistency, as predicated by the uncertainty relation that led to the Copenhagen view of things.

The Heisenberg uncertainty relation says that the magnitude of two conjugate variables describing the state of a system, cannot be simultaneously known with arbitrary precision. If position q and momentum p represent a conjugate pair, the uncertainty relation claims that the product of the observed position uncertainty Δq, and momentum uncertainty Δp, is subject to the inequality

$$\Delta p \Delta q \geq h, \tag{1}$$

in which h is Planck's constant. The Copenhagen School boldly claims that any object from planet to elementary particle is everywhere and at any time subject to this inequality.

The relation originally was obtained using an idea that any observation had to disturb the object of observation. This original argument serves as a means for obtaining the uncertainty relation; it carries a connotation that uncertainty is an all-pervading feature of nature, contingent on the act of observing.

There is a Schroedinger-based derivation of this uncertainty relation, which, all by itself, carries a suggestion of observer-independence. When taken in the spirit of the Copenhagen single-system view, this Schroedinger uncertainty assumes the nature of an all-pervading *a priori* feature of all single systems. Yet, when taken in the spirit of an ensemble view, this uncertainty becomes a feature of ensemble disorder. The precise statistical nature of uncertainty, whether taken in the single system or the ensmble vein, is according to prevailing teachings believed to be unknown. The temperature does not have a role as a statistical distribution parameter in this so-called "nonclassical statistics."

Since inequality (1) translates into an equivalent state of continual motion, uncertainty thus indicates the existence of a residual state of kinetic energy. It has become customary to call this uncertainty-based energy a zero-point energy.

In assessing the details of these relations, the reader should be aware that the inequality (1) can be affected by, give and take, a numerical factor, depending on the line of reasoning used to obtain it (*e.g.*, the Heisenberg or Kennard approach). Since it is an inequality, not anywhere close to experimental verification, not much attention is given to these small changes. It proves experiment has rarely been close enough to even testing true limits of these inequalities.

Since uncertainty suggests a measure of indeterminism in physics, it has produced a large literature, some of it clever, but not so relevant, and some not so clever, but perhaps relevant. Let us aim here at relevance, even if it may not reach the pinnacle of cleverness.

2. Relative Indeterminism

The cited less radical, yet less popular, point of view holds that uncertainty and zero-point energy are typical ensemble manifestations and not to be associated with any or every isolated single system. Uncertainty thus emerges as a criterion of ensemble "awareness." Since there are many phases of ensemble awareness, as substantiated by the solid and liquid states of matter all the way to diluted gaseous states, a set of typical specifications is necessary to tell us under what circumstances an ensemble resides in the zero-point state.

Planck,[1] who introduced the concept of zero-point energy in 1912 as an ensemble-based phenomenon, gave us the first physical specification of that zero-point ensemble. The ensemble statistics is pre-thermal, as would be fitting for a zero-point state, and its universe of discourse is determined by the mutual phases of the constituent elements of the ensemble. It is hereby assumed that Planck's ensemble consists of identical dynamical periodic systems, the phases of which are taken to be random.

There are mechanisms known that can create a phase order in such an ensemble. If such a situation prevails, there is no longer a statistical situation and there is reason to speak of a macroscopic quantum effect! The quantum Hall effect is a recent discovery that belongs in this category.

Yet, as long as phase-disorder prevails, there is a statistics with the help of which a statistical probability of the ensemble can be evaluated. In this manner, Planck showed that the zero-point energy is a *sine qua non* for a phase random ensemble to have a nonnegative probability. By excluding negative probabilities as meaningless, the ensemble is forced to assume a lowest average energy of $\hbar\omega/2$ per ensemble element.

3. Comparing Absolute and Relative Indeterminism

An ensemble-based interpretation of the Schroedinger equation has been around for a long time. The philosopher Popper[2] has been known as an articulate spokesman supporting an ensemble view. The epistemological arguments he used have proven to be lasting; they are as valid today as when they were launched for the first time in 1934.

Einstein strongly supported Popper's epistemological point of view, even if he did not agree with some of Popper's physical arguments. Popper conceded to having made physical mistakes, yet, apart from these mistakes, the basic issues, raised by Popper, have remained very much alive.

However, after one lonely ensemble supporter (Popper) had to admit to the error of his ways to another ensemble supporter (Einstein), many took for granted that nothing much was left of the ensemble point of view. Those willing to read the footnotes in the published Einstein-Popper correspondence[2] will find that Einstein supported Popper's position.

Rather than directly supporting the ensemble view, Einstein and coworkers, Rosen and Podolsky, were, at the time, attempting to home in on conceivable internal inconsistencies of the Copenhagen point of view. As it was, the cited differences in procedures (*i.e.,* Popper's frontal attack and the EPR consistency check) turned out to be fatal to the ensemble point of view in the Thirties and served neither Popper nor Einstein, Rosen and Podolsky.

Slater may have been an earlier supporter of the ensemble idea when the Copenhagen interpretation was still *in status nascendi.* He first reluctantly went along with Bohr and Kramers, but later changed his position. Slater[3] reports that his change of heart led to a considerable cooling in his relations with Bohr.

It is not precisely known whether either Popper[2] or Slater[3] did anything to specify their ensembles so as to bridge the gap to Planck's zero-point ensemble.[1] The phase-random ensemble of Planck yields, per element, the same zero-point energy as the Schroedinger equation, except that Planck's zero-point energy is an ensemble average, whereas Schroedinger's value, in the Copenhagen spirit, is a "quantum mechanical" average for which there is no universe of discourse, as required by statistical theory.

Contemporary physics makes this absence of a universe of discourse explicit in its terminology by calling the statistics "nonclassical" in nature.

In the early days of Planck's zero-point discussions, there was, of course, not yet a Schroedinger equation to tell us about zero-point energy. When Schroedinger's equation was first solved for the harmonic oscillator, how did it became known that the extra 1/2 quantum number, demanded by the Hermite polynomials, should have a zero-point connotation?

While Schroedinger did not fail to mention Planck, those who followed left Planck completely out of the picture. Few of the Copenhagen-based textbooks, if any, find it necessary to quote Planck on this subject. The reason is simple: Planck's argument does not exactly reflect supportively on the Copenhagen point of view. Our contemporary textbook authors practice a policy of letting sleeping dogs slumber. Having the Copenhagen point of view comes across, unencumbered by undue and perhaps "premature" doubts, has had an unwarranted priority.

It is downright astonishing, though, that neither the textbook claiming to present the ensemble point of view have mentioned Planck's zero-point ensemble. Even Ballentine's[4] most recent scholarly contribution to the ensemble-based literature has no reference to Planck's zero-point discussions, notwithstanding recent evidence in which the existence of such a connection has been reiterated.[5] ***How could the ensemble supporters have missed out on such a golden opportunity of getting Planck's authoritative evidence of 19i3 to help them?***

It is hard to find a conclusive answer to this question, because, in the enduring mist of nonclassical conceptualization dependence on logical inference no longer holds a major position. Since Planck's zero-point work took place prior to the inception of the Schroedinger equation, it presumably was taken for granted that his phase-randomized ensemble of elements could not possibly have any bearing on a physical understanding of an equation, which saw the light of day more than a decade later. It is a fact that even the ensemble proponents still appear to be blinded by the nonclassical mystique of the new quantum mechanics of 1925-1926.

Along with the Copenhageners, the ensemble people presumably also feel that a Schroedinger-type ensemble could not possibly have been anticipated by Planck in 1912. Yet perhaps Planck did just that! At least here was, and still is today, an option that should have been thoroughly investigated prior to launching into a long string of nonclassical conceptualizations. What has happened, however, is that this option has been tacitly pushed aside as inconvenient by Copenhageners and ensemble people both, without any explicit or obvious justification whatsoever.

What is the upshot of this comparison between absolute and relative indeterminism? At first it seems rather unscientific to rule out relative indeterminism and then replace it with an absolute indeterminism with its host of nonclassical conceptualizations. Even under Copenhagen domination, the option of an ensemble-based origin of quantum mechanical indeterminism should have had an opportunity to defend itself against the indiscriminate onslaught of the absolute, all-pervading, indeterminism that has ruled supreme with near-totalitarian inflexibility for so long.

In the heat of these discussions, it is necessary to keep in mind that relative indeterminism means restoring determinism to its proper place in physics. The tools permitting to accomplish such an assignment are discussed in section 6.

4. The Position of Determinism

Assuming we do away with the absolute rule of indeterminism, then where do we go from there? Presumably, it should be necessary to readdress the question as to whether any role is left to determinism, which, in turn, means restoring a measure of causality to physics. Pending problems, precipitated by an all-encompassing absolute indeterminism, now can be resolved. It means we now dare to ask questions which were forbidden before. For a long time, we have been intimidated not to ask questions that were said not to be meaningful. Here are some questions that testify to an ignorance of the iron rules of acceptable manners in "modern" quantum mechanics.

If the Schroedinger equation now is taken to be an instrument describing solely phase-random Planck ensembles, how are we going to describe physical situations that cannot possibly accommodate a feature of phase randomness? Planck had an answer to that question. His quantization rules, similarly as those of Bohr-Sommerfeld, effectively imposed a discreteness of the action integrals of Hamilton-Jacobi dynamics, thus retaining, in the classical sense, a truly deterministic single-system description.

Seen in retrospect, Planck thus injected an ensemble indeterminism by imposing phase randomness between the elements of the ensemble. In two dimensions, phase randomness is an adequate characterization. In three dimensions, mutual-phase comparison invokes also the mutual orientation of the ensemble elements. Hence, in three dimensions, random phase can be accompanied by random orientation.

Let us assume Planck was correct in his inference that zero-point energy is a condition of statistical equilibrium for a phase-and-orientation randomized ensemble. Now, consider that phase randomization gives, according to Planck, a Schroedinger result of $(n+1/2)\hbar\omega$, whereas directional randomization gives, as explained in the Feynman Lectures,[6] another Schroedinger result of $\sqrt{n(n+1)}\hbar$ for the average modulus of angular momentum per element in the ensemble. In witness of this rather unusual coincidence, would it not be fair to conclude, or to at least consider, the possibility that Schroedinger's equation describes perhaps a Planck ensemble?

Yet, neither the Feynman lectures nor the text by Kompaneyets[6] explicitly relate the quantum numbers $(n+1/2)$ and $\sqrt{n(n+1)}$ to an ensemble interpretation. The calculations are presented as potential curiosities.

Any responsible answer would have to give the perfectly classical phase-and-orientation statistics priority over a nonclassical statistics without a universe of discourse, as proposed by the Copenhagen School. Yet, for unexplained, and perhaps unexplainable reasons, even contemporary ensemble proponents have failed to take advantage of this early opportunity offered by Planck to obtain a measure of understanding for the solutions of the Schroedinger equation.

Probing further into this situation, it is necessary to become more explicit about the deterministic tools of quantum mechanics, because the Schroedinger equation now has a restricted realm of applicability. Another reason for probing further has to do with a superstructure of the Schroedinger equation, known as quantum electrodynamics (QED). The conceptual structure of QED is contingent on Copenhagen views, if the Copenhagen view is abandoned, how is that going to affect QED?

5. The Field Tools of Quantization (Chapter VI)

The quantization tools, used during the era preceding the Schroedinger process, were all related to analytical dynamics. The Bohr-Sommerfeld integrals have become regarded as the prototype tools of that era. In keeping with prevailing thought of that time, these tools were considered as quantum superstructures of the deterministic tools of analytic dynamics.

From a point of view of field theory, the Bohr-Sommerfeld integrals share the restrictions of a theory that has been developed around the concept of the point-particle. The Schroedinger process initially got rid of the point-particle abstraction by smearing out its presence. When many particle situations made this position untenable, the Copenhagen interpretation, instead, further consolidated the point-particle abstraction. A postulated vanishing spatial presence of the particle was needed to give the Copenhagen probability interpretation an approximate measure of physical reality. Hence, more than before in the Newtonian context, the point-particle had become a cornerstone in the Copenhagen process. In the Newtonian process, by contrast, the definitions of centers of mass and gravity made the point-particle abstraction at least mathematically permissible. Yet, Newtonian multi-particle situations share three-body complications, also in the case of Bohr-Sommerfeld quantization.

There is, however, a good field analogue of the Bohr-Sommerfeld relations in the form of the one-dimensional Aharonov-Bohm integral.[7] The latter also is known to work well in multiple-particle situations. The superconducting ring is a striking example for a boson accumulation in the same quantum state. Many particles (pairs) may be circulating in the ring, yet they all cooperate to give a single quantized flux state!

There is a two-dimensional analogue of the one-dimensional Aharonov-Bohm integral, which comes straight out of electromagnetic theory. It is the Ampère-Gauss integral,[8] which counts the number of particles circulating in the ring.

The one-dimensional Aharonov-Bohm integral and the two-dimensional Ampère-Gauss integral are spacetime integrals. One counts quanta of flux units, the other counts quanta of electric charge units. These integrals have many remarkable properties. First of all, they are period integrals that can be assessed in the framework of a de Rham[9] cohomology for electromagnetic fields. This means these integrals are spacetime topological invariants. Hence, unlike the general relativistic Dirac equations, these integrals comply in a simple and beautiful manner with Einstein's principle of general spacetime covariance!

There is much more, though. Since these integrals count fundamental units of physics, such as flux and electric charge, their counting should not be affected whether flux and charge are expressed in cgs, MKS, or, if you will, in nonmetric units. Hence, these integrals must be metric-independent, general spacetime invariants. This permits us to draw the following conclusion:

Since the metric is the one and truly only criterion distinguishing between macro- and microphysical situations, metric-independence now becomes an exclusive feature for securing both macro- and micro-applicability.

A third three-dimensional integral is necessary for accomplishing a complete cohomological assessment of electromagnetic field configurations. An integral of this nature has been introduced by R.M. Kiehn.[10] Together with the earlier-mentioned one- and two-dimensional integrals, this three-dimensional integral completes the set of period integrals to assess one-, two-, and three-connectedness of electromagnetic field configurations. The physical function of this three-dimensional integral is that of counting action units of integrated spin-and-angular momentum. Knotting of the integration cycles here discriminates between spin-and-angular momentum action components (chapters VI, VIII, XI, XIII, XIV).

All of these integrals already have been in use in physics in one way or another. In the context of a de Rham cohomology, they appear as an organized tool for establishing connectedness and counting flux charge and action units.[11] These integrals are, in fact, a natural global superstructure of Maxwell theory.

It is in the nature of a field theoretical transition if the "point" features of mechanics are replaced by the distributed connotation of the electromagnetic field. The mechanical connotations of the electromagnetic field, in the sense of a continuum mechanics, already were recognized by Maxwell. The ensuing universal reduction of mechanical characteristics to a common electromagnetic origin has been an ideal, which has enjoyed an off-and-on viability in the wake of Maxwellian theory. In the light of a cohomological assessment it will appear that more fundamental particle structures can be reduced to an electromagnetic origin than hitherto was thought to be possible. The metric-free global superstructure of electromagnetic theory, in the sense of a de Rham cohomology, makes this extension a more feasible proposition than in the past (chapters XIII and XIV).

The deterministic nature of this global superstructure of Maxwell theory needs to be emphasized, because it is a natural extension of deterministic theory in the form of the existence of discrete quanta of electric charge and flux. Nowhere has a statistical element been injected. The three period counting laws are as deterministic as the Bohr-Sommerfeld relations. The only uncertainty to be envisioned here is a counting error of quanta. The fundamental thesis of this discussion holds out for a position that such uncertainties are not *a priori,* but induced by ensemble behavior.

6. QED and Quantum Superstructure (QS)

There is some evidence that the quantum superstructure (QS) of Maxwell theory exhibits an overlap of applicability with traditional quantum electrodynamics (QED). Yet, the latter definitely is indeterministic, whereas the other (QS) is deterministic. Some inquiry is necessary to understand how this can be reconciled.

An example of such an overlap occurs in the case of an evaluation of the anomalous magnetic moment of the electron and muon. Either method can give the observed answer, yet QED requires a subtraction of infinities in the sense of renormalization. The other method of using QS, by contrast, does not invoke a processing of infinities, but demands, instead, a more detailed model of what the electron or muon are supposed to be.

QED operates with the artifact that physical space is permeated with zero-point energy, with each degree of freedom holding an energy $\hbar\omega/2$. Integrated over the free-space spectrum from 0 to ∞, this energy is divergent. Depending on the physical situation at hand, the QED process subtracts out the infinities and can retain finite meaningful end results.

A reasonably transparent example is provided by the van der Waals forces between two parallel plates. Casimir[12] has shown how this force can be calculated as a radiation pressure differential between the outside space and the inside space between the plates. The finite force differential comes about because, between the plates, there is a low-frequency cut-off determined by the distance between the plates.

The infinite forces on either side of the plates defy our normal sense of physical reality. Yet, the finite differential between infinite radiation pressures serves as a useful calculational expedient confirming an observed result. This final result seems perfectly deterministic in nature. However, to obtain it, space is imagined as filled with an infinite zero-point energy. Since the latter earlier was identified as a special manifestation of quantum uncertainty, it reveals the fundamentally indeterministic nature of QED. The subtraction process of infinities thus leads us to believe, or even conclude, that the process of renormalization eliminates the indeterministic features. QED results thus have a chance of being reproduced with the help of QS methods of quantum cohomology (chapter VIII).[13]

Let it be known that the record of achievement of QED is, at this point, much more extensive than that of QS. So the overlap of results is hardly a reason to abandon QED for QS, solely because the latter does not deal with infinities. In cases where QED and QS can be made to agree, there is, however, a remarkable difference of information input. The QED method, which, in principle, rests on the point-particle notion, seems, for all practical purposes, model-independent. The QS method, here earlier referred to as quantum cohomology, QC, demands a specification of topology and metric features of the object under consideration.

The QED feature of model-independence frequently is hailed as a mark of superiority of an abstract approach. Yet, the potential equivalence of model-independent and model-dependent processes provide intriguing perspectives, deserving of more extensive investigations. Eq.33, chapter XI:7, gives the most metric-independent, yet model-dependent example of using QS and QC methods.

7. Determinism and Indeterminism in Schroedinger's Process

If the Schroedinger equation describes an indeterministic Planck ensemble of deterministic single systems, the feature of determinism and indeterminism should be simultaneously present in the Schroedinger process. This statement holds, regardless of whether one is an ensemble supporter, or a supporter of the Copenhagen interpretation.

For further insight into these matters, let us compare some deterministic Bohr-Sommerfeld results with some results of the Schroedinger process. The two methods give exactly the same result for the stationary states of the hydrogen atom. Hence, if the Bohr-Sommerfeld result is deterministic, so is the Schroedinger result. In standard quantum mechanical terminology, the hydrogen states are said to correspond to the diagonal elements of the matrix of quantum states. Their expectation value is one, as expressed by the normalized wave functions.

For the harmonic oscillator, the two results are, however, distinct. The stationary states, according to the Schroedinger equation, are shifted by the zero-point energy with respect to the states obtained with the Bohr-Sommerfeld recipe. Yet, from a Schroedinger point of view, the normalized Hermite polynomials assign an expectation value onto the stationary states $(n+1/2)\hbar\omega$, regardless of whether n=0 or n≠0. Hence, the deterministic Bohr-Sommerfeld states, shifted by the indeterministic zero-point energy, acquire, in the end, a deterministic veneer through the Schroedinger process.

In the Copenhagen vein, this result may become acceptable by considering the energy uncertainty relation $\Delta E \Delta t \geqslant h$. For stationary states $\Delta t \rightarrow \infty$ hence, $\Delta E \rightarrow 0$.

The upshot of this preliminary inquiry shows how stationary states appear to be deterministic, regardless of whether they are looked at from a Bohr-Sommerfeld or from a Schroedinger point of view. The true nature of indeterministic features must be expected to manifest themselves with the transition states.

The Bohr-Sommerfeld recipe has no precise quantitative predictions about transitions, but the Schroedinger process does. The off-diagonal elements of the state matrix are measures for the occurrence of transitions. A transition is forbidden if the corresponding element is zero. It was this more encompassing feature of transitional selection that caused the Schroedinger process to win out over the Bohr-Sommerfeld description. The latter was merely restricted to stationary states; there were some *ad hoc* procedures, though, for anticipating transitions. By contrast, the Schroedinger-Heisenberg state matrix is able to give precise information.

If the Schroedinger process truly is ensemble-based, transitions would be expected to have an ensemble aspect, combined with an individual disposition of the element in question, to make a transition. For isolated single systems there is at this time no QC theory of transitions. Of course, the Copenhagen supporters will tell us that the Schroedinger equation has all the answers we are looking for. Here is the strongest practical argument of support yet for the Copenhagen point of view. Indeed the Schroedinger equation does it, but without giving us physical insight as to why it can do it. It certainly was not designed to do so; it happened to be able to do so!

Even if presently there is no complete theory of transitions for isolated systems, a few words are in order regarding how transition events differ topologically from stationary states. Stationary state particles, atoms or molecules are represented by worldlines or rather world-tubes with cross section structure, all governed by the earlier-mentioned period integrals. The three-dimensional integral, however, needs a closure in spacetime. The only way of having closure for an indefinitely extending world-tube is by having a periodic structure on the world-tube. Elementary structures have at least one periodicity. Compound structures can have many more. The here-cited feature of worldline periodicity relates to the topological concept of Poincaré torsion.

A transition event in spacetime is different from a world-tube in that it has always a very distinct beginning and end in the time direction. Events are isolated spacetime domains from which world-tubes emanate, after other world-tubes have terminated there, so as to cause the event to occur. Spontaneous events terminate a single world-tube. Induced events are collision nodes in spacetime, they terminate more than one world-tube.

The mentioned characteristics of world-tubes and events are the topological characteristics of Feynman diagrams. The question is whether the difference between event-domain and world-tube can be successfully described by period integrals. The transition-event should be characterized by an integral that is guaranteed to vanish when it is applied to a world-tube configuration; otherwise the physical object characterized by the world-tube would be unstable and cease to exist. There are significant mathematical perspectives indicating that meaningful distinctions can be made to indeed convey such a message. The key to this criterion is that the integration domains for events are simply connected, whereas world-tubes are product domains, so that the corresponding integrals become product integrals.

The mentioned topics have, in part, been explored (chapter XIII) and are, in part, suggestions for further inquiry. There is presently no precise idea how single system instability and ensemble interaction work together to bring about the observed transition propensities. The facts is, the Schroedinger equation yields transition propensityin nearly full automatic, without requiring any detailed understanding of the transition mechanism.

It is most likely the ensemble's zero-point energy, which triggers the "spontaneous" transition in the single-systems that are susceptible to such inputs. Whatever the true mechanisms may be, all its results are rather magically described by the Schroedinger equation. Yet, amazingly, this equation was never constructed to do all this. It just happened to be able to render those services as a gift from heaven.

The worst the Schroedinger equation ever did to physics was to raise the expectations so high that it was thought to be able to cure problems far beyond its realm of applicability. The attempts at justifying these extraterritorial excursions were bound to invite a wave of absolute indeterminism for washing out the unavoidably ensuing conceptual discrepancies.

8. The Method of Path Integrals

Throughout these discussions, the method of period integration has been characterized as a method designed for the treatment of deterministic situations, with the enjoining recommendation of restricting the process of Schroedinger to indeterministic ensembles. In recent times, the method of path integrals has developed into a major instrument for the purpose of solving problems in quantum mechanics. Since path integrals and period integrals both testify to a measure of global processing, it raises questions whether path integrals have a period integral status. Contingent on this inquiry, it may be possible to decide whether path integral methods deal with the deterministic or the indeterministic aspects of quanta.

A monograph on the subject of path integrals by Feynman and Hibbs[14] answers the last part of this inquiry. In the course of their discussion, these authors show, by example, how the path integral method leads to the Schroedinger approach. From the point of view taken in this endeavor of quantum reprogramming, the Schroedinger approach has been classified as pertaining to indeterministics ensembles. It then follows from the apparent equivalence of path integral and Schroedinger approaches, as proven by Feynman and Hibbs, that the path-integral method needs to be categorized as indeterministic in nature. The latter fact then excludes a direct physically consequential relation between path- and period- integrals, except the asymptotic relationship sketched in chapter VI;7.

Seen in the perspective of chapter VI;7, the Feynman-Hibbs process may be viewed as a quantum mechanical adaptation of Hilbert's integral equation formulation of eigenvalue problems. An integral equation so becomes the equivalent of a differential equation with boundary condition, while the extremum properties of eigenvalues are found to tie in with a corresponding variational process. An account of these structural interrelations have been superbly highlighted from algebraic as well as analytic angles in a classical text by Courant-Hilbert.[15]

Seen from the angle of Hilbert's integral equations, the path-integral process is close to a mathematical reformulation of Schroedinger's eigenvalue process; yet it goes a step further in that also the time-dependent Schroedinger equation is obtained from path-integral procedures. The path integral has the familiar reflexive feature of a mixed local-global process by relating the wave function in a given point to its value anywhere else or, in Feynman's own words, "as a sum over a history of states."

A purely mathematical recasting of a given physical situation cannot be expected to have a necessarily incisive effect on its physical interpretation. Yet, reading the Feynman-Hibbs account suggests that Feynman's characteristic portrayal of physical phenomena must have been a principal motivator for the path-integral process, because any reference to the Hilbert alternative of integral equation versus differential equation with boundary conditions is absent in the Feynman-Hibbs monograph.[14]

The path-integral process may thus, in principle, be assumed to remain neutral in the interpretation issue of ensemble versus single system. Yet once we come to think of it, the path-integral, which, in Feynman's words, is a "sum over a history of states" seems conceptually closer to the mixed local-global connotation of an ensemble of identical systems than to a strictly global description of a single-system constituent.

9. Where Does Physics Go from Here?

While the familiar contrast expressed by the words *classical* versus *nonclassical* has been very productive in creating new horizons of mystery in physics, by the same token, too much new mystery can be counterproductive by presenting a predicament of choice. It also awakens a taste for conceptual extremism. Life in a state of self-imposed spiritual imprisonment tends to seek solace in the realm of magic.

After rescue from the confinement of universal quantum uncertainty, and after having gained spiritual independence from the dogma of an indiscriminate universal Lorentz invariance, where does physics go from here? The answer is: just about anywhere! To get our bearings in this newly-created navigational environment, let us retrace some steps that have been instrumental in questioning existing dogma.

While Schroedinger's transcription leading to his wave equation had a single-system connotation, in the course of time other transcriptions emerged that favored an ensemble background for the Schroedinger equation. Work of this nature was initiated by Fuerth, Fenyes, Bohm-Vigier and Weizel, pointing out relations with diffusion and the equations of continuum mechanics. An over-enthusiastic overview of these matters by Nelson[16] makes it appear as if all these nonclassical escapades would have been unnecessary. Nelson cuts short all those detours of the human mind occurring in the process of exploring new territory. He injects a zero-point diffusion potential in classical equations, and behold, all of nonclassical quantum mechanics emerges.

While Nelson's account has the virtue of reducing matters to the mathematical essentials, a better physical perspective on the physical nature of this zero-point potential is needed. Some precious insights to this effect have been added in a series of papers by Boyer.[17] Where Einstein's famous derivation of Planck's distribution law leaves out the zero-point energy, Boyer shows that the Planck distribution, plus zero-point spectrum, is a solution of a modified fluctuation equation, originally used by Einstein and Hopf to derive the Raleigh-Jeans law. Since the latter was recognized as unsatisfactory, Einstein was led to his famous derivation of Planck's law, using his A and B coefficients. This result did not accommodate the zero-point spectrum.

Boyer's revision of Einstein's derivation with the zero-point contribution may have been motivated by QED's compelling requirements of lending operational physical significance to the zero-point infinities of vacuum.

In the previous sections it has been shown, however, that there are finite alternatives to some "infinite" QED processes. In the light of these facts, we therefore choose the option of siding with Einstein, with regret for not using Boyer's ingenious derivation. This decision is here based on the desirability of avoiding an infinite vacuum energy that cannot be tapped.

From Copenhagen extremism of having everything discrete, to Boyer's[15] valiant work of reconciling Copenhagen with notions of continuity, Kiehn's[10] thesis that quanta are periods on physical manifolds emerges as a constructive synthesis of this earlier extremism. Yet, from de Rham's theorem that closed forms can be constructed having assigned periods, to the physical fact that those periods are integral multiples of given basis periods (corresponding to the universal constants of nature) is a reduction feature that transcends de Rham's theorem.

In his further attempt at substantiating this physically indicated reduction to basis periods, Kiehn[10] calls on arguments relating to Brouwer's mapping theorem. Here I like to accept this reduction to basis periods as a feature of nature's clearly demonstrated option for counting identical quanta. Period reduction to multiples of universal constants stresses period counting, which, in turn, ties in with the generally invariant metric-independent aspects required for the counting tools.

By our earlier siding with Einstein, quantum uncertainty has become a purely environmental contingency, which is exclusively associated with matter. Anybody who has ever been assigned to coordinate the behavior of a plurality of comparable material items (say in an automated mass production scheme) will appreciate a measure of universal relevance in the idea that uncertainty is exclusively an environmentally induced phenomenon. We decline here to incorporate the non-material free-space of vacuum in that environment, because doing so yields intractable infinities.

Carrying this idea to the extreme, it would imply that an excited atom, isolated from the rest of the universe, would arbitrarily reside in its state of excitation, because there is no environment to initiate a transition. The idea seems akin to the Wheeler-Feynman absorber theory of radiation. It claims that a radiation transition can take place only if there is an absorber recipient. This idea is relevant also to an ensemble-based derivation of the thermal statistics of Bose-Einstein (B-S) and Fermi-Dirac (F-D) by Tersoff and Bayer.[18] For Maxwell-Boltzmann statistics, energy transitions are presumed to depend only on the emitter. The B-E and F-D statistics, by contrast, depend over and above on the availability of final states.

In making a comparison with standard derivations, let us be reminded that such derivations of B-E and F-D statistics require the mystical counting artifact of reduced particle individuality. ***Copenhagen's need for reduced particle individuality stands to reason in the light of Copenhagen's silent obliteration of the distinction between ensemble and the single systems of which it consists.***

Let us accordingly conclude that there are not two major quantum statistics; there are three, each with their own well-defined and distinct universe of discourse.

(1) A **Fermi-Dirac** statistics, where every state is exclusive.

(2) A **Bose-Einstein** statistics, where states are not exclusive; objects can accumulate in the same state.

(3) A **Schroedinger-Planck** statistics, which is nonthermal. It relates to phase and orientation disorder prior to (1) and (2).

Number (3) is the fateful nonclassical statistics of Copenhagen. It has now been made classical by virtue of the identification of its universe of discourse in the form of phase-and-orientation data of the plurality of ensemble constituents. Einstein's objection "God does not play dice" was in opposition against a statistics pertaining to a single object.

It has been argued that Einstein changed his conviction about the role of statistics in physics as a result of the 1925 quantum revolution. Einstein had great expertise in statistical matters. His fluctuation theory (Brownian motion) and his derivation of Planck's law of radiation on the basis of the concepts of spontaneous and induced emission are still today cornerstones of that discipline. It seems unlikely that he would have distanced himself from these earlier achievements with his pronouncement: "God does not play dice." We may well assume that this expression of Einstein's feelings was intended to target a single-system statistics, which was taken to exist in Nature, *a priori*!

This message indicates that some existing extremist trends in statistics can be brought to a synthesis by finding a universe of discourse. Without that universe of discourse, physics is forced to operate on a set of religious ground rules, which invite over-abstract procedures. Contemporary theory testifies to this development. The ensuing reluctance in making visual images in the micro-domain reinforced a belief in point-particles.

The reluctance of endowing point-particles with a domain of spatial presence thus led to the complementary act of endowing surrounding space with the calculation expedients of energy infinities. QED normalization became the method of processing these infinities to obtain finite and physically meaningful answers. Viewing these results shows that the power of religious-type abstractions in physics are not to be underestimated as an important and viable means. Yet, once established, religious conventions are also hard to shake.

The tenacity of religious convictions in physics is vividly illustrated by the enduring thesis of magnetic charge. Experiments have given a resounding "NO" in response to its hypothesized existence. Yet the theorists are not convinced; they have not even been swayed by the magnetic monopole's incompatibility with the law of Aharonov-Bohm and the latter's many relevant applications in physics. For over half a century, differential topology has been able to provide proof of the incompatibility of the magnetic monopole hypothesis and the law of Aharonov-Bohm. Ironically, this A-B law is, even today, viewed with greater suspicion than the imagined magnetic monopole, despite experimental proof to the contrary!

To the extent that topology-related arguments have been able to play a role in this controversy, they have been received with commentary questioning such thinking, because it does not fit the contemporary mold of a discrete world of physics. There are extremists in the Copenhagen School, who no longer accept the existence of a continuous and differentiable spacetime as a physical reality. An appeal to differential topology and theorems of de Rham is accordingly sidetracked and the search for monopoles continues.

These religious undertones of modern quantum mechanics cannot avoid a multidenominational diversity. We have to live with that diversity, if we want to have a platform from which further realistic decisions can be made. Fear of diversity in scientific opinion easily leads to domination by extremists guarding their brand of faith. Of course, proliferation of diversity also can adversely affect the quality of science. It is a chance that needs to be taken. There is little virtue in dictating a single version of truth. Sometimes, even ill-advised courses of action yield useful results, despite the fact that they were initiated for the wrong reasons. Let us admit that serendipity as been a major factor, especially in the development of quantum mechanics.

Einstein was keenly aware of this diversity of motivations driving his fellow physicists.[19] In a tribute to Planck, he gave us rare insight into the humanity of his times (chapter XV). He portrays Planck as chosen by God's angel and as a colleague loved for his sincerity of motivation.

What better testimony do we need that intellectual prowess alone can not bring the grace of bliss, unless it comes with the matching wisdom that goes with a pure heart.

10. Measurement Theory

A statistical theory of measurement was initiated by Gauss for the purpose of developing well-defined ideas of errors and limits of precision in the art of surveying the surface of the earth. The techniques developed by Gauss subsequently found a fertile domain of application in physics in general. A new element entered the picture with the fateful distinction made by the Copenhagen interpretation between classical and nonclassical statistics. The so-called quantum statistics of Bose-Einstein and Fermi-Dirac, though, still formally belong in the classical realm, because they have reasonably well-defined universes of discourse. They, after all, emerged prior to the nonclassical ψ function statistics of Copenhagen.

The recognition of a quantum theory of measurement, as something distinct from standard measurement theory, emerged for the first time with the acceptance of the Copenhagen interpretation and its presumed nonclassical connotation of the ψ function statistics. On the other hand, if, in the spirit of an ensemble interpretation, a phase-and-orientation-based universe of discourse is recognized as relevant to the ψ function statistics of the Schroedinger equation, then the idea of a separate quantum theory of measurement becomes somewhat of a nonissue. Only typical quantum features have to be recognized in the statistical counting. The Bose-Einstein and the Fermi-Dirac laws testify to such needed modifications. In the previous section, it was already recognized that those two statistical laws needed to be supplemented with a Schroedinger-Planck type statistics accounting for the feature of element orientation and element phase in the ensemble.

Yet, not recognizing the viability of a Schroedinger-Planck statistics, there is no end to the chain of arguments that can be brought to bear by carrying the Copenhagen single-system extrapolation *ad absurdum*. They are all predicated by giving the Schroedinger equation a bigger-than-life absolute stature of perfection, as behooving any gift from heaven. Seen in this perspective, it becomes understandable why Schroedinger was uncomfortable with the magic power assigned to his equation by the followers of the Copenhagen School. A famous example of absurdity has been provided by Schroedinger himself, yet his tongue-in-cheek wit may have escaped some followers.

Assuming, solely for the sake of argument, that the Schroedinger wave equation can argue the (quantum) state of a single cat being dead or alive, one encounters troubles with the notion of probability. The poor housepet might be pronounced to be half-dead or half-alive. Most of us are used to thinking of dead and alive as a purely binary state. It is either one or the other; no civil administration or hospital would let us get away with decision-making based on the idea that a person is only a little bit dead or a little bit alive. The same holds for a cat.

Now, going a little step further by considering, instead of one cat, an ensemble of cats, the probability of finding a cat alive or dead makes perfect sense. Schroedinger's "cat" could be taken as one of the compelling arguments in support of an ensemble interpretation, if we were willing to accept that Schroedinger's equation would indeed be applicable to the "quantum state" of life and death. We may safely assume that Schroedinger would hardly have been inclined to so extend the realm of applicability of his equation, yet we can't be quite so sure whether some Copenhageners might not go a step further than Schroedinger.

If our memory serves us well, there have been instances where people have considered a wave function for the whole world, or even the universe. If the whole universe has a wave function, why not a cat, or a human being? John S. Bell never was at ease with those ideas; he felt that kind of talk was blasphemous.

Much standard lore of quantum measurement theory has, of course, started with the notion of single-system uncertainty. The latter can be made plausible by noting that no single (atomic) particle can be individually observed without substantially disturbing its position and momentum. Yet the fact that such disturbances are of the order as inferred from Schroedinger's equation does not justify a summary identification of the two. The nonclassical label becomes here an easy, and therefore tempting opportunity in support of the single-system extrapolation. Once there is a mind-set to confirm a preconceived situation, an amazing amount of material can be brought to bear to substantiate the chosen preconceptions.

A more educational perspective, though, is obtained by realizing that modes of measurements exist that completely circumvent the uncertainty rationale. The Schroedinger-Planck statistics, responsible for the uncertainty phenomena, is absent in the wholly ordered situations of the Josephson AC effect and the quantum Hall effect. They give the most reproducible measurements of the $\hbar$ and e quanta available today.

In both cases, a dynamic-ordered situation prevails, which is continuously observed. One would be hard put indeed to explain how these measurements disturb the dynamics of the situation that is being scrutinized. In the Josephson case, a potential and frequency are measured with great precision. The potential is required to bring about the oscillation between two distinct quantum states. In the quantum Hall effect, the ratio of transverse potential and forward current are measured. Their magnitudes may vary, yet their ratio is constant in the plateau domains. The observed Hall impedance is characteristic of the cyclotron dynamics. That dynamics is not affected whether the lattice of cyclotron states travels faster or slower, within the chosen plateau range. In fact, in both cases the dynamics is ironically the same, whether or not measurements are made.

These new macroscopic quantum effects show up the deficiencies in the old lore of quantum reasoning. They are precipitating needed changes in the metaphors of understanding. The major change argued in this chapter is a transition from Copenhagen's absolute *a priori* single-system uncertainty to an ensemble-based relative uncertainty. Now in retrospect, it would have been possible to make this long story short, if, right from the start, we had considered that an absolute single-system uncertainty somehow does not sit well with the special theory of relativity.

What is the frame of reference with respect to which an absolute uncertainty can be established? Such reference must itself have an absolute character to secure the possibility of establishing an absolute uncertainty. The existence of a reference of this kind, however, militates against the principles of relativity. By contrast, the notion of an ensemble-based uncertainty elegantly circumvents these absolute frame predicaments, because the ensemble uncertainty does not need an absolute reference. The ensemble uncertainty is a mutual uncertainty between ensemble constituents. Hence, the crutch of an absolute reference is not needed and can, therefore, be avoided. This compatibility argument has no doubt floated around in the minds of many. If it did not make the grade, it merely proves to what extent the prevailing physics consensus has given up reconciling relativity and quantum mechanics.

CHAPTER XVIII

THE DIFFEO-4 MANDATE OF MICHELSON-SAGNAC

1. Objective and Synopsis

The first impression of a discussion of the Michelson-Morley and Sagnac experiments in the context of a reprogramming of quantum mechanics is bound to raise questions about the conceptual connection of these topics. The answer is simply that the principle of general covariance, when taken in a global context, ties them together. In this chapter, it is shown how an old dichotomy can be removed by treating both experiments from a global point of view. This global spacetime treatment cannot be well accomplished without honoring the principle of general covariance. Since the same principle has also enabled us to delineate a two-tier view of quantum mechanics (chapters XV; XVII), the conceptual relation between these topics may now no longer be as far-fetched as they initially appeared.

The Michelson-Morley null-experiments historically led to the discovery of the Lorentz transformations, because they, and not the Galilei translations, could account for the observed null-results. Subsequently, Sagnac's experiment showed, however, how Galilei rotations could account for a nonzero observation for rotating interferometers.

A global approach to interferometer performance, using cyclic integration of ray paths, permits a common analytical treatment of Michelson-Morley and Sagnac-type interferometers. This step from local to global procedures leads to the surprising result that both experiments can be accounted for by general Galilei transformations, which, in the spirit of Chasles' theorem, cover translations and rotations both.

Essential for obtaining this joint result is the choice of a global procedure taking the place of *experimenting* with local options of velocity addition. Far from sounding a retreat from relativity, this joint treatment gives general covariance priority over Lorentz covariant formulations. Two classical experiments show how Diffeo-4, as used in the general theory of relativity, takes precedent over L(4). The special theory's restriction to uniform translations, in conjunction with an overemphasis on a local conceptualization of velocity addition, have stood in the way of a common treatment of the two experiments.

Conclusion: The Michelson-Morley experiment may not be taken as a compelling and unique path to the Lorentz group, yet Michelson and Sagnac's experiments together give joint evidence, making Diffeo-4 quite compelling.

2. Introduction

The general policy of contemporary physics vis-à-vis its principles of invariance has been dominated by a contradictory thesis. On the one hand, Lorentz invariance has been taken to rule supreme in physics; Dirac equations and quantum electrodynamics are believed to have, among others, provided ample evidence to support such a position. On the other hand, there is this remaining, half silent lipservice to an ill-formulated Kretschmann-Bridgman doctrine (chapter XV;3), which claims (on incomplete epistemological grounds) that a principle of general covariance cannot be expected to have compelling physical implications.

How do we resolve this obvious contradiction? If the Lorentz group holds a major position in the Diffeo-4 hierarchy of invariances, as indeed it does, one ought to be very reluctant in declaring the Diffeo-4 requirement as void of any necessary physical implications. The word earlier mentioned, as holding an answer to this predicament, is the *hierarchy* concept of invariances. This *hierarchy* feature has been traditionally underemphasized in the literature, a circumstance that has led to simplifying, all-encompassing, blanket statements such as: all laws of physics have to be Lorentz invariant, or Einstein's presumed more radical position; all laws of nature have to be invariant under general spacetime substitutions: Diffeo-4. Instead, it is the *Principle of Invariance Hierarchy*, discussed in chapter XIV;4, which can restore some logic order in statements that otherwise appear as dogmatically imposed requirements.

Kretschmann and Bridgman had already sounded a retreat from a blanket imposition of Diffeo-4 invariance. In fact, it wnt from blanket imposition to blanket retreat. Continuing in their vein of criticism, one could equally infer that in some cases a blanket imposition of Lorentz invariance does not make sense either. What purpose would be served by insisting on an explicit Lorentz invariant rendition, say, of the very fundamental laws of crystal symmetries?

The fact that all crystal symmetries are subgroups of the rotation group O(3) obviates the need for wider invariance. The process, as is, could be said to meet Lorentz invariance, by virtue of rotations being a subgroup of the Lorentz group. However, if spatial crystal symmetries were to be extended by time reversals, O(3) would no longer be adequate. Seen in this perspective, the Kretschmann-Bridgman criticism thus becomes a premature rejection ensuing from a subjective restriction of physical perspective on the part of its authors. Avoiding such subjectivism, the physical implications of the hierarchy of transformation groups should be given a needed attention.

Diagram **II** of Diffeo-4 decompositions shows two disjoint sequences of subgroups that have no mutual intersections, except the unit element. Yet, all these subgroups hold important positions in contemporary physics. Their property of being subgroups of Diffeo-4 is their most striking common characteristic.

DIAGRAM II : SUBGROUPS OF DIFFEO-4

C(4)	← **Diffeo-4** →	G_d(3,1
Conformal group of translations, orientation- and scale changes.		General Galilei group of accelerated motion translation,dynamic rotation.
↓		↓
L(4)		G_u(3,1)
Lorentz group of static rotations and uniform translations.		Galilei group of uniform translations.

This places Diffeo-4 in a unique position for acquiring a common perspective on procedures of contemporary physics. Continued adherence to the Kretschmann-Bridgman suggestion of pragmatically restricting the mathematical ambition for creating invariant descriptions would unfortunately bereave us of such a perspective.

3. The Michelson and Sagnac Experiments

In the last part of the Nineteenth Century, Michelson started doing experiments to establish the possible existence of an "aether" wind. He used several interferometer designs. In some, the beam path looped around a finite surface area; in others the beam path folded on itself going back and forth without enclosing a net surface area to speak of. Later designs, in cooperation with Morley, led to multiple-beam traversals for the purpose of enhancing the sensitivity of the device.

The outcome of these experiments led to smaller and smaller values for the aether wind that could not have escaped observation. Since no clearly measurable fringe shifts could be observed, even while the threshold sensitivity was well below the circumferential or even the orbital velocities of the earth, the aether wind was taken to be zero.

The expected fringe shifts had been calculated, based on a premise of local Galilean additivity of velocities $c \pm v$, with one of the components equalling the light velocity c. Since the experiments contradicted this premise, Lorentz first formally resolved the predicament by introducing a new kinematics, which was to replace the old Galilean kinematics, at least in electromagnetic situations. Physics thus became endowed with two types of kinematics: Galilei kinematics in mechanics, and a Lorentz kinematics in electromagnetism.

This dichotomous situation was subsequently resolved by Einstein by also bringing mechanics in line with electromagnetics. Newtonian mechanics thus became replaced with what is now normally referred to as *"relativistic mechanics."* It is a mechanics, covering uniform and nonuniform motion, as viewed from an inertial frame of reference. By 1920 the observable physical consequences of this mechanics (*e.g.*, the mass-energy theorem and the hydrogen fine structure), had helped to consolidate its relevance. Einstein's bold initiative literally had rescued the conceptual unity of physics.

However, between 1910 and 1920, something happened to disturb the peace of mind of a sizable number of people. Sagnac[1] made an interferomer with a beam loop enclosing a finite area, such as had been used earlier by Michelson. Yet, instead of making observations on a slowly rotating and orbiting earth, Sagnac placed the interferometer on a much faster moving turntable. This time there was no longer a null-result. The experiments showed a fringe shift proportional to the rotation rate of the turntable and proportional to the surface area enclosed by the beam path.

In earlier experiments, Michelson had no observable result, which was presumably due to the rate of rotation of the earth as being too small to register for the experimental sensitivity of that time. The orientation of the device with respect to direction of rotation also played a role. Sagnac's turntable thus saved the day for Michelson's earlier null-results. Later, in cooperation with Gale, Michelson showed how also the rotation of the earth could be measured by such an interferometric device, provided the area enclosed by the beam path is big enough. Today, small size ring-laser versions of the Sagnac interferometer easily measure the earth rate of rotation. These ring-laser devices compete with mechanical gyros and they have become powerful aids in inertial navigation.

At this point, the burning question is: How did Sagnac calculate the fringe shift observed with his interferometer? The answer: He used the Galilean addition theorem $c \pm v$, which had just been outlawed by Lorentz and Einstein. As is to be expected, the outcome of the Sagnac experiment caused a measure of upheaval, which was ultimately resolved by Einstein and von Laue by stressing the uniqueness of the **uniform** motion covered by the Lorentz group.

The upshot of these deliberations may be summarized as follows: It is impossible to detect, by intrinsic means, an absolute state of uniform translation, yet a state of absolute rotation is detectable and that is exactly what the Sagnac experiment does. *The position of the Lorentz transformation now became clearly delineated as a tool to be used solely when interrelating the family of inertial frames.* Hence, strictly speaking, the Lorentz group is not applicable to the Michelson-Morley experiment; it is, at best, asymptotically acceptable.

Yet, an asymptotic validity does not quite suffice for resolving a matter of fundamental principle. This marginal logical consistency still causes many to retain lingering doubts as to why the Galilean addition of velocities (which is invalid for uniform translation) suddenly needs reinstatement for rotation, or as soon as there is a deviation from uniform motion. This *ad hoc* change in the rules of velocity addition seemed a beauty defect of the special theory of relativity. The next section shows how a global Diffeo-4 assessment of the interferometric function removes this conceptual discontinuity between uniform and nonuniform motion.

4. A Global View of Interferometric Path-Length Changes

Since Michelson and Sagnac interferometers are used to determine the absence or presence of states of motion of the interferometer as a whole, it must be considered as essential that the instrument be regarded as a rigid body. The condition is obvious, because, if this requirement were not met, one would measure all sort of fringe shifts (or frequency shifts for ring lasers) due to deformations of the equipment itself. Since forces of acceleration can bring about path-length changes and corresponding fringe shifts or beat frequency readings, a near-perfect rigidity of the equipment is a *sine qua non* for making observations about the presumed aether-wind.

An epistemologically reduced role for the Lorentz transformation now is almost unavoidable, because rigidity under the Lorentz group does not really exist. All of this is indicative that concepts such as Lorentz contraction are to be considered as apparent, not as real, manifestations.

A global assessment of interferometric behavior is, in essence, an investigation of the interrelation of changes in the time and spatial components of the spacetime phase integral taken between the spacetime points of comparison 1 and 2

$$\phi = \int_1^2 k_\nu dq^\nu \; ; \; (\nu = 0,1,2,3), \qquad 1$$

in which $k_\nu = (\omega, k)$ is the frequency wave vector and dq^ν the spacetime path element of integration. Tensor notation is used for later transition to standard physical observables.

In the Michelson-Sagnac case we need to evaluate the changes in the components of this integral due to the changes induced by a one-parameter spacetime group

$$dq^{\nu} = v^{\nu} d\tau \, . \qquad 2$$

Operating with this group on the integral of Eq.1 gives

$$\delta\phi = \int_1^2 \delta k_\nu dq^\nu + \underset{v}{\pounds} \int_1^2 k^\nu dq_\nu \delta\tau , \qquad 3$$

which is the sum of two integrals. The first of these integrals gives the changes in the integrand and the second gives the effect of changes in the path of integration as a result of the group operation. The change in integration domain is here accounted for as a change in the integrand by using the so-called "Lie derivative $\underset{v}{\pounds}$" with respect to the generating vector v of the group Eq.2 operating on the one-form defined by k_ν. In an explicit tensorial rendition the Lie operation becomes:[2]

$$\underset{v}{\pounds} k_\nu = v^\sigma (\partial_\sigma k_\nu - \partial_\nu k_\sigma) + \partial_\nu (k_\sigma v^\sigma) \qquad 4$$

Since the light ray obeys the Hamilton equations of motion, the first term in Eq. 4 vanishes. This "equation of motion" for light rays is also known as the Sommerfeld-Runge law[3]

$$(\partial_\sigma k_\nu - \partial_\nu k_\sigma) \rightarrow \mathrm{curl}\, \mathbf{k} = 0, \qquad 5$$

it is the light-ray analogue of the London superconductivity requirement curl**p**=0, and also used by Einstein in proving the period properties of the Bohr-Sommerfeld conditions (Eq.10, chapter VI); it means the one-form k is closed, though not exact. Substitution of Eqs.4,5 in Eq.3 gives

$$\delta\phi = \int_1^2 \delta k_\nu dq^\nu + (k_\sigma \delta q^\sigma)_2 - (k_\sigma \delta q^\sigma)_1 \; , \qquad 6$$

which is in a form to be transcribed in terms of its time and space components ω and $\mathbf{k}$.

For a continuous transition between uniform and accelerated motion, the Galilean option is needed for the group defined by Eq.2; shown in diagram II as $G_u(3,1) \rightarrow G_d(3,1)$. It means $d\tau = dt$ so that $v^\sigma = (1;\mathbf{v})$. Using Eq.2 the spatial components of the integrated part of Eq.6 are hereto rewritten as an integral with respect to group parameter $\tau \rightarrow t$ integrated over the period T of light circulation in the loop:

$$\delta\phi = \delta\omega\, T + \oint \delta\mathbf{k}\cdot d\mathbf{r} + \omega\delta T + \int_0^T \mathbf{k}\cdot\mathbf{v}\, dt. \qquad 7$$

The time integral over the period T can be converted in a spatial loop integral, because the spatial line element ds=cdt, with c the vacuum light velocity. One thus obtains

$$\int_0^T \mathbf{k}\cdot\mathbf{v}dt = \frac{k}{c}\oint \mathbf{v}\cdot d\mathbf{r}. \qquad 8$$

In the context of the interferometer, the modulus $k=|\mathbf{k}|$ of the spatial vector **k** is a constant, and so is c, which means they can be taken out of the integral. Since **k** and **dr** are, by definition, in the same spatial direction, the scalar multiplication $\mathbf{v}\cdot d\mathbf{r}$ can replace $\mathbf{k}\cdot\mathbf{v}$.

Eq. 8 can now be written in the form

$$\delta\phi = \delta\omega T + \oint \delta\mathbf{k}\cdot d\mathbf{r} + \omega\delta T + \frac{k}{c}\oint \mathbf{v}\cdot d\mathbf{r}, \qquad 9$$

which finally lends itself to physical interpretation.

Assuming the motion-induced changes in interferometric conditions to be adiabatic in nature, $\delta\phi = 0$ may be taken as a spacetime invariant criterion of adiabaticity. Since the Michelson- and Sagnac-type interferometers operate with a fixed frequency monochromatic light source, one further assumes $\delta\omega = 0$. There is no change in the time of circulation T in a closed loop of the undisturbed interferometer; $\delta T = 0$. The integral $2\pi\oint \mathbf{k}\cdot d\mathbf{r}$ defines the number of nodes Z in the closed loop light path, so that the fringe shift per single loop is $\delta Z = 2\pi\oint \delta\mathbf{k}\cdot d\mathbf{r}$. Substitution in Eq.9 gives, after using the free-space wave length $\lambda_0 = 2\pi/|k|$, the fringe shift expression per loop:

$$|\delta Z| = (c\lambda_0)^{-1}\oint \mathbf{v}\cdot d\mathbf{r}. \qquad 10$$

The corresponding frequency shift $\delta\omega$ in the ring-laser mode of operation follows from the condition $\delta Z = 0$. The result equals

$$|\delta\omega| = \oint \mathbf{v}\cdot d\mathbf{r} \,/\, c\oint ds, \qquad 10a$$

and obtains directly from the adiabatic type relation $-\delta Z/Z = \delta\omega/\omega$. The integral $\oint ds$ is the path length of the loop.

Mindful of the earlier-stated rigidity requirement, it is now useful to consider a kinematic theorem due to Chasles,[4] which says that any arbitrary rigid-body motion can, at any moment, be decomposed into a superposition of a translation and a rotation:

$$\mathbf{v} = \mathbf{u} + \mathbf{r}\times\boldsymbol{\Omega}. \qquad 11$$

In Eq.11, the symbol $\mathbf{v}$ represents the general motion of the rigid body, **u** is the purely translational component, **r** is the radius vector of the point where $\mathbf{v}$ is locally observed, and **Ω** is the vector rate of rigid body rotation. Since the body is taken to be in rigid motion, **Ω** is defined as a constant for the complete spatial realm of the interferometer under consideration. Since curl**u**=0, a little algebra yields the familiar relation:

$$\mathrm{curl}\,\mathbf{v} = 2\mathbf{\Omega}. \qquad 12$$

The essence of the transition from Eq.11 to Eq.12 rests in the fact that Eq.12 lifts the global information out of the local Eq.11.

Applying Stokes' theorem to Eq.10, and using Eq.12, one obtains

$$\delta Z = 2(c\lambda_0)^{-1} \iint_A \mathbf{\Omega}\cdot d\mathbf{A}\ . \qquad 13$$

The A in Eq.13 represents the surface area enclosed by the beam path, which is the integration path of Eq.9.

The familiar Sagnac result follows if the totality of enclosed area can be denoted by a single surface vector **A**, and after multiplying by 2 to account for the double loop comparison between clock- and counterclock-wise loops. The **Ω** being constant throughout the interferometer equipment may thus be taken out of the integration process, which then yields Sagnac's observed result:

$$Z = 4\mathbf{\Omega}\cdot\mathbf{A}/(c\lambda_0). \qquad 14$$

This Sagnac process (*i.e.*, the laser variations thereof) presently provides the basis for the most sensitive devices for sensing absolute rotations. When operated in the form of a ring laser, its sensitivity is capable of detecting 10^{-5}, or better, of the relatively slow earth-rate of rotation. Note how Eq.9 is starting point for ring laser device and interferometer.[5]

Keeping in mind that Galilei addition of velocities is still the standard tool for obtaining Eq.14, the thought is justified to reconsider somewhat the standard conceptual framework surrounding the Lorentz group, the Galilei group, and the Michelson-Morley and Sagnac experiments. The here chosen path of relating Eq.14 to the loop integral Eq.9 provides a common umbrella for discussing both null-results and finite results.

Two major factors are now instrumental in the decision of whether or not the integral

$$\oint_{c_1} \mathbf{v}\cdot d\mathbf{r} = \iint_A \mathrm{curl}\,\mathbf{v}\cdot d\mathbf{A}$$

vanishes. One has to do with the beam-enclosed area A. It is determined by the interferometer design and depends on how the integration loop c_1 bounds a net surface area A. The other major factor is contingent on the type of motion itself to which the device is submitted. The integrand $\mathbf{v}$ or rather curl$\mathbf{v}$ is the determining factor. The just-mentioned factors instrumental for the value of integral $\oint \mathbf{v}\cdot d\mathbf{r}$ lead to a number of conclusions that are unorthodox, when seen in the perspective of somewhat distorted, yet prevailing, opinions about relativity.

The simple Michelson-Morley interferometer qualifies as a zero (beam-enclosed) area design, because the beams travel back and forth, tracing the same path. Yet a possible exception may have to be made for some of the later designs with multiple-beam traversals; depending on design detail, they may enclose small residual areas. For that reason, they could give nonzero results, where zero results were expected.

However, assuming the enclosed area to be zero, it follows:

The ideal Michelson-type interferometer of zero enclosed beam path area should give a null result regardless of whether its motion is a uniform translation, a rotation, or both.

In the spirit of Chasles theorem, this covers arbitrary rigid body motion in a Galilean sense. This null property of the traditional Michelson interferometer is, in retrospect, a fortunate circumstance indeed, because it led to the discovery of the Lorentz and conformal groups (see diagram II in this chapter).

Modern inertial navigation devices are known to be capable of measuring very small fractions of an earth rate of rotation, well into the range of orbital rates. Since these small rates of rotation are now measurable with Sagnac-based ring-laser devices, *the customarily suggested exclusive relationship between the Michelson-Morley experiment and the special theory of relativity are now no longer in order.*

After almost a century of teaching how Lorentz transformation are essential in explaining the Michelson-Morley null result, *the amazing feature of the present discussion is the total absence of any explicit reference to the Lorentz group.* Some further thought, then, reminds us that the exclusive bond between translation and Lorentz group makes the joint treatment of Michelson's null result and Sagnac's nonzero result impossible.

How does relativity get into the picture? Since the physical relevance of the Lorentz group is backed by impressive evidence, the cited situation is not to be regarded as grist for the mill of those who don't like the theories of relativity. It may be necessary, though, to give the Lorentz group a more abstract role in physics than is presently fashionable. The principal question, at this juncture, centers around the Diffeo-4 premise that goes into a derivation of the universal fringe shift formula, here given by Eq.13. Comparative derivations of this expression, covering the more general case of refracting media in the beam path, have been discussed in ref.5.

The starting point for an expression involving the loop integral $\oint \mathbf{v}\cdot d\mathbf{r}$ remains, however, a *spacetime* form of the Hamilton principle for the trajectories of light beams. The optical wave equation leads, in the geometric optical limit, to the Hamilton equations of motion. The latter assume in the case of light rays, a form which is referred to by Poeverlein[3] as the Sommerfeld-Runge law: curl**k**=0, where **k** is the wave vector. The corresponding expression for a material particle of momentum **p** is curl**p**=0. In chapter VI, Eq.10, it is shown how Einstein inferred the one-form $(E;\,\mathbf{p})$ to be closed, thus establishing the period-integral nature of the Bohr-Sommerfeld integrals. Here, it determines the 1-form $(\omega;\mathbf{k})$ as a closed form.

The integral of the Hamilton principle becomes, through the use of a Legendre transformation, a spacetime line integral, which is then subjected to the one-parameter (nonlinear) Galilei group $(d\mathbf{r}=\mathbf{v}d\tau;\ dt=d\tau)$. Since a general invariant treatment is implied by the variation treatment of the Hamilton integral, the result obtained acquires a measure of validity in the realm of the general theory of relativity. In fact, no spacetime metric has an explicit role in these considerations; therefore, the procedure is still metric-independent at this point. Here is a case in point, earlier cited by Birss (chapter VI), the spatial metric is essential for the end result, not the spacetime metric.

By recognizing that the general theory of relativity, with or without a metric, is the realm of operation for discussing the here-mentioned Michelson and Sagnac experiments, the conceptual situation is found to be closer to a resolution. Yet, that very fact creates a new hurdle, because modern renditions of the general theory are almost exclusively gravity-oriented. The principle of general covariance, originally one of the cornerstones of Einstein's theory, has these days been reduced to a nonphysical status. If this principle is reinstated, diagram II can help to reconcile the Galilei-Lorentz controversy. It shows the Lorentz group and the linear as well as the nonlinear Galilei groups as subsets of the set of general (*i.e.,* linear and nonlinear) spacetime coordinate substitutions, or, in modern jargon, the set of spacetime diffeomorphisms: Diffeo-4.

While it is not essential at this point to discuss these matters with the help of a spacetime metric, there is no objection against using a metric-based argument. The spacetime metric is an invariant under the Lorentz group, but not under the Galilei group. Langevin[6] was the first to show how ***it is exactly the noninvariance of the metric under Galilei rotations which explicitly leads to the Sagnac effect.***

5. Conclusion and History

The unified Diffeo-4 rendition of the Michelson and Sagnac observations now emerges as a striking example how physics has shortchanged itself in the past by following prematurely the advice of a "covariance-rejecting" pragmatism. Instead of making things simpler by avoiding an implementation of the principle of general covariance, matters became more complicated by not giving the principle a chance to demonstrate its coordinating potential. Another compounding factor has been the establishment's predilection for cure-all local approaches versus an inadequate perception for situations calling for global approaches.

The coordinating function of general covariance manifests itself in the principle's ability to remove disturbing dichotomies in standard discussions. Here is a list of instances encountered in these chapters that may be said to have profited from a contextual revival of general covariance in conjunction with options for global description. Here are major instances where general covariance (referred to as Diffeo-4) shows its virtue:

1. Diffeo-4 procedures invite distinctions between metric-dependent and metric-independent descriptions.

2. Diffeo-4 procedures invite distinctions between local and global descriptions by revealing unique global features of pre-constitutive fundamental law.

3. In the light of the delineations given under 1 and 2, the principle now permits an enunciation of metric-independent Diffeo-4 quantum-laws in the form of period-integrals.

4. The obstacles encountered in converting the Dirac equations into a Diffeo-4 form now become understandable, as having been invited by an inappropriate Diffeo-4 imposition for ensemble-based situations.

5. A Diffeo-4 global approach permits a unified description of the Micheson-Morley and Sagnac experiments.

A word of criticism is in order about the original wording of the principle of general covariance. The goal was stated as meaning that locally Diffeo-4 conditions were to be imposed on all physical laws. Stated in this form, the principle's use is contingent on a definition of physical law. The principle of invariance hierarchy (chapter XV; 4) helps here to home in on Diffeo-4 eligibility.

Diffeo-4 laws are taken as fundamental if they no longer retain constitutive specifics pertaining to the media to which they apply. The four-field (**E**,**D**,**B**,**H**) Maxwell equations belong in that category, and so do the quantum counting period integral laws of chapter VI. The two-field (**E**,**H**) Maxwell equations don't belong in that category, because they invoke parameters related to the spacetime metric. The metric-tensor indeed defines the constitutive properties of spacetime. The gravitational field equations, which postulate a proportionality of the energy-momentum tensor and a differential concomitant of the metric tensor, qualifies as medium independent if the gravitational constant of relativity can be taken to be a universal constant similar to e and $\hbar$.

A different situation prevails for the laws that directly describe the physical properties of a medium. Pierre Curie has said, in the context of piezo-electricity, c'est l'assymetrie qui fait l'effect! meaning that only the absence of certain symmetries cause a medium to display certain effects. For instance, inversion symmetry rules out piezo-electricity. Since wider symmetry reduces the number of possible effects, there is this glimpse of truth to early Kretschmann[7] and Bridgman[8] criticism (chapter XVI) of the principle of covariance, when seen from the angle of constitutive law. However, that would be wrongly applying Diffeo-4 (numerical) invariance, because the encompassing set of all conceivable symmetries would effectively rule out all local constitutive effects. Indeed, from this angle, Diffeo-4 numerical invariance could not have any physical consequences.

Yet, by the same token, as earlier observed, **Diffeo-4 form invariance** can distinguish locally a primordial pre-constitutive state of physical law prior to media specifics, say, the Maxwell equations. Here, form-invariance takes precedent over numerical-invariance. This distinction is to be regarded as a first step for homing in on the organizational feature alluded to in Einstein's response[9] to Kretschmann.[7]

Since imposing local Diffeo-4 numerical invariance becomes, indeed, a trivial operation, *the only nontrivial Diffeo-4 numerical invariances are necessarily global in nature.* The period integrals identifying quanta and the cyclic integrals expressing flux-and-charge conservation discussed in chapter VI are prime examples of such global laws. These integral laws are also characterized by their fundamental *pre-constitutive form,* because any media references are absent in those renditions.

So, bringing the Kretschman-Bridgman criticism to its logical conclusion we have just seen that imposing numerical Diffeo-4 invariance is locally trivial; yet globally, it has a way of singling out preconstitutive fundamental law. Mindful of the triviality of imposing locally Diffeo-4 numerical invariance, the next question becomes: What are the *nontrivial local results* of imposing *global Diffeo-4 numerical invariance* on those integrals?

Here are some relevant answers: The global numerical Diffeo-4 invariance of an integral defines how, locally, the integrand is to **"covary"** with transformational changes of the elements of integration. This Diffeo-4 **covariance** of the integrand defines a tensorial quality. The covariance of the tensor elements is designed to retain an intrinsic property of its associated **form**, which may have led to the language use why covariance is also referred to as **form-invariance**. The invariant contraction of tensor and integration elements defines a differential form in the sense of Cartan. The form is said to be "impair" if it changes sign under orientation-changing transformation of the frame of reference; if it does not change sign, it is said to be "pair" (compare chapter VI).

The guaranteed Diffeo-4 invariance of the Stokes and Gauss laws, now usually referred to as Stokes' generalized theorem, can now be called upon to define Diffeo-4 invariant operations on tensors and/or their associated forms. This operation, defined for arbitrary dimension, is called the exterior derivative. It covers in three dimensions the familiar differential operations of vector analysis known as gradient, curl and divergence.

This excursion in the land of differential invariants (with its, at times, confusing terminology and equally confusing connection to physics) has been generated by two interrelated controversies (*i.e.*, Michelson versus Sagnac, and Kretschmann-Bridgman versus Einstein). Both can be resolved by reinstating general covariance as a physical principle and by making a long-overdue delineation of local and global procedures (chapter XVI).

Mindful of the conceptual pitfalls associated with the principle of general covariance (if attending circumstances are neglected), an inquiry into its history is in order. The rudiments of general (form) covariance go back to Lagrange. A first step from space to spacetime (and general covariance) came about with the one-form of energy-momentum. This took place early last century through the Legendre transformation of Hamilton's principle. Sommerfeld's famous calculation of the fine structure of hydrogen in 1917 took full advantage of a built-in spacetime Diffeo-4 invariance of the Hamilton-Jacobi process.

The global approach to interferometry discussed in this chapter is, in essence, the light-ray version of Hamilton's principle, in which the spacetime phase is the counterpart of action. Since the equations of motion emerge as integrability conditions of the one-form of energy-momentum, a metric-independent general covariant quality of the fundamental laws of mechanics and geometric optics are here apparent.

Kottler, Cartan, and Hargreaves have, each in his own way, focused on these aspects. In the Thirties, van Dantzig reiterated again how metric-independent general invariance permeates the fundamental laws of electromagnetism and mechanics. All these references can be found in Whittaker's classic on the history of the theories of "aether."[10]

In preceding chapters (*e.g.*, chapter VI), ample evidence has been accumulated for the purpose of extending the cited "Whittaker testimony" with substantial physical evidence in the form of a set of metric-independent period integrals, which quantize single systems and ordered ensembles thereof. During the past half-century, contemporary physics has done itself a disservice by banking solely and too exclusively on aspects of Lorentz invariance. As a result, distinctions between local-global and numerical-versus-form invariance have remained obscure. If mathematical-physical pragmatism was the initial justification to settle for Lorentz invariance, in retrospect it now appears that this restriction has extracted a high price in terms of having to ignore evidence that is calling out for a more incisive look at the role of general invariance in physics.

In no way is it possible to reconcile the here-cited predicaments of physics by persisting with the presently prevailing mathematical methods; fragmented as they are by local-global indecision and arbitrary choice of invariance options. These mathematical inadequacies compound the task of physical interpretation. They have been part and parcel of *ad hoc* nonclassical imagery and quantum mysticism. All of this has contributed to growing incoherence and undue splintering of beckoning conceptual wholeness.

EPILOGUE FOR EXTRAPOLATING A FAVOR OF FORTUNE

A summary of these exercises in quantum reprogramming carries, in essence, a direct and simple message for physics. The first part of this message says: The Schroedinger equation is a slightly less-than-perfect tool, notwithstanding its amazing applicability. The second part says: The Copenhagen interpretation is much less perfect than the less-than-perfect Schroedinger equation. Perhaps surprisingly, this change in assessment does not in the least detract from the physical relevance and usefulness of the Schroedinger equation. Yet, by contrast, hardly any essential physical role can be retained for the vague conglomerate of mysticisms that is known under the name of Copenhagen interpretation.

A fair number of physicists silently agree with this assessment. To this effect, some may even testify in public. Yet few are willing to draw compelling conclusions from this proven near-irrelevance of the Copenhagen view for a future course of action. It may not be lack of courage of conviction causing physics to settle for more than half a century of *status quo*, but rather a lack of clearly beckoning perspectives as to what is worthy of new convictions on which to build. Then in the back of many heads, there remains this tempting belief that contemporary quantum mechanics is so close to the truth that minor changes might still bring the whole truth.

If the introductory statement of this epilogue restores a peace of mind concerning the Schroedinger equation, by the same token, it totally removes support for the Copenhagen interpretation. So, *if we really need an interpretation*, what other view might be regarded as closer to the reality of the Schroedinger equation? and how does this interpretational change affect the totality of quantum mechanics?

The evidence presented in these reprogramming exercises identifies the Schroedinger equation as the tool which comes closest to describing dilute ensembles of randomized single systems, but not quite the isolated single-system itself. It is this asymptotic relation between these two options, which has literally held physics in a prison of "uncertainty" for more than half a century. What made this amazing feat possible?

One of the pioneers of the group assessment of quantum descriptions, the mathematician Herman Weyl, is cited by Morris Kline[1] as having said: "It was not merit but a favor of fortune when in 1923.... the spectral theory of Hilbert space was discovered to be the adequate mathematical instrument of quantum mechanics." How should we presently view Weyl's exquisite characterization of the 1925 quantum revolution? Since this quantum reprogramming started with the intention of lessening dependence on fortune and enhancing fact and reason, it can now be said that a better idea about the revolution's realm of validity is emerging, if a comparison is made with the early days of quantum euphoria.

From the mathematical point of view, the spectral theory of Hilbert spaces has done well, even without the additional stimulus of physical applicability. It is an intellectually elegant monument, a demonstration masterpiece, if you will, as to what the formalistic approach in mathematics is capable of accomplishing. When this spectral theory of Hilbert spaces was found to hold an important key to many quantum problems, the justifiable mathematical euphoria carried over into a not-so-justifiable physical euphoria.

Then, as frequently happens, after the initial physical euphoria subsided, a period of introspection follows. Quantum mechanics that was generating all those Hilbert spaces (*i.e.*, the Schroedinger equation), refused to yield to a derivation reducing it tightly and succinctly to first principles. Therefore, men of conscience were, and should have been, concerned with the possibility of having perhaps over-invested in a prematurely presumed exact physical status of the Schroedinger equation. A physical exactness matching the mathematical exactness of the theory of Hilbert spaces became an appealing ideal, even if it not quite borne out by perceived reality. It was less the Schroedinger equation itself, but rather the nitty-gritty of its physical interpretation, which, through the years, retained evidence of a measure of conceptual incompleteness. It was this sense of incompleteness that literally sought a substantiation through the suspected existence of hidden variables. The local commitment, inherent to Schroedinger's method, gave those hidden variables a local basis as formulated by Bell's inequalities. Aspect's experiments then removed the basis for local relevance by not confirming Bell's inequalities.

A multitude of interpretational propositions have seen the light of day, without seriously affecting or, for that matter, clarifying the applicability range of the Schroedinger method. All of which goes to show that the Schroedinger method was wiser in finding its way to relevant answers than our efforts at understanding why it could find its way. While this seemingly unerring capability of giving right answers reinforced belief in its magical potential, by the same token, this fact also became grist for the mill of the pragmatists who preached their gospel of "not to worry" about interpretations. Some pragmatists are known to use ***any*** interpretation or none at all, contingent on what the situation calls for.

Whenever situations arise where an interpretation is believed to be necessary, then the so-called Copenhagen single-system view of Schroedinger's equation is found to be, by far, the most favored. This reprogramming identifies the Copenhagen single-system extrapolation as the major interpretational stumbling block in contemporary quantum mechanics. It also is the source of almost all quantum mechanical mysticism. In fact, the single-system premise is easily the least-justified and probably the most-flawed premise of the Copenhagen view.

There have been many competing points of view, yet none achieved the prominence of the Copenhagen view. This reprogramming has homed in on one particular competing view; it is one that is not based on the single-system premise. The Schroedinger equation is here taken to have an ensemble connotation, a view pioneered by Popper in the Thirties. Pragmatism, and the already existing mystical, though workable, Copenhagen view, left only a lukewarm reception for the original Popper proposal. For all practical purposes, it was relegated to a position of just another one of many unessential proposals.

If nothing else, the reprogramming shows that properly chosen ensemble specifications bring the Popper proposition into a revealing quantitative relationship with the Schroedinger equation. While this fact still does not establish beyond a shadow of a doubt that the Schroedinger equation is an ensemble tool, it does make the ensemble option a good deal more probable than the single-system option. As a remaining restraint preventing an all-out support of the ensemble option, one should consider that even these explicit quantitative relations between ensemble statistics and Schroedinger results still fall short of aiding in a complete derivation of the Schroedinger equation from first principles.

In fact, considering the lacunar nature of the derivational origin of the Schroedinger equation, there should be real doubt whether this equation will ever be derivable from first principles. After all those years of investigation and contemplation, it is more likely that Herman Weyl's words are to be taken more literally than ever intended: *the Schroedinger equation's relation to Hilbert space was* (and still is) *"a favor of fortune."*

With all reservations in place, it is thus concluded that the Schroedinger equation comes conceivably closest to describing dilute ensembles of identical single systems in quantum states that are randomized in phase and orientation. The word "conceivably" here conveys that there is no proof by derivation to completely substantiate this statement. All of this indicates that there is no known context for which the Schroedinger equation can be given a status of absolute physical exactness. The many statements to the contrary in the quantum mechanical textbook literature, therefore, are to be regarded as the products of wishful thinking.

Overwhelmed by the equation's unparalleled success, earlier theorizing had always tacitly assumed an absolute and exact status for quantum mechanics' major tool of inquiry. Physical imagery kept drifting around on this magic cloud, which had brought so much "fortune" beyond what physics had deserved on the basis of "merit." Hence, when the Copenhagen interpretation became available as an intellectual vehicle for consolidating this equation's position of "fortune," the Copenhagen view automatically, yet unjustifiably, assumed the same aura of near-perfect truth that had already been identified with the Schroedinger equation and spectral theory. Now, in retrospect, physics should have had some real doubts that such a lofty assignation, even for the Schroedinger equation, might not be on altogether solid grounds.

In the course of this reprogramming, evidence has been compiled which amply supports the Popper-ensemble proposition in conjunction with a required phase-and-orientation randomness of its constituent elements. It is the best that can be done for the Schroedinger equation, which, incidentally, is a lot better than what the textbook literature has done for the mutual relation of the Schroedinger equation and the single system. The textbook method simply postulates an interrelation between single-system and Schroedinger equation, and then, depending on Copenhagen-related convictions, this premise is made "plausible" with mystifying statistical considerations, which, until this day have not found their "universe of discourse." So physics is confronted with a choice between a rather classical ensemble statistics versus a very nonclassical single-system statistics.

An inducement for a change of heart (*i.e.*, from nonclassical single system to randomized classical ensemble) was precipitated by actual limitations ensuing from an indiscriminate Schroedinger treatment of certain macroscopic quantum phenomena that don't meet randomness criteria. This monograph elaborates some cases in detail. However, the literature is full of them, that is to say, once the mind is alerted to making the distinctions.

Confronted with the greatly enhanced physical reality of the randomized ensemble association of the Schroedinger equation, can it still be regarded as an adequate tool for an incisive description of the isolated single-system constituents of ensembles? A similar question holds for ensembles that no longer obey the randomness criteria, because they are ensembles which respond as single-systems by virtue of their internal order.

The answer is that Schroedinger's equation and associated Hilbert space can give, at best, an asymptotic picture of what is going on at the single-system level, sometimes less. This new state of affairs has thus been created by a physics-dictated change. It leads to questions as to where we go in terms of a more precise mathematical description for single-systems, now that Hilbert space is no longer a last word.

The introductory chapter of this quantum reprogramming identifies as instrumental the transition from a local-global Schroedinger eigenvalue description to an *ab initio* global description with the help of period integrals. Schroedinger's process explores ***from the inside outwards***; starting with the wave equation's local statement and then applying global boundary conditions; it is accordingly called a local→global process. The *ab initio* global process, by contrast, explores ***from the outside inward*** and can yield system information well beyond that what the local→global Schroedinger process needs as a prerequisite for operation. These global methods were mathematically brought to fruition by de Rham and Hodge in the Thirties. A cursory inspection of names of people involved in this more recent mathematical development gives an indication that physics is now leaving the territory of the rather strict formal mathematics of the spectral theory of Hilbert spaces and is entering a much more "intuitional" domain of mathematics.

In (differential) geometry, this so-called formal→intuitional transition became a local→global reorientation. A preoccupation with formalities of differential concomitants of local curvature led to a more intuitive global concern with topological structure, such as can be created by an integrated view of local curvature. Hence, from studying the trees in the forest, the emphasis has shifted to the forest as a whole, which ultimately would foster a better understanding of the individual trees in the forest.

For physics, the study of "local" curvature properties had culminated in the general theory of relativity. Although this theory is known to have global ramifications, its experimentally confirmed aspects, so far, are mostly local in nature. Clearly, global theorizing literally puts the theorist out on a limb; global methods, therefore, tend to be more intuitive than the approaches prevailing in the more accessible local domains.

In the course of the reprogramming, quantization has made itself definitely known as reflecting global connotations of spacetime manifold structure. The relation between quantization and period integration can hardly leave any doubt about that fact. There is a deceptive irony, though, in the circumstance that the global approach lends itself especially well to assessing "whole" structures such as particles in the microdomain. In the Einstein field equations, by contrast, local spacetime curvature has a dominant role for the physical macrodomain.

The general theory of relativity thus remains a local theory, notwithstanding its global perspectives. Looking at these matters in retrospect, it can hardly escape attention that physics now is confronted with a situation in which a choice must be made about the mathematical machinery necessary for exploring the global realm. In making this transition towards period integration, physics is faced with potential ingredients for conflict, which could be very similar to the war that was raging in mathematics during the Twenties.

It was the war between Formalism and Intuitionism. Hilbert came to be regarded as the leader of Formalism, and Brouwer headed the Intuitionists. In retrospect, it may now be said that the differences in mathematical convictions reflected natural differences of the respective mathematical domains. One or the other type of reasoning would prevail as being most effective in getting results. Unfortunately, these natural policy changes carried over into the realm of personal relations.

The tension built to a point where Hilbert fired Brouwer as coeditor of the "Mathematische Annalen." Efforts of colleagues to avoid this confrontation were to no avail. Einstein, called upon to mediate, felt incapable of bringing the parties together. Later, he would compare this episode of conflict to Aristophanes' play of the "Frogs" waging war against the mice.

Of course, neither Formalism nor Intuitionism stood to gain from the unfortunate personal aspect of this development. Let it suffice to say that those who used to call themselves "intuitionists" were guilty of using formal-type arguments and those calling themselves "formalists" were guilty of using intuitional arguments. Life has many more shades and hues than just black and white, and those involved in this conflict knew it and did not have to be convinced to recognize that human reasoning can be an unpredictable mixture of both. In fact, the protagonists in this real-life drama practiced exactly such opportunism!

The Brouwer-Hilbert episode helps dispel the thought that emotions cannot rise to a boiling point in a supposedly logical discipline such as mathematics. It is the technical similarity with the present situation in physics, which gives physics cause to proceed with caution, combined with willingness to understand. Yet, circumspection should not stop or delay attempts at needed change. Time will tell whether physicists are more or less volatile than mathematicians. Let us delineate the current situation in physics as contrasted against the past of mathematics.

For more than half a century, physical theory has been riding high on a bandwagon of one-sided abstract formalism. During that time, an intuitive modelling of microphysical structures was anathema. The thesis of universal uncertainty blocked the mental access to information necessary for realistically completing such models. It was deemed to be outside the realm of human knowing. Unlike the situation in mathematics, the wave of formalism in physics had assumed a much more radical position than in mathematics. Abstraction in physics had become a path of last resort. The romantic dictum of single-system quantum mechanical uncertainty had been quite effective in ascertaining this result!

The history of physics over the past six decades testifies to a near deification of abstraction. This purely formalistic phase in physics has lasted much longer than the comparable formalistic phase in mathematics, because the intuitive counterpart was, in fact, literally outlawed by the dictum of uncertainty. In mathematics, formalism and intuitionism were always simultaneously active in a more balanced manner, notwithstanding some of the terrible fights among its practitioners (*e.g.*, the Kronecker-Cantor conflict and the mentioned Brouwer-Hilbert controversy).

Ironically, physics had initially made a successful attempt at globally assessing quantum situations. Bohr-Sommerfeld's quantization integrals and Sommerfeld's famous fine-structure calculation bear witness to how physics had attempted an all-global approach with reasonable success. Yet, a decade later, this all-global method had been displaced by an eigenvalue compromise, which is of a mixed local→global nature. Possible shortcomings in the exact status of the Schroedinger equation had not yet loomed on the horizon. The mystique of uncertainty had been successful in holding off the determinists.

That, in a nutshell, is how Hilbert space and its spectral properties became the realm of reality for the Twentieth Century physicist, almost to the exclusion of spacetime itself. Fortunately, the representation theory of groups retained a last powerful link between spacetime and Hilbert's configuration space of quantum states. The group theoretical method was a last concession to a phenomenology of microphysical symmetry. Somehow, and again ironically, symmetry had been permitted to penetrate the fog of uncertainty shrouding microphysical structure. Weyl and Wigner had shown the way.

The emergence of macroscopic quantum effects, however, brought us back from the abstract multi-dimensional Hilbert spaces into a reality of more intuitive spacetime model-making. Over and above symmetry, the mathematical machinery instrumental for implementing greater balance between formal and intuitive procedures in physics must be expected to call, more than before, on the topology-oriented studies initiated in mathematics by Poincaré, Brouwer, Hopf, de Rham, and Hodge.

A recognition of this new impending reality has been very difficult for physics. For more than half a century, it has preached the gospel of a quantum mechanics, in which Hilbert space became almost a sole storage place of fundamental physical truth. Those involved in the study of quantum mechanical fundamentals have been brushing up Hilbert space formalisms, using group theoretical methods for finding out more about the spacetime structure of the objects generating the Hilbert space superstructures. There is, at this time, no real acceptance of a need to complement this procedure with intuitive conjecture by following in both directions the threads of logic interconnecting suspected cause and perceived effect. This attitude is understandable in an interpretational atmosphere, which, as a result of its own premises, had been forced to restrict the rules of causality in the microdomain.

While the use and function of Hilbert space, as it now stands, is not to be underestimated, its primary role is instrumental for ensembles, not for single systems! The Hilbert integral equation as definer of eigenvalue situations has an ensemble connotation, because it defines the wave function ψ in any point in terms of ψ everywhere else through the process of integration in the physical domain (chapter XVII;8).

The Hilbert integral equation as Schroedinger equivalent has an inherent "smeared-out plurality" which seems out-of-context for a single system. The period integral assessment, by contrast, does not have this reflexive interconnectedness of its spatial parts, which is so characteristic of the Schroedinger plurality. The period integral's domain of integration can be shrunk unto the single (microphysical) object of observation. It is capable of exploring topological structure, where Schroedinger's process is confined to essential physical features of the ensemble constituents only.

This inability of making an adequate ensemble versus single-system distinction led quantum mechanics into the dead-end street of formal abstraction. This escape in abstraction became misleadingly acceptable. Getting out of this dead-end situation, the man-made fog of "uncertainty" needs to be lifted so that horizons become visible again. After that, a view of the single-system domain comes into focus, thus opening up a potential for the more intuitively oriented methods of topological modelling in actual spacetime, not Hilbert space! There is presently, at best, a general but vague awareness of the nature of these deficiencies in contemporary quantum mechanics. The existing situation of a long standing *status quo* reveals physics' reluctance in coming to grips with the cited predicament.

To illustrate this point, let us focus on a "foundations" meeting, in which the late John Bell attacked several quantum colleagues on their views about measurement theory. Bell's purpose may have been a form of shock therapy to break out of an impasse. He forcefully assigned his colleagues to categories to which they were reluctant to belong. From the eulogy for Bell by Kurt Gottfried,[2] it appears that, in private, Bell used to say about orthodox quantum mechanics: "Something is rotten in the state of Denmark." While Bell's offhand remark may reveal a foreboding of conflict, the situation, as is, does indicate the existence of a formalist-intuitionist predicament in physics.

It is an irony of fate that Bell, once hailed as the savior of orthodox quantum mechanics, does not mince words about his own doubts concerning the status of the discipline he supposedly saved. One wonders whether **he** said he saved orthodox quantum mechanics (OQM), or whether those who wanted OQM saved as is, said **he** saved OQM. It shows how careful one should be in accepting praise for achievement. Bell's work effectively took position on the issue of local hidden variables, **from the single-system angle**. His later misgivings about Copenhagen vividly illustrate how Bell himself had not overcome the insidiousness of Copenhagen's single-system extrapolation. It is this inconspicuous, yet least justifiable, basic Copenhagen premise which keeps haunting Contemporary Orthodox Quantum Mechanics (COQM). ***Quite amazingly, the textbook literature hardly considers the viability of this thesis of single-system extrapolation a point of worthwhile discussion.***

Given this *de facto* coexistence of ensemble and single-system views of contemporary quantum mechanics, and mindful of the small minority status of the ensemble folks, the question is: Has the majority shown awareness for the minority point of view? One truly has to comb the literature for a representative answer. Let us scrutinize, for this purpose, the choice of language used by an authoritative source in verbalizing the quantum mechanical "unspeakables."

Even if it is difficult to place a label on Feynman, most people familiar with his work might concur that he was a highly individualistic member of the Copenhagen quantum mechanical majority. On p.22 of his book with Hibbs[3] on *Path Integrals* in a section on *Some Remaining Thoughts,* one finds the following statement under the heading of *fundamental concepts of quantum mechanics* :

I "From what does uncertainty arise? Almost without doubt it arises from the need to amplify the effects of single atomic events to such a level that they may be readily observed by large systems."(Feynman Hibbs 1965)

It would seem as if Feynman and Hibbs are making a plea supporting an ensemble origin of uncertainty, which is exactly how Planck introduced it in 1913 in the guise of zero-point energy! It is also close to what Popper said in 1934. The difference between Feynman and Popper is in the choice of epistemological priorities. Feynman's QED commitments force him to hang on to Copenhagen's premise of single-system extrapolation. By contrast, Popper feels the epistemological reality of a plurality of ensemble constituents should be accepted!

A little further on p.22 of the Feynman-Hibbs text, it says:

II "It would be an interesting problem to show that 'no other' consistent interpretation can be made." (F-H)

This remark reveals a wishful element hiding behind their objective. Feynman and Hibbs are having second thoughts about the road they are going. If possible, they want to be reassured by a uniqueness proof to make sure that accommodating Copenhagen's single-system extrapolation is a truly deserving goal.

A little later, on p. 23 of the F-H text we are getting closer to a more definitive idea about the nature of their remaining doubts. The last sentence of the top paragraph may well be regarded as an invitation, in the spirit of measurement theory, to involve the observed as well as the observer and his apparatus. Paraphrasing Bell's words, Feynman and Hibbs don't go to the *blasphemous* extreme of introducing a wave function for the universe, yet they plead for a concession. As in statement **I**, they speak again of "amplifying" the effects, to which end an "apparatus" is now invoked:

III "What seems to be needed is the statistical mechanics of the amplifying apparatus." (F-H)

It can hardly be denied that the F-H arguments are laced with some wishful thoughts that somehow are reminiscent of ensemble ramifications hiding in the background. Their predicament seems traceable to the single-system extrapolation. The reader might, at this point, be willing to place the F-H language battle with unspeakables in the perspective of a quantum-reprogramming alternative.

Since Heisenberg and Schroedinger at least agreed on the single-system nature of their equivalent procedures, let us examine the Schroedinger recipe to his approach to trace the single-system origin and viability. Rather than deriving wave equations, or variations thereof, from ever-evasive first principles, we do better to accept instead Schroedinger's recipe as is, and establish what it means.

In chapter VI; 8, the Schroedinger transcription is discussed in a global perspective. The transition from Hamilton-Jacobi process to wave equation is based on de Broglie's $(\mathsf{E};\mathbf{p})=\hbar(\omega:\mathsf{k})$. The latter entails an inescapable point-particle connotation, which has been a much too silent, man-injected concomitant of standard quantum mechanics. $(\mathsf{E};\mathbf{p})$ is a vector with domain connotation, and $(\omega;\mathbf{k})$ is a field vector with spacetime point association. *One cannot equate dissimilar vector-species without impunity, and then expect fundamental results.* The de Broglie-Schroedinger imposed point-element, though, is a fitting and adequate abstraction for ensemble consideration. Situations that don't permit such point-abstractions need to account for the global nature of quanta with the help of period integrals.

The subsequent variational process, as used by Schroedinger, gives, in good Hilbert tradition, the eigenvalue extrema. *Degeneracy provides families of solutions adjoint to distinct eigenvalues of energy and angular momentum. These families are ensembles with random parameters (phase and orientation) that are identifiable as universes of discourse of their statistics.*

Chapter VI explains how the picture of solution degeneracy as activator of ensemble statistics gives only two options of interpretation: the Gibbs option (Copenhagen) and the real physical ensemble. The Dehmelt *et al* single-particle experiments of 1986 then conclusively put the Gibbs option to rest, because to reconcile their observation with the Gibbs option, Dehmelt *et al* had to take recourse to converting a single-system statistical motion into a perfectly circular motion.

Now, so many years later with new experimental evidence, it is understandable why, in 1965, Feynman and Hibbs still went out of their way to accommodate the single-system Gibbs option. In addition, it is now also more understandable why Feynman (as one of the architects of quantum electrodynamics) was reluctant to pull the plug on nonclassical statistics, even after a classical statistical derivation of the angular momentum quantum number $\sqrt{n(n+1)}$ had been reproduced twice in the Feynman Lectures.

These reprogramming exercises, we hope, may now have collected enough evidence to finally give down to earth philosophy some benefit of the doubt over Copenhagen's speculation of single-system extrapolation. The chances of producing a uniqueness proof sanctioning this extrapolation have lost their momentum. Yet, before any of this can go forward, COQM needs to face up to its principal interpretational flaw (*i.e.,* the untenability of the single-system extrapolation). Pragmatist or no pragmatist, this flaw has invited procedures replete with excessive mysticism. ***Quantum magic has its limitations, even if the magic ensues from what Herman Weyl had so appropriately called "a favor of fortune."***

NOTES AND REFERENCES

Chapter I

1. Popper, K.R., Naturwissenschaften **22**,807 (1934)
   Popper, K.R., *Logik der Forschung* (Vienna 1935), English translation includes Einstein's letter: *The Logic of Scientific Discovery* (New York 1959)
2. Einstein, A., J. Franklin Inst. **221**, 313 (1936)
3. Blokhintsev, D.J., *Principles of Quantum Mechanics* (Boston 1964)
4. Kemble, E.C., *The Fundamental Principles of Quantum Mechanics* (New York, 1937)
5. Whittaker, E., *History of the Theories of Aether and Electricity,* Vol.II (London, 1953) pp.191-196
6. Barut, A.O., Found. of Phys. **20**,1233 (1990)
7. van Fraassen, B.C., *Quantum Mechanics: An Empiricist'sView* (Oxford University Press 1991)

Chapter II

1. Riordan, M., *The Hunting of the Quark* (Simon & Schuster, N.Y., 1987)
2. A relation between spatial invariance and covariance properties and physical dimension is due to Dorgelo and Schouten. A discussion along those lines by J.R. Ericksen, in the *Handbuch der Physik,* III/I (Springer, Berlin 1960). A spacetime approach along the same lines by E.J. Post, *Formal Structure of Electromagnetics* (Amsterdam, 1962) and Found. of Phys., **12**,169 (1982).
3. Whittaker, E., *Aether and Electricity* II (London 1953) p.191-196.
4. Kuessner, H.G., *Principia Physica* (van der Hoeck and Ruprecht, Goettingen, 1946)
5. Truesdell, C.A. and R.A. Toupin, in *Handbuch der Physik* III/I (Springer, Berlin 1960)
6. Maxwell, J.C., Proc. London Math. Soc. **III**,224 (1871)
7. Jammer, M., *The Philosophy of Quantum Mechanics* (Wiley, N.Y., 1974)
8. Post, E.J., Physics Essays **I**,279 (1988)

Chapter III

1. Przibram, K., ed., *Letters on Wave Mechanics* (Phil.Libr., N.Y. 1967) p.31.
2. Jammer, M., *Philosophy of Quantum Mechanics* (Wiley, N.Y. 1974)
3. Planck, M., *Theory of Heat Radiation* (N.Y. 1959, transl. 1912 ed.) p.141.
4. Kompaneyets, A.S., *Theoretical Physics* (Dover, N.Y. 1962) p.308
   Feynman Lectures (Reading 1964) Vol.II and Vol.III (appendix)
5. Reviews of Modern Physics, see Reviews of particle properties and recommended values of fundamental constants between 1950 and 1980.
6. Duane, W., Proc. Nat. Ac. of Sc.**9**,158 (1923)
7. Aspect, A.,*et al,* Phys. Rev. Lett. **47**,460 (1981); **49**,91 (1982)
8. Rauch, H., *et al,* Phys. Rev. **A34**, 2600 (1986)
9. A number of papers by Berry, Herrick and Kellman have appeared on "planetary" motion in atoms: Phys. Rev. **A11**,97 (1975); **A21**,418 (1980); **A31**,3502 (1985); Phys.Lett. **A103**,35 (1983); Physic.Rev.Lett. **55**,1738 (1985)
10. Walls, D.F., Nature, **306**,141 (1983); C.M. Caves, Phys.Rev. **D23**,1693 (1981)
11. Heisenberg, W., Zeitschr. fuer Phys. **33**, 879 (1925)
12. Schroedinger, E., Ann. der Phys. **81**,112,133 (1926)

13. Doll, R., and M. Naebauer, Phys. Rev. Lett. **7**, 51 (1961)
Deaver, B.S., and W.M. Fairbank, Phys. Rev. Lett. **7**,43 (1961)
14. Landau, L.D., and E.M. Lifschitz, *Quantum Mechanics* (Reading, 1957) pp. 66, and 457, also L.D. Landau, Zeitschr.Phys. **64**,629 (1930), a translation in D. ter Haar, *Men of Physics* (Oxford, 1965)
15. Heisenberg, W., Zeitschr. fuer Phys. **43**,172 (1927)
16. Kennard, E.H., Zeitschr. fuer Phys. **44**,326 (1927)
17. Weyl, H., *Group Theory and Quantum Mechanics* (London, 1931); translation from earlier German edition.
18. Kemble, E.C., *Fundamental Principles of Quantum Mechanics* (New York, 1937)
19. Misner, C.W., K.S. Thorne, and J.A. Wheeler, *Gravitattion* (San Francisco, 1973), chapter 43, p.1182.

Chapter IV

1. Duane, W., Proc. Nat. Ac. of Sc.**9**,158 (1923)
2. Popper, K.R., Naturwissenschaften **22**,807 (1934)
Popper, K.R., *Logik der Forschung* (Vienna, 1935), English translation includes Einstein's letter: *The Logic of Scientific Discovery* (New York, 1959)
3. Einstein, A., J. Franklin Inst. **221**,313 (1936)
4. Fuerth, R., Z. Phys. **81**,143 (1933)
5. Fenyes, I., Z. Phys.**132**,81 (1952)
6. Weizel, W., Z. Phys. **134**,264 (1953); **135**,270 (1953); **136**,582 (1954)
7. Bohm, D., and J.P. Vigier, Phys. Rev. **96**,208 (1954)
8. Nelson, E., Phys. Rev. **150**,1079 (1966)
9. Jammer, M., *Philosophy of Quantum Mechanics* (Wiley, NY, 1974)
10. Kemble, E.C., *Fundamental Principles of Quantum Mechanics* (New York, 1937)
11. Blokhintsev, D.J., *Principles of Quantum Mechanics* (Boston, 1964)
12. Ballentine, L.E., Revs. Mod. Phys. **42**,358 (1970)
Ballentine, L.E., *Quantum Mechanics* (Prentice Hall, NY 1990)
13. Groenewold, H.J., Physica **12**,405 (1946); Statistical Interpretation in *Induction, Physics, Ethics*, eds.Weingartner-Zecha (Dordrecht, 1970)
14. Collins, R.E., Found. of Phys. **7**,475 (1977)
15. Post, E.J., Phys. Lett. **A119**,47 (1986)
16. Haefliger, A., Compt. Rend. **243**,558 (1956)
Milnor, J., Enseignement Mathématique **9**,198 (1963)
17. Walls, D.F., Nature **306**,141 (1983)
Caves, C.M., Phys. Rev. **D23**,1693 (1981)
18. Post, E.J., Phys. Rev. **D25**,3223 (1982)
19. Post, E.J., Phys. Essays **3**,344 (1990)
20. Planck, M., *Theory of Heat Radiation* (Dover '59; '14 transl. of '13 German ed.)
21. Post, E.J., Phys. Essays **2**,55 (1988)
22. Bell, J.S., *Speakable and Unspeakable in Quantum Mechanics* (Cambridge University Press 1987)
23. Gottfried, K.E., Physics World, Oct. 34 (1991)

Chapter V

1. Popper, K.R., Naturwissenschaften **22**,807 (1934)
   Popper, K.R., *Logik der Forschung* (Vienna, 1935), English translation includes Einstein's letter: *The Logic of Scientific Discovery* (N.Y., 1959)
2. Bell, J.S., *Speakable and Unspeakable in Quantum Mechanics* (Cambridge, 1987)
3. von Neumann, J., *Mathematische Grundlagen der Quantum Mechanik* (Berlin, 1932); English transl. (Princeton, 1955)
4. Einstein, A.N. Rosen and B. Podolsky, Phys. Rev. **47**,777 (1935)
5. Bohm, D., *Quantum Theory* (Prentice Hall 1951) pp.614-619.
6. Aspect, A., *et al*, Phys. Rev. Lett. **49**,91 (1982)
7. Planck, M., *Theory of Heat Radiation* (transl., N.Y., 1959) p.141.
8. Feynman, R., R.B. Leighton and M. Sands, *The Feynman Lectures,* (Reading, 1967) Vol.II and Vol.III appendix; an earlier discussion of this statistics of orientation has been given by A. Kompaneyets,*Theoretical Physics* (Moscow, 1961) p.308.

Chapter VI

1. Chern, S.S., editor of *Studies in Global Geometry and Analysis* (Mathematical Association of America, Vol.4, 1967) compare article by Morse: *What is Analysis in the Large?*
2. London, F., *Superfluids* I (New York, 1950) p.152.
3. Aharonov, Y. and D. Bohm, Phys.Rev.**115**,485 (1959)
4. Doll, R. and M. Naebauer, Phys. Rev. Lett. **7** (1961) 51.
   Deaver, B.S. and W.M. Fairbank, Phys. Rev. Lett. **7**,43 (1961)
5. Schwarzschild, B., Phys. Today, **Jan.17** (1986); this article gives a coherent review of recent work by Webb, Washburn, Imry, Stone, Landauer and Pinhas. The work was started in Russia by Altshuler, Spivak, and father and son Sharvin.
6. Einstein, A., Verh. Dtsch. Phys. Ges. **19**, 82 (1917)
7. de Rham, G., *Variétés Differentiables* (Paris 1955) also
   Hodge, W.V,D., *Harmonic Integrals* (Cambridge, England, 1952) the second de Rham theorem on p.100 is a special case of the first theorem p.88.
8. Kiehn, R.M., J. Math. Phys. **18**,614 (1977)
9. Schouten, J.A., *Tensor Analysis for Physicists* (Oxford, 1951)
10. Birss, R.R., J. Math. Phys. **23**,1153 (1982)
11. Post, E.J., Phys. Rev. **D25**,3223 (1982)
12. Post, E.J., Phys. Lett. **A79**,288 (1980)
13. Flanders, H., *Differential Forms with Applications to the Physical Sciences* (New York 1963)

Chapter VII

1. Doll, R. and M. Naebauer, Phys. Rev. Lett. **7**,51 (1961)
   Deaver, B.S. and W.M. Fairbank, Phys. Rev. Lett. **7**,43 (1961)
2. Onsager, L., Phys. Rev. Lett. **7** (1961) 50.
3. Feynman, R.P., R.B. Leighton and M. Sands, *The Feynman Lectures*, (Addison Wesley, Reading, Mass., 1965); footnote Vol.III, p.21-12.

4. Post. E.J., Phys. Lett. **A94** (1983) 343; **A125** (1987) 225; Phys.Essays **2** (1988) 55.
5. Hodge, W.V.D., *Harmonic Integrals* (Cambridge, England, 1952) de Rham's first theorem p.88, and second theorem p.100.

Chapter VIII

1. Haefliger, A., Compt. Rend. **243**,558 (1956)
2. Yennie, D.R., Revs. Mod.Phys. **59**,781 (1987) for integer effect;
Laughlin, R., Phys. Rev. Lett. **50**,1395 (1983) for fractional effect, for a unified description of the two effects compare
Post , E.J., Phys.Lett. **A94**,343 (1983); **A125**,225 (1987); Physics Essays **II**,55 (1989)
3. Bohm, D. and J.P. Vigier, Phys. Rev. **96**,208 (1854), and Bohm Festschrift: *Quantum Implications* , eds. Hiley and Peat (London, 1987) Vigier et al, p.169. See also E. Nelson, Phys.Rev. **150**,1079 (1966): Compare for different physical views and uses of the quantum potential. references to Fuerth, Fenyes, Weizel, Bohm and Vigier as well as the appendix 1-8 of Vol.III of the *Feynman lectures* (Chapter IV;3)
4. Duane, W., Proc.Nat.Ac.Sc. **9**, 158 (1923)
5. Jammer, M., *The Philosophy of Quantum Mechanics* (Wiley, N.Y., 1974)
6. Planck, M.,*Theory of Heat Radiation* (1959 Dover reprint of 1914 translation of earlier German text) p.141.
7. Kompaneyets, A.S.,*Theoretical Physics* (Dover reprint 1962) p.308; also*Feynman Lectures*, Addison Wesley, Reading 1965) Vol.III, appendix.
8. Walls, D.F., Nature, **306**,141 (1983);
Caves, C.M., Phys. Rev. **D23**,163 (1981)
9. Mehra, J., Inst. Phys., Un. of Neuchatel (Schueler,S.A. 1963)
10. Barut, A.O., Zeitschr. f. Naturfor. **44a**,1051 (1989)
11. Elbaz, C., C.R. Ac.Sc.II **297**,455 (1983); Phys. Lett. **A109**,7 (1985) and **A127**,308 (1988)

Chapter IX

1. Landau. L. and E.M. Lifschitz, *Quantum Mechanics* (Oxford 1977) p.457.
2. Deaver, B.S. and W.M. Fairbank, Phys. Rev. Lett. **7**,43 (1961)
3 . Doll, R. and M. Naebauer, Phys.Rev. Lett. **7**, 51 (1961)
4. Prange, R.E. and S.M. Girvin, eds.,*The Quantum Hall Effect* , (Springer, Berlin, 1986)
5. Tsui, D.C., *et al* , Phys. Rev, Lett. **48**,1559 (1982); R. Willet *et al,* Phys.Rev. Lett.**59**,1776 (1987)

Chapter X

1. Brillioui n, L., *Relativity Reexamined* (New York, 1970)
2. Einstein, A., Verh. Deutsche Phys. Gesel.**19**,82 (1917)
3. Whittaker, E., *History of Theories of Aether and Electricity* (London, New York, 1953) Vol.II, Chapter IV.
4. Goldstein, H. *Classical Mechanics* (Reading, Mass.1965) p.302
5. Phipps, T.E., Jr., Essays in Physics **3**,414 (1990)

6. Dirac, P.A.M., *Principles of Quantum Mechanics* (Oxford 1947)
Other sources are:
Kramers, H.A., *Quantum Mechanics* (Amsterdam, 1957) or
Schiff, L.I., *Quantum Mechanics* (New York, 1955)
7. Thomas, L.H., Nature **117**, 514 (1926)
8. Pauli, W., Zeitschr. fuer Phys. **43**,601 (1927)
9. Bethe, H.A. and E.E. Salpeter, Handb. der Phys. **35** (Berlin,1957)
Further experimental information in:
Dayhoff, E.S. Triebwasser and W.E. Lamb, Phys.Rev. **89**, 106 (1953)
10. Lamb, W.E., Jr., Rep. Progress in Phys. **14**,19 (1951)
11. Dresden, S., *H.A. Kramers, Biography* (Springer 1991)

Chapter XI

1. Chamberlain, J.R., *et al,* Phys. Lett. **A67**, 239 (1978)
2. Post, E.J., Phys.Rev. **D25**,3223 (1982)
3. Whittaker, E., *History of the Theories of Aether and Electricity,* Vol.II (London 1951) pp.191-196.
4. Jehle, H., Phys. Rev. **D11**,2147 (1975)
5. *Particle Properties Data Booklet* (CERN, April 1984)
6. Haefliger, A., Compt. Rend. **243**,558 (1956)
7. van Dyck, R.S., P.B. Schwinberg and H. Dehmelt, Phys.Rev. **D34**,722 (1986); Phys.Rev.Lett. **59**,2 (1987)
8. Huang, K., Am.J.Phys. **20**,479 (1952)

Chapter XII

1. Alexandroff, P. and H. Hopf, *Topologie* (Berlin, 1935); Ch.X, §1-10,.p.390; Ch.XI, §3-12, p.440; preliminary remarks on p.212.
2. Kiehn, R.M., Proc. IUTM*Symposium on Topological Fluid Mechanics,* editors: H.K. Moffat and A. Tsinober, Cambridge University Press 1989; p.447.

Chapter XIII

1. Alexandroff, P. and H. Hopf, *Topologie* (Berlin, 1935)
2. Kiehn, R.M., J.Math.Phys.**18**,614 (1977)
3. Kiehn, R.M., Proc. IUTM, *Symposium on Topological Fluid Mechanics,* editors: H.K. Moffat and A. Tsinober (Cambridge Un. Press, 1989) p.447.
4. Post, E.J., Physics Essays **I**, 279 (1988)
5. Einstein, A., J. Franklin Inst. **221**,313 (1936)
6. Popper, K.R., *Logik der Forschung* (Vienna, 1935), English translation includes Einstein's letter: *The Logic of Scientific Discovery* (New York, 1959)
7. de Rham, G.,*Variétés Differentiables* (Paris, 1955)
8. Hodge, W.V.D., *Harmonic Integrals* (Cambridge, 1952)
9. Schouten, J.A., *Tensor Anlysis for Physicists* (Oxford 1951)
Post, E.J., Found.Phys. **7**,255 (1977)
10. Whittaker, E., *History of the Theories of Aether and Electricity,* Vol.II, (London, 1951) pp.191-196.
11. Post, E.J., Phys.Rev. **D25**,3223 (1982)
12. Post, E.J., Phys.Lett. **A94**,343 (1983); **A125**,225 (1987); Physics Essays, **II**, 55 (1989)
13. Post, E.J., Phys.Lett. **A119**,47 (1986)
14 Cohen, E.R. and J.W.M. Du Mond, Revs.Mod.Phys. **37**, 593 (1965)

15. Haefliger, A., Compt.Rend. **243**,558 (1956)
16. Witten, E., Comm.Math.Phys. **121**,351 (1989)

Chapter XIV

1. Riordan, M.,*The Hunting of the Quark* (Simon & Schuster, NY 1987)
2. Jammer, M., *Philosophy of Quantum Mechanics* (Wiley, NY 1974)
3. Planck, M., *The Theory of Heat Radiation* (Dover, New York, 1959) p.141.

Chapter XV

1. Einstein, A., J. Franklin Inst. **221**,313 (1936)
2. Popper, K.R., *Logik der Forschung* (Vienna, 1935), English translation, *The Logic of Scientific Discovery* (New York 1959) ncludes Einstein's letter.
3. Jammer, M., *Philososphy of Quantum Mechanics* (Wiley, NY, 1974)
4. Cartan, E., Bull. Soc. Math. France **41**,53 (1913)
5. Haefliger, A., Compt. Rend.**243**,558 (1956)
6. Milnor, J., Enseignement MatheÁmatique **9**,198 (1963)
7. Kretschmann, E., Ann. der Phys. **55**,241 (1918)
8. Bridgman, P.W.,*The Nature of Physical Theory* (Princeton, 1936) p.80.
9. Schroedinger, E., *Spacetime Structure* (Cambridge,1950)
10. Infeld, L. and B.L. van der Waerden, Prussian Ac. Sc., 380 (1933)
11. Einstein. A. and W. Mayer, Prussian. Ac. Sc., 522 (1932)
12. Schouten, J.A.,*Tensor Analysis for Physicists* (Oxford, 1951)
13. Misner, C.W., K.S. Thorne and J.A. Wheeler,*Gravitation* (San Francisco '73)
14. de Rham, G., *Variétés Differentiables* (Paris 1955) also in English Translation (Springer, Berlin 1966)
15. Cunningham, E., Proc. London Math. Soc.**8**,77 (1910)
16. Bateman, H., Proc. London Math. Soc. **8**,223 (1910)
17. Kottler, K., Vienna Ac. **131**,119 (1922)
18. Cartan, E., Ann. de l'Ecole Norm. Sup.**41**, p.1 and 12 (1924)
19. van Dantzig, D., Proc. Cambr. Phil. Soc. **30**,421 (1934)
20. Whittaker, E., *History of Theories of Aether and Electricity* (London 1951)
21. Truesdell, C. and R.A. Toupin, *Handbuch der Physik* III/I (Berlin '60)
Post, E.J., *Formal Structure of Electromagnetics* (Amsterdam '62)
22. Kuessner, H.G., *Principia Physica* (Goettingen 1946)
23. Post, E.J., J.Math.Phys. **19**,347 (1978)
24. Aharonov, Y. and D. Bohm, Phys. Rev.**134**,1410 (1964)
25. Kiehn, R.M., J. Math.Phys.**18** (1977)
26. Post, E.J., Phys. Rev. **D25**,3223 (1982)
27. Post, E.J., Essays in Physics **3**,344 (1990)
28. Post, E.J., Phys. Lett. **A119**,47 (1986)
29. Alexandroff, P. and H. Hopf, *Topologie* (Berlin 1936) p.440.
30. Planck, M., *Theory of Heat Radiation* (Dover '59,transl. of 1913 German ed.)
31. Feynman, R.P., R.B. Leighton, and M. Sands, *Feynman Lectures*, (Reading, 1964), Vol.II and Vol.III, appendix
32. Kompaneyets, A.,*Theoretical Physics* (Dover,,1962), p.308.

Chapter XVI

1. Bohm, D., *Quantum Theory* (Prentice Hall 1951) pp. 614-619.
2. Bell, J.S., *Speakable and Unspeakable in Quantum Mechanics* (Cambridge 1987)

3. Aspect, A., *et al*, Phys.Rev.Lett. **49**,91 (1982)
4. Morse, M., *What is Analysis in the Large?* Chapter II in *Studies in Global Geometry and Analysis,* editor S.S. Chern, Volume 4 of *Studies in Mathematics* (Mathematical Association of America, 1967)
5. de Rham, G.,*Variétés Differentiables* (Paris 1955) also H. Flanders ref.4 (p.76)
6. Planck, M.,*Theory of Heat Radiation* (Dover, N.Y.1959) p.141.
7. Kompaneyets, A.,*Theoretical Physics* (Dover, N.Y. 1962) p.308.
Feynman Lectures, Vol.II and Vol.III appendix.
8. Kretschmann, E., Ann. der Phys. **53**,575 (1917)
9. Bridgman, P.W., *Nature of Physical Theory* (Princeton '36) p.80.
10. Einstein, A., Ann. der Phys. **55**,241 (1918)

Chapter XVII

1. Planck, M.,*The Theory of Heat Radiation* (Dover 1959) p.141.
2. Popper, K.R., *Logik der Forschung* (Vienna 1935), English translation includes correspondence with Einstein: *The Logic of Scientific Discovery* (New York, 1959)
3. Slater, J.C., *Solid State and Molecular Theory: A Scientific Biography,* (NY, Wiley, 1975), Ch.2.
4. Ballentine, L.,*Quantum Mechanics* (Prentice Hall, NY 1990)
5. Post, E.J., Phys.Essays **I**,279 (1988)
6. Feynman, R.P., R B Leighton and M. Sands,*The Feynman Lectures,* (Reading 1964) Vol.II and Vol.III appendix.
Kompaneyets, A.,*Theoretical Physics* (Moscow 1961) p.308.
7. Aharonov, Y. and D. Bohm, Phys.Rev. **115**,485 (1959)
8. Post, E.J., J.Math.Phys. **19**,347 (1978)
9. de Rham, G.,*Variétés Differentiables* (Paris 1955)
10. Kiehn, R.M., J. Math. Phys. **18**,614 (1977)
11. Post, E.J., Phys. Rev. **D25**,3223 (1982)
12. Casimir, H,B.G., Koninkl. Akad. Wetenschap. **51**,793 (1948)
13. Post, E.J., Phys. Essays **3**,344 (1990)
14. Feynman, R.P. and A.R. Hibbs, *Quantum Mechanics and Path Integrals,* (Mc Graw Hill, New York 1965)
15. Courant, R. and D. Hilbert, *Methoden der Mathematischen Physik* (Springer, Berlin 1931) Vol.1.
16. Nelson, E., Phys.Rev. **150,**1079 (1966)
17. Boyer, T.H., Phys.Rev. **182**,1374 (1969); **186**,1304 (1969)
18. Tersoff, J. and D Bayer, Phys.Rev.Lett. **50**,553 (1983)
19. Holton, G., *Thematic Origins of Scientific Thought* (Harvard Un. Press) p.376.

Chapter XVIII

1. Sagnac, G., Compt. Rend. **157**,708, 1410 (1913)
2. Schouten, J.A., *The Ricci Calculus* (Springer, Berlin 1954)
3. Poeverlein, H., Phys.Rev. **128**, 956 (1962)
4. Goldstein, H., *Classical Mechanics* (Addison-Wesley, Reading, 1965)
5. Post, E.J., Revs.Mod.Phys. **39**,475 (1967)
6. Langevin, P., Compt. Rend. **173**,831 (1921)
7. Kretschmann, E., Ann. der Phys. **53**,575 (1917)
8. Bridgman, P.W., *Nature of Physical Theory* (Princeton '36) p.80.

9. Einstein, A., Ann. der Phys. **55**,241 (1918)
10. Whittaker, E., *History of Theories of Aether and Electricity* (London 1951) Vol.II, pp.191-196.

Epilogue for Copenhagen's Extrapolation

1. Kline, M., *Mathematical Thought from Ancient to Modern Times,* (Oxford University Press, 1972) Vol.3, p.1091.
2. Gottfried, K., Physics World, Oct. 34 (1991)
3. Feynman, R.P. and A.R. Hibbs, *Quantum Mechanics and Path Integrals,*

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