The Review of Metaphysics Volume 33 Issue 1 / September1979

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Compton on the Philosophy of Nature

EVEN in Aristotle's day, there were some problems about the status of the "mixed sciences," mechanics, optics, astronomy, harmonics. They were mathematical in form, and depended on generalizations drawn from repeated and careful observation. In both respects they differed from "physics," as Aristotle saw it; he made them "the most physical part of mathematics," and thus inaugurated a long two-thousand year history of separation between two ways of approach to nature, the philosophical (which was usually assumed to be more basic because it aimed at coherent explanation), and the mathematical (which provided the predictive account needed for practical ends in navigation, calendar-making, machine-construction, and the like). Galileo's central achievement, perhaps, was to end this split; he showed that a single account was possible (though he himself was not the one who finally gave it). Though he claimed the titles both of "Philosopher" and of "Mathematician" to the Grand Duke of Tuscany, the "new science" he proposed in 1638 would not need two separate talents. Mechanics for a while to come would still be classified as "natural philosophy," but it would rapidly become evident that philosophers could no longer claim the competence to intervene in it on their own account. True, some of the first great exponents of the new physics, Descartes, Newton, Leibniz, were also philosophers. But it was abundantly clear that the new science of nature was mathematical-experimental in form, not "philosophical" as that narrowed term had come to be understood.

This splitting-off from philosophy of an autonomous new science (which had been presaged in the earlier separation between "mathematicians" and philosophers) posed two immediate challenges for philosophers. One was to determine whether there was still some properly "philosophical" mode of approach to nature. Could philosophers still lay claim to their own "science" of nature, and if so, how did it relate to

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Galileo's science? And even more basic, did the quantitative categories of Galilean science allow room for the presence of man in the world? Could the new science of nature accommodate *human* nature? Clearly, it had to. But *how*? Much of the history of philosophy in the centuries that followed focused about this latter challenge. And occasionally there would be those, like Kant, who would attempt a response to the former one also, constructing a new "philosophy of nature."

Compton's paper, "Reinventing the Philosophy of Nature,"[1] returns once again to these same two challenges, made even more insistent by the enormous successes of "Galilean" science in our own time. He argues, first, for the need to "reinvent" an authentic philosophy of nature as a necessary complement to the immense array of technical and often mystifying sciences that confront us. He argues, second, that this philosophy of nature ought to have as central preoccupation the securing of a proper place for man in nature; this is indeed its "motivating problem." He suggests that this may require us to construe nature as a "hierarchy of forms of life-world" (sec. 7). This last term is intended to signal us that he takes the phenomenological approach, as found in the works of Husserl and Merleau-Ponty, to provide the needed clue as to how all of this is to be brought about.

It should be noted that his proposal is a promissory one. He is not claiming that there already exists a philosophy of nature which needs to be situated relatively to other fields. He is not claiming to have constructed such a philosophy of nature; indeed, the entire tone of his essay is very tentative: "it might be that there is some non-discipline-specific, prescientific, primary knowledge of nature" (introd.). Philosophy of nature, after a long lapse, now needs to be reinvented, he urges, and he goes on to give some hints as to how one might set about doing this. It is difficult to assess such a proposal; until the philosophy of nature is actually reinvented, it will

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be impossible to tell whether the quite specific difficulties that caused the abandonment of this terrain in the past have been successfully overcome.

I

One hesitation I should voice from the outset. How serious is Compton in his employment of the phenomenological approach? Do his arguments really depend on specifically phenomenological principles? Or is he simply using the code-terms, inserting, for example, 'lived' as a prefix where possible? There is, of course, an ample precedent for the claim that philosophy of nature must look to phenomenology for its reinstatement. This was the theme of Husserl's last work, *The Crisis of European Sciences and Transcendental Phenomenology* (1954). In the well-known section on "Galileo's Mathematization of Nature," one subheading summarized his thesis (and what sounds like Compton's also): "The Life-world as the Forgotten Meaning-fundament of Natural Science."

I am sceptical, however, of the help that may be expected from this quarter. There have, indeed, been attempts before to build a philosophy of nature along the lines suggested in *The Crisis*. But none of these efforts, so far as I know, have succeeded. The reasons for this are doubtless many, but two quite basic ones are easily pointed out. Husserl shared the generally positivist understanding of natural science prevalent in the middle Europe of his day. What counts most for the scientist (he writes) are the abstract "formulae" which extend the range of the "possible deductive conclusions concerning new facts of quantified nature."[2] These formulae, when interpreted, express laws or functional correlations between measurable quantities. Such laws make accurate prediction and practical control possible; science thus progresses as the approximations improve. Husserl differed

from the positivists on the sort of idealization that the measure-quantities themselves entailed: though physics began originally from an intuited pre-given nature, there has been a progressive "transformation of its experiencing, discovering way of thinking," and a consequent "covering-up" of the meaning of the terms used and of the endeavor as a whole.[3] But he sounds very like Carnap as he describes science in terms of a "cooperative interplay" between the mathematician who provides the abstract formal calculus, and the experimentalist who uses this calculus as a means of summarizing and extending his results.

This view of science would by now be quite generally rejected, for reasons that are so well-known as scarcely to need listing. Instead of seeing science as a set of *laws*, philosophers today would stress the centrality of *theory*, of explanation rather than functional correlation. And theory ordinarily involves postulated theoretical entities; the consequent partial insight into the structure of the real constitutes the primary explanatory aspect of natural science. The work of science cannot be neatly divided into a mathematical part taken to be the extension of an uninterpreted *mathesis universalis* (a "science of the forms of meaning of the something-in-general which can be constructed in pure thought"[4]), and an experimental part which consists in the recording of empirical regularities. The explanatory models of the scientists are *not* purely mathematical; they are physical, and they are developed for the most part by means of physical imagination that differs in quite basic ways from that of the pure mathematician.

And this recalls a second defect in Husserl's presentation of "Galilean science." He assumes first that mechanics can properly be taken to represent the nature of the new science, and second that this mechanics can properly be regarded as a "mathematization" of nature. He is wrong in both respects, although his error is one that was shared by many of the protagonists of the new science and for that matter by some modern philosophers. Mechanics turns out to be a singularly *bad*

paradigm, because it does not involve the sort of structural explanation that is characteristic of chemistry, of biology, of geology, indeed of virtually all other parts of natural science.[5] Mechanics does not progress by postulating theoretical structures whose nature is gradually made more precise. The ontological status of its central constructs, force and energy, remains quite enigmatic. It thus lends itself to the positivist reading given it by Mach, the reading which splits it conveniently into formal calculus and experimental results.

Nor can even Galileo's achievement in mechanics be safely described as "geometrical idealization," as a "mathematization of nature."[6] These descriptions of it (supported by Galileo's own memorable aphorism about the language of the Book of Nature) enabled Husserl to argue that "immediately with Galileo, then, begins the surreptitious substitution of idealized intuited nature," and to assert that in "the world of actually experiencing intuition…the world in which we ourselves live…we find nothing of geometrical idealities, no geometrical space or mathematical time with all their shapes."[7]

Much could be said of this way of contrasting the "real" world of prescientific experience with the "abstract" world of the scientists. But it will have to suffice here to say that one of the suppositions from which it takes its origin is false. Galileo did, it is true, use a geometric formalism to represent the uniform acceleration of free fall. But this did not automatically make the concepts of space and time utilized in mechanics into geometric concepts. Because he was able to omit the concepts of mass and force from his simplified descriptive account of motion, he could be pardoned for supposing (if he did suppose) that the resources of geometry might also suffice

for dynamics.[8] But Newton's concepts of mass and force were surely *not* geometrical: indeed, though he named his book *The Mathematical Principles of Natural Philosophy*, the principles of his work were *not* purely mathematical, but were rather only "mathematical" in the loose sense in which that term had been used in the earlier mixed-science tradition. That is, though Newton did develop a successful mathematical formalism in which such physical quantities as force could be represented algebraically, all that this meant was that the syntactic relations between the force, mass, length, time, symbols could be specified algebraically, enabling computation to be made. It did not mean that the concept of force somehow became an algebraic concept, as though an understanding of algebra would suffice to enable one to understand what the concept of force means in the *Principia*. The *semantics* of the concept was still a physical matter, as Newton very well knew and as the complicated later history of the concept amply testifies.

Though a much more detailed argument would be needed, perhaps enough has been said to warrant the claim that the founder of phenomenology based his critical analysis of contemporary science on philosophical and historical suppositions that are open to serious question. After this detour, let me return now to Compton's "phenomenological" philosophy of nature. The shortcomings of Husserl's approach are relevant here, since Compton claims him as patron. But, it might be said, why should the sins of the fathers be visited on their children in this case? Compton goes out of his way to defend a realistic account of theoretical entities. They have, he argues, a "nature-revealing and penetrating power, and not merely an instrumental, predictive value" (sec. 3). This is, as he himself notes (n. 8), at odds with the broadly instrumentalist orientation of Husserl who sees physical science as "but one among the many practical hypotheses and projects which make up the life of human beings in this life-world."[9] Furthermore, Compton argues that

there is a fundamental continuity between the forms of thinking characteristic of scientists and those of the life-world generally. Husserl takes the opposite view, as do most later phenomenologists who have written on this topic.

Now, of course, Compton knows this perfectly well and can claim that the title 'phenomenologist' ought not be restricted to those who follow Husserl to the letter. But the differences between his approach and the traditional phenomenological one are sufficiently great to lead one to question whether the philosophy underlying this proposal *is* best understood within the context of traditional phenomenology. Might one, in fact, not find insights similar to his among epistemologists of other persuasions who do not share the burden of Husserl's account of natural science in *The Crisis*, who indeed defend the very positions (realism of theoretical entities, continuity between prescientific and scientific forms of reasoning) that Compton regards as central to a sound philosophy of science? This is a question the reader ought keep in mind in the discussion that follows.

It will be convenient to take Compton's argument in three stages. First, there is the question of how the relationship to science of his reinvented philosophy of nature is to be described. One has then to characterize the sources and the aims of philosophy of nature in a more specific way. Finally, he proposes for discussion a special "coherence" condition: an adequate philosophy of nature must "take seriously the experienced presence of human reality within nature" (sec. 6), and perhaps even go on to establish the existence of "life-world structures" in nature at *every* level, even that of the elementary particle.

II

At least three different views of the relationship of "philosophy of nature" to the enterprise of natural science can be found among modern writers.[10] Compton appears

sympathetic to each of the three in turn. Yet it would seem that they are ultimately incompatible with one another. We shall have to press, therefore, and ask which of the three he finally chooses.

The Identity Thesis: This makes the weakest claim of the three. It asserts that natural science is also, under some construal, the philosophy of nature. There is no separate philosophic account of nature, then, with its own distinctive questions and principles. Rather the two are identical. Why, not, then, say that philosophy of nature has been replaced by science, and thus no longer exists in its own right? Are there any conditions on natural science's qualifying as a philosophy of nature? Two suggest themselves. One is that natural science itself must be constructed in a realistic manner, i.e., its theoretical constructs must be taken to give a genuine insight into the structures of nature. The second is that natural science must be presumed to be the *only* source of insight into these structures. If there are some aspects of nature in principle inaccessible to science but accessible somehow to philosophy, the two obviously cannot be identical. The identity thesis is favored by scientists like Heisenberg and Schrödinger and by philosophers like Dewey, who see contemporary science as an extension of the older natural philosophy, and are skeptical of the suggestion that there is a philosophy of nature which stands over against science and judges it.

Compton begins by affirming the identity thesis:

We must accept the view that natural science, in its deepest sense, is a philosophical inquiry into the fundamental entities, processes and structural relations of the natural world. Rightly interpreted, it *is* the philosophy of nature (intro.).

Later, he urges that:

There is but one world, the lived world, in which scientific activity takes place and to the understanding of which it is historically devoted.... It is only on such a view, I believe, that

the intelligibility of natural science as natural philosophy can be made clear (sec. 3).

The task of the philosopher, he suggests, is to defend scientific realism, and to develop criteria which will relate the theoretical entities of the scientist to the familiar entities of the life-world. But is this not the concern of the philosopher of science? Is a separate philosophy of nature what is called for? And if natural science and natural philosophy are identical, surely this identifies the "philosopher of nature" with the scientist?

The Priority Thesis: The "classical" view of the matter was that there is a philosophy of nature which is prior to, and normative of, natural science. This was implicit in the Aristotelian placing of "physics" as a source of intermediate principles for such "mixed" sciences as astronomy or optics. It was explicit in Descartes, Kant, Hegel, though each of these went about the task of ensuring the priority of philosophy of nature in a quite different way to the others. All three ways in which they proposed to do this have been challenged, and would find relatively few defenders today.

Husserl is quite convinced of the priority thesis, and feels that it has never really had a chance until now:

There has never been a scientific inquiry into the way in which the life-world constantly functions as subsoil, into how its manifold prelogical validities act as grounds for the logical ones, for theoretical truths.[11]

The sciences take the life-world for granted. But to use it in this way "is not to know it scientifically in its own being."[12] It must be made "an independent, totally self-sufficient subject of investigation";[13] it is the task of the phenomenologist to discern its structures, structures (like spatiality and causality) which prefigure and serve as the indispensable ground for the abstract structures of the scientists. At a more detailed level, he will focus on "essences." The classical distinction between substance and property is set aside; each color, for example, has an essence which can be grasped in

consciousness, because the essence of a color (like the essence of any other object of consciousness) is contained in the experience of that color. This is the priority thesis in a quite radical form.

Compton argues that the philosophy of nature enunciates certain conditions that govern any knowledge of nature; these conditions "do not simply stem from the sciences of nature" and they are normative in regard to these sciences (intro.). Sounding like Husserl, he goes on to say that there is "a primary, pretheoretical experience of nature" (sec. 1) which can and must be elaborated in advance, and which serves as ground for the later work of the scientist. It is thus a source of "methodological constraints" on that work (sec. 3). And he is serious about this notion of prior constraints. The "world-variations" proposed in the theoretical models of the scientist "must have *some* limits"; the models themselves "must share *some* structural features with perceived realities if they genuinely are to be taken to specify aspects, parts, or structures of the world" (sec. 4).

So the philosophy of nature specifies certain minimum conditions which the scientist *must* respect in his model-making. This is to make it prior and normative in the fullest sense. It is not just the philosopher of science who needs to take a "philosophy of nature" into account in order to develop an adequate philosophic treatment of the realism issue (the suggestion we have seen to be allied with Compton's version of the identity thesis). Here it is the *scientist* who has to be governed by norms elaborated in advance for him by the philosopher. And the norms are not just logical in character; they are substantive categorial principles about the sort of concept-systems he can employ.

Suppose he disobeys? Compton's response is that this would force science in the direction of instrumentalism (sec. 4). But what if the scientist were to say: well, what of it? Perhaps this is how science *must* ultimately go. Heisenberg, for one, has for years been advocating a "Pythagorean" approach (as he calls it) to elementary-particle physics which would be instrumentalist, in many of its implications. It is, he admits, not what the "Democritean" physics of recent centuries would have led one to expect. But, he insists, it is

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inevitable given the fact that the entities of theoretical physics no longer obey even the minimal "reality" conditions drawn from the macro-entities of prescientific experience.[14]

Heisenberg and Compton thus agree in tying realism to a set of minimal conditions specifiable a priori by the philosopher. But where Compton assumes that realism *cannot* be given up, and that these conditions are therefore normative on present and future science, Heisenberg's approach is just the opposite. He argues that the conditions *cannot* be maintained, indeed that they have been violated for decades past in elementary-particle physics, and hence that this constitutes a *prima facie* case for the abandonment of "particle" realism in microphysics.

This issue has been central to recent discussions of realism by Hesse, Putnam, Sellars, and others. It is much too complex to take up here. But it is important to see its relevance to the "priority thesis," as Compton construes it. Take the "object-condition," as he defines it: "objects are identified and reidentified as typical unities presented through multiple aspects over time" (sec. 2). This is derived from an analysis of our prescientific experience of nature, and then presented as a condition which should also govern the theoretical entities of microphysics. But *are* electrons and photons reidentifiable? They do not obey Boltzmann statistics, the classical formalism governing reidentifiable objects. Physicists do speak of the "same" electron as causing the series of events constituting an ionization track in a cloud chamber. But reidentifiability seems quite clearly not to be a general condition one can impose on the entities of contemporary microphysics. And the forces and fields which Compton also mentions in this context raise (sec. 3) even more severe problems.

There are five options here. One is to say that this development in recent physics is, despite appearances, compatible with Compton's criteria. The second is to hold it to be

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illegitimate. The third is to suppose that Compton may have misidentified the necessary conditions that "restrain the reconstructive inventiveness of scientific thought," conditions that retain for its constructs "the nature-revealing and penetrating power we typically believe that they have" (sec. 3). The fourth is to conclude that this "typical belief" of ours is one that we must give up, in the light of recent scientific developments. The fifth is to argue that the conditions for realism need *not* be derived from an analysis of our everyday experience of macro-objects, as Compton assumes.

It is this last that I would lean to. Much depends on the precise structure that one assigns to the argument for scientific realism. Compton supposes that it runs as follows: (1) one identifies in advance the conceptual conditions for a realistic interpretation of a particular conceptual framework; (2) one notes that these conditions are *in fact* satisfied by contemporary micro-physics; (3) one concludes that this latter can then be interpreted realistically. Of course, if at any time (2) *ceases* to be satisfied, one seems forced either to instrumentalism or to the assertion that scientific conceptualization has gone wrong.

I would prefer to maintain: (1) the historical development of specific scientific theories can only be understood by supposing that the structures posited by them have some sort of "fit" with the real world which the scientist is attempting to understand; (2) the most general categories appropriate to the real cannot be known with assurance in advance, but can only be identified by reflection on the knowledge that is most successful in explaining the characteristic features of the level at which one is working. What allows one to know that it is the real one is coming to, therefore, is not that one knows in advance the criteria that distinguish knowledge of the real from purely instrumental knowledge. Rather, the argument is: the presumption that it is the real (*however* conceptually determined) towards which theoretical progress tends, is the *only* one that makes intelligible the course of this development (as we discover it empirically through historical records).

Compton's objection to this would presumably be that this is to allow an "arbitrariness" (sec. 3) in terms of the

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familiar categories of the everyday to characterize the constructs of the scientist, and thus to heighten the separation between the two. I am inclined to think that there is no way to prevent this. Indeed, I am prepared to argue that there was every reason to *expect* something like this to happen, though the entire structure of assumptions surrounding early modern science (Newton's Third Rule of Reasoning, the distinction between primary and secondary qualities, etc.) was designed to exclude all thought of it.

The only way, in the end, to set up strictly regulative antecedent conditions would be in the manner of Kant, who attempted to determine the conditions of possibility of an experience of nature and to demonstrate the "real possibility" of the (Newtonian) concept of matter. The conditions he specified (involving categories like substance, cause, space, time) proved much too restrictive, as Newton's physics yielded way to Einstein's.

Compton agrees with Kant that to be real is to be a possible object of experience, and hence that an analysis of the conditions of possibility of experience should also yield the criteria appropriate to the realist claim about the theoretical entities of science. The conditions he produces are not unlike Kant's; indeed he feels he has unearthed the "enduring experiential meaning of Kant's insights." They are just "more amorphous," "more schematic and flexible" than Kant realized (sec. 5).

Is he opting, then, for a neo-Kantian version of the priority thesis? The answer is: no. One reason is obvious. His conditions come, he says, from combining "direct experiential self-reflection" with such interpretive human sciences as anthropology and child-psychology (sec. 2). This seems to me an uneasy combination, one that potentially might not at all reduce to the simple "life-world" of prescientific experience to which he also appeals on occasion. But in any event, this is remote from the Kant of the *Critique*. And the amorphous conditions he comes up with, and the hesitant assent he gives to them (sec. 2), reflect the uncertain character of the ground on which he is working.

A second reason why his approach could not be characterized as neo-Kantian is that he finally concedes that the

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conditions constituting his philosophy of nature are not of the necessary kind that Kant insisted upon. They are revisable, modifiable in the light of advances in science. This altogether alters their status, which brings us to the third possible way of aligning philosophy of nature and natural science.

The Dialectic Thesis: We may suppose that philosophy of nature, though having some kind of independent status, does not enjoy absolute priority and the normative rights that would accompany this. Rather, each side can influence the other; each side progresses in a sort of "dialectic" with the other. Thus, the scientist would be bound to take the philosophical principles derived from prescientific experience of the life-world seriously, but he would not be absolutely governed by them. Thus, if a Hoyle calls for continuous creation of matter or a Feynman for time-reversal, he must be prepared for opposition from the philosopher, and must make a rather stronger case both conceptually and empirically than he would ordinarily be required to do. But on the other hand, the philosopher must be ready to modify his own principles in the light of the long-term success in science of a principle at first thought to be unacceptable.

This is a most unphenomenological move, but Compton feels forced to make it. If there is a tension between our experience of "intuitively lived nature and our theoretical reconstructions of it," neither side can be entirely sacrificed. They must somehow be made to "converge" (sec. 5). And this may mean that we will have to revise descriptions of the original experience of nature with which we began. "In the process, our understanding of the criteria of natural reality, implicit in primary experience, itself evolves." As an example of the consequent "forced reexamination of the primary experience of nature," Compton mentions the rejection of many features of Aristotle's "physics of the world of primary experience" by the physicists of the seventeenth century.

Now the difficulty about this "revisionist" approach is that if descriptions of our primary experience are modified in the light of later developments in theoretical science, they no longer testify to primary experience alone, which was

supposed to be the source of their warrant for the scientist. If our intuitions on simultaneity at a distance are altered by the Special Theory of Relativity, for example, can they any longer be said to carry weight in their *own* right? Does their authority not derive now from the STR? Once one allows the testimony of primary experience to *yield* to the findings of science, the normative force attributed to this experience by those philosophers who recognize a prior philosophy of nature seems to vanish. Science serves as corrective, gradually sharpening, correcting, and ultimately warranting the intuitive principles derived from our prescientific experience of nature. This in the end reduces to something very like the identity thesis.

The main problem with the dialectic thesis, then, is that it dilutes the authority attributed to "primary experience" by the priority thesis, making it difficult in consequence to discover what sort of warrant this experience is supposed to possess, and from whence this warrant derives. The dialectic thesis has undoubted appeal nevertheless, because it attempts to take both sides seriously. It suggests that the scientist has got to allow some normative weight to the prescientific certitudes of the life-world, precisely in their character *as* "primary." And philosophy of nature itself becomes a progressive enterprise, as the intuitive insights of the life-world are modified in the light of advances in science. Despite its appeal, however, it must be said that this is epistemologically shaky ground. Dividing authority nearly always creates new problems.

It is not my intention to explore these problems here. The point of the preceding has been to indicate that three different ways of relating philosophy of nature to natural science are found in Compton's paper, and to argue that they are in the end not mutually compatible. One must opt for one or the other. A "pure" phenomenologist would prefer the priority thesis. Most scientists would defend the identity thesis. Compton seems to lean more to the dialectic thesis. Though it offers an attractive compromise, it lacks, however, the epistemological clarity that the other positions possess. In order to defend it, one must develop in some detail an

epistemology which will show just why, and to what extent, the insights of the life-world should inform the work of the scientists.

III

Compton has some interesting suggestions to offer in this regard. He stresses, in particular, the *continuity* of scientific knowledge of the life-world. But instead of seeing the continuity as basically one of meaning as Husserl does, a claim that is open to obvious objection,[15] he finds three other more sophisticated ways of arguing for a close connection between science and life-world.

Reality criteria: The first of these we have already noted. The criteria of objectivity which ought to govern the discussion of the "reality" of the theoretical entities postulated by the physicist must be the same as, or at the very least be derived from, the criteria of objectivity "implicit in the perceptual encounter with things" (sec. 3). It is from this that they take their warrant. He finds that physicists do indeed utilize criteria of the expected sort, and regards this as "conclusive evidence" for the continuity of science with prescientific knowledge. Is he claiming that physicists *must* use such criteria (though in an "attenuated" form perhaps, sec. 4), or that they do *in fact* use such criteria? The philosophical implications of these alternatives are very different, as we have seen.

One feature of this argument is that it presents science as a new way of representing perceived objects, one which satisfies "the demands for predictability and regularity implicit in perception" (sec. 3). What science does, then, is to "enrich the content of perception by suggesting new observational means of access to otherwise hidden entities." There is a continuum, therefore, from the macro-objects of perception down to the micro-objects postulated by theory, which still can be said to be in principle "observable" in an enlarged

sense of this term. (This directly opposes Husserl's claim that the theoretical constructs of science are reached by a form of idealization and are "in principle not perceivable.")[16]

The question of reality-criteria is surely a philosophical, not a scientific one, as Compton maintains; the scientist does not *need* to ask "reality" questions about his constructs for the pursuance of his own goals. Newton attempted to set down such criteria in his Third Rule, which required that all bodies, even the imperceptible corpuscles of matter-theory, share the primary qualities of the bodies of immediate experience (extension, hardness, impenetrability, mobility, and inertia).[17] The shortcomings of this rule illustrate the danger to which this effort to specify definitive reality-criteria is prone. It is much too restrictive, and it assumes that notions like extension are somehow fully given in experience. Compton suggests as criteria: capacity for interaction; source of unexpected perspectives; constancy or unity through time. These are much broader than Newton's, and are not subject to the same objections. They are presented as very general conditions which would have to be further specified by the scientist in terms of his own framework. The question of what counts as genuine interaction, for example, was a matter of deep disagreement between Newton and his critics, and the debate around Mach's principle in present-day cosmology shows that this issue is still an unsettled one.

But the central issue in that of the *warrant* on which such proposed criteria should rest. Is it that these criteria have been found to govern the entities of primary experience? But then why *these* particular criteria rather than the many others satisfied by these entities? Or is it that metaphysical reflection on various potential criteria suggests which of these might plausibly qualify as minimal conditions for a physically real entity? Such criteria could in the first place be prompted by the experience of the scientist as well as by reflection on perceptual encounter. The fertility criterion so

important in the long-range evaluation of theory might, for instance, hint at something like the perspectival "reality" criterion mentioned by Compton. The *source* of such suggestions is not what counts. It is the coherent account of physical nature that the question requires one to formulate. What is at issue here is the extent to which this account must derive specifically from "life-world" considerations. It is from these that we first learn how to tell what is real from what is not. But whether we can by an analysis of the life-world alone attain "the goal of a truth about objects which is unconditionally valid for all subjects," "fix once and for all and in a way accessible to all" the criteria of objecthood, as Husserl supposes,[18] seems very doubtful. What *is* clear (and this after all is Compton's main point) is that the issue itself has been insufficiently studied by those who have recently made scientific realism the focus of their concern.

Forms of rationality: The second "continuity" claim made by Compton has to do with the forms of rationality characteristic of the life-world and of science.

Although natural scientific thinking is carried on in special communities and in response to special demands of a constructive method, it nonetheless recapitulates and refines the forms of prescientific, embodied, intersubjective experiences of the natural world (sec. 3).

More specifically, the roles played by abstraction, hypothesis, experiment, analogy, and so on, in science must be seen to be "continuous with ordinary observing, manipulating, typifying, and projecting."

Once again, this is diametrically opposite to Husserl's insistence that the two forms of rationality are to be treated as in most respects contrasting, not as continuous. To what extent *may* we suppose that the complex rational procedures of modern natural science are a "natural" outgrowth of the procedures of the life-world? This is a very complex question, one which has deeply divided recent philosophers of science. The customary view in the past was that "scientific method" was a unique achievement of the age of Galileo and Newton,

the achievement which most definitively constituted that age as one of "revolution." Though this view is still enshrined in the introduction to our science textbooks, few would defend it today in its original simple form. The roots of scientific method are now sought in the axiomatic methods of Archimedes and Euclid, in the Aristotelian theory of demonstration, in the idealizations of fourteenth century Paris or Oxford, in the technical practices of the engineer, the physicians, the alchemists.

On the other side it is noted that the grasp on scientific rationality in the seventeenth century was far from sure, that for example the goal of science was still taken to be the "eternal and necessary truth" which Aristotle had sought, so that the roles of hypothesis and approximation were not yet fully appreciated. The evolution of contemporary scientific rationality has thus been a laborious one, in which outcomes could never have been predicted in advance.

What would Aristotle have thought of the suggestion that accurate prediction is the first testimony to the value of a theory? Not much, one suspects. How would Newton have reacted to the assertion that the laws of motion must be conceded to be permanently provisional, ever-capable of further conceptual revision? Not very favorably, one may be sure.

Our scientific rationality is so hard-won that we are inclined to view with some skepticism the claim that it is a "natural" development from commonsense modes of rationality. But much depends on the import given the term ‘natural'. After all, the human organism can be regarded as a "natural" development from the first reptile that crawled on land some hundreds of millions of years ago. No one would say that this development was a predictable one, an unrolling of logical consequences. The part played by environment was as great as that of the original reptilian DNA. But it assuredly was a *natural* development. How far ought this analogy between biological evolution and the evolution of scientific rationality be taken? Very far indeed, Toulmin says. Others note a greater constancy in the goal of intelligence than in organic goals generally, and argue that the development of scientific rationality shows a "progressive" or a "convergent" character which is lacking in biological evolution.

These are difficult issues.[19] Our concern here is with the more limited (though still complex) theme proposed by Compton: Science, in "its fundamental rationality" is "rooted in prescientific knowledge of nature" (sec. 5). There are once again two ways in which the question he puts might be understood. First, to what extent do the forms of scientific rationality in fact recapitulate prescientific modes of understanding and proof? Second, to what extent *must* they? Or to put the second one differently: to what extent does scientific method rest upon an implicit reference to earlier prescientific procedures? I would be inclined to argue that the authority of present ways of "doing science" comes not from a presumed recapitulation of, or affinity with, more intuitive forms of rationality but from their acknowledged success in accomplishing the tasks that scientists set themselves. And in regard to the first question above, it must be said that even in Aristotle's day there were elements in the "doing" of science (think of the specification of necessary first principles) that were—here Husserl was right-quite unlike anything to be found in the simpler procedures of the life-world.

To speak of "success" obviously raises new questions, however, since our understanding of what counts as the tasks of science, as well as what would count as success in achieving them, has developed and presumably continues to develop. Compton might be taken to be saying that no matter how far this development should go, there never could come a time at which the original forms of rationality of the life-world would turn out to be invalid. But even this is risky because certain of these original forms (reliance on tradition or on the "given" language, for example) *would* be called into question by those imbued with latter-day scientific rationality. Of course, one might say that these were, to begin with, not genuine parts of the basic prescientific rationality we are attempting to retain as somehow foundational. But then once

again comes the by-now familiar complaint that we are apparently determining that these are not integral to "true rationality" by the very fact that they were long ago undermined by the growth precisely of scientific rationality. So the supposedly normative reference to a prescientific "basic" rationality turns out to be (or at the very least, runs the risk of being) spurious. This allegedly basic rationality has itself been altered; however we are to construe this process, it is not really one in which a pregiven rationality serves as foundation of, or norm for, what comes after.

The Perceptual Basis: The last, and perhaps most widely-shared, of Compton's "continuity" claims is that science is "rooted for its evidence in prescientific knowledge of nature" (sec. 5) that it "is under constraint to explicate, not to negate, primary experience" (sec. 6). This is a theme dear to Husserl, who insists that life-world experience functions for the scientist:

not as something irrelevant that must be passed through but as that which ultimately grounds the theoretical-logical ontic validity [of science] for all objective verification, i.e. as the source of self-evidence, the source of verification.[20]

There are at least two ways in which this sort of claim might be taken. The first is that science *must* rely upon the validity of the observations cited as evidence for its theories. Put in another way, science *could* not validly call in question the general perceptual abilities which allow the scientist to register pointer-readings, classify kinds, and so on. For this would be to cut off the very branch from which science itself hangs. The overall reliability of our perceptions of such parts of the world as measure-instruments is a presupposition of science, and hence cannot itself be denied on the grounds of science. This point was frequently stressed by Bohr in the debates that went on in the 1930s around the implications of the quantum theory of matter. And it is clearly valid. One outcome of the development of science that we can confidently exclude is that our perceptions of the world could turn out on *scientific* grounds to be basically unreliable.

But the import of this is more limited than at first sight it

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seems to be. In particular, it would not suffice as warrant for holding that science serves to "explicate primary experience." To assert this would be to make a stronger and much more vulnerable claim. The "observational" language of modern natural science is no longer the language of "primary experience." The "observations" are made in almost all cases by instruments of one or other sort; the only role that perception plays is, therefore, in the reading of the instrument. This is why Hanson, when trying to construe scientific observation after the manner of perceptual (and "theory-laden") *gestalt*, took as paradigm of scientific "observation" the biologist's identification of a micro-organism on a slide. But this is not at all typical of the sort of "observation" on which physics and chemistry and genetics depend. Indeed, the trend over several centuries has been to reduce reliance on this sort of skill-based recognition as far as possible.

What science explains, then, is not life-world experience but rather a set of artfully contrived measures, many of them of properties which are in principle not perceptible to us directly. It is true that the qualitative categories in terms of which we perceive our world must somehow be accounted for by science. The dispositions in physical bodies to bring about, under suitable conditions, certain perceptions in us must be integrated into our general understanding of the natures of these bodies. But this is by no means the same as to say that the aim of science is to explain our primary prescientific experience of the life-world. This would be far too limiting, as Galileo long ago pointed out.

Many philosophers have urged a stronger claim: that the language of science can in principle *replace* the language of ordinary experience and that there is in consequence nothing "primary" about this latter. This claim has taken many forms. Note that the point of replacing one language with the other is not that the first is unreliable; the motive is not to challenge the veridical character of perception or its usefulness for everyday purposes. Rather what is claimed is that the familiar language of perception does not lend itself to *science*, and thus that if we want to express ourselves in a way which does, we have to import back into perceptual experience categories which have already been tested in the

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scientific framework and which can be shown to be equivalent in the relevant ways to the qualitative categories of perception. This is, of course, a controversial position. If it is correct it would tell heavily against the sort of continuity that Compton is defending.

In this section, we have followed Compton's lead to three clusters of problems that arise when one tries to specify more exactly just what the relationship is between science and everyday experience. The first set would be generally-regarded as metaphysical; the second is widely discussed in recent philosophy of science; the third has involved epistemologists, in particular. What Compton would like is to group them together under a single roof and call them "philosophy of nature." They *could* also be taken to belong to philosophy of science. But his claim is that they require attention to the detail, not of nature as such, so much as of prescientific experience. We have seen some reasons to question this. The discussion of reality-criteria comes closest, perhaps, to his ideal, but even there the "world-variations" required in order to determine the minimum reality-conditions are more reminiscent of metaphysics than of philosophy of nature, as traditionally understood. The "life-world" emphasis of phenomenology directs attention not so much to nature as to nature-as-experienced and ultimately to the experiencer. Thus it comes as no surprise when in the last section of his essay Compton takes the return to the "philosophy of nature" that he is advocating to be concerned centrally with *human* nature, and specifically with the safeguarding of the reality of man in the face of science.

IV

In the *Phaedo*, Socrates is portrayed as distrustful of the direction that the physics of his day had taken because it appeared to leave no place for the most basic human perceptions of the world in terms of purpose and the good. The impact on philosophers of the "new science" of the seventeenth century was very similar. All that was distinctly human in the world appeared to be called in question. The *real* qualities of the

world were those which made it subject to mechanics; perceptual qualities were "secondary," not *really* there. Teleological modes of explanation appropriate to human action no longer were acceptable in the science of nature. Most serious of all, man himself was coming to be regarded as differing only in physical complexity from other natural beings and as open, therefore, to scientific explanation in the categories common to all of nature. These problems, originating as they did from the science of Galileo and Newton, have dominated philosophy from then till now. Compton believes that since they have a common origin in the science of nature, they should be brought under the single broad title of "philosophy of nature."

Eliminative reduction: His first concern is a simple one: "no view of nature will be counted adequate which does not permit coherent inclusion of human reality, as it experiences itself, within nature" (sec. 6). He takes this to rule out "one persistently recurring way of making sense of man in nature," namely that of the reductionist who would "eliminate human life and the life-world from nature." Are there really philosophers or scientists who set out to give an account of nature in which there can be no place for man? If this were what eliminative reductionism amounted to, it would hardly constitute much of a threat. Even classical materialists like Hobbes and La Mettrie would not have thought of themselves as eliminating man from nature; their aim was to *explain* man in a scientifically more promising language than the everyday one.

Who, then, are the targets of Compton's criticism here? What makes someone an "eliminative reductionist"? Several different answers may be given. Someone may propose a conceptual system in which the reality of some of the distinctively human features of the world: intention, meaning, or quality, say, is explicitly diminished. They might be described as "epiphenomenal" for example, not significant for a properly scientific account of the world. A second possibility is that someone may propose to explain the human dimensions of the world in terms of physical categories assumed to be basic; others may then adjudge his system to be inadequate to the reductive task, so that the imposition of the

system would *equivalently* eliminate all or part of the life-world even though this had not been the intent of the original proponent. It is important to notice that in the latter case, the "eliminative" label would not be accepted by the proponent; it is his *critics* who say that his system is in effect eliminative.

When someone like Monod or Crick asserts that the entire functioning of man can be explained in physio-chemical terms, he is obviously making an "in principle" claim. He does not mean that this reduction can be performed here and now, or that no further development in physics and chemistry would be needed in order to carry it out. What he intends to claim is that there is no barrier in principle to a complete explanation of man on the part of a science substantially like our present-day sciences of physics and chemistry. To call him an "eliminative reductionist" would be to say either that he explicitly eliminates such aspects of human experience as freedom on the grounds that it cannot be fitted into a scientific world-view, or that some acknowledged features of human experience can be shown to be in principle incapable of reduction by a system of the sort proposed, despite the claims made for it. Either way, the assertions of the reductionist could be said to "conflict with the way we experience ourselves in nature" (sec. 6), which makes them "eliminative" as that term is used here.

Compton reminds us that the world has men in it not "as it has molecules but as world-experiencers, world-interpreters, world-makers" (sec. 6). The demand that this lays on science is that it should in the long run take into account the distinctively human capacities for experience, interpretation, and creation. It does not require that science should retain the *language* of the life-world; to "reduce" this is not necessarily to eliminate the realities designated by it. Compton appears to envisage that the "final" language of science might be quite remote from that of the life-world.

His inclusion of Sellars in his critique of "eliminative reduction" introduces a troublesome ambiguity. Sellars is criticized not because he wishes to reduce the human world to scientific terms, quite the reverse in fact. Because Sellars insists that this reduction is impossible, Compton concludes that he is "exiling" human reality from "scientifically

described nature." This is to "eliminate" or mark off man from the rest of nature, not to deny his reality however, but to assert his uniqueness. (Descartes would presumably be another example of someone who "exiled" man from scientifically describable nature.) Such an approach is clearly the opposite of reductive, and is "eliminative" only in a quite different sense. The trouble here lies with Compton’s "coherence condition" which can be interpreted in two different ways, on the one hand as requiring that man must be "coherent with" the rest of nature, and on the other requiring that the reality of man’s life-world be respected. When it is taken in the former sense, reduction becomes the goal; when taken in the latter, it becomes the threat.[21]

The Scope of Reduction: This leaves him with a problem. Science must aim at a thoroughgoing reduction, an enlarged "fundamental physics" which would allow one with the aid of new dispositional concepts, to explain human behavior (sec. 7). How can he be sure that this is achievable? Because (he says) the continuity of nature from which it derives is "rooted in primary experience," the possibility of ineliminable barriers to such a reduction can be "refuted" by reference to this same experience.

This is where Sellars might turn the tables by charging that this unrestricted reduction could be just as "eliminative" in its implications as the sorts of reduction Compton rejects. Sellars argues at some length that the language of intentions and community is irreducible, that it cannot be incorporated in the "ultimate" science, and thus that the goal of a single language would be eliminative in its consequences.

I am not going to try to adjudicate between Sellars and Compton here. But this outcome does illustrate rather well how slippery the term ‘eliminative’ can become, and how much depends on who uses it. It seems to me difficult to exclude the possibility that there may ultimately prove to be an

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unbridgeable separation between the language of the life-world and that of the sciences. One might have expected Compton himself to argue for this possibility, given his insistence on the richness of the life-world. It is hard to see how an appeal to "primary experience" alone can exclude it. A "levels" solution in which certain of the levels remain irreducible would seem to be as consonant with our "primary experience" as is the full-reduction view. Indeed, a host of philosophers have supposed it to be much *more* consonant. So far as I can see, both possibilities are open.

A Hierarchy of Life-Worlds?: But Compton would want to go further. He asserts the possibility that what is characteristic of the subjective human life-world as a life-world may not really be peculiar to that level, as we ordinarily understand it to be, but may have "structural analogues" at every level down through the animal and plant kingdom even to the level of the elementary particles of quantum field theory. The "heuristic value" of this suggestion "seems considerable" for the future development of the sciences, he declares, calling on Whitehead in his support (sec. 7).

I must confess to being rather skeptical of this "downward" reduction, as it might be called, now constructing the molecule after the pattern of man instead of the reverse. Compton provides no argument in support of it other than that it brings about an impressive "coherence" in our world-view. We ought, he suggests, somehow become "responsive" to these "life-world structures" that he takes to exist at all levels. But how? Is there direct evidence for them? Or are we imputing them on the grounds that though not at first sight perceivable to us they *must* be present?

This debate has a familiar ring; it has gone on within evolutionary philosophy for a century past. The main motivation for the view Compton proposes (one associated in the past usually with idealist philosophies) has been that it avoids ontological separations of level while preserving the primacy of distinctively human categories. Thus, by supposing that there is "something like" the subjective human life-world even at the level of the molecule, one can represent the transition to man not as the appearance of some radically

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new kinds of activity but rather as a progressive disclosure of a set of activities which were there in a diminished but nonetheless real form at all earlier levels.

One objection to this is that it neglects the all-important category of *potentiality*. To say that A is potentially a B, is to say (among other things) that it is not at present a B. Further, to say that molecules have the *capacity*, when properly organized, to constitute a being that can perceive or reflect, is to tell us something crucially important about them, perhaps the *most* important thing about them. But it is *not* to say that molecules themselves can perceive or reflect in some sort of scaled-down way. This would not be a valid implication, nor is it ontologically required for the evolution of mind to occur.

Compton believes he needs to postulate the presence in matter of these life-world analogies in order to avoid what he takes to be the inevitable concomitant of Newtonian physics: that matter should become no more than inert bulk, "radically cut off from all forms of human experience and meaning" (sec. 8). But this is to misunderstand the process of idealization exercised by Galileo and his successors. Newton deliberately laid aside the more complex features of the entities he was dealing with in order to focus on some simple idealized mechanical properties. By proceeding thus he did not intend to deny the existence of the more complex features. He assumed the presence of real dispositional properties which would be the basis for the appearance at a higher level of the more complex behavior. Earlier Boyle had insisted that the surface colors of bodies had to be regarded as *real* dispositions, ones which a later structural chemistry would be able to explain. Omitting them from *initial* consideration in mechanics in no way implied that their reality was being questioned or that a barrier was being raised to their eventual incorporation in a single scheme.[22]

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It is unhelpful then, to say that the corpuscles of Newton's physics were "radically cut off from all forms of human experience." If this means only that this physics could not explain the modalities of human experience, it is obvious and is in nowise a criticism—unless one supposes (as Compton surely does not mean to do) that Newton ought to have tried to construct a set of categories which would suffice to explain everything at once. If it means that Newton's physics excluded the possibility of the presence in his postulated corpuscles of the dispositions required to allow the development of the human organism, then it is incorrect. To suppose that the entities of Newtonian physics were radically cut off from human experience is to take this physics to have been in intent a complete and final description of what is. Now there were doubtless some reductionists of Newton's day who believed this, but this surely does not authorize us to make the same incorrect assumption and then use it in order to criticize physics for its "exclusion" of the human life-world.

There is a hint of idealism in the insistence in Compton's closing paragraphs that nature is essentially nature-for-man, that to depict it in Newtonian terms of force and mass, omitting explicit reference to meaning and subjectivity, is to lose all possibility of finding a place for man in it.

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If to postulate a "life-world" at the quantum level is no more than to say that electrons interact and exhibit potentialities (sec. 7), this adds nothing to the Newtonian repertoire. But if it means that electrons display something like mentality, what we need is proof of this as well as an explanation of why this sort of claim is needed in order to secure man's place in nature. The weakness of the romantic philosophies of the eighteenth and nineteenth centuries lay in a too-great readiness to impose on nature the lineaments of man, taking this to be the only way in which the integrity of man could be secured. A modern-day Laplace, noting the explanatory resources that contemporary science already so persuasively displays and its openness to fundamental revision of its most basic categories, might well exclaim: we have no need of that hypothesis! The romantic infusion of human features into nature is not the proper way to secure the reality of the object of concern.

It is almost fifty years now since the distinguished American physicist, Arthur Holly Compton, published a speculative work called *The Freedom of Man*, in which he sought to secure the integrity of man in the face of the overwhelming explanatory successes of his own science of physics.[23] We should celebrate the tenacity of family tradition as his son continues to evoke, and to evoke so effectively, this same theme today. His contention that a cluster of topics which all would agree to be central to contemporary philosophy must be seen to be interconnected in quite specific ways, is worthy of our most serious attention. Whether it should be labelled "philosophy of nature" is less important. When he urges that the natural world ought still be a matter of direct concern to the philosopher, however, he is speaking for those many of us who might secretly admit to finding more cause for philosophic wonder and reflection in the columns of the *Scientific American* than in those of the *Journal of Philosophy*.

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Footnotes

[1] Delivered as the Presidential Address to the Metaphysical Society of America at its 1979 meeting, and printed in this issue of the *Review of Metaphysics*. References are to section numbers; if no specific reference is given for a quoted passage, it may be assumed to be from the section just previously quoted. [2] *The Crisis of European Sciences and Transcendental Phenomenology*, trans. David Carr (Evanston: Northwestern University Press, 1970), p. 47. [3] *Ibid*., p. 48. [4] *Ibid*., p. 45. [5] See E. McMullin, "Structural Explanation," *American Philosophical Quarterly* 15 (1979): 139-47. [6] See Husserl, p. 48 ff., and also Aron Gurwitsch's useful summary: "Galilean Physics in the Light of Husserl's Phenomenology" in *Galileo: Man of Science*, ed. E. McMullin (New York: Basic Books, 1967), pp. 388-401. [7] Husserl, p. 50. For a good discussion of this and the other issues raised by Husserl's characterization of "Galilean science," see Gary Gutting, "Husserl and Scientific Realism," *Philosophy and Phenomenological Research 38* (1978): 42-56. [8] See E. McMullin, "The Language of the Book of Nature," *Proceedings Sixth International Congress of Logic, Methodology, and Philosophy of Science*, Hannover, 1979. [9] Husserl, p. 131. [10] It is interesting to find the debate between these three alternatives dominating Soviet philosophy in the 1920-30 period, before discussion was terminated by Stalin in 1931. See David Joravsky,

Soviet Marxism and Natural Science (London: Routledge, 1961); E. McMullin, "Soviet Marxism and Natural Science," *Natural Law Forum 8* (1963): 149-59; "Is There a Philosophy of Nature?" *Proceedings International Congress Philosophy*, Vienna, 1968, 4: 295-305. [11] Husserl, p. 124. [12] Ibid., p. 125. [13] Ibid., p. 133. [14] See *Across the Frontiers*, (New York: Harper, 1974), esp. chap. 9, "Natural Law and the Structure of Matter"; see also his seminar discussion of this topic in *The Nature of Scientific Discovery*, ed. Owen Gingerich (Washington: Smithsonian, 1965), pp. 556-73. [15] See Gutting, sec. 2. [16] Husserl, p. 127. [17] See E. McMullin, *Newton on Matter and Activity* (Notre Dame: University Press, 1978), esp. chap. 1. [18] Husserl, p. 139. [19] They are discussed in detail by Fred Suppe in the afterword to the 2d ed. of *The Structure of Scientific Theory* (Urbana: University of Illinois Press, 1977); see also E. McMullin, "The Ambiguity of 'Historicism'," *New Directions in the Philosophy of Science*, eds. P. Asquith and H. Kyburg (E. Lansing: PSA, to appear 1979). [20] Husserl, p. 126. [21] See sec. 6, "Matter, Perception, and Reduction" in the introduction to E. McMullin, ed., *The Concept of Matter in Modern Philosophy* (Notre Dame: University of Notre Dame Press, 1978), pp. 23-42. [22] Compton's claim that Sellars is forced into a fundamental incoherence because of his failure to "recognize that the object of scientific investigation, the natural world, precisely includes the individual and communal intentional life of those who investigate" (sec. 6) would be challenged by some. Gary Gutting argues: "Sellar's fundamental philosophical project is the attempt to delineate

the mutual relations of the manifest and scientific images of man. In spite of his insistence on the ontological primacy of the scientific image, Sellars does not follow the simplistic course of rejecting the manifest image as false and enthroning the scientific image as 'The Truth'. Rather, he emphasizes the need for synthesis, for seeing how the two images form a single unified vision." Going on to the very passage quoted by Compton, which speaks of a "joining" of the two images, Gutting says that this is a "potentially misleading" remark on Sellars's part. It would be "perfectly appropriate if we interpret it (in accord with Sellar's following sentence) as saying that the synthesis is not to be effected by developing scientific language to the point where we can translate into it without remainder, all the truths of the framework of persons. In such a case *reconciling* the frameworks would actually be a matter of *reducing* one to the other, a move that Sellars firmly rejects. But the comment will mislead us if we take it to mean that the synthesis is merely a matter of making conceptual room for a *juxtaposition* of two independently articulated frameworks." ("Philosophy of Mind, Action, and Freedom" in *The Synoptic Vision*, ed. C. F. Delaney et al. [Notre Dame: University Press, 1977], p. 127). [23] *The Freedom of Man* (New Haven: Yale University Press, 1935).