THE PSYCHO-LOGICAL FOUNDATIONS OF MODERN SCIENCE

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INTRODUCTION

1. The intent of this paper is to sketch a restructuring of the foundations of science and indicate how it can lead to solutions of some well-known problems about the nature of science—and to some specific changes of direction in certain areas of scientific and metascientific research. The *general features* of the account given are more in line with what practicing scientists think of as 'the common sense view' than those proposed by philosophers of science like Comte, Carnap, and Popper—or Habermas and Foucault today. The *specific results* are not so obvious (and not established in detail here): they include implications for the analysis of the concepts of probability, taxonomy, information, evaluation, randomness, causation, measurement, and many others, all of these results having significant (but not shocking) effects on the practice of scientific research and the teaching of science.

The song that is sung in these pages has two melodies in it that were alien music to the neo-positivist; an emphasis on the *subjective* element in science, meaning the personal, internal, cognitive element, and an emphasis on the *context of epistemic activities* rather than the *content of epistemic claims*. While this song is not the war-cry of the neo-positivists' traditional opponents, it is hoped that a new harmony emerges which may appeal to those on both sides looking for new music.

2. The metaphysical foundations of science—and hence science itself—are in some crucial but elusive way involved with the nature and limitations of the mind. Towards the end of his famous analytic study, ***The Metaphysical Foundations of Modern Science***, E. A. Burtt says:

> The whole vast realm which science reveals finds its rational order and meaning in the knowing activity of the human mind.[2]

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[1] Many thanks to the other contributors to this volume who were at Asilomar for their comments and especially to Willis Harman for valuable suggestions about the second draft, several of them incorporated here.

[2] p. 322 of the Humanities Press edition (1952) of the 1932 (revised) version.

The key question is whether—or to what extent—the order that science 'reveals' is imposed *by the mind on the world* or *by the world on the mind*.

3. In the classical form of the view referred to by philosophers as idealism, even the *existence* of the outside world is rejected, hence all of its apparent order comes from the mind which creates it. This view has recently had a revival of popularity in association with critical theory, various versions of constructivism such as social constructionism, deconstructionism, post-modernism, etc. Another radical possibility is instrumentalism—the view that the tangible world may exist, but that the invisible entities and laws of science which provide it with its 'rational order' are merely 'mnemonic' devices invented to facilitate prediction and the simplification of description, with no claim to reality. A stronger version of this treats the hypothesis of the existence of the external world as itself a device for improving predictions. Burtt himself favored the view that *most* of the order is in the mind[3], but allowed that there is some kind of external world. However, he thought that the *mind* was a mysterious entity that "must find its total explanation beyond the material world"[4].

4. The view put forward here differs from each of these positions. (i) It proposes a radical reanalysis of the logic of science—that is, of the framework concepts for the whole body of science, concepts such as explanation, classification, evidence, probability—in terms of two quasi-psychological (specifically, cognitive) concepts. The concepts are those of knowledge and understanding—and their 'communication counterparts' information and explanation. (ii) The method of analysis itself is novel, and is based on the same approach, which lays much more emphasis on the importance of context and communication than does classical logic. (iii) The position taken is that the requisite sense of these concepts—and hence of all science—is on the one hand subjective in a sense that justifies some but not most of what the anti-objectivists have claimed, while on the other hand being objective in a sense that supports most but not all of what is claimed by those proclaiming the traditional objectivity of scientific claims. (In the usual technical language, it is close to realism but with some concessions to instrumentalism; constructionist but with little concession to relativism.) (iv) One perspective on these conclusions is to say that the role of the mind in science, properly conceived, clarifies much about the nature of science, in particular about its essentially evaluative functions, including its real standards of evidence and theoretical adequacy, by contrast with its too-frequent intolerance; and its real power, by contrast with its occasional forays into megalomania and paranoia.

PSYCHOLOGICAL EPISTEMOLOGY—PRELIMINARIES

5. The view here should be referred to by a label that reflects its roots in the fusion of the psychic with the logical, the subjective with the objective, the internal world with the external world. The language of the title of this paper might therefore be read with the hyphen explicit, as "The Psycho–Logical Foundations of Modern Science"; correspondingly, the tool discipline to which we are referring might be thought of as psychologic—where we can without ambiguity drop the hyphen—and the underlying philosophy as psychological epistemology. An alternative terminology might try to reflect the fusion here of a new approach to information theory with logic, but the term "info-logic" has been used in another context with another meaning, so we will use the first alternative. This choice has another advantage: the similar term 'psychologism' was regarded as derogatory by the logical postivists, for

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[3] "The natural world after all is more the home and theater of mind than its unseen tyrant..." (loc. cit., p. 324)

[4] loc. cit., p. 324

invalid reasons, and we need to confront their rejection with reinstatement of the psychological element in the logic of science[5].

A third reason for the suggested term is the belief that psychology itself needs to stabilize after the swings from introspection to behaviorism to constructionism; and in the face of the current tension between phenomenological and clinical psychology, on the one hand, and cognitive and experimental psychology on the other. The term "psycho-logic" points the way towards a greater respect for the role of reasoning in the study of psychology, a counterpart to the movement that has recently emerged in sociology with the emergence of the "rational-action paradigm"[6].

6. Although only a sketch of a new approach is presented here, it represents a move towards a more balanced approach in which we render considerable domains both to the material world of Caesar and to the spiritual or mentalistic realm of 'the inner life'. There's nothing new about attempting some kind of reconciliation; the trick here, as in human affairs, is to get the relationship right. It is not enough that the two domains recognize the legitimacy of each other—although one should bear in mind that sometimes even this is going too far, as in the way some theologians divide the turf of life between science and religion—for science has more to say about mental phenomena than merely to acknowledge their reality. It is, on the other hand, too strong to talk of complementarity, for there are parts of either territory which have no complement in the other (the stab of pain itself, on one side; the psi function, on the other).

It is also too strong to propose unity under one government or the other (epiphenomenalism or idealism); and too much to argue for a single citizenship (an identity theory). We must insist that the territory of science overlaps much—but not all—of those of the other two jurisdictions, because it is defined in a different way, as the territory of the Kurds overlaps the countries of Iraq and Turkey. Thus the appropriate treatment of the external world and the internal world is not achieved by trying to destroy the Skin Curtain that separates them, nor by denying the reality of what is on one or the other side of it, but rather by recognizing that the legitimate territory of science is defined on an overlay, on a second level. The solution must be sought in the foundations of that meta-territory, in the discipline which links and analyzes all of science's specific subject-matters—in the 'grammar of science' of which the account proposed here is referred to as psycho-logic.

7. Thus, the psycho–logical approach follows the common sense and scientific position in rejecting skepticism about the external world's existence and order (hence it is, in the philosopher's sense, a form of realism[7]); it equally rejects skepticism about the internal world's reality, the behaviorist's position. It rejects the conventionalistic aspect of instrumentalism without jumping into the opposite camp of supposing the order to be *entirely* 'out there'. While it is perfectly consistent with the first quote from Burtt it differs from his interpretation of the situation in several ways; for example, it differs from his

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5 The term "psychologism" as used here is defined below; it was used in another sense by Popper and those who followed him in time as well as spirit, and has recently acquired a slightly different sense again, in the discussions of.

6 One symptom is the founding of the journal ***Rationality and Society***, under James Coleman's editorship. Another is the new effort by sociologists to avoid the excesses of the New Sociology: a valuable and scholarly contribution to this effort is Jeff Coulter's ***Mind in Action*** (Polity/Blackwell, 1989).

7 Unsurprisingly, philosophers are not unanimous in their definition of realism. Thus Arthur Fine opens his contribution to the anthology ***Scientific Realism*** (University of California, 1984) with the words "Realism is dead", but goes on to give Bohr as an example of a non-realist or anti-realist (he calls him "the archenemy of realism"). It seems simpler to say that Bohr's position is that the entities inhabiting the sub-atomic level do not have the same properties as those at the macro level. Fine goes to support a position himself which many would take to be realism; its key component is accepting the literal truth of scientific claims. This is not much better than Piaget's definition of realism as the view that one's own conclusions are always objective, a straw man that he knocks down even more quickly than Fine knocks down his version.

further conclusion about the mind in that it takes the mind to have *a* total explanation within the material world, while conceding that there are *aspects* of the mental life—its flavor, so to speak—that cannot be *reduced* to the language of (the rest of) science although they can be extensively *related* to it.

The mind-body problem is no more a legitimate philosophical problem than it is an impediment to normal discussions of how one's feelings are affected by one's circumstances or by medication. The more extreme versions of physicalism and reductionism are not supported here—the versions that deny the reality of these other aspects of the mental life or insist they are "nothing more than" the physical reduction of them.

8. The order in the world of which Burtt spoke is, however, one of the matters that can be clarified by reference to the physical explanation of the mind's operation. In these terms, it presupposes (depends on, recognizes) a reality that is indeed ordered in the sense of being far from chaos, but a reality on which much of the order is a byproduct of the evolutionary attempt of our limited brains to cope with—to describe and explain—the complexity of the universe.

9. This sounds like 'evolutionary epistemology', but the approach here is crucially at variance with that of those associated with that label, notably Popper and Campbell[8], who incorrectly (and unnecessarily) denied the existence of a legitimate form of inductive inference in favor of the 'random trial and selective retention' account. One might say that they insisted on a first-level analogy with the way evolutionary adaptation takes place, when they should have seen the necessity for an extension to the second level, where new abilities and senses evolve that cut through the laborious process of trial and error.

10. So the position taken here is that there is an external world and it is more or less as we normally and scientifically describe it, these two types of description being complementary, contrary to the Eddington et al. incompatibilism thesis ("The scientist has shown that what appears to be a solid table is really mostly empty space" etc.). But this assertion involves no acceptance of the kind of infallibilism that is so often ascribed to science (by its enemies), or to the logical positivist interpretation of science, by its critics[9]. We can be sure that many details, some important facts, and some major perspectives that we have accepted in a certain period in the history of science will turn out to be wrong. It's just that these errors are, at any historical moment, quite minor compared with the constancy of the overall picture comprising the findings of all sciences (and other inquiry disciplines). It is in fact because the changes are incremental that they can be recognized at all, and for the same reason the idea of wholesale scepticism is essentially self-contradictory. Those who say that *everything* is uncertain have not realized that their claim refutes itself. If they do sense this, they naively suppose that this in some way supports their position; not realizing that they have rejected all positions, not all positions besides their own. They

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[8] Together with their antecedents—Poincare, and others—well identified in Campbell's essay in his recent anthology, and their successors such as Hallden in ***The Strategy of Ignorance: from decision logic to evolutionary epistemology*** (Theoria Press, 1986).

[9] The rejection of positivism in this paper is so thoroughgoing that perhaps it will not be dismissed as defensive to say that criticism of the kind referred to here is one of many ways in which the increasingly 'politically correct' anti-scientist/anti-positivist line makes a straw man out of the opposition. It is well to remember that logical positivism was a considerable advance on the miasmic metaphysics it replaced. We stand on the shoulders of giants, in the metasciences as in the sciences that metascience studies.

It should also be remembered that what Burtt defined as positivism we are more inclined today to call empiricism, since the current discussion tends to ignore the crucial distinctions between positivism and logical positivism. The definition of positivism provided by the term's originator (Comte) involved, for example, the denial of the legitimacy of the search for causes and theories involving abstract entities, something which Russell, who called himself a logical empiricist, also endorsed, but the later logical positivists completely rejected. (While this is not intended to be a scholarly review of the literature of positivism, the interpretations it involves are based on many years of interaction with Feigl, Carnap, and Tarski.)

want to eat their cake and have it too; to be right in their conclusion, although reaching it by denying the legitimacy of all conclusions[10]. If you kill enough civilians in a war you haven't liberated the country from a tyrant, you've become one. The fact that many legal cases are appealed does not support the view that law is arbitrary.

11. While this is not the place to deal with the whole problem of epistemological skepticism about the external world, it may be useful to indicate a secondary reason for rejecting the leading versions of it, because it provides a preliminary example of one kind of analysis—"bootstrapping"—that is part of the repertoire for the reanalysis of all the concepts in the logic of science.

The highly sceptical view about the external world that philosophers refer to as idealism—because of its emphasis on the reality of 'ideas' rather than an external reality—essentially involves a confusion between the nature of experience and the nature of what we infer from experience. While our knowledge of many sensory and other experiences is, with respect to a *particular* claim—more reliable than most conclusions we form about the external world, there is nothing logically improper about hypothesizing the existence—and properties—of that world, and confirming that hypothesis in the usual ways. Eventually, the level of confirmation by many people and long experience may make *public* claims about the external world more reliable than the claims of any one person about their experiences, and in some cases more credible than the shared beliefs of large numbers of people about their experiences. The primacy of sensory information in the *normal* process of inference should not be confused with its ultimacy.

The mistake made by the idealists in claiming that only ideas are real is like that of supposing that only the detective's clues are real whereas the crime is imaginary. The crime is just as real as the clues although we'll never see it directly as we do the clues. But there are no grounds for supposing that the real is restricted to what we can see on demand.

Of course, the deeper insight is that we cannot even learn the language for describing the world of experience without reference to the external world, indeed without reference to things which exist although we cannot see them—either now or, in some cases, even when we wish to. The idea that the existence of the outside world is more problematic than that of the world of experience is of course exactly the same mistake which seduces the sceptic about induction into thinking that the present is certain but the existence of the future is problematical. The manner of the crime or the matter of the future are indeed inferred rather than directly observed but in due course, as the evidence accumulates, they not only become reliable beliefs but more reliable than most of the individual evidences from the senses on which we build their existence.

12. Even if one continues to believe in the reality of that part of the external world which could *in principle* have been observed (or could be observed in the future), another radical suggestion has been widely canvassed—the instrumentalist claim that *abstract or theoretical* scientific concepts are mere mnemonics, convenient inventions to facilitate thought, but lacking any ontological status. That view was proved simplistic as instruments improved to the point where we came to see the molecules and then the genes which were supposedly only *façons de parler*. While in principle the instrumentalist position might still be true of other entities besides the ones where it was proved wrong, it has failed as a logical point: theoretical constructs are, in general, entries in the reality stakes and often successful ones. Thus, independently of the historical refutation of specific instrumentalist claims, exactly the same

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[10] Another way to approach this fundamental flaw in relativism/scepticism is via the impossibility of defining "error" within such a system of thought. (Bunge stresses this flaw amongst others in an excellent essay which also deals with counter movements in science: "A Critical Examination of the New Sociology of Science", especially Part 2, which is in ***Philosophy of the Social Sciences***, March, 1992.)

argument against instrumentalism can be used as was applied above to the other sceptical positions; that argument is simply reinforced by the historical examples.

13. Finally, the approach here leaves the door open for some—but not all—of the new forms of knowing or knowledge that have recently been advocated by critics of the narrow-mindedness of traditional science, which is better described as the simplistic interpretation of scientific method by many scientists. Several of the other essays in this collection are devoted to explorations of this latter possibility, but no attempt to review them is provided in this essay, which might be described as primarily concerned with establishing the limits and legitimacy of the *general* domain of the subjective in science, and of the objective in the realm of the mind. Its target is thus nearer to the original one aimed at by Burtt, but more directly aimed than his paper at reconstructing the *logic* of science than the *epistemology* of science.

GOING BEYOND LOGICAL POSITIVISM

14. It may be helpful to see the present approach to the logical analysis of science as a natural extension of efforts from two opposing directions. In the first place—the perspective addressed in this section—it is a 'natural' extension of the reductionist program of the logical positivists (logical empiricists, operationalists, etc.), but an extension which for them was completely taboo. The extension from the other end was an extension of the reductionist effort made from the very different starting point of the phenomenologists, constructionists, and others. Whereas the logical positivists wanted to reduce the concepts of science to observational data, the other approach wanted to reduce even the observations to something more mentalistic—sense data, phenomena, mental constructs. Both efforts have proved unsuccessful—in principle and not just in practice—although not refutable in quite such a straightforward way as instrumentalism. We need to understand the reason for the failure, and how it suggests a natural extension of the effort.

15. The reductions attempted by the logical positivists in the early days of the Vienna Circle—that is, their attempts at definition of e.g., theoretical entities in terms of observational ones and historical events in terms of present ones—was restricted to the use of *syntactic* (purely grammatical) procedures of explicit definition, and later, recursive definition and implicit definition via sets of axioms. With this restriction it eventually became clear that the project was impossible—theoretical entities could not be defined away in terms of observational predicates[11].

In the effort to increase the plausibility of the reductionist thesis, the later logical positivists—notably Carnap and Tarski—then extended the allowable analytic techniques to *semantic* analysis, where meaning rules involving linkages to the external world—not just from one linguistic entity to another—were allowed. (Ostensive definition was the paradigm.) This yielded negligible further success, although for some time it was a cult approach that extended to general philosophy (e.g., Sellars). The natural third step would have been to draw on the third and last division of semiotic—*pragmatics*—for analytic tools. In this approach, some reference would have been allowed to parameters referring to the specific contexts of use. But the logical positivists drew the line at this, which they felt went beyond the spirit of logic as they conceived it, and the spirit of logical positivism as they defined it[12].

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[11] Apart from the trivial case of Craig's Theorem which showed the reduction was possible in the useless hypothetical case where one could employ an infinite number of axioms, and under the always false assumption that one could sharply demarcate the observation predicates from the theoretical ones.

[12] Specifically, Carnap *defined* the logic of science as a subject which involves "*abstraction from* the psychological and sociological side of the language". By "abstraction from" he meant the elimination of reference to "beliefs,

16. The most distasteful aspect of this step, for them, was the inevitability of having to include reference to the knowledge-states of the people using the terms—for example, in the case of "explanation", they would have had to include reference to what the audience knew. This is what they called 'psychologism'. They felt that this kind of reference was part of the 'context of discovery'—a matter of historical or biographical interest but not relevant to logic, which involved the 'context of justification'. Definitions—the real meaning of terms—had to be appropriate for the latter context, and hence independent of considerations which dominate the former.

17. In this essay it will be argued that the extra step, the inclusion of 'pragmatic' (a.k.a. contextual, a.k.a. psychologistic, a.k.a. cognitive state) considerations, if done in a certain way, does in fact lead us to an improved analysis of the concepts and understanding of the nature of science. This view involves a sense in which scientific objectivity is—in a sense—eventually subjective, while showing that the sense in which this is true is one in which the subjectivity is no threat to validity. Thus the logical positivists' critics—today, for example, the constructionists—were right in supposing that the logical positivists' separation of the agent, the scientist, from the world was unsound—but wrong in supposing that the objectivity of science was illusory. It is merely an elusive ideal, like justice. It is right to point out that we quite often miss it, even when the whole scientific establishment thinks it has been attained; but it is a blunder to suppose that we *usually* miss it, or may have *entirely* missed it. The routine process of the law, as of science, attracts little publicity mainly because there is little news value in just punishment for conceded or irrefutably demonstrated offenses, as there is little scientific excitement about the accurate measurement of samples of conventional substances. It does not follow that justice or accurate measurement is an illusion; it is no more an illusion than the external world or the future—or the present. But sometimes we are wrong about it.

FALLING SHORT OF PHENOMENOLOGY

18. Extending logical positivism beyond the point at which it ceases to accept the extensions does not salvage it as logical positivism, but it does establish some continuity with the proposals made there, and it can show how the movement stopped on the brink of breakthrough to a workable analysis. In the same way, restraining the claims of phenomenology—and of its allies in the cause of subjectifying science—will also make the position unacceptable to the originators, perhaps in part because we take away the cachet of the iconoclast. However, it does create a viable position rather than a self-contradictory one, and it does locate it in a context which shows it shares a border with the opposition. To perform this task, however, we need to clarify the key notions of objectivity and subjectivity. As we'll see, a great deal of the confusion involves switching from one to the other of two quite different interpretations of these terms.

THE TWO FACES OF SUBJECTIVITY

19. The key to the trouble is that "subjectivity" has both an *evaluative* sense and a *descriptive* sense. (A parallel situation applies to "objectivity".) These senses are best understood in terms of their contrasting concepts as well as their connotations. In the *evaluative* sense, subjectivity (or 'subjective') connotes unreliability, a mere matter of opinion, unreality, or fallibility; it contrasts with factuality and validity. In the *descriptive* sense, it connotes something private, idiosyncratic, personal, or specific to a

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images, etc., *and the behavior influenced by them*" (emphasis added in both quotes, which are from the opening section of "Logical Foundations of the Unity of Science", in the ***International Encyclopedia of Unified Science: Volume I*** (Chicago, 1938).

particular subject, and it contrasts with what is intersubjectively observable or determinable. The antonym, objectivity (and 'objective' in one of its many senses), refers directly to the contrasting concepts, that is, in the *evaluative* sense it refers to lack of bias, to truth or validity, and in the *descriptive* sense it refers to what is public or "external to the mind"[13].

Behaviorists often argued for the move from introspectionism in psychology to behaviorism by incorrectly identifying the two senses of objectivity, that is, by rejecting all purely personal experiences as lacking in the kind of objectivity that can be of value to science. But the sense in which personal experience is (necessarily) subjective does not support the conclusion that it is necessarily unreliable; otherwise, general practitioners, psychotherapists, optometrists, and space medicine specialists would be hard put to do competent work. A less partisan view of the situation would have been that an *excessive* focus on introspection as *the* method of psychology had led to undervaluing what could be done in terms of intersubjectively testable claims. The use of the latter type of claim was of course so successful in the physical and biological sciences of the time that it was inevitable a movement would begin in psychology to jump on that bandwagon.

However, to do so—to restrict validity-objectivity to intersubjectively verifiable experience is simply a mistake. Introspection, about some matters, when practiced by some observers, in some conditions, can pass the tests to establish objectivity in the sense in which it connotes an unbiased account, that is in the sense that establishes the phenomena experienced as deserving the status of (putative) facts. Perhaps the most important example of this arises in research on drug effects where the reports of the subjects on their experiences are a crucial part of the data, and are of a type that investigators often have no basis for inferring from prior data or their own experience. The kind of tests such data must pass of course overlap with the ones appropriate for external events, to a degree that ensures they both refer to the same quality of reality. Hence they include: tests of internal consistency, test-retest consistency, the use of analogies with publicly accessible experiences, information content which can be partly verified in other ways, truth-telling tests or track record (especially where the incentive to deliberate deception can be minimized), the identification of brain, electrogalvanic, or sensorimotor correlates, overt behavior cues, and reports from other subjects of their own private experiences. Only some of these are relevant in a most cases, but the accumulation of those available makes it unreasonable to deny the data status of the reports—as reports of inner experience, not just of shared verbal behavior (cf. religious revelations).

20. In the more recent rush to jump *off* the behaviorist bandwagon, many people have continued to treat the two senses of subjective/objective as one and have simply reinstated the fallen angel of subjectivity (in the personal-experience sense) as the leading divinity in the pantheon of knowledge. This gives it an epistemological role it cannot support and ensures contemporary opposition and a return swing of the pendulum in the future. Even in the misleading epistemological picture according to which we infer from inner experiences to conclusions about outer reality, it is clear that there is a distinction between the truth—that we only *access* the external world via our perceptions or sensations (etc.)—and the related error of supposing the world to be *no more than a construction* out of sensations (etc.). On that distinction hangs the difference between fact and fantasy—the difference between the crime and the clues. We will not make progress by merely reversing the earlier mistake.

21. The behaviorists were precipitate; they oversimplified the situation and ran into absurdity, at its most absurd in the position of the 'radical behaviorists' led by Skinner who argued for the mind as merely a fiction for explaining overt behavior. This was no more and no less foolish than arguing for the world as a fiction for explaining mental events. But the behaviorists were not wrong to suppose that: (i)

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[13] The quote is from the ***Random House Dictionary***, Unabridged Second Edition. A much longer discussion of this crucial case of linguistic schizophrenia can be found in "Objectivity and Subjectivity in Educational Research", in the ***Seventy-first Yearbook of the National Society for Study of Education*** (1972), published by The National Society for the Study of Education.

there are great difficulties in *verifying* claims about personal experience; (ii) there is no way to *define* all the concepts of science in terms of such claims; (iii) a great deal of scientific research in psychology (and other social sciences) could be done without depending in the slightest on assumptions about the nature and content of the mind; and (iv) one cannot even define many of the crucial mental states, such as knowledge, without conceding that some of the necessary conditions for its applicability are external, and that some sets of external conditions are sufficient for its application.

Nor, on the other hand, were the idealists of whatever stripe wrong in stressing the reality of the mental world, the privileged access to it of one individual, and the impossibility of constructing the external world by some simple process of defining its entities in terms of mental events. We have now moved to a better understanding of the complex truth about the relation of language about mental events, states, and processes, to language about behavior, through the work of Wittgenstein, Ryle, Austin, and others following them, but spelling the story out in detail is not essential for the purposes of the present exercise[14].

22. The solutions to the ontological problem about the status of the mind, and to the epistemological problem about the nature of scientific knowledge, do not lie in a straightforward proof that the construction which one of them thought impossible is in fact feasible. Given the construction tools they knew about and accepted, both constructions were impossible. The solution lies in improving the tools, thereby reconceptualizing the problem. Using the improved tools, the realist turns out to be right, but given the old tools, used in the traditional way, the idealists were equally well entitled to their conclusion. However, the way we save the realist requires an immediate rejection of the logical positivist position. It is not done by embracing idealism but by insisting on what the positivists rejected as 'psychologism'.

THE PROGRAM FOR PSYCHOLOGIC

23. Thus both parties, the idealists and the realists (in the first decades of this century the two parties were mainly represented by the metaphysicians and the logical positivists) labored under three handicaps. Their definition of *subjectivity* was confused, their definition of *definition* was excessively restrictive, and their conception of *legitimate inference* was also too narrow. We've already suggested some refinements in the first of these, and have indicated how the second and third caused trouble. The aim of the rest of this essay is to follow through the strand of subjective (mentalistic, psychologistic, etc.) involvement in the conceptual apparatus of science, in order to understand the underlying fusion of the subjective and objective. The three concepts just referred to are only three of the many to which we'll refer. The key set includes and does not go far beyond the following twenty groups with sixty elements, although we'll only provide comments on a representative subset:

- concepts

- observation & observable entities

- data, facts, information, propositions, & discoveries

- description

[14] It is well described from a point of view that does the best that can now be done for the behaviorist position ('linguistic behaviorism' perhaps) in Jeff Coulter's book referenced earlier.

• estimation & judgment

• evaluation

• classification

• ideal types & natural kinds

• generalizations & laws

• prediction & correlation

• explanation & reduction

• causation, experimentation, & determinism

• meaning & definition; criteria & indicators; necessary & contingent connections

• measurement

• approximation

• truth, probability, & possibility

• confirmation, corroboration, justification, verification, & falsification

• scientific evidence & inference (including deduction, induction, abduction, & probative inference); significance; & proof (demonstration)

• parsimony & simplicity

• models, analogies, theories, paradigms; theoretical (abstract) entities

• subjectivity & objectivity

It will be clear from the sections above that it is also being suggested that the reconceptualization of these concepts provides a basis for reconstruing some of the important *issues* in the philosophy of science. In fact, it seems plausible to suggest that such a reconceptualization affects most of the important methodological, epistemological, and ontological issues involved in understanding science, although determinism is the only *position* included in the above list.

24. So the main contention here is simple enough to describe although not so simple to establish. It is maintained that more elaborate and imaginative notions of logical construction, definition, and inference (in particular) makes it possible to retain the appropriate degree of objectivity for scientific concepts while allowing the degree of mental involvement that is essential if we are to provide a reasonably plausible analysis of the grammar of science and the functions of thought in science. The recipe involves getting the analysis of scientific concepts extended beyond syntactics and semantics into the 'psychologistic' domain of pragmatics. Unless we do this, the analyses are wrong; if we do this, a lasting marriage of the subjective and the objective can be accomplished. The secondary contention, which is independent of the other, is that the notions of knowledge and understanding (or information and

explanation) are the only epistemological primitives in the set required for the logic of science, that is, the rest are reducible to these.

THE CONCEPTS OF 'BASIC MEANING' AND 'STANDARD CONTEXT'

25. Each of the terms we will deal with has a *basic meaning* which includes contextual, 'psychologistic', state of mind, or 'subjective' factors[15]. It is in terms of this basic sense that we come to understand the concepts, and we often have to turn back to this basic sense in order to resolve puzzles involving the concepts. But each of the terms also has one or more *standard contexts* where the psychological element 'falls away' because it is so well understood that the reference to it becomes implicit, a hidden assumption. In those contexts, we can often employ the standard dictionary kind of definition, which is usually a syntactical translation rule, sometimes a semantic rule; and where that fails, we can often turn to the definition implicit in a set of axioms or laws—or even just true statements—which involve the term. The most common standard contexts for scientific contexts are those of the science classroom and the scientific research community. The existence and familiarity of these standard contexts misled logicians into thinking that the real meaning of the concept was encapsulated in the syntactical definitions. But even the scientific meaning was not encapsulated in the syntactical definition used by the scientific community, as we quickly saw when problem cases arose, and a switch to another definition was made (thermodynamics): not paradigm shift, just parachute switch.

DISCOVERY

26. We start with an example of historical interest because it is the one to which the logical positivists appealed in their distinction between real logic—the appropriate tool in the 'context of justification'—and history/biography—the 'context of discovery'. The concept of a scientific discovery—or any other kind of discovery—clearly refers to the knowledge-context of the time when the discovery is supposed to have been made. A discovery is an *increment* in what is known. Here the logical positivists illustrated psycho–logic admirably, with an example which does not form a significant element in the logical vocabulary of science, so they could afford to concede the involvement of the psychological or cognitive element. There is an interesting although simple problematic application of the concept of discovery which forces us to be clear about that element. Even though this case does not come from the philosophy of science, it illustrates the kind of problem which we'll find there in connection with other concepts. The problem concerns a comment that used to be in every history text, to the effect that "Columbus discovered America". Of course, this refers to the personal knowledge context of Columbus in particular, readily generalized to the standard context of European knowledge of the time. Today, most of us understand why it is offensive to Native Americans to find this is in school history texts, since it disregards their many cultures—for whose knowledge context 'America' was not a discovery but a foreign name for their homeland.

27. This simple example leads us on to a more profound problem with the separation of the context of discovery from that of the context of justification—and the attempt to discard the former for purposes of 'real logic'. This problem arises because the way we learn *every* concept is in the context of discovery, and with nearly all concepts that involve reference to 'states of the mind' this means that the *logical* criteria for the concept initially include both intersubjectively observable phenomena and the 'inner

[15] In the language of semiotic, that means the definition has to go beyond syntactic and semantic considerations into the realm of pragmatics.

states'. One can hardly argue that these terms change their meaning after we learn them, since in that case it would not be these concepts that one had learnt. So it is *always inappropriate* to treat them as divorced from the inner states—as behaviorism does—just as it is always inappropriate to treat them as divorced from the external criteria—as the idealist, phenomenologist, or constructionist does (or has difficulty not doing).

Note that this is different from the way in which we learn about invisible causes. These are defined from the beginning in terms of a definite description, as "the hidden cause of these things you can see". While this is marginally defensible as a way to introduce the language of phenomenal pain through pointing at pain behavior in others, it is not the way in which we introduce notions such as knowledge and understanding. They must *always* meet external tests as well as internal ones, since we are often wrong about our beliefs concerning the extent of our knowledge and understanding. (If not, we could replace marking examinations by taking a lie-detector test.) And it is *those* concepts which form the basis for the whole structure of science.

KNOWLEDGE & INFORMATION

28. Knowledge and understanding—so it is suggested here—are the building blocks for all other concepts in the repertoire of the logic of science. As commonly and correctly used, whether in science or outside it, the *basic* sense of each involves a crucial reference to the psychological—specifically, the cognitive—state of some individual, audience, or recipient (not necessarily human) *and* to some externally-observable criteria. The concept of knowledge is in most respects the simpler of the two; although its *content* overlaps with the notion of understanding (a.k.a. comprehension, or, sometimes, insight), *context* will always distinguish them.

Closely related to the concept of knowledge is the concept of information, just as the concept of understanding is closely related to the concept of explanation. In each of these conjugate pairs, the second notion involves the *communication or acquisition* of the first. Information is knowledge that is conveyed, or set up to be read or conveyed, or acquired by an knower from a process of observation or reflection. By a slight extension of meaning, perhaps not even metaphorically, we speak of the information in DNA being read by RNA because the RNA has the ability to 'read' the structure of DNA; note the way that *transmittal* comes in. In the same way we speak of Watson and Frick as those who broke the code, meaning only that they worked out (the general principles of) how to interpret—acquire particular information from—the structure of genes. Analogously, explanations are the *vehicles* of understanding, devices whose criteria of merit include their comprehensibility to another agent as well as their truth.

29. The 'communication-conjugate' in each pair of concepts is developmentally prior to it. That is, the way in which we learn the concept of *knowledge* is via the passage of *information* (about, in particular, the use of the term "knowledge"); the concept of "*understanding*" is only understood because it is *explained* to us. This developmental primacy is another reason, besides the external criteria consideration, for being very careful that in throwing out the context of discovery one does not also throw out the essence of the context of justification.

30. In general terms, then, information is knowledge transmitted or received via a communication process (or encoded for transmittal in such a process)—or acquired from a process of observation or

inference—whereas the knowledge itself may be just an 'inner state'[16]. Information is thus a configuration which conveys or can convey some truth about some thing or state. While information is often configured by an agent using a conventionalized code, it may be incorporated in any configuration whose significance becomes understood. For example, the meteorologist gleans information from configurations of atmospheric conditions, the physician is informed by examination of the patient's condition. Analogously, explanations are understanding-transporters and must themselves be phrased or cast in terms of some communication procedure.

31. Information is a narrower concept than knowledge since the latter clearly includes 'knowing-how' (Ryle) (or tacit knowledge (Polanyi)), our 'procedural' knowledge of how to do things that involve skills which must be acquired by demonstration, practice, and mentoring, and cannot be acquired by following a stateable procedure[17]. The cognitive rather than the manual-dexterity sub-species of this kind of knowledge is important in science, although one might argue that manual skills with instruments, including computers, has more importance than has commonly been acknowledged. Knowledge also includes experiential knowledge, our knowledge of the taste of things, the flavors that only experience can bring; the respect for participant observation field experiences in anthropology is an important example of recognition of the importance of this in science.

There can be external studies of these two kinds of knowledge (its causes and effects, etc.), and the results of those studies can be communicated, but that communication and the information it contains does not contain the knowledge to which it refers. Acquisition of a skill and of experience, while they are cases of learning, of knowledge acquisition, are not naturally described as acquiring information, although a determinist might argue that in such cases one's brain even if not one's mind has acquired information. It is commonly supposed but not obviously true that such knowledge is less important to science than communicable and inferrable information, especially propositional knowledge. Yet even there we have underestimated the range of the processes of communication, explanation, and education in science; they include not just the written or spoken word, but demonstration, training, portrayal, and acting (role-playing). Still, science is mainly a public activity whose history is not biography nor poetry nor art, where the other kinds of knowledge are of paramount importance.

32. The basic sense of information—the sense with which we must begin—is far more general and psychologistic a notion than the Turing/Shannon notion, as developed in conventional 'information theory'[18]. Theirs was a convenient and fruitful surrogate, immensely powerful in highly stylized contexts, but far from the core meaning. It deals well with situations where the range of possibilities can be clearly delineated in advance, and information is incorporated in some message by selecting a particular combination from among the possibilities. Of course, it can do little with those parts of scientific knowledge that involve knowing-how or experiential knowledge, but it is also of little use in

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16 The quotation marks are intended to remind the reader that while cognitive states are conventionally referred to as inner states, the reason for this is simply to stress that they are the possession of an individual who normally has access to them and who normally has the knowledge that they have that knowledge. These are the criteria for non-evaluative subjectivity, not for the pejorative sense of subjectivity. However, this way of describing cognitive states oversimplifies their status. They also have external criteria that (logically) must be satisfied. For example, since knowledge is only knowledge if it is true, the external world will have to match the knowledge claim in order for it to represent knowledge. And understanding is only understanding if the person said to have it is in fact able to deal with problems related to the matter said to be understood. That is, cognitive states are not like pain, which is conventionally thought to be (this is still an oversimplification, but a less gross one) uniquely accessible to those who have them.

17 Experiential and procedural knowledge are not entirely distinct, since an act of recognition is often expressed in both ways ("I know how to identify *agaricus campestris* by sight" and "I know what *a. campestris* looks like").

18 Any monotonic increasing function of the number of possibilities is acceptable, usually the logarithm to the base 2.

many important cases of propositional scientific knowledge, the kind of information we commonly convey in science education and research. The simplest version of the theory has some difficulty with defining the information content of a message without reference to the linguistic repertoire of the recipient. Is the information conveyed by a message in French the same as that of the translation of that message into English? The natural answer is to say that it depends on what languages the recipient understands. Of course, that's a psychologistic answer, referring to the 'context of discovery' not the context of justification; but it's the right answer for understanding the basic sense of the term "information". There are other types of context-dependent information; besides those which depend on the *cognitive* context, i.e., involve 'pragmatics' or 'psychologistic' considerations, there are also 'semantic' examples which also involve explicit reference to features of the *physical* context, local or more general, e.g., the information conveyed by "You were accused by the man on your left", when uttered in an appropriate context. (There are also borderline cases, like "He's not a very tall man, by our standards".) This kind of information can only be expressed ito the technical definition if we start the process of building information about the environment into the information content of a proposition, not a process for which it is easy to give rules.

There are many other problems. The 'technical definition' of information (as we'll call the Turing-Shannon definition) can do little with the subtleties of metaphor, and since most theories are or embody metaphors, it can do little with the information contained in many theoretical claims and hypotheses. Now the process of explaining the meaning of important terms in science involves, to a crucial extent, subtle nuances that cannot be—certainly never have been—expressed in any finite set of propositions, and hence cannot be reduced to bits of information.

The technical concept can do even less with the information created by modes of presentation or by techniques of analysis. The format of information may create further information, and reformatting may alter the amount of information 'in' a dataset[19]. Thus, in the Turing-Shannon sense, a table of figures may represent facts, hence constitute data, which is information; but the information it contains may only be of an item-by-item type. When graphed or represented in some other pictorial way e.g., by using pseudo-coloring, that dataset may reveal a trend or a feature which represents *further* information[20]. Appropriate statistical analysis can similarly create information; it is not a mere transformation of the same amount of information into a different publishing format. Supercomputers similarly may create information when they do data-crunching exercises that were previously impossible. Their role is then closely analogous to that of the electron microscope and not just to that of the calculator; both make the invisible visible, and thus, sometimes, they convert theoretical concepts into observable ones. Relatedly, maps of terrain, models of molecules, diagrams of instruments and machines, may be informative where no verbal description of the same phenomena is informative, although it may 'contain the same information'.

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[19] It's thus better to say "the information associated with (or implicit in)" a dataset rather than "contained in" or "represented by" the dataset.

[20] One of the most important examples today is to be found in computer-assisted EDA (exploratory data analysis) where a three-dimensional representation of a dataset is made to rotate steadily while changing with time to exhibit the dependence on a fourth variable. This approach often reveals patterns and aberrations in a way that no finite approach through conventional statistical analysis has any real chance of uncovering. This is indeed the context of discovery, but it is now being treated as something really important instead of being caricatured as 'guessing' in the neo-positivist and evolutionary epistemologist approaches. There is no 'context of justification' until there is a discovery, and the process of discovery is one where the yield from exploration can be vastly improved by systematic procedures, without jumping into the absurdity of supposing that there can be an algorithmic procedures for making all discoveries. Further excursions towards multi-media will do more still; the use of sound, motion, and true 3D, via virtual imaging, will eventually be followed by true tactile and, no doubt, olfactory representations. These will tell us more, show us more, teach us more-although perhaps not enough more to make all of the increments worth their considerable cost.

The subtleties of graphical representations, where the configuration conveys information but at a level we have not yet succeeded in atomizing is a crucial matter in clinical diagnosis, for example, where it has been shown that graphical representation of the results of tests conveys more than tabular representations—the work on the MMPI[21] has perhaps been the most intensive.

The technical concept of information cannot even cope with the difference between informative and trivial tautologies, that is, between "Triangles have three angles", a definition incorporating only linguistic knowledge, and Pythagoras' Theorem, a great discovery although demonstrably containing no more Turing-Shannon information than the definitions from which it can be deduced. Since a number of key physical and economic laws are expressed in terms that makes them definitionally true, defining information in that way rules out the expression of a great deal of knowledge. The positivists of course rejected all tautologies including all mathematical theorems as lacking in 'empirical content', but it is simply implausible to say that Pythagoras' Theorem tells us nothing about the length of the sides of physical triangles; surveyors use it all the time exactly because it does just that. The positivists were right to stress that geometry isn't like simple empirical generalizations, but they failed to realize that it is very like virtually every actual physical law or theory since these are nearly always expresed in terms of abstractions or idealizations.

However, the fundamental point they missed was the fact that geometry is a conversion device, just like a computer or graphics, and these devices create information about whatever it is applied to, *because information is in the mind of the audience*. They insisted that the information had to be in the propositions themselves, regardless of how they were massaged, combined, or portrayed—and that just isn't the nature of information. It led them to an absurd position on this as on many other issues. Tautologies, such as interesting theorems and theories, are discoveries which enable us to create information—not about linguistic rules or abstract entities, but about whatever we apply them to. They are intellectual microscopes, making visible what was invisible, making new and important information out of what was unenlightening data[22].

Thus the basic, i.e., the psycho-logical sense of information, takes it as the knowledge passed on by some message or inferred from something perceived, and the extent as well as the value of that information will depend on the interpretive skills and the prior knowledge of the recipient or inferrer. There is, it should be clear, no way in which this is subject to quantitative assessment, indeed a serious loss—and the saving grace of the technical definition. It should be equally clear that the technical definition is hopelessly inadequate to cover the whole range of scientific knowledge and information. We do not need to throw away even its conceptual contributions: the relation of entropy to thermodynamics, of noise to randomness, is still an insight in terms of the psychological sense. But we must expand its vision in order to handle its problems—and be cautious about how we interpret its results.

33. What Turing and Shannon did was entirely justifiable, if not taken too literally. It follows the long tradition of definition by simplification—especially definition by reduction to a roughly related but clearly quantifiable concept—that has generated great bodies of useful theory and practice, from the kinetic energy definition of temperature to the relative frequency approach to probability. But one must understand that such 'definitions' are merely convenient equivalences, limited in their applicability and utility, often to be abandoned as we see beyond them to a larger vision, and always subject to modifications based on our increasingly richer understanding of the concepts. Taking them literally—as sound conceptual analyses—is like taking radical behaviorism or inductive skepticism literally.

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[21] Minnesota Multiphasic Personality Inventory.

[22] A better analysis goes one step further, to deny the claim that the propositions of mathematics and logic are tautologies at all, and reconcile Mill with Wittgenstein.

Considering them seriously is a sensible intellectual exercise, but acting on them is absurd in any practical terms. In this essay, that kind of absurdity is taken seriously as a reason for abandonment of the approach, contrary to the view of many philosophers who think that a little geocentric complexity is no problem for an ontological theory.

34. Even for practical application, these approaches had serious problems. The relative frequency theory turned out to have great difficulties with a notion that it needed to be able to count on—the notion of the limit of the ratio of two types of event in an actual sequence of events. It turned out to be simply inapplicable to the probability of the single case, most importantly, the probability of a new hypothesis or theory. The psycho-logical sense of probability is the fundamental one, and in that sense it refers to the most reasonable betting level. (We discuss this concept a little more below.)

In the case of information, systems with definite discriminable states turn out to be less common than was supposed, outside the special worlds of fundamental particles, thermodynamics, circuits, and computers, and in particular, little of the brain's functioning meets that requirement. We now find that the "co-localization of neurotransmitters and growth factors in neurons... [results in the use of] entirely different combinations of signals at different times, dictated by the environment." [23] This can be construed as a nudge from the neurons towards developing contextual (pragmatic) definitions rather than content-only (syntactic) ones.

For the psycho-logical sense of information used here, these difficulties are avoided. There are no reductionist definitions, the meaning of the terms is contextual and flexible, never fully expressed in quantitative terms, but not arbitrary or subjective in the pejorative sense.

UNDERSTANDING & EXPLANATION

35. Understanding or comprehension of a phenomenon, as of a communication, might be called 'second-order knowledge'. It is the capacity to answer a range of questions about the explanand (whatever it is that is said to be explained or understood). The relation of knowledge to understanding is like the relation between knowing the meaning of the words on a menu in Italy and being able to discuss the chef's approach with the waiter in Italian. Thus understanding is a process of coming to master or having mastered the keys to a cognitive network—one in which the phenomenon or communication is embedded—a network which meets certain standards of relevance and quality, and which enables those who have mastered it to accommodate new questions or occurrences. Sometimes the range of relevance may be very limited ("I understand why he did it" is very close to "I know why he did it", i.e., his motive), but in general the range is quite wide (e.g., understanding the phenomenon of diffraction). Understanding modifies the understander's set towards further information, and often renders the new information redundant. Understanding, like information, may be created by reformatting or reanalyzing data; one may 'understand what's happening' only when one runs a correlation matrix, or a pseudo-color timeslicing animation of the data. Understanding is therefore a property of sets made up of related items of information, information-generating propositions like laws and classifications, and information formats.

36. Understanding is communicated via explanations, although not all understanding can be thus communicated, for some of the same reasons that ensure not all knowledge can be converted into

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[23] ***Information in the Brain*** Ira B. Black (MIT, 1991), p. xiii. It is ironic that this book contains no definition, explicit or implicit, of the concept of information. This can be taken as indirect evidence of the need for a more general concept than that provided by information theory.

information. The criteria of merit for an explanation are therefore its truth and the extent to which its recipient can pass the tests for understanding. Explanations are thus judged like teaching, and of course much teaching consists in giving explanations, a considerable skill which goes well beyond mere understanding. The extent of the gap is clear when one finds people with advanced credentials in an esoteric discipline who are very poor at explaining it—they can even be found on the faculty of universities.

37. The logical positivists and the neo-positivists naturally rejected the notion of understanding as psychologistic, part of the context of discovery, and nothing to do with scientific explanation. The best they could do was to equate 'explaining X' with 'proving X to be true (or probable)', a completely different enterprise. People that are puzzled by X already accept (presuppose) that X is true; it is not enlightening for them to have this proved, let alone made likely. Nor were the logical positivists entirely consistent in their approach. They had to deny that explaining the meaning of something was a case of scientific explanation, because of course the inquirer in such a case is *clearly* not asking the explainer to prove that the puzzling passage (for example) is true; yet it did not occur to them that the same applies to the inquirer about a scientific phenomenon. The difference between the two is simply one of subject matter. To explain a physical phenomenon one needs (normally, i.e., in standard contexts) to explain it in terms of other physical phenomena. Exactly which phenomena should be appealed to naturally depends on what the inquirer already understands, individually or as assumed in some standard context[24].

CONCEPTS & LAWS

38. Understanding is best seen as the most complex in a series of cognitive survival mechanisms the brain develops to handle the influx of sensory and internally generated information. Some of that information is the knowledge stored in memory which provides the wherewithal to find food stores, game trails, and recognize allies. The problem for the brain is surfeit and sorting this knowledge. If it cannot be organized, it cannot be stored in the finite space available, and, more importantly, it cannot be quickly retrieved in the limited time when it can save its possessor. So there first develops the *concept*—and, if language is available, its linguistic representation, the word—followed by the explicit version of a concept, the *classification* and then the *generalization*, the *law*, the *model or metaphor* and eventually the *theory*. Constancies in the sensory (etc.) field are the referent of, and presupposed by, concepts, and explicitly formulated in classifications and generalizations[25].

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[24] The decline and fall of the neo-positivist attempt to keep cognitive states out of the logic of science began in the early 1950s, when William Dray began his attack on the covering law analysis of historical explanation, and I began a series of critical analyses of the corresponding analysis of scientific explanation, which I named "the deductive model of explanation". Hempel's extension of the model to probabilistic inference I christened 'the inductive model'—together termed 'the inferential model'. Details of the attack are in 1956 in my Oxford D.Phil. thesis "Explanations" (reproduced by the University of Illinois library in 1962); some later installments were in "Definitions, Explanations and Theories", ***Minnesota Studies in the Philosophy of Science***, Vol. I, 1958, pp. 99–195, and in "Explanations, Predictions and Laws", ***Minnesota Studies in the Philosophy of Science***, Vol. II, 1962, pp. 170–230.

[25] A considerably more detailed analysis of processes discussed in this section is to be found in "The Concept of Comprehension: From Semantics to Software", in ***Language Comprehension and the Acquisition of Knowledge***, R. O. Freedle and J. B. Carroll, eds., V.H. Winston & Sons, Washington, D.C., 1972. In that paper a "Comprehension Theorem" is proposed, which argues that exactly those information structures required for surviival-oriented information-storage purposes provide the condition referred to as comprehension.

PROBABILITY

39. The first cushion which allows knowledge claims to be both mnemonic and comprehensive is the slippage in the loose definition of concepts we have already discussed—definitions in terms of criteria, examples, and contrasts rather than necessary and sufficient conditions, as in geometry. This was something the positivists saw as weakness, but which was in fact success at the task of survival with a finite mind in an untidy world.

The next two buffers were probability and approximation. In the case of probability something very significant occurred, because we find the neo-positivists—notably Carnap in his later stages—conceding a minor legitimacy to the psychologistic element by allowing that there was one sense of probability which referred to degree of belief. However, he made the almost inevitable mistake of thinking that in this sense a probability claim was incorrigible. That is, they took "belief" to be *entirely* subjective, not logically evaluable in the way that a scientific statement would be. This was a blunder; the simple fact is that we have a large experience with the evaluation of beliefs as reasonable or unreasonable, and if they had not been so anxious to depart the realm of the 'subjective' for the context of justification, it might have occurred to them to define probability in terms of degree of reasonable belief, i.e., support without guarantees. As Toulmin rightly and devastatingly pointed out, we know very well how to criticize someone's probability estimate, although it correctly reports their own degree of belief[26]. Thus the 'subjective' area of degree of belief is entirely corrigible by considerations from the objective domain of public evidence.

The neo-positivists however, quickly moved on the more congenial 'logical sense' of probability—the implicit definition of probability in the axioms of probability theory—and the 'empirical sense'—the relative frequency notion. The logical sense requires saying that obviously corrigible claims about the probability of an event—or theory—are analytically true, an absurd conclusion. (Of course, they may be analytically true *relative to one's assumptions* but the positivists had long since rejected that kind of defense for astrology; truth claims bring one assumptions and not just one's deductions into question.) Since it was clear and not disputed by anyone that the relative frequency sense can't handle the probability that a new hypothesis is true—and some other important cases—the neo-positivists were left without a plausible account of probability, *just because they were unwilling to accept the double-aspect feature of epistemic concepts* such as knowledge, understanding, and now probability.

They were forced to the multiple-meaning approach simply because they had such a narrow conception of meaning. There is not the slightest reason to suppose there's more than one sense of "probability", one which reflects itself in the 'subjective sense' because we try to believe what's true, in the logical sense because we try to make our axioms reflect the truth about the concept being axiomatized, and in the relative frequency sense because that's a useful standard context. Of course, probability is a function of the state of our knowledge at a particular time; in the basic meaning, it's the knowledge of an individual, and in standard contexts, it's the pooled knowledge of the scientific community at a particular time, as when we talk about the plausibility of a new scientific theory.

APPROXIMATION

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[26] For example, we may decide that a doctor did in fact believe that a certain medicine was very unlikely to hurt a certain patient (a probability claim) but—far from accepting this as proof that in Carnap's sense of probability as degree of belief, the claim is incorrigible—we may be able to prove that this belief in the low probability of harm was incorrect, and even culpable. And we would do this, not by switching to one of the other definitions, but in terms of the standards of reasonable belief about likelihood.

40. It was significant that the logical positivists never discussed approximation, and came pretty late to probability. Their paradigm of scientific truth was the exceptionless generalization, and they thought that natural laws were normally of that kind; in fact, there are almost no such laws. The importance of laws is that they are simple and usefully near the truth for a useful part of the range of their variables; in fact, almost all of them are simply approximations, and inaccurate by several hundred per cent for large slices of their range[27]. We are lucky to get that much; it makes survival possible. And also wholesale self-destruction.

Approximation is simply one more survival device. Our brains are not capable of memorizing anything like the true graph showing the behavior of the simplest gas of all (hydrogen), let alone the differences between that and other gases. We settle for the General Gas Law. If we need exact figures, we determine them experimentally, but we rarely have that need. What we constantly need is a general sense of how gases behave, and the approximation gives us that; it gives us understanding, more precious by far than specific factual knowledge, because so much more far-reaching. It gives us the link to the kinetic theory of gases, the explanation of how internal combusion engines work, and a million other insights. A little inaccuracy is a small price to pay for all that; *if* you approach these matters pragmatically, not in terms of mathematics as an ideal and with demands for standalone syntactic translation rules for your definitions.

IDEALIZATION

41. Ideal types and natural kinds are further elements in the pragmatics of the mind/brain struggle for survival. They emerge implicitly in concept formation, and come to the front as an explicit element in the definition of taxons in developing classifications. And we have just seen the thin end of the wedge that gets them into the formulation of laws; by defining the ideal gas as one that obeys the General Gas Law, the perfectly elastic body as one that obeys Hooke's Law, and so on, we are able to formulate the laws as correct instead of incorrect (albeit approximately true) generalizations. In fact, they become tautologies, but properly understood, remain as enlightening as ever, contrary to the positivist view that they must then cease to be informative about the external world.

Idealizations are not accidentally given names like 'ideal gas', 'perfectly elastic body', names that have evaluative connotations, as does the term 'idealization' itself. Such substances are ideal *for the purposes of science and of understanding*. And those are the purposes with which the scientist is concerned. This terminology is a clue to the true function of the logic of science, namely to discover the way in which concepts and laws are formed, found, and framed, so as to resolve the tension between the mental limitations and the physical complexity which have to be co-ordinated in order to develop a satisfactory science.

CAUSATION

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[27] The aversion to discussions of the concept of approximation persists, as we can see by the absence of the term from the index, let alone the glossary, of the recent 800pp anthology of key work in the philosophy of science referenced earlier. The extent of the resistance to the notion was brought home to me when I introduced the discussion of it in "The Key Property of Physical Laws—Inaccuracy" (in ***Current Issues in the Philosophy of Science***, Feigl and Maxwell, eds., Holt, New York, 1961, pp. 91–101). Herbert Feigl, one of the editors of this volume, implored me to change the title of my paper since he thought I couldn't *really* mean what it implied.

42. Analysis of the concept of cause provides a particularly illuminating example of psychologic. One still encounters the simplistic suggestion that cause means necessary condition[28], despite the obvious counter-examples, e.g., those provided by overdetermination. There are still lingering beliefs that causation cannot operate at a distance, whereas the simplest thought-experiment about a new power that operates instantaneously and without the benefit of a propagating field shows we would not hesitate to use causal language about its —what can we call them except—effects. The best account that can be given in syntactic terms was established long ago and it still leaves counter-examples[29]. The crucial further step is to bring in contextual criteria, which is most easily done by introducing the notion of a 'contrast class'[30]. Thus, to someone in a room watching someone else closing all the windows, the last of which breaks, the answer to the question Why did that window break? must be in terms of differentiae of that closing-event from the other closing-events, e.g., that it was slammed harder, already had a crack, etc. But for someone who walks into that room after the window breaks and asks 'exactly the same question' (i.e., uses the same language), a more appropriate answer will be to point to the person who was closing the window and say that s/he broke it when closing it. The contrast class for the first inquirer is the windows that did not break under conditions that seemed to be the same; for the second inquirer, the contrast class is with the broken window's unbroken state a few minutes earlier. Causes are mini-explanations, and hence must relate to the gaps or tensions in the understanding of those who call for or seek them. There is no syntactic solution to analyzing causes any more than explanations, *but*, on the other hand, there is nothing invalid, unreliable, or idiosyncratic about the answers provided. We see here the general message: the concepts in the logic of science are designed to work in—and hence they involve implicit reference to—a cognitive (psychological) context because, to put it bluntly, that's what science itself is created to do.

43. And that is the context of what has been called 'volitional causation' or the 'rational-action paradigm' referred to earlier. The entire misguided attack on 'reasons-explanations' spearheaded by Weber's attack on the value of suicide notes was just another example of the overreaction to anything which had an element of reference to internal states, a kind of paranoia that confused something that was descriptively subjective with something that was evaluatively subjective, i.e., unreliable and not meeting the standards of science[31]. It led, unfortunately but inexorably, to the denial of the legitimacy of free will in the strong sense in which that holds, again because of the insistence that the inner story must be superficial and the true (reductionist) story correct. There is indeed a sound idea behind reductionism, which can be expressed simply by saying that explanatory reduction is a legitimate ideal. But ontic reduction is not only counter-intuitive, there are no sound or even plausible arguments for it now.

One of the great stumbling blocks in the way of understanding the mind has been the implicit acceptance of the idea of explanation as proof, even by those in the anti-positivist camp. Thus, it is thought to be an objection to reasons-explanations that they may be given of two different actions, between which an agent is trying to decide. But why not? That is the logic of causation, something which has served us admirably throughout the history of science and before that throughout the history of technology. Explanations are usually just proximate causes, not sufficient conditions. That is a limitation, indeed, but

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[28] As is mentioned in the glossary for the 800pp anthology ***The Philosophy of Science***, edited by Boyd, Gasper, and Trout (MIT Press, 1991).

[29] A cause is a non-redundant member of a set of conditions which are jointly sufficient. See, for example, "Causes, Connections, and Conditions in History", in ***Philosophical Analysis and History***, William Dray, ed., Harper and Row, 1966, pp. 238–264; reprinted in various anthologies in the US and Italy. John Mackie's analysis was essentially similar.

[30] Originally done in the citation of the last footnote, and now widely accepted.

[31] An extension of the same point leads naturally to the compatibility of conventional accounts of free will and determinism

only in the sense in which having a net worth of between a million and a billion dollars is a limitation in buying a house. Certainly, there is something fancier than a causal explanation, but what it provides is enough to change the universe, and moreover, it's attainable by mere mortals.

EVALUATION

44. If there was one concession the logical positivists were less likely to make than adding psychological factors to the logical analysis of scientific concepts, it was that of adding evaluation to the list of legitimate scientific activities. Indeed, they tarred it with the same brush—they thought evaluation was just one more example of bringing in psychological factors, since they made the same kind of mistake they made with the careless dismissal of belief in working on the analysis of probability. That is, they smelled the odor of subjectivity (in the descriptive sense of internal access), and concluded that this meant objectivity (in the evaluative sense of validity) had been banished. They thought that evaluation was just the expression of opinion, taste, or preference, an entirely subjective matter—and of course, that meant subjective in the bad sense as well as the descriptive sense. They were far off the mark on both points; evaluation is neither the mere expression of taste, nor is it necessarily or even typically non-objective.

This would have become obvious to them if they had been more serious in their examination of the process of science, since that process essentially involves and is impossible without evaluation—the evaluation of experimental designs, of the quality of research papers, of data, of arguments, of instrumentation, and so on. Their model of an evaluative statement was a straw man: for a budding researcher to say that she or he *likes* to work in cryogenics is an expression of personal, subjective, value. To say that cryogenics is a *good* area for a beginner to work in, or that someone has just done some very *good* work in cryogenics, on the other hand, is to go much further—and also less far; it requires support of an entirely different kind, and has entirely different consequences.

The nature of evaluation and its place in science have recently been elaborated on at great length by the present author, so the temptation to go into it any further here will be resisted[32], except for one short comment. Properly done, evaluation serves, amongst other purposes, the data-synthesis task served by measures in descriptive statistics. The letter grade awarded to a student—by the same social scientist who proclaims that evaluation is just an expression of preference—is an extremely efficient, concise and objective measure of their work, for certain crucial purposes (when appropriate care and skill is exercised). Thus evaluation serves as one more weapon in the armory of the brain in dealing with the complex world; we remember which brand to buy, which restaurant to visit, although we have long forgotten the details of the proof of its merit.

45. The logical positivists here overestimated the psychological element in a concept, whereas in other cases they underestimated it. The result in either case was to seriously handicap, not just the philosophy of science, but science itself. It seems likely that we lost most of the contribution the social sciences could have made to human affairs in the twentieth century mainly because the doctrine of value-free science dominated the practice of the social sciences.

It is somewhat ironic that the attacks on the value-free doctrine from the 'left wing'—critical theory, neo-constructionists, et al.—are essentially irrelevant since they misunderstood the doctrine itself just as badly as the logical positivists misunderstood evaluation itself. Supporters of the value-free doctrine never argued that scientists do not have values that influence their choice of areas to work in, sometimes

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32 "Introduction: The Nature of Evaluation" in ***Evaluation Thesaurus,*** (4th edition, Sage, 1991), pp. 1–43.

inappropriately affect their conclusions, nor did they deny that science can be put to use in the cause of many values, often immoral ones. To produce those truisms as the death knell for the doctrine of value-free science is announce you have just killed a straw man. The doctrine of value-free science is much more sophisticated, and supported by interesting arguments from Hume, Moore, and others, e.g., about the impossibility of drawing evaluative conclusions from factual premises. It is the doctrine that *within science* one cannot infer facts from values, and hence that value conclusions have no place within science. Nevertheless, the doctrine fails miserably, for reasons which are so straightforward that the failure to see them suggests there were some other factors at work besides simple errors of reasoning. One possibility is the fear of evaluation which haunts us all since we know very well how vulnerable we are to its strictures; another is the wish not to confront the power elite, which was certainly Weber's motivation when he introduced the doctrine to sociology.

INFERENCE & PROOF

46. If evaluation was the last candidate the logical positivists would ever accept, deductive proof was the last they would give up—or perhaps next to last, after observation. Proof was derivation by use of logical principles of inference, and it was an 'effective' notion in the technical sense of being checkable for validity in a routine way without special skill. Thus it was the epitome of objectivity in the sense of intersubjectivity, and with the connotation of desirability. However, the notion of proof is far from routine. Wittgenstein's most literate student, John Wisdom, once put the psychologistic view of proof in these terms: "Logic is proof, proof persuasion, and philosophy logic played with especially elastic equations". The logical positivists would have said that persuasion is the task of rhetoric, not logic, and the trouble with philosophy was simply the tendency to get into the areas where questions were not decidable, because not expressed in terms of observables. Proof was safe haven: whether or not someone was persuaded by a proof was their business, subjective business, but a proof was a proof, objectively so.

This view became increasingly untenable, first with the constraints of the Godel theorems, then as the full significance of the non-Euclidean geometries and intuitionistic mathematics sank in, and finally with the use by Reichenbach and others of bridge laws, ostensive, and other semantic definitions, which made the validity of proofs about physical world entities dependent on the consistency of bridge laws and their application, a chancy business at best. Today there is the extra problem of whether computer-created proofs are really proofs. The objectivity of proofs, *as the logical positivists conceived that objectivity*, has evaporated. Of course, while this meant the end of the Erlanger program along with various other dreams of the Vienna Circle, it did not mean the end of rigorous proof, because rigor is something a little less than machine-effective, and that little less salvages all the objectivity we need. It doesn't mean there are never any errors, or even that machines can do the work for us; but we can have almost no errors and machines can do a great deal of the routine work for us. In the end however, we have to look at our hole card—the ultimate assumptions of the proof—and we have to settle the *pragmatic* question which of these are most appropriate for the particular kind of proof we are interested in.

Thus, there was no need to panic at the sight of a subjective element in the sense of a reference to the present cognitive states and even the foundational assumptions of the participants. We can manage those and we can't eliminate them. They are no more threatening to sound science than the Supreme Court is to sound justice.

47. When it came to the other half of scientific inference—inductive or abductive inference—the logical positivist stance was bivalent. The Carnap team went for statistical inference as a legitimate option, although validation was always a worry (hence Hempel's introduction of the 'requirement of total evidence'). The Popper group (and the evolutionary epistemologists) rejected the very idea, settling

instead for guessing, checked out by confirmation and rejection. Both were in trouble because both took too narrow a view of evidence, inference, and hence proof. They made the mistake of thinking it had to be governed by stateable rules, following on their commitment to the idea that definition had to be in terms of stateable rules, even if expanded to include semantic or implicit ones. They could hardly maintain the effectiveness of proof and allow sloppy definitions, since deduction is, in a sense, just derivation by means of the rules that define the meaning of the terms involved.

The absurdity of the idea that scientists should be described as reduced to guessing at every non-deductive conclusion they espouse never bothered the US wing of the neo-positivists and was the UK (Popperian) *piece de resistance*. They were, they said, not concerned with analyzing matters to suit current usage, only with correct analysis. However, it was their analysis which was at fault. Once we have seen the extent to which the intuition of highly trained mathematicians still enters into deductive proof, and is normally highly reliable, it should hardly be unbearable to concede that the same is true in science.

What are the hallmarks of a type of sound non-deductive inference that is not just guessing (that turns out to be right)? It must be notably more successful than mere guessing, and it must be learnable, teachable, and on most occasions testable. Now we all know that it is possible to teach science students—for example, medical students—how to infer conclusions from evidence, and that their level of reliability in doing so can become very high in relatively stable evidential environments. How should we describe this achievement? Of course, the correct description is that this *is* teaching and learning inductive scientific inference in the medical field. This process is assisted by 'rules of thumb' of various kinds, sometimes even by generalizations of an exceptionless kind. But it is not reducible to deduction from any set of such rules. Considerable judgment is involved in the application of the rules, and in training students to make appropriate responses in cases where no rules come near to supplying the answer.

It is here that evolution exercises its powers to select those who have or can learn a 'feeling' for the natural order'. The evolutionary epistemologists only needed to carry their line of reasoning one step further, so that evolution's role would not just be weeding out false conclusions but the reasoning processes that generate them. They were too fascinated by the chance of improving the analogy with evolution by letting random variation correspond to something, and finished up making it correspond to guessing.

All that can legitimately be meant by 'guessing' in Popper's sense is that the step to the conclusion is not deductive. Nobody thought it was, except perhaps the other wing of the logical positivists who kept coming up with ways to reduce induction to rule-governed inference and then facing the problem of how they would know that the rules were correct. Inductive inference is often highly reliable, highly learnable and teachable, and checkable; only someone on a philosophical binge could suggest that it should be called guessing.

So inference of either type involves a psychological element, even a judgemental element, as well as heavy contextual elements. This is the element of truth in all attacks on the logical positivist program; but it does not justify to even the slightest extent the sceptical or relativist conclusions.

CONCLUSION

48. We could pursue these matters considerably further, for example by looking at measurement, at the paradigmatic psycho-logical concept of randomness, and in more detail at the way in which the concept

of law serves as an information compression device that helps us to deal with the *future* information input flood, and not just the past, despite our limited memory space and calculating power. But perhaps enough has been said to help us see the scientist's struggle to discover the truth about the world and its inhabitants as a struggle to meet needs for knowledge and understanding, both of which have survival value. Of course such studies also have intrinsic interest, as we evolve more esoteric value systems; but their pragmatic context should be seen as their main driving force.

So instrumentalism is not enough, because truth that survives new methods of looking is more valuable than truth which does not. Logical positivism is not enough, because its model of scientific knowledge and understanding was a very primitive oversimplification of a very sophisticated set of procedures and their results. The phenomenological and constructivist alternatives are too much to be workable and too relativistic to be sensible. Yet they were entirely right to fight back against the superficial arguments for the dismissal of the mental. The internal world is as legitimate a world for study as the external; it needs somewhat different tools, and it contains significantly different phenomena. The legitimation of that world, need not, however, be at the expense of the legitimacy of the study of the other. And neither study should be pursued without careful investigation of the links between the two worlds.

In this paper we have tried to explicate an approach that provides some of those links, at the methodological level. It has tried to extend what we now know, in ways that still make sense, towards a synthesis of the inner and outer worlds, a synthesis that offers a step forward in the logic of science—and a foundation for steps forward in science itself.