A Fallibilist Social Methodology for Today's Institutional Problems

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By John Wettersten

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### INTRODUCTION

The rational thinking of individuals is a social activity. It follows rules. But these rules vary from context to context. Sometimes they are rather effective in furthering the pursuit of truth and/or the discovery of solutions to both practical and theoretical problems. But sometimes they are not. Central tasks of the social sciences are the identification of which rules are followed in which contexts, the appraisal of their good and poor qualities, and the development of new alternatives which do better. These problems go beyond the social sciences into political and moral problems on how to live well. The series of essays in this and a forthcoming book explain why these tasks are central to the social sciences and how attempts to solve them can improve to some degree both research in the social sciences in general and moral and political discussions in particular. They thereby offer specific suggestions for how important tasks may be more usefully pursued in various contexts.

The view of rationality as a social activity, carried out by individuals in social contexts, is explained. This perspective is an outgrowth of Popper's philosophy of science and the widespread critical discussions of it. Popper's philosophy developed from its beginnings, as he was still a member of Würzburg School in psychology; it continued until he passed away after his highly productive years as a professor at the London School of Economics. This development was at times quite good, as he moved from his limited description of the logic of empirical research to his broader studies of rationality. But at times it failed to move forward as, for example and central to the perspective of this book, when he rejected the application of his observations, that all rationality is both social and critical, to the rational thought processes of all individuals at all times. And at times his innovations moved backward as, for example, when he tried to reinvigorate the three-world theory he adopted as a student of Karl Bühler: he mistakenly took psychology out of the picture, just as psychology was needed for his three-world theory and he neglected the importance which metaphysical theories may have for research of varying kinds.

#### Introduction

The emerging fallibilist perspective forcefully leads to significant modifications in virtually all the social sciences. These modifications are analyzed here in detail. In both economics and in sociology the widespread adherence to methodological individualism—Popper's perspective—or the rationality principle—often a justificationist version of Popper's view— has to be revised. If individual rationality is social, the social conditions and social practices of individuals have to be studied and used to form the basis of broad economic and sociological explanations. If rationality is social, then cognitive psychologists need to study how thinking individuals interact with each other to solve both their everyday and their theoretical problems. And psychology alone cannot provide a theory of how knowledge can be attained, as so many psychologists from Wilhelm Wundt to the present have sought to show without success.

In order to explain how attempts to solve today's problems can be improved, it is of crucial importance to demonstrate how mistakes are made today. But this problem has been handled rather poorly. A central reason for this result is that social scientists and philosophers have sought far too intensively on finding an ideal approach, when the majority of problems can be better handled and more significant progress made, when partial improvements to existing practices are sought.

This mistaken approach is largely a consequence of the mistaken perspective which permeates nearly all social scientific research, that is, the widespread influence of the positivistic philosophy of Rudolf Carnap and his students, collaborators and defenders. The theoretical mistakes of this philosophical movement and its widespread and damaging impact are here examined. A crucial aspect of the reform of the ways we try to solve problems lays in the rather detailed identification of the rules and techniques, which are products of Carnapian positivism, and which cause avoidable difficulties in our ability to solve problems.

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## Part 1

### CARNAPIAN BLOCKS TO PROGRESS TODAY

When Popper put forth his early methodological theories of empirical research, one of his major competitors was Rudolph Carnap. Carnap had already built up a significant reputation with his *Aufbau der Welt*. He had raised hopes high that a new philosophy of science, based on the new developments in logic, would provide an explanation of how certain knowledge based on logic and sensations alone could be obtained. Speculation and errors could be avoided, if one only stuck to the two real aspects of knowledge, that is, to formal logic and combinations of sensations. It was a reincarnation of the hope raised by Francis Bacon; certain knowledge would be attained with indubitable methods. Young scholars such as Willard van Orman Quine came with great hope and excitement to Prague to absorb from Carnap the new revolution in the theory of scientific knowledge.

But Carnap's doctrine was not a finished product. It was at best a work in progress with significant gaps that were not easy to fill. But the hope of finding a certain and final theory of knowledge led many to pursue an allegedly known path to find the needed ways forward to epistemological certainty. If the mistakes in Carnap's approach could be found and overcome, the rewards would be considerable. But Carnap never came to some settled and generally recognized result. The work on finding the perfection of his program simply proceeded until today. And, as the weakness of Carnap's program became clearer, the alternative developed by Popper became stronger. The Carnapian School attracted far more members. It was, after all, an inspiring attempt to reach a goal which had been sought for centuries. But, after Popper's time in New Zealand and his return to Europe following the war, he attracted highly talented followers, who never let the Carnapian scholars forget, that Carnap's program suffered from central difficulties, which might appear somewhat differently at different times, but which were never overcome. The Carnapians knew the strength of Popperian alternatives and sought to block their influence, wherever and whenever they could.

Part 1

One of the most influential of the attempts to save Carnap's program was made by his student Quine. Quine changed it significantly, but he maintained the Carnapian aim of reducing all knowledge as completely as he could to two aspects, that is, formal logic and the combinations of sensations. The first essay in this section tells the story of how he proceeded from one task arising out of Carnapian inadequacies to another, without ever reaching his Carnapian goal.

# 1a. Quine's Journey: Did He Find His New and Glittering Sights?

Abstract: The question is posed: Did Quine overcome the difficulties, which he found in Carnap's philosophy? In order to find an answer, the development of Quine's philosophy from his early Carnapian view to his mature philosophy is portrayed. The central difficulty to be overcome is the gap between empiricism and science. Quine's attempt to bridge the gap is deemed two-fold: he offers an empiricist theory of real languages and a so-called naturalized epistemology. In both aspects of his philosophy, he stays too close to Carnap to bridge the gap between empiricism and science. He puts closing the gap with better theories of language learning, perception, etc. on the agenda. But the Carnapian goal of unity vanishes like a mirage, just when one thinks one is approaching it.

### 1. Quine's Journey

Quine is widely reputed to be that philosopher who has traveled the farthest of all. He started his journey from a position within the confines of the logical positivist camp. But, according to his reputation, he developed his own criticisms of logical positivist views with such sophistication, breadth and depth that his own philosophy became one of the most original, most comprehensive and most challenging of all contemporary alternatives.<sup>1</sup> This widely held view of the development of Quine's

<sup>&</sup>lt;sup>1</sup> The list of essays and commentaries on Quine's philosophy is enormous. It is difficult to give a list of such publications which is anything like comprehensive and which would still be critical. They seem to me to fall primarily into three categories. On the one hand, there are detailed studies which seek to get Quine right with, perhaps, a correction here and there. There are also studies or discussions of Quine's philosophy which stem from his disagreements with Carnap or with modal logicians. These studies seek, in my opinion, an even more positivist view than Quine's. There are also a few studies of Quine's philosophy which are critical of his theory but not from a positivist point of view such as Ernest

philosophy has hardly been studied. It is important that a philosopher be accessible to outsiders—in this case to people not belonging to the camp of the logical positivists and their immediate descendants. But Ouine is very hard to follow. This may be no blemish since the terrain is rough. And after a pioneer finds a trail to new destinations, those following may hurry along the newfound trail without examining it to arrive at new destinations quickly. But to understand where one is, it is good to know how one got there. A new study of Ouine's philosophical journey might throw light on the puzzling difficulties in reading him. Are the difficulties Quine's philosophy faces systematic? Can one explain them by its origins in Carnap's philosophy? Notoriously Quine's philosophy has a paradoxical air: its conventionalist leanings are combined with realist doctrines; its skepticism and fallibilism are combined with conservative faith in up-todate science. The air of paradox is often taken as a product of Ouine's philosophical development. Can it be more basic and objectionable? Is the fascination of this philosophy produced by surreptitious, back and forth movement between old-fashioned Carnapian ideals and new views of science rather than by the substance of a unified view? An outsider's study of Quine's movement from the Carnapian positivism of his youth to his mature philosophy may still, then, be useful.

The question of whether Quine's philosophy is a replay of Carnap or something quite new has, of course, a trivial and uninteresting answer: It is partly new and partly old. But, if we concentrate on one problem, we may be able to say whether, regarding this problem, Quine's philosophy vindicates Carnap even while modifying his view, creates a new and unified philosophy and/or fails to overcome the difficulties Carnap faced

Gellner's, "The Last Pragmatist," *Times Literary Supplement*, July 25, 1975, pp. 848ff. and Joseph Agassi's, "Ixman and the Gavagai," *Zeitschrift für allgemeine Wissenschaftstheorie*, Bd. XIX, 1988. My own point of view is close to these and I will use them here and there. Dirk Koppelberg in the introduction to his own historical study of Quine's philosophy (*Die Aufhebung der analytischen Philosophie*, Frankfurt: Suhrkamp, 1987, p. 12 and ft. 1) points out how few historical studies of Quine's philosophy there are. Koppelberg wants to fill that gap by discussing the development of Quine's philosophy as well as the place of Quine's system in a historical context. The former task seems to me, however, to be not so thoroughly carried out due to the fact that Koppelberg sees Quine's philosophy—from beginning to end—as a successful system. Nor can I accept the way he carries out the second task, where he views Quine's philosophy as a unification of empiricism and analytic philosophy using Neurath's views. See Agassi above for criticism.

by staying too close to him. This problem is: Can Quine bridge the gap between Carnap's empiricism and science?

To what extent does Ouine adhere to Carnap's empiricism? May his view be seen as a continuation of Carnap's program? Quine first sought to employ fundamental ideas of Carnap's Der logische Aufbau der Welt. He saw that Carnap failed to bridge the gap between his empiricism and science. Quine then rejected Carnap's empiricism and developed his own. Yet it is not clear how different it is. The question is, does Quine's version of empiricism withstand Quine's critique of Carnap's empiricism? Can Quine's empiricism account for science? This problem is: Does Quine offer us a (re)constructed language powerful enough for science but restricted enough to conform to his own empiricist principles? Alternatively, does he offer some substitute for a language, an empiricist interpretation of scientific language that will perform the same task? Can Ouine attain the Carnapian goal of the exclusion of (meaningless) metaphysics from science? If not, does he allow for metaphysics and does he demarcate it from both science and language? In short, are Quine's empiricism and science reconcilable?

The youthful Carnap had hoped to create a unified language for science based on given experience (or terms referring to given experiences) and logic alone, to the exclusion of all metaphysical frameworks. As Quine's numerous references to this project indicate, he was deeply impressed. Somewhere along the way, however, he lost his faith that Carnap could bridge the gap between his empiricist-logical point of departure and real science: the languages which Carnap sought to (re)construct could not carry one from his point of departure all the way to science. Quine went his own way.

As Quine drew his new travel plans, he did not fully abandon the hopes of reaching those beautiful sights of a completely scientific landscape which Carnap had so vividly pictured as he related his youthful dreams of far-off places. The new plans were designed to find new sights similar enough to those unreachable ones, which Carnap had dreamed of, to retain the empiricist attractions of the old but modern enough to be found along new and passable roads. Quine undertook to modify Carnap's travel plans just enough to find these new roads but to modify them not too much to avoid landing in some wholly foreign, thoroughly metaphysical land.

Was Quine's new journey successful? Did he find a passable road to new sights which enable him to find a language adequate for science, which

can detect and avoid metaphysics, and which conform to revised but still sufficiently empiricist principles? Or does he fall prey in more subtle ways to the difficulties encountered by Carnap on his journey and discovered by Quine? Does he set out to find some apparently new destination which he cannot reach because he, just like Carnap, allows himself only empiricist provisions and vehicles and these are insufficient to travel along those roads to science, which are open? Or does he smuggle in quite other means of travel when the going gets rough?

Quine's journey was not, of course, the only one whose point of departure was the breakdown of positivism. The path from the positivists— especially Russell and Carnap—to Quine or the path from the positivists to Popper's later philosophy are in this respect two quite similar examples. The attempt by Leonard Nelson to find a quite different road by capitalizing on Hilbert's advances in logic in order to refurbish the philosophical method of Fries collapsed and fused with Popper's.<sup>2</sup> In each case the need to go beyond the limits of old fashioned positivism to new attitudes toward meaning and demarcation on the one hand and/or toward methodology on the other hand were crucial aspects in the development of new views as well as steps which lead to far more comprehensive views of science.

The journey undertaken by Popper was wild and adventurous with interesting side trips and many surprising discoveries. At first blush the journey undertaken by Ouine seems in comparison to have been a rather dull trip, which not only stayed as close as possible to the directions which the tour guide—Carnap—had worked out in advance but did so even after it had become obvious that they led one far away from the desired destination of a modern theory of science. Many of the interesting sights that one might have found along the way were thereby ignored. Just why one should try to stay close to the views of early Carnap after they have failed may seem just as puzzling as if one were determined to visit Livorno because one found the road to Florence blocked. Quine's explanations of the rationale for his travel plans are rather sparse and not very satisfying. We might, however, be able to see the wisdom or folly of following Carnap's directions as closely as possible if we look at the journey which Quine undertook. Perhaps we metaphysically inclined foreigners have failed to appreciate the attractions of the austerity of the

<sup>&</sup>lt;sup>2</sup> For discussion of the relation of Nelson's to Popper's attempt see John Wettersten, "The Road through Würzburg, Göttingen and Vienna," *The Philosophy of the Social Sciences*, Vol. 15, No. 4, Dec. 1985. pp. 487-506.

neo-Carnapian natives. But as an alternative we should keep Popper's journey in mind in order not to miss the best sights, should they fail to be on Quine's plans.

### 2. Quine's Youthful Home

The point of departure for Quine's philosophical journey is Carnap's research program of *Der logischer Aufbau der Welt.*<sup>3</sup> In this book Carnap tried to explain how knowledge could be unified: all scientific concepts should be reduced to logic, given experiences and constructions built with them alone, to avoid all metaphysical frameworks.<sup>4</sup> This austere research program sought to exclude from knowledge everything outside of science to base all that remained on given experiences alone. Carnap's reconstruction should show the boundary between meaningful science and meaningless metaphysics. Carnap's chief problems were to construct with his limited means a foundation which was broad enough and a superstructure which was strong and high enough to meet the demands of those towering edifices already built by scientists with other, richer and less tidy means.

This task could be accomplished Carnap hoped, by defining all (meaningful) concepts in terms of elementary experiences, perhaps even in terms of one (remembrance of similarity), with the tools of the new logic. A new formal language should thereby be developed in which all true sentences can be constructed with the use of these concepts alone. All meaningful synthetic sentences would be verifiable, since their meaning is rigorously constructed out of immediate experience, and all analytic sentences are trivially true.

This is the core of Carnap's program which so engaged Quine.<sup>5</sup> It is unclear whether Quine ever accepted Carnap's version of this program since it included the construction of a new formal language for science. Already in the thirties Quine proposed that the definitions which laid the basis for axioms or postulates had to evolve out of previous views rather

<sup>&</sup>lt;sup>3</sup> Rudolf Carnap, *Der logische Aufbau der Welt*, Hamburg: Felix Meiner, 1961, (first published, 1928).

<sup>&</sup>lt;sup>4</sup> ibid., pp. 2-3.

<sup>&</sup>lt;sup>5</sup> Quine suggests, following Carnap, that there are two aspects of a theory of science. These are a theory of concepts or of meaning and a theory of doctrine. We cannot say much of anything about doctrine since we cannot overcome the Humean predicament. (Epistemology Naturalized, p. 72) The theory of meaning, then, is central. Quine is concerned, then, with the conceptual problem of the *Aufbau*.

than be constructed as parts of artificial languages.<sup>6</sup> This task is part of science or mathematics or logic. It is a difficult but technical task. Since the nature of the methods is clear, there is little room for philosophy.

This view, as well as his later view, is nevertheless in accord with Carnap's philosophy insofar as it deems the primary problems of the philosophy of science to be problems of language and the problems of language to be those of explaining how the meanings of words are derived from observation. If we have a more or less pure empirical language, the appraisal of scientific theories should follow automatically.<sup>7</sup> There is little

<sup>&</sup>lt;sup>6</sup> Quine begins "Truth by Convention," (first published in 1935) in The Ways of Paradox, New York: Random House, 1966, p. 70 as follows: "The less a science has advanced, the more its terminology tends to rest on an uncritical assumption of mutual understanding. With increase of rigor this basis is replaced piecemeal by the introduction of definitions. The interrelationships recruited for these definitions gain the status of analytic principles; what was once regarded as a theory about the world becomes re-construed as a convention of language. Thus it is that some flow from the theoretical to the conventional is an adjunct of progress in the logical foundations of any science." The purpose of this essay is to question the sense of the distinction between the physical and the natural sciences according to which this progress may be completed in mathematics but not in physical sciences. Mathematics might be deemed conventional in the sense that it expresses through definitions logical truths but this does not mean that logic is purely conventional. To show how and to what degree mathematics is a conventional transcription of logic is an important and difficult technical task. But even what it might mean to say that mathematics is merely conventional requires further clarification. In "A Logistical Approach to an Ontological Approach," (first circulated as pre-print in 1939) in The Ways of Paradox, pp. 64ff. Quine presents his view that to be is to be the value of a variable. One problem which he wishes to solve in this essay, however, is how we may limit the entities whose existence we presume while extending the power of our language. His proposal is to extend the language by definitions which may function as if they were the names of entities but which may be removed. We may thereby introduce fictions or fictitious entities. There is still an ontological question, however, which is: How economical an ontology can we achieve and still have a language adequate for science? In each of these essays reduction is deemed an aim but in each case it is treated also as a task whose outcome we cannot yet be sure of. Rather, we need methods for carrying out reductions as far as we can. His problems, here, were to devise methods for carrying out these tasks.

<sup>&</sup>lt;sup>7</sup> The solution to a conceptual problem, that of determining similarity of objects, is developed within a science and renders appraisal of sentences in science clear. The problem of induction thus dissolves into a problem for science. Quine, "Natural Kinds," in *Ontological Relativity and other essays*, New York: Columbia University Press, 1969, pp. 114ff. esp. pp. 121, 138.

more that needs be said about science. Quine's new stance is, thus, quite in accord with Carnap's early philosophy even if not identical to it. On Carnap's philosophy we aim to construct a formal language adequate for mathematics and science and based in a minimum of elementary experiences. On Quine's youthful view one seeks the same goal by modification or refinement of existing theories or better, by translating them into a more proper language.

In his youth, then, Quine was Carnap's fellow traveler. They shared the same goal and many of the same methods. They seemed not to have too much to quarrel about. Even if Quine had already in 1935 stopped following quite strictly the lead of his neighbor, who then brought out his first sequel to *Der Aufbau*, that is, *Logische Syntax*,<sup>8</sup> he set out to reach the destinations proposed by him with many, if not all, of the same methods.

Quine proposed, then, a modification to Carnap's program quite early on. He rejected the view, namely, that the conventional nature of logical truths—should their truth be conventional—laid the foundation for the possibility of the construction of varying languages for science. He proposed instead that movement toward the translation of synthetic or borderline statements into conventional truths had to follow by refinement of languages which one found at hand. The goal of the expression of all mathematics and science in a formal language remained. Reductionism was still deemed possible. But the path to its realization was different. The task at hand was not to show how to construct a language for the expression of all scientific and mathematical truths but to investigate the real possibilities for the translation of such truths into sentences demonstrably reducible to observations and logic. The goal, then, is to find an appropriate translation for all such truths.

### 3. Carnap's Destination is a Mirage

When Quine announced in 1951 that he deemed central dogmas he had endorsed in his youth to be false, he chose to ignore all those, outside of Carnap, who preceded him and who might have given the new traveler some useful tips about where the interesting sights were to be found if these dogmas offered poor directions.<sup>9</sup> He writes instead of two dogmas of empiricism, where "empiricism" may be deemed an alias given to the

<sup>&</sup>lt;sup>8</sup> Rudolf Carnap, *Logische Syntax der Sprache*, Zweite, unveränderte Auflage, Wien, New York: Springer Verlag, 1968 (first published 1934).

<sup>&</sup>lt;sup>9</sup> Willard van Orman Quine, "Two Dogmas of Empiricism," *From a Logical Point of View*, New York: Harper & Row, 1963, pp. 20ff.

logical positivism of his first tour guide Carnap. The alias should not mislead us into thinking that the discussion of alternative plans has a wide scope. Indeed, Quine relates his journey as if, at the time of his departure, he knew of no previous refutations of dogmas of "empiricism ". Only those two dogmas and only Quine's criticisms of them—criticisms which have been subsequently so much discussed while others have been ignored—are mentioned. Quine may like to travel but he mentions foreigners rather reluctantly and the Carnapian natives follow suit.

There are many reasons why Carnap's program of the reconstruction of a unified language for science or even Quine's program of the attainment of the same goal by refinement of languages at hand cannot be carried out. They were quickly apparent to some.<sup>10</sup> The (re)constructions of scientific sentences were by no means adequate, since, for example, there are dispositional terms in science (which thus had to be taken as meaningful) which could not be adequately defined in terms referring only to immediate experiences—even if these latter terms did exist. It was in any case already known by psychologists such as Popper that simple given experiences could not be identified.<sup>11</sup>

<sup>&</sup>lt;sup>10</sup> See, for example, Karl Popper, "The Demarcation Between Science and Metaphysics," in *Conjectures and Refutations*, London: Routledge and Kegan Paul, 1963. The development of Carnapian philosophy is hardly well studied due to the fact that the members of Carnap's school do not openly discuss the role of Popper in this development and the members of Popper's school are not interested in it. If we look at Quine's short historical portrayal," Five Milestones of Empiricism," in *Theories and Things*, Cambridge: Harvard University Press, 1981, pp. 67ff., we find hardly any description of the context in which the portrayed developments occurred. Such an essay is too short, of course, to expect much; but it does, in my opinion, reveal the tunnel vision of this school. Quine likes to travel and this was not, of course, the only journey he took. The most important other trip was in logic and mathematics. Hao Wang in "Quine's Logical Ideas in Historical Perspective," in *The Philosophy of W.V. Quine*, edited by Lewis Edwin Hahn and Paul Arthur Schilpp, La Salle: Open Court, pp. 623, has described this development though I do not think that this is the last word.

<sup>&</sup>lt;sup>11</sup> Carnap in the *Aufbau* appealed to Gestalt psychology among others. (p. 92-93). This could, perhaps, allow him to take a relation—similarity or remembrance of similarity—as a given experience though this still sounds rather associationist. In fact he was not clear as to which psychology should be used and thus what should be taken as given. This had to be left to psychological research. (p. 148) The assumption that something is" given" is, however, already a psychological hypothesis. For Popper on the psychological base of science see *Logik der* 

Although this was already known in the thirties, Quine apparently held to his modified reductionist program described above until the 1950's. Just when he rejected this program is, perhaps, not clear. The views presented in 1951 where, he says, developed earlier. But if Quine had been clear about these dogmas in 1947 he would hardly have written his famous essay with Goodman in which nominalism, that is, reductionism, is so clearly proposed.<sup>12</sup> During the fifteen or sixteen years between his first modification of Carnap's program in the middle thirties and the outbreak of a real struggle in the early fifties we find, apparently, a restlessness but no clear-cut outcome.

In "A Logistical Approach to the Ontological Problem" of 1939 his proposal that to exist is to be the value of a variable is presented along with a reductionist program. In "On What There Is" of 1949 this proposal is refined and more elegantly presented but with no reductionist program. In his essay with Goodman, he still seeks a reductionist way out. The program of "A Logistical Approach" splits; the two aspects of his program co-exist for a time and then one part, the strict reductionist part is dropped and a substitute for it sought while the view of ontology is maintained. The heroic efforts undertaken by Quine during this period in the foundations of mathematics were designed to further one aspect of his modified Carnapian program, that is, to carry out some of those technical tasks which were the necessary groundwork for the translation of mathematics into a more refined logical language.<sup>13</sup> How did this effort fit into the various aspects of Quine's long-term program? After the breakdown of reductionism this was no longer clear. A new alternative was needed.14

Forschung, Siebente Auflage, Tubingen: J.C.B. Mohr (Paul Siebeck), 1982, pp. 60ff.

<sup>&</sup>lt;sup>12</sup> In *From a Logical Point of View*, p. 175, Quine forewarns the reader from trying to reconcile the opening sentence of "Steps toward a constructive nominalism," *Journal of Symbolic Logic*, Vol. 12, 1947, pp. 105-122 with views in that book. It should rather be deemed a hypothetical statement for conditions for the construction at hand. In his *Autobiography, The Philosophy of W.V. Quine*, ed. by Lewis Edwin Hahn and Paul Arthur Schilpp, LaSalle: Open Court, 1986, p. 26 he repeats that nominalism was the statement of a problem and not his view. It is not reconcilable with the position found in "On What There Is", which was published the following year. He would be a nominalist, he says, if he could make it go.

<sup>&</sup>lt;sup>13</sup> Quine, in "Truth by Convention," p. 99, describes, I presume, his own undertaking.
<sup>14</sup> Quine's essay "On What There Is" seems to be the defense of a point of view but in fact is a statement of a program. This program starts with an analysis of how

The doctrine which Ouine labels the second "dogma of empiricism," and which—he announces in 1951--must be false and which he now deems the most important.<sup>15</sup> is the following: each meaningful sentence may be reduced to some construction built solely on terms referring to immediate experiences with the tools of logic alone. The central tenet of Carnap's youthful philosophy-a tenet which was maintained even after Quine's early modification of Carnap's program—is false.<sup>16</sup> Before his Wanderlust got the better of him in 1951. Ouine's hometown was, indeed, remote. How could he remain for so many years under the impression that hardly anyone had noticed the breakdown of Carnap's philosophy? (He mentions no one and he is, above all, a gentleman.) To be fair one must note that Quine does not merely criticize the more radical reductionist thesis just mentioned, which he announces at the beginning of his essay, but also the weaker thesis that to each synthetic statement there is associated a unique range of possible sensory events such that the occurrence of any of them would add to the likelihood of truth of the statement, and there is associated also another range of possible sensory events whose occurrence would detract from that likelihood.<sup>17</sup> (This is, as we shall see, very close if

we may analyze what a language presumes to exist: to be presumed to exist in a language is to be a value of a bound variable. This is, according to Quine, a better methodological tool than to talk about names which can be in any case eliminated. But what will this tool bring us? Here the going gets rough. We want both a simple language and a language for science. We want both a phenomenalistic language. because it is epistemologically prior, and a physicalistic language, because we need it for science. We further need a language that is adequate for science. We cannot reduce the phenomenalistic language to a physicalistic one nor do we know how to find a simple physicalistic language which is also adequate for science. We have a host of problems and conditions for neo-Carnapian philosophy but scarcely any answers. Simplicity is a goal but has differing characteristics vis-à-vis differing languages. Reduction hangs in the air. It is seen as not possible but the reductionist tendencies remain strong as in, for example, the discussion of meanings. Mental entities or meanings are apparently rejected because they do not help us reduce meanings to behavior. Can all that be sorted out? We have to look at Quine's development to see how much he can clear the ground in his Carnapian jungle of roots which go this way and that and how far he may travel over seemingly impassable gorges. Finding his way out of his Carnapian jungle is, indeed, a serious matter, for, only when he does so can he explain that integrated view of the philosophy of language, of mathematics and of science upon which all his efforts should rest.

<sup>15</sup> Quine, "A Comment on Agassi's Remarks," *Zeitschrift für allgemeine Wissenschaftstheorie*, Bd. XIX, 1988, p. 118.

<sup>16</sup> Quine, "Two Dogmas," p. 20, pp. 37ff.

<sup>17</sup> ibid., pp. 40-41.

not identical to his own reductionist theory of observation sentences.)<sup>18</sup> Quine notices, then, that Carnap's various attempts to demarcate science from metaphysics on the basis of a demarcation of meaningful from meaningless sentences do not work.<sup>19</sup> They do not work because, as Popper had argued, reductionism fails. The reductionist goal Quine had shared with Carnap is, he now sees, unreachable. It is a mirage.

Although Quine saw that the goal which he and Carnap had been trying to reach was a mirage, he still deemed Carnap a good travel guide. He surprisingly maintains that his response to the breakdown of Carnap's various attempts to demarcate science from metaphysics or to the breakdown of reductionism--the response that our statements about the external world face the tribunal of sense experience not individually but only as a corporate body—issues from Carnap's doctrine of the physical world in the *Aufbau*. This is curious. Carnap's own weakened forms of reductionism were rendered necessary by the breakdown of the *Aufbau* and, as Quine sees, even they will not do.

Quine's appeal to Carnap here, I think, can only be explained if we note an ambiguity between a mere physicalistic hypothesis, on the one hand, and an epistemological hypothesis, on the other hand. Or, to put the same point differently, between a metaphysical and epistemological theory. On the metaphysical conjecture there are only physical events or entities. All theories refer to these only. On the epistemological thesis all knowledge is reducible to statements referring to sensations alone. Having found something like the second in Carnap but having to abandon it, he falls back to the first which he also finds in Carnap. But this now appears to be metaphysics. And this Quine will not have. He needs a new view of it. He later calls this view a naturalistic epistemology, that is, it is not metaphysics but science. We will have to return to this view below to see if it is plausible.

The footnote, incidentally, to this statement concerning the source in Carnap of the idea of his response to the breakdown of Carnap refers to Duhem, who, Quine says, argued well for the (Carnapian?) doctrine he

<sup>&</sup>lt;sup>18</sup> See discussion of observation sentences below.

<sup>&</sup>lt;sup>19</sup> In his "The Demarcation between Science and Metaphysics," Popper defends the same thesis. Popper's account is, however, far more detailed and historically oriented. Although this essay was published in 1964 (circulated since 1956), it relates sequentially those earlier developments which are ignored by Quine and thus gives a far more accurate and informative account of the difficulties of what Quine, in a somewhat provincial frame of mind, calls empiricism.

proposes.<sup>20</sup> But he first learned of Duhem, he also says, only after he wrote this essay. The footnote is added in its republication.<sup>21</sup> We will do better to look more closely at his travels to see how he reaches his own theory of science and to see just what his own destination is. Quine apparently moved somewhat unwittingly, but quite directly, to a Duhemian view of science. But this impression is, as we will see below, misleading: his adherence to Carnapian doctrines led to deep differences between his view and that of Duhem. Perhaps he did not then realize just where the road he was on would lead him and tried later, after he discovered where he was unwittingly headed, to find a different road which would lead him to destinations where he could feel more at home.

# 4. The Analytic-synthetic Distinction Breaks Down: Quine in Search of a New Destination and a Road to Get There

Quine began his portrayal of the breakdown of Carnap's view with a discussion of the breakdown of the analytic-synthetic distinction.<sup>22</sup> This distinction breaks down, he explains, because there are sentences which we intuitively recognize as analytic even though we have no theory which is adequate to account for them. There are two classes of apparently analytic sentences. One class is problematical and the other not. Non-problematical analytical sentences are logical truths. We may deem them analytic on the grounds that they remain true under any substitution of terms other than independently specified logical connectives such as "and," "or," etc. The problematical sentences are not logical truths as they stand but they can apparently be rendered logical truths if we substitute synonyms for certain parts of them.

Quine chooses his central example to reveal the difference between the two types of sentences. "No unmarried man is married." is clearly analytic since it is a logical truth: if we take the expressions "no" and "un" to be logical particles, then we can change all other components of the sentence by substitution, and it will remain true. The sentence "No bachelor is married.", on the other hand, would also seem to be analytic: it can be turned into a logical truth by the substitution of a synonym, i.e., "unmarried man," for "bachelor". As it stands it is not a logical truth. To show that it is nevertheless an analytic sentence we need clear cut means of substitution or of reinterpretation under which the preservation of its

<sup>&</sup>lt;sup>20</sup> Quine, From a Logical Point of View, p. 41.

<sup>&</sup>lt;sup>21</sup> Quine, "A Comment on Agassi's Remarks," p. 118.

<sup>&</sup>lt;sup>22</sup> Quine, From a Logical Point of View, pp. 20ff.

truth would be guaranteed. Quine discusses alternatives and says they are not adequate to provide a theory of the analyticity of this second kind of sentence.

The alleged breakdown of this distinction is not, then, a full breakdown of the distinction between analytic and synthetic statements as traditionally conceived. In logic we may appeal to logical truth to identify so-called analytic statements. What bothers Quine is a breakdown of the ability to use the distinction between analytic and synthetic statements to identify any sentence found in (scientific) language as belonging without ambiguity to either the class of analytic sentences or to the class of synthetic sentences. Quine is bothered by the fact that sentences which we ordinarily deem analytic such as "all bachelors are unmarried," cannot be identified as analytic in a clear-cut way without, say, appeal to intuition concerning the meaning of words.

Why should Quine be bothered by that? We have seen that Quine ended his philosophic dogmatic slumber in the fifties. This awakening was due above all to the reverberating sounds of the collapse of reductionism which not even the intense concentration on making improvements in the foundations of mathematics could block out. In order to see why its breakdown was so important for him, we might ask, then, what is the role of the so-called breakdown of the analytic-synthetic distinction in his early view?

This question might seem superfluous. According to Quine's own statements the breakdown would not seem to have been crucial for his change of view. Quine maintains that he, along with Tarski, had long held that the distinction could not be upheld. He deemed this observation something merely negative however and thus saw no reason to publish. This explanation of his failure to publish is curious, however; and thus dubious. If he found a paradox in a Carnapian system would he not publish it? Is that positive? Is Rosser's discovery of a paradox in Quine's system merely negative and thus not worth publishing? We need not reject Quine's explanation entirely, however, if we want to find a fuller explanation for Quine's silence concerning the breakdown of the analytic-synthetic distinction in the years preceding his "Two Dogmas."

On Quine's early modified Carnapian view there could very well be any number of statements whose status was unclear. The task of scientifically minded philosophers or logicians such as himself was to reformulate or translate such sentences into more refined languages to make them clear. The borders of ambiguity should be thereby pushed back and two clear realms of synthetic (empirical) statements and of analytic statements extended. The disagreement with Carnap over whether synthetic sentences and analytic sentences could be (now) clearly distinguished was a methodological problem: should one start with formal languages which presume such a distinction and seek to construct a language which rigorously holds to it or should one work towards such a language by extending the range, power and simplicity of existing formal languages such as that of *Principia Mathematica*?

This disagreement had already been aired in the thirties. The rejection of the adequacy of current views of the analyticity of such sentences as "all bachelors are unmarried" was, in the light of this early dissent, merely a clarification of his view that there exists a wide gray area of sentences which need to be clarified and analysed. One could even leave open the degree to which one could do that provided that one could approach the same goal. And this goal was still the reduction of all sentences to formal, analytic ones on the one hand and statements of fact, on the other hand. As long as the goal is clear and as long as we may get closer and closer to it, the possibility that it could not be fully reached could be deemed to pose no serious problem. We would learn these things more clearly as we got closer to the goal and discovered the limits of our progress in detail.

In the light of the rejection of reductionism, however, this all looks quite different. The rejection of reductionism means that the goal which had been sought can be sought no longer. One cannot get closer to it as one refines the language(s) of mathematics and science. The existence of the gray area of sentences which are neither clearly analytic nor synthetic is now deemed inherent in language. So far as I have noticed Quine does not discuss possibilities. But it seems to be his view or to follow from it that an increase in precision in one area might bring with it some disadvantages elsewhere as the uses of a language in varying contexts are brought together. The need not only to use sentences which are not clearly classifiable as either analytic or synthetic but also to use sentences which are embedded in particular languages in such a way as to defy further refinement without too great a cost poses not merely a technical problem, that is, a problem of finding ways of translating them into a language in which their status will be clear. Rather, we must come to terms with the fact that all of them are not even in principle capable of clarification. To judge analyses of individual sentences or languages, new criteria, new goals are needed.

We find here a shift away from technical problems of how to increase the scope, power and simplicity of formal languages to new, deeper problems of how to appraise the progress of analysis. Quine does not make this switch in concerns clear. One role of the theory of analytic statements on his earlier view, he fails to explain, was to provide us with a goal: as many sentences as possible should be identified as either synthetic or analytic. Technical success is progress towards this goal. The theory of this goal had been, following Carnap, reductionist. This is clear from Quine's criticism and even his suggestion that the breakdown of reductionism and the analytic-synthetic distinction are the same.

Quine's claim, then, that he had rejected the view that the analytic-synthetic distinction was adequate before he rejected reductionism is ambiguous. Before he rejected reductionism he had only rejected the theory that our demarcation of all sentences into synthetic (empirical) and analytic could be complete. He could still maintain, however, that we knew what our goal was. We knew what it meant for a sentence to be analytic. Indeed, if reductionism is possible, we must have a good theory of the nature of the difference: any successful reduction will make things clear.

In his later view he not only rejects the view that methods for demarcating analytic and synthetic statements can ever be complete but also that we do not even know how to analyze some sentences. We have no theory of analyticity. When he explains that he had rejected the distinction for some time before he published "Two Dogmas," he fudges the difference in levels, between being able to demarcate all sentences, on the one hand, and being able to theoretically explain the difference for a wide and indeterminate class of sentences, on the other hand. Before "Two Dogmas" he held that all statements were not now identifiable as analytic or synthetic, perhaps even that we could not place all statements in one class or the other. After "Two Dogmas" he held that the goal was not clear, that we had no methods for extending such classifications beyond logical truths.

We find here, then, the significance for Quine of the "breakdown" of the analytic synthetic distinction. This significance lies in the fact that, without the view that one can aim to put any sentence into one of the two classes, one can no longer use this distinction as a strict methodological guide. It no longer sets the goal for one's research.

## 5. Quine's Criticism of Analytic-Synthetic Distinction: The Importance of Quine's Positivist Home

Ouine begins his criticism of the analytic-synthetic distinction by showing that Carnap's theory of state-descriptions does not explain how all meaningful statements can be demarcated into two classes of synthetic empirical and analytic. This we grant immediately. It was known long before 1951 that this view was untenable: it continues to identify synthetic sentences with empirical sentences. But Quine proceeds and says that we cannot explain the analyticity of his problematical sentence as due to the fact that "bachelor" is defined as "unmarried man." Since we are quite naive—among those who are easily soothed—that seems all right to us. Indeed, Quine's argument looks suspicious. For, Quine asks how we would know that the words are defined as the same. We see here no problem. They are so because we stipulate that they are or because we ordinarily presume that they are. Ouine is not satisfied with that. He wishes to know how we can find out if the sentences really have the same meaning and suggests we have no way of doing that if we do not know in some other independent way which words are synonymous with which. Now we readily admit we can make mistakes. Our stipulations may, for example, turn out to be inconsistent or we may find that we use words we thought were synonyms with different meanings or shades of meanings. There are no guarantees. But this does not bother us either. We deem some sentences analytic on the basis of our stipulations and assumptions and, if we discover we are wrong-if differences crop up-and they are synthetic, we change our minds.<sup>23</sup>

But, Quine insists, we do not even know what we are talking about. Our words, he says, need clarification and presumably with reference to linguistic behavior.<sup>24</sup> We are puzzled. Why do they need clarification and why with reference to linguistic behavior? This seems to be an appeal to a reductionist theory of meaning which is rejected in the same essay. Why should standards for a theory of science or of language be higher than those for science itself? Quine hurries on without explaining. While underway we seek to understand. The only plausible interpretation we find

<sup>&</sup>lt;sup>23</sup> For a comparison of Quine with Popper on translation see John Wettersten, "The Place of Bunge," in *Scientific Philosophy Today*, Dordrecht, D. Reidel, 1982, pp. 465ff. See also Karl Popper, "Logic without Assumptions," *Proceedings of the Aristotelian Society*, 1947, pp. 251ff.

<sup>&</sup>lt;sup>24</sup> Quine, From a Logical Point of View, p. 24.

is that our views of synonymy and definition on Quine's view will only be clear enough when we specify necessary and sufficient conditions for them.<sup>25</sup> This seems to us no problem. We borrow from and modify somewhat Quine's proposals which he rejects and suggests that it is necessary and sufficient for one word to be a synonym of another, if we can replace one word for the other in any sentence and the sentence remains unchanged in all other respects.

We do not expect Quine to be satisfied. He will complain that we do not know just what "all other respects" means and that there will be problematical cases where, for example, one sentence may remind us of some other context or have emotional content, etc. which we might deem a relevant change or not. We are not bothered by this. For, we can judge from case to case and context to context if these are relevant or not and whether, for the purposes at hand, we should deem the words synonyms or not. This we say, in answer to Quine, is good enough. We concede, of course, that the boundary between analytic and synthetic statements in ordinary and even scientific languages is not sharp and do not even preclude that a sentence may be deemed analytic in one context and synthetic in another due to shades of meaning. But we find it possible to separate our theory of analytic sentences-our idea of them-from the process of their identification. We may admit mistakes. Perhaps, as Quine suggests, we have admitted words into expressions which make analytic appearing sentences synthetic. We propose to inquire into that piecemeal and provisionally and believe the costs of that are far less than the costs Quine will have to pay for his all-or-nothing strategy: either we have necessary and sufficient conditions for the unambiguous identification of all analytic sentences or we have no clear distinction.

We see here that Quine's criticism of the analytic-synthetic distinction has two aspects which he deems inseparable simply because he remains so positivistic. On the one hand, there is the theory of what an analytic sentence is. On the other hand, there are the methods for correctly identifying analytic sentences. Without a sure-fire method Quine maintains we have no theory. This is to identify the truth of a statement with its proof—or provability.

<sup>18</sup> 

<sup>&</sup>lt;sup>25</sup> ibid., p. 25.

### 6. The value of the Breakdown for Quine's Homesickness

Quine's own conclusion concerning the breakdown of the analytic-synthetic distinction seems to deepen the problems for Carnapian positivism, which had been raised by the breakdown of reductionism. But in Quine's hands the rejection of this doctrine turns out to have another, positive purpose as well.<sup>26</sup> Before the publication of Quine's essay it was known by Popper and others that Carnap's reductionism could never be successful. Once having seen that, it would be quite natural to reject attempts at reduction, as Popper did, and pursue different methods. Quine's difficulties with these approaches appear to have been a consequence of his view that they moved too sharply away from Carnap's program: they abandoned the project of deriving all meaning from observation. Though Quine's rejection of the analytic-synthetic distinction appears as a criticism of Carnap, it is also a means of saving him by stopping the movement away from reductionism from getting out of hand. We do want to be carried away by Wanderlust!

If we reject reductionism and maintain the view that there exists a clear distinction between analytic and synthetic sentences—even while conceding that we cannot always say which are which and that some are ambiguous—we have at least three types of sentences in (scientific) language. We have clearly analytic sentences. (Even Quine admits these in the cases of logical truths and of shorthand expressions at least.)<sup>27</sup> We have empirical statements whose meaning is not reducible to observations but which are clearly synthetic. But we also have statements which seem to express some content—even content found in science—which are not empirical at all. These sentences would be, one would think, metaphysical and meaningful and especially so when they can express some of that content found in science.

When Popper abandoned reductionism along with the theory that all meaningful statements are proven, he took this move immediately. If we take, for example, a sentence like "each force can directly vary into any other force," we may deem it metaphysical and even part of a scientific

<sup>&</sup>lt;sup>26</sup> Quine says that the two doctrines are "at root identical." ibid., p. 41. But this is only true—whatever exactly "at root identical" means—if one continues to presume that all synthetic sentences are so only in so far as they are empirical. Only under this assumption does the failure to achieve demarcation of synthetic sentences by reduction indicate without further ado a failure to demarcate synthetic from analytic ones as well.

<sup>&</sup>lt;sup>27</sup> Quine, From a Logical Point of View, p. 26.

research program.<sup>28</sup> This sentence is no definition and not empirical. It might be consistent or inconsistent with physical theory. Quine cannot condemn it outright as meaningless. It would seem that Quine has no other choice here than to follow Popper's move. Perhaps Agassi offers an even better example: Quine's own doctrine of the indeterminacy of physical theory.<sup>29</sup> It is neither analytic nor empirical but, we may presume, meaningful.

The hidden agenda of two dogmas is, it would appear, to devise a strategy for limiting and/or reinterpreting such sentences even while allowing them, out of necessity, in science (and in Quine's own philosophy). Quine's procedure, it would appear, is to explain the character of (metaphysical) sentences with his positivist principles, that is, with reference to analytic or synthetic (empirical) sentences. They may then be deemed analytic-like insofar as they concern the meanings of words or empirical-like insofar as they have content. Or, highly metaphysical sentences have less and less meaning, so we should and can limit ourselves to meaningful metaphysics.<sup>30</sup>

Quine's rejection of the analytic-synthetic distinction, then, helps him to avoid, as Carnap's program requires, the growth of metaphysics. He can avoid this growth by deeming those sentences which are not reducible to observations but which are also not analytical as occupying a gray zone between the two. This gray zone can be treated as a matter of language or of the relations between concepts, on the one hand, and as nearer to or farther from empirical statements, on the other hand. Unavoidable sentences traditionally deemed metaphysical may be placed there. The distinction between an analytic statement which says nothing about the world and a metaphysical statement, which says something about the world but is not testable, is thereby fudged: The more metaphysical a statement is the more it resembles an analytic statement, i.e., the less it says about the world. The rejection of the distinction between analytic and synthetic sentences is necessary to create this new and obscure status for

<sup>&</sup>lt;sup>28</sup> Joseph Agassi, *Faraday as a Natural Philosopher*, Chicago: The University of Chicago Press, 1971, p. 210ff.

<sup>&</sup>lt;sup>29</sup> Agassi, "Ixmann and the Gavagai," p. 113.

<sup>&</sup>lt;sup>30</sup> Quine, "A Comment on Agassi's Remarks," p. 117. "Repeatedly you have me banning metaphysics. Maybe you have in mind my rejection in "Epistemology Naturalized" of a "first philosophy." I see metaphysics, good and bad, as a continuation of science, good and bad, and grading off into meaningless." I am still puzzled. Is metaphysics a mixture of analytic and empirical of some sort? If not, how does it "grade off" into meaningless? If he does not wish to ban metaphysics—this is, unfortunately, impossible—does he not want to limit it wherever he can?

statements which one must accept as meaningful but which are neither reducible to observation nor clearly identifiable as analytic in some traditional sense. Since all analytic statements are now deemed conventional, apparently metaphysical statements or metaphysical aspects of scientific statements may be allegedly treated either in the same way as analytical statements, that is, as conventions concerning the use of words<sup>31</sup>, or as statements with some empirical content or, more likely, as a mixture of the two.

Ouine's new reconstruction of language is only possible if new structural supports are found. These should play that role in the new reconstruction, which analytic sentences played in earlier efforts. The presumed breakdown of the analytic-synthetic distinction is, however, helpful here. Without the breakdown nothing like analytic sentences could serve this purpose. The analytic sentences one could identify and/or count on would not be numerous enough or strong enough. Metaphysics would be needed. With the breakdown things look different. One may fudge a bit and this seems to positivists a good thing: sentences may be deemed to pass gradually from empirical ones to analytic ones and those in the middle of this transition may serve as the new supports. The new structure may even be deemed a dome-like construction with no pillars at all.<sup>32</sup> The pressure of the parts of the structure against each other is what holds it up. Such pressure depends, of course, on the maintenance of some definite arrangement of the parts and this function is filled by analytic-like sentences. In order to preserve this pressure, then, the system must only be changed slightly.

<sup>&</sup>lt;sup>31</sup> Quine, *From a Logical Point of View*, pp. 44. Quine here defends a strict instrumentalism or conventionalism. He denies belief in objects, and science is a mere tool for prediction of future events on the basis of our knowledge of past events.

<sup>&</sup>lt;sup>32</sup> Quine uses this metaphor in *Word and Object*, Cambridge: Massachusetts Institute of Technology Press, 1960, p. 11. Paul Gochet, Quine zur Diskussion, Frankfurt: Ullstein Materialien, 1984, p. 53, sees here an advantage for Quine. His view of the empirical foundation of science—it is supported by all of the stones of the foundation simultaneously—is more radical than Popper's. Popper clings to the metaphor of pillars which are driven deeper into a swamp. It is, indeed, true that Quine sometimes seems to adopt more radical and skeptical views than Popper does. But Popper develops his view in accord with his principles. When Quine's empiricist principles lead him into trouble he retreats to naive views which he thinks he can defend by appeal to scientific common sense—as we shall see below—or even metaphor. Popper avoids such difficulties. But we, also, do not wish to make too much of metaphors: they help us to explain points of view but are, themselves, no arguments.

The new analytic-like but not quite analytic sentences have content as glue or as organization but not directly as descriptions of the world. They have to be maintained by maintaining convention.

For those of us, however, who have not grown up among Carnapian natives, it seems to be one of Quine's unexamined and somewhat wooly metaphysical dogmas that being naturalistic or pro-science or against wooly metaphysics is the same as being (neo-)Carnapian, that is, is the same as accepting either the doctrine that all evidence and therefore all meaning issues from the senses or some other weakened empiricist doctrine of meaning.<sup>33</sup> But even though we might find Quine's stance somewhat dogmatic and prefer an experimental to a dogmatic attitude toward neo-Carnapian views,<sup>34</sup> we can still examine this program to see what insights, if any, it has to offer, to see whether this journey might be more interesting than those others which seem to offer some excitement and adventure and to see whether it is inconsistent and/or subject to effective external criticism, say, from psychology. By looking at Quine's development we may be able to better understand his motivation and to

<sup>&</sup>lt;sup>33</sup> For a discussion of the relationship of views which are pro-science or antiscience with differing attitudes toward rationality see, John Wettersten, "Russell and Rationality Today," in *Methodology and Science*, Vol 18, No. 2, 1985, pp. 140-163.

<sup>&</sup>lt;sup>34</sup> Ouine emphasizes that he himself favors an experimental attitude. See "On What there Is," in From a Logical Point of View, p. 19. Yet on the same page he asserts that the phenomenalistic conceptual scheme has epistemological priority. I would think that would be a matter for science and metaphysics to decide if tolerance and pluralism were to be taken seriously. In "On Mental Entities," in The Ways of Paradox and Other Essays, New York: Random House, 1966, pp. 208ff., Quine discusses the postulation of mental entities but as the positivists did, i.e., primarily as sense data. He argues convincingly that perception depends not merely on input or stimulation but on, what some of those who postulate the existence of mental entities call, mental activity. He proposes a materialist account as a hypothesis. This is fine. But why should we not have two competing scientific hypotheses? The problems which Quine discusses seem, indeed, to call not for the rejection of mental entities but for new ideas about them as Oswald Külpe proposed. This has nothing to do with the alleged crucial insight of empiricism that any evidence for science has its end points in the senses (p. 212). So far as I can see this "crucial insight" is trivial unless it is taken, say, to be a doctrine of meaning according to which all meaning is derived from the senses as Ouine sometimes seems to think. Otherwise, it has hardly anything to do with methodology or metaphysics. It is consistent with Popper's methodology and three world view. Yet Quine gives the impression he wishes to slide from this thesis to empiricism. If he does so, it would seem to be a slight of language.