DISKUSSION

Ixmann and the Gavagai*

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> When he [Einstein] says that fear
> of metaphysics is the contemporary
> malady I am inclined to agree.
> (Russell, "Replies", p. 696)

> On deeper reflection, radical
> translation begins at home,
> (Quine, "Ontological Relativity", p. 46)

I. BACKGROUND

Dirk Koppelberg is an ambitious new arrival to take notice of. His first book, "Die Aufhebung der analytischen Philosophie: Quine als Synthese von Carnap und Neurath" (Suhrkamp, 1987, pp. 416) is extremely detailed and comprehensive. In succinct 300 pages or so (plus 40 pages of notes and 30 pages of (not too successful) bibliography) he manages to touch on W. V. Quine's diverse concerns, to synthesize them, to relate them to their background and to correct practically every commentator on his hero. Quine himself is too much of a gentleman to openly complain about maltreatment; I hope here he may be pleasantly surprised. For my part, I find many if not most of the comments on Quine here cited too tedious and will try to ignore them altogether. I have enough to quarrel with Koppelberg about. Indeed, I do not know what we share other than a great interest in Quine and whatever else is nowadays required of a decent philosophical scholar (which is precious little, alas!). At least his commentary is competent and interesting and it has helped me understand Quine better, I hope.

A few things are going on here, however, and I do not quit know which of them is dominant and/or intended. Koppelberg might be describing the demise of analytic philosophy and he might be describing its transformation into Quine's work and tradition: it depends on the choice of one of the two meanings of the word "Aufhebung", whose ambiguity is so deeply entrenched by the Hegelian tradition that many an author and translator these days uses it in their English texts as utterly untranslatable. (Untranslatables tend to be

incorporated). He might, however, comment on Sir Karl Popper's boast that he is the one who has killed analytic philosophy (p. 89), to say either that Quine deserves the honor or that none does, depending on the same choice of meaning of "Aufhebung". It is hard to say, since valuation here matters much: one who values the philosophy of Ludwig Wittgenstein will easily distinguish phases in it; Paul Feyerabend sees it as one (p. 72); and one who values the Vienna Circle will gladly debate the pros and cons of their claim to have followed Wittgenstein and of Wittgenstein's repudiation of this claim of theirs; Mario Bunge delightfully mixes up everything in this connection (p. 323) – much to Koppelberg's annoyance, which betrays doubts as to his own sense of the significance of the Vienna Circle, of course. Yet it was from him that I have learned to see Quine's debt to Rudolf Carnap, the leading member of that Circle. I do not mean, however, to conceal the fact that I am a fan of Quine despite of, not because of, his appreciation of Carnap; the same, incidentally, goes (in a different way) for my attitude towards Popper.

Perhaps Koppelberg's main concern is to present Quine's views straight and he takes R. G. Collingwood's advice and presents it as alternative solutions to extant problems. After all, when we find some assertions strange, whether they stem from philosophy or from exotic lands, we do make them sound more cogent by presenting some relevant background information; and in either case this may constitute some relevant part of some conceptual framework; but, in addition, the philosophical assertions in the rationalistic tradition of ours are often enough problem-oriented (I am yet to find a problem-oriented presentation of the assertions of, say, Martin Heidegger or Herbert Marcuse). Perhaps, even, Koppelberg is deeply influenced by the profound observation of Samuel Butler that given enough distance, all competitors look alike. And then the background information is most significant for those who wish to comprehend the fine differences forgotten by history. To my chagrin I cannot say whether Koppelberg tries to make cogent what sounds strange or to amplify some fine differences. Here I miss some of his own background and perspective as he discusses his own views much too seldom.

I really do need the perspective. Quine shares with Popper realism and a fallibilist view of science (see his contribution to P. A. Schilpp's "The Philosophy of Karl Popper" and his comment on p. 621 of "The Philosophy of W. V. Quine") and he calls himself a non-justificationist. Also he thinks Carnap's empiricism a dogma that imposes irrealism as well as the poorly thought out empiricist criterion of meaning (pp. 156, 172 and 270). Yet there is no doubt he thinks Carnap a central figure and Popper marginal – I could not see why until I read Koppelberg. But whereas he leaves it at that with no valuation, I can explain my dissent and regret at the variance of perspectives here and attempt to see how it effects the perceptions of different parts of the contemporary philosophical scene.

To conclude this introductory section, I have intended to employ in it only observation statements, very much in agreement with Quine's (as well as Popper's) view of these statements (pp. 210 to 215), which well accords with

ordinary usage (unlike almost anything else in the philosophical and/or the psychological literature, traditional or contemporary, which is obsessed with tables and toothaches and such). That some of these observations may not accord with some of Quine's central contentions seem to me to be not threat to his enormous significance on the contemporary scene – again in full accord with his fallibilism (and Popper's).

II. ELIMINATING NEURATH FROM THE STORY

The book at hand is divided into two parts, one background, one on Quine. The main menu in the first part offers some preparatory material, including material on the debate between Carnap and Neurath on protocol statement, to which we shall return soon, and briefs on Gottlob Frege, Bertrand Russell, Wittgenstein, Carnap and Popper. This list is surprising: Neurath is missing from it and Popper is dragged in. A lot of debates are reported which Quine had with Carnap but not with Neurath – yet they share the subtitle of the book. Quine is never cited referring to Neurath – only to his boat which is regularly repaired on the high seas for want of a dry dock. Now this metaphor reflects the view of any fallibilist – C. S. Peirce and Pierre Duhem, as well as Neurath, Popper and Quine.

Quine defines his own views as naturalistic epistemology, to mean that science is autonomous, has received opinions and is always ready to detect error in them and then try to correct them (p. 314, lines 2–6). I have modified the definition somewhat, making explicit the central unstated but often used thesis of the autonomy of science. One may gasp at the cavalier manner in which Quine takes his democratic politics. He does stress that language is a social institution and that there is no clear borderline between science and language. Nevertheless, I miss the explicit discussion of politics here. (Koppelberg ignores politics even when he refers to Kuhn and to Feyerabend. He even tends to lump them together despite the former's defence of some undemocratic procedures now rather common in the scientific community and the latter's complaint against the current tyranny of science in society at large.) Even Neurath's politics is absent here, though he was very active, and never saw any boundary between politics, economics and philosophy. Perhaps Carnap's politics may be ignored as he thought his concern was meaning and meaning politically neutral; for both Neurath and Quine this is impossible, as they say, meaning come in lumps as parts of whole intellectual systems. Does holism link science and politics significantly? How? (Jagdish Hattiangadi sees Quine as a follower of Duhem and Ernest Gellner sees him as similarly politically conservative. See "Rationality: the Critical View", edited by myself and I. C. Jarvie.)

Quine observes that the only reason to suppose that meanings of non-logical words can be introduced singly is the defunct phenomenalism – the defunct theory of founding science on sensations. This is why Carnap divided cognitive language into three: the analytic, the observational and the theoretical. And Quine gave up all foundation of science and argued that there is no

clear-cut boundary between any two of these three (p. 183). Quine does not deny, of course, that the distinctions are practically sharp enough: he presents no practical problem and intends none. On the contrary, he points at an insurmountable theoretical difficulty which he thinks should make those who feel it relinquish their theories. In ordinary language, including much of the language of science, there is no sharp dichotomy between logical and non-logical words [as negation and order-relations, for example, are often absorbed into the descriptive vocabulary in sentences like, independence is best]; those whose views require sharp dichotomies have difficulties; not so Quine: his concern is not the boundary which is but the resultant interdependence of meanings: it is the interdependence itself! This interdependence is extremely significant for any fallibilist theory of science, such as that of Pierre Duhem's; we shall return to it presently.

Quine mentions that his fallibilism is influenced by Peirce (p. 313) and Koppelberg dismisses this claim on the spot with no explanation at all. His own linking of Quine with Neurath rests on Quine's reference to Neurath's boat and on his own confusion of Neurath's linguistic physicalism (all science can be stated in the language of physics) with Quine's metaphysical one (all change accompanies some physical change) (pp. 279–81).

There are two passages here in which Neurath is cited to side with Duhem, both rather curt and question-begging. The one relates his own theory of protocol-statements to Duhem's theory, which will be discussed next, and in all the relevant details. The other, from a letter to Carnap, says, Carnap should not be impressed with Popper who is a step backward from Duhem. (Koppelberg adds here that almost all contemporary critics of Popper harp on the same theme, and I think he is right but as I ignore inept criticism as much as I can I am no authority on this matter.)

Otto Neurath's only specific contributions are, (a) his form of protocol-statements: "Otto protocol on day x time y + 1 minute, [in place z, at time y Otto speak-thinks (in place w, at time y – 1 minute an Otto-experienced table)]." (p. 27), and (b) his decleration that his holism permits their revisability-without-withdrawal. This is so grotesque, it invites some comment. None is offered. How is such a grotesque verbiage revisable? How is Duhem related to it? No answer. Things get worse when Koppelberg endorses Neurath's passing claim that all this is pluralist, thereby clinching the criticism levelled against him by Russell and by Popper (p. 36), who perceive this as a total retreat from empiricism.

Koppelberg has a view on this matter, it seems (p. 36, line 2 from bottom): the tentativity of observation reports can, indeed, be misused to overrule unpleasant empirical data, yet this privilege should be restricted to historical reconstructions, thus blocking misuse. Koppelberg ascribes this restriction to Otto Neurath, and to a few others. It belongs exclusively to Imre Lakatos, and it is useless: at time y history includes time y – 1 minute. Moreover, offering a criterion to prevent misuse Koppelberg concedes Popper's demand for a criterion – so as to prevent dogmatism. (Pity: let anyone who wants to dogmatize do as they please; the rest of us can be sensible and seek knowledge

even without criteria.) Furthermore, if we charitably understand Koppelberg to limit the history of any given science to the time preceding the last paradigm shift in it, then all paradigm shifts will become incomprehensible (as all critics of Kuhn and of Lakatos have noted). In brief things get increasingly complicated; it is time for a drastic operation.

Make the following experiment. Leave as they are the two passages of Neurath refering to Duhem and the two references to his celebrated boat (pp. 309 and 314). (Ignore also fleeting clusters of names.) In all other passages in which he is named do the following. Try first to replace Neurath's name with that of Duhem and make the necessary small adjustments required by this transformation. If this is not straightforward and simple, then omit the passage, making the omission as small as possible provided the adjustments which this omission requires are straightforward and simple. If such an exercise leaves a book relatively unharmed – and in the present case it makes a great improvement – then it is recommended. The only possible loss here is of the brief report on the Neurath-Carnap debate – which is hardly regrettable, as it is a pale anticipation of the Quine-Carnap debate, here sufficiently well-presented to stand on its own. Why should Koppelberg try so hard to pass to Neurath the credit which his evidence amply shows to be obviously due to Duhem? On the face of it this seems like academic politics, and I will not ascribe such a mode of conduct to anyone without first investigating matters to beyond the confines of a given book.

III. DUHEM'S THEORY OF KNOWLEDGE WITH FALLIBILITY

The finality of observation reports, we remember, would permit us to have a formal language into which we could add non-logical terms one by one – by ostensive definitions, so-called – contrary to Quine's claim that there is no sharp boundary between the logical and the non-logical words (p. 183). Suppose we have a formal language and wish to introduce a batch of non-logical words in one go. How? The method of implicit definition offers us a way: the non-logical words in the axiom system are any terms which make them true. This way David Hilbert introduced the Eulidean space. Notice that considering Euclidean geometry false will exclude the intended, Euclidean reading of the axioms! We may, therefore, constrain the meanings of the typically Euclidean words in the system so as to exclude all observation reports as irrelevant or to include them only after they untergo some radical alteration in wording. To take Henri Poincaré's classical example, we will exclude the claim that certain kinds of light rays are straight lines upon discovering that their paths are somewhat curved. Euclideanism is thus rescued from refutation. Yet the truth we then ascribe to Euclidean geometry thus becomes independent of empirical informations; it is truth by convention, then. Implicit definitions, however, are as artificial and as arbitrary as ostensive definitions. Perhaps this is the reason why Quine prefers not to use the term "implicit definitions" (p. 329). His view of them as merely pointless is no doubt permissible though contrary to the intentions of the conventionalists,

Duhem, Poincaré and the young Hilbert, since they decreed only theories worth rescuing [implicit] implicit definitions.

The attraction of conventionalism is exactly in its being a theory of science as certain-yet-fallible! The desire to have science as certain is rooted in the ancient theory of rationality as certitude. (Not in vain does Koppelberg include a new theory of rationality as distinct from certainty: it kills the attraction of conventionalism and thus opens the road for an alternative, such as Quine's. The theory of rationality Koppelberg proposes he ascribes to writers regrettably unknown to me, adding in a note that they agree with I. C. Jarvie and myself (see our above-mentioned book) – thus managing to keep Popper's name out of the discussion yet again.) The desire to have science fallible is rooted in the fact of human fallibility. Peirce and Collingwood are the two great thinkers who stand out as having wished and failed to combine fallibilism with certitude. But other not less impressive and valiant efforts can be mentioned. Duhem has succeeded, and this puts him in the pantheon of leading modern philosophers of science.

Duhem left ordinary discourse out of his discussion – as contaminated with metaphysics and with commonsense. (He was a great admirer of both, but as strictly divorced from science, which is the chief difference between him and Quine.) Since scientific observation reports are worded within scientific language, they have to undergo change with every theory change. (Here is Quine's central similarity with Duhem and the kernel of the Duhem-Quine thesis of the irrefutability of any hypothesis in isolation as well as of the interdependent Duhem-Quine radical untranslatability thesis.) Yet in another sense each observation report is irrefutable or unalterable, since empirical content is preserved in its translation from one theoretical framework to another. Even so, observation reports are all regularly subject to modification, since translation is never quite adequate. The same exactly holds for theory: uninterpreted, theory is mathematical (i. e., formal) and so final; interpreted, theory has only partial empirical meaning – as much as evidence gives it. (Here, Carnap's debt to Duhem becomes evident: all his life Carnap vacillated between logicism and formalism and he attempted to have scientific theories acquire empirical meaning only to the extent to which known observations can endow them.) Thus, the more a given theory is tested, the clearer its domain of applicability becomes and thereby the clearer its overall meaning becomes. When a new application is attempted, the attempt may fail, but the failure is that of (the one who performs) the attempt, not of the theory.

The two-tier attitude, of certainty and fallibility, goes here with a two-tier theory of truth – the absolute-formal and the instrumental or pragmatic-empirical or relative-empirical. Needless to say, this is both the most popular aspect of Duhem's view and the most objectionable. It is of course extremely interesting to see what Quine makes of it despite his straightforward and comprehensive (too comprehensive, says W. W. Bartley, III) fallibilism and his marvellous rejection of all vestiges of relativism. In part his task is easier, since he defiantly stresses the claim that any theory, and with it any meaning, goes well beyond what evidence could ever guarantee (even were evidence

utterly reliable). For the interdependence of meanings thereby becomes stronger and the radical untranslatability more evident. In part, however, the task becomes harder: rejecting Duhem's instrumentalism Quine faces the question, what makes science what it is? I will try to show that on this question Quine's theory wants some modification and invites certain simplifications and Koppelberg is not too reliable on this as he glosses over some troublesome passages. Needless to say, I will present Quine as rather hostile to metaphysics – unintentionally perhaps, as he would view metaphysics and science as lacking a sharp boundary between them, of course – not only in passages which I will quote later, but also in his aloofness about it, in his dislike for ontological commitments other than those of mathematics and science and, above all, in his operational criteria, in his use of rules of conduct to characterize the different attitudes and the different categories of statements.

IV. HOW CARNAP SUPERSEDED WITTGENSTEIN

Ludwig Wittgensteins magnum opus, his "Tractatus Logico-Philosophicus" of 1921, declares the class of properly worded sentences coextensive with the class of all scientific sentences: knowledge stateable in words equals scientific knowledge.

The first objection to this is that tautologies and contradictions are well-worded and yet not empirical. They are, he retorted, the by-products (cp. p. 153) of the rules of grammar and they have meanings only by courtesy of sorts. As borderline cases they scarcely matter, anyhow. This kind of answer is prima facie legitimate; except that here it is objectionable at it makes light of mathematics: it deems mathematics as a part of logic and logic as marginal. This is one of Russell's three criticisms of Wittgenstein (in his Introduction to the book), all deadly.

Russell's second objection is that some information is not within language – and Wittgenstein himself offers some striking examples (the one I appreciate is, there are natural laws) in his book (his book itself included). Russell claimed that Wittgenstein's examples (especially his own message) are conveyed well enough well within language. Moreover, they are all meta-linguistic and why should we not postulate the existence of a hierarchy of languages. This way, clearly, Wittgenstein's exclusion of well-worded metaphysical sentences is rendered a merely local affair. Carnap accepted the meta-language, and even enthusiastically: it is the home of philosophy, since, obviously, philosophical analysis is meta-linguistic. Yet he endorsed Wittgenstein's thesis, and even improved on it: metaphysics is (not any old verbal incoherence but) the confusion of the object-language with the meta-language.

This will not do: the metaphysical doctrines stated in Aristotle's "Metaphysica Alpha", the paradigma of metaphysics, are clearly about objects yet clearly unscientific or proto-scientific. Responding to this kind of criticism, Carnap protested that he was not opposed to any theory which stimulates the growth of knowledge. He attempted to provide for such metaphysics, yet he

could not leave it in the object-language, where (as proto-science or first philosophy) it naturally belongs. He could only permit to incorporate metaphysics – the conceptual schema, Quine calls it – into the range of variables within language. Quine endorsed this attitude (p. 223). This, incidentally, well accords with Carnap's principle of tolerance which allows everyone their metaphysics but does not cater for the need to have critical debates between the proponents of different conceptual schemas. Somehow this does not sit well: if metaphysical disagreements do not matter, then there is little value in their tolerance; if they matter, how can they be settled? Meeting this dilemma Arthur Pap endorsed the narrowing of the range of variables in the language (such as deciding that the range of colors is the color cone) is a matter of a synthetic a priori judgment! This narrowing can be conjectural, and obviously, it can be made in any of the two languages as one pleases: the case happens to be exactly the opposite of Carnap's claim that every statement must declare domicile in one language and always display its proper address or be charged with vagrancy as a crime against logic. Tarski's schema for the definition of truth put an end to all this.

Russell's third objection to Wittgenstein's concerns the narrowness of his language even as an object-language. On this point they parted company, since Russell took offence when Wittgenstein feigned incomprehension in response to Russell's statement that there are at least three objects in the universe. (See Russell's obituary of Wittgenstein in "Mind" and his autobiography.) Ever since Carnap (obliquely) expressed agreement with Russell (see his "Die Aufgabe der Wissenschaftslogik", Einheitswissenschaft Heft 3, Wien 1934, S. 25), this point was dropped from the analytic literature. Quine certainly finds no objection here, as he has a theory of objects: to be is to be a value of a variable (p. 183; cp. p. 336), is his famous witticism literally meant. And he speaks of a speaker's ontological commitment with ease, thereby allowing metaphysics to the extent that the conceptual schema of the speaker's language allows for it and science has not yet stepped in. (Yet he leaves unscientific rational debate between different speakers scarcely possible: any argument which employs factual evidence will make their disagreement scientific (pp. 177 and 309) and anything else will be settleable, if at all, by mere convention!)

In conclusion of this point, Carnap's – and more so Quine's – system may easily be construed as the modification of Wittgenstein's language-inbuilt hostility to metaphysics – a modification made to take care of Russell's three deadly objections. The legitimate aspect of metaphysics is permitted and absorbed into the frame of the language, insuring that all significant ontological commitments are left so science; the construction of mathematics and of the language of science, as well as the analysis of the situation, are permitted to be properly executed in the meta-language; and the objectionable part of metaphysics is unmasked as a confusion between the object and the meta level. Yet something should be added: Wittgenstein's assurance that at bottom ordinary discourse is but a distorted formal discourse had to be rejected as a naturalism which is inconsistent with the new developments in

formal logic and in empirical linguistics. Carnap's alternative methods of formalizing ordinary discourses are inadequate (p. 152). Instead, Quine offers a system which purports to take care of ordinary discourse and which invites scientific amplification (loc. cit.). This would make Wittgenstein a leading philosopher, and it looks impossible to view him as small fry yet admire Quine's contribution. And the conclusion from this is, "Der Philosoph behandelt eine Frage wie eine Krankheit" (p. 324): asking questions is sick! Yet if there is one significant distinction to Quine, it is that he is as problem-oriented as any philosopher. Total deadlock!

V. HOW POPPER SUPERSEDED WITTGENSTEIN

The above presentation omits Wittgenstein's most significant and influential insight unmentioned. I have, indeed, found it nowhere except in my own writings. It is the move from science actual to science potential – as a linguistic move, indeed, yet as one easily separable from the philosophy of language. It is as follows.

Until the twentieth century, science was deemed the body of proven theory. Any theory contradictory to a scientific theory is therefore pseudo-scientific. Any theory independent of scientific theory (in the logical sense of logical irrelevance) is to be rejected as false, we are told in Descartes' rules of philosophising. This rule is clearly the counsel to be inconsistent. It led to the traditional confusion of falsehood, contradiction and meaninglesness. (This is why Russell's paradox is historically so important: it led to the first clear formulation of the distinction between the meaningless and the inconsistent.) Following Russell, Wittgenstein had to accept the negation of any statement as equally meaningful and therefore, by his identification of meaning with science, as both equally scientific. By default, therefore, he replaced science actual with science potential – as his Vienna Circle followers were at pain to explain since the transition from science actual to science potential was very difficult, especially since it was implicit.

It is my present view – contrary to much that I have said before and in response to many questions I have published – that Popper's magnum opus, his "Die Logik der Forschung" of 1935, has two criteria of demarcation, not one: science potential is the set (not of all properly worded sentences but) of all refutable theories, past present and future; science actual – to be (pro-tem) endorsed and applied – is the proper part of science potential; of all the presently available competing theories it includes the most refutable one as long as it has undergone severe tests yet thus far resisted all attempts to refute it.

I do not endorse this view; I suggest that it is the best interpretation of Popper's great masterpiece. His lack of clarity on the matter – but for which I would not have found it so hard to come up with an interpretation of his book that seems to me satisfactory enough – is the lack of clarity of the transition from science potential to science actual in Wittgenstein and his Viennese followers as well as in Popper's own failure to convince them that he was

speaking not of the language of science but of science within language. This point is somewhat very hard to follow. How else would Carnap have ascribed to him a bastard, inconsistent linguistic version of his view all his protests notwithstanding? Interestingly, when Quine has language absorb the conceptual schema of science, it looks as if he has endorsed Popper's view in its bastard, inconsistent version, since his view of metaphysics as outside object-language seems to force him to consider science as coextensive with language à la Wittgenstein. Yet this is an error: Quine rejects any empirical criterion of meaning – just like Popper – and so his demarcation of science is within language; he nevertheless refuses to allow metaphysics into language, since it may impinge on science! We shall soon return to this point.

Koppelberg notices (p. 92) with his usual care Popper's complete rejection of any linkage of any meaning theory with his demarcation of science. He declares this to be incompatible with the [Duhem-]Quine radical untranslatability thesis (p. 96; cp. p. 97). On the contrary, the radical untranslatability thesis legitimates some approximate translation of observations from the older theory (-language) to the new one, and as the gateway to the construction of a crucial experiment. Example: the paradigm of a crucial experiment is Eddington's 1919 solar eclipse observation, which rests on Einstein's celebrated paper, in which he, Einstein, presents Newton's theory of gravity and his own, deducing from both conflicting predictions for the crucial experiment. Yet he words Newton's theory there in a manner utterly different from anything the vast literature on it had previously presented. Once Quine explicitly allows the holders of different conceptual schemas free expression and rational deliberation – and how can he forbid that? – and his theory will be remarkably easier to grasp and will easily merge with some variant of critical rationalism or another. Rather than ban metaphysics he can ban it only when it impinges on science. (This is even in line with Quine's celebrated modification of Russell's resolution of his own paradox, a modification restricting Russell's preclusion of unstratified classes to the troublesome domain of abstraction.) Moreover, his desire to see his own [metaphysical!] views scientifically amplified (p. 436) will become the general attitude to metaphysics as possible proto-science.

VI. FROM A LOGICAL POINT OF VIEW. OR, THE POVERTY OF QUINE'S CRITICS

It is an amazing fact that the debate on the foundations of mathematics has fizzled out. Yet this is a fact. Quine is one of the leading heirs of the logicist school of Frege and Russell, yet he is also the acknowledged critic of logicism, who takes the inclusion sign of Boolean algebra as part and parcel of logic yet the inclusion sign of abstract set theory as introducible by one or another set of axioms, so that it is anything but a purely logical sign. Yet what observation of what facts can decide between the differing variants of abstract set theory?

It is this question which carries an enormous weight in Quine's system, and which he attempts to solve by excluding ontological commitments when the exclusions are not too inconvenient. This way he seemingly avoids all

metaphysics – unless it is inconvenient – and leaving some questions of truth-values hang in mid-air. This rule of conduct bespeaks of a supposition which, following John Watkins, I reject out of hand, and which Quine, too, should wish to reject. It is the identification of the synthetic with the empirical. There is little doubt that there is gradation between the analytic and the synthetic; there is little doubt that there is a gradation between the analytic and the empirical; and all empirical theories are synthetic, of course. Yet not vice versa; there is no reason to identify the two sets. As Popper has stressed ever since 1935, logical positivism or logical empiricism (Koppelberg rightly changes Quine's title of two dogmas of empiricism to that of two dogmas of logical empiricism, p. 103; but he could have been more explicit about it) suffers from the inheritance of Wittgenstein's mistake when forcing the synthetic to become empirical in the face of fantastic, in-principle-not-empirical reasonably-worded statements. Even when philosophers contest Quine's claim that there may be two competing theories such that all facts, known and unknown, past and future, may not suffice to decide between them, even then they are making synthetic judgments not given to empirical decision in principle. Even those who refuse to discuss such a remote possibility (p. 276) thereby admit that not all synthetic statements are in principle empirical. Moreover, here we have an ontological commitment of Quine; there are indeterminate languages to the maximal degree, namely such that suffer the articulation of maximally indeterminate theories.

This is not to agree or to disagree with Quine's extreme form of indeterminacy. It is evidently a wild speculation and he is welcome to it. My point is merely that it is a synthetic non-empirical statement. Can it be viewed as metaphysical? The empiricist tradition from the scientific revolution onwards often deemed all unempirical claims metaphysical – especially in a hostile mood towards metaphysics, as exhibited by, say, Lichtenberg, Wittgenstein or the young Popper. Now, for the distinction between the synthetic and the empirical, this matters not. But it matters terribly when we come to speculations which may impinge or science, such as either atomism or plenism, as the scientific case may be. Quine is at pain to deny the existence of such systems – conceptual schemas, as he calls them. "A triad – conceptual scheme, language, and world – is not what I envisage. I think rather . . . in terms of language and the world . . ., Where I have spoken of a conceptual schema I could have spoken of language" (p. 223).

This is the oblique and harmful influence of Wittgenstein on Quine. It is out of tune with Quine's general tenor as presented here, and yet is colors repeatedly much of what he says – such as his willingness to absorb metaphysics, the intellectual framework, into language, into the logical framework. It may very well be the chief cause of the difficulty to comprehend him, experienced on both sides of the barricade – both by Wittgenstein fans and by Popper fans. Since most of Quine's critics are Wittgensteinians of various colors, the poor impression the Quine literature gives is thus explained as the struggle to rescue him from the total apostasy to which he is committed from a logical point of view. To return to the above quote from Quine, the

thinking in terms of language and the world which it recommends is often quite inadequate; on the one hand the distinction is superfluous (p. 152), of course, as language is a part of the observed world (it is amazing to find philosophers who deny this observation and try to reduce observed verbal behavior to something else); on the other hand, clearly, language will not do, especially for science, which handles explicitly stated theories (pp. 177 and 183), of course, as theories cannot be construed as observations cast in theoretical language (it is amazing to find philosophers who are attracted to this view and are thus caught in what C. G. Hempel once called a theoretician's dilemma); in what intermediate context, then, does Quine find it congenial to speak of language and the world? The context, I propose, is his defence of science as against metaphysics (p. 246), a defence which is not at all in the best inerest of science (see my "The Nature of Scientific-Problems and Their Roots in Metaphysics" in my "Science in Flux"), since at times science has some use for metaphysics and at other times it can ignore it. I propose to modify Quine's views in a manner hopefully congenial to him, and by two specific moves: one, by omitting as uncharacteristic anti-metaphysical statements of his (Koppelberg does this tacitly instead of explicitly); and two, by admitting as complementary all sorts of holistic divisions, even though never quite clear-cut, yet as clear-cut as required depending on specific contexts.

It is strange to hear people disagree with Quine's radical indeterminacy thesis on the ground that it does not signify in all contexts as if this would render it obviously false or even as if it were scarcely comprehensible and not given to even possible instantiation in any context. After all, we can construe one theory once with one abstract set-theoretical reading of its underlying mathematical structure and once with another, and we have thereby constructed such a case (the variations may be classical, Robisonian or in line with suggestions of Paul Cohen). Moreover, the question, are there laws of nature or are the observed regularities merely local? Is a respectable metaphysical question which is for ever indeterminate!

It is always a respectable scientific move to suspect that a certain regularity is merely due to local conditions and try to vary the conditions so as to break it. In a very subtle move Nelson Goodman suggested ("Fact, Fiction and Forecast") that a regularity may hold locally everywhere and then look like a law of nature even if it be but accidental. (See also Popper's "Logic of Scientific Discovery", Appendix *10 "on Natural or Physical Necessity" and my "What is a Natural Law?" in my "Science in Flux".) Here we have a strong metaphysical indeterminacy – where does natural law end and mere regularity begin? – which is very hard to evade and which Quine is reluctant to go into for want of a criterion of a rule of conduct to distinguish the two, yet he is committed to the distinction and the indeterminacy involved. This is the same case as Quine's proposal to identify indiscernibles in the universe of discourse in which they are indiscernible: indiscernibility may be the quality of the universe of discourse and it may be the result of the decree of Nature – and the rule he follows, to use rules and conveniences to determine such matters, does gloss over this difference. For, obviously, metaphysics is largely indetermi-

nate, yet in observed fact criticism of metaphysical doctrines does happen – and is exhibited often enough in Quine's writings and in the better part of the Quine literature. I am cognizant of the fact, however, that most of Quine's critics take him for an enemy of metaphysics who from time to time does not quite consistently keeps his system free of all metaphysics as they think he should. Will the real Quine stand up, please?

VII. TWO DOGMAS OF EMPIRICISM TRANSCENDED

The reason Quine's "Two Dogmas of Empiricism" has aroused such a lengthy and detailed controversy is that its critical knife sinks in fully and with its Quinean hilt: it seems hard to admit the criticism and not to concede Quine's main theses. Here is Koppelberg's strength: he noticed that Quine had it all in his earliest publication yet waited with his thrust until its critical part could carry the positive part with it (Section 3.1 and p. 183). This is how he has helped me see what Quine was after, since I had always conceded his criticism with ease and yet was not moved by his positive theses, especially since he sounds unnecessarily committed to an instrumentalist methodology of sorts and to a behaviorist psychology of sorts – even to one which carries some Deweyite social Darwinist overtones. I was particularly puzzled by his insistent Platonism in matters mathematical coupled with his insistent parsimony in more commonsense ontology. Koppelberg presents Quine's view as a realistic acceptance of science (with the fallibilist proviso) including some scientific psychology, including scientific linguistics and language-acquisition theory: he hopes that his work is helpful in their developments (p. 346). Here Quine presents some of his own ideas as proto-scientific or metaphysical! Koppelberg also plays down Quine's parsimony as limited by current scientific doctrine and presents Quine's insistence on the behavioral side of psychology as a mere concern for some empirical testability. This seems to me just lovely, of course.

The possibility of empirical underdeterminacy in principle has occurred to Peirce, and he tried to devise a theory of (empirical) meaning that will identify any two empirically indistinguishable theories. He failed, of course. Quine's "Two Dogmas" purports to explain why: radical empiricism is sensationalist and thus inevitably reductionist and verificationist-phenomenalist, i. e., with all meaning reducible to those of reports on sensations. (See, in this connection, Koppelberg on Ernst Mach.) The most mature expression of this is Carnap's "Der logische Aufbau der Welt". Why Quine lavishes such complements on this book I do not know: it is poorer than Russell's earlier work in logic, in epistemology, in methodology and in psychology (as it used outdated theory). Its non-holistic treatment of meaning dooms it to failure. (The old Mach and the young Wittgenstein, incidentally, ended up with the same sort of mystical extra-scientific holism.)

This brings us straight to the positive side of things, to the sixth and final section of the classical "Two Dogmas" where Quine pleads for a non-dogmatic empiricism. I always read it as a variant of Duhem's philosophy. "As

an empiricist I continue to think of the conceptual scheme of science as a tool,"
he says there, "for predicting future experiences in the light of past
experiences." This is straightforward instrumentalism and Koppelberg does
his best to obliterate it – with no small measure of success, yet he does not
quote the statement quoted here. "Physical objects are conceptually imported
into the situation as convenient intermediaries – not by definition in terms of
experience, but simply as irreducible posits", continues the above quotation
from Quine. This sounded to me as Duhemian instrumentalism par excellence.
Duhem was at pain to explain, and he did so magnificently, that he posits the
existence of atoms, but merely as truth by convention, that he is not
committed to the ontological assertion that matter is atomic. Indeed he was
committed, as a devout Roman Catholic, to Aristotle's plenism. Quine is not
committed to any theory: he is refreshingly undogmatic and that is his secret
charm. Is he committed to physicalism as he defines it? I think yes. Is
physicalism scientific? Hardly. The same goes for his other non-scientific
commitments: they all make his "Two Dogmas" out of character.

Quine's refusal to see sharp boundaries is his moderate holism, which Mario
Bunge calls systemism. They both view this idea as anti-reductionist. Bunge
views systemism as an ontology conflicting with reductionist ontology. Quine
may be read the same way as Bunge and as using the lack of sharp boundaries as an
argument for this ontology and against reductionist ontology. He may also be
viewed as a philosopher concerned with language alone, not with any ontology.
The latter is the received reading of Quine. The former is Koppelberg's.

There are three important scientific philosophers these days who share an
attitude which is admirable, and which puts them in the forefront of
contemporary scientifically-oriented philosophising – all differences between
them not withstanding (not to mention my own misgivings concerning each of
them): Popper, Quine and Bunge. They are all commensense philosophers (1)
in that they take science seriously yet as fallible, and they mean by science the
complex social and intellectual conglomerate which one finds in the modern
world and which is as far from perfection as any social entity; and, most
importantly, (2) in that they take its theories literally and at face value. (Also,
they are, all three, anti-reductionist metaphysicians.) The end of the
philosopher of science, after all, is not to defend science militant and not to
celebrate science triumphant but to try to contribute, perhaps humbly perhaps
even spectacularly, to (its) improvement and progress. I am grateful to
Koppelberg for his presentation of Quine in this light and for his helping me to
understand Quine better and thus appreciate him all the more.

Koppelberg's book will doubtless see new editions. I hope he improves the
next one, in line with some of the criticism tendered here in an appreciative
mood and with the addition of a subject index.

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