

W.V. Quine: Perspectives on Logic, Science and Philosophy

Interview by Bradley Edmister and Michael O'Shea

HRP: What is the role of philosophy vis-a-vis science?

Quine: I think of philosophy as continuous with science, but philosophy differs by degree in various respects. Philosophy undertakes to analyze the general, basic concepts of science — the sort of concepts the practicing scientist will typically take for granted. These are such basic notions as truth, existence, and necessity. Also, philosophy investigates questions of evidence for science — that's epistemology. It seeks a better understanding of the tremendous transformation that takes place between the input that we receive through the irritation of our sensory surfaces, and our torrential output in the form of scientific theory. It tries to analyze theory, and see how much of it is really dictated by the input ("by nature," we say, but that's only going to be in the input), and how much is only a matter of our accommodation and organization of it. These are considerations that aren't ordinarily taken up by any particular science.

In these studies, philosophy will sometimes elicit paradoxes, which the scientist, even if he is told about them, isn't likely to worry about. In normal scientific practice, he can simply dodge that end of his theory. But the philosopher is going to be concerned.

HRP: So the existence of paradoxes in philosophy of science doesn't affect the workings of scientific theory?

Quine: Right. This is brought up most dramatically in the familiar paradoxes of set theory — Russell's Paradox and the like. Even mathematicians, as a whole, didn't worry about them, because they weren't going to be dealing with self-membership in classes, or classes of all non-self-membered classes. They worked in mathematical domains where, when intuitively surveying the assumptions and axioms involved, they felt they were on solid ground. It's the sort of thing that falls quite naturally into the philosopher's domain.

W. V. Quine is Edgar Pierce Emeritus, at Harvard University. Among his many books and articles are *Web of Belief*, *Pursuit of Truth*, *Word and Object*, *Roots of Reference* and *Set Theory and Its Logic*.

HRP: That view seems to reduce the work of philosophy to simply tying up the loose ends of science. Is that accurate?

Quine: Yes, I think “tying up loose ends” is a good way of condensing philosophy’s purpose.

HRP: How much science should a philosopher know in order to do his job competently?

Quine: It’s important that he have a respectable grasp, at least at the undergraduate level, of a hard science. It’s extremely important to have had the experience of really *knowing* something without a shadow of doubt, even if it takes complicated argument, and having what one feels is a firm basis of evidence. Ideal for this purpose is mathematics, especially mathematical logic.

I have always felt that a good course in modern logic ought to be required, not only of every candidate for a philosophy degree, but for every undergraduate in every field. It could even be valuable for students specializing in the literary domain, where one doesn’t aspire to logic’s sort of certainty and conviction. It’s important for those students to see the difference.

HRP: What is the role of first-order logic in the scientific project, in physics, for example?

Quine: In relation to physics, I don’t see much of a role. I think that as far as elementary logic is concerned, the common sense of the physicist today (and of centuries ago, perhaps, before modern logic began) is enough. But modern logic is important for a systematic understanding of theoretical relationships; and when it comes to more analytical studies, both in foundations of mathematics and in philosophical analysis of concepts generally, logic is vital.

The illumination that modern logic has brought to philosophy of mathematics is the most illuminating instance. Gödel’s incompleteness proof contradicted what *everybody*, even mathematicians, had taken for granted, namely, that mathematical truth consists in demonstrability. [It was thought that] you may not find the proof, but the proof can be found, if the thing is true — and a proof purely mathematical in content and formulation. This is what Gödel showed to be an impossible situation.

HRP: How has the role of logic in philosophy changed from the 1920s and 1930s, when there was a lot of excitement about the foundational role of logic?

Quine: I don’t think the foundational role of logic has changed. But there’s been a lot of progress in specialized directions, with perhaps the greatest being in axiomatic set theory and higher categories of infinity, and then also a tremendous amount of progress in proof theory, of which Gödel’s proof is a shining example.

HRP: You crowned your recent *Pursuit of Truth* (1987) with the motto, “Save the surface and you save all.” How have your empiricism and behaviorism affected your view of what it means to give a philosophical

explanation?

Quine: That epigraph was accompanied with another one from Plato — “save the phenomena.” I find it particularly interesting that Plato appreciated this attitude, but it’s essentially just the statement of empiricism, the statement that what we’re trying to do is explain what we can observe. If we don’t respect our observations, and stick to them rather than revising them to fit our theory, then we’re not succeeding in our effort to achieve truth.

Behaviorism, as far as I’m concerned, is only an intersubjective empiricism. It’s empirical in attitude, but one doesn’t settle, in the manner of Husserl and the old epistemologists, for private, introspective data. When you take as your data your own perceptions, and pool these with those of your colleagues, and get the common denominator, then you have data which are pertinent to science from the standpoint of intersubjective behaviorism. I don’t see that as going beyond what every modern scientist would subscribe to as a matter of course.

HRP: In the reduction of common-sense terms for introspective human states to publicly-accessible, empiricist terms, is there a danger of losing something? Can empiricism do justice to our inner lives?



Quine: Here one must distinguish two factors: the dreaming-up of hypotheses and the amassing of evidence for them. All sorts of undisciplined thinking can be valuable as a first step in thinking up bright ideas, way-out, highly imaginative, which turn out to be just what’s needed once you get down to finding logical connections through experiments.

I don’t think that this process neglects the natural mentalistic input — it just means not settling for it. Daniel Dennett was very good on this topic in an essay defending introspection, which took pretty much this line, as I read it.

HRP: You wrote in *Pursuit of Truth* that “there is nothing in linguistic meaning beyond what is to be gleaned from overt behavior in observable circumstances.” Does the indeterminacy of translation hinder the intersubjective communication of scientific concepts, a communication which seems to be important for the project of “naturalizing epistemology”?

Quine: No, I think not. The indeterminacy of translation doesn’t get in the way of translation — it allows that there are good translations and bad. It’s

rather a question of the data that are available to see whether a translation is good or bad. It proposes external, behavioral criteria for the test.

Here's the situation: you have two translation manuals that are both as good as can be. This is according to the empirical test of using them in normal communication with a native, and finding that they lead to smooth dialogue and successful negotiation, which could mean getting directions somewhere, or the native's giving up some priceless relic for some glass beads, or whatever you're trying to bring about. Those are the tests.

If you have two manuals, arrived at independently, which both pass those tests to perfection, then you try meshing them. Suppose it turns out that if you use one of these manuals for a given text, it gives perfect, smooth translation, and if you use the other one it works just as well, but when you try alternating them on the same text it turns out to give incoherence. This illustrates what I mean by the indeterminacy of translation. My conjecture is that [this situation] is to be expected when we get into theoretical dialogue. However, it's not something that would show up in entering into scientific communication; both of these manuals are completely successful on the empirical score. In that case, the moral is: stick to the same manual.

"I think 'tying up loose ends [of science]' is a good way of condensing philosophy's purpose."

But translation isn't always possible in science. Today's physicist talks about neutrinos, for instance, and says "neutrinos are particles which

lack rest mass." That sentence certainly isn't translatable into the English of 1930, because there's no translation of "neutrino," not even a long paraphrase, because "neutrino" has never been defined. None of the particles, not even "electron," has ever been defined in the strict logical sense of definition. There aren't even contextual definitions comparable to Russell's definition of singular descriptions. No word or phrase, either by itself or in context, can take the place of the word "neutrino" or the word "electron." This is no obstacle; people use the word with no problem, and the native above can even use the word, given a certain substructure of explanation. Explanation, but not translation.

HRP: Your view explains intersubjective communication on the basis of readily observable events. But what happens when we're testing "manuals" about theoretical concepts or philosophical ideas? Presumably, such manuals could conflict with each other, yet they don't necessarily allow of observable evidence for the success of translation. This is more acute given the potential existence of radically different conceptual schemes among those communicating. What do we do in this case?

Quine: The way I see it, two different translators could in theory come up with quite different conceptual schemes for the same people. I see this as a matter of richness rather than poverty. It helps bring out the point that conceptual

schemes are almost completely a matter of human creativity — creativity to the purpose, however, of matching up with the neural input. Theory is so overwhelmingly much stronger and broader than the neural input that, of course, you expect slack, and that's what makes me believe in the thesis of indeterminacy.

In the case we're imagining, we have two accounts of the natives' metaphysics, which are not much alike. It is then a chastening experience to find that they're equally *faithful* accounts. Now what there is to be communicated of an objective, scientific kind is being communicated all the better, through our appreciating the theoretical latitude.

HRP: Would it be fruitful for philosophers to become irritated about this state of affairs and seek metaphysical explanations for this metaphysical looseness?

Quine: I don't think so. The distinctive thing about science is that there are checkpoints of observation. Everything that is compatible with those checkpoints [is acceptable]. One is always trying, as Sir Karl Popper pointed out, to "break" one's theory, devising the least hopeful experiment possible to subject it to. That's what marks the difference between responsible science and fancy, and it remains undisturbed through all this.

HRP: Your philosophy played a pivotal role in the demolition of the logical positivism of the 1930s. Do you feel any nostalgia for the philosophical optimism of those days?

Quine: I can see the attractiveness of that idea, but I also see something hopeful that seems to be taking its place: the tendency in certain philosophical circles (Dennett is again a shining example of this) to rub out, or at least blur, the boundaries between philosophy and various sciences. Here, Simon Saunders and others are trying to rub out the boundaries between physics and philosophy, along with others like Abner Shimony. It's not only getting physics into the philosophical circle, but philosophy into the physical circle — collaboration. These people take serious, advanced seminars in physics. Roger Penrose, in *The Emperor's New Mind*, is hoping that someone will come up with a new force, a new particle, that will give us a more intuitive understanding of new finds in quantum mechanics, of scientific concepts anyway. That's of as much interest to philosophers as physicists, and the addition of philosophers trained in physics might improve the situation in physics itself.

HRP: So the paradigm is not philosophy grounding physics, but rather a mutual strengthening and smoothing between philosophy and physics.

Quine: Exactly. Furthermore, it's not just in physics. This effect is present in the work of Dennett, with his seminars over at Tufts with people from neurology, computer science, linguistics, and psychology participating. The boundaries between philosophy and all these fields are wavering and disappearing, and this bears promise of a great new era.

HRP: Are we, then, on the verge of a new optimism about the role of phi-

osophy and science? Is there a feeling of possibility finally “getting things right”?

Quine: I don't know about getting things finally right — it's an interesting question whether there's an end point, but I have no firm opinion there. There *is* certainly progress, that seems very clear. What excites me in this collaboration is the prospect of progress, not necessarily getting to the end of the line.

HRP: What have you found to be the most exciting intellectual development of the last 20 years?

Quine: Near philosophy, and of close philosophical relevance, I think of the work of Hubel and Wiesel in neurology, and the utterly new picture that they've given of the neurology of vision. It seems that it's not a matter of the visual field being reflected isomorphically somewhere in the nervous system, but a matter of various dominant features coming in separately from one another, and not even being clearly synthesized until the moment of reflection. All of this goes on in hundredths of seconds. It gives us a new angle of approach.

Reaching back before 1974, I think of advances in psychology of vision made by Edwin H. Land, inventor of the Polaroid camera, and his theory of color vision. There's no guessing what they're going to come up with next.

HRP: You seem to have considerable respect for the work being done in cognitive science. How do you feel about the attitudes characteristic of cognitive science — for instance, the conjecture that machines can duplicate the intelligence of humans?

Quine: First, I find the Turing test for artificial intelligence unhelpful as a test, because human behavior depends so much on everything that's gone on through many years, reaching back to infancy, and even to the distant history of our corner of the world. Intelligence, it seems, is a matter of degree, and I'm hindered by the fact that I have no satisfactory criterion for what constitutes “thinking.” When we just go by output, of course, computers are already doing a quite remarkable job of thinking.

But I doubt that a machine could ever pass the Turing test. That was itself an arbitrary criterion to separate thinking from computing, and I have no hope for the development of a sharp criterion.

HRP: Famously, several of your papers written in the 1950s denied that the analytic/synthetic distinction had any foundational value, at least in the sense that Carnap and others wished. Does the distinction have any methodological significance?

Quine: In *The Roots of Reference* I suggested a definition of analyticity which seemed to me to approximate the layman's intuitive conception — not that the layman uses the term “analytic.” The analytic is what the layman calls “just a matter of words.” He dismisses someone's assertion as just a matter of words, as a question of how to use the words.

But we still want a criterion for analyticity, and one that has occurred to me is that a sentence is analytic (for a given native speaker) if he learns the truth

of the sentence in the course of learning one of its words. The obvious example is "No bachelor is married." A native speaker clearly learns the truth of this sentence by learning how to use the word "bachelor." A foreign speaker might have learned the word rather by translation. This criterion covers not only all sentences like that, but surely also all truths of elementary logic. Somebody who affirms " p and q " but denies " p " is, we say, misusing "and." He hasn't learned to use "and." That's true for all the basic principles of elementary logic, and from the basic principles you get *all* of elementary logic.

Elementary logic, in the sense I intend it here, is complete. It covers truth-functions, quantification, and identity — not set theory. This fits nicely with what Frege, Carnap, and Kant all held. I don't think of the rest of mathematics as being analytic. Furthermore, this isn't a concept that I can see applying across the board. This is what makes it unfruitful as a tool of philosophical analysis. Nobody remembers how they learned the truth of each sentence, and on the face of it, it shouldn't matter.

So a new word, "momentum," comes in. By definition, it's mass times velocity. No questions there, an arbitrary definition. We simply dig up an obsolete Latin word for this technical purpose. But even so, relativity theory

denies that momentum is simply mass times velocity. You have to get something in there about the square of the speed of light. So there's nothing about analyticity that compels particular uses of words. That's why I felt that Carnap's blanket application of the term in theoretical contexts generally was obscuring the issues and getting on the wrong track. This matter of truth by virtue of language does come in as a component, but not, I would argue, sentence by sentence.

Clearly, verbal convention is a factor in the truth of any sentence. If the syllables of the words in any sentence, however empirical it may be, were used for another purpose, the sentence might be false instead of true. So the degree to which language contributes to this or that sentence, this or that part of science, can be methodologically and scientifically significant.

HRP: Is the gradation of analyticity and syntheticity in ordinary language a philosophically interesting topic? What are some ways it might be talked about?

I think that there's no end to the important non-empirical beliefs and truths of history and sociology, and perhaps theoretical physics. You can add a whole bunch of them together and they won't be enough to imply any observations, and yet they're important.... Science would be paralyzed if we excluded the untestables.

Quine: I wouldn't think of it as coming into issue with particular words. But sentence by sentence...it seems reasonable that there would be gradations, maybe consisting in the magnitude of the network of definitions that are necessary to connect what we learn ostensibly with a sentence in question. That's a highly theoretical question, and perhaps that would capture the gradation.

HRP: You gave us a sense in which elementary logic is analytic, but were just now careful to exclude set theory from that sense, presumably because of the implications of Russell's Paradox. What is the status of set theory, and given its use in grounding the axioms of mathematics, where does that leave arithmetic?

Quine: Russell's Paradox made all the difference. Frege thought the whole of set theory was analytic, although even in the days before Russell's Paradox he noted somewhere that the axiom that people would be most dubious about in

terms of its analyticity was the universal comprehension principle [all predicates determine classes]. The question applies to higher mathematics generally, from set theory on up.

I see mathematics as sharing empirical content in a way connected with holism.

"[C]onceptual schemes are almost completely a matter of human creativity — creativitiiy to the purpose, however, of matching up with the neural input. Theory is so overwhelmingly much stronger and broader than the neural input that, of course, you expect slack, and that's what makes me believe in the thesis of indeterminacy."

In general, if you're testing a scientific hypothesis, typically you're going to need a whole cluster of other hypotheses before you get down to the level of observables with implied consequences. Among the auxiliary hypotheses you'll have to bring in, there will be some purely mathematical principles, differential and integral calculus, and so on.

So you might say that in general a cluster of scientific truths and hypotheses has an empirical "critical mass" if it is enough to imply observable consequences. And often, to get critical mass you're going to need some purely mathematical sentences. If you regard essential participation in a set of hypotheses with critical masses as what it means to have empirical content, it means that [all] applicable mathematics has empirical content. But it's not empirical in the sense of John Stuart Mill; that is, arithmetic is not the product of generalization from counting and counting again. Sometimes it is, I expect, *historically*, but that's not the point.

Then I would apply this in particular to set theory. But this matter of having empirical content must be separated from the old positivistic exclusion prin-

ciple of the meaninglessness of metaphysics. I think it's a mistake to require that a sentence must have empirical content in order to be meaningful. In fact, I think that there's no end to the important [non-empirical] beliefs and truths of history and sociology, and perhaps theoretical physics. You can add a whole bunch of them together and they won't be enough to imply any observations, and yet they're important. They seem plausible by virtue of symmetry, simplicity, fitting-in to the things that we have well established by tests. These are indispensable, moreover, in suggesting further hypotheses which one can test. Science would be paralyzed if we excluded the untestables.

HRP: Within a "critical mass" of hypotheses in mathematics, exactly how do the purely mathematical sentences relate to the empirical ones?

Quine: Here's something characteristic of mathematics, but perhaps not only of mathematics: That these sentences are needed as components of sets with critical mass, but that no set purely of mathematical components has critical mass. This is a sense in which mathematics can be viewed as auxiliary to science. But then history — something as unlike mathematics as can be — would also be auxiliary in this sense. Again, just a set of historical



sentences about, say, ancient Rome, and nothing more, can't be tested without the help of a lot more earthy generalizations. The tolerance that I'm urging regarding sentences that don't, of themselves, have empirical consequences, becomes curiously enough not merely a defense of highly speculative metaphysics or of history, but of hard mathematics itself.

HRP: Your work is marked by a clear concern for style of expression. What are your stylistic aims in writing philosophy?

Quine: Well, I certainly haven't thought about style enough to have principles of consequence, but one thing that I tend to try to avoid is mutually conflicting etymological metaphors.

One of the persistent errors that strikes me funny, which I first saw in a newspaper, and now it keeps popping up in print, is "stirring up tensions." Another is "it was at the height of the Depression."

Another specific thing I dislike is the heterogeneity of languages in new coinages. I like homogeneity of the components of a new word. For example,

a mathematician's "hypernatural numbers." No! "Supernatural numbers." *Hyper-* is Greek and *natural* is Latin. There's so much of that. I would hope that a scientist who needs a new technical word for something would consult a literate colleague, someone who knows Latin and Greek and could help him get it right.

In more general terms, I strive for clarity and brevity. It happens so often that by going over something and making it shorter I improve it.

HRP: Where did you pick up your rhetorical principles and skills?

Quine: When I look back to my first book, *A System of Logic* (1934), and articles from those years, I find that it's a more stilted style — I wasn't as sure of myself. My style's improved a lot since then. It's been mostly a matter of practice. I don't know of any source for my distaste for heterogeneities and so on.

HRP: At its best, it seems that American analytic philosophy tends toward

a radically clear, taut style that one sees particularly in both your work and Nelson Goodman's. It reflects a very different spirit from the German philosophical prose of

"One is always trying, as Sir Karl Popper pointed out, to 'break' one's theory, devising the least hopeful experiment possible to subject it to. That's what marks the difference between responsible science and fancy."

someone like Heidegger. Do you think that the tradition, and the values of those who work in it dictate a particular style?

Quine: That's a very interesting point, and I believe perhaps it does. There are certainly affinities between [my writing] and Goodman's. But when I think of how clear and succinct Carnap's German was...

HRP: Perhaps it's not national, but rather a divide between the analytic and Continental traditions.

Quine: Yes, I think that's it. It's the same sort of taste that makes for mathematical and logical elegance. It's linked to the desire for clarity and brevity. But my interest in overtones in etymological metaphors would be an independent factor. It comes from an interest in natural language as such that I've had from way back; it just happened. I got into etymologies when I was still in high school — I remember taking books out of the library about the origins of words. I still have the same urge; I'll want to know what the origin of a word is and have to look it up.

HRP: What modern philosophers do you follow with the most interest? Who do you think is on the right track?

Quine: I suppose that would have to include the work of my former student,

Donald Davidson. I don't agree with him on everything, but we're after nearly the same thing, and along the same lines. We have very profitable discussions.

HRP: That's interesting — in a controversial reading of Davidson's philosophy, Richard Rorty has taken Davidson as an exemplar of his own type of ironist, relativist anti-representationalism. What do you think of Rorty's stance?

Quine: I was surprised by reading Rorty's big book [*Philosophy and the Mirror of Nature*], the one that made such a stir. He said very favorable things about my philosophical views, which surprised me because I didn't like his position. It strikes me as defeatist, and it shows a bit of the anti-scientific bias that is more extreme in people like Derrida and Heidegger.

HRP: Those "Continental" thinkers are often distinguished from analytic philosophers by saying that they do not take scientific inquiry as an intellectual model. Your own project of "naturalizing epistemology," on the other hand, reflects the conviction that scientific theory provides the paradigm for what philosophical accounts should be like. Do you think it's possible to do viable philosophical work which is organized around other paradigms, like poetry or *belles lettres*?

Quine: It seems to me that the place where that sort of thinking has a function, and is still within the domain of philosophy, is in philosophy of poetry and the graphic arts. Whether we choose to call it philosophy or not, there is a place for the strictly artistic use of language in conjuring up thoughts and visions which help you get the point of an art work that you had missed before. I don't think that this function could ever be managed by strict scientific prose. But on the other hand, I don't see a place for it in scientific philosophy.

HRP: If a young philosophy student came to you for advice, and asked where the most pressing work in philosophy currently lay, where would you advise the student to turn?

Quine: One big project that has struck me as promising would be to take some limited part of hard science, like Newtonian mechanics, and try to trace out explicitly the logical connections of implication from its basic principles to the observational checkpoints. In other words, an *explicit* amassing of evidence. It would have the advantage not only of illuminating the epistemology of the science, but also might even be a contribution to the science, by suggesting shortcuts and simplifications, or by showing how some bit of theory just didn't get used or serve its purpose. That method, if it succeeded with a sample chunk of science, might spread to other cases, and, if it succeeded, might help make philosophy a handmaiden to science, along with mathematics. ∅