

their predecessor. But Wittgenstein is no superficial use-theorist. His conception of use is tied to the idea of institution and practice, and crucially, institutions and practices do not reduce to the applications and dispositions of a moment. For Wittgenstein, the context that matters, in understanding what we do with language, is broad and open-ended. The idea that the content of a given assertion will automatically be determined by dispositions specific to that local discursive context alone, and so that contextual variation in dispositions of application automatically bespeaks contextual-dependence of content, is foreign to him. I close with two passages, one familiar, one less so, that make this point well:

33. . . . What, in a complicated surrounding, we call “following a rule” we should certainly not call that if it stood in isolation.

34. Language, I should like to say, relates to a way of living.

In order to describe the phenomenon of language, one must describe a practice, not something that happens once, no matter of what kind.

It is very hard to realize this. (Wittgenstein, 1978, pp. 335–6)

How could human behaviour be described? Surely only by showing the actions of a variety of humans, as they are all mixed up together. Not what one man is doing now, but the whole hurly-burly, is the background against which we see an action, and it determines our judgment, our concepts, and our reactions. (Wittgenstein, 1980, p. 213) ¶

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INTERVIEW

On Mathematics, Realism, and Ethics

An Interview with Hilary Putnam



HRP: Let’s start at the beginning of your career. What was it like working on Hilbert’s 10th Problem?

Putnam: I am, of course, a mathematician as well as a philosopher. And I’m very happy about that [*laughs*]. I started working in mathematical logic when I taught in Princeton in the fifties. Georg Kreisel, who was really my mentor in logic, was at the Institute for Advanced Studies. I went to see him and told him that I taught what was called symbolic logic in the philosophy department, and that I’d like to see if I could prove an original theorem. I do mathematics rapidly when somebody explains it to me on a blackboard—I can pick

up a proof in an hour by listening to someone explain it and asking questions, but it would take me weeks to read it in a published paper. Mathematics papers are not written to be easily understood. There’s a real divide between the way mathematicians explain proofs to one another and the way they write them up.

So Kreisel explained some concepts and then gave me a problem, which I solved, and then he said, “Now you publish this result.” The paper I wrote (“Decidability and Essential Undecidability”) came out in 1957. It was my first mathematical publication. It wasn’t on Hilbert’s 10th Problem¹, which I went on to afterwards. In fact, our relationship cooled eventually because Kreisel was dead set against my working on that problem. He thought it was a dead end—that nothing would come of it. But around the same time, a person who is still my very close friend, a mathematician named Martin Davis, also came to the Institute for Advanced Studies. We and our wives became constant companions. There was a summer institute in mathematical logic in 1957 at Cornell. The Davises and the Putnams were there together. Martin had published some papers working towards a solution to Hilbert’s 10th, and I suggested that we try to extend his

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work and try to go further.

So that was the beginning. We didn't have much time that summer, because there was a conference on just about every topic in logic going on, but the next two summers we rented places near one another in Connecticut, north of Hartford, and we worked full time. In fact, as Davis described it, I was the idea man and he was the filter. In 1959, Martin Davis and I proved a theorem which, together with results by Julia Robinson and a later result by Yuri Matiyasevich, added up to a "negative solution" to that problem. As Davis describes our way of working in George Csicsery's film "Julia Robinson and Hilbert's 10th Problem,"² I was a "fountain of ideas," and Martin's role was to say, "That's ridiculous . . . that's ridiculous . . . that's ridiculous" until every so often one of my idea wasn't ridiculous, and then we would both set to work on it.

So those summers I would sometimes work on the problem until four in the morning. I was still young, in my very early thirties with lots of energy. And by 1959, we had *almost* a proof—not of the unsolvability of Hilbert's 10th Problem, but of the unsolvability of what are called "exponential Diophantine equations." These are polynomial equations in which the exponents can be variables, and the coefficients are all integers and one asks for a solution in integers. $x^3 + y^3 = z^3$ is a Diophantine equation, that was proved unsolvable in integers by Fermat himself, and he claimed he had a proof (though no one knows if he really had one) that $x^n + y^n = z^n$ is also unsolvable in integers whenever n is bigger than 2. Now that's not a Diophantine equation, because n is variable, so that's an exponential Diophantine equation. But Martin and I had a proof that the decision problem for exponential Diophantine equations is unsolvable, *if* the following hypothesis is true, which we called "PAP" for Primes in Arithmetic Progression: there are sequences of prime numbers as long as you like in arithmetic progression. PAP was, in fact, proved in 2004 by Ben Green and Terence Tao. If PAP had really been necessary to prove the unsolvability of exponential Diophantine equations (and thus, necessary to the proof of the unsolvability of Hilbert's 10th Problem), then the solution to Hilbert's 10th would only have been completed in 2004, and Green and Tao would have to be considered as completing the solution of Hilbert's 10th.

However, going back to the summer of 1959, we already knew, via a result of Julia Robinson's, that the unsolvability of the decision problem for exponential Diophantine equations *implies* the unsolvability of Hilbert's 10th Problem if there is even one Diophantine equation with the following property: it has infinitely many solutions, but as you change one of the coefficients, one of the parameters, it has only finitely many solutions for each value of that parameter, and the largest of those solutions grows roughly exponentially as the value of the parameter is increased. So at that point, the solution to Hilbert's 10th Problem had been reduced to two things: first, the problem of PAP—but when we sent our proof to Julia [Robinson], she showed, in fact with a variant of an argument of mine, that we didn't need PAP! So we did have a proof of the unsolvability of *exponential* Diophantine equations. And the three of us published that together in the early 60's in *Annals of Mathematics*. Then completing the (negative) solution of Hilbert's 10th Problem was a matter of somebody coming up with a single example of such a Diophantine equation, and within ten years, Yuri Matiyasevich in Russia did come up with such an example—an example closely related to the Fibonacci numbers,

by the way. Martin and I had thought that the Fibonacci numbers might hold a solution, but we were unable to prove it. But Matiyasevich did give an example, so now The Gang of Four had a proof of the negative solution to Hilbert's 10th.

After that, still focusing on my mathematical work, I turned to set theory. My work was concerned, among other things, with what is called the "fine structure of Gödel's constructible sets."

HRP: I think most people are probably less familiar with that aspect of your work. Would you mind describing what that entailed?

Putnam: Well, Gödel proposed an axiom that he himself rejected later, on philosophical grounds. He didn't like it, and few set theorists liked it—although it's consistent with the other axioms of set theory, so you can't *refute* it. It puts a certain kind of structure on the universe of sets: it says they're all constructible, in a sense I won't try to define [*laughs*]. But if all the sets there are constructible in Gödel's sense, then that would imply the axiom of choice, and it would imply Cantor's famous continuum hypothesis. It would essentially close all the major open questions there are about the structure of the universe of sets—that's what makes it so interesting. I started a field called the examination of the fine structure of Gödel's sub-universe of constructible sets, which is still an active field of research. Mainly, contemporary set theory is looking for alternatives to Gödel's axiom of constructability, but the constructible sets themselves are one of the major tools.

HRP: Some active research in set theory and logic is philosophically motivated—you mentioned that Gödel gave up this axiom on *philosophical* grounds. Was there any relationship between your work on constructible sets and your philosophical leanings?

Putnam: Not directly, I don't think, although I've thought a lot about the philosophy of set theory, and I am a realist about set theory.

HRP: The topic of realism brings up another question I'd like to ask. A lot has been made about how frequently you've changed your position on various philosophical issues over the years, and I want to get to that, but first I'd like to ask, are there any conclusions or arguments or ideas you've held firmly onto your whole career?

Putnam: Well, first of all I'd say there are certain themes that characterize my work, throughout my whole career. I think we could say that I have always been seeking a way of doing philosophy that's scientifically respectable and scientifically informed—in fact, you know I try to be sophisticated about and draw on a wide range of sciences. So I think philosophy should be scientifically respectable and scientifically informed, but at the same time, I don't think that philosophy is reducible to the sciences. That sort of reductionism, I think, is wrong. Although there's common ground between philosophical arguments and actual scientific practice, one danger of being too much in awe of science

is that you have to steer clear of identifying science with a number of different ideologically driven definitions of science, which have been common in the past 100 and more years. So I would say, ideally, this scientifically informed way of doing philosophy, which doesn't believe that philosophy is going to *become* a science or that philosophy is going to be reducible to various ideological notions of science, is a common theme in all my work.

As far as conclusions are concerned, quite a few. I'd say first of all, I'm only aware of two *major* issues on which I've actually changed my mind. One is computer-machine functionalism, or computer-program functionalism—this I've certainly changed my mind on—and the other is what I called *internal realism*, a position I held for about fourteen years. And quantum logic, if you want to count that. But, for example, I think all but about four or five of the papers in my first collection of papers I still agree with. I mean, obviously I think some arguments were good, some arguments I wish I had given, but basically I think I agree with just about every paper in my first two Cambridge University Press volumes, if not with every argument. But what philosopher who lives as long as I have agrees with every argument he ever gave?

To be more specific, let me describe the papers in which I found my own philosophical voice. I came to Princeton in 1953, very much as a student of Hans Reichenbach—very much a logical empiricist. But Reichenbach I see as a very atypical logical empiricist in that he was a scientific realist, and he believed, I think incorrectly, that positivism, or the verifiability theory of meaning, and realism, were reconcilable. That's all over the place in his book *Experience and Prediction*, and he also attacked the idea that theoretical entities in science are constructs, which was a hallmark of Carnap's view. Reichenbach always held that they are not constructs; they're inferred entities. He held that we make inductive inferences to the existence of things we can't see.

HRP: Something that you also took over from Reichenbach?

Putnam: That's right. And I think I took from Reichenbach the idea that it is important to ask what the implications of the great scientific theories are. He was a friend of Einstein's and of course he knew relativity very well, and in fact I had my introduction to relativity through Reichenbach. But he asked, what are the implications of these new theories—quantum mechanics, though he was less successful there—but also what are the implications of relativity theory, general and special, for classic philosophical questions about the nature of causality and the nature of time? So I would say it's unfortunate that he made an alliance with Carnap. I think Carnap talked him out of some good ideas, and also made him fit what he was doing into a framework that was incompatible with it. But that interest I kept from Reichenbach—asking “What does contemporary physics have to tell us about the nature of physical reality?”

Now the first paper in which I found my own voice, the paper in which I ceased being a Reichenbachian, was a paper I wrote in about 1958 (although it was published many years later), “The Analytic and the Synthetic.” The day I met my wife Ruth Anna, which was August 27th, 1960, I gave a paper called

“What Theories Are Not,” and in a way, those two papers were my manifestoes. In those two papers together, you can see an awful lot of my later thought. Even some of the ideas in “The Meaning of ‘Meaning’” can be seen in “The Analytic and the Synthetic.”

HRP: If I had to guess, I would have also put “It Ain't Necessarily So” in that group of early papers.

Putnam: Oh yes, yes, but most of “It Ain't Necessarily So” is in “The Analytic and the Synthetic.” Of course, “It Ain't Necessarily So” was written several years afterwards.

HRP: I would like to turn to the issue of realism now. It's obviously a major theme in your work, whether a defense or a criticism of it. Now, you mentioned earlier that one of the two things you've given up was internal realism. Can you give us a brief history of what happened with internal realism—why you were motivated by it, and why you gave it up?

Putnam: Starting with a Presidential Address I gave here in Boston to the Eastern Division of the American Philosophical Association in 1976, I slipped back into a form of verificationism. Partially that was the influence of Michael Dummett, who had recently given the William James Lectures here. Well, first, my well-known “model-theoretic argument” occurred to me—that was a dilemma for me. The model-theoretic argument drove me crazy for about four years before I gave into it. I spent about four years trying to refute it, which I think can only be done if you reject some of its assumptions, which I did *not* do. I didn't see that the model-theoretic argument itself really depended on questionable assumptions. It was an argument that assumed, as it were, that the external world impinges—is available to us, is a constraint on what we can say—only via observation sentences. From Dummett, I took the idea that there's no privileged observation vocabulary. But I ultimately identified observations with something going on *internally*. I think that my internal realism was very closely connected to the problem of skepticism—basically, I slipped back into a Cartesian picture: here I am, locked inside of my brain, I have these inputs, and the only thing that can constrain what I say is these inputs plus what I called “theoretical constraints” like simplicity and elegance and so on. Once you accept that picture, I think then the question of how can a *unique* interpretation, a unique correspondence relation between our terms and anything external, be fixed becomes unsolvable. And having tried to refute that for four years the wrong way, instead asking myself whether this wasn't a hopelessly positivistic picture of the mind, I decided “if you can't lick 'em join 'em.” [*laughs*]

Most of the critics of the model-theoretic argument up to this day don't really talk about the need to have an alternative picture—except McDowell, of course, who later influenced me very much. But by 1990, I had pretty much decided that picture was wrong. My late “internal realist” books and papers used the term “internal realism” but mainly they talked about conceptual relativity. They said less and less about verificationism.

There was a conference on my philosophy, immediately after I had given the Gifford Lectures [published as *Renewing Philosophy*], called the Gifford Conference in Saint Andrews in November of 1990, and in some of my replies, particularly my reply to Simon Blackburn, I said that verificationism was a mistake, and I went on to explain why it was a mistake in things that I published over the next four years. There was an issue of *Philosophical Topics* on Hilary Putnam in 1992 in which in one of the papers I explained why it was a mistake. Unfortunately, that issue is hard to find—I've discovered that in almost all the libraries in the world, that issue has been stolen! Every time I visit a university and say "Well, you should read that issue," the students say "Oh, well that issue's been stolen from our library." [laughs]

HRP: That might be flattering.

Putnam: It's flattering in a way, but I wish that the thieves had just contented themselves with photocopies [laughs]. But then in the Dewey Lectures [collected in *The Threefold Cord; Mind, Body and World*], I did explain why I thought that internal realism was wrong, and I called my new position "commonsense realism," but I have discovered that this name is a Rorschach test. It was a mistake to use it, because everybody who sees it comes up with a different understanding of what I meant by "commonsense realism." I thought it was pretty clear, but I'm not going to use that term any more. Now I just say that I am a realist in metaphysics.

HRP: In the Dewey Lectures, you give a critique of functionalism and representationalism in general, and I take it that such a criticism is motivated by these issues of realism—

Putnam: No it wasn't. Jerry Fodor and I—he was my student—have, I think in many ways, converged much more than it might seem, although he draws different conclusions. For example, he's given up the notion of narrow content now, in his most recent book. He rejected the notion of narrow content for good reasons, although he throws out the baby with the bathwater by deciding that we have to throw out the notion of intentional meaning entirely, and *only* talk about reference, which is not a conclusion I draw. But I would say that the reason [those critiques] didn't have to do with issues of realism, is—well, first of all, Jerry's review of *The Threefold Cord*, was a strange review in that he described the book as an attack on mental representation. I looked in the index, and there's only one passing reference to mental representation! I mean, just take a look at the book—when an English speaker thinks, yes, there may well be mental representations that are connected with the words she uses, but the question is are there language-independent, *innate* mental representations? That's a very different kettle of fish. That wasn't the topic of *The Threefold Cord*, which was perception—which Jerry has very little to say about.

Now I think the real issue is this: let's go back to the "The Meaning of 'Meaning.'" In "The Meaning of 'Meaning,'" I did suggest the notion of narrow content; I wrote "psychological states in the narrow sense." I suggested there that

narrow content might *not* be useful for psychology, and in fact I don't think that it's useful for psychology, and I try to spell that out in my Royce Lectures. But Jerry, for a long time, thought that psychology is *about* narrow contents. Now he thinks as I do, that that isn't really what psychology is about—that you can't put psychology in that procrustean bed. If a psychologist says, the rat saw the cheese and pressed the bar, she's talking about seeing cheese and pressing bars, which are not inside the rat's brain [laughs]. To demand that psychologists must never talk about bars and cheese is rather odd. The issue was about narrow content, and the main point about narrow content is that it abstracts from reference, so that the issue of realism doesn't really arise with respect to narrow contents.

Now, about my criticism of computational models of narrow content. I think that the problem with the notion of narrow content (this is the argument I spell out in about, I think, six pages in *The Threefold Cord*) is this: we really possess only a sufficient condition for saying that something's the narrow content of an utterance, and one which is never realized in the real world. Namely, if somebody is in the same *brain state* as someone who thinks a thought with the same *broad content* as "The cat is on the mat," then that person has a thought with the same *narrow content* as "The cat is on the mat." But the trouble is that "A and B utter sentences with the same narrow content *when* they are in the same brain state" is not a very useful criterion of identity. And if you say "Well, it's enough that he or she be in a brain state such that somebody else *could* be in it while thinking a thought which has the same broad content as 'The cat is on the mat,'" then the notion of narrow content then becomes parasitic on the notion of broad content, that is, on the ordinary notion of meaning, because broad content is just a rational reconstruction of the ordinary notion of meaning. I don't believe we have an *independent* notion of narrow content. If you could identify narrow contents with computer programs, then you could say, "Okay, there is a computational criterion for sameness of narrow content." But both Jerry and I have come to the conclusion that there is no *computational* criterion for saying what is "meaning" in *any* sense of meaning: broad content, narrow content, or reference.

HRP: I take it that the arguments for that are in *Representation and Reality*?

Putnam: And now, Jerry also accepts the idea that mental states, intentional states, and meaning-bearing states, are not only compositionally plastic, but computationally plastic as well.

I don't want to say that sameness of meaning is reducible to a notion of good interpretation (because I don't think one is prior to the other), but I think that sameness of meaning and reasonable interpretation are entangled notions. That to formalize the notion of having the same content in any of these senses, you would have to be able to formalize the notion of good interpretation—and while I can't prove that it is impossible, there is no reason to think that it is possible.

I remember once asking Noam [Chomsky], "Do you think that there is a computational criterion for when the brain is acting up to its competence in scientific reasoning?" There is supposedly a scientific competence like our linguistic competence, and all of that, he thinks, is innate in a different way and

in a different place than our language organ. And he said “Yes.” But surely one should be able to prove that if our total rational capacity—inductive logic, and deductive logic and mathematics—is formalizable, then we *couldn't know* the program [*laughs*]. I think it's easy to sketch a proof that that is indeed the case. But maybe someone will show that there's something different about interpretation, and that interpretation really is formalizable, but all I want to say is that we don't even have a sketch of an idea how to formalize it, and there's no *a priori* reason to think that it has to be formalizable. The example of the Gödel theorem suggests that things are otherwise. In fact, I have a paper that just came out titled “The Gödel Theorem and Human Nature,” that argues that it's more than just a suggestion.

HRP: I think that people working in cognitive science and psychology in general find claims like that a little hard to understand. I think they would suspect there has to be some kind of good description of how we engage in reasoning and interpretation. So what does it mean for these things not to be formalizable?

Putnam: Well, take the case of whether the intuitive notion of proof, not proof in a particular formal system, but the intuitive notion of a correct mathematical proof, is formalizable. There is a sense in which that's not formalizable, in the sense that if there were an algorithm which generated all and only correct mathematical proofs, we could not *prove* that it was correct [*laughs*]. And I believe the same thing stands to inductive logic. If you could formalize degree of confirmation, as Carnap hoped—if there were a program which assigned an “accept” or degree of confirmation greater than 0.5 to all and only those hypotheses it is rational to accept, then the hypothesis that that program, or any particular program, *does that* would itself be not one that we could confirm. It can happen that this is the great surprise of the Gödel theorems—that some of our abilities are such that we can partly formalize them, but with respect to any partial formalization, we are able to prove that it is only partial.

However, I agree that I was too hard on cognitive scientists in some of my writing. In “Cognitive Science or Science Fiction?” I did suggest that all cognitive scientists are basically computer nerds, that they're all engaged in writing programs and in trying to reproduce our mental abilities entirely by writing programs, and I now think that was wrong. There's a lot of cognitive science out there that is not *that* reductionist.

HRP: So then in what direction would you like to see the program of cognitive science take?

Putnam: Well, obviously “let a thousand flowers bloom.” I think there's a lot of good cognitive science. The work of Amos Tversky, and people who followed him, like Maya Bar-Hillel, is also cognitive science. This is an attempt to really describe what you might call useful errors in reasoning—including errors we had better *make*, because there isn't always the time in the real world to use the best algorithm. I find that whole field fascinating. There is also the sort of thing that

Ruth Millikan talks about—which again, I think I was too hard on in *Renewing Philosophy*—the idea of “normal biological functions,” which is a side of cognitive function and cognitive science that connects with evolutionary theory. In general, I think that our mental states are functional states—functional states not in the sense of computer programs—but they are states which have functions. I would call them functional states with long arms [*laughs*]. Basically that's what externalism says: knowing the meaning of a word is having a long-armed ability, an ability that reaches out and can only be described by also describing the normal environment it's geared to and what it responds to in that environment.

HRP: So, to that extent, evolution can explain representation—the title of one of the chapters in *Renewing Philosophy*.

Putnam: Well, let's say evolution is certainly part of the explanation, but obviously most of it is cultural.

HRP: I'd like to turn back to realism for the moment. You discussed why internal realism failed, but I'd like to ask you about your current views on realism. What does that view look like?

Putnam: I would say that I am, in some sense of metaphysics, a realist in my metaphysics, although not a metaphysical realist in the overly narrow and overly specific sense that I attacked in my internal realist period. In my internal realist period, I saddled the metaphysical realist with having to reject conceptual pluralism and conceptual relativity—and of course if you do that, then you don't really need the verificationism. As long as you recognize conceptual relativity and conceptual pluralism as real phenomena, then you're already defeating metaphysical realism. But back then, I used to smuggle the verificationism in under the umbrella, but I would not do that now.

I would say yes, I am a realist in my metaphysics. I would also say I am a scientific realist; scientific realism is a *part* of my realism, but have never thought of it as the same as metaphysical realism. I think scientific realism is something that I believed in during my internal realist period, and before and after, because scientific realism deals with the issue of whether scientific theories are just computing devices or just prediction devices, and says, “No, they are descriptions of reality.” And you can be a metaphysical realism and reject scientific realism—Bas van Fraassen is my example. And on the other hand, you can be a scientific realist and reject metaphysical realism, as I did in my internal realist period. I believe my “no miracles argument” [in *Meaning and the Moral Sciences*] is correct—that is, I believe the argument that positivism and instrumentalism make the success of science a miracle, and that can't be right. And for me that's still a cornerstone.

HRP: You said at a recent lecture at Boston University that scientific realism need not be entity realism. What does such a scientific realism look like?

Putnam: Well, remember Wittgenstein's *Tractatus*. He said the world consists

of facts, and not of things. I think there are facts, more broadly states of affairs, which are mind-independent, but I think the particular language we use, not just the natural language we use (although that's a big factor) but also the scientific language we make up, often provides different descriptions of the same state of affairs, and very often those descriptions don't have the same ontology or ideology in Quine's sense. I used an example [in that lecture] from quantum mechanics, cases where what physicists regard as one and the same state of affairs, can be described in terms of fermions or in terms of bosons. This is a very startling and striking example, called duality in modern quantum physics, and there are lots of examples of duality—in fact, it's one of the big themes in string theory. This is a phenomenon that I call conceptual equivalence. So basically here I would say duality seems to be compatible more with Tractarian realism than with Quinean realism—not realism about *entities* but realism about states of affairs.

HRP: So then there's no tension conceptual relativity and pluralism on the one hand and scientific realism on the other?

Putnam: That's what I think, yes.

HRP: Again, I imagine that view is motivated by the success of science.

Putnam: The success of science, yes. The fact is that these cases of conceptual relativity are cases where the theories are actually intertranslatable. These aren't translations that a linguist would regard as correct, but why should a physicist care? [*laughs*]

HRP: So we have a harmony between conceptual pluralism and scientific realism, and in your discussions about speaking objectively and having objective knowledge, you've spoken about "warranted assertability." Could you describe what it is for an assertion to be warranted?

Putnam: Well, for an assertion to be warranted is for it to be reasonable to accept it. And of course, Tim Scanlon, whom I assume you have covered in the magazine or will cover in the magazine (I'm referring to the Locke Lectures he recently gave) defends what he calls "reasons fundamentalism," by which he means the idea that reasonableness is not reducible to anything else, but nevertheless we should not conclude that it is all just culturally relative or, like the late Richard Rorty, what your cultural peers like to believe, etcetera. And I'm a reasons fundamentalist in Scanlon's sense—once you go soft on the notion of reasonableness, then of course you've automatically given up scientific realism; as far as I can see you've given up every form of reasonableness and that does not seem reasonable to me [*laughs*].

HRP: But certainly how we cash out the notion of reasonable here is not supposed to be, say, phenomenological—that is, something is reasonable if it just *seems* reasonable. Can you describe what reasonable is in this sense? You can't formalize it, certainly . . .

Putnam: It's not just that you can't formalize it, but we have criteria for being reasonable and unreasonable, and I think the world itself *forces* us to revise those criteria from time to time. That's one of the reasons it defeats formalization. And even one of the great positivists, Comte—who, I guess, gave us the name "positivism"—said there is no such thing as *the* scientific method. Each science has its own method. Certainly he didn't mean there's nothing in common—all the sciences owe something to Bacon. They all recognize the idea of putting questions to nature, they all share the Baconian rejection of the idea that science can be done *a priori*. We replaced the idea of asking what Aristotle said by asking what nature says and putting questions to nature.

Of course, Habermas introduced the idea of discourse ethics, but I think you can find all the principles of Habermas' discourse ethics in Dewey. I once taught a seminar with Seyla Benhabib that turned into a sort of Dewey versus Habermas seminar. She was arguing for the superiority of Habermas over Dewey, and I pulled out things that Dewey wrote in the thirties, things that contain explicitly every one of the principles of discourse ethics that Habermas listed [*laughs*]. You know, principles like people must have the right to make objections, ask questions, freedom of thought, freedom of inquiry, taking objections seriously, etcetera. These things are all that I think Dewey meant by "being scientific," and so any serious field of scholarship should aim at being scientific in that wide sense.

But in the more narrow sense, I think you learn what it is to proceed scientifically if you're going to be a molecular biologist, in your first couple of years of graduate school; and you learn what it is, in say string theory, in your first couple years of graduate school. And what the string theorist learns and what the molecular biologist learns are not the same thing. And in fact, if you believe Peter Galison, what the experimental physicist and the theoretical physicist learn are not the same thing—that's why they have to use what he calls "local pidgens" and "creoles" to communicate with one another. Wittgenstein famously said "mathematics is a motely" but it is much more true that science, as a whole, is a motely.

But there's something else that keeps us honest besides discourse ethics. Of course discourse ethics is a part of objectivity—and according to Rorty it was *all* of it; according to Rorty, all there is to the notion of objectivity, at least in *our* Western culture, which is the one [Rorty] liked, are these principles of discourse ethics. And he was a great admirer of Habermas, paradoxically. [Rorty] thought "Yes I can just talk about discourse. I don't have to talk about the world."

But it's not just methodology—you know, the methodology of let's say sophisticated empiricism—which I think was Newton's correction to Bacon (that's another subject), but I think Newton saw Bacon as putting too much weight on direct observation, and that what you have to do is combine the notion of induction—Newton does use the word *induction*—with mathematical analysis. And Newton was the first to see that there could be a mathematically sophisticated empiricism. If Dewey had known enough mathematics to really understand Newton's *Principia*, and not just admire it, I wonder whether the history of pragmatism would have been different [*laughs*]. So I think there is

something we can say about the methodology of objectivity, and it has been said.

But also we have to say that scientific methodology itself is something that the world sometimes forces us to correct. I think that in the case of quantum mechanics, quantum mechanics violates—I hope it won't always—a lot of what people in the nineteenth century thought and had good reason to think was required of a scientific theory. One thing that was required of a scientific theory was that you should be able to understand it. And of course the question of how you could understand quantum mechanics is controversial to this minute. So we had to widen our concept of what's even allowable in theoretical physics. And that's a manner of being sensitive to the demands of nature itself.

HRP: To follow up on that a little, in a lot of your writings you talk about things such as principles of charity, principles of economy, or cases such as choosing between Einstein's relativity or Whitehead's alternative theory of gravitation—or in this case, choosing to keep the reference of terms like "momentum" the same across theories. Are these what you mean by "constitutive standards of rationality"?

Putnam: These are *provisionally* constitutive. Nature may force us to go back and rethink. I don't believe in any absolute *a priori*. But these are provisionally *a priori* principles of scientific procedure.

HRP: You mentioned Habermas, and it seems, at least, that as your career has progressed, you have grown more and more interested in Continental thinkers such as Derrida, Foucault, Habermas, and the like. Can you describe the advent of this interest and what you get from these thinkers?

Putnam: When I first hear that question, I think "That would have been true in 1990," but it hasn't really been true since. My interest in the French intellectuals has somewhat declined, with one big exception, but he's not a postmodernist or a deconstructionist—Pierre Hadot—who died a couple of weeks ago. In fact, we corresponded just before he died. A "Philosophy of Hilary Putnam" volume is going to appear in the Library of Living Philosophers, and Hadot has a paper in that—and I had just sent him my very approving reply to his paper, and we exchanged emails about that, just before he died. But Pierre Hadot is not a deconstructionist—in fact, he was the first French philosopher to write on Wittgenstein.

Really, my interest in Derrida very much declined after 1990. I wrote a critical chapter on him in *Renewing Philosophy*, and I do think some of his ideas are interesting. For instance, asking whether a philosopher's tropes are really compatible with what appears to be the literal meaning of his theses is a very good idea. A particular example: one might ask in the case of Quine, when he says that "reference floats," when he employs the trope of reference floating, and says that this is all right as long as the boats stay together—well, in that trope, a good Derridean might point out that it looks as if the *water* has to be something like a noumenal reality [*laughs*]. The trope is totally incompatible with Quine's

philosophy. I think in general looking closely at the literary devices a philosopher uses and asking how compatible they are with what seems to be the literal meaning of the theses he defends is a good idea.

But nevertheless, there's an awful lot of gobbledygook. I mean, the idea that we *discovered* in France that there's no such thing as truth, and no such thing as the real world—this is the way that English departments and French departments expressed their enthusiasm for Derrida, as if we had *discovered* that there is no such thing as objectivity—is absurd. Even Derrida stayed short of saying that there's no such thing as truth, but he sometimes suggested that if we were daring enough, we would go that far, but for some strange reason we can't. So I don't find very much I can use in Derrida.

Foucault as well goes too far sometimes, in my view, but Ian Hacking and a former student of mine—Arnold Davidson at Chicago—have gotten something important from Foucault, namely the idea that there's no contradiction between analytic philosophy, that is analyzing concepts, and historical analysis. The fact that you're interested in the analysis of concepts doesn't mean that you're committed to a kind of Platonism in which you treat concepts as the sort of things that have no history.

In particular, Foucault and Hacking and Arnold Davidson are right that the concepts in the human sciences used to describe people may actually change the way people behave. Consider the way the concept of homosexuality succeeded the medieval concept of perversion, an example that Arnold Davidson has used: there was no "medical problem" of homosexuality for the medievals—"perversion" then was just a sin. And then suddenly, when it became a medical category, first people tried to explain homosexuality as a sign of genital malformation (!), but it turned out they couldn't find a genital malformation, so they tried to find a brain malformation. And psychoanalysis succeeded that, with the idea that homosexuality is a "neurotic symptom," and after that (and fortunately) we realized that the assumption that there is something *wrong* with homosexuality is what needed to be challenged. So in these areas, doing Foucaultian analysis, with perhaps more responsibility to the facts than Foucault had, and doing conceptual analysis are not incompatible, and they shed light on one another.

HRP: Something that seems to emerge later in your writings is an interest in ethics. Can you describe the relationship between your other work and your ethical philosophy?

Putnam: I tried in my last book, *Ethics without Ontology*, to argue that if you step back, you can see that many of the puzzles raised about how ethics can be objective have exactly the same form as puzzles raised about how mathematics can be objective. And when you see virtually the same arguments being applied to subject matters as different as ethics and mathematics, then asking "What picture are these philosophers in the grip of?" is a good idea. And here I think it is something like a picture that objectivity requires objects—it's almost built into the nature of the word "objectivity." You might think there must be funny ethical properties to make true ethical statements, or there must be funny objects—Quine once said

that numbers are intangible objects—to make mathematics true. I think that’s a mistake. I think ethical judgments are judgments of what is required by certain fundamental human interests, and that ethics has a *history*. My problem with evolutionary ethics is that it confuses explaining altruism with explaining ethics, which is curious, because “ethics” is a Greek word, but “altruism” is a notion that doesn’t appear in Aristotle’s *Nicomachean Ethics* at all; there is no general virtue of altruism, and certainly not altruism in the sense of respecting the needs of the other regardless of social class or ability. Something like sociobiology can certainly shed some interesting light on how we’re *able* to be ethical, but again, that’s not all there is to ethics.

Historically, we have I think at least two huge competing ethical systems—one I call “macho ethics.” And fascism was in many ways a conscious intellectual revival of macho ethics, via a misreading of Nietzsche, whom I would not blame for it, but fascist intellectuals basically erected an ethical system based on the importance of virility, courage, daring, etcetera. Perhaps you could give an evolutionary explanation of our attraction to macho ethics, and you could give an evolutionary explanation of our attraction to compassionate ethics—but those causal explanations are not going to help you decide between them.

HRP: You say there don’t have to be ethical objects to talk objectively about ethics, and I take it that what makes our ethics objective is what we were talking about earlier—warranted assertability and what’s rational to accept.

Putnam: What’s rational to accept, *given* certain interests. And I think those interests, including the interest in human flourishing, the Scanlonian interest in giving one another reasons, and the interest in equality, generally support one another. There are of course cases where they conflict. The reason that they become a part of a single ethical package is partly historical. The interest in equality becomes a more powerful interest when we begin to have constitutional democracies. It’s no accident that equality becomes thematized so much at the time of Kant—this is precisely the time that constitutional democracy (or at least constitutional monarchy) is on the historical agenda. But I don’t think that fact entails cultural relativism, because science also has a cultural history, but that doesn’t diminish its objectivity. I’m not a utilitarian, but most people are happier not living in a tyranny; and utilitarianism is a quick and dirty—too quick and too dirty—way of responding to the question “which makes one way of living better than another?”

HRP: You said that what’s rational to accept is conditional upon certain interests, but I suppose you don’t want to say that one of them will trump the others. They’re pluralistic on a plane, or something like that.

Putnam: They’re so interlocked. It’s so easy to write a book deriving $n-1$ of them from the n th [*laughs*]. And of course that game has been played and will go on being played. I prefer to see each of them having a somewhat different history, but because they support one another, and especially in conditions of democracy

and mass public education, that they have come to be virtually a single package. But—and this is a Habermasian point—it’s important that when people were denied some of them in the past, people had to come up with empirically false claims to support denying them. So it’s not *simply* arbitrary. Thus, for example, the long, long inequality of women was justified by absurd claims, such as Aristotle’s claim that women don’t have active *nous*, the highest, intellectual form of *nous*. Similarly, racial oppression was justified by false claims.

HRP: So that’s what happens when they jive, but when they conflict, let’s say, do you have a pragmatic maxim for how those kinds of conflicts should be resolved?

Putnam: Try to be intelligent [*laughs*]. No, there is no decision algorithm. That’s something Kant said: there is no rule which will not give bad results if conjoined with universal idiocy. The one thing there’s no rule for is good judgment. Here he uses a nice feminist term: “mother wit.”

HRP: I’d hate to ask an overly general question, and it might be unfair, but I think people would appreciate hearing an answer from such a distinguished philosopher. Where do you think philosophy is going in the current century, and where do you think it should go?

Putnam: One theme in my work is the idea of a philosophy that respects science and takes science seriously, but doesn’t have a positivist or otherwise ideological picture of what science is, and that doesn’t think that philosophy can be reduced to science. Obviously that’s the way I think philosophy should go, and I’m optimistic that there are probably more people who think that way now than there were when I started. The other main theme is that objectivity, truth, and warranted assertability are not restricted to science; values judgments and descriptions of facts are “entangled,” to use a term I learned from John McDowell, and the objectivity of science is connected with and presupposes the objectivity of values.

HRP: Can you speak more about this distinction between a scientific view, or an ideologically motivated conception of science, as opposed to science itself? I take it you have in mind something like Bernard Williams’ “absolute conception of the world.”

Putnam: Well, I also have the positivists in mind. I was just re-reading the first volume of the *Minnesota Studies in the Philosophy of Science*. There’s a long essay at the beginning by Herbert Feigl, in which he says that positivist philosophy of science has become more realistic, but we have to insist on the sharp line between the analytic and the synthetic, we have to say the meaning of a term is the *rules* of its use, and so on. There was a very strong feeling among the positivists that they had told us exactly what science is—they had married Bertrand Russell’s romance with mathematical logic with classical empiricism. That’s, in the wide sense, an ideologically driven concept of science.

Bernard Williams was a physicalist who knew no physics. I loved him, and he was a brilliant philosopher, but his admiration for physicalism was in inverse proportion to his actual knowledge of the physical sciences.

HRP: But it seems surely no one anymore is a positivist in *that* sense.

Putnam: I suppose you could find one somewhere [*laughs*].

HRP: So do you think this ideologically driven notion of science has dropped out, or do you think it's taken new form?

Putnam: Well, it may reappear. It may certainly reappear. It's always premature to say that a mistake in philosophy has been defeated once and for all. If someone wants to try and show that the program can be carried through, they should try. We learn from our failures in philosophy. Right now, I would say one of the greatest dangers is reductionism, which often blinds people from considering more sophisticated alternatives—take evolutionary psychology, for instance. I don't think that it's illegitimate, and I think there's a lot to be learned from it, especially when the evolutionary psychologist, as it happens in the best cases, can suggest consequences of his theory that can be tested now. Those are the exciting cases. But in the worst case, it becomes Just-So Stories. And again, the problem with AI ["Artificial Intelligence"] was that there was so much oversell. There we tremendously exaggerated claims both about what had been achieved, and what would be achieved in the next five or ten years.

HRP: What do you have in mind when you say "sophisticated alternatives to reductionism"?

Putnam: In general, one can study how A influences B, or how A depends on B, without thinking that that's the whole story.

HRP: So essentially you mean tampering the stringent requirements of classical reductionism—bridge laws, lawful covariance, and such?

Putnam: That's right.

HRP: One picture that emerges from this view of reductionism, and indeed the foregoing conversation, is a skepticism about theories of meaning and theories of intelligence, for all the reasons we've discussed. Jerry Fodor put it in a review that he took you to be denying that meaning is a natural kind. Is that a fair characterization of your views?

Putnam: No, I don't think I've ever said that. I think meaning *might* be, in a sense, a natural kind—in the way that biological kinds are natural but don't have sharp boundaries. This is what Ernst Mayr taught us all: yes, species are natural kinds, but it's sometimes arbitrary when you say here is one species and here are two.

If meanings are natural kinds, they're much more like biological kinds than, say, chemical kinds: they're *historical* natural kinds. Now my old friend Noam Chomsky—we've been friends since college—has written that there are no such things as languages, for example, because there are only criteria for idiolects but not languages. And I would say, sure, it's arbitrary where you put the precise borders of the French language, for example. There are so many dialects of Dutch, and yeah, there's not a sharp line there. But it's really scientism in the bad sense to say we can only study what can be mathematized—we'll only look for algorithms to describe what goes on in the brain when it uses certain idiolects. In fact, Noam even rejects talk of reference, because it's a hopelessly unscientific notion, so just forget it. Well, no! The world won't let us! [*laughs*] It's very important to us what people are referring to when they use a term. So I would say that it's not that there are no natural kinds here, but that explanations have to seek their own level. The standards of explanation of one science can't just be simply transported to a different science.

I think there's a lot to be done in the study of meaning. My own prejudices are that I still think that lexical semantics is sadly neglected. There are some very good exceptions—mostly on the West Coast. There's something about the East Coast, maybe it's too close to MIT [*laughs*], that they talk less about the meanings of words. But I think it's very important to study the meanings of words.

HRP: That's an interesting division between you and, say, Davidsonian semantics. Why such an interest in lexical semantics?

Putnam: Well, that's how you learn a language, to begin with. You learn to use words!

HRP: You've written on Jewish philosophy recently. What do you take from it, and what's the interest?

Putnam: I think for me, as a religious person and as a naturalist, that means that Judaism can be a form of spirituality—but not necessarily a theory about immaterial, supernatural objects. And spirituality interests me both because I think I experience a form of spirituality that the Jewish tradition makes available to me, and I don't think that spirituality is something that's going to go away. I think Richard Dawkins has this simplistic division of the world into fundamentalists, who believe in the literal truth of their local myth, and smart people like him who are hard, scientific types [*laughs*]. And recently, he started admitting that there are more liberal types—but that seems to be a late afterthought. But that's just not the way human life is. I think religion can either make you do terrible things. Dawkins was right about that, although I'm not sure at all that, say, the Thirty Years' War was really a war fought between Catholic states and Protestant states—how come Catholic states were on opposite sides of the Thirty Years' War, and how come the main Catholic general was a Protestant from Sweden? You could probably make a stronger case that it was an imperialist war, and you had to tell your local population *something* about it, so you invoke the church. Just as

we invoke democracy with equal fraudulence. But religion has led to fanaticism and other evils at times, without question.

My affection for Jewish philosophy fits in, I suppose, with my general sense that we can't make *everything* scientific—that philosophy is closely related to the sciences, but philosophy also has a humanistic side. And I have long felt, as Kant did—this is something that the editors of one of my Harvard volumes pointed out—that philosophy needs both a humanistic conception of what it's doing and a scientific conception of what it's doing. It's always been the case that what's lasting in philosophy arises from the interaction between the scientific side of philosophy and the humanistic and ethical side of philosophy.

Regarding the part of philosophy that is *more* than just science, I don't think it can or should hope to find final answers to age-old questions, or even to remain content with those questions as they were traditionally formulated, but I think there are certain questions in ancient and medieval and early modern philosophy that we can recognize as being continuous with questions we're asking today—for example, why is there a difference between right and wrong? How come there is such a thing as mathematical truth? These are certain questions that won't go away. Although their verbal formulation may go away, something like them always comes up in their place. ϕ

Notes

¹ A good account of the Hilbert Problems and their solvers is Ben Yandell, *The Honors Class* (Natick, Mass.: A.K. Peters Ltd., 2002). Hilbert's 10th Problem was to give a decision method for determining whether an arbitrary diophantine solution has a solution. The "negative solution" referred to was a proof that the decision method Hilbert asked for does not exist.

² The film (a one hour documentary) is available from Zala Films: www.zalafilms.com/films/juliarobinson.html.

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