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Thick and thin methodologies in the history of economic thought

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Outsiders make the same complaint about philosophers as they do about economists, saying, These writers thin down the question so. And so they both do. The economist thins the question of the good society right down to matters of price and marginal cost. The philosopher thins the good argument right down to matters of modus tollens and infinite regress. Precision comes from the conversational thinness, as does employment and other goods. But even after such achievements, we should not be surprised if outsiders want to get back to the main and fatter point.

The trouble with using Karl Popper's thinking for a history or methodology of economic thought is not mainly some flaw in its technique, though Daniel Hausman has made the pervasiveness of the flaws clear. The main problem, even in this the richest of philosophies, is its thinness. Rich as Sir Karl's thinking is, supplemented by Lakatos, elaborated and applied with wonderful ingenuity by their followers, it looks thin beside the actual conversation of science. A conversation begun in the primeval forest, as Michael Oakeshott once said, extended and made more articulate in the course of centuries, is probably not going to fit easily into a few lines of philosophy. Or rather, since the issue is empirical, it might–it might be that a philosophy could describe well what goes on in the conversation of science–but it hasn't. One can imagine a world, perhaps, in which the growth of knowledge was interestingly philosophizable. But it doesn't seem to be our world (cf. Rorty, 1982, p. xiv).

A methodology of economics "based" (that hopeful word) on philosophy, especially on philosophy as construed in the English-speaking world, is too thin to work. Such a remark is not to be taken as anti-intellectual, antirational, antiphilosophical, or even antianalytic. Thinking is good, even when thin, and so is thinking about thinking. No one wants to abandon first-order predicate logic, even though it might not be a complete model of sound thinking. Nor is the remark to be taken as one of those sneers at methodology, the sort that grace the exordia of methodological papers by Paul Samuelson and George Stigler. Thinking about thinking about thinking is good, too.

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Yet even those of us who, from time to time, make use of philosophy of science complain about its thinness. Roy Weintraub, for example, complains rightly that Popper reduces the rich conversation of empirical work down to a falsifying "fact." Lakatos's philosophical work was an extended complaint about the lack of thickness in Popper's work, as Popper's was a complaint about earlier and still thinner philosophies of science. None of it works. Mark Blaug, J.J. Klant, and Lawrence Boland accept Lakatos's program, the "rational (which is to say, philosophical) reconstruction of research programs," as what methodology should do, but strain at its limits when applying his program to real work in economics. Boland, for one, skirts the edge of an economic literary criticism (e.g., 1982, pp. 116–17). And Weintraub notes in *General Equilibrium Analysis: Studies in Appraisal* (1985, p. 142) that his case study "raises several other problems that rest uneasily in a Lakatosian bed."

The thinness of the philosophy comes from the thinness of the question it asks. The question in a rational reconstruction of a piece of science is: Does the discourse fit, say, a Lakatosian model? What is the hard core, the protective belt, a typical negative heuristic? Can it be made to lie down on the bed, with suitable trimming at head and feet?

The question will strike the outsider as odd. A study that verifies or falsifies the fit of such a simple notion as sophisticated falsificationism to a part of economics does not ask very many questions. The one question it does ask would not strike a working scientist as interesting. At the end of the day, you are led to ask what has been accomplished.

Consider again Roy Weintraub's recent work, the brilliant imitation of Lakatos just mentioned and his elegant paper for this conference, "The Neo-Walrasian Research Program Is Empirically Progressive." All right, suppose that by the Lakatosian definition the neo-Walrasian program is empirically progressive. (Weintraub certainly persuades on the point: His work exhibits precision and candor well beyond the call of duty in intellectual history.) But what follows? What at the end of the day has been accomplished? We are now persuaded (set aside the problem of induction in talking about the problem of induction) that neoclassical economics can be rationally reconstructed to correspond with a pattern adumbrated by a certain philosopher. Well, so what?

The question is pragmatic, but not in a vulgar sense. It will be perfectly satisfactory if the cash value of the Lakatosian categories shows up merely in their value for further thinking: for setting economics in context, for making economists more self-aware, for telling persuasive stories about the history of economics, for understanding why economists go on as they do. Among professional intellectuals these should count as

good reasons. There is no vulgar demand here for "better economic predictions" from the philosophy, or some market test.

This is fortunate, since philosophical formulas for science have failed to yield vulgar cash rewards in other fields. The lack of correspondence or coherence between the history of science (as written over the past couple of decades) and the philosophy of science (as thrown into confusion over the past couple of decades) is epistemologically striking. Near enough, the philosophy of science has been falsified. Though working scientists will occasionally use a philosophy for a rhetorical purpose, no one seems actually to have carried out a Baconian program, much less a Popperian or Lakatosian program.

But whether there is any practical payoff or not, professional intellectuals can reasonably require that ideas have at least intellectual consequences. If you explain that the orbit of the moon arises from the "orbital character" of the moon you have a handsome turn of phrase, applicable to other moons as well, but not rich in consequences and not answering human questions. If, on the other hand, you explain that the orbit has to do with $F = ma$, the consequences are many, answering questions that people might ask: Why is a moon like an apple? Where did the moon come from and where is it going? How do you get to the moon from here?

The Lakatosian character of some piece of economics has no consequences. It does not answer a question that an economist, or even a non-Lakatosian philosopher, would ask.

The question it does ask is one of the nature-of questions that Popper explicitly spurned. Problem situations, not natures, he said, are the proper subject of science. Popper and Lakatos have emphasized repeatedly that new questions – in other words, a continuity in the conversation – characterize progressive science. One is led to ask: Is the program of applying Popperian or Lakatosian or other philosophical ideas to the history of economic thought itself empirically progressive? What is the problem of which the Methodology of Scientific Research Programs is a progressive solution?

The answer seems to be that it is considered important for economics to be adjudged "empirically progressive" (in Lakatos's sense), just as a little earlier the talk of economists was abuzz with the importance of Popperian falsifiability; and before that of Bridgemanian operationality; and before that of Millsian methodicalness; and before that of Baconian inferentiality. Around 1980 the task of the Lakatosian methodologist or historian of thought was to check out this Lakatosian virtue in economic science. Yet Lakatos might alternatively have called a science "scientific" or "free of false consciousness" instead of "progressive"; and these, without further argument, would amount to synonyms for "Lakatos-beloved" (compare Euthyphro, Stephanus 10e–11b). But why, in turn, would we care that economics would be beloved of Imre Lakatos?

We might indeed care about economics being "empirically progressive" if "progressiveness" in Lakatos's sense were shown historically to correspond to progress. But this is doubtful, and is indeed explicitly denied by both Popper and Lakatos. They do not pretend to give persuasive histories of how science actually did progress. Theirs is rational, not historical, reconstruction. As I just said, if their appeal rested on a claimed fit to science, they would be in serious trouble with present-day historians and sociologists of science, not to speak of Paul Feyerabend, Michael Polanyi, and Stephen Toulmin.

To take another possibility, we might care about Lakatos-belovedness if a "progressive" scientific research program could be shown to lead to Truth. But it is reliably reported that there is a problem with Truth. The problem is not with lowercase truth, which gives answers to questions arising now in human conversations, requiring no access to the mind of God: On a Fahrenheit scale, what is the temperature in Iowa City this afternoon? On a historical scale, what is the quality of the President's decisions in foreign affairs? You and I can answer such questions, improving our answers in shared discourse.

The problem comes when trying to vault into a higher realm, asking whether such and such a methodology will lead ultimately to the end of the conversation, to the final Truth about economics or philosophy. This is the question asked by Plato and reiterated by Descartes and Bacon. The modesty of the sophist Protagoras, that man is the measure of all things, was not pleasing to Plato, Descartes, and Bacon: "For it is a false assertion that the sense of man is the measure of all things. On the contrary, all perceptions as well as of the sense as of the mind are according to the measure of the individual and not according to the measure of the universe. And the human understanding is like a false mirror, which, receiving rays irregularly, distorts and discolours the nature of things by mingling its own nature with it" (Bacon, 1965, ch. XVI).

The "measure of the universe," however, cannot be taken direct; it can only be taken from the sublunary mirrors we have. Questions such as "What will economics look like once it is finished?" are not answerable on this side of the Last Judgment. Wolfgang Pauli used an economic metaphor to scold physicists for anticipating the physics that would arise once judgment was ended, claiming "credits for the future." Economists, with their dismal jokes that lunches are not free and $500 bills do not lie about unclaimed, should have no trouble seeing that little

can be hoped for prescience in such matters. The problem is that it is precisely prescience, knowing before knowing.

What then? If methodology – Popperian, Lakatosian, or whatever – is not a guide to the history of thought or to the completion of science, one may ask what it is a guide to. What, really, is the philosophy of economic science about? The answer appears to be that it is about morality. And there is no sin in this.

Popper and company are not so much concerned to tell a persuasive story or lead a march to the godhead as to persuade scientists to be good. The sneering and name calling and good-guy identifying and horrified-viewing-with-alarm that characterize methodological discourse fit a program of goodness. We berate and banish the criminal, the bad person. The rules of the game give us a way of classifying scholars as citizens or as thought criminals. If we can tag the nasty descendants of Nietzsche as "irrationalists," for example, we can shut them up, or at any rate protect innocent students from their words. Again, if we can identify the Freudians and Marxists as aliens, we can conveniently deport them from our open intellectual society. (An unhappy side effect of such a policy, strictly enforced, would be the deportation of most economists forthwith, pleading from the back of the truck their falsification credentials.)

A moral purpose explains the strength of feeling against John Dewey, Milton Friedman, Richard Rorty, and other harmless pragmatists. Moral and political purposes are not always denied by advocates of the received view. At certain moments they will admit to them. In 1938, for example, before it was fully received, the father of us all wrote thus in favor of neopositivism in economics:

> The most sinister phenomenon of recent decades for the true scientist, and indeed to Western civilization as a whole, may be said to be the growth of Pseudo-Sciences no longer confined to hole-in-corner cranks . . . but organized in comprehensive, militant and persecuting mass-creeds. . . . [Testability is] the only principle or distinction practically adoptable which will keep science separate from pseudo-science. (Hutchison [1938], 1960, pp. 10–11)

One can agree with a purpose here of attacking Nazism without agreeing that some method of Science will achieve it. One can argue in fact the other way around. After all, the Nazis were gloriously Scientific in their experiments. Victorian and even British Science, with its elaborate ceremony of testability (most skillfully practiced by the psychologist Sir Cyril Burt), was a major source of racist theories (cf. Gould, 1981). And on the other side it is not easy to blame, say, Jewish numerology or Gypsy legerdemain for the rise of Nazism.

There is little doubt, however, that a desire to defend liberal values against the barbarians feeds methodology. The unargued moral and political message in positivism and its offshoots explains perhaps the fascination with the demarcation problem. When you consider it, it's not clear why it should matter whether economics is or is not a science. Of course, there are certain material advantages: a place under the National Science Foundation's tiny budget for social sciences; a Nobel Prize in Economic science; a few memberships in the National Academy of Science. The label gives economists license to sneer at sociologists and philosophers. But the main reason for demarcation seems to be that astrologers and parapsychologists are thought to be bad people, touchie-feelies from Santa Monica perhaps. It is taken as given that such intellectual criminals, violators of the rules of the game, are not to be tolerated in the open society. The appeal of methodology is moral and political.

These moral and political fears of the methodologists are not scrutinized. If they were, they would take a more realistic form. As Bruce Caldwell notes wisely, "The fear of anarchy [by which he means "chaos," the war of all against all], or of a totalitarian response to anarchy, cannot be based on a correct perception of science as it is currently practiced in free societies" (1985, p. 5). To solve the German problem between the wars, or the Slavic problem after them, some rigid rules might make sense, the more rigid the better – the better to defend a conversation from the state. When the party man in charge of the scientist's soul detects some deviation, the scientist can pull out a sheaf of computer paper and ask mildly, "Yes, perhaps I have made a mistake; please show it to me, comrade." But this ploy (which is more than an armchair possibility) is not nearly so sweet in an open, plural, and pragmatic society. The appeal to the character of the Scientist in such a place more often supports authoritarianism in the Department of Defense or the National Aeronautics and Space Administration. The barbarians against which philosophical methodology should fight are inside, not outside, the gates.

It would be good to see the defenders of the fact–value split scrutinize their moral agenda. They might find enlightenment in the long conversation among philosophers about virtue. If they knew that their methodologies were about virtue, they could start with the Old Testament and the Gorgias and work forward.

The psychological literature on moral development is worth reading, too. This very conference and the wider discussion beyond it of the rules of the game have notably few women participants. Carol Gilligan, in *In a Different Voice: Psychological Theory and Women's Development* (1982), quotes Janet Lever's study of the games of boys and girls – "[B]oys were seen quarreling all the time, but not once was a game terminated because of a quarrel" – and explains that "it seemed that the boys enjoyed the legal debates as much as they did the game itself, and even marginal players of lesser size or skill participated equally in these recurrent squabbles. In contrast, the eruption of disputes among girls tended to end the game" (p. 9). The parallel with methodological disputes is suggestive. Gilligan reports on Piaget's observation of "boys becoming through childhood increasingly fascinated with the legal elaboration of rules . . ., a fascination that, he notes, does not hold for girls" (p. 10). The girls stressed community, conversation, solidarity, and the other nonrule values of those known affectionately as the "new fuzzies" (Rorty, 1987). Arjo Klamer and I can be viewed therefore as presuming to bring a feminine perception to the matter.

I suggest, with Klamer, that the good that lies behind methodological thinking is the goodness of community, solidarity, openness to ideas, educated public opinion, and a better conversation of humanity. By their moral fervor the methodologists reveal their values. Their values are fine, and not much different from those of the terrible fuzzies they fear.

The word is *sprachethik,* speech morality, the ethics of conversation. That the word comes from a hive of Marxist fuzzies in Frankfurt-am-Main should not be alarming, for it is liberalism incarnate: Don't lie; pay attention; don't sneer; cooperate; don't shout; let other people talk; be open-minded; explain yourself when asked; don't resort to violence or conspiracy in aid of your ideas. These are the rules adopted by the act of joining a good conversation. Socratic dialogue – flowing first from a pen devoted to finishing conversation – is the model for Western intellectual life. An American philosopher put the point well. What is crucial, writes Amelie Oksenberg Rorty, is "our ability to engage in continuous conversation, testing one another, discovering our hidden presuppositions, changing our minds because we have listened to the voices of our fellows. Lunatics also change their minds, but their minds change with the tides of the moon and not because they have listened, really listened, to their friends' questions and objections" (1983, p. 562). Good science is not good method but good conversation.

We know when conversations are going well among our own intellectual friends. Most economists would agree, for example, that the conversation about international trade since 1950 has been through a bad stretch, relieved only temporarily by a burst of creativity fifteen years ago on the financial side. They would agree, too, that economic history improved radically after 1958 and has flourished ever since. Working economists do not need the advice of a philosopher – least of all an economist in philosopher's clothing – to know when things are going well or badly in their neck of the woods.

It is a crucial point about the conversational view of intellectual life that conversations overlap. You are almost as sure about neighboring conversations as about your own, which is what research panels, editorial boards, and tenure committees depend on. If good conversation is maintained in one part of the conversation of humanity, the overlap provides standards for others. The overlap of the overlap spreads good standards, such as care in reading earlier work (and bad habits, too, such as the mechanical use of statistical significance). This free market – not the central planning proposed in the official methodologies – gives the only promise worth having that the economy of intellect will continue to run as well as can be expected.

The argument replies to certain monists, who insist that other people stick to what they call "standards." They exempt their own conversation from such rhetorical scrutiny: They reckon that Plato's beard or Descartes's cogito will suffice for serious men. The alleged standards of philosophical empiricism (distinct from empirical work, which no reasonable person speaks against) have persuaded some scientists to spurn whole classes of evidence: Economists have spurned surveys, psychologists the evidence of their own minds, and policymakers the moral reasoning necessary for the making of policy. It is hard to take the claim of philosophically imposed standards seriously. The real standards, after all, reside where they should, on the lips of men and women of science conversing together.

The methodologists, then, accept the *sprachethik* of the fuzzies even as they attack what they think it is. So also more directly do Bruce Caldwell, Husain Sarkar, and other pluralists. As human conversationalists they can hardly avoid doing so. The methodologists are drawn thus into the "cultural sensitivity" of which Klamer speaks, though they do not like it and will not admit it and grow cross when it is mentioned. While having a culture-bound conversation about whether knowledge is culture bound, they insist that conversation is not culture bound. They think they can assume an Archimedean point with which to lever the world of conversation. They do not want rhetoric, but rules of perfect knowledge for all time. They are not discouraged by the failure of 2,500 years of the epistemological conversation to find a single one.

The question is how to converse about this culture-bound conversation of humanity. We know how to make the conversations lie down on the guest beds of philosophers, but agree that the result is unhelpful. Science doesn't fit well on a bed of science-is-modus-tollens or science-is-positive-heuristics. It has to be trimmed to fit.

Happily, there exists alternative thinking about how to do the thinking, thick and rich. It is called the humanities. The humanistic tradition of the West can be used to understand the scientific tradition. What historians and methodologists of economic thought mainly do anyway, without knowing it, is literary criticism. Sophisticated criticism is merely understanding how the texts of economists produce their effect, as one criticizes poetry. Criticism in this sense is neither "assault" nor "ranking." It is not a murder trial or a beauty contest – it is not, as Northrop Frye puts the point, "the odious comparison of greatness," a "pseudo-dialectics, or false rhetoric," "an anxiety neurosis prompted by a moral censor" that has "made the word critic a synonym for an educated shrew" (1957, pp. 24–7). The usual philosophical criticism lets in the shrewishness: thin, bad-tempered, superficially judgmental. By the standards of good literary criticism, a philosophical criticism – Lakatosian, say – seems thin and harshly normative, unattractive stuff. The literary model can lead to a better way of examining the conversations of economists.

Its merits show up when placed side by side with some of the other alternatives to philosophical criticism. They are all at least as thick as philosophy. For example, the history of economic thought can be written as biography. George Stigler has attacked this tradition persuasively, though doubtless it will survive even his pen (1976). The biographical approach is certainly thick: One can know all about Ricardo's businesses and Keynes's love affairs, yet still have more questions to ask. The conversation may be irrelevant to matters of import, as Stigler would argue, but there is at least nothing thin about it. This probably explains its vitality in the face of much lofty methodological sneering.

Another thick alternative is the Whiggish theory, advocated by Stigler and practiced by Mark Blaug, that the progress of science can be viewed as successive approximations to the right answers. Eventually we'll get it right. In the meantime, we can look on the history of the field as a dawning of enlightenment. This is history of science as examination question: Quick, Ricardo: would it matter to your argument if labor was only 93 percent of costs? Quick, Malthus: How would you draw your theory in the wage–population plane? Most history of thought taken seriously by most economists (Schumpeter a while ago, Blaug et alia nowadays) takes this line and has done great service. Though more useful for enriching economic thinking, the Whiggish approach is not so thick as the biographical. As with slow students disfiguring their examination scripts, one runs out of patience with the errors of the past.

Another thick alternative to philosophical criticism has again been advocated by the polymorphous Stigler. It is to turn economics on itself and view the history of the field as itself a consequence of economic forces. Here again, as America's leading vulgar Marxist, Stigler shows a characteristic openness to left-wing thought. The program is as rich as is empirical economics (Diamond, 1984; 1987). It amounts to an especially narrow version of the next and better alternative to philosophical criticism.

The better alternative is sociology of science, advocated for economics most prominently by A.W. Coats (1984). It is attractively thick. There is little limit to what one can ask about the sociology of the economics profession in England c. 1900 or the sociology of journal editing c. 1987. Furthermore, it is relevant to what we wish to know: If knowledge is social, as it is, the growth of knowledge will be a social growth.

I should like to argue at the end, however, that the thickest parts of the so-called Strong Program in the Sociology of Science overlap with a specifically rhetorical criticism. Sociology and rhetoric are one.

An illuminating example of the Strong Program is a book by Harry Collins, *Changing Order: Replication and Induction in Scientific Practice* (1985). It is about physics; the Strong Program has not had much of a trial in economics yet. Collins, a sociologist at the Science Research Centre at the University of Bath, calls his approach "sociological," which is fair enough: Science is social, Collins is a sociologist, and sociological phenomenology has played a part in his thinking. But it is not sociological in the sense that earlier sociologists of science, such as Robert Merton, would recognize. He does not, for example, collect biographical data on the scientists, though he could have done so with less trouble. He did not because the scientists are not his subjects.

His subjects are controversies, debates, words, argumentative ploys – that is, the rhetoric of science. The quantified gossip that constitutes sociology of science in the Mertonian or Stiglerian vein is missing. This is notable because Collins has elsewhere done Mertonian tasks (Collins and Pinch, 1982). What interests Collins in *Changing Order* is not the resumés of his people but the course of the debate among them. He speaks repeatedly of the "argumentative strategy" of this or that scientific remark. He never attributes a move in the argument to party or passion. Toward the end of the book he rejects the usual social correlates in favor of a definition of his subject that focuses attention on debate:

> The set of allies and enemies in the core of a controversy are not necessarily bound to each other by social ties or membership of common institutions. . . . If these enemies interact, it is likely to be only in the context of the particular passing debate. This set of persons does not necessarily act like a "group." They are bound only by their close, if differing, interests in the controversy's outcome. (1985, p. 142)

Collins treats the debate among physicists about gravity waves, for example, as just that: as a debate, showing how one or another rhetorical move led to the result. At its turning point, for instance, the chief proponent of gravity waves, Joseph Weber, "in accepting . . . electrostatic calibration . . . accepted constraint on his freedom to interpret results" (p. 105). Collins notes that Weber did not have to accept the calibration (which itself, by the way, is a rhetorical turn common to many fields: the selection of a quantitative standard). It was a rhetorical choice. But having made it, Weber was constrained by rules of debate, rules that can themselves be studied and partially understood, and have in fact been studied and partially understood since the time of the Greeks.

The varied rules of human debate, not godlike tests, decide the outcome. "It is control on interpretation which breaks the circle of the experimenters' regress [Collins's phrase for Duhem's dilemma], not the 'test of a test' itself" (p. 106). That is to say, it is rhetorical considerations, the workings of a human conversation, not mechanical applications of rules within a closed system, that end a scientific debate. Scientists do not commit a crime when they argue beyond the constricted realm of formal logic. "Scientists do not act dishonourably when they engage in the debates . . . ; there is nothing else for them to do if a debate is ever to be settled and if new knowledge is ever to emerge from the dispute" (p. 143). It is not the logic of inquiry that allows scientific progress, but the rhetoric of inquiry (cf. p. 153, note 5).

I am asserting that Collins and other observers of scientific controversy contribute unawares to the rhetorical tradition. Rhetorical criticism is the thickest approach. It draws on an immensely long tradition from the Sicilian sophists to the present, running parallel to philosophy, though spurned by philosophy in every age. At present the tradition lives in law schools, in the literary world (Booth, 1974; Fish, 1980), and in writings on argument emanating from specialists in rhetoric (Scott, 1967). Occasionally it can be seen half-conscious in a philosopher gone wrong (Toulmin, 1958; Steiner, 1975; Rosen, 1980; Rorty, 1982; Walton, 1985).

It is not "mere" rhetoric, and not an ornament to be distinguished from the substance of argument. It is rhetoric in the ancient and honorable sense:

> the art of probing what men believe they ought to believe, rather than proving what is true according to abstract methods . . . , of discovering good reasons,

finding what really warrants assent, . . . of discovering warrantable beliefs and improving those beliefs in shared discourse. (Booth, 1974, pp. xiii, xiv)

It is, in brief, the art of argument, argument not confined to syllogism or meter reading. It includes arguments from pure logic and simple measurement, to be sure, but includes the other 90 percent of scientific argument, too – the ubiquitous "models" of scientific thinking, for example, which are arguments from analogy, and the ubiquitous appeals to the reputation of the scientist, which are arguments from authority.

Scientists, even economists in the grip of philosophy of science, argue with all the means their culture makes available, honestly if they have the will and thoroughly if they have the energy. Their official rhetoric does not admit this, because the officials have been enchanted since the time of Plato with a thin quest for certainty. They have hidden most of the argument, uncertain as it is, in hallway conversation and conference room retort, in what is implied rather than stated. An economic criticism and literary sensibilities can bring economic arguments out into the light.

The rhetorical concern, in sum, is how we really do convince each other, not "what is true according to abstract methods." The point is that it is also the concern of the scientists; they couldn't care less what is true according to abstract methods; they want to persuade, to bring a particular debate to a conclusion. Scientists in all fields, psychology and economics as much as physics and biology, talk incessantly about rhetorical matters. They talk as though engaged in a debate at the Oxford Union or a case at law or an important business judgment. The scientific conversation is not governed by rules convenient for a pocket-sized card. It is a thick and complex rhetorical matter. It is a matter of listening, really listening, to what our fellows say; then answering, really answering.

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