In Defense of Extreme Rationalism: Thoughts on Donald McCloskey's *The Rhetoric of Economics*

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The Relativism of Hermeneutics and Rhetoric and the Claims of Rationalism

For some time, the philosophy establishment has been under attack by the likes of Paul Feyerabend, Richard Rorty, Hans G. Gadamer, and Jacques Derrida. A movement of sorts that has already won over numerous members of the philosophy profession is steadily gaining ground, not only in such soft fields as literary criticism and sociology, but even in the hard natural sciences. With Donald McCloskey's *The Rhetoric of Economics* (Madison: University of Wisconsin Press, 1985), this movement is ready to invade economics. Yet, it is not only the orthodox, neoclassical Chicago economist McCloskey who preaches the new dispensation; there is also G.L.S. Shackle, and at the fringes of the Austrian school of economics are Ludwig Lachmann and the George Mason University hermeneuticians who lend support to the new creed.

However, this creed is not entirely new. It is the ancient tune of skepticism and nihilism, of epistemological and ethical relativism that is sung here in ever-changing, modern voices. Richard Rorty, one of the outstanding champions of the creed, has presented it with admirable frankness in his *Philosophy and the Mirror of Nature.*[1] The opponent of the new old movement is *rationalism* and, in particular, *epistemology* as a product of rationalism. Rationalism, writes Rorty:

> is a desire for constraint—a desire to find "foundations" to which one might cling, frameworks beyond which one must not stray, objects which impose themselves, representations, which cannot be gainsaid. (p. 315)

> The dominating notion of epistemology is that to be rational, to be fully human, to do what we ought, we need to be able to find agreement with other

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Review of Donald McCloskey, *The Rhetoric of Economics* (Madison: University of Wisconsin Press, © 1985 by the Board of Regents of the University of Wisconsin System).

> human beings. To construct an epistemology is to find the maximum amount of common ground with others. The assumption that an epistemology can be constructed is the assumption that such common ground exists. (p. 326)

However, Rorty claims that no such common ground exists: hence the false idol of rationalism must fall and a "relativist" position termed hermeneutics must be adopted.

> Hermeneutics sees the relations between various discourses as those of strands in a possible conversation, a conversation which presupposes no disciplinary matrix which unites the speakers, but where the hope of agreement is never lost so long as the conversation lasts. This hope is not a hope for the discovery of antecedently existing common ground, but *simply* hope for agreement, or, at least, exciting and fruitful disagreement. Epistemology sees the hope of agreement as a token of the existence of common ground which, perhaps unbeknown to the speakers, unites them in common rationality. For hermeneutics, to be rational is to be willing to refrain from epistemology—from thinking that there is a special set of terms in which all contributions to the conversation should be put—and to be willing to pick up the jargon of the interlocutor rather than translating it into one's own. For epistemology, to be rational is to find the proper set of terms into which all contributions should be translated if agreement is to become possible. For epistemology, conversation is implicit inquiry. For hermeneutics, inquiry is routine conversation. (p. 318)

What Rorty terms hermeneutics, McCloskey calls rhetoric. In *The Rhetoric of Economics*, he attempts to persuade us that in economics, just as in any other language game that we might play, rationalist and epistemological claims of providing a common ground that makes agreement-on-something-objectively-true possible are out of place. Economics, too, is merely rhetoric. It is another contribution to the conversation of mankind, another attempt to keep a routine going. It exists not for the sake of inquiring about what is true, but for its own sake; not in order to convince anyone of anything based on objective standards, but in the absence of any such standards, simply in order to be persuasive and persuade for persuasion's sake.

> Rhetoric is the art of speaking. More broadly it is the study of how people persuade. (p. 29)

> Rhetoric . . . is the box of tools for persuasion taken together, available to persuaders good and bad. (pp. 37–38)

> [Economics should learn its lesson from literary criticism.] "Literary criticism does not merely pass judgements of good or bad; in its more recent forms the question seems hardly to arise. Chiefly it is concerned with making readers see how poets and novelists accomplish their result. An economic criticism . . . is

> not a way of passing judgement on economics. It is a way of showing how it accomplishes its result. It applies the devices of literary criticism to the literature of economics. (p. XIX)

> [The categories truth and falsehood play no role in this endeavor. Scholars] pursue other things, but things that have only an incidental relation with truth. They do so not because they are inferior to philosophers in moral fiber but because they are human. Truth-pursuing is a poor theory of human motivation and non-operational as a moral imperative. The human scientists pursue persuasiveness, prettiness, the resolution of puzzlement, the conquest of recalcitrant details, the feeling of a job well done, and the honor and income of office. . . . The very idea of Truth—with a capital T, something beyond what is merely persuasive to all concerned—is a fifth wheel. . . . If we decide that the quantity theory of money or the marginal productivity theory of distribution is persuasive, interesting, useful, reasonable, appealing, acceptable, we do not also need to know that it is True. . . . [There] are particular arguments, good or bad. After making them, there is no point in asking a last, summarizing question: "Well, is it True?" It's whatever it is—persuasive, interesting, useful, and so forth. . . . There is no reason to search for a general quality called Truth. (p. 46–47)

> [Economics in particular, and science in general are like the arts;[2] the law of demand is persuasive or unpersuasive in exactly the same way as a Keats poem;[3] and in just the same way as there exists no methodological formula for advancing artistic expression there exists none for advancing economics. Rhetoric] believes that science advances by healthy conversation, not adherence to a methodology. . . . Life is not so easy that an economist can be made better at what he does merely by reading a book. (p. 174)

Surely, after all this one has to catch one's breath. Yet has not rationalism refuted this doctrime time and again as self-contradictory and, if taken seriously, as fatally dangerous nonsense? Books such as McCloskey's may indeed not make life better or easier. But is this not only insofar as one ignores their advice; and would not life in fact be worse if one were actually to follow it?

Consider this: after reading Rorty and McCloskey, would it not seem appropriate to ask "What, then, about their own pronouncements?" If there is nothing like truth based on common, objective ground, then all of the preceding talk can surely not claim to say anything true. In fact, it would be self-defeating to do what they seem to be doing: denying that an objective case can be made for any statement, while at the same time claiming this to be the case for their own views. In so doing, one would falsify the content of one's own statement. One cannot argue that one cannot argue.[4] Thus, in order to understand Rorty and McCloskey correctly, one must first realize that they cannot truly be saying what they seem to be saying. Nor can I here say anything claiming to be objectively so and true. No, their talk as well as mine can merely be understood as contributions to their and my entertainment.

But then, why should they or I listen and be entertained? After all, if there is no such thing as truth and, accordingly, no objective distinction between truth-claiming propositions and any others, then we are evidently faced with a situation of all-pervasive intellectual permissiveness.[5] With every statement just another contribution to the conversation of mankind, anything at all that is said is just as good a potential candidate for my entertainment as anything else. But why bother listening to such permissive, everything-goes talk? McCloskey might reply, "Because your talk or my talk is persuasive." But that will not change much, if anything at all. For according to his doctrine, the categories "persuasive" and "unpersuasive" are not simply other names for "true" and "false." The whole point would be lost if they were. No, he is saying that something is persuasive because it has in fact persuaded; because it has resulted in agreement. To go beyond this and ask, "Well, has one been persuaded of something correct?" would be an entirely inappropriate question. As a matter of fact, regarding any such question, he would have to point out that the very problem of determining whether or not a persuasion was based on correct talk would once more have to be decided on the actual persuasion of having been correctly persuaded; hence, that he is consistent in his rejection of the idea of objective truth; that the idea of breaking out of mere talk and of grounding talk in something that is not again simply talk is fallacious; and that truth then is itself no more than the subjective belief that what one believes is objectively true.[6] But if this is his position, then his talk, persuasive or unpersuasive as it may be, can indeed be no more than mere entertainment. Nor can this statement regarding what it means to talk claim to be objectively true; it, too, can only be meant to entertain.

Hence, it seems the first appropriate question regarding such books as McCloskey's would have to be "Are we being entertained?" Without a doubt, many a reader will reply that he is and McCloskey might then think that he has indeed achieved what he intended. But did he? Or was the readers' feeling of being well entertained only due to the fact that he misinterpreted what he read and understood it as something claiming to be true, which, in fact, it was never meant to be? And would not the reader, once he had realized this, have to change his opinion? For then McCloskey's talk clearly would not fall into any different category from that of a novelist or poet. But as compared with their prose, and in direct competition with any novel or poem written for our entertainment, I submit that McCloskey's book is merely boring and fails miserably in its objective.

Yet, can his book be even *bad* entertainment without still having to be committed indispensably to the notion of a common ground that serves as the basis of objective truth? Rationalism denies that it can. It claims that the notion of truth, of objective truth, of truth grounded in some reality outside that of language itself, is indispensable for talk of any sort, that language presupposes rationality, and hence that it is impossible to rid oneself of the notion of

objective truth as long as one is capable of engaging in any language game whatsoever. For how else could we find out whether someone was in fact entertained by something, or that he was persuaded by it, that he understood or misunderstood what it was that had been said to entertain and persuade, and even further, whether there was something that meant anything at all and so could be understood, rather than merely being meaningless rustling in the wind? Clearly enough, we could not claim to know any of this unless we had a common language with commonly understood concepts such as "being persuaded" or "entertained" as well as any other term used in our talk. In fact, we could not meaningfully claim to deny all this without having to presuppose yet another set of commonly understood concepts. And just as clearly, this common ground that must be presupposed if we want to say anything meaningful at all is not simply one of free-floating sounds in harmony with each other in midair. Instead, it is the common ground of terms being used and applied cooperatively in the course of a practical affair, an interaction. And again, in making this claim, one could not possibly deny that this is so without presupposing that one in fact *could* cooperatively establish some common ground with respect to the practical application of some terms.

Language, then, is not some ethereal medium disconnected from reality, but is itself a form of action. It is an offshoot of practical cooperation and as such, via action, is inseparably connected with an objective world. Talk, whether fact or fiction, is inevitably a form of cooperation and thus presupposes a common ground of objectively defined and applied terms.[7] Not in the sense that one would always have to agree on the content of what was said or that one would even have to understand everything said. But rather, in the sense that as long as one claimed to express anything meaningful at all, one would have to assume the existence of *some* common standards, if only to be able to agree on whether or not and in what respect one was in fact in agreement with others, and whether or not and to what extent one in fact understood what had been said. And these common standards would have to be assumed to be objective in that they would involve the application of terms within reality. To say, then, that no common ground exists is contradictory. The very fact that this statement can claim to convey meaning implies that there is such common ground. It implies that terms can be objectively applied and grounded in a common reality of action as the practical presupposition of language.

Thus, if McCloskey were right and there were indeed no objective truth, he could not even claim to entertain anyone meaningfully with his book. His talk would be meaningless, indistinguishable from the rattling of his typewritter. He would advocate even greater intellectual permissiveness than first thought. Not only would he have to drop the distinction between truth-claiming propositions and propositions that merely claim to be entertaining, but his permissiveness would go so far as to disallow any distinction between meaningful talk and a meaningless assemblage of sounds. For one cannot even claim to

entertain with talk that involves no truth-claim beyond that of being meaningful talk, without still having to know what objective truth is and be able to distinguish between truth-claiming propositions and those statements (for example, in fictional talk) that do not imply any such claim.

And there is more. For how can McCloskey or Rorty reconcile their view of science as mere talk with their own advocacy of a talk-ethic, an ethic described by McCloskey as follows:

> Don't lie [but how could we, if there were no such thing as objective truth? H.H.H.]; 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 and conspiracy in aid of your ideas. (p. 24)

Why should we follow his advice of paying attention to talk and not resorting to violence, particularly in view of the fact that what is advocated here is talk of the sort where anything goes and where everything said is just as good a candidate for one's attention as anything else? It certainly is not evident that one should pay much attention to talk if that is what talk is all about! Moreover, it would be downright fatal to follow this ethic. For any viable human ethic must evidently allow people to do things other than talk, if only to have a single human survivor who could possibly have any ethical questions; McCloskey's talk-ethic, however, gives us precisely such deadly advice of never to *stop* talking or stop listening to others talk. In addition, McCloskey himself and his fellow hermeneuticians must admit that they can have no objective ground for proposing their ethic anyway. For if there are no objective standards of truth, then it must also be the case that one's ethical proposals cannot claim to be objectively justifiable either.[8] But what is wrong, then, with not being persuaded by all of this and, rather than listening further, hitting McCloskey on the head straightaway rather than waiting until he perishes from following his own prescription of endless talk? Clearly, if McCloskey were right, nothing could be said to be objectively wrong with this. (In fact, would one not have to conclude that McCloskey could not even say that anything objective had happened?) He might not regard my act of aggression as a contribution to the conversation of mankind (though we know by now that he could not even objectively claim to know this to be the case), but if the talk-ethic cannot itself be grounded in something objective outside of talk, then if I happened to be persuaded of an ethic of aggression instead, and I ended our conversation once and for all with a preemptive strike, McCloskey could not find anything objectively wrong with this either.

Thus, it is not only *intellectual* permissiveness that is preached by hermeneuticians and rhetoricians, it is total *practical* permissiveness as well—epistemological *and*, as the other side of the same coin, ethical relativism.[9] Yet such relativism is impossible to follow and thus wrong in the most objective

sense of being literally incompatible with our nature as actors. Just as it is impossible to say and mean to say that there is no such thing as objective truth without in so doing actually presupposing objective criteria for the application of terms, so is it impossible to actually advocate ethical relativism. Because in order to advocate any ethical position whatsoever, one must be allowed to communicate rather than be coercively shut up and silenced, and thus, contrary to the relativist message itself, its messenger, in bringing it to us, must in fact presuppose the existence of objectively defined absolute rights. More specifically, he must presuppose those norms of action as valid whose observance makes talk as a special form of cooperation between physically separate talkers possible, while they must also allow everybody to do things other than engage in endless talk; and whose validity must then be regarded as objective and absolute in that no one could possibly ever be alive and talkingly challenge them.[10]

Hermeneutics versus Empiricism—Rationalism against Both Round I

McCloskey's and Rorty's general thesis then, the very thesis that brought them their notoriety, is dead wrong. In fact, McCloskey and Rorty can only do and say what they do because *what* they say is false.

There is certainly much left to be said about rationalism, the age-old opponent of relativism. However, the perennial claims of rationalism remain unchallenged by this most modern, relativist attack: the claim that there exists a common ground on the basis of which objectively true propositions can be formulated; the claim that a rational ethic objectively founded in the nature of man as actors and talkers exists; and finally, the claim, only somewhat indirectly established in the previous argument and still to be substantiated, that one can know certain propositions to be objectively true a priori, (that is, independent of contingent experiences) as they can be derived deductively from basic, axiomatic propositions whose truth cannot be denied objectively without running into a practical contradiction, that is, without presupposing in the very act of denial what is supposedly denied (so that it would be literally impossible to undo the truth of these propositions).[11]

With this fundamental criticism out of the way, what about McCloskey's pronouncements, if for the sake of argument we are willing to ignore that he cannot really claim to say anything? It is not entirely surprising, as will be seen, that the general flaw of the book—its lack of argumentative rigor—also comes to bear here.

The very starting point of McCloskey's argument is marked by a misconception of the problem he faces. For in order to advance the thesis that economists

should conceive of their jobs as keeping the conversation between economists going without ever claiming to say anything true (i.e., without ever supposing that anyone might ever have a decisive, conversation-stopping argument at his disposal), McCloskey would have to direct his argument against and refute the most extreme available opposition. He would have to choose as his target the claims of *rationalism* regarding the epistemological foundations and methodology of economics. And while only accounting for a small minority among today's theoreticians of economics, there surely exist some such dogmatic, doctrinaire, extremist, absolutist (or whatever other depreciating label one may choose) rationalists.[12] The foremost representatives of this persuasion are Ludwig von Mises and Murray N. Rothbard, who, within the general framework of a Kantian or, respectively, Aristotelian epistemology, conceive of economics as part of a pure theory of action and choice (praxeology).[13] Lionel Robbins advances only slightly less uncompromising views, in particular in the first edition of his *Nature and Significance of Economic Science.*[14] And from a very different position within the political-ideological spectrum are Martin Hollis and Edward J. Nell, who in their *Rational Economic Man* propound similar archrationalist claims regarding the logic of economics.[15] McCloskey would have to attack all of them, since they are the most radical conversation stoppers in that they all, despite some important differences, are completely uncompromising in insisting that economics not only can and *does* produce propositions that are objectively true and can be distinguished from propositions that are not, but, moreover, that some propositions of economics are grounded in incontestably true axioms or real (as contrasted with arbitrary, stipulative) definitions, and hence can be given an a priori justification.[16]

However, nowhere in his book does McCloskey attack these various representatives of an archrationalist methodology of economics, nor does he attack anyone else who falls into this camp. Nowhere in his book does he attack, much less refute, the very position that is the polar opposite of his. Robbins, Rothbard, Hollis, and Nell are never mentioned in McCloskey's text, nor do they appear in his bibliography. Nor does Mises' name appear in the bibliography, but it is mentioned twice in the text in support of some of McCloskey's own pronouncements (pp. 15, 65). Yet there is no reference to Mises' extremist, rationalist position. Austrian methodology is only cited in passing and described in a way that would strike anyone only faintly familiar with this intellectual tradition as no more than a naive misrepresentation: "Austrian methodology says: The history of all hitherto existing societies is the history of interactions among selfish individuals. Use statistics gingerly if at all, for they are transitory figments. Beware of remarks that do not accord with Austrian Methodological precepts" (p. 25).[17]

Rather than doing battle with his direct logical adversary, McCloskey chooses to establish his own relativist position through an attack on empiricism-positivism. But knocking down empiricism-positivism is no more than knocking

down a straw man, in that from its downfall, absolutely nothing follows in support of McCloskey's own claims. In fact, all of the previously mentioned archrationalists have leveled much harsher criticism against empiricism-positivism and still apparently did not think that in so doing they would commit themselves to relativism. On the contrary, it is their view that any criticism of empiricism-positivism, if it is one that has any intellectual weight at all, would have to vindicate the very claims of rationalism. Thus, and this is the fundamental misconstruction of his entire argument, McCloskey, given his objective, simply fires at the wrong target and, worse, does not seem to notice.

However, as much as empiricism-positivism may deserve to be intellectually destroyed, McCloskey does not even succeed here. He begins with a description of empiricism-positivism or of economic modernism, as he terms the application of this philosophy to the field of economics, and lists its major precepts (pp. 7–8): prediction is what ultimately counts in science; there is no objective truth without observations; only quantifiable observations are objective data; introspection is subjective and worthless; science is positive and does not deal with normative questions; explaining something positively means bringing it under a general law; and a general law's validity is forever hypothetical, requiring permanent testing against objective observational data.

There is little to quarrel with regarding this characterization of modernism. Quite correctly, McCloskey also cites the most influential modern exponents of this creed: the Vienna Circle, analytical philosophy, and Popperianism in philosophy proper,[18] as well as such representative figures within the economics profession as T.W. Hutchison, Milton Friedman, and Mark Blaug.[19] And McCloskey is certainly correct, too, in identifying this modernist worldview as the current textbook orthodoxy. Nonetheless, from the outset, his understanding of empiricism-positivism is insufficient in that he fails to reconstruct the fundamental assumptions of modernism (i.e., those assumptions that underlie its various precepts). He neglects to assign them a specific place in a general, logically unified conceptual structure. He fails to clarify that the various specific modernist precepts flow essentially from the acceptance of one crucial assumption. The assumption, fundamental to modern empiricism, is that knowledge regarding reality, or empirical knowledge, must be verifiable or at least falsifiable by experience; that whatever is known by experience could have been otherwise, or, put differently, that nothing about reality could be known to be true a priori; that all a priori true statements are simply analytical statements that have no factual content, but are true by convention, representing merely tautological information about the use and the transformation rules of signs; that all cognitive meaningful statements are either empirical or analytical, but never both; and hence that normative statements, because they are neither empirical nor analytical, cannot legitimately contain any claim to truth, but must be regarded instead as mere expressions of emotions, saying in effect no more than "wow" or "grr."[20] And in failing to clarify this, McCloskey precipitates

his subsequent failure to bring even empiricism-positivism, his chosen opponent, down. His attack is simply unsystematic, and it thereby necessarily misses its goal.

McCloskey's first criticism is well targeted. He shows that contrary to the claims of Popper and his school in particular, following the advice of the empiricist-falsificationist philosophy would ultimately lead one to skepticism. Whenever a hypothetical law is empirically tested and found to be lacking, within the very framework of an empiricist methodology it is always possible to immunize one's theory by denying the recalcitrant observations outright and declaring them illusory, by acknowledging them but ascribing their recalcitrance to measurement errors, or by postulating some unobserved, intervening variable, whose lack of control is to blame for the seemingly falsifying observations. Observes McCloskey:

> Insulation from crucial test is the substance of most scientific disagreement. Economists and other scientists will complain to their fellows, "Your experiment was not properly controlled"; "You have not solved the identification problem"; "You have used an equilibrium (competitive, single-equation) model when a disequilibrium (monopolistic, 500-equation) model is relevant." . . . There is no "falsification" going on. (p. 14)

And, he remarks further, have we not known since Thomas Kuhn's *Structure of Scientific Revolutions*[21] that the actual history of natural science does not seem to come anything close to the Popperian illusion of science as a rational enterprise steadily advancing through a never-ending process of successive falsification. "Falsification, near enough, has been falsified" (p. 15).

McCloskey also shows some understanding of the sociopsychology of modernist methodology: a philosophy such as empiricism, that starts with the assumption that nothing about reality can be known with certainty and hence everything is possible, and that has no place for anything such as objective a priori considerations; an epistemology, that is to say, that puts us under no constraints whatsoever when it comes to choosing our variables to be measured and determining the relation between such variables (except insofar as the chosen relation must fit the data), can be followed by almost everyone and almost everyone can justly feel that if this is what science is all about, he can be as good a scientist as anyone else. Anyone can measure whatever he feels like measuring, then with the help of a computer fit some curves or equations on his data material, and finally change or not change the curves or equations depending on new, incoming material and/or new hypotheses about measurement error or uncontrolled intervening variables. Empiricism is a methodology suited to the intellectually poor, hence its popularity.[22] Notes McCloskey:

> Graduate students in the social sciences view courses in econometrics, sociometrics, or psychometrics as courses in how to become applied economists,

> sociologists, or psychologists. . . . The delusion is nourished by democracy, which partly explains its special prevalence in America. Everyone of normal intelligence can after such a course decipher the output of the Statistical Package for Social Sciences. No elite culture is necessary, no longer subordination to Doktor Herr Professor,[23] no knowledge accumulated through middle age. (p. 163)

Quite naturally, he sees all this as strong talk against modernist epistemology. And indeed, it might be enough to persuade someone to cease giving credence to modernism, and that would certainly be for the better. But even if true, does it constitute proof of a systematic flaw in the empiricist-positivist philosophy? And does it constitute proof in the hands of a hermeneutician?

As regards this latter question, it must be noted that for McCloskey himself to understand his statements about modernism as a *criticism* of this philosophy should strike one as simply odd. For in his discussion of empiricism-positivism, he clearly blames this philosophy for allowing scientists to engage in some all-too-pervasive intellectual permissiveness; for producing a science that advances nowhere but is a mere random walk of ideas through time to be understood only ex post by historical or sociological explanation; and thereby for opening the floodgates to the invasion of scholarship by intellectual barbarians. Yet McCloskey wants to replace this permissiveness with an even greater one. He wants us to engage in talk, endless and unconstrained by any intellectual discipline whatsoever. Thus, instead of criticizing empiricism-positivism, should he not embrace it enthusiastically for already coming so very close indeed to his own relativist ideals? If empiricism sounds ridiculous to McCloskey, his reason for this can only be that it is just not ridiculous enough, that empiricism is ridiculous because hermeneutics is even more so, and that pure nonsense must prevail over only partial nonsense.

Yet, apart from McCloskey's own position, his arguments directed against modernism cannot count as amounting to anything. "So what," the empiricist could reply. McCloskey has shown that following the modernist precepts leads to a peculiar form of relativism. Admittedly, some empiricists, most notably Popper and his school, have not and still do not recognize this.[24] McCloskey is right in pointing this out again. But then he must admit that this has also been realized by empiricists without causing them much intellectual pain. Was it not Feyerabend who first and most forcefully drove the relativist message home to Popperianism?[25] And was not he himself a leader of this very school who simply drew the ultimate logical conclusions of Popperianism?[26] Empiricism cannot explain the process of scientific development as a rational enterprise. True enough. But it cannot account for it because the process is not rational. And what is wrong with this? What is wrong with empiricism once it admits its own relativism?

McCloskey gives no answer to these questions. He does not advance any principled arguments that would prove empiricism to be a self-defeating

position. Nor does he challenge empiricism on the much more obvious empirical front. It would seem to be evident that at least empiricism's claim of providing us with a correct epistemology of the natural sciences should, in view of the facts, be regarded as incorrect. For whatever the true state of affairs with respect to economics and the social sciences might be, with respect to the natural sciences it seems difficult to deny that hand in hand with their development went a steady, universally recognized process of technological advancement and improvement, and that this fact of technological progress can hardly be brought in line with the empiricist view of science as a relativistic, noncumulative enterprise. Empiricism then simply seems to have been *empirically* refuted as an appropriate methodology for the natural sciences.[27]

Yet such a refutation in no way supports McCloskey's own position. For the existence of technological progress would have to stand just as much in the way of hermeneutical relativism as in that of empiricism.[28] Only a rationalist methodology of the natural sciences could account for such progress. Only a methodology that begins with the recognition of the fact, as an undeniably true fact of our human nature as actors and talkers, that language in general and scientific theories in particular are ultimately grounded in a common, objective reality of action and cooperation can explain why such progress is possible without thereby having to deny some partial correctness of Kuhn and Feyerabend's relativistic portrayals of the history of the natural sciences.

The relativistic impression is due to the fact that Kuhn and Feyerabend, typical of empiricists since Locke and Hume, ultimately misconceive of scientific theories as mere systems of verbal propositions and systematically ignore the foundation of these, or of any, propositions in a reality of action and interaction.[29] Only if one regards observations and theories as being completely detached from action and cooperation, not only does any single theory become immunizable, but any two rival theories whose respective terms cannot be reduced to and defined in terms of each other must then appear completely incommensurable and no rational choice is possible. If statements are merely and exclusively verbal expressions hanging in midair, what reason could there be for any one statement to ever give way to another? Any one statement can perfectly well stand alongside any other one without ever being challenged—unless we simply decide otherwise for whatever arbitrary reason. It is this that Kuhn and Feyerabend demonstrate. But this does not affect the refutability of any one theory and the commensurability of rival theories on the entirely different level of applying these theories in the reality of action, of using them as instruments of action. On the level of mere words, theories may be irrefutable and incommensurable, but practically they can never be. In fact, one could not even state that any single theory was irrefutable or any two theories were incommensurable and in what respect, unless one were to presuppose a common categorical framework that could serve as a basis for such an assessment

or comparison. And it is this *practical* refutability and commensurability of theories of natural science that explains the possibility of technological progress—even though it accounted for technological progress in quite a different manner than Popper's failed attempt.[30]

Popper would have us throw out any theory that is contradicted by any fact, which, if at all possible, would leave us virtually empty-handed, going nowhere. In recognizing the insoluble connection between theoretical knowledge (language) and actions, rationalism would instead deem such falsificationism, even if possible, as completely irrational. There is no situation conceivable in which it would be reasonable to throw away any theory—conceived of as a cognitive instrument of action—that had been successfully applied in a past situation but proves unsuccessful in a new application—unless one already had a more successful theory at hand. And to thus immunize a theory from experience is perfectly rational from the point of view of an actor. And it is just as rational for an actor to regard any two rivals, in their range of application overlapping theories t_1 and t_2 as incommensurable as long as there exists a single application in which t_1 is more successful than t_2 or vice versa. Only if t_1 can be as successfully applied as t_2 to every single instance to which t_2 is applicable but still has more and different applications than t_2 can it ever be rational to discard t_2. To discard it any earlier, because of unsuccessful applications or because t_1 could in some or even in most situations have been applied more successfully, would from the point of view of a knowing actor not be progress but retrogression. And even if t_2 is rationally discarded, progress is not achieved by falsifying it, as t_2 would actually have had some successful applications that could never possibly be nullified by anything (in the future). Instead, t_1 would outcompete t_2 in such a way that any further clinging to t_2, though of course possible, would be possible only at the price of not being able to successfully do everything that an adherent of t_1 could do who could successfully do as much and more than any proponent of t_2.

Trivial as such an account of the possibility of progress (as well as retrogression) in the natural sciences may seem, it is incompatible with empiricism. In systematically ignoring the fact that observations and theories are those of an actor, made and built in order to act successfully, empiricism has naturally deprived itself of the very criterion against which knowledge is continually tested and commensurated: the criterion of successfully or unsuccessfully reaching a set goal in applying knowledge in a given situation.[31] Without the explicit recognition of the universal operativeness of the criterion of instrumental success, relativism was inescapable. However, such relativism would once more literally be impossible to adopt, because it is incompatible with our nature as acting talkers and knowers. Relativism could not even meaningfully claim to deny the operativeness of this criterion, as this very denial would itself have to be an action that presupposed some objective standard of success. Rather, in each of our actions, we confirm rationalism's claim (as regards the natural

sciences) that one can objectively identify a range of applications for some knowledge and then test it for its success within this range, and, hence, that competing theories must be considered commensurable as regards such ranges of applications and success.

Hermeneutics versus Empiricism—Rationalism against Both Round II

McCloskey's first round against empiricism then is a complete failure. Nor is his second round of criticism any more successful. There, McCloskey takes issue with the modernists' emphasis on prediction as the cornerstone of science. Though he does not deny the possibility of prediction in the natural sciences, he doubts its overwhelming importance. However, prediction in economics, he claims, is impossible. "Predicting the economic future is, as Ludwig von Mises put it, 'beyond the power of any mortal man' " (p. 15).

In order to defend this thesis, we would expect him to establish two separate but related claims. First would be the claim that something is wrong with methodological monism—the program of an *Einheitswissenschaft*—and methodological dualism should be adopted. Otherwise it makes no sense to say that predictions are possible in one field of inquiry but impossible in another. The second claim would be that on the basis of such a dualist position, it can be demonstrated why predictions are possible in one field but not in another. McCloskey does nothing of this sort. It entirely escapes his notice that his position vis-à-vis modernism requires him to attack empiricism on account of its monism; that its monist stand makes it actually impossible for empiricism to explain how predictions, which allegedly constitute the very heart of the empiricist program, can conceivably be possible—and impossible for precisely the same reason that empiricism could not account for the possibility of progress in natural science; and that a dualist position (which McCloskey would be required to take if he wanted to systematically challenge modernism) would be incompatible with hermeneutics—itself being a monist position, though a different sort than empiricism's—and can again only be reconciled with a rationalist methodology, which alone can account for the possibility of the empiricist dream of predictions.

Empiricism is observational monism, stating that all our empirical knowledge is derived from observations and consists in interrelating these observations; and, further, that observations as well as relations have the permanent status of only being true hypothetically. This is the case in economics as well as in any other field concerned with empirical knowledge, and hence the problem of prediction must be the same everywhere. McCloskey does not answer this systematic challenge. He does not present the conclusive refutation

of such monism by pointing out that in claiming what empiricism claims, one in fact falsifies the content of one's statement. For to claim what it claims, empiricism must actually presuppose that in addition to observations, meaningful objects exist—words tied to reality via cooperation—that, along with the relations among them, must be understood rather than observed. Hence the need for methodological dualism.[32]

Nor does McCloskey notice the incompatibility of observational monism with the notion of prediction. The idea of prediction and causality (i.e., that there are constant, time-invariantly operating causes that allow one to project past observations regarding the relationship between variables into the future) is something (as empiricism since Hume has noticed) that has no observational basis and hence cannot be said to be justified (within the empiricist framework). One cannot observe the connecting link between observations, except that they are somehow contingently related in time. And even if one could observe it, this observation would still not prove that such an observed connection was time-invariant. Strictly speaking, within the framework of observational monism, it does not even make sense to place observations in objective time.[33] Rather, the observed relationships are those between data in the temporal order in which an observer happens to observe them (clearly something very different from our notion of being able to distinguish between a real, causally effective order and sequence of observations and the mere temporal order in which observations are made). Hence, strictly speaking, according to empiricism, predictions are epistemologically impossible. It is irrational to want to predict, because the very possibility of prediction cannot be rationally established. And this, then, is also the ultimate reason for empiricism's skeptical stand regarding the possibility of scientific progress. For if one cannot rationally defend the very idea of causality, how can one expect anything from science but an array of incommensurable observational statements? Progress, as it is commonly understood, is the advancement of predictive knowledge. But surely no such thing can be possible if prediction itself cannot be established as possible.[34]

McCloskey also does not confront the challenge of explaining how hermeneutics accounts for a dualism and the very possibility of prediction (if only in the natural sciences). Nor could he have succeeded in this. For an argument such as dualism would establish that certain propositions can be said to be objectively true—in fact to be a priori true—and this would contradict the relativist message of hermeneutics. Yet as a monist position, hermeneutics cannot account for causality any more than empiricism can. As an observational monism, empiricism would like to reduce all our empirical knowledge to observations and observations of contingent relations between observations, and it is therefore ultimately forced to abandon the idea of time-invariantly operating causes. Hermeneutics would like to reduce it to a talk-monism; to talk disconnected to anything real outside of talk itself; to sequences of talk hanging in midair with no objective, talk-constraining grounding whatsoever. For this

reason, hermeneutics cannot account for causality. For in the absence of any common, objective standard, all talk is simply incommensurable, and no objective connection whatsoever can exist between any talk apart from the mere temporal order of talking.

Both dualism and causality can only be explained by rationalism. Rationalism begins with the insight that empiricism is self-refuting, since it cannot actually state its own position without implicitly admitting that in addition to observations and contingent relations of observations, other meaningful things and relations (i.e., words sustained through action and acquiring meaning in the course of such action) must also exist. Similarly, rationalism rejects hermeneutics as self-refuting, because a talk-monism, too, could not be stated without implicitly admitting it as false in that it would have to presuppose the very existence of actions guided by observations, if only in order to sustain talk—thus falsifying the claim of talk ever being unconstrained by anything objective. And rationalism then concludes that the key to the problem of causality must lie in the recognition of the fact (ignored by both empiricism and hermeneutics) that observations as well as words are constrained by action, and that this can be established neither by observation nor by mere talk, but rather must be understood on account of our knowledge of action as the practical presupposition of any observation or talk as an a priori true fact of human nature.

It is from such a priori understanding of action that the idea of causality can indeed be derived.[35] Causality is not a category of observations. It is a category of action whose knowledge as an a priori feature of reality is rooted in our very understanding of our nature as actors. Only because we are actors and our experiences are those of acting individuals can observations be conceived of as occurring objectively earlier or later and as being related to each other through time-invariantly operating causes.[36] No one who did not know what it meant to act could ever experience events occurring in real time and in invariant causal sequences. And no one's knowledge of the meaning of action and causality could ever be said to be derived from contingent observational evidence, as the very fact of experiencing already presupposes action and causally interpreted observations. Every action is and must be understood as an interference with the observational world, made with the intent of diverting the "natural" course of events in order to produce (i.e., to *cause* to come into being) a different, preferred state of affairs—of making things happen that otherwise would not happen—and thus presupposes the notions of events placed in objective time and of time-invariantly operating causes. An actor can err with respect to his particular assumptions about which earlier interference produced which later result, and thus his interference might not actually turn out to be successful. But successful or not, any action, changed or unchanged in light of its success or failure, presupposes that there are constantly connected events in time, even if no particular cause for any particular event can ever be

preknown to any actor at any time. In fact, attempting to disprove that observational events are governed by time-invariantly operating causes would require one to show that some given event cannot be observed or produced based on some earlier interference. Yet trying to disprove this would again necessarily presuppose that the occurrence or nonoccurrence of the phenomenon under scrutiny could, in fact, be effected by taking appropriate action, and that the phenomenon must thus presumably be embedded in a network of constantly operating causes. Hence, rationalism concludes that the validity of the principle of causality cannot be falsified by taking any action, since any action would have to presuppose it.[37]

McCloskey notices none of this. And so it is no surprise that the arguments raised in support of his claim regarding the impossibility of prediction in economics are off the mark, too. Though in themselves correct arguments, they simply do not constitute the impossibility theorem that is needed.

What McCloskey offers as proof, which he incidentally claims to be "more precise" than some earlier, related Austrian thoughts (p. 90), is the following insight: "If economists could do [predict] better than business people, the economists would be rich. They are not" (p. 93). Hence, we should not trust people who claim to have information about future economic events. For if they really did have such knowledge, why would *they* not strike it rich, instead of telling *us* how to do it (p. 16)? Realistically, we should regard economic forecasters as providing information that, generally speaking, is economically worthless in that it tells us no more about future economic events than what concerned people on the average believe and expect anyway and have already accounted for in their present actions (p. 93f.).

Good enough. However, a much more succinct presentation than this can already be found in Mises.

> There are no rules according to which the duration of the boom or of the following depression can be computed. And even if such rules were available they would be of no use to businessmen. What the individual businessman needs in order to avoid losses is knowledge about the date of the turning point at a time when other businessmen still believe that the crash is farther away than is really the case. . . . Entrepreneurial judgement cannot be bought on the market. The entrepreneurial idea that carries on and brings profit is precisely that idea which did not occur to the majority. It is not correct foresight as such that yields profits, but foresight better than that of the rest.[38]

Yet this, as Mises but not McCloskey knows, does not prove the impossibility of causal predictions in economics.[39] All it proves is that differential profits can only emerge from *differences* in knowledge. The question is, however, if such knowledge—regardless of whether it is unequally distributed and thus allows for the possibility of differential profits and losses, or equally distributed and thus tends to only account for a uniform rate of return for the forecasters—is

such that it could be expressed in a prediction formula that could legitimately make use of the assumption of time-invariant causes and hence could be conceived of as a systematically testable and improvable formula.

Surely McCloskey does not want to deny the possibility of prediction as such in economics. We constantly make such predictions. Moreover, while economic forecasters may not generally be rich and thus evidently may not know more than the rest of us, some of them are, and certainly there are some businessmen who are rich. Evidently, people not only can forecast, but can forecast correctly and successfully. The impossibility theorem cannot be meant to prove that no (successful) prediction whatsoever can be made in the field of economics, but rather only that a certain type of prediction is impossible here that is possible elsewhere. Yet the argument does not prove this. For we have no difficulties applying the idea of differential predictive knowledge and differential returns from forecasting to the field of the natural sciences, and still conceiving of them as gradually progressing and producing ever-improved prediction formulae. One natural-science forecaster may know more than another and even stay ahead of the competition permanently, but this does not imply that his relative advantage is not one that could not possibly be expressed, at all times, in terms of a formula that uses predictive constants and is capable of systematic improvement by means of successive testings. Why, then, should this be any different in the realm of economic forecasting? Why can the rich businessman not have gained his position in the same way as the relatively more successful natural-science forecaster?

This is what must be answered by the impossibility theorem. On this, however, McCloskey is silent. Nor can an answer be formulated by a hermeneutician. For an impossibility theorem would be precisely the kind of conversation-stopping argument that McCloskey claims to be nonexistent. To prove that economic forecasting is categorically different from natural-science forecasting would only mean confirming the claims of rationalism. Such proof would not have relativistic consequences regarding economic predictions as it may at first seem—such as to say that no systematic mistake whatsoever could be made by an economic forecaster and that any economic forecast's failure or success would thus be due entirely to bad or good luck. Instead, even if it were to show that there were indeed some ineradicable element of luck in economic forecasting, making progress as it exists in technological forecasting impossible in the field of economics, at the same time such proof would establish the existence of a priori true propositions on the subject matter of economics, which would then systematically constrain the range of possible predictions about future economic events and open up the possibility of predictions that were systematically flawed in that they would be at variance with such fundamental, a priori valid knowledge.

And indeed, argues rationalism, economic predictions that would make use of the assumption of time-invariantly operating causes must thus be considered

systematically flawed.[40] While every action presupposes causality, no actor can conceive of his actions as ever being predictable on the basis of constantly operating causes. Causality can only be assumed to exist outside of the field of human action, and economic predictions as predictions concerning future actions are impossible. This follows from the very modernism that McCloskey criticizes, incidentally proving this position a self-refuting one once again. Empiricism claims that actions, just as any other phenomenon, can and must be explained by means of causal hypotheses that can be confirmed or falsified by experience. Now, if this were the case, empiricism would be forced to assume—contrary to its own doctrine that there is no a priori knowledge about reality—that time-invariantly operating causes with respect to actions exist. One would not know a priori which particular event might be the cause of a particular action. Experience would have to reveal this. But in order to proceed in the way that empiricism wants us to proceed (i.e., to relate different experiences regarding sequences of events as either confirming or falsifying each other and, if falsifying, then responding with a reformulation of the causal hypothesis), a constancy over time in the operation of causes *as such* must be presupposed. (Without such an assumption, the different experiences would simply be unrelated, incommensurable observations.[41]) However, if this were true, and actions could indeed be conceived of as governed by time-invariantly operating causes, what about explaining the explainers (i.e. the persons who carry on the very process of creating hypotheses), of verification and falsification? Evidently, in order to assimilate confirming or falsifying experiences—to replace old hypotheses with new ones—one must presumably be able to learn. However, if one is able to learn from experience, then one cannot know at any given time what one will know at a later time and how one will act based on this later knowledge. Rather, one can only reconstruct the causes of one's actions after the event, as one can only explain one's knowledge after one already possesses it. Thus, the empiricist methodology applied to the field of knowledge and action, which contains knowledge as its necessary ingredient, is simply contradictory—a logical absurdity.[42]

Moreover, it is plainly contradictory to argue that one could ever predict one's knowledge and actions based on antecedent, constantly operating causes. For to argue so is not only absurd because it implies that one can know now what one will know in the future; it is also self-defeating, because to do so would actually be saying that there was something that was not yet understood, but rather had to be learned about and examined as regards the acceptability of its validity claims, with as yet unknown results with respect to the outcome of this (either for our future kowledge, or for our and others' knowledge about the knowledge of others).

Thus, as McCloskey states yet does not prove, causal empirical explanations regarding knowledge and actions are indeed impossible. Whoever pretends, as empiricist economists invariably do, to be able to predict future knowledge

and actions based on constantly operating antecedent variables is simply speaking nonsense. There are no such constants in the field of human action, as Mises insisted over and over again. Economic forecasting is not and never can be a science, but will always be a systematically unteachable art. Yet, and I shall return to this shortly, this does not mean that such forecasts would not be constrained by anything. While no particular action can ever be predicted scientifically, each and every prediction of future actions and the consequences of actions is constrained by our a priori knowledge of actions as such.

Rationalism and the Foundations of Economics

In the second round of its criticism of empiricism-positivism, hermeneutics fails just as it failed in the first. And again it is philosophical rationalism—equally critical of hermeneutics and empiricism—that is vindicated. Yet McCloskey makes one more point worth mentioning, as he reminds us that modern hermeneutics is an outgrowth of the discipline of interpreting the Bible.[43] In line with this traditionalist orientation, the case for hermeneutics ultimately boils down to an uncritical appeal to and acceptance of authority. We are asked by McCloskey to embrace the new old creed because some authorities tell us to do so. In his view, empiricism is not wrong as such—as a matter of fact, there was a time when it was quite all right to follow empiricist advice. But that was when philosophical authorities were all sold on empiricism. In the meantime, empiricism is out of favor with the philosopher kings and only the practitioners of science still cling to it—not realizing that fashion has changed. It is high time, then, that we shift and follow the new trend setters. Writes McCloskey: "The argument that Hutchison, Samuelson, Friedman, Machlup, and their followers gave for adopting their metaphysics was an argument from authority, at the time correct, namely that this was what philosophers were saying. The trust in philosophy was a tactical error, for philosophy itself was changing as they spoke" (p. 12). And the same goes for the mathematization of economics. Once it was good; now it is becoming bad. The winds of fashion change and we had better pay attention to this. "Economists before the reception of mathematics fell headlong . . . into confusions that a little mathematics would have cleared up." Imagine, they

> could not keep clear, for instance, the difference between a movement of an entire curve and a movement along a curve. . . . But now, so long after the victory, one might ask whether the faith which supported it still serves a social function. One might ask whether the strident talk of Science in economics, which served well in bringing clarity and rigor to the field, has outlived its usefulness." (pp. 3–5)

Surely, this lives up again to truly relativistic form. Yet as we have seen, there is no reason in the world to accept such relativism. Relativism is a self-

contradictory position. And just as it is impossible to defend the hermeneutical relativism as the methodology of today, so is it impossible to defend the empiricism-positivism of yesterday. Empiricism-positivism, too, is a self-defeating doctrine, and not only because of its observational monism, which cannot be stated without implicitly admitting its falsehood and accepting a dualism of observable and meaningful phenomena to be understood on account of our knowledge of action and cooperation. Empiricism's fundamental distinction between analytical, empirical, and normative propositions is equally indefensible. What then is the status of the very proposition introducing this distinction? Assuming that empiricist reasoning is correct, it must be either an analytical or an empirical proposition, or it must be an expression of emotions. If it is understood as analytical, then according to its own doctrine it is merely verbal quibble, saying nothing about anything real but rather only defining one sound or sign by another, and hence one would simply have to reply "so what?" The same response would be appropriate, if, instead, the basic empiricist proposition were taken to be an empirical one. For if this were so, it would not only have to be admitted that the propositions might well be wrong. More decisively, as an empirical proposition, the most it could state would be a historical fact and it would thus be entirely irrelevant in determining whether or not it would be *impossible* to *ever* produce either a priori true propositions that were not analytical or normative propositions that were not emotive. And finally, if the empiricist line of reasoning were assumed to be an emotive argument, then according to its own pronouncements, it is cognitively meaningless and one would not have to pay any more attention to it than to a dog's bark. Thus, one must again conclude that empiricism-positivism is an utter failure. If it were correct, its basic premise could not even be stated as a cognitively meaningful proposition; and if it could be so stated and empiricism were indeed making the proposition that we all along thought it did, then the analytical-empirical-normative distinction would be proven false by the very proposition introducing it.[44]

How then, could it ever have been right to follow a false doctrine? To conceive of economics, or more precisely of actions, as empiricism does, and accordingly to treat economic phenomena as observable variables, measurable and tractable by mathematical reasoning, must have always been wrong. And the surge of positivism in economics could never have added to clarity, but from the very beginning must have helped instead to introduce ever more falsehoods into the field.

There *is* empirical knowledge that is valid a priori. And such knowledge informs us that it has never been correct to represent relationships between economic phenomena in terms of equations containing the assumption of empirical causal constants, because to conceive of actions as being caused by and predictable on the basis of antecedent variables is contradictory. Moreover, the very same a priori knowledge reveals that it is at all times incorrect to conceive

of economic variables as observable magnitudes. Rather, all categories of action must be understood as existing only as subjective interpretations of observable events. The fact that knowledge and talk are those of an actor and constrained by our nature as actors cannot be observed, but rather must be understood. Nor can causality or objective time ever be simply observed, but our knowledge of it is based on our prior understanding of what it is to act. And so it is regarding the rest of the economic categories, as Mises above all has shown. There are no values to be observed, but things can be understood as valued only because of our prior knowledge of action. As a matter of fact, that there is such a thing as actions also cannot be observed, but must be understood. It cannot be observed that with every action, an actor pursues a goal and that whatever his goal, the fact that it is pursued by an actor reveals that he places a relatively higher value on it than on any other goal of action that he at the very start of his action could think of. Further, it can neither be observed that in order to achieve his most highly valued goal an actor must interfere (or decide not to interfere) at an earlier point in time to produce some later result, nor that such interferences invariably imply the employment of some scarce means (at least those of the actors' body, its standing room, and the time absorbed by the interference). It is unobservable (1) that these means must also have value for an actor—a value derived from that of the goal—because the actor must think their employment necessary in order to effectively achieve the goal and (2) that actions can only be performed sequentially, always involving the making of a choice (i.e., taking up that course of action that at some given point in time promises the most highly valued result to the actor and excluding at the same time the pursual of other, less highly valued goals). It cannot be observed that as a consequence of having to choose and give preference to one goal over another—of not being able to realize all goals simultaneously—each and every action implies the incurrence of costs (i.e., forsaking the value attached to the most highly valued alternative goal that cannot be realized or whose realization must be deferred because the means necessary to effect it are bound up in the production of another even more highly valued end). And lastly, it is unobservable that at its starting point, every goal of action must be considered (1) worth more to the actor than its costs and (2) capable of yielding a profit (i.e., a result whose value is ranked higher than that of the forgone opportunities, and yet that every action is also invariably open to the possibility of a loss if an actor finds, in retrospect, that the actually achieved result—contrary to previous expectations—in fact has a lower value than the relinquished alternative would have had.

All of these categories (values, ends, means, choice, preference, cost, profit and loss, time, and causality) are implied in the concept of action. That one is able to interpret experiences in such categories requires that one already knows what it means to act. No one who is not an actor could ever understand them, as they are not "given," ready to be experienced, but experience is cast in these

terms as it is constructed by an actor. And then to treat such concepts, as empiricism-positivism would, as things extending in space and allowing quantifiable measurements is missing the goal entirely. Whatever one might explain in following empiricist advice, it has nothing whatsoever to do with explaining actions and experiences cast in the categories of action. These categories are ineradicably subjective ones. And yet they represent empirical knowledge in that they are conceptual organizations of real events and occurrences. They are not merely verbal definitions; they are real definitions of real things and real observations.[45] Furthermore, they are not only empirical knowledge; contrary to all relativistic aspirations, they incorporate a priori valid empirical knowledge. For it would clearly be impossible to disprove their empirical validity, as the attempt to do so would itself be an action aimed at a goal, requiring means, excluding other courses of action, incurring costs, and subjecting the actor to the possibility of achieving or not achieving the desired goal and so making a profit or suffering a loss. The very possession of such knowledge can never be disputed, and the validity of these concepts can never be falsified by any contingent experience, since disputing or falsifying anything already presupposes its very existence. As a matter of fact, a situation in which these categories of action would cease to have a real existence could itself never be observed, as making an observation is in itself an action.

Economic reasoning has its foundations in this a priori knowledge of the meaning of action.[46] It concerns phenomena that, though existing objectively, cannot be subjected to physical measurements, but must be *understood* as conceptually distinct events. And it concerns phenomena that cannot be predicted based on constantly operating causes; and our predictive knowledge about such phenomena, accordingly, cannot be said to be constrained by contingent empirical laws (i.e., laws that one would have to discover through a posteriori experiences). Instead, it concerns objects and events that are constrained by the existence of a priori valid, logical, or praxeological laws and constraints (i.e., laws whose validity is completely independent of any kind of a posteriori experience). Economic reasoning consists of (1) an understanding of the categories of action and the meaning of a change in values, preferences, knowledge, means, cost, profit, or loss, and so on, (2) a description of a situation in which these categories assume specific meaning and definite individuals are described as actors, with definite things specified as their goals, means, profits, and costs, and (3) a logical deduction of the consequences which result from the introduction of some specified action in this situation, or of the consequences which result for an actor if this situation is changed in a specified way. Provided there is no flaw in the process of deduction, the conclusions that such reasoning yields are valid a priori because their validity would ultimately go back to the indisputable axiom of action. If the situation and the changes introduced into it are fiction or assumptions, then the conclusions are true a priori only of a possible world. If, on the other and, the situation and situational

changes can be identified as real, perceived, and conceptualized as such by real actors, then the conclusions are a priori true propositions about the world as it really is. And such realistic conclusions, which are the economists's main concern, act as logical constraints on our actual predictions of future economic events. They do not guarantee correct predictions—even if the empirical assumptions are indeed correct and the deductions are flawless—because in reality, there can be all sorts of situational changes happening concurrently or following the explicitly introduced change in the action-world data. And though they also affect the shape of things to come (and cancel, increase, decrease, accelerate, or decelerate effects stemming from other sources), such concurrent changes can in principle never be predicted or experimentally held constant, because to conceive of subjective knowledge (whose every change has an impact on action) as predictable on the basis of antecedent variables and as capable of being held constant is an outright absurdity. The experimenter who so wanted to hold it constant would in fact have to presuppose that his knowledge, specifically his knowledge regarding the experiment's outcome, could not be assumed to be constant over time. However, while they cannot render any specific future economic event certain or even predictable on the basis of a formula, such a priori conclusions nonetheless systematically restrict the range of possibly correct predictions. Predictions that are not in line with such knowledge would be systematically flawed and would lead to a systematically increased number of forecasting errors—not in the sense that anyone who based his predictions on correct praxeological reasoning would necessarily have to be a better predictor of future economic events than someone who arrived at his predictions through logically flawed deliberations and chains of reasoning, but in the sense that in the long run, certeris paribus, the first group of forecasters would average a better record than the second.

Regarding any specific forecast, it is very possible to falter despite one's correct identification of a situational change as described in terms of the a priori categories of action and one's correct analysis of the praxeological consequences of such change, because one might err regarding one's identification of other, accompanying changes. It is equally possible to arrive at a correct forecast in spite of the fact that one's inferences drawn from one's correct description of a situational change were praxeologically wrong, because other concurrent events might be of such a kind as to counteract such a wrong assessment of consequences. However, if it is assumed that, on the average, forecasters with or without a solid grasp of praxeological laws and constants are both equally well equipped to anticipate such other concurrent changes in the action-world and to account for them in their predictions, then the group of forecasters that makes its predictions in recognition of and accordance with such laws will be more successful than that which does not.

As are all economic theorems, the law of demand (which causes empiricists as well as hermeneuticians considerable uneasiness because of its apodictically

assumed central position in economics) is an a priori true constraint on actual predictions regarding the consequences of certain actions. Empiricism tells us to conceive of it as an in-principle falsifiable hypothesis about the consequences of price changes. Yet, if we accept this and empirically test the law, we frequently find that a price increase, for instance, goes hand in hand with an increase in the quantity demanded, or that a price decrease is accompanied by a reduced demand. The law holds sometimes and for some goods, but at other times, for the same or other goods, it does not. How then, concludes empiricism, can economists assign to this law the axiomatic position that it occupies in economic theory and build a complex network of thought based on it? To do so must seem to an empiricist to be nothing but bad metaphysics that needs to be expelled from the discipline as soon as possible in order to bring economics back onto the right track.[47]

Hermeneutics is no more successful in justifying the law of demand. McCloskey realizes that the empiricist case for the law is weak at best. Yet he believes it acceptable to stick with it—as, despite their professed empiricism, most economists indeed do—because the law of demand is allegedly persuasive in light of other hermeneutical evidence (pp. 58–60). Such supportive evidence supposedly comes from "introspection," from "thought experiments," and from illustrative case stories; there is the persuasive fact that "business people" believe in the law, and "many wise economists"; the "symmetry of the law" makes it esthetically appealing; "mere definition" adds power; and "above all, there is analogy. That the Law of Demand is true for ice cream and movies, as no one would want to deny, makes it more persuasive also for gasoline" (p. 60). None of this, however, could make the law of demand any better founded and give it the authority it indeed seems to command. To be sure, introspection is the source of our knowledge of the law of demand. This particular law is no more founded in observations than are the laws of logic and mathematics. Yet introspections as such, or thought experiments, can no more establish the law of demand than can observational evidence. Introspective evidence, too, is nothing other than contingent experience. Here and now somebody arrives at this thought, and there and then someone else reaches the same or a different one. As McCloskey himself states, "if properly socialized in economics," introspection and thought experimentation make the law highly persuasive (p. 59). But, mutatis mutandis, then, if one is not so socialized, introspection might render the law far less appealing. Then, however, introspection as such can hardly be said to lend any systematic support to it. In fact, to appeal to the economists' introspective evidence would amount to a begging of the question, as it would have to be explained why one should accept this economic socialization or brainwashing in the first place. In the same way, case stories or convictions of certain businessmen or wise economists are not proof of anything. Aesthetic criteria and mere definitions, too, have no epistemological value. And conclusions per analogiam are only conclusive if the analogy itself can be said

to be correct—besides the fact that it would certainly not be impossible for someone to say that the law of demand sounds unpersuasive even for ice cream and movies.[48] Hence, hermeneutics offers nothing substantive to vindicate our belief in the law of demand.

And yet the law of demand is objectively true despite the fact that it is not based on contingent external or internal experiences. Rather, its foundation lies in our introspective understanding of action as the practical presupposition of our external as well as our internal experiences and in the recognition of the fact that this understanding must be considered epistemologically prior to any contingent act of understanding in that it could not possibly be falsified by it. The fact that in order to exchange successive units of a good A for successive units of a good B, the exchange ratio of A to B must fall follows from the law of marginal utility: as the supply of A decreases and the marginal utility of a unit of A increases, the supply of B increases and B's marginal utility decreases, and hence successive units of A will become exchangeable for successive units of B only if counteracting these divergent changes in the valuation of As and Bs that follow each exchange, B becomes successively cheaper in terms of A. And as the foundation of the law of demand, this law of marginal utility then follows directly from the undeniably true proposition that every actor always prefers what satisfies him more over what satisfies him less.[49] For then any increase in the supply of a homogeneous good (i.e., a good whose units are considered to be interchangeable and of equal serviceability) by one additional unit can only be employed as a means for the attainment of a goal that is considered less valuable (or the removal of an uneasiness that is deemed less urgent) than the least valuable goal satisfied by means of a unit of such a good if the supply were one unit less.[50] And, as required of any a priori law and again independent of any contingent experiences, this law also precisely delineates its range of application and explains what possible occurrences cannot be considered exceptions or falsifying events. For one thing, the validity of the law of diminishing marginal utility is not at all affected by the fact that the utility of the marginal unit of some good can increase as well as decrease over time. If, for instance, a hitherto unknown use for a unit of some good is found that is considered more valuable than the least urgent present use of a unit of this good, the utility derived from the marginal employment would be higher now than previously. Yet despite such an increase in marginal utility, there is no question of such a thing as a law of increasing marginal utility. For not only would the actor whose supply of the good in question was unchanged and who realized such new employment have to give up some previously satisfied desire in order to satisfy another one; he would give up the less urgent one. Moreover, if with this new state of affairs regarding an actor's knowledge about possible employments for units of some given good, its supply increases by an additional unit, its marginal utility would decrease as it would be employed to satisfy precisely that desire that previously had to be excluded from satisfaction because of its relatively lesser urgency.

Nor is it an exception to the law of diminishing marginal utility that an increase in the supply of a good from n to $n + 1$ units can lead to an increase in the utility attached to one unit of this good if such a larger supply, considered and evaluated as a whole, can be employed for the satisfaction of a want deemed more valuable than the value attached to all the satisfaction that could be attained if the units of supply were each employed separately for the various goals that could be achieved by means of one individual unit of such good.[51] However, in such a case, the increase in supply would not be one of supply-units regarded as equally serviceable, because the units simply would no longer be evaluated separately. Rather, in increasing the supply from n to $n + 1$, a different, larger-sized-unit good would be created that would be evaluated as such, and the law of diminishing marginal utility would then apply to this good in the same way as it applied to the smaller-sized good in that the first unit of this good of size $n + 1$ would again be employed for the most urgent use to which a good of this size could be put, the second unit of supply of such sized good would be employed for the second most important goal to be satisfied by such sized good, and so on.

The law of demand then, as grounded in this a priori valid theorem, has never made the unqualified prediction that less of a good will be bought if its price rises. Rather, it states that this will be the case only ceteris paribus i.e., if no increase in the demand for the good in question occurs over time and if the increase in its supply does not effect a different, larger-sized-unit good and, mutatis mutandis, the demand for money does not decrease nor does its smaller supply effect separately evaluated smaller-sized money units.[52] Since it is impossible to have a formula that allows one to predict whether or not such changes occur concurrently with the given rise in price (such changes being dependent on people's future states of knowledge and future knowledge being in principle unpredictable based on constantly operating causes), such a priori knowledge then has a rather limited usefulness for one's business of predicting the economic future. Nevertheless it acts as a logical constraint on predictions in that of all forecasters who equally correctly guess that no such concurrent change will take place, only he who recognizes the law of demand will indeed make a correct prediction, while he whose convictions are at variance with the law will blunder. Such is the logic of economic predictions and the function of praxeological reasoning.

Empiricism recommends the law of demand because it supposedly looks good—yet we can neither see it, nor would it survive empirical testing. Hermeneutics, on the other hand, recommends it because it supposedly sounds good—yet to some it sounds bad. And without some objective, extralinguistic criterion of distinguishing between good or bad, it is impossible to say more in support of the law of demand than somebody said so.

Austrians, as should be clear by now, have no reason to take either the old empiricist fashion or the new hermeneutical one very seriously. Instead,

they should take more seriously than ever the position of extreme rationalism and of praxeology as espoused above all by the "doctrinaire" Mises, as unfashionable as such a stand might now be.

Notes

1. Princeton, N.J.: Princeton University Press, 1979.

2. This is also the thesis of a book by Paul Feyerabend, *Wissenschaft als Kunst* (Frankfurt/M.: Suhrkamp, 1984).

3. See the interview with McCloskey in the Institute for Humane Studies Newsletter *Institute Scholar,* vol. 6, no. 1 (1986): 7.

4. On this "Apriori of Argumentation," see K.O. Apel, *Transformation der Philosophie,* vol. II (Frankfurt/M.: Suhrkamp, 1973).

5. In connection with the hermeneutical movement, the phrase *intellectual permissiveness* was coined by Henry Veatch in his essay "Deconstruction in Philosophy: Has Rorty Made it the Denouement of Contemporary Analytical Philosophy?" *Review of Metaphysics,* 39, December 1985.

6. McCloskey asks: "[Do we not] need something . . . besides the mere social fact that an argument proved persuasive?" No, he counters, "talk against talk is self-refuting. The person making it [i.e., raising the preceding question] appeals to a social, nonepistemological standard of persuasiveness by the very act of trying to persuade someone that mere persuasion is not enough" (pp. 38–39).

Ironically, however, this argument does *not* prove his point. On the contrary, the argument can be said to be persuasive only because a self-contradictory position is considered to be false, and not regarded as false because it has been agreed upon. Otherwise, if I did not agree, would not the argument have to be considered false?

7. On the inseparable connection between language and action, see esp. Ludwig Wittgenstein, *Philosophische Untersuchungen,* in *Schriften,* vol. I (Frankfurt/M.: Suhrkamp, 1963).

8. On this, see also H. Veatch (note 5), esp. p. 319f.

9. It is by no means an accident, then, that advocates of every conceivable political ideology can be found among the hermeneuticians. The creed goes with libertarianism and anarchism (McCloskey and Feyerabend), with socialism (Ricoeur and Foucault), and with fascism (Heidegger) as well as with, in most cases, middle-of-the-roadism. Gadamer—the special hero of Don Lavoie and the George Mason University hermeneuticians and one of the murkiest "thinkers" of them all, who manages to fill hundreds of pages without saying anything and who rambles endlessly about interpreting without ever actually interpreting any text in an intelligible way (witness his masterpiece, *Wahrheit und Methode,* Tübingen: Mohr, 1960; English transl., 1975)—successfully advanced his career under Nazism, communism, and liberal democracy. On his philosophy and his life as a vivid illustration of the meaning of hermeneutics, see the brilliant essay by Jonathan Barnes, "A Kind of Integrity," *London Review of Books,* November 6, 1986; see also David Gordon, "Hermeneutics vs. Austrian Economics," Occasional Paper (Ludwig von Mises Institute, Washington, D.C., 1986).

10. On the absolutist, a priori foundations of ethics, see Hans-Hermann Hoppe, "From the Economics of Laissez Faire to the Ethics of Libertarianism," in *Man, Economy*

and Liberty, Llewellyn H. Rockwell and Walter Block, eds., Auburn, Ala.: Ludwig von Mises Institute, 1988; Hoppe, *Eigentum, Anarchie und Staat,* Opladen: Westdeutscher Verlag, 1986. Ethical absolutism as much as methodological absolutism is very much in disrepute. T.W. Hutchison (*The Politics and Philosophy of Economics,* New York: New York Press, 1981, esp. pp. 196–97) goes so far as to smear everyone committed to such a position as a dangerous, potential dictator—revealingly, without ever going to the trouble of explaining what the ethical or methodological principles *are* whose a priori grounding allegedly implies such a threat. Instead, pluralism—ethical and methodological—is what the enlightened person today professes. Only such pluralism, it is said, permits tolerance and freedom. (See as another typical pluralist Bruce Caldwell, *Beyond Positivism,* London: Allen & Unwin, 1982, chapter 13.) Must it be stressed that this doctrine is entirely fallacious? Without a priori foundation, pluralism is itself just another unfounded ideology and there is no reason to adopt it rather than any other one. Only if a priori valid reasons could be given for adopting pluralism could it claim to safeguard tolerance and freedom. A pluralism that would be merely one of plural values would actually be destructive of both. See on this in particular Henry Veatch, *Rational Man: A Modern Interpretation of Aristotelian Ethics* (Bloomington: Indiana University Press, 1962), pp. 37–46. As contrasted to our modern pluralists, Benito Mussolini understood all this quite well. Veatch cites him on p. 41: "From the fact that all ideologies are of equal value . . . the modern relativist infers that everybody has the right to create for himself his own ideology and to attempt to enforce it with all the energy of which he is capable."

11. In defense of the idea of synthetic a priori propositions, see A. Pap, *Semantics and Necessary Truth* (New Haven: Yale University Press, 1958); B. Blanshard, *Reason and Analysis* (LaSalle, Ill.: Open Court, 1964); P. Lorenzen, *Methodisches Denken* (Frankfurt/M.: Suhrkamp 1968); P. Lorenzen, *Normative Logic and Ethics* (Mannheim: Bibliographisches Institut, 1969); F. Kambartel, *Erfahrung und Struktur* (Frankfurt/M.: Suhrkamp, 1968); F. Kambartel and J. Mittelstrass, eds., *Zum normativen Fundament der Wissenschaft* (Frankfurt/M.: Athenaeum, 1973); Ludwig von Mises, *Human Action* (Chicago: Henry Regnery, 1966); Murray N. Rothbard, *Man, Economy, and State* (Los Angeles: Nash, 1971).

12. On the faulty reason for the use of such labels, see note 10. Recently their use has also become increasingly popular among Austrians such as Mario Rizzo and Don Lavoie in order to characterize and distance themselves from the Mises–Rothbard school within the tradition of Austrianism.

13. Ludwig von Mises, *Epistemological Problems of Economics* (New York: New York University Press, 1981); Mises, *Human Action,* (Chicago: Henry Regenery, 1966); Mises, *Theory and History* (Washington, D.C.: Ludwig von Mises Institute, 1985); Mises, *The Ultimate Foundation of Economic Science* (Kansas City: Sheed Andrews and McMeel, 1978); Murray N. Rothbard, *Man, Economy, and State* (Los Angeles: Nash, 1971); Rothbard, *Individualism and the Philosophy of the Social Sciences* (San Francisco: Cato Institute, 1979); Rothbard, "Praxeology: The Methodology of Austrian Economics," in Edwin Dolan, ed., *The Foundations of Modern Austrian Economics* (Kansas City: Sheed & Ward, 1976).

14. London: Macmillan, 1932.

15. Cambridge, England: Cambridge University Press, 1975.

16. Lionel Robbins, just as the earlier Austrians Carl Menger and Eugen von Böhm-Bawerk, admittedly does not use the term *a priori,* but it should be sufficiently

clear from his arguments as well as his frequent, approving references to Mises (who does) that Robbins actually *means* to provide an a priori justification of the basic propositions and theorems of economics.

The aprioristic character of economic propositions is explicitly stressed also by Frank H. Knight in "What Is Truth in Economics," in *On the History and Method of Economics* (Chicago: University of Chicago Press, 1956).

For those familiar with the tradition of rationalist philosophy, it hardly needs to be shown that the claim of having produced an a priori true proposition does *not* imply a claim of being infallible. No one is, and rationalism has never said anything to the contrary. Rationalism merely argues that the process of validating or falsifying a statement claiming to be true a priori is categorically different from that of validating or falsifying what is commonly referred to as an empirical proposition. However, since McCloskey does seem to think that rationalism assumes infallibility and, hence, that the fact (triumphantly cited on pp. 33–34) that even in as pure a science as mathematics some alleged water-tight arguments have turned out to be inconclusive after all, constitutes proof of a fundamental flaw in rationalism—assuming here in McCloskey's favor that something such as fundamental flaws can exist at all in the absence of any truly objective standard—this point needs to be stressed here. Revisions of mathematical arguments are themselves a priori. They show only that an argument thought previously to be a priori true is not.

17. His description of Marxist economic methodology, on the same page, is not much better.

18. Karl R. Popper, in order to distinguish his falsificationism from the verificationism of the early Vienna Circle, prefers to label his philosophy "critical rationalism." To do so, however, is highly misleading if not deceptive, much like the common U.S.-practice of calling socialists or social democrats "liberals." For in fact, Popper is in complete agreement with the fundamental assumptions of empiricism (see the following discussion in the text) and explicitly rejects the traditional claims of rationalism, i.e. of being able to provide us with a priori true empirical knowledge in general and an objectively founded ethic in particular. See, for example, his "Why Are the Calculi of Logic and Arithmetic Applicable to Reality," in Karl R. Popper, *Conjectures and Refutations* (London: Routledge & Kegan Paul, 1969), where he advances the traditional empiricist thesis that "only if we are ready to accept refutations do we speak about reality" (p. 212) and "refutes" the idea of the rules of logic and arithmetic being laws of reality by pointing out that "if you put 2 + 2 rabbits in a basket, you *may* soon find seven or eight in it" (p. 211). For a correct placement of Popper's philosophy within the general framework of empiricism, see the sovereign discussion by a leading analytical philosopher, W. Stegmueller, *Hauptstroemungen der Gegenwartsphilosophie,* vol. I (Stuttgart: Kroener, 1965), chapters 9–10.

In fact, it is only fair to say that it is Popper who contributed more than anyone else to persuading the scientific community of the modernistic, empiricist-positivist worldview. In particular, it should be emphasized that it was Popper who is responsible for Hayek's and Robbins' increasing deviations from their originally much more Misesian methodological position. See on this Lionel Robbins, *An Autobiography of an Economist* (London: Macmillan, 1976); Friedrich A. Hayek, "The Theory of Complex Phenomena," in Hayek, *Studies in Philosophy, Politics and Economics* (Chicago: University of Chicago Press, 1964); Hayek, "The Pretence of Knowledge," in Hayek,

New Studies in Philosophy, Politics, Economics and the History of Ideas (Chicago: University of Chicago Press, 1978), esp. p. 31f. See also Hayek's "Einleitung" to Ludwig von Mises, *Erinnerungen* (Stuttgart: Fischer, 1978), and his "Foreword" to Ludwig von Mises, *Socialism* (Indianapolis: Liberty Fund, 1981).

19. Terence W. Hutchison, *The Significance and Basic Postulates of Economic Theory* (London: MacMillan, 1938); Milton Friedman, "The Methodology of Positive Economics," in Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953); Mark Blaug, *The Methodology of Economics* (Cambridge, England: Cambridge University Press, 1980).

20. See on this the excellent discussion in Martin Hollis and Edward J. Nell (note 15), "Introduction."

21. Chicago: University of Chicago Press, 1970.

22. See on this also the trenchant observations of Mises, *Human Action,* p. 872f., "Economics and the Universities."

23. The correct nomenclature is "Herr Professor Doktor."

24. See Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge, England: Cambridge University Press, 1970). Empiricists such as Blaug (note 19), p. 17ff., argue that Popper actually realized the possibility of "immunizing stratagems" yet "solved" this problem and thus escaped relativism and skepticism. Nothing could be further from the truth. It is correct that Popper has always been aware of the possibility of immunizing one's hypotheses from falsification. (See his *Logik der Forschung,* Tübingen: Mohr, 1969, chapter 4, sections 19, 20.) His answer to such a threat to his falsificationism, however, can hardly be accepted as a solution. For he actually admits that he cannot show such "conventionalism" to be wrong. He simply proposes to overcome it by adopting the methodological convention of not behaving as conventionalists do. Yet how can such *methodolical conventionalism* (i.e., a methodology without epistemological foundation) claim to establish science as a rational enterprise and to stimulate scientific progress? For such an assessment of Popperianism, see A. Wellmer, *Methodologie als Erkenntnistheorie* (Frankfurt/M.: Suhrkamp, 1967). Thus, the preceding classification of Popperianism as relativism and skepticism.

25. See Paul Feyerabend, *Against Method* (London: New Left Books, 1975); Feyerabend, *Science in a Free Society* (London: NLB, 1978).

26. On the complex relation between Feyerabend and Popper, see H.P. Duerr, ed., *Versuchungen. Aufsaetze zur Philosophie Feyerabends,* 2 vols. (Frankfurt/M.: Suhrkamp, 1980).

27. Strictly speaking, such an empirical refutation would not be entirely decisive and other, a priori reasons would be required to bring empiricism down. (On such reasons, see the discussion in the following text.) For empiricists could in turn challenge the validity of one's description of the facts as being indeed those of technological progress. They could, given their own framework, deny that one can know even simple facts, much less complex phenomena such as technological progress, to be so or so, because even the description of something as a fact would ultimately be hypothetical and hence one's alleged empirical refutation could not be considered crucial in any strict sense. See on the hypothetical character of basic propositions Karl Popper, *Logik der Forschung* (Tübingen: Mohr, 1969), chapter V and appendix X. Ironically, the hypothetical character of basic propositions invalidates Popper's claim, central to

his entire falsificationist philosophy, that an asymmetrical relationship between verification and falsification exists (i.e., that one can never verify a hypothesis, but can falsify it). See on this A. Pap, *Analytische Erkenntnistherie* (Wien, 1955).

28. See also Juergen Habermas, Der Universalitaetsanspruch der Hermeneutik," in K.O. Apel et al., *Hermeneutik und Ideologierkritik* (Frankfurt/M.: Suhrkamp, 1976), esp. pp. 129–31.

29. See Hans-Hermann Hoppe, *Handeln und Erkennen* (Bern: Lang, 1976).

30. See on this W. Stegmueller, *Hauptstroemungen der Gegenwartsphilosophie,* vol. II (Stuttgart: Kroener, 1975), chapter 5, esp. pp. 523ff.

31. See on this also Juergen Habermas, *Erkenntnis und Interesse* (Frankfurt/M.: Suhrkamp, 1968), esp. chapter II, sections 5–6; and K.O. Apel, *Die Erkaeren: Verstehen Kontroverse in Transzendental-pragmatischer Sicht* (Frankfurt/M.: Suhrkamp, 1979), esp. p. 284.

32. See on this also K.O. Apel, "Die Entfaltung der Sprachanalytischen Philosophie und das Problem der Geisteswissenschaften," in Apel, *Transformation der Philosophie,* vol. II (Frankfurt/M.: Suhrkamp, 1973); Apel (note 31).

33. See on this also Hans-Hermann Hoppe (note 29), chapter 3 and esp. pp. 62–65; also Immanuel Kant, *Kritik der reinen Vernunft,* in Kant, *Werke,* vol. II, W. Weischedel, ed., (Wiesbaden: Insel, 1956), esp. p. 226ff.

34. It is worth emphasizing here that these remarks on the skeptical, relativistic conclusions of empiricism regarding the possibility of prediction also fully apply to Popperianism. Popper, with great self-assurance, claims to have solved—through adopting his falsificationist methodology—the Humean problem of induction and thereby to have reestablished science as a rational enterprise. (See in particular Karl R. Popper, *Objective Knowledge,* Oxford, England: Oxford University Press, 1972, p. 85ff.) Alas, this is simply an illusion. For how can it be possible to relate two or more observational experiences, even if they concern the relations between things that are perceived to be the same or similar, as *falsifying* (or *confirming*) each other, rather than merely neutrally record them as one experience here and one experience there, one repetitive of another or not, and leaving it at that (i.e., regarding them as logically incommensurable) unless one presupposed the existence of time-invariantly operating causes? Only if the existence of such time-invariantly operating causes could be assumed would there by any logically compelling reason to regard them as commensurable and as falsifying or confirming each other. However, Popper, like all empiricists, denies that any such assumption can be given an a priori defense (there are for him no such things as a priori true propositions about reality such as the causality principle would have to be) and is itself merely hypothetical. Yet clearly, if the possibility of constantly operating causes *as such* is only a hypothetical one, then it can hardly be claimed, as Popper does, that any particular predictive hypothesis could ever be falsified or confirmed. For then the falsification (or confirmation) would have to be considered a hypothetical one: any predictive hypothesis would only undergo tests whose status as tests were themselves hypothetical. And hence one would be right back in the muddy midst of skepticism. Only if the causality principle as such could be unconditionally established as true, could any particular causal hypothesis ever be testable, and the outcome of a test provide rational grounds for deciding whether or not to uphold a given hypothesis.

35. See on this (Kantian) idea F. Kambartel, *Erfahrung und Struktur* (note 11), chapter 3, esp. pp. 122f, 127, 144; Hans-Hermann Hoppe (note 29), chapter 4, esp. p. 98.

36. See on this Ludwig von Mises, *Human Action* (note 13), chapter I.5; Carl Menger, *Grundsaetze der Volkswirtschaftslehre* (Vienna: Braumueller, 1871), pp. 3, 7ff.

37. Though quite frequently mentioned as an empirical counterexample, it should be noted that quantum physics, or more precisely the indeterminacy or Heisenberg principle of quantum physics, correctly interpreted, is in accordance with this. What has been previously said does not preclude—and this is precisely the situation in quantum physics—that in order to experimentally produce a result, two or more measurement acts must be performed and that because any two separate actions can only be performed sequentially, the performance of the latter act of measurement might change the results of the former one, so that if this proves to be unavoidable, the results in question can only be predicted statistically and a deterministic explanation proves impossible. But even here each separate measurement act presupposes the validity of the constancy principle—otherwise, neither of them would have been performed; and the sequence of acts, too, presupposes constantly operating causes as it would otherwise be simply impossible to repeat two experiments in the field of quantum physics and state this to be the case. Moreover, the experience of quantum physics is in exact line with the preceding conclusion regarding the characteristic of causality as an action-produced phenomenon and as a necessary (known to be valid a priori) feature of reality. If causes can indeed only be measured and identified sequentially, by means of actions that have repercussions for each other, then they can, in principle, only be causes whose constant operation is of a probabilistic kind—and this, to be sure, can again be known to be true a priori. Quantum physics then only reveals that cases such as this are not merely conceivable, but do in fact exist. See on this F. Kambartel, *Erfahrung und Struktur* (note 11), p. 138ff.; also P. Mittelstaedt, *Philosophische Probleme der odernen Physik* (Mannheim: Bibliographisches Institut, 1968).

38. *Human Action* (note 13), pp. 870–71.

39. Mises correctly emphasizes that the decisive argument against causal predictions in economics must be the absence of "constant relations" in the field of human knowledge and actions. See, for instance, *Human Action* (note 13), p. 55f.

40. See on the following Hans-Hermann Hoppe, *Kritik der kausalwissenschaftlichen Sozialforschung* (Opladen: Westdeutscher Verlag, 1983); Hoppe, "Is Research Based on Causal Scientific Principles Possible in the Social Sciences," *Ratio,* XXV, no. 1, 1983.

41. On this, see note 34.

42. Interestingly, this proof has been first formulated by Popper in the preface of his *The Poverty of Historicism* (London: Routledge & Kegan Paul, 1957). However, Popper failed to realize that such proof actually invalidates the idea of a methodological monism and demonstrates the inapplicability of his falsificationist philosophy in the field of human action and knowledge. See on this Hans-Hermann Hoppe, *Kritik der kausalwissenschaftlichen Sozialforschung* (note 40), pp. 44–49; K.O. Apel, *Die Erklaeren: Verstehen Kontroverse* (note 31), pp. 44–46, note 19.

43. See on this H. Albert, *Traktat ueber kritische Vernunft* (Tübingen: Mohr, 1969), esp. chapter 5.V, VI.

44. Mises writes:

> The essence of logical positivism is to deny the cognitive value of a priori knowledge by pointing out that all a priori propositions are merely analytic. They do not provide new information, but are merely verbal and tautological. . . . Only experience can lead

> to synthetic propositions. There is an obvious objection against this doctrine, viz., that this proposition is in itself a—as the present writer thinks, false—synthetic a priori proposition, for it can manifestly not be established by experience. (*The Ultimate Foundation of Economic Science* [note 13], p. 5.)

It is remarkable to notice how utterly helplessly empiricists react to such arguments establishing the case for synthetic a priori propositions. Witness, for instance, Mark Blaug, *The Methodology of Economics* (note 19), pp. 91–93, where he engages in an all-out smear attack on Mises ("Mises' . . . later writings on the foundations of economic science are so cranky and idiosyncratic that we can only wonder that they have been taken seriously by anyone," p. 93) without presenting a single argument and without noticing how strangely his self-assuredness and the apodicticity with which he presents his antiapriorist methodological pronouncements contrast with his very own professed falsificationism. The same discrepancy between, on the one hand, a complete lack of argument and, on the other, apodictic arrogance, also marks his "discussion" of Hollis and Nell's *Rational Economic Man* (note 15) on pp. 123–26.

45. Empiricists, of course, would brand such definitions as tautological. Yet it should be perfectly clear that the preceding definition of action is of a categorically different nature than a definition such as "*bachelor* meaning "unmarried man." Whereas the latter is indeed a completely arbitrary verbal stipulation, the propositions defining action are most definitely not. In fact, while one can define anything as one pleases, one cannot help but make the conceptual distinctions between goals and means and so on as "defining something by something" would itself be an action. It is thus contradictory to deny, as empiricism-positivism does, the existence of "real definitions." Hollis and Nell (note 15) observe "Honest definitions are, from an empiricist point of view, of two sorts, lexical and stipulative" (p. 177). But

> When it comes to justifying [this] view, we are presumably being offered a definition of *definition*. Whichever category of definition the definition falls in, we need not accept it as of any epistemological worth. Indeed, it would not be even a possible epistemological thesis, unless it were neither lexical nor stipulative. The view [then] is both inconvenient and self-refuting. A contrary opinion with a long pedigree is that there are "real" definitions, which capture the essence of the thing defined. (p. 178)

See also B. Blanshard (note 11), p. 268f.

46. Hollis and Nell (note 15, p. 243) contend that not "action" but "reproduction of the economic system" is the primary concept on which economics, conceived of as an a priori science, rests. Noticing this disagreement among apriorists has led Caldwell (note 10, p. 131ff.) to the curious conclusion that something must be wrong with apriorism and to then advocate a do-not-commit-yourself-to-anything pluralism. (See note 10.) Yet such reasoning is about as conclusive (or, rather, inconclusive) as inferring from the fact that disagreements among people regarding the validity of certain empirical propositions exist, that no empirical facts exist and hence no empirical science is possible. Indeed, Caldwell's conclusion is even more curious, given the fact that in the dispute at hand, the solution is as clear as daylight: Whatever an economic "system" might be, it can certainly not exist or be able to reproduce itself without acting people. Moreover, to say that "reproduction of the system" is the primary concept for economic analysis is plainly contradictory—unless it were simply synonymous with saying that action is such a concept—because saying so would in fact presuppose an actor saying it.

47. On the empiricist position regarding the law of demand, see Mark Blaug (note 19), chapter 6.

48. Moreover, why would the argument not also go the other way? If, empirically, the law of demand does not seem to work for some goods, why would not analogy lead us to question it for those in which it does? (I owe this argument to David Gordon.)

49. See on this Ludwig von Mises, *Human Action* (note 13), p. 124.

50. Robert Nozick ("On Austrian Methodology," in *Synthese,* 36, 1977) believes Austrians to be inconsistent in (1) claiming that actions invariably show preference (and never indifference) and (2) employing the idea of "*homo*geneity" and "*equal* serviceability" of goods in their formulation of the law of marginal utility (p. 37ff.). However, such a charge would only be correct if "preference" and "indifference" were both considered categories of the same type. This has been correctly pointed out by Walter Block ("On Robert Nozick's 'On Austrian Methodology'," *Inquiry,* 23, 1980), who insists that "indifference" is not, unlike "preference," a *praxeological* category. Yet his classification of indifference as a "psychological category" instead (p. 424) is also incorrect. In fact, "sameness" is an *epistemological* category: humans are knowers *and* actors; they only act because they know, and they only know because they act. That something is the same (or different) than something else we know qua actors who *know.* (Indeed, "sameness' is a universal epistemological category in that one could not even say anything, for instance about actions, without the notion of something being an instance of some particular *type* of thing.) That something that is known to be the same can never actually be treated with indifference we know qua knowers who *act.* The law of diminishing marginal utility then is a law regarding knowers who *act.*

51. See Ludwig von Mises, *Human Action* (note 13), p. 125; M.N. Rothbard, *Man, Economy, and State* (note 13), p. 268ff.

52. Empiricists will complain that such a formulation of the law will make it tautological and unfalsifiable. Both classifications are false. Clearly, the discovery of a new, more highly valued use for, for instance, a unit of a given good, i.e. the event "increase in demand," and the event "a higher price is paid for it" are two conceptually distinct events, and to logically relate such events then is a categorically different thing than to stipulate that "*bachelor* means 'unmarried man'." (See also note 45.) That the use of ceteris paribus clauses in economics implies an immunization strategy, on the other hand, would be true only if economic propositions were indeed concerned with contingent empirical causal laws. In the natural sciences, where laws do have this status, such complaint would be appropriate—yet there, interestingly enough, one hardly ever finds ceteris paribus clauses. In the natural sciences, predictive hypotheses of the form "If . . . then" are in fact treated as applicable whenever the if-condition is given, no matter what else is or is not the case. And it is only because this is done that such hypotheses can be validated at all. (There is only one way of testing hypotheses about contingent empirical causal relations: in and through factual applications.) If, contrary to this, one were to demand that in order to apply a hypothesis or to repeat its application, a *full* description of the world at the moment of application be given, or that *everything* be the same in the second application as in the first (beyond the sameness of the condition explicitly stated in the if-clause), the hypothesis would become inapplicable and thus empty for the practical reason that such a demand would literally involve describing all of the universe, and for the theoretical reason that no one at any point in time could possibly know what all the variables are that make up this universe (as this question remains open to new discoveries).

The situation is entirely different in economics, and it is curious indeed that this should not have been realized—given the facts that the use of ceteris paribus clauses in the empirical sciences would render such sciences futile and that such clauses are nonetheless constantly employed in economics. Why, then, not give serious consideration to the idea that economics might be an altogether different science? Indeed, as we have seen, this is the case. Economic propositions can be validated independently of any factual application as implied (or not implied) in the incontestable axiom of action plus certain situations and situational changes described in terms of the categories of action. Yet then ceteris paribus clauses are completely harmless. In fact, their use simply serves to remind us that the deduced consequences only follow if the situation is indeed as described (and not a logically praxeologically different one), and that it is impossible in all actual applications of economic theorems (i.e., whenever the situation analyzed can be identified as real) to hold the ceteris experimentally constant (as the "holding constant" then can, in principle, only be done logically, by means of thought-experimentation. See on this also Hans-Hermann Hoppe, *Kritik der kausalwissenschaftlichen Sozialforschung* (note 40), p. 78–81.