

Rhetoric and Rationality: A Review Essay of McCloskey's *The Rhetoric of Economics*

William N. Butos*

INTRODUCTION

Donald McCloskey's forays into methodology, first in his 1983 article and now in an expanded, more restrained book, *The Rhetoric of Economics*, are likely to infuriate many and delight others. The book is a sustained, frontal assault on "modernism" and the desirability and viability of any methodology. These, he argues, are the lingering manifestations of obsolete ideas that stultify the pursuit of knowledge and, in any case, establish criteria that are not realizable or even adhered to in practice. The thrust of McCloskey's positive thesis is to argue for the primacy of "rhetoric" in economics, the literary character of economic science, and to show through case studies that various rhetorical categories (such as figures of speech, metaphor, appeal to authority) do indeed pervade economists' discourse.

McCloskey's book is witty, iconoclastic, irreverent, and sometimes even brilliant. There is much in the book that I am in agreement with and that I believe needs to be said. Yet, I also harbor concerns about McCloskey's advocacy of "anything goes" (methodologically) and what I take to be his (unintended) trivializing of the nature and significance of rhetoric. I shall argue in Section II that in matters methodological anything does not go and in Section III that McCloskey's conception of rhetoric is incomplete. As a result, his desire to purge prescriptivism from methodology (which I endorse) is overwhelmed by his rejection of any rules (which I do not endorse), and his reduction of rhetoric to "literary criticism" leads to the epistemological irrelevance of rhetoric and hence to an argument for its second-rate status.

DOES ANYTHING GO (METHODOLOGICALLY)?

According to McCloskey (1985a) the "official methodology of economics is modernist" (p. 5), an amalgam of philosophical tenets roughly corresponding to positivism, but also including "scientism, behaviorism, operationalism, positive economics, and other quantifying enthusiasms of the 1930s" (p. 4). Economists pay homage to modernism but do not actually practice economics according to the "Ten Commandments and Golden Rule of modernism" (pp. 7-8). Discussion in Chapters 5-7 of the work of Samuelson, Becker, Solow, Muth, and Fogel amply demonstrates that the criteria for rationality embedded in positivism do not provide useful guidance for scientific activity. In contrast to positivism, McCloskey's concern is appropriately the rationality of people, not the rationality of propositions as true or false determined by verification, confirmation, or probabilistic inference.

*Department of Economics, Trinity College, Hartford CT 06106

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The point of his discussion is to claim that "any rule-bound methodology is objectionable" and that the methodologist's "proper business, if any, is an anarchistic one, resisting the rigidity and pretension of rules" (p. 20). Modernism, moreover, is "obsolete in philosophy" and "the philosophical arguments for it have long been known to be unpersuasive" (pp. 11-12).

There are, perhaps, a number of ways to interpret or at least give content to this charge. I propose to explore only two: the first derives from the nonjustificationist approach to the philosophy of science (Bartley, 1984). The second arises within the context of the "science as spontaneous order" perspective.

Within the framework of the justificationist approach, the quest for certain (or true) knowledge is identified with procedures that verify or confirm the truth of propositions. Proven or true knowledge is justified by appeal to epistemological authority, whether Lockean sense experience or Cartesian rational intuition.

Justificationism fails as a theory of rationality because the inherent limitations on knowledge prevent the justification of an ultimate epistemological authority. Recourse to avoiding an infinite regress of justification is to base one's beliefs on dogmatism, fideism, or to deny the possibility of genuine knowledge at all (skepticism). Unless one has solved the problem of induction (that is, succeeded in justifying an ultimate rational authority) intellectual honesty requires one to accept the principle that no rational basis exists for belief. As a result, there is a retreat to commitment (to borrow Bartley's phrase) or paradoxically a "rational" basis for irrationalism.

Recent philosophy of science is usefully interpreted as a rejection of justificationism and as the advocacy of nonjustificational approaches to the theory of rationality. The justificationist claims of instant rational assessment (by verification, confirmation, or probabilistic inference), gradual accumulation of genuine (proven) knowledge, and the rational reconstruction of the growth of knowledge, have given way to opposing views, among them science as an error-correcting and critical activity (Bartley, 1984; Popper, 1959, 1962), facts as theory-dependent (Popper, 1959; Kuhn 1970), notions of incommensurable paradigms and research programs (Kuhn, 1970; Feyerabend, 1975; Lakatos, 1970), theoretical pluralism (Feyerabend, 1975), evolutionary epistemology (Campbell, 1974; Popper, 1962, 1974; Bartley, 1984), and concern about the psychological and sociological aspects of knowledge acquisition and transmission (Weimer, 1979; Kuhn, 1970, 1974; Polanyi, 1966, 1969).

Despite these tendencies, a significant element of authoritarianism remains and with it the pretension of rules to which McCloskey objects. For the Popperians, the specter of irrationality in science compels them to reconstruct and prescribe scientific enterprise in accordance with objective methodological rules. Thus, the growth of science "*takes place essentially in the world of ideas, . . . in the world of articulated knowledge which is independent of knowing subjects*" (Lakatos, 1970, pp. 179-180, his italics).¹ Moreover, the Popperian position rejects psychological accounts of science because they cannot be anchored to objective methodological criteria; as a result, Kuhnian "*scientific revolution is irrational, a matter for mob psychology*" (Lakatos, 1970, p. 178, his italics).

Opposed to this Popperian view, Kuhn's conception of scientific activity extends to the level of analysis beyond (or behind) theories. According to Kuhn, an implicit, inarticulate (tacit) vision guides research in normal science activity, while paradigm clashes reflect incommensurable points of view that cause the same domain to be perceived or conceived differently.² Kuhn's account of paradigm selection (or allegiance) rests on a psychological-sociological view of science and knowledge acquisition. Thus, Kuhn reformulates the notion of scientific rationality to extend beyond purely methodological rules or philosophical criteria.

No one has delighted or been as effective in subjecting Popperian prescriptivism to

historical scrutiny as Paul Feyerabend. In *Against Method* and elsewhere, Feyerabend argues that for every methodological rule historical examples abound that demonstrate that advances arose because rules were ignored, broken, or inverted. He argues that "the only principle that does not inhibit progress is: *'anything goes'*" and that the principle "is both reasonable and *absolutely necessary* for the growth of knowledge" (Feyerabend, 1975, p. 23, his italics).

McCloskey's endorsement of Feyerabend's "anything goes" provokes the charge of irrationalism from the Popperians and of subversion from economic methodologists (Caldwell and Coats, 1984). However, the Popperian claim that scientific activity must be constrained by objective methodological rules if the growth of knowledge is to occur provides no guidance for rational rule selection and no criteria for rule assessment, especially between paradigms and incommensurable research programs. As a result, a charge of irrationalism directed at McCloskey's position is misplaced and pointless.

In appropriately subverting "cookbook methodologies," however, McCloskey (like Feyerabend, but unlike Kuhn) overstates his case by failing to acknowledge that a coherent and amply documented theory of scientific enterprise is available that does not require the ratiomania of prescriptivism or the anarchism of "anything goes." The basic issue here, as suggested earlier, concerns the quest for a theory of rationality in scientific activity. I shall suggest that when account is given to science as a spontaneous order, a context of constraint necessarily emerges that renders "anything goes" untenable.

The idea of spontaneous order³ is the central organizing principle in the work of Carl Menger and F. A. Hayek, in whose hands it receives its most systematic and complete treatment.⁴ Hayek's point of departure is the rejection of theories that claim that social structures are necessarily the products of conscious design. Rather, his focus is on those spontaneous, grown orders that evolve endogenously (like the market, language, the common law, and social mores) without having been arranged or constructed by anyone and which are not directed toward particular ends specified in advance. The complexity of spontaneous orders constrains the kind of knowledge we can gain about them. In general, our knowledge will refer to abstract rules of conduct ascribed to the elements within orders rather than the particular circumstances confronting the elements. Explanations of a spontaneous order are limited to its general features or patterns because it is beyond our power to know the facts sufficient for explanation (Hayek, 1967).

At the most general level, the evolutionary pattern of spontaneous orders involves an "essential tension between tradition and innovation, stability and change" (Weimer, 1987, p. 2). Since general rules of conduct permit infinite applicability to particular circumstances, novel and diverse outcomes (at the level of particular) are inherent to (evolving) spontaneous orders. But such novelty does not remain unopposed: mechanisms that work to preserve the previous organization typically exercise restraint on novelty, thereby limiting or perhaps discarding innovations.⁵ Whatever the outcome, the process is one of trial and error operating within a context of constraint, and it is the specification of the context of constraint that reconciles the unboundedness suggested by change and innovation with the restrictiveness suggested by tradition.

In outlining the theory of spontaneous order, no mention has been made of science. Nonetheless, the discussion above contains the necessary ingredients to warrant viewing science as a spontaneous order. This claim together with a nonjustificational theory of science forms the basis for suggesting that science is rhetorical exchange, as will be discussed in Section IV.

Like other cultural phenomena, science is social. It exists as a social structure, as a community, whose accumulated knowledge exceeds that of any constituent individual. In Hayekian terms, a division of knowledge exists in the scientific community. Hence, the

community faces the problem of the transmission of knowledge so that its utilization surpasses that attainable by a single mind. In exactly the same sense as the market order, no distinct locus of control exists within the scientific community that renders the (social) activity of science conscious.

The claim that science may be rational even though it is not conscious is illustrated in Kuhn's conception of scientific activity. Learning from exemplary puzzles, a disciplinary matrix, and the passing of a "world vision" from teacher to student involve the transmission of tacit knowledge.⁶ What is learnt is a variety of abstract principles and rules of conduct for research that permit activity to occur within a structured framework.

But as I have suggested earlier, the application of abstract rules of conduct to particular circumstances within a spontaneous order will generate unanticipated, undesigned results.⁷ The rules are generative so that even though they are finite, the diversity they permit is not. In like fashion, the application of general rules in scientific activity does not require the researcher to reach a specific outcome ("you *must* find this particular result"). Rather, rules require research to be structured but not determined. Hence, anomalies emerge not from design but from within a framework of rule governed activity that sets up tensions between creativity and innovation on one hand and tradition and entrenched views on the other.

Science, then, cannot properly be viewed as anarchistic since its growth requires its practitioners to submit to those rules of conduct that are necessary to preserve its integrity as an ongoing spontaneous order. Nor does this claim imply that scientists are any the less "free" because they yield (even blindly) to the weight of rules; indeed, the opposite is more nearly the case since it is only within a framework of rules (like the Law) that freedom and diversity flourish.

I have tried to show that science, like other kinds of social phenomena, may be understood as an (evolving) spontaneous order. While innovation and novelty are essential characteristics of such orders, it has also been suggested that they operate within a context of constraint. In science, constraint is exercised by abstract rules, indeed, traditions, that inhere within the scientific community. Such traditions, even if they are tacit, provide the context within which research essentially defines itself; they represent the scientist's inheritance, desired or not, wise or not, from which escape is virtually impossible. This means that scientific activity takes place within a community to which the researcher, no matter how revolutionary, must reconcile himself. In short: "anything" will not go.

At the same time, I do not wish to give the impression that rules and traditions in the social activity of science require methodological prescriptivism. If consistently applied (or worse, enforced), prescriptivism turns the spontaneous order of science into an "organization," from an open-ended order into a deterministic one, in which scientific findings are assessed in conformity with the "correct" method or logic of discovery. What I am suggesting is that science, like other kinds of social activity, cannot escape from or legislate away its traditions.

There is still the question: what is it that preserves the character of science *qua* science? Traditions and rules, after all, are not necessarily conducive to the growth of knowledge. What then, if anything, serves to weed out the relatively blacker claims of science? McCloskey claims that we must look to the "rhetoric of economics" for the answer; that we can understand economic thought by describing the way economists argue.

THE RHETORIC OF SCIENCE VS RHETORIC-AS-EPISTEMIC

The claim McCloskey (1985a, p. 27) wishes to defend is that "good science is good conversation." Good science requires that economists give up their "philosophical baggage . . .

and begin to look at how they converse, really converse" (p. 28). The examination of conversation McCloskey calls rhetoric. Rhetoric is "a literary way of examining conversation" (p. 28), "exploring thought by conversation" (p. 29), "the art of speaking" (p. 29), "the study of how people persuade" (p. 29), "the paying attention to one's audience" (p. xvii), "the proportioning of means to desires in speech" (p. xviii), and "a way of showing how [economics] accomplishes its results" (p. xix).

There is value to what McCloskey says. The propositions, facts, and theories economists accept, reject, or suspend judgment about represent arguments for looking at reality in a certain way. Even "logical proofs" do not necessarily carry force since systems of logic are self-referential and, therefore, argumentative. When one accepts "minimum wage laws cause unemployment" or "the money supply is endogenous" or "the derivative of the function $y = 2x$ is 2," it means that one has been persuaded of the claim's validity (or, if one probes more deeply, of the correctness of the underlying premises from which the claim is deduced). As Perelman (1971, p. 119) puts it: "The rhetorical dimension is unavoidable in every philosophical argument, in every scientific discussion which is not restricted to mere calculation but seeks to justify its elaboration or application."

If rhetoric is the argumentative mode of discourse, does that mean rhetoric is exclusively descriptive and analytical, as McCloskey suggests? The argument that rhetoric may be epistemic is apparently not considered by McCloskey, largely (it seems) because he claims epistemology has nothing useful to say about "truth."⁸ Even if (certain) truth is beyond the pale, does that claim imply that rhetoric does not have epistemic significance? I claim that rhetoric not only is a way of knowing, but also that all knowing (and hence science) is rhetorical. Once the justificationist quest for proven knowledge is abandoned, scientific rationality merges with rhetorical activity and all knowledge becomes rhetorical. As a result, conceptions of rhetoric that see it as a description of discourse, as a set of principles for effective conversation and writing, or as the analysis of how people persuade, confine the domain of rhetoric to form and deprive rhetoric of any epistemic value.

McCloskey's apparent rejection of rhetoric-as-epistemic is surprising in light of the direction pursued by rhetorical theorists during the past couple of decades. Prior to Scott's seminal article in 1967, the dominant view emphasized rhetoric as dealing with the aesthetics of style and the transmission of truth obtained from scientific inquiry. Rhetoric's function was to make truth effective, but it could not enlarge or create truth since that was the province of science. In general, contemporary rhetoricians reject this conception and argue for one that sustains an epistemic role for rhetoric.

Rhetoricians have claimed an epistemic role for rhetoric by advancing arguments that may be grouped conveniently into three categories. First, rhetoric is epistemic because reality is a social construction created by discourse; second, rhetoric serves as a "meta-logic" that is able to resolve conflicts over first principles; and third, science is rhetorical because all knowing and claiming involves the argumentative use of discourse. These arguments adopt a distinctly anti-modernist stance in claiming the role of audience agreement in determining knowledge, the limitations of pure reason, and the creation of knowledge by rhetorical transaction. By exploring each position in somewhat greater detail, I hope to show that McCloskey neglects to exploit arguments that could have strengthened his argument and given it greater substance.

Reality as a social construction finds broad support in recent analyses of the epistemic role of rhetoric. Scott (1967) originally proposed that interaction between scientist and audience yields justified intersubjective knowledge of problematic particulars. The agreement that arises in the "rhetorical situation" (Bitzer, 1968) serves as a standard of truth; hence, rhetorical exchange creates knowledge in terms of standards fixed by agreement. Others have argued that

intersubjective knowledge of basic principles also is created by rhetorical exchange. Thus, Farrell (1976) claims that the rhetorical character of social knowledge "*comprises conceptions of symbolic relationships*" (p. 4, his italics) that are not static or permanent but provide a context for generative "collective inference-making" (p. 10) that fosters desirable public behavior.⁹

The question of what mechanism validates social knowledge arises once the normative role of rhetoric is admitted. Brummett (1976) addressed this problem by claiming that people create their own reality by participating in its construction through discourse in which they give and gain meaning about reality. Hence, reality and our knowledge of it are rhetorical constructions created (and altered) by discourse. Conflicts in "shared meanings" are resolved by a "validating context" that establishes the situation- or issue-specific criteria necessary to reach intersubjective agreement. As in Farrell's treatment, agreement is not static. Instead, agreement "is made and unmade by rhetorical discourse" in which "active agents . . . *urge* meaning upon us" (Brummett, 1976, p. 35, his italics).

Farrell and Brummett treat social knowledge as relativistic because of its dependency upon standards of truth—consensus and agreement of "shared meanings"—that change. Both authors use such consensual standards to "validate" social knowledge and, thus, to justify individuals' commitment to rhetorically determined truth. Cherwitz (1977) also seeks to secure validation for social knowledge, but without the subjectivism and relativism of Farrell and Brummett. He does this by arguing that validation requires rhetorical methods geared toward criticism and public scrutiny. Moreover, Cherwitz differentiates between reasoned understanding achieved through rhetorical criticism and unfounded, pre-rhetorical, personal knowledge that has not been validated.¹⁰

The rhetoric-as-epistemic theories noted above implicitly endorse a justificational approach to rhetoric. Instead of seeking justification from the usual epistemological authorities, Farrell and Brummett find justification in consensus and Cherwitz in the public airing of views. For them, therefore, rhetoric functions as a means to justify truth and truth resides in beliefs reached through consensus.

A second (and yet stronger) rhetoric as epistemic position claims that rhetoric functions as a method for deciding between alternative conceptions of reality, as a "kind of meta-logic . . . to secure the first premises of science . . . by overcoming the limitations in formal systems of argument" (Leff, 1978, p. 80). According to this view, the first principles of scientific systems are often simply assumed among the givens. But in "crisis" situations, when reality is seen differently, equally internally consistent systems offer no formal means of demonstrating their superiority. A method beyond the self-referential mode of formal deductive argument is required to resolve conflicts. Grassi (1976, p. 203), for example, in contrast to McCloskey, claims "the problem of rhetoric . . . cannot be separated from a discussion of its relation to philosophy." (1976 p 214)

Grassi's position on the epistemic role of rhetoric is characteristic of many contemporary rhetoricians who find an important place for rhetoric in philosophy. Yet, the status of rhetoric within philosophy that Grassi and others (Arnold, 1971; Johnstone, 1971) advance rests on a bifurcation of knowledge that makes rhetoric second-rate. The purpose of rhetoric, according to Grassi, is to bridge the gap between rational and emotive speech and in so doing to achieve epistemic significance. But rhetoric attains this status only because the problem at hand resists resolution by rational thought. Grassi endorses rhetoric as a necessary step for acquiring knowledge when higher epistemic standards are inapplicable and, thus, he establishes a role for rhetoric that is inferior to the clarity of rational thought attributed to science and logic.

Thus far, I have discussed views of rhetoric-as-epistemic that see rhetoric as an instrument

for validating knowledge and as second-rate to rational thought. But if justificationism is rejected, and with it the quest for (rhetorically) validated knowledge, a conception of rhetoric emerges that rejects its second-rate status. While this view shares some similarities with those previously discussed regarding the social construction of knowledge and rhetoric as a method for choosing among alternative conceptions of reality, it does so in an explicitly nonjustificational context. Its view of rhetoric, clearly the boldest of those considered here, is that all knowing is rhetorical.

According to the "all knowing is rhetorical" position, claims about knowledge become (and always remain) conjectural and subject to criticism. Conjectures, however, are not passive claims, but arguments that the presenter urges others to accept so that reality can be perceived in a particular way. This conclusion also emerges if knowledge is explicitly approached as a social construction. Here, explicit social knowledge transcends the knowledge attainable by any individual. The division of knowledge in science, for example, implies that the rationality of an individual (eg., what facts, theories, test results, etc., the individual accepts and how research is conducted) depends upon a complex social order in which the role of fellow scientists and a common language make knowledge rhetorical. This applies to all forms of scientific activity in the form of tuition and communication in normal science, theoretical clashes in revolutionary science, as well as presentation to wider audiences (taxpayers, funding agencies, and the public). In all cases, scientists present arguments, not data, apodictic claims, or unproblematic method, to support their views (Weimer, 1984, 1979; Wander, 1976; Overington, 1977).

Fundamental to this view of the relationship between knowledge and rhetoric is the role of symbolic inducement, the "invitation at all levels of symbolic activity . . . to join and in some way become consubstantial with particular modes of symbolizing" (Gregg, 1978, p. 143).¹¹ According to Gregg (1981) and Weimer (1977, 1984), all ascription of meaning, from the social construction of reality to the cognitive functioning of the individual, both at the explicit and tacit levels of knowledge, is inherently symbolic, hence rhetorical.

CONCLUDING REMARKS

McCloskey's book is a welcome addition to an increasingly influential literature critical of positivism in economics. My hope is that the book will be widely read and enjoyed, both by academics and college and university students at all levels. There is much about the book to recommend it for serious study and contemplation, especially by those against whom McCloskey directs his argument. My sense is that McCloskey's book will work to the profession's advantage in the long run.

If this is to happen, however, it will be because McCloskey has effectively brought to the profession's attention issues others will build upon, refine, and extend. *The Rhetoric of Economics* is a provocative beginning that beckons others to follow. Along these lines I have attempted to explore two of McCloskey's themes that to me appear important for his overall argument. I also have suggested in what directions the further study of these themes might proceed.

McCloskey's desire to rid economics of methodological prescriptivism is entirely laudable. Progress and creativity in science are particularly unsuited to cookbook methodologies. But this does not imply that "anything goes." Science is a tradition, and a fairly recent and fragile one at that, which requires a framework of rules that render scientific activity structured but not determined. In order to secure their training, certification, and financial viability, scientists must submit to the rules and standards of the scientific community, even if their intention is to overthrow and replace the existing organization. When science is viewed as a spontaneous

order, such rules serve as constraints—not as prescriptions for scientific activity—that allow diversity to occur within an ordered context.

McCloskey's aim is to persuade the reader that economics is rhetorical. In this sense, he will secure a stunning victory. Unfortunately, however, the success threatens to be transitory because the conception of rhetoric employed in the book is neither compelling as an alternative to modernism nor especially useful for addressing methodological questions. McCloskey so compresses his view of rhetoric that it becomes indistinguishable from a form of literary criticism. Although he draws attention to rhetoric's "fine and ancient" meaning, McCloskey fails to explain what might be its substantive content beyond its use in "understanding how economists speak" (McCloskey, 1985a, p. 29). Rhetoric, to be sure, does this. But the interesting question is whether it can do more.

This question finds a decidedly affirmative response in the rhetorical theory literature. Although a variety of viewpoints are encountered, the dominant theme is that rhetoric is epistemic and hence entails something more than "good conversation." The rhetoric-as-epistemic viewpoints discussed in this paper serve primarily to demonstrate that rhetoricians have engaged this issue in a variety of interesting, if not all equally satisfactory, ways. A large and arguably relevant literature is available for economists interested in exploring the relation between rhetoric and knowledge. McCloskey's apparent reluctance in this regard seems mainly to have compromised unnecessarily the force of his otherwise useful insights.

FOOTNOTES

1. Feyerabend (1970, p. 219, n.1) "playfully" notes: "As a matter of fact, everything that is noticed by some organism . . . will be found in the third world [Lakatos' "world of ideas"] which will therefore contain the whole material world and all the mistakes mankind has made. It will also contain 'mob psychology'."
2. "When Aristotle and Galileo looked at swinging stones, the first saw constrained fall, the second a pendulum" (Kuhn, 1970, p. 121).
3. The notion of spontaneous order elaborated by Adam Ferguson, David Hume and other moral philosophers of the Scottish Enlightenment was applied to a broad spectrum of social phenomena in addition to economics to explain structures "which are indeed the result of human action, not the execution of any human design" (Ferguson, 1966 [1767], p. 137). Although the idea fell from favor in England with the rise to dominance of Benthamite political economy, it gained something of a foothold on the Continent with German critics of rationalism, among them Savigny, Humboldt, Herder, and Dilthey, stressing the historical, evolutionary, undesigned character of law, language, and social institutions.
4. Menger (1950[1871], 1963[1883]), Hayek (1967, 1973). For recent assessments of Menger see Alter (1982) and Butos (1985). Among the several treatments of Hayek that have appeared recently, perhaps the most useful overview is Gray (1984).
5. Campbell (1974, p. 421) says: "A blind-variation-and-selective-retentive process is fundamental to . . . all increases in fit of system to environment." Also see Bartley (1984, Appendix 2).
6. As Polanyi (1966, p. x) puts it: "We can know more than we can tell and we can tell nothing without relying on our awareness of things we may not be able to tell."
7. Kuhn (1970) notes: "But nothing that has been said or will be said makes [the growth of science] a process of evolution toward anything (p. 170, his italics). . . . And the entire process may have occurred, as we now suppose biological evolution did, without benefit of a set goal, a permanent scientific truth" (p. 173).
8. "The 'fuzzy' [a non-pejorative term McCloskey uses to describe himself] doesn't know anything, and therefore has a sharply limited interest in conversations about epistemology" (McCloskey, 1985b, p. 135, his italics). Apparently, conversations among epistemologists are either misguided or non-rhetorical.
9. Farrell (1976) argues that social knowledge is an autonomous body of knowledge based on consensus that is "actualized through the decision and action of an audience" (p. 4). Farrell's conception does

not separate valuing and knowing. Social knowledge is normative since it implies (when accepted) "certain notions of preferable public behavior" (p. 4). Two additional points also can be made: first, Farrell excludes "technical and specialized knowledge" from social knowledge because it is validated by "correspondence to the external world" and is not "actualized through the decision and action of an audience" (p. 4); second, Farrell does not claim that "personal knowledge" is rhetorical. With respect to the first point, Finocchiaro (1977) shows that logical interpretation does not capture the ambiguity of science and, thus, scientific activity may be rhetorically successful because it lacks logical success. Consequently, "rhetoric has an important role . . . in scientific rationality and the rhetorical aspects of science should not be neglected" (Finocchiaro, 1977, p. 112). Also, Simons (1980) argues that scientists are "rhetors in disguise," while Wander (1976) documents instances of the rhetorical character of science. Regarding the second point, Gregg (1981) argues that the social vs personal knowledge dichotomy cannot be maintained because all knowledge is symbolic construction and hence rhetorical.

10. Gregg (1981, p. 138) criticizes Cherwitz by arguing: "Our social behavior is riddled through with implicit meanings. Nor is it the case that we cannot know them or act on the basis of them, for we do it all the time even though we cannot always articulate what we know or why we behave the way we do. . . . The difficulty with the distinction is that if we look at the nature of human knowing, what can be rendered explicit is only a part of knowledge and not a very extensive part at that." Also see Polanyi (1966, 1969) for a detailed treatment of personal (or tacit) knowledge.
11. Weimer (1984) also includes mathematics and logic as being rhetorical. He notes that as an historical matter logic has changed and then goes on to argue that "Before one can recognize a system as logical there are decisions that must be made as to what signs are and how they are interpreted, what formation and transformation rules apply to them, and so on, and these degrees of interpretation are subject to argument and revision" (p. 69, his italics). For an argument that almost all of logic can be revised, see Quine (1960). Bartley (1984, appendix 5) suggests a "revisability criterion" that identifies a minimal logic necessary for engaging in critical argument. All other claims, including non-minimalist logics, are exposed to criticism in argument and potentially subject to revision. Bartley's aim is to maximize criticism while preserving (within an argument) the possibility of an exchange.

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