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The Rhetoric of Science: Strategies for Logical Leaping*

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Is there a rhetoric of scientific discourse? Much has been written on the subject in recent years, but the analysis has generally focussed on specific scientific disciplines or on specific rhetorical topics. I would like to use these earlier studies to map the rhetoric of science itself. To do so I will focus on the feature which distinguishes scientific discourse from all others, namely, the adherence to scientific logic.

In the traditional view, this characterization of science's uniqueness is beyond question. Francis Bacon, for instance, wrote that "logic, which governs by the syllogism, extends not only to natural but to all sciences" (1960, p. 116). John Stuart Mill called logic "the sovereign judge" of all scientific arguments (1888, p. 24). And Max Weber held that:

> All scientific work presupposes that the rules of logic and method are valid; these are the general foundations of our orientation in the world; and, at least for our special question [Tolstoy's query, "What shall we do and how shall we live?"], these presuppositions are the least problematic aspect of science. (1946, p. 143).

More recent discussions of science, many approaching questions of method from an interpretive and anti-positivist stance, have cast doubt on the traditional position. Religion, say, or culture, in this view, is no less logical than is science. But in pluralizing the idea of logic these authors have not discarded it. One such proponent of logical pluralism, Peter Winch, held that "within science or religion actions can be logical or illogical: in science, for example, it would be illogical to refuse to be bound by the results of a properly carried out experiment" (1958, p. 100). Clifford Geertz echoes this sentiment: "To abandon the hope of finding the 'logic' of cultural organization in some Pythagorean 'realm of meaning' is not to abandon the hope of finding it at all" (1973a, p. 405). Thus the vision of science as entailing its own peculiarly logical discourse is still widespread, even as other

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aspects of the traditional view of science have fallen by the wayside.

Now, the essence of scientific logic is the inference--that is, the drawing of a conclusion on the basis of sufficient conditions. This may be formulated most simply as "If A and B are true, then C is true," where A and B are conditions (either premises or evidence or both) and C is the conclusion. (See, for example, Elster, 1978, p. 11; Searles, 1948, pp. 4-5.) Even hermeneutic scientists' conclusions must follow logically from the conditions they set out.

Yet, as Lewis Carroll deftly remarked a century ago, the conditions of an inference can never be perfectly sufficient for the conclusion (see Appendix 1): the principle of inference must be included among the conditions, and also a statement that the principle applies in any given case, and also a statement that the previous statement applies in the case, and so on, infinitely. Or, to make the same point from a different direction: the number of factors which might possibly affect the drawing of a proper inference is infinite, so any given inference must implicitly assume many of these factors to be non-problematic. In both cases, the logical inference is necessarily incomplete; there is, in other words, a logical leap inherent in all inferences. Conclusions are necessarily non sequiturs, from a logical point of view.

The consequences of this finding for scientific argumentation are far-reaching. All scientists need conclusions that follow from previous arguments and evidence. In order to buttress the "follow from" claim, which Carroll has called into question, all scientists face the logical leap.[1]

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1. It may be noted that my argument, too, is based on logical inference, namely that:

> Condition: Science, in the traditional view, is uniquely logical.
> Condition: Interpretive revisions of the traditional view have pluralized the idea of logic, but have not abandoned it.
> Condition: Logic is based on inference.
> Condition: Inference involves a logical leap.
> Conclusion: Therefore, scientific arguments contain logical leaps.

According to my own argument, then, I face a logical leap and my conclusion is lacking in justification. However, the existence of a logical leap does not invalidate my conclusion, since--again according to my own argument--"correct" and "incorrect" are social determinations based at least in part on the persuasiveness of an argument, not on the insufficiency of the argument's conditions, which all arguments share alike. It will be seen that I, too, use rhetorical strategies to cover my logical leap. I leave it to the reader to decide if they are successful.

Scientific rhetoric, in this view, refers to the various textual strategies through which scientists try to deal with the logical leap. Rhetoric is therefore at the heart of scientific argumentation. It is not, contrary to some other formulations of the concept, a mere poetic flourish attached to an otherwise "scientific" argument (as in Sorokin [1956] and Weigert [1970], among others). Nor is it merely a set of formulae accepted by a certain scientific community (as in Knorr-Cetina [1981] and Gilbert and Mulkay [1984]), though certainly communal acceptance is crucial to the success of rhetorical strategies. Rather, scientific rhetoric is a central part of drawing logical inferences. As such, it is part of scientific persuasion, the process whereby particular texts become accepted as containing scientific knowledge.[2]

I'm not going to argue that rhetorical strategies are the sole determinant of the social acceptance of knowledge-claims. Philosophers have championed four sorts of factors which might make a scientific argument persuasive, and I will discuss these factors briefly in the conclusion. In the body of the paper, I plan to analyze the forms of scientific rhetoric, not to prove that these forms are effective. I am going to discuss seven rhetorical strategies; for all but the first strategy I will analyze "classic" articles or parts thereof, on the assumption that successful articles are likely to make successful use of rhetoric. (The procedure for collecting the "classic" articles is described in Appendix 2.) This categorization of rhetorical strategies will, I hope, provide the basis for a meaningful analysis of the acceptance of scientific knowledge--an analysis which includes rhetoric as one among several features.

Rhetorical Strategy 1: Claiming to Be Objective.

> As we have seen, in research papers experimental data tend to be given chronological as well as logical priority. Neither the author's own involvement with or commitment to a particular analytical position nor his social ties with those whose work he favours are mentioned. Laboratory work is characterized in a highly conventionalized manner, as instances of impersonal, procedural routines which are

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2. Here I take exception with Edmondson's optimistic assumption that authors and readers share a common interest in communication (see, for example, 1984, pp. 6, 27-29). Scientific authors, I will assume, have an interest in persuading the reader; readers have an interest in judging the author. These interests do not necessarily coincide, so authors cannot count on readers to cooperate in the leap from conditions to conclusions.

> generally applicable and universally effective. Although the content of experimental papers clearly depends on the experimenters' actions and judgments, such papers are overwhelmingly written in an impersonal style, with overt references to the authors' actions and judgments kept to a minimum. By adopting these kinds of linguistic features, authors construct texts in which the physical world seems regularly to speak, and sometimes to act, for itself. Empiricist discourse is organized in a manner which denies its character as an interpretive product and which denies that its author's actions are relevant to its content. (Gilbert and Mulkay, 1984, p. 56)

This analysis of several biochemistry papers echoes the findings of a number of other analyses: Knorr-Cetina (1981) describes a chemistry paper in similar terms and shows how successive drafts adopt an increasingly impersonal style. Bazerman (1984) reviews a sample of spectroscopic physics articles and finds that complexity and abstraction are on the rise. Nor are such analyses limited to the natural sciences. Frye (1957, pp. 330-331) finds impersonal presentation to be crucial to a 19th-century essay on government. Gusfield (1976, p. 20) argues that public policy research on drinking drivers uses "clinical, detached, depersonalized" language. Anderson (1978, p. 123) shows how articles in the sociology of youth invoke "author authority" to buttress their arguments. Geertz (1988, p. 16) describes a similar dynamic in anthropology: "Ethnographers need to convince us ... not merely that they themselves have truly 'been there,' but ... that had we been there we should have seen what they saw, felt what they felt, concluded what they concluded."

In each of these cases, the authors under analysis portray themselves as objective scientists. They set themselves up in their writings as neutral observers of a given reality, following commonly accepted scientific procedures and drawing the inevitable conclusions. The object of study is made to speak for itself, through the transparent medium of the scientist.

This rhetorical strategy attacks the logical leap in two ways. First, it creates the persona of the objective scientist (see Campbell, 1975) as an authoritative figure whose conclusions are not to be doubted. Second, it promises that any rational being who had followed the same procedures would have come to the same conclusions.

Yet this strategy, despite its intuitive appeal and widespread use, particularly in the exact sciences, is not the only one available to scientific authors. In what follows, I will give examples of six others.

Rhetorical Strategy 2: Obfuscation.

Pierre Bourdieu, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason"

Bourdieu promises, in the second half of the title of this article, to describe the social conditions which will allow reason to progress. He repeats the promise five times throughout the paper (first sentence, p. 19; first sentence of section 5, p. 31; lines six through 11 on p. 32; first sentence of lower paragraph, p. 36; last clause of first full paragraph, p. 40). Thus it is fair, I think, to call this one of the basic arguments of the article:

> Condition: particular social conditions
> Conclusion: the progress of reason

Stated in this stark fashion, Bourdieu's argument would appear to raise a number of hard questions: How can he be sure that certain social conditions will lead to the progress of reason? Have these social conditions existed in the past, and did they then lead to the progress of reason? Is Bourdieu himself benefitting from particular social conditions which allow his reason to have progressed; and if not, why should we believe his argument? In sum, how can the author assure us that his condition leads to his conclusion?

Bourdieu goes about solidifying his logic with the method of obfuscation. It seems odd, given the repeated posing of the question, but the article never actually comes out and says which social conditions lead to the progress of reason. Three times, a generous reading of the text appears to comes close to answering the question. The first time, at page 33, Bourdieu writes,

> The antagonism which is the basis of the structure and transformation of any field tends to become more and more radical and more and more fruitful because the *forced agreement* in which reason is generated leaves less and less room for the unthought assumptions of doxa. (His emphasis.)

The word "fruitful" sets one to thinking that this is the promised "progress of reason." And what causes the fruitfulness? Well, "antagonism" refers to the previous mammoth sentences: it appears to describe the competition of scientists for recognition and importance. The "structure" of a field, as Bourdieu defines it earlier in the article (pp. 27, 29), comprises the hierarchy of authorities within the field, and the relation of the field to other fields. The "transformation" of a field involves the raising of entry costs to the field, the creation of expensive and efficient methods and tools, and the restriction of the field to a few well-equipped scientists (p. 33). Doxa is defined as "the aggregate of the presuppositions which the antagonists regard as self-evident and

outside the area of argument, because they constitute the tacit condition of argument" (p. 34).

In other words, it appears that big-money, high-tech scientists can hold the most fruitful debates, because they have the wherewithal to attack each others' hidden assumptions. But surely Bourdieu is not saying this; it sounds too much like an apology for contemporary natural science. Bourdieu, elsewhere in the article, is bitingly critical of such apologists, who present "the ideal as realized" (p. 40) and naively believe "that productivity and competition are directly linked" (p. 37). So the fruitfulness of natural science must be a side benefit of big money and high technology, not the promised "progress of reason."

The second time Bourdieu hints at the social conditions of the progress of reason is on the next couple of pages: "a particular field's degree of autonomy in relation to external determinations" (p. 34) may, in the extreme, "bring about the necessary imposition of the universal norms of reason" (p. 35). This autonomy, he continues, consists of two elements: "the social demands of the dominant class and the internal and external social conditions of that autonomy" (ibid). This second element suggests that autonomy itself is not the much sought-after social condition, but relies in its turn on social conditions for its efficacy. One would expect the next sentences to detail those social conditions. Instead, Bourdieu goes on to restate, in lengthy and ornate sentences, what he has just said: that some scientific fields are autonomous, and that others are merely pretending. The only new feature is his comment that the social sciences are necessarily less autonomous than the natural sciences, since the social sciences are forced to compete with the political field for the power to represent the social world (p. 36)--but if this passage indicated the social condition necessary for the progress of reason, then reason, at least in the social sciences, would never progress.

Towards the end of the article, on page 40, Bourdieu again holds out the promise for some sort of progress:

> A scientific sociology of science (and the scientific sociology which it helps to make possible) can only be constituted on condition that it is clearly seen that different representations of science correspond to different positions in the scientific field, and that these representations are *ideological strategies* and *epistemological positions* whereby agents occupying a particular position in the field aim to justify their own position and the strategies they use to maintain or improve it, while at the same time discrediting the holders of the opposing position and their strategies. (His emphasis.)

Two elements make up the scientific sociology of science, which appears here to be necessary for the progress of sociology (if not

of reason itself). One element is the attack on science's ties to the dominant class (note 45, p. 45); the other is a holistic view of the scientific field (contrasted with partial views, p. 32). Neither of these elements, though, really fit the bill as "social conditions"--nor does the sociology of science, for that matter.

So we're back where we started: Bourdieu refuses to answer the central question he poses. Every time he raises the question of the social conditions of the progress of reason, his next sentences shift the subject to some tangential subject. On page 19, the discussion veers to what is at stake in scientific struggles. At the bottom of page 31 and the top of page 32, Bourdieu turns to attack other philosophers of science. On page 36 he shifts to attack those who would deny that his question is important. And on page 40, his last chance, he stops to defend himself against charges of relativism.

Several things, I think, make this rhetorical strategy successful.[3] First, the convoluted writing style makes readers feel they have missed something and blame themselves for not following the argument: the author's status relative to the reader is enhanced.[4] Second, the density of the text raises the author's status relative to accessible, commonsensical popularizers (Pease, 1981, p. 266). Perhaps, in addition, Bourdieu shares with James Mill the "peculiarly Victorian sense," which Northrop Frye identifies, "that the more difficult the style, the tougher the moral and intellectual fibre one develops in wrestling with it" (Frye, 1957, p. 330). Lastly, Bourdieu's digressions are fascinating: his observation that reactions to Thomas Kuhn's work depend on one's position in the scientific hierarchy (p. 38); his argument that epistemological conflicts are also political conflicts (p. 21); his point about social science competing with the political field to represent the social world (p. 36). The paper is full of similarly intriguing remarks.

Perhaps Bourdieu is operating according to the adage that people remember digressions more than they remember main arguments. But I've not come across any defense along these lines. Instead, he remarks in the preface to *Distinction*:

> Likewise, the style of the book, whose long, complex sentences may offend--constructed as they are with a view to reconstituting the complexity of the social world in a language capable of holding together the most diverse things

3. Feyerabend (1975, pp. 81-87) and Knorr-Cetina (1981, p. 124), give other examples of scientific obfuscation. In the context of sociology, see Mills (1959, Chap. 2) and Cowley (1956).

4. This ties in with the first rhetorical strategy, setting oneself up to be an objective scientist.

> while setting them in rigorous perspective--stems partly from the endeavor to mobilize all the resources of the traditional modes of expression, literary, philosophical or scientific, so as to say things that were de facto or de jure excluded from them, and to prevent the reading from slipping back into the simplicities of the smart essay or the political polemic" (1984, p. xiii).[5]

This strikes me as an attempt to raise obfuscation to the level of conscious methodology: the social world is tough to understand, he seems to be saying, so my writing will be too. This may just be an excuse, but it may be, to judge by Bourdieu's status in contemporary social science, a successful one.

Rhetorical Strategy 3: Circularity.

Clifford Geertz, "Deep Play:
Notes on the Balinese Cockfight"

This is a beautifully written article. Its prose is simple, smooth, and clear. Its ethnographic evidence--both anecdotal and systematic--is presented richly, so as to grab and hold the reader's interest, and one does not feel manipulated or set under siege, as one might on reading, say, Bourdieu. These surface characteristics, though, differ from the rhetorical strategies which I am discussing in this paper. They differ, first, in that they operate at different levels, the syntactical and the logical; and they differ in that the logical rhetoric of the piece is by no means simple, smooth, and clear. Geertz may be a straightforward writer, but his response to the logical leap is anything but straightforward.

Geertz's rhetorical strategy becomes apparent the moment you try to identify the argument of the piece. Let us take, for example, part of the article's introductory paragraph, located after the introductory anecdote, on page 417:

> But, aside from a few passing remarks, the cockfight has barely been noticed, although as a popular obsession of consuming power it is at least as important a revelation of what being a Balinese 'is really like' as these more celebrated phenomena [mythology, art, ritual, social organization, patterns of child rearing, forms of law, and styles of trance]. As much of America surfaces in a ball park, on a golf links, at a race track, or around a poker table, much of Bali surfaces in a cock ring.

5. In a footnote, Bourdieu then thanks his translator, Richard Nice (who also translated the "Specificity" article) "for having been as faithful to the intention of my style as the demands and traditions of English will allow" (1984, p. 562).

Words like "to reveal" and "to surface" seem to indicate that one can learn about the deeper aspects of Balinese culture by studying the cockfight--similar phrases are repeated in other passages: "the cockfight renders ordinary, everyday experience comprehensible" (p. 443); it is "an example" of Balinese social life (p. 446); it "reveals" Balinese hierarchical sentiments (p. 447); it is "a paradigmatic human event" (p. 450); it is an "eloquent cultural statement" about Balinese life (p. 452). Indeed even the Balinese are said to learn about their society from attending and participating in cockfights (p. 449). This argument might be schematized as:

> Condition: The Balinese cockfight has a certain meaning: that of "deep" status-risking (see, for example, pp. 432, 434).
> Condition: The cockfight is representative of Balinese culture.
> Conclusion: Balinese culture is very interested in status hierarchies (see p. 447).

If this is Geertz's argument, it suffers from a huge logical problem: how can he say that the cockfight is representative of Balinese culture without already knowing what Balinese culture is like? (All synechdochal arguments face this problem, I think--see White, 1973, pp. 35-36.) He can't say that everyone in Bali takes part in the cockfight; he makes no mention of cockfighting in Balinese cities, and even in the villages, women, children, and the poor are excluded. He can't say that the practice is widely accepted; the only evidence presented on this score is negative: a popularly elected national government banned cockfighting.

However, this is not the only argument Geertz is making. The last sentences of the article suggest another one:

> But whatever the level at which one operates, and however intricately, the guiding principle is the same: societies, like lives, contain their own interpretations. One has only to learn how to gain access to them (p. 453).

Here Geertz's argument would seem to be:

> Condition: The Balinese cockfight has a certain meaning, that of status-risking.
> Condition: Balinese culture is also interested in status hierarchies.
> Conclusion: The cockfight is representative of Balinese culture.

And yet elsewhere, Geertz "invokes" (p. 436) the status preoccupation of Balinese society to demonstrate that the cockfight is indeed "a status bloodbath" (ibid):

> The easiest way to make this clear, and at least to some degree to demonstrate it, is to invoke the village whose cockfighting activities I observed the closest--the one in which the raid occurred and from which my statistical data are taken.
>
> Like all Balinese villages, this one--Tihingan, in the Klungkung region of southeast Bali--is intricately organized, a labyrinth of alliances and oppositions. But, unlike many, two sorts of corporate groups, which are also status groups, particularly stand out, and we may concentrate on them, in a part-for-whole way, without undue distortion (ibid).

Here and on the following pages (436-440), the argument is:

> Condition: Balinese culture is interested in status hierarchies.
>
> Condition: The cockfight is typical of Balinese culture.
>
> Conclusion: The cockfight has the meaning of status-risking.

The three arguments I have delineated are circular. In each argument, the conditions are predicated on the conclusions of the other arguments, which are themselves based on the conclusions of the other arguments. Thus, whichever argument you feel is the central one for the paper--the final-sentences injunction about representativeness seems to be the point with which the paper is most often identified--that argument is circular. Its conclusion is presupposed in its conditions.

It might be objected that Geertz's hermeneutic method is not amenable to such logical analysis, that he is not trying to be logical. This objection fails on two grounds: First, Geertz himself, as quoted above, does not wish to abandon logic entirely; he merely wants to pluralize it, to seek internal logics rather than a single universal logic. Second, hermeneutic analysis, for all of its opposition to positivism, still needs conclusions which follow logically from conditions; it has challenged the conditions conventionally used by positivists, but it has merely replaced these with another set of more humanistic conditions. Thus Geertz may fairly be said to engage in a reduction to circularity.

This rhetorical strategy is not merely a flaw in an otherwise excellent article; it is not a flaw at all. Rather, the reduction to tautology is "Deep Play's" strength. It conveys the substance of the argument in the form of the argument: that the cockfight, its representativeness, and the culture are intricately entwined. Elsewhere, Geertz has raised this rhetorical strategy to the level of methodological prescription: the social scientist should tack back and forth, he says, between local detail and larger structures, in order to understand what the natives conceive a person to be, what

general form their lives take, and what vehicles embody that form (1983, pp. 69-70). Circularity has negative connotations, but it is particularly appropriate in this context.[6]

Rhetorical Strategy 4. Citing Authorities.

Ted Gurr, "A Causal Model of Civil Strife:
A Comparative Analysis Using New Indices"

Citing authorities is--along with claiming to be objective--perhaps the most normal of scientific rhetorical strategies, both in the sense of frequent and also in Kuhn's sense of "normal science" (1962, p. 10). This is part of what it means to work within a scientific paradigm: you accept certain previous problems as solved (ibid, p. 47). Yet such acceptance can easily go unstated; the act of giving credit to previous solvers raises new issues. Gilbert (1977, pp. 116-117) lists several reasons for citations: to justify one's position, to demonstrate the novelty or the importance of one's results, to display one's allegiance, or to contribute to consensus. I would like to add another reason: to cover one's logical leaps.

Gurr's article creates "a causal model of the general conditions of several forms of civil strife, using cross-sectional analyses of data collected for 114 polities" (p. 1104). Using multivariate regression (despite the categorical nature of most of his data--a flaw he acknowledges, p. 1106), Gurr shows that indices proxying psychological deprivation ("actors' perceptions of discrepancy between their value expectations ... and their value capabilities," p. 1104) are significantly correlated with indices proxying "civil strife" ("all collective, nongovernmental attacks on persons or property," p. 1107) in the period 1961-1965. There are two sets of arguments here, the substantive and the technical:

> Condition: The higher the levels of psychological deprivation the populace suffers...
>
> Condition: The fewer the non-governmental institutions the populace has with which to press its claims...
>
> Condition: The lower the government's coercive potential...
>
> Condition: The greater the populace's history of civil strife, the more accessible regions are, the larger the Communist Party, and the larger the external support for strife...
>
> Conclusion: The more civil strife a polity will have.

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6. Common sociological tautologies are discussed in Turner and Edgley (1980, p. 600).

> Condition: Each of the above variables is adequately represented by proxy indices.
> Condition: The indices proxying the condition-variables above are correlated in significant ways with the indices proxying the conclusion-variable above.
> Conclusion: The condition-indices caused the conclusion-indices.

It is clear from Gurr's method that the first, substantive (he calls it "theoretical," p. 1104) argument is dependent on the second, technical one. The second argument itself depends on the justification for the "operationalization" (p. 1106) of the theoretical variables, and on the justification for the causal inferences drawn from the patterns of statistical correlation. At both of these critical junctures, Gurr falls back on authorities.

Gurr seems to admit that his indices need explanation:

> Because of the very considerable difficulties of operationalizing a number of the variables, and the fact that most of the indicators constructed are new, this article gives relatively close attention to the data collection and scaling procedures (p. 1106).

So the justification of the operationalization is buttressed with the phrases "Following Blalock's recommendation" (p. 1106), "one can infer from frustration-aggression theory" (p. 1107), "adapted ... from Rummel" (p. 1107), "information was supplemented from a variety of other sources" (p. 1108), "the measure is from Raymond B. Nixon" (p. 1108), "On the basis of prior theoretical and empirical work" (p. 1109), "Coding judgments ... were made on the basis of country studies" (p. 1110), "from United Nations sources" (p. 1111), "two indices are reported in *ibid*" (p. 1113), "recoded from Arthur A. Banks and Robert B. Textor" (p. 1113), "derived from the Eckstein data" (p. 1114).

The recurrent word "from," along with its synonyms, sometimes refers merely to the source of the raw data, saying nothing about the coding to which Gurr subjects them. But even in these cases the references serve to legitimate the enterprise, to show that the indices Gurr creates do not come "from" thin air.

The second matter, the logical leap from correlation to causation, is similarly buttressed by the citation of authorities. Gurr is fairly humble about his use of statistical procedures: the data "are open to further, more refined analysis and interpretation" (p. 1119), and a "number of additional causal inference analyses can be made" (p. 1123). By admitting he is not using state-of-the-art statistics, Gurr sacrifices his authority to make causal inferences from statistical correlations. So he makes the argument himself ("if X1 is an indirect cause of X3 whose effects are

mediated by an intervening variable X2..." p. 1119), and then adds in a footnote: "These and other fundamental arguments about causal inference are well summarized in Blalock" (ibid). The message is threefold: "well summarized" suggests that causal inference is so routine as to have been summarized in many places; further, Gurr seems to be saying, "if you don't believe me, believe Blalock"; lastly, the reference to specific sections of a specific book by a specific publisher meets scientific standards for precision in referencing, as distinct from non-scientific citations of authority.

The citation of authority saves this article from utter incoherence. Without these references to accepted data, accepted indices, and accepted causal inference, the article would stand naked as a somewhat silly attempt to hang numbers on social-psychological generalizations which are themselves dubious (particularly egregious is "institutionalization," p. 1113); as a misapplication of regression analysis to categorical variables; and as a mistaken analysis of causal inference (the allegedly mediating variables fail to eliminate the correlation between deprivation and strife, p. 1120). Gurr's ability to drape himself in the cloak of authority is the only reason, as I see it, that this article is taken seriously--well, that and the reluctance of theorists of revolution to examine quantitative procedures closely.

Rhetorical Strategy 5: Alternative-Knocking.

Carroll Smith-Rosenberg, "The Female World of Love and Ritual: Relations between Women in Nineteenth-Century America"

Smith-Rosenberg sets out to do two things in this article: first, she intends to fill in one gap in the historiographical record (p. 1); second, she wants to suggest a new approach to female friendships in 19th-century America (p. 2). The first goal is accomplished by the very publication of the article, but the second goal is a bit more problematic. Smith-Rosenberg's new approach is to view female friendships "within a cultural and social setting rather than from an exclusively individual psychosexual perspective" (p. 2). She concludes:

> It is possible to speculate that in the twentieth century a number of cultural taboos evolved to cut short the homosocial ties of girlhood and to impel the emerging women of thirteen or fourteen toward heterosexual relationships. In contrast, nineteenth-century American society did not taboo close female relationships but rather recognized them as a socially viable form of human contact--and, as such, acceptable throughout a woman's life. Indeed it was not these homosocial ties that were inhibited but rather heterosexual leanings. While closeness, freedom of emotional expression,

> and uninhibited physical contact characterized women's relationships with each other, the opposite was frequently true of male-female relationships. One could thus argue that within such a world of female support, intimacy, and ritual it was only to be expected that adult women would turn trustingly and lovingly to each other. It was a behavior they had observed and learned since childhood (pp. 27-28).

The logic of this argument can be schematized, I think, as follows:

> Condition: 19th-century American social structure gave many opportunities for same-sex friendships, and fewer for opposite-sex friendships.
> Condition: 19th-century American culture deemed same-sex friendships to be acceptable.
> Conclusion: Many 19th-century American women chose to have life-long same-sex friendships.

The logical leap here has to do firstly with evidence: Were same-sex relationships really as acceptable as Smith-Rosenberg states? Were they really as widespread? In addition, there is the question whether the acceptability of such friendships actually mattered to 19th-century American women. Do people generally choose their friendships and censor their personal letters and diaries (Smith-Rosenberg's primary source material) in accordance with social norms? Similarly with opportunity: do people usually fall in love with the people they happen to come in everyday contact with?

Smith-Rosenberg deals with these problems by staging a recurrent attack on what she calls "psychopathological" explanations of same-sex friendships (p. 2). First, she summarizes these alternative explanations: they set up a dichotomy between normal and abnormal, and they attribute deviant, same-sex relations to "childhood or adolescent trauma" (p. 2). Then she starts to chip away at them: they "are frequently contradictory or based on questionable or arbitrary data" (p. 2). They "have been subjected to criticism both from within and without the psychological professions" (p. 2). Their dichotomy of deviance and normality does not capture 19th-century emotions and attitudes (p. 8). Indeed, Smith-Rosenberg goes on to vilify any explanation located in the individual:

> the scholar must ask if it is historically possible and, if possible, important, to study the intensely individual aspects of psychosexual dynamics. Is it not the historian's first task to explore the social structure and the world view which made intense and sometimes sensual female love both a possible and an acceptable emotional option? (p. 8).

Wherever Smith-Rosenberg mentions her social-structural explanation of same-sex friendships, she mentions her psychopathological whipping-boy as well for contrast (pp. 2-3, 8-9, 27-28).

The benefits of such a rhetorical strategy seem obvious. Attacking alternative explanations makes one's own seem stronger by comparison. Attention is drawn away from the weaknesses of one's own arguments. There is even a justification for this strategy in the philosophy of science: theories are supposed to be judged against other theories, not against the "facts" per se (see for example Finocchiaro, 1980, pp. 421-423). Indeed, treating alternatives *as theories*, and holding them to "scientific" standards, distinguishes alternative-knocking from non-scientific vilification.

Alternative-knocking seems particularly conducive to a field, such as feminist history, which is relatively new, fairly radical in its approach and implications, and often scorned by the more powerful mainstream competition. The potential vehemence of this rhetorical strategy may attract much-needed attention, and the practitioners of such a strategy may feel that it is the only one left to them by an academic milieu which has summarily dismissed more temperate approaches.[7]

In addition, alternative-knocking seems conducive to a field which sees academic research as part of a political struggle, since the usual restraints of professional courtesy and dispassion may be violated in political debate. Smith-Rosenberg, for example, appears to be describing 19th-century female friendships in order to critique contemporary attitudes towards homosexuality: her last sentences compare the Victorian era favorably with our own, in terms of flexibility towards motion along the homosexual-heterosexual spectrum (p. 29). The attack on the Freudian dichotomy between deviance and normality is thus not just a matter of interpreting the letters and diaries of several dozen 19th-century women; it is also a contribution to the many-faceted feminist program of social change.

Rhetorical Strategy 6: Exhortation.

Alvin Gouldner, "The Sociologist as Partisan: Sociology and the Welfare State"

Gouldner is making several arguments in this paper. The first is that "glib rejection" of the myth of value-free social science does not solve the very real problems of social scientific objectivity (p. 27). Another is that glib rejections of objectivity

7. Thanks to critics at the "Inside Berkeley Sociology" graduate student conference for the points in this paragraph.

play into the hands of national elites (p. 41). In making these points, Gouldner relies primarily on a strategy of alternative-knocking, using Howard Becker as his whipping-boy.

A further argument seems to be that objectivity is in fact attainable, through what he calls "moral character" (p. 60). I would like to narrow the focus to this relatively minor portion of Gouldner's paper. The scheme:

> Condition: If the social scientist has a "moral character"...
> Conclusion: The social scientist can achieve objectivity.

Yet Gouldner has problems defining both of the key terms. The moral element is referred to variously as "humanistic understanding" (p. 52), as "reflective and tempered partisanship" (p. 53), as "struggle in and with the sociologist's self" (p. 60), as "a concern with the serious kind of morality" (p. 61), and as "value commitments that call for something more than pure truth alone" (p. 65). In sum, though, a general impression emerges of a morality which avoids complacency:

> For it is complacency which allows us to think, *a la* Myrdal, that we have solved the problem of objectivity by good-naturedly confessing that, yes, we do indeed have a standpoint and by openly specifying what it is. Confession may be good for the soul, but it is no tonic to the mind (p. 54).

I will restrict my objections to this concept to a footnote.[8]

Objectivity, too, goes through various definitions. It may consist of honoring opposing viewpoints (p. 53), or of honoring one's own standpoint and not those of the subjects of study (pp. 56-57), or of judging on the basis of norms ("normative objectification," p. 57), or of avoiding self-deception ("personal authenticity," p. 59), or of seeking to tell "the 'whole' story" (p. 66). Gouldner never actually dismisses any of these conceptions of objectivity (the only conception dismissed is that of "transpersonal replicability," pp. 61-63). However, the final grand passages of the article seem to favor the last image, tha of objectivity as wholeness.

8. How can you know when you have achieved the noble state of non-complacent morality? More to the point, how can you know if others lack this morality? In my mind, Gouldner is too quick to condemn others for complacency and to congratulate himself for the lack thereof.

So we may rewrite the article's second argument as:

> Condition: If the social scientist has a non-complacent morality...
> Conclusion: The social scientist can achieve an objectivity which "tells the 'whole' story."

The logical leap seems obvious to me: why should a self-critical moral attitude ensure that social scientists are going more successfully "to fit the partial and broken fragments together; to provide a picture that transcends the nagging sense of incompleteness; to overcome the multiplicity of shifting perspectives" (p. 66)?

In any case, Gouldner's last six paragraphs drop all references to "moral character." His final argument for objectivity has nothing to do with the logic of self-reflective partisanship he has been developing for the previous 15 pages. Instead, he translates the search for the whole into normative terms and commands obedience.

His first step takes "telling the 'whole' story" and turns it into "the hope of dissolving the differences that divide and the distances that separate men by uniting them in a single, peace-bringing vision of the world" (p. 66). The second step links this "peace-bringing vision of the world" with both religion and early-nineteenth-century positivism: "In other words, the realm of objectivity is the realm of the *sacred* in social science" (ibid, his emphasis).

A short footnote denies that Gouldner plans to exhort us to embrace this sacred objectivity: "It should be noted that this is not a 'call to religion' but a statement concerning the imputed meaning of 'objectivity' to conventional sociologists" (p. 68n). But Gouldner proceeds to link objectivity with "peace-bringing human unity" (p. 66). Who could be against peace-bringing human unity? Gouldner tells us who: the "power-tempted," the overly frail, the macho types, the rightfully humble--plus, "oddly enough," some social scientists who "doubt the very value of peace itself" (p. 67).

The reader is not to be found in this rogue's gallery of peace-and-unity haters. No, the next paragraph speaks to readers: "Perhaps what has been most discrediting to the quest for human unity is that, since its classical formulation, its most gifted spokesmen have often had totalitarian proclivities" (ibid). "Quite justifiably," Gouldner says, we may have mistaken peace and unity for its "nightmare form" (ibid). Now, of course, the mistake has been cleared up, and--footnotes be damned--we are presented with the underlying choice: "Whether objectivity is thought possible comes down then to a question of whether some vision of

human unity is believed workable and desirable" (ibid). Gouldner has left out Apple Pie and Motherhood, but human unity should do the trick. Just in case, the last sentence of the paragraph comes out against human suffering too. Gouldner gets in one last swipe at Howard Becker and his ilk, then sums up pithily: "It is to values, not to factions, that sociologists must give their most basic commitment" (p. 68).

This final sentence is a gem. I should admit right off that I have no idea what it means; I suspect that Gouldner used it simply because it sounds so nice, especially the wordplay on Weber's "values and facts" (1946).[9] But two features stand out, regardless of the meaning of the sentence. First is the choice presented: if you are not committed to values, then you must be committed to factions. Even if the preceding discussion of human unity were not being alluded to, nobody in the English-speaking world is in favor of factions: the word is practically reserved these days for condescending reference to squabbling revolutionaries. The options presented take the form, "Are you going to eat your peas, or do you want to burn in hell?" As in the human-unity passages, Gouldner forces us to agree with him by giving us highly unbalanced choices.

The second feature of the sentence that I want to point out is the word "must": "sociologists must give their most basic commitment" to values. This seems to me a dead giveaway: the only way to convince the reader that moral character will lead to holistic objectivity is to command acceptance of the argument. Bazerman (1981, p. 378) has identified the same rhetorical strategy in an article by Robert K. Merton: "The sociological audience, sharing no uniform framework of thought or criteria of proof, must be urged, persuaded, and directed along the lines of the author's thoughts." Friedrichs (1970, Chs. 3, 5-6) argues that the priest and the prophet are widespread roles of the sociologist. Weimer (1977, p. 12) sees injunction as common to science in general:

> Both scientific articles and research training given to novices *enjoin* their audiences to behave in a certain way. In this regard science very literally is a 'cookbook' endeavor--it is a matter of recipes for conceiving, perceiving, and doing, and the recipes are given as injunctions. Scientific communication becomes a set of commands that will enable the researcher to have the appropriate experience...

9. Do "values" refer to the preceding discussion of objectivity? To "moral character"? To human unity? To value-laden sociology (which even Becker supports)?

In all these cases, scientific exhortation is distinguished from non-scientific exhortation only by the author's status as a scientist.

Rhetorical Strategy 7: Throwing One's Hands Up.

Robert K. Merton, "The Unanticipated Consequences of Purposive Social Action"

Merton is trying to develop, out of the philosophical, theological, technological, and other scattered theories on the subject, a simple categorization of the factors leading to unanticipated consequences. He lists five factors: inadequate knowledge, error, the pursuit of immediate interests (ignoring longer-term interests), the pursuit of basic values (sometimes changing the basis of these values), and public prediction (influencing future actions). His argument, then, in schematic form, is:

> Condition: five factors
> Conclusion: unanticipated consequences

Yet Merton gives no concrete evidence of the effects of his five factors. He gives no argument to rule out alternative factors (for instance, such superhuman influences as Vico's Hand of Providence, Hegel's Cunning of Reason, or Kant's Plan of Nature). He makes no reference to the possible interactions between the five factors. He does not deal with the methodological problems he describes in the first section of the paper. Instead, he admits his faults:

> For the same reason of limitation of space, I have had to eliminate most of the concrete material upon which the discussion is based.... It must be freely admitted at this junction that these [methodological] problems have not been further treated in the ensuing discussion... (pp. 894n, 898).

He deprecates his article ("The foregoing discussion represents no more than the briefest exposition...", p. 904), and he concludes:

> If the present analysis has served to set the problem, if only in its most paramount aspects, and to direct attention toward the need for a systematic and objective study of the elements involved in the development of unanticipated consequences of purposive social action, the treatment of which has for much too long been consigned to the realm of theology and speculative philosophy, then it has achieved its avowed purpose (p. 904).

In sum, Merton throws his hands up with an implicit, "Oh, well, this is the best I can do." He admits that his article does not

exhaust the possible factors producing unanticipated consequences;[10] he requests that readers not disapprove of his argument merely because it lacks evidence; and he promises that future research will substantiate his claims.

The appeal of such an approach seems to lie in its good-natured honesty. It does not presume to have all the answers, or to have presented an airtight case, or to have solved the logical leap. In admitting their limitations, authors pursuing this strategy can make themselves seem eminently reasonable and thoroughly open-minded. Thus throwing up one's hands creates a persona of the credible scientist in a parallel manner to the objective-scientist strategy (see Becker, 1986, pp. 36-37). Both deny the scientist's commitment to anything but the truth; both attempt to let reality speak for itself (though one strategy apologizes for doing so imperfectly); and both paint a picture of the scientist as someone to be believed. In both cases, also, the persona of the credible scientist has only a passing likeness to the personalities and behavior of actual scientists.

It is perhaps worth noting that Merton was still a graduate student when he published "The Unanticipated Consequences." The humble persona he adopts might be particularly suited to the graduate student's relative lack of power, seniority, and credentials.

Conclusion.

The bulk of my paper is similar in form, if not in stature, to Merton's. I have presented a categorization of rhetorical strategies, and I hope that future analyses will support or modify this categorization. I do not pretend to have come up with any authoritative answers, though I hope to have come up with some mildly persuasive ones. In conclusion, I would like to try to head off three objections to my arguments.

The first of these objections is: "You are anti-science (or anti-left or anti-feminist) and your analysis is an attempt to discredit this worthy enterprise." In response, I would like to point out that I found almost all of the articles I analyze to be persuasive and largely correct. Indeed, their persuasiveness is part of what led me to these articles. Apart from my personal views, the analysis of an article's rhetoric discredits the article only if one holds the outdated belief that "good" arguments have no rhetoric

10. Part of this tentative approach is what Sacks (1963, p. 10) calls "the etcetera problem": "To any description of a concrete object (or event, or course of action, or etc.), however long, the researcher must add an etcetera clause to permit the description to be brought to a close."

at all, that only "bad" arguments have recourse to such unscientific pandering. It should be clear from my introductory argument that I believe that all scientific arguments contain rhetorical strategies. It is not a question of discrediting an argument by revealing its rhetoric; rather, it is a question of revealing the rhetoric so as to discover why an argument is so credible.

The second objection is: "Rhetoric isn't the only factor in the reception of an article; you ignore the other factors." I don't believe rhetoric *is* the only factor in the acceptance of knowledge-claims. I only focus on it because it has been insufficiently analyzed, and thus cannot properly compete with the other factors which studies have identified as affecting the acceptance of knowledge-claims. These include:

> 1) Its truth: an argument with good evidence or explanatory power or breadth might have an advantage over arguments lacking such a close relation to the truth.
> 2) Its novelty: a new approach to an old problem, or the application of an old approach to a new problem, might attract more adherents than more traditional arguments.
> 3) Its social context: a given social group (a nation, a class, a scientific paradigm, etc.) might be predisposed to accept particular scientific arguments.
> 4) Its rhetorical power: an effectively worded argument might be more persuasive than an ineffectively worded argument.

These factors are no doubt related in complex and interconnected manners: for instance, I have already mentioned the likelihood that a particular social context (radical underdog) might be conducive to a particular rhetorical strategy (alternative-knocking). I don't believe, however, that rhetoric can be entirely subsumed into the other three factors.

Third: "You never show that a scientific article's rhetoric actually has an effect on its reception." This is true; I have undertaken no audience studies to analyze the four factors-- truth-content, innovation, social context, and rhetoric--and their relative, combined, and interrelated effects. But this is because a preliminary project had to be accomplished first, namely an analysis of the rhetoric of science which would allow it to be considered on a par with the other three factors. I hope to have accomplished this preliminary task, so that the more important questions about the creation and acceptance of knowledge-claims can now be approached.

Appendix 1: The Logical Leap.

The illogic of logical inference is a somewhat complicated matter, so its consideration is shunted to this appendix. I know no better way to explain the point than to cite virtually the entire Lewis Carroll (Charles Dodgson) essay which converted me to the view. Two characters, the Tortoise and Achilles, are discussing the First Proposition of Euclid. The Tortoise begins:

> "Well, now, let's take a little bit of the argument in that First Proposition--just *two* steps, and the conclusion drawn from them. Kindly enter them in your note-book. And, in order to refer to them conveniently, let's call them A, B, and Z:
>
> (A) Things that are equal to the same are equal to each other.
> (B) The two sides of this Triangle are things that are equal to the same.
> (Z) The two sides of this Triangle are equal to each other.
>
> "Readers of Euclid will grant, I suppose, that Z follows logically from A and B, so that any one who accepts A and B as true, *must* accept Z as true?"
> "Undoubtedly...."
> "And might there not *also* be some reader who would say, 'I accept A and B as true, but I *don't* accept the Hypothetical'? ... Well, now, I want you to consider *me* as a reader of (this) kind, and to force me, logically, to accept Z as true...."
> "I'm to force you to accept Z, am I?" Achilles said musingly. "And your present position is that you accept A and B, but you *don't* accept the Hypothetical--"
> "Let's call it C," said the Tortoise.
> "--but you don't accept:
>
> (C) If A and B are true, Z must be true."
>
> "That is my present position," said the Tortoise.
> "Then I must ask you to accept C."
> "I'll do so," said the Tortoise, "as soon as you've entered it in that note-book of yours...."
> "If you accept A and B and C, you *must* accept Z."
> "And why *must I*?"
> "Because it follows *logically* from them. If A and B and C are true, Z *must* be true. You don't dispute *that*, I imagine?"
> "If A and B and C are true, Z *must* be true," the Tortoise thoughtfully repeated. "That's *another* Hypothetical, isn't it? ... I'm quite willing to grant it, as soon as you've written it down. We will call it

> (D) If A and B and C are true, Z must be true.
>
> "Have you entered that in your note-book?"
> "I *have*!" Achilles joyfully exclaimed, as he ran the pencil into its sheath. "And at last we've got to the end of this ideal race-course! Now that you accept A and B and C and D, *of course* you accept Z."
> "Do I?" said the Tortoise innocently. "Let's make that quite clear. I accept A and B and C and D. Suppose I *still* refuse to accept Z?"
> "Then Logic would take you by the throat, and *force* you to do it!" Achilles triumphantly replied. "Logic would tell you 'You can't help yourself. Now that you've accepted A and B and C and D, you *must* accept Z!' So you've no choice, you see."
> "Whatever *Logic* is good enough to tell me is worth *writing down*," said the Tortoise. "So enter it your book, please. We will call it
>
> (E) If A and B and C and D are true, then Z must be true.
>
> "Until I've granted *that*, of course, I needn't grant Z. So it's a quite *necessary* step, you see?"
> "I see," said Achilles; and there was a touch of sadness in his tone. (Carroll, 1936, pp. 1226-1229)

Lewis Carroll's characters go at it for months, and it is clear they will never reach Z. The moral of the story: No conditions for a logical conclusion will ever be logically sufficient. Or, conversely: If you accept a logical conclusion as justified, you are doing so in part on non-logical grounds. These grounds may be very noble, but they are not themselves "logical."

Appendix 2: Designating the Classics.

I have no intention of creating a canon for the social sciences. I merely wanted to find articles which were successful, because I felt that successful articles would have something to teach about successful rhetorical strategies. Without going to the trouble of surveying social scientists (and that *would* increase the pressure to create a canon), I saw only three alternatives for gauging the success of an article. I could bestow the classic status myself, on the basis of my observations in the field of sociology. I could rely on the commendation of particular well-respected social scientists. Or I could count up citations from the *Social Sciences Citations Index* (SSCI), on the premise that classic articles must be dealt with by subsequent writers in a field, and are thus most often cited. I have used all of these techniques.

"Specificity" is Bourdieu's most frequently cited article, at least in the literature surveyed by the SSCI. This distinction, combined with Bourdieu's status as a trendy social-science guru, elevates the article, I think, to something approximating classic status.

Geertz's "Deep Play" and Gouldner's "The Sociologist as Partisan" both achieved their widest distribution as chapters in books, and are thus not amenable to citation-counting in the SSCI, which often omits the chapter reference for books cited. In any case, Gouldner's earlier article, "The Norm of Reciprocity: A Preliminary Statement" (*American Sociological Review*, Volume 25, April 1960, pp. 161-177) is cited far more often than even *For Sociology*, the book in which "Partisan" appeared. My primary evidence for the papers' classic status, aside from the positive comments which several professors have made about them, is that both were assigned in two classes in as many years at the U.C. Berkeley Sociology Department (classes I happened to be taking). Similarly, Merton's paper was recommended to me as a classic by two U.C. Berkeley Sociology Department professors.

Gurr's paper, "A Causal Model," is not particularly classic, I'm afraid. It is much less well known than Gurr's book, *Why Men Rebel*. Nonetheless, Gurr is one of the key figures in the theoriography of revolution: the concept of relative deprivation with which he is associated keeps cropping up, often unattributed, in more recent theories, such as Tilly's (1978, p. 204), Skocpol's (1979, pp. 63, 93, 145), and Bourdieu's (1984, pp. 143-144, 168). And this is Gurr's most often-cited article in the SSCI (and cited twice in Skocpol, 1979, pp. 296, 319).

The Smith-Rosenberg article, her most often-cited and the first piece in the first issue of a major new journal, received the appellation "classic" from Adrienne Rich (1980, p. 631n).

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