Davida Charney

Empiricism Is Not A Four-Letter Word

A new preoccupation with research methodology and its implications has overtaken composition studies, particularly in the area of technical and professional communication. As in previous visits to this topic, the major concerns are whether empirical methods have any legitimate place in composition studies, and, if not, how we are to achieve intellectual authority without them. The earlier debate—highlighted by exchanges among James Berlin, Patricia Bizzell, Robert Connors, and Linda Flower—pitted on-going investigations of students' writing processes against calls for studies of the social contexts for writing. Lester Faigley's corresponding call for ethnographies of writing in the workplace was eagerly taken up in technical and professional communication, perhaps because non-academic contexts are so integral to the field—with students about to enter professions, faculty working off-campus as writing consultants, and genres that address audiences from technical specialists and managers to the general public. A new feature of the debate is that the acceptability of socially-situated studies themselves is being called into question. Another new issue, at least for nonacademic writing, has to do with the political implications of research. Technical writing researchers have tended to care more about how to improve manuals for computer users than about how their success might enrich Bill Gates. Recently, unease with the direction of technical and professional writing has led scholars like Nancy Blyler, Paul Dombrowski, Carl Herndl, and Mary Lay to criticize both the methods and the topics of research. They fault the discipline for not purging itself of lingering scientific propensities and for wavering

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resistance to dominant ideologies in the academy and the workplace. The current critique of science within the technical and professional writing community is interesting not only for its similarities to concerns in composition as a whole, but also because 15 years of studies of scientific rhetoric by members of this community have produced important insights about science and its methods that bear on this debate.

A significant recurrent issue involves the motive for using scientific methods. In her recent critique, Elizabeth Flynn revives the familiar claim that compositionists have historically emulated the "masculinist" techniques, beliefs, and attitudes of more powerful fields, such as the sciences and social sciences, in an attempt to overcome marginalization and increase our status (354). Similarly, Mary Lay claims that "in affiliating with scientific positivism and in defining itself as the objective transfer of data, truth, and reality, traditionally defined technical communication ranks higher than other supposedly subjective types of writing, engages in dualistic thinking, and maintains closeness with patriarchal institutions of power. Therefore to enhance legitimacy for their field, technical communication scholars and teachers may resist redefinition that divorces technical communication from this source of power" (358). Flynn and Lay posit that rejecting a scientific outlook jeopardizes intellectual authority in the academy, suggesting that those who maintain such outlooks must have venal motives. Other critics, such as Nancy Blyler and Carl Herndl, question the motives of those who conduct research in the workplace, suggesting that they are easily co-opted by the managers and administrators who sponsor or permit the research.

Certainly, the research methods we employ have important consequences for the intellectual authority of our field, for the ethical, political, and intellectual value of our work, and for its potential to effect beneficial changes in the classroom and the workplace. But recent work in the rhetoric of science suggests that the motives and consequences of methodological choices are more complex than these critics have assumed. I will argue here that critics of science often conflate methods and ideologies in simplistic ways that have been challenged by others sharing their political commitments. It seems absurd to assume that anyone conducting a qualitative analysis or ethnography must be compassionate, self-reflecting, creative, and committed to social justice and liberation. Or that anyone who conducts an experiment is rigid and unfeeling and automatically opposes liberatory, feminist, or postmodernist values. But such assumptions underlie the current critiques—including the rising suspicion of the ideological commitments of ethnography—when other consequences of over-reliance on qualitative methods are more serious. Rather than endorsing or condemning methods a priori by ideological purity, we should consider how they affect our ability to work with each other to conduct the very best research we can and to expand our understanding of academic and nonacademic discourse.

In the world view that the critics offer, intellectual authority becomes a commodity that the academic elite buys into at will. With the means of producing authority unfairly monopolized by scientific disciplines, empirical researchers in composition are portrayed as petty sycophants, imitating scientific merchandizing in a futile effort to attract a better market share. In rejecting this perspective, I argue that no research method per se can deliver up authority or acceptance. Rather, credence—and provisional credence at that—emerges from day-to-day critical negotiation in which disciplines identify interesting questions, decide what kinds of answers to consider, and actively critique both methods and results. Drawing on philosophical, historical, and rhetorical studies of science, I will argue that the very qualities that the critics most object to in scientific work are those that afford the most productive communal discussion. Conversely, the qualities that the critics most laud in subjectivist methods may also inhibit our ability to attain the intensive cooperative focus we need for defining and solving disciplinary problems. Consequently, by disparaging objective methods and advocating increasingly subjectivist methods, we may also be impairing our ability to improve our own work and use it to promote social justice.

Radical Mischaracterizations of Science

Critics of science in composition studies often treat it as a timeless, unitary ideology that blends objectivity and quantitative analysis with whatever other philosophies they dislike. For example, Blyler concocts a position that has been "variously labeled functionalism, empiricism, rationalism, positivism, or modernism" (288) which she also associates with realist materialism, absolutism, and utopianism. Similarly, while disparaging simple dualities, Dombrowski calls for the "diminution" or "disprivileging of received specialized knowledge; the rejection of foundations and foundational authority; the rejection of rationalism; even the rejection of the very idea of science including its claims to objectivity, absoluteness, and disinterestedness" (167).

In more sophisticated critiques of science and objectivity, like that of historian Theodore Porter, these merged strands are acknowledged to be largely extricable. Porter notes that Karl Pearson, a founder of modern mathematical statistics, was a positivist but also an anti-materialist and anti-elitist. Pearson advocated objective methods—and quantification in particular—on socialist grounds. He wanted to avoid relying on the perceptual experiences of the elite few. Objective methods, and the information they yield, tend to be public, available for acquisition and scrutiny, while personal knowledge (especially of elite experts) tends to be closed and exclusive (Porter 20–21, 74–75). Similarly not all empiricists are positivists or absolutists. Karl Popper, the critical empiricist who advocated holding theories to tests of falsifiability, opposed positivism and denied that science entails scientism, a dogmatic belief in the automatic authority of scientific methods and results (Popper, *In Search* 41). For Popper, science in no way depends on absolutism; all findings and claims are subject to subsequent challenge from the community. It is precisely because individual scientists are biased and cannot be trained to be neutral that they need objective methods that are open to scrutiny and challenge. Popper defines scientific objectivity as "the inter-subjectivity of scientific method" (*Open* 217, 374–75).

It is equally misleading to equate subjectivity with ethnography and qualitative analysis—the fact that ethnographies are not inherently ideological is a matter of increasing concern to critics like Herndl and Blyler. Qualitative methods in fields like linguistics, history, and anthropology are often objective and systematic. Conversely, subjective personal insights and experiences have long played an important role in "hard" sciences. Objectivity then is not a fixed feature of particular methods. Rather, as Porter describes, it is a means for large communities that lack extensive personal interaction to reach provisional consensus by holding personal and professional judgments in check by appeal to shared rules and procedures, which in the sciences are often formalized and involve quantification (4–8). While objective procedures run the risk of devolving into mechanical rule-following, they help to sustain disciplinary cohesion and foster criticism.

While absolutism and positivism are not intrinsic to science, the assumption that they are underlies much of the uneasiness about science. A crude sketch may help to sort out the points that call for response. To begin with, critics don't like the ethos of the scientist as constructed in scientific discourse; they see the stance of impersonal competence as a hypocritical mask to cover outright prejudice, self-interest, or complacent support of the status quo. Even worse, such studied neutrality either ignores or condones political and social injustices in which scientific work and practices are implicated. Critics also dislike the artificial manipulation of natural phenomena. They see the move to the laboratory as a retreat from the complexity and richness of the natural world which defies such heavy-handed control—or would do so if it actually existed. Critics of the social sciences don't like the apparent determinism underlying efforts to explain or predict human behavior, and they don't like the concept of abstracting over large numbers of faceless "subjects." They don't like the concept of the "normal distribution" (the familiar bell curve) that is the basis for many statistical analyses—especially nowadays when the upper and lower tails of the curve are being politicized in ways we haven't seen since the heyday of Social Darwinism. And no one likes the way scientists seem to privilege numbers and disparage words—the way numerical and graphic evidence is treated as clean, precise, and solid, while narratives and descriptions are treated as unreliable, biased, and squishy. The critics see objective, quantitative, and empirical methods as ways for scientists to avoid interpretation, eliminate the human element of subjectivity that supposedly contaminates the study of individual cases, and go on misrepresenting the world as manageable, fully determinate, and reducible to clear and accurate formulas.

Implicating Science in Injustice

The most important charge against science may well be that it is implicated in injustice, both in its internal practices and in its effects on society. Critics making such sweeping generalizations about science often exhibit their own propensity to dehumanize the Other, imputing bad motives to anyone who uses experimental and other quantitative methods. They claim that objectivist research reproduces the material and social conditions in which it occurs, a position that both assumes inequity and precludes reform. Some assert that scientists know exactly what evils they perpetrate; others see them as oblivious or callous dupes of industrialists and bureaucrats. This position criticizes science for its "instrumentalism," a charge that David Shumway identifies in Berlin's critique of cognitive rhetoric. More recently, Blyler associates "functionalist" research of workplace communication with conservativism, bias toward management, and valuing of profit over employee welfare (294). And Flynn claims that

> beliefs in the objectivity of the scientist and the neutrality of scientific investigation serve the interests of those in positions of authority and power, usually white males, and serve to exclude those in marginalized positions. Identification by women or by feminized fields with the sciences and social sciences, therefore, may necessitate association with discourses that ignore issues of concern to those in marginalized positions and that arise out of epistemologies antithetical to their needs and interests. (358)

Key to the charge of injustice is the idea that objective methods are sexist. For Blyler, Flynn, and Lay, feminism is consistent only with subjective, qualitative, narrative, ethnographic research because of the "inherent sexism" of science (Blyler 289). Lay claims not only that feminist critiques have "expose[d] the scientific positivist and androcentric bases for scientific objectivity" (349), but that "feminist traits are inherent in contemporary ethnographic methodology" in their narrative, impressionistic accounts of lived experience (360). Dombrowski claims that "the privileging of scientific knowledge in our society is not neutral or innocent because it disprivileges other sorts of 'knowledge' such as intuitions, traditions, and personal experience." Flynn says that "feminist critiques of the sciences and social sciences suggest as well that these fields may be especially inappropriate or dangerous models for feminized fields, that is fields in which women are disproportionately represented" (358)—notably composition.

Some who seek to represent the feminist position on science merely repeat old charges second-hand, ignoring both genuine efforts at reform and the range of feminist positions. Flynn's representation actually borders on the irresponsible: she attributes a litany of feminist objections to quantitative methods to Toby Jayaratne and Abigail Stewart (359), ignoring their explicit intention to reject such critiques as counter-productive, essentialist, and largely unsupported by the evidence. In fact, Jayaratne and Stewart's goal was to "emphasize the value of quantitative methods as effective tools to support feminist goals and feminist ideologies" (85). Along with feminist sociologist Barbara Risman, psychologists Letitia Peplau and Eva Conrad, and others, they deny that methods can be inherently misogynist or androcentric. These feminists point out that sexism and other injustices have often been most effectively exposed by quantitative studies that provide stronger evidence of the prevalence of a problem and its trends than can individual testimony. They warn that feminist attacks may perpetuate the under-representation of women by discouraging them from entering scientific fields or using experimental methods. They regret the doctrinaire attitudes of some feminists, citing a prominent feminist sociologist whose survey research on wife abuse was rejected by a feminist journal on the grounds that quantitative methods are "inherently patriarchal" and "could contribute no feminist insights" (Peplau and Conrad, 380).

Porter makes the complementary point. Scientific collaborations have historically transcended political, religious, racial, and national boundaries. While conceding that the record is spotty on the inclusion of women, he notes that "the net effect of the modern emphasis on quantification has probably been to open up professional cultures to women and ethnic outsiders," (76) by creating alternative entrance standards that diminish the power of exclusionary and elitist networks of clubs and informal contacts. By bringing both criteria and results into the open, scientific methods tend to expand the circle of professionals to include those not previously considered eligible. For example, Philip Kitcher describes how Darwin, by soliciting reliable reports from plant and animal breeders, beekeepers, and pigeon fanciers, paved the way for the wide-spread adoption of evolutionary theory in agriculture (34). Further, the impersonality of scientific discourse arguably fosters greater acceptance of work by women.

For instance, using initials rather than first names in reference lists reduces the effect of biases against women (Paludi and Strayer).

The more general charge of injustice is that scientific approaches in the lab or in the workplace serve the interests of oppressive power structures (Blyler 294; Flynn 358). Some critics assume that scientific methods work—that scientists really can manipulate the world sufficiently to make it come out the way they or their masters would like. But the assumption of efficacy is not necessary to radical critique. Hostility toward science is possibly strongest among hyper-constructivist anti-realists, who deny that science works and reduce it to an extravagant and expensive fantasy, a construction of the scientists themselves. How could scientists manage to attract so much power and prestige in and out of the academy unless they are directly involved in political domination? Responding to this charge requires a detailed dissociation of indeterminism from irrationality.

The Equation of Indeterminacy and Power Politics

Radical critics begin by imposing an ideal standard on science. They insist that, to call itself a rational and progressive endeavor, science must always proceed in a strictly orderly and rule-governed fashion. Then they show that the course of science cannot be completely accounted for by an explicit set of logical standards. Followers of Feyerabend cite the diversity of methodological rules, the scarcity of universal principles, and the presence of variance in every set of data, as signs that scientific beliefs and actions are fatally underdetermined by "objective" factors. Insisting that any result short of Universal Truth and Certainty is a failure on science's terms, the critics conclude that objective methods are a sham and that scientific knowledge cannot grow. They deny that any rational grounds underlie the development of theories in science and conclude that it must really be governed instead entirely by social dynamics and power politics. Philosopher Richard Watson traces such "dogmatic skepticism" from Hegel and the 19th century British idealists to Heiddeger to Derrida. His description of radical critique as mounted in archeology provides a useful summary:

> All social interactions, including the practice of archeology, are motivated by desire for prestige and political power, and all societal activities either support or attack the status quo. 'True' propositions support the power structure, 'false' propositions do not. Positivist science, with its goal of universal understanding and its techniques of manipulating nature from elementary particles to human beings, should be rejected because it grew out of and supports oppressive capitalism. And there is no way out. To do science is to support oppression. (674)

Similar critiques of science in composition have a long history: Bizzell's early enlistment of Kuhn to discourage reliance on empirical research; Connors' pronouncement that a little empirical research is okay as long as no one is foolish enough to think of it as science; and Berlin's linking of cognitive rhetoric to "the technocratic science characteristic of late capitalism" (484). Shumway's recent analysis of the Berlin/Flower debate is interesting because he grants some legitimacy to scientific argument in composition from the perspective of critical theory: he absolves Flower of naive empiricism and extends provisional dispensation to cognitive rhetoric as long as it renounces instrumentalism. Doubts about the purposes to which research can be bent has led critics in technical and professional writing to mandate an oppositional stance toward powerholders in workplace research. Nancy Blyler criticizes what she sees as the unwholesome trading of "functionalist" research for corporate favors like funding and access. Carl Herndl faults researchers for confining themselves to description and explanation, thereby reproducing the dominant discourse of the worksite, rather than critiquing that discourse on the ideological grounds that radical theorists require ("Teaching" 353). This approach has also led to a degree of pedagogical squeamishness—a hesitation to teach effective rhetorical strategies for fear that students may use them for purposes we do not approve. For example, Dombrowski, while nominally rejecting radical postmodernism (165), identifies with anti-realist constructionists who "question the traditional rhetorical advice to accommodate carefully one's audience, because communication resulting from such accommodation only reproduces and reinforces the prevailing power and economic structures, structures which many times are discriminatory and oppressive" (170).

In challenging this perspective, I certainly do not deny that scientific knowledge and methods are, at least in part, socially constructed. In fact, social construction is essential to the argument I will develop below. The important point for now is that indeterminacy does not vitiate rationality. Popper emphasizes this point: "the fallibility of our knowledge—or the thesis that all knowledge is guesswork, though some consists of guesses which have been most severely tested—must not be cited in support of skepticism or relativism. From the fact that we can err, and that a criterion of truth which might save us from error does not exist, it does not follow that the choice between theories is arbitrary or non-rational: that we cannot learn, or get nearer to the truth: that our knowledge cannot grow" (*Open* 375). For Popper, "knowledge is guesswork disciplined by rational criticism" (*In Search* 40), and it grows by "working with guesses, and by improving upon our guesses, through criticism" (*Open* 381).

Other philosophers of science—such as Richard Watson, Susan Haack, and Philip Kitcher—join Popper in the search for a middle ground between dogmatic skepticism and absolutist scientism, even if they take issue with his formal analysis of scientific reasoning. Kitcher's account draws on close readings of historical scientific texts. Denying the attribution of simplistic realist epistemologies to scientists, he analyzes how knowledge can accumulate across scientific revolutions, and, while acknowledging the influence of social factors, he challenges the assertion that they always prevail over whatever scientists observe or conjecture about the world. The underdetermination of scientific methods does not boil down to "anything goes"; scientific communities move toward consensus by classifying and prioritizing the rival claims that must be explored, by drawing on prior knowledge, by establishing criteria for consistency, and by systematically addressing the sources of indeterminacy. To Kitcher, science is made up of "flawed people, working in complex social environments, moved by all kinds of interests, [who] have collectively achieved a vision of parts of nature that is broadly progressive and that rests on arguments meeting standards that have been refined and improved over centuries" (390).

It is worth noting that while Thomas Kuhn and Clifford Geertz have been enlisted in hyper-constructivist critiques of science, each stopped short of the radical conclusions of some of their followers that "anything goes." It is also worth noting that the view of science as communal critique is shared by some of the best-known researchers of writing processes. Carl Bereiter recently described science as collectively self-critical, progressive discourse about the world. John R. Hayes challenged Egon Guba's claims that the "positivist paradigm" excluded intuition, discovery, and critical inquiry. And Linda Flower noted that statistical evidence has meaning only as part of a cumulative, communally constructed argument, in which "the special virtue of a claim that has earned the name 'result' is that it has been subjected to a given research community's more stringent rules of inference" (300).

The second part of critics' argument is that scientific methods are bound to entrenched power structures. Karl Popper's work is significant for his attention to this charge. He throws it back against the critics. He questions the radical agenda itself as a means of fostering social justice or preventing oppression in light of its easy accommodation to totalitarian and fascist regimes. Popper traces the totalizing association of science with capitalism to a conflation (or monism) of facts with values in philosophies derived from Plato, Hegel, and Marx, including those of Adorno and Habermas. It is this conflation of facts and standards, the real and the ideal, that equates power structures either with absolute good or absolute evil. This equation has dangerous implications, "for even where it does not identify standards with existing facts—even where it does not identify present might and right—it leads necessarily to the identification of future might and right. Since the question of whether a certain movement for reform is right or wrong (or good or bad) cannot be raised, according to the monist, except in terms of another movement with opposite tendencies, nothing can be asked except the question of which of these opposite movements succeeded, in the end, in establishing its standards as a matter of social or political or historical fact" (*Open* 393). Because it precludes reform, the conflation of facts and values is at the heart of attacks on liberalism from either the right or the left. On the right, it leads to commitment to entrenched power structures (present might), on the radical left, it leads to advocacy of perpetual flux (future might). Conflating facts and values is antithetical to critical thinking and to learning from mistakes. For Popper, it is critical empiricism that sustains the open society.

Some critics (including Herndl) portray opposition and resistance to power structures as conducive to reform. But projects for fostering reform would seem to require criteria by which to recognize and measure changes in the condition of an individual or group. Reformers have to admit the possibility that some hegemonic practices are not very oppressive or can cease to be so. They might have to question whether a group's power relations and prevalent practices necessarily determine the problems they face and consider that the power structure might sometimes be irrelevant. These admissions would effectively dissociate facts from values, ruling out the automatic identification of right with current or future might. Rhetorical theory should be useful for accomplishing this dissociation: by articulating strategies for linking and dissociating facts and values, rhetoric denies the intrinsic association of a particular value (like evil) with a particular fact (like power). Perelman and Olbrechts-Tyteca, who set the standard for nuanced exploration of the relations of facts and values, treat their equation as just another rhetorical tactic. They cite Hegelian realism to exemplify one species of pragmatic argument that adopts success as the criterion value for establishing a fact, by making "reality a guarantee of value and causes what has been born, has developed and survived to present itself as success" (268). Rhetorical theory reminds us that while facts may never be represented neutrally, the values associated with them are not preordained.

Alternative Motives for Objectivity

Compositionists readily assume that disciplines that adopt scientific methods do so for reflected glory and access to institutional power. Other motives deserve consideration. Theodore Porter provides an interesting and useful study of the historical conditions that led professional economists, physicians, actuaries, and engineers to take up objective and quantitative methods. He describes intricate connections among science, business, government, and the public, emphasizing that these are not simple alliances, and that the interests of these groups do not usually converge. His startling and persuasive thesis is that objectivity is a defensive strategy used by relatively weak disciplines to ward off interfering administrators, corrupt politicians, or meddling infiltrators from neighboring fields. Objective methods are a shield against oppression from powerful outsiders who want to steer the work toward their own ends. Of course, "interference" may also be a responsible effort at public regulation and accountability. In any case, objective methods enable many embattled professions to salvage or foster some degree of independent action while satisfying external demands for evidence of productivity, progress, or adherence to standards.

The motives of the parties to this interaction should not be characterized simplistically. Porter shows that each party can operate not only in its own interests but also in what it conceives to be the interests of the public, in an explicit spirit of self-sacrifice. And these motives are not mutually exclusive. One of Porter's central themes is that moves toward objectivity in America respond to our national suspicion of powerful outsiders and experts and our confidence in the judgments of the public. Objectivity facilitates public (as well as private) scrutiny of information and the methods used to collect it. Methods that can be learned and shared, compared and tested have a greater potential for scrutiny than the habits of individual genius or the proprietary practices of closed-shop guilds. Scrutiny is never guaranteed; the various publics involved must become and remain active critics. What concerns Porter is, first, that objective methods can become mechanical when they are removed from on-going refinement and critique and, second, that they diminish the scope for judgments based on experience (expertise) and personal knowledge (intimacy).

A second motive for adopting objective methods is to facilitate communication. Formalized procedures and language, including quantification, overcome physical and temporal distance, disparities of experience and background, and absence of a shared natural language. To increase the scope of their communication, professionals reduce their reliance on the sort of intimate, personal knowledge and judgment that can only build up over time in small, tight-knit, and highly interactive groups. Porter clearly sees this bargain as questionable (especially in the case of public agencies and industries). I'm inclined to see it as a pretty good deal, especially for scientific research. Relying on intimacy can produce stifling small-town parochialisms and closed-shop xenophobias. The gain of broader communication is crucial for the very reason that the sciences are intensely and openly socially constructed, to an extent that Porter may underestimate.

The Social Construction of Quantitative and Objective Methods

It is ironic indeed that scientists are accused of avoiding self-critique, creative interpretation, and negotiation of meaning because scientific disciplines work hard at active social construction, harder in many respects than disciplines like English. After all, scientific disciplines are more likely than the humanities to encourage collaboration and to give credit for co-authored publications. It is the sciences that hold researchers most responsible for citing their colleagues' relevant recent work (as Susan Peck MacDonald's cross-disciplinary study shows)—and it is in the sciences that a gap or contradiction in the recent literature provides sufficient grounds for an original contribution (as John Swales has found). It is routine in the sciences for researchers to extend the work of others (by returning to the same site) or to challenge it (by testing the method or reanalyzing the data). Scientists routinely visit each other's labs and use their own research projects as training grounds for their graduate students. Numerous studies of technical and scientific discourse attest to the ways in which the social practices of science contribute to the construction and refinement of scientific knowledge and methods (see Blakeslee; Kaufer and Carley; Myers; Paul and Charney; Swales; and Winsor).

The social construction of knowledge means that most studies are far from definitive. Unlike many critics, most scientists do not assume that their methods ensure certainty and universally generalizable results (Blyler 290) or even take this as a goal. Some disciplines seek at least some exceptionless generalizations, but many are perfectly satisfied with amassing context-dependent generalizations that are far from universal (Kitcher 118). The sources of this misconception are relatively easy to understand. Most of us learn about science from media that inflate probabilities into certainty claims. We read about science in popular articles that strip away the hedges, qualifications, and nuances from the scientists' original texts, as Jeanne Fahnestock and Katherine Rowan found. The genre of popular science reinforces the public perception that scientific research routinely produces simple determinate answers to complex questions and that results have clear and immediate applications. Similar but less sensational processes of simplification are at work in traditional forms of science education. Science textbooks smooth out the tortuous historical path toward the currently favored theory. Even so, it is worth noting that science and social science texts may yet preserve more of the contested quality of research than do textbooks in the humanities. In her analysis of texts from psychology, history, and literature classes, MacDonald found that the proportion of "epistemic" language to "knowledge" claims was higher in the developmental psychology texts than in the history texts, and lowest in the literary texts (179–86). Those who rely on secondary sources may well overlook the contested nature of scientific practice.

Those who examine isolated primary texts may also fail to appreciate their status as arguments to readers in the discipline. The procedures of an experiment can seem preordained and statistical tests can seem like incantations—creating powerful effects on the initiated but conveying little to outsiders. Because routine procedures for collecting and analyzing data are seldom explained or defended, they may seem interpretively sterile or mechanical. Appearances, as is often the case, are misleading. No experiment is ever definitive and every method is subject to challenge—so experimental articles are more properly seen as persuasive than as expository. Charles Bazerman describes how scientists in the 17th and 18th centuries developed conventions for reporting experimental methods as a way to anticipate as many objections as possible from their living and vocal audiences. The genre of the experimental report still serves the same function. Readers use method sections to evaluate the researcher's actions and to establish bounds on plausible generalizations. MacDonald's analysis of articles in developmental psychology illustrates in detail how scientific discourse conventions facilitate communal discussion and refinement of concepts and methods. High proportions of sentences in scientific texts use epistemic nouns as subjects, epistemic verbs, and point-first structures, all of which promote communal knowledge building (172–74). On a larger scale, disciplines continually debate their methods in the scientific literature, challenging old methods and proposing new or improved ones. Sometimes extended arguments occur within a research article, justifying its choices (Thompson). More often methodological debates are the topics of independent journal articles; Ann Blakeslee's analysis of physicists attempting to persuade chemists to try a new statistical method is an especially interesting case study. In some disciplines, entire journals and conference sessions are devoted to methodology. A new method or procedure that gains acceptance in a particular arena may become inscribed for a time as the standard of an experimental paradigm—and the article that proposed it is likely to be among the most highly cited in its field. A similar inscription process occurs in day-to-day exchanges among engineers (Winsor, *Engineer's*). The constant discussion of methods suggests that even standard procedures achieve only provisional credence over time.

Just as methods accrue provisional credence with continued evidence of their productivity, so do some researchers. As studies of the reception of journal articles have shown, a scientist's reputation plays an important role in attracting readers. But reputation is no guarantee that scientists will read an article in its entirety, agree with any or all of its claims, judge the claims to be important, or cite them favorably in their subsequent work

(Bazerman; Charney; Kaufer and Carley; Paul and Charney). Scientific articles that gain wide acceptance do so for many reasons; not least is how well the author connects the study to work that readers see as related to their own projects.

Granted that many scientists say, and may even believe, that their discourse is free of argument and interpretation, that as readers, they need only comprehend the literal meaning of a text, verify its accuracy, and integrate its results with their prior knowledge. This is one of those cases where introspection is misleading and research is most useful. In a similar case, the scientists observed by Jone Rymer were sure that no new ideas ever occurred to them while writing up their research—until they caught themselves having ideas during a thinking-aloud writing protocol. However much scientists and engineers attempt to portray their discourse as expository, studies of their actual practices show it to be a thoroughly argumentative and interpretive enterprise. Scientists observed at work are quite willing to critique methods and interpret data. While writing, scientists attend to rhetorical issues of selecting evidence and adapting to audience (Blakeslee, Myers). The predominant issue, or stasis, of their journal articles is usually a contested claim about the existence or character of a scientific phenomenon—leading to arguments that turn on methods of detection, discrimination, and analysis (Fahnestock and Secor). While reading, some scientists deliberately adopt strategies to maintain their critical edge (Charney). Though many scientists may prefer numbers to words, most recognize that they have to be proficient at both forms of expression and that either form can be precise or misleading, explanatory or reductive, appropriate or irrelevant.

The point of all this should by now be clear: authority does not devolve automatically on anyone who uses an objective, quantitative method. In the course of doing their work, scientific disciplines and sub-specialties communally develop, apply, and refine a repertoire of methods that they consider appropriate for some types of inquiries. Researchers who work on those types of problems rarely must justify their use of a standard method beyond a careful description showing that they applied it appropriately. Those introducing an innovation must provide plausible support of its reliability and productivity. But not even standard methods are exempt from later critiques or technological advances which may alter subsequent interpretations of studies that employed them. It is just this character of science—not the supposed neutrality or disinterestedness of individual scientists—that Popper defines as scientific objectivity (*Open* 213).

Certainly disciplines and professions can come to rely too heavily on pet methods. As Porter warns, objective measures can atrophy into mechanical routines through overuse, intellectual laziness, or bureaucratic decree. Scientists, as well as bureaucrats, can be lulled into studying what can be measured. Mechanical objectivity in classroom lab projects can also inadvertently downplay the rhetorical and critical character of scientific work. None of these problems is intractable unless one denies the possibility of reform in the face of prevalent practice.

If teachers and scholars persisted too long in treating scientific and technical discourse as the bare transmission of determinate facts, it is because we failed to recognize its rhetorical character. Is it fair to hold scientists responsible because we did not appreciate the rhetoric of their discourse better than they did? We are supposedly the ones skilled in discourse analysis and steeped in rhetorical theory. But if we now dismiss objective methods as irrelevant or as opposed to the social functioning of scientific disciplines, we will again be misconstruing the case. As both Kitcher and Porter make clear, scientific consensus building occurs with the aid of, not despite, the use of objective methods. By facilitating communication and effective social organization, objective methods promote sustained focus on specific problems and the refinement of concepts and methods. Those who use objective, scientific, or experimental methods may not be nearly as self-aware as they should be about the nature and consequences of their rhetoric—but the same may well be true of those who use more subjective, qualitative methods.

Researchers and Participants

Scientists' interactions with one another clearly turn on rhetorical, interpretive, and critical skill. But perhaps scientists' relationships to other scientists is not the real problem with objective methods. Perhaps the problem is a deleterious effect on researchers' attitudes toward their objects of study, especially when these are people. According to the critics, social scientists who use quantitative and experimental methods dehumanize the participants of their studies by taking a distant, impersonal, and superior stance; by failing to consult participants about methods and results; and by generalizing from samples to populations. As Dombrowski sums it up, "treating people as objects of study rather than as people implicitly elevates the investigator over the people studied, who are tacitly debased" (172; see also Blyler 306; Lay 351). Qualitative case studies and ethnographies are meant to address these concerns: participant/observers enter a community on supposedly equal footing with the indigenous population, categories and measures emerge from the experience, and no one attempts to generalize—the goal is thick description of a unique interaction.

What is striking is how much these characterizations smack of the worst kind of exclusionary identity politics. They essentialize researchers on the basis of their methods. Methodological choices are taken as reliable indicators of morality, personality, and epistemology. To wit: those who reduce people to statistics cannot possibly appreciate the richness and complexity of an individual human life, while those who write insightful and vivid descriptions of a unique situation must be sensitive and caring and therefore more trustworthy as observers. Essentializing is no more legitimate when applied to researchers than to any other sociocultural group. While objectivity can distance social scientists from the people they study, it is not obvious that distance entails antipathy or even apathy. Certainly some social scientists who use objective methods are uncaring and arrogant but so are some ethnographers and some critical theorists. While acutely aware of the moral and psychological dangers that objective distancing can create, Theodore Porter resists the simplistic equation of impersonal methods with inhumanity, citing evidence in the personal papers of early social scientists and statisticians that they exuded "benevolence and good will" toward the marginalized groups they studied (77). If early social scientists tended rather towards paternalism than Social Darwinism, a susceptibility for "missionizing" is also exhibited by some who conduct subjectivist research in our field, as Ellen Cushman notes in her recent *CCC* article on "The Rhetorician as an Agent of Social Change."

The diametric opposition that is sometimes drawn between qualitative and quantitative methods is difficult to sustain. It is more productive to view these methods as complementary or even as overlapping. Qualitative methods—including ethnographies—can produce more or less objective categorical data that often may be (and sometimes are) analyzed quantitatively. Many social sciences (such as cognitive psychology and sociology) use both methodologies—and even combine them in a single study—to pursue a broader range of questions. The importance of retaining a wide array of methods is acknowledged by Sandra Harding, who notes that "there are things we want to know about large social processes—how institutions come into existence, change over time, and eventually die out—that are not visible through the lens of the consciousnesses of those historical actors whose beliefs and activities constitute such processes" (19). Similarly, Jayaratne and Stewart emphasize the value of both methods to a feminist sociologist who noticed that the victims of marital rape to whom she spoke often described abusive childhood experiences. She used surveys not only to check whether childhood abuse was indeed common among wives who had been raped, but also to see whether it was more prevalent than for a comparable group of wives who had not been raped (91–92). Quantitative studies are especially useful for checking the prevalence of some natural phenomena, for testing the relevance of contextual factors, and for tracing trends.

Some disciplines move back and forth between observational and experimental methods over time, using each to check the other. MacDonald clearly illustrates the interplay of observational and experimental methods in her close analysis of research on infant attachment in developmental psychology: descriptive case studies of mothers interacting with their babies led to refined classifications of behaviors that were then used in studies in which behaviors could be observed and quantified in systematically varied conditions. Bereiter and Scardamalia outline a scheme for recursive "levels of inquiry" in research on writing processes. Both qualitative and quantitative inquiry can turn up regularities or anomalies that researchers will try to explain by making predictions that can be tested with both natural observation and experiments. Regularities can usually be explained in more than one way, so they are often integrated into rival theories whose implications can also be explored further.

The fact that the same researchers and the same disciplines sometimes use both qualitative and quantitative methods undermines simple associations between methods and motives. Still, given the persistent claims that ethnographies are more humane than experiments, it is worth considering what inferences can reasonably be drawn about researchers' motives toward their participants from their published accounts and their methods. At least some ideological judgments of methods center on the researchers' professed attitudes toward participants in published research articles. In praising ethnographers' sensitivity, respect toward their subjects, and propensity for self-reflection, it is easy to fall into the old mentalist trap of taking a written product as a faithful mirror of thoughts, experiences and attitudes. If experimentalists report no personal insights and reflections, it is easy to assume that they either had none or consider them unimportant; if descriptions of participants are dry and formalized, it is easy to assume that the researchers are cold and uncaring. It is also easy to overlook the fact that ethnographers' renderings of their experiences are just as selective and just as calculated as reports of large-scale experiments—ethnographers also publish partial accounts, not raw field notes, transcripts, or thinking-aloud protocols of their real-time reactions. Success at writing a plausible ethnography is a function of both observational and rhetorical skill.

Perhaps it is time to acknowledge that a researcher's sympathies cannot be deduced completely and reliably from any report, no matter whether it reads as impersonal or as self-reflexive. If we grant that experimentalists depersonalize their published accounts partly because their credibility depends on constructing an ethos of disinterested competence, then we should also grant that ethnographers invest their accounts with personality partly to establish an ethos of caring and to create an air of mise-en-scène.

Surely we have learned enough by now from rhetorical and literary theory not to take textual self-representation at face value. Researchers, like all writers and speakers, have a repertoire of voices, no one of which is exclusively authentic or comprehensively self-expressing. Experimenters are less likely to express their ethical concerns toward participants in journal articles than in other social contexts, such as classes on research methods, planning sessions for studies, or meetings of human subject review panels.

Alternative Motives for Proximity and Distance

No matter what the method, the individuals involved in a study (including the researcher) are idiosyncratic, unpredictable, subject to biases, and unrepresentative of a group or a community as a whole. Qualitative and quantitative methodologies address this fact—and the ethical concerns that it raises—in divergent ways that have surprising consequences for their accessibility to criticism.

In quantitative studies, researchers have two ways to assemble a naturalistic sample of participants. Stratified sampling involves close analysis of some community in an effort to include each important constituency in representative proportions. No matter how many categories are formed in a stratified sample, none may be reliably represented by only one person. Samples include more than one woman because women differ in significant ways. So do single, black, middle-class women over forty who have more than 12 years of education. The more individuals in the sample, the less each one may be mistaken as typical of the whole group. The second approach is to choose a large number of participants as randomly as possible. Random sampling does not ignore or suppress individual differences; rather it treats such differences as too subtle and too complex to apportion and it gives them free play. Both methods assume that individuals (even those sharing certain demographic characteristics) vary in their personal traits, beliefs, politics, habits, moods, and states of mind; having large numbers of varied participants all completing similar tasks lessens the chance that any convergences that do emerge in the data are the spurious effects of skewing from some idiosyncrasy.

Some experimental methods provide warrants for causal claims about group tendencies; for example, that children from the most economically disadvantaged groups make the greatest gains in Head Start programs. But because individual differences matter, quantitative methods do not warrant predictions or judgments of the outcomes for individual participants, such as the likelihood of academic success for any particular child in a Head Start program. Generalizations about the central tendency of a group are not distributive to the members; in other words, claims about the group as a whole are not assumed to hold of each member—some students in the most economically disadvantaged group may have made no gains at all. In this way, quantitative methods resist totalizing or deterministic conclusions. Claims about the central tendencies of a group are meant to be read in this light and not taken as normative; in fact, statistical conventions for reporting mean scores and variances help readers assess a group's heterogeneity.

Using large numbers of participants can thus be a way of respecting individual differences, even if it certainly makes a study less personal. But impersonality is not necessarily bad; it is also a way to preserve participants' freedom of action. By taking an impersonal stance, a researcher minimizes the chances of influencing participants to adapt to his or her predispositions, as in placebo effects. Rather than producing impartial researchers, objective methods aim to reduce the effects of biases by limiting and systematizing interactions. They reduce opportunities for intimacy but they also minimize unwelcome or exploitive intrusion. Protocols for behavior toward participants are intended to prevent unethical exploitation by providing an external review of the procedures' risks and benefits, obtaining participants' informed consent, ensuring their privacy, and ensuring their right to withdraw. Systematizing the interactions and describing them in the method sections of research articles opens them to scrutiny by the research community at large, allowing problematic procedures to be challenged effectively. For example, it is largely because of routine reporting of sampling procedures that feminists documented the unwarranted exclusion of women participants in some social science and medical studies, and it is because of such reports that ongoing reforms can be monitored.

Qualitative studies cannot avoid the difficulties of selecting research sites and participants. Over-reliance on studies with small numbers of individual cases compounds some dangers. One danger is the attraction of special sites where retrofitting to known outcomes is hard to avoid. For example, analyzing the discourse surrounding known breakthroughs (like the discovery of the double-helix) and disasters (like the explosion of the Challenger) can lead to specious causal claims about the importance of specific textual features (Winsor *Asking*). Another danger is picking a site opportunistically—because of a consulting opportunity or access through a friend or relative—and then treating it as emblematic. If we only have one report of discourse practices at a nuclear power plant, it is easy to let it stand for all. It is also dangerous to draw conclusions about individuals or to take them as typical of particular segments of society. Qualitative studies in the workplace often involve small numbers of people, with perhaps only one occupying each major role (like manager or technical writer). Idiosyncrasies may then take on more significance than they should. With no way to compare how other individuals in similar roles function in similar situations, researchers may view participants through the lens of totalizing categories like gender, class, and rank and mistakenly attribute some actions to these characteristics. Finally, qualitative studies that depend on close personal interactions may actually increase the danger of exploitation. Social scientists in fields like anthropology are beginning to recognize this danger; Carolyn Fleuhr-Lobban argues for extending the protections of informed consent, with some modifications, to participants in ethnographies and other qualitative studies.

Some subjectivists seem to believe that while immersion in objective methods fails to suppress researchers' prejudices and self-interests, subjective methods and attention to ideology can foster sufficient self-critique (Blyler 303; Lay 351, 361). But as Geoffrey Cross argues, efforts to steer ethnographic analysis toward the subjectivities of any party in the encounter (researcher, subject, or research community) or to downplay attention to descriptive data, increase the chances of solipsism, fraud, and groupthink (124–25). Self-criticism does not safeguard against "objectifying" the other or eradicate problematic power relations. The pit-falls of well-intentioned relations with participants in ethnographies have been detailed by both Kirsch and Ritchie and by Cushman. Feminist sociologist Barbara Risman points out that "even if warm, integrative, and complex human attachments result from the production of research, the researcher is much more free to disentangle herself from these relationships than are the subjects. The authorship is owned by the researcher. She may express her subjects' analytic perspectives, but whether to do so or rather to provide her own alternative understanding remains the researcher's decision" (20). While advocating social activism, Cushman is wary of a tendency toward "missionizing" in literacy research and liberatory classrooms. Herndl also notes the dangers of imposing one's ideology on students. It is laudable that subjectivist researchers look for and share their concerns about their methods, but these accounts do not allow readers to gauge the extent or effectiveness of the self-critique independently and do not help the research community agree on less problematic strategies for interactions.

It is not at all clear how these problems can be addressed from an exclusively subjectivist perspective. A standard response of some qualitative researchers is to use "phenomenological" approaches, such as triangulation of diverse methods and crosschecks of descriptive claims among fieldworkers. However, subjectivist critics like Blyler and Herndl oppose these remedies on the grounds that they seek to "maintain the ethnographer's authority" (Herndl, "Writing" 322), reproduce the dominant discourse of the site (Herndl, "Teaching" 349), or smack too much of scientific values like reliability and objectivity (Blyler 293). They insist that qualitative research resist formalization. Another response is to ask researchers to describe predispositions that might skew the results, with the idea that readers can sort out where these have had some effect. Sandra Harding calls on researchers to describe their "class, race, culture and gender assumptions, beliefs and behaviors"; since these personal characteristics are "part of the evidence readers need to evaluate the results of the research, they should be presented with those results" (29). However, judging the research by these characteristics may encourage an essentializing and deterministic philosophy of human behavior that is more likely to suppress diversity than encourage it. Some ethnographers, like Kirsch and Ritchie, call for changes in discourse forms to "allow multi-vocal, dialogic representations in our research narratives" (21) that would identify the stances of individual participants and researchers, though Herndl concedes that this kind of technique is "only a useful gesture" that does not fully disperse the researcher's authority ("Writing" 327). It remains to be seen whether researchers can sum up their own subjectivities, even with an unlimited amount of narrative, let alone spell out the ideological "differences and struggles within professional discourses" that Herndl sees underlying any social or institutional practice ("Teaching" 354).

Qualitative and quantitative approaches struggle with quite similar issues for establishing ethical relationships between researchers and participants. Bad qualitative research is just as facile, reductive, and exploitive as bad quantitative research. At their best, both approaches seek to foster socially and intellectually significant research in which the participants' contributions are treated with respect, whether by minimizing the intrusiveness of the encounter or by establishing trust-worthy relationships. The question that we must consider is the effect of our favored approaches on our ability as a discipline to define and achieve our goals.

Objectivity As Collective Rather Than Personal Authority

Choosing a subjective rather than an objective method does not introduce argumentation where it was formerly excluded. Instead, it changes what kinds of arguments are appropriate—and in unexpected ways. Rather than dispersing the authority of the individual researcher as the critics desire, an over-reliance on subjectivist methods may actually intensify it.

Porter argues that the way to diminish the importance of argument from authority is to adopt formalized objective methods; historically, such methods have reduced reliance on the judgments of a closed set of highly practiced experts or connoisseurs. He found that experts and elite professionals sacrificed a considerable degree of personal autonomy (and some precision) to achieve the widespread communication that systematic objective methods allow and many did so unwillingly, at the instigation of public officials (97–98). Whatever authority objective methods convey derives from having many people striving to do similar things in similar situations and to produce reliably similar results. That is, it is not the use of quantitative methods per se or the approval of an elite group of scientists that confers authority. Rather, confidence accrues through the day-to-day shared experiences of the disciplinary community in replicating, challenging, reanalyzing and extending each other's data. MacDonald's case study of psychologists studying infant attachment is illustrative. Over the 20 year period that psychologists have converged on the issue of attachment, their concepts, procedures, and explanations have become more "compact" through shared use: "Such compacting can only result from lengthy, sustained attention to the same problems by an extensive group of researchers who collect data in the same conceptually driven manner and build on, refine, and dispute each others' work" (67). By operating within the constraints of formalized conventions for collecting and interpreting data, researchers create the potential for communal scrutiny and refinement of disciplinary work. In composition, researchers have used similar techniques to refine our understanding of such topics as revision and summary writing (see Fitzgerald; Hidi and Anderson).

In contrast, the hallmark of subjectivist ethnography is the exploration of a single site with emergent measures and with results that may be unique to that researcher, that site, and that observational experience. Because each researcher's methods and interpretations are shaped by his or her backgrounds and discourse communities, "ethnography does not claim that anyone using the same methods would come to the same conclusions" (Lay 360). Because emergent methods and intersubjectivities can't be replicated and can't be reapplied, no one besides the researcher has any experience of using them. Only the researcher has access to the full array of information collected or even to a reliable summary (as various tables in a quantitative study would supply). Subjectivist methods preserve the full autonomy of the individual researcher but reduce the relevance and applicability of the research to the community at large. Similarly, in her analysis of texts from literary studies, MacDonald notes that their highly narrative and personal language "mitigate(s) against sustained professional negotiation over the legitimacy of specific academic claims" (192). The credibility of a subjective study settles almost entirely on whatever signs of skill and personal authority the researcher can muster.

From where can such authority derive? When Porter laments the diminished scope for intimacy and experience allowed by objective methods, the people he has in mind are connoisseurs who derive their authority from years of practical field experience, like physicians who have performed so many exams in their specialty that their diagnoses are superior to those of high-tech instruments. Similarly, the cultural anthropologists whose ethnographic methods we have so blithely adopted represent a highly trained and sophisticated group. They go into a field site only after extensive training in a variety of research methods as well as in the linguistics, history, geography, and physical and social culture of the site. As for us in composition studies, the sites we know best are classrooms. And we know our way around certain kinds of texts. But most of us do not have formal study, background knowledge, or years of practical field experience to assert ourselves as experts on any particular non-academic workplace.

Many of the excellent qualitative studies in composition have relied on the phenomenological strategies that people like Herndl and Blyler are currently critiquing as overly objectivist and insufficiently ideological. If phenomenological strategies and objectivist methods are to be excluded or derided, and if shared disciplinary knowledge of the cultures of specific worksites is lacking, then what grounds do the subjectivist critics allow for establishing credibility other than ad hominem appeals for the individual researcher's personal worth as an observer and interpreter? These presumably come in the form of the explicit self-descriptions that Harding recommends, supported implicitly by the researcher's skill at crafting a plausible narrative and making the appropriate gestures (to use Herndl's term) of inclusiveness, caring and self-reflexivity. At best, even with objectivist overtones, appeals for personal authority achieve a one-shot granting of the benefit of the doubt. That is, readers may be willing to believe a researcher's interpretation of what he or she saw but extend their credence only to this account of this site. If the site is familiar, like the classroom, readers can at least compare the account to their own experience. But if the sites are unfamiliar ground, like most settings for non-academic writing, then readers must rely more heavily on the word of the author or fall back on their preconceptions. More importantly, if subjectivist methods and findings are truly local and context-bound, if they are deliberately disqualified as grounds for reliable or valid generalizations, they cannot extend a discipline's repertoire of methods or deepen its knowledge. Research devolves from a shared communal practice into a collection of singular accounts.

Implications

Our over-reliance on qualitative studies and repeated disparagement of objective methods is creating a serious imbalance in studies of technical and professional writing—and the same may be true in composition studies as a whole. The numerous socially-situated ethnographies and case studies, excellent though each may be, cannot by themselves sufficiently extend and refine our methods and our knowledge base. It is rare for the same site or even the same kind of site to be studied by multiple scholars. Even where this should be easiest, where the sites are textual, only a few cases come to mind: Watson and Crick's announcement of the double-helix, the documentary record surrounding the Challenger disaster, Jack Selzer's collection of analyses of a landmark article by Gould and Lewontin. In each case, scholars have insightfully interpreted the texts and allowed some degree of comparison of methods and theories. But these scholars rarely challenge or extend each other's findings. On what basis could one decide which analyses were most productive? It is even harder to imagine how to extend qualitative worksite studies. Carl Herndl has re-examined the ethnographies of Lucille McCarthy (in "Teaching") and Stephen Doheny-Farina (in "Writing"). In both cases, he uses these studies to illustrate what he wishes other researchers would do in gathering and presenting their work. In fields with stronger traditions of objective research, such observations would merely introduce the author's detailed reanalysis of the original data or a new study that incorporated the suggested changes and showed exactly what gains they yielded. By producing numerous individual subjective studies, we have constructed a broad shallow array of information, in which one study may touch loosely on another but in which no deep or complex networks of inferences and hypotheses are forged or tested.

In rejecting logocentrism, subjectivist critics veer toward "ethocentrism," a fixation on claims to rightful authority. They grant researchers full autonomy and freedom from institutionalized evidentiary practices, but leave them to their own devices to coax out emergent methods and defend their choices with their very identities. Without the means to contest and refine our methods and our data directly—through shared use and critique—all we can do is fight over which authority to valorize: the author, the critic, the experimenter, the trendiest theorist or philosopher, the political activist, or the participants whose interests we claim to define and promote. Perhaps this is why we seem to see so many articles (like this one, admittedly) telling us what kind of research to do, and so few describing substantive research. And perhaps this also explains why people like Nancy Blyler call on us to subordinate the goal of collective scholarly work altogether in favor of radical political activism in the workplace to liberate the oppressed.

Through a congeries of epithets, critics in composition have demonized scientific practices and practitioners. To promote the growth of a complex and inter-connected framework of knowledge and methods, we need both qualitative and quantitative empirical methods. Surely we only hamstring ourselves by demanding that every encounter between researchers and participants involve intense personal interaction, by sniffing for traces of objectivism in qualitative studies, and by imposing ideological and epistemological preconditions. Of course we should be critical of our methods. Arguably, scientists are not as self-conscious of their methods as they should be, but their practices engage them more deeply in collective knowledge construction than ours do. We should take seriously our responsibility to improve our methods by setting higher expectations for training in research methods and in the terrain we wish to study. We should promote the publication of research that extends and refines previous work. And we should encourage reviews of previous studies that compare findings and methods in particular kinds of sites, to generate questions and hypotheses that can be pursued with a full range of methods.

Power and authority will never be handed over just because of what kind of people we are. The only way to progress as a discipline is to undertake the hard task of inter-connecting our work, by building up provisional confidence in our methods and our knowledge base by challenging and impressing each other—and anyone else who cares to look.

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Works Cited

Bazerman, Charles. *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science.* Madison: U of Wisconsin P, 1989.

Bereiter, Carl. "Implications of Postmodernism for Science, or, Science as Progressive Discourse." *Educational Psychologist* 29 (1994): 3–12.

Bereiter, Carl and Marlene Scardamalia. *The Psychology of Written Composition.* Hillsdale: Erlbaum, 1987.

Berlin, James. "Rhetoric and Ideology in the Writing Class." *College English* 50 (1988): 477–94.

Bizzell, Patricia. "Thomas Kuhn, Scientism, and English Studies." *College English* 40 (1979): 764–71.

Blakeslee, Ann. *"Inventing Scientific Discourse: Dimensions of Rhetorical Knowledge in Physics."* Diss. Carnegie Mellon U, 1993.

Blyler, Nancy Roundy. "Research as Ideology in Professional Communication." *Technical Communication Quarterly* 4 (1995): 285–313.

Charney, Davida. "A Study in Rhetorical Reading: How Evolutionists Read 'The Spandrels of San Marco.'" *Understanding Scientific Prose.* Ed. Jack Selzer. Madison: U of Wisconsin P, 1993. 203–31.

Connors, Robert J. "Composition Studies and Science." *College English* 45 (1983): 1–20.

Cross, Geoffrey. "Ethnographic Research in Business and Technical Writing: Between Extremes and Margins." *Journal of Business and Technical Communication* 8 (1994): 118–34.

Cushman, Ellen. "The Rhetorician as an Agent of Social Change." *CCC* 47 (1996): 7–28.

Dombrowski, Paul. "Post-Modernism as the Resurgence of Humanism in Technical

Communication Studies." *Technical Communication Quarterly* 4 (1995): 165–85.
Fahnestock, Jeanne. "Accommodating Science: The Rhetorical Life of Scientific Facts." *Written Communication* 3 (1986): 275–96.
Fahnestock, Jeanne, and Marie Secor. "The Stases in Scientific and Literary Argument." *Written Communication* 5 (1988): 427–43.
Faigley, Lester. "Nonacademic Writing: The Social Perspective." *Writing in Nonacademic Settings.* Eds. Lee Odell and Dixie Goswami. New York: Guilford, 1985. 3–83.
Fitzgerald, Jill. "Research on Revision in Writing." *Review of Educational Research* 57 (1987): 481–506.
Flower, Linda. "Cognition, Context, and Theory Building." *CCC* 40 (1989): 282–311.
Flynn, Elizabeth. "Feminism and Scientism." *CCC* 46 (1995): 353–69.
Fluehr-Lobban, Carolyn. "Informed Consent in Anthropological Research: We Are Not Exempt." *Human Organization* 53 (1994): 1–10.
Haack, Susan. *Evidence and Inquiry: Towards Reconstruction in Epistemology.* Oxford: Blackwell, 1993.
Harding, Sandra. "Is There a Feminist Method?" *Feminism and Science.* Ed. Nancy Tuana. Bloomington: Indiana UP, 1989. 17–32.
Hayes, John R. "Taking Criticism Seriously." *RTE* 27 (1993): 305–15.
Herndl, Carl G. "Teaching Discourse and Reproducing Culture: A Critique of Research and Pedagogy in Professional and Non-Academic Writing." *CCC* 44 (1993): 349–63.
———. "Writing Ethnography: Representation, Rhetoric, and Institutional Practices." *College English* 53 (1991): 320–32.
Hidi, Suzanne, and Valerie Anderson. "Producing Written Summaries: Task Demands, Cognitive Operations, Implications for Instruction." *Review of Educational Research* 56 (1986): 473–93.
Jayaratne, Toby E., and Abigail Stewart. "Quantitative and Qualitative Methods in the Social Sciences." *Beyond Methodology: Feminist Scholarship as Lived Research.* Eds. Mary Fonow and Judith Cook. Bloomington: Indiana UP, 1991. 85–106.
Kaufer, David, and Kathleen Carley. *Communication at a Distance: The Influence of Print on Sociocultural Organization and Change.* Hillsdale: Erlbaum, 1993.
Kirsch, Gesa and Joy S. Ritchie. "Beyond the Personal: Theorizing a Politics of Location in Composition Research." *CCC* (1995): 7–29.
Kitcher, Philip. *The Advancement of Science: Science without Legend, Objectivity without Illusions.* New York: Oxford UP, 1993.
Lay, Mary. "Feminist Theory and the Redefinition of Technical Communication." *Journal of Business and Technical Communication* 5 (1991): 348–70.
MacDonald, Susan Peck. *Professional Academic Writing in the Humanities and Social Sciences.* Carbondale: Southern Illinois UP, 1994.
Paludi, M. A. and L. A. Strayer. "What's in an Author's Name? Differential Evaluations of Performance as a Function of Author's Name." *Sex Roles* 12 (1984): 353–61.
Paul, Danette, and Davida Charney. "Introducing Chaos into Science and Engineering: Effects of Rhetorical Strategies on Scientific Readers." *Written Communication* 12 (1995): 396–438.
Peplau, Letitia, and Eva Conrad. "Beyond Nonsexist Research: The Perils of Feminist Methods in Psychology." *Psychology of Women Quarterly* 13 (1989): 379–400.
Perelman, Chaim, and Lucille Olbrechts-Tyteca. *The New Rhetoric.* Notre Dame UP, 1969.
Popper, Karl. *In Search of a Better World.* London: Routledge, 1992.
———. *The Open Society and Its Enemies,* Vol. 2, 5th ed. Princeton: Princeton UP, 1971.
Porter, Theodore. *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life.* Princeton: Princeton UP, 1995.
Risman, Barbara. "Methodological Implications of Feminist Scholarship." *The American Sociologist* 24 (1993): 15–25.
Rowan, Katherine. "Moving Beyond the What to the Why: Differences in Professional and Popular Science Writing." *Journal of Technical Writing and Communication* 19 (1989): 161–79.
Rymer, Jone. "Scientific Composing Processes: How Eminent Scientists Write Journal Articles." *Advances in Writing Research* Vol. 2. Ed. David Jolliffe. Norwood: Ablex, 1988. 211–50.
Selzer, Jack, ed. *Understanding Scientific Prose.* Madison: U of Wisconsin P, 1993.

Shumway, David R. "Science, Theory, and the Politics of Empirical Studies in the English Department." *Writing Theory and Critical Theory.* Eds. John Clifford and John Schilb. New York: MLA, 1994. 148–58.

Swales, John. *Genre Analysis: English in Academic and Research Settings.* Cambridge, England: Cambridge UP, 1990.

Thompson, Dorothea. "Arguing for Experimental Facts in Science: A Study of Research Article Results Sections in Biochemistry." *Written Communication* 10 (1993): 106–30.

Watson, Richard. "Ozymandias, King of Kings: Postprocessual Radical Archaeology as Critique." *American Antiquity* 55(1990): 673–89.

Winsor, Dorothy. "The Construction of Knowledge in Organizations: Asking the Right Questions about the Challenger." *Journal of Business and Technical Communication* 4 (1990): 7–20.

———. "An Engineer's Writing and the Corporate Construction of Knowledge." *Written Communication* 6 (1989): 270–85.