

Paradigm and Punish

Davida Charney

University of Texas at Austin

The goal of my article was to challenge some recent critiques of empirical research in composition and technical writing. Marilyn Cooper presents here a kinder, gentler version of those critiques than many I have seen in print. If I read her right, there is considerable agreement in our hopes for writing research, including desires to encourage more and better research, support multiple research approaches, and support the use of public and sharable methods that are objective in the sense that they "allow truth claims to be assessed and understood by others" (561). But there is also an area of outright disagreement between us that is a pretty basic one.

Cooper's response draws heavily on Egon Guba and Yvonna Lincoln's four paradigms for research: positivism, post-positivism, critical theory, and constructivism. Her point seems to be that research should be measured against the criteria of the paradigm within which it is conducted. She suggests that my critique of subjectivist research is inappropriate because I seem to judge it by "post-positivist" rather than by "critical" criteria. But Cooper herself does not stick to so relativist an outlook. It is clear that she, like Guba and Lincoln, compares and judges these paradigms on ideological grounds.

Guba and Lincoln portray scientists as positivists, naive realists, and dualists who have for centuries now aimed to predict and control the natural world, ignored issues of context, and excluded creative insight and discovery. The use of the term *post-positivism* only strengthens the claim for the existence of positivism as the collective scientific dogma. As in the coining of post-structuralism and post-modernism, the move to "post-it" reifies some collection of characteristics, fixes it to a definite historical period, and signals its obsolescence. In this case, the succeeding period is portrayed as so close in continuity to positivism that the benefits of progress and recency that are normally conveyed with the "post" are slight indeed. Post-positivists are allowed to believe in probabilistic truth claims that are subject to falsification, but they are still realists, still aim at prediction and control, avoid self-reflection, and exclude values and transformative discovery from inquiry. Both forms of positivism are denied the "more informed, sophisticated, and historically dependent constructions" that the critical and constructivist paradigms are privy to (558). And only those in the critical paradigm aspire to social action and reform (Guba and Lincoln 113).

In contrast, my central purpose was to discredit efforts to slot researchers into neat ideological pigeonholes in the first place. Guba and Lincoln's particular assumptions about scientists' beliefs and practices are as prob-

lematic as those I critique at length. Though I do not address their work directly in my essay, I do cite the critique of their paradigms by John R. Hayes in "Taking Criticism Seriously" (*RTE* 27, 1993, pp. 305–315). Hayes challenges the historical accuracy of the supposed recent shift from 400 years of positivism to post-positivism, showing that the practices attributed to post-positivism have actually been around since the beginning of organized science. Even the claimed characteristics of positivists do not accurately represent scientific practice, currently or historically. For the same 400 years, scientists have rejected naive realism, worked with probabilistic claims, and concerned themselves with biases and the contexts of empirical observations. My essay presents more evidence from observations of scientific practice, analysis of their discourse, and scientists' own testimony about their beliefs and practices. Scientists represent far too broad an array of epistemologies to be herded into the pens of positivism and post-positivism. Historically, scientists have developed disciplinary conventions that promoted active social construction of methods and findings. Social scientists have striven for longer than a few decades to develop ethical practices for studying people that, even if impersonal, respect the status of participants and promote social reform as much as can close interaction.

In the face of evidence of the importance of constructivism as an active force in science, and evidence that researchers and disciplines routinely employ approaches that seem to cut across paradigms, Cooper tries to salvage the post-positivist slot by stretching it, asserting that "post-positivism holds to social constructivist, relativistic, and indeterminate notions of knowledge" (558). This is a departure from Guba and Lincoln, who say that "for constructivists, either there is a 'real' reality or there is not... and thus constructivism and positivism/post-positivism cannot be logically accommodated any more than, say, the ideas of flat versus round earth can be logically accommodated. For critical theorists and constructivists, inquiry is either value-free or it is not; again, logical accommodation seems impossible. Realism and relativism, value freedom and value boundedness, cannot coexist in any internally consistent metaphysical system" (116). So Guba and Lincoln insist on isolating categories that may look neat and consistent but that don't actually describe what researchers believe and how they act.

What's the point of trying to hang on to a set of ideological labels that just don't fit? One reason may be that the depersonalization of talking about paradigms blunts the apparent force of aspersions cast on real people. The reified paradigms, rather than the researchers operating within them, become the object of ideological critique. Whereas I see radical critics ascribing venal motives to real researchers, Cooper ascribes the criticism instead to the paradigm. She says that "the relationship between the

researcher and the participants, and the representation of that relationship, primarily reflect the assumptions of the research paradigm, not the personal attitude or ethics of the researcher" (559). A little later, she says that "the overriding purpose of the construction of ethos in research writing is not textual self-representation but the representation of the goals and standards of the research paradigm" (6). Similarly, she treats my concerns about critiques of triangulation in ethnographic research as simply a question of "paradigm fit" (5). While Cooper seems to agree with me that the values associated with specific methods are not preordained, she still aligns herself with those who more explicitly demonize scientists on the basis of their supposed paradigm. It's the paradigm that is sexist or authoritarian, not necessarily the researchers—but researchers who adopt its methods and discourse conventions are still "further[ing] the goals of [a] research paradigm" (560) with its concomitant "ideological effect" (561).

It is this conflation of facts and values that I oppose. No, scientists are not value-free and neither are their methods or findings. But giving up the assumption that scientists are neutral does not entail a new assumption that they all share some particular suspect dogma. Nor must we accept that the very use of quantitative research methods, for example, univocally connotes some specific social, cultural and political values. We can instead simply say that the choice of method *per se* provides insufficient evidence of the researcher's beliefs, attitudes, and values and refrain from generalizing about the researcher or the discipline. In my essay, I argue that we should adopt this stance even toward qualitative methods and discourse conventions, some of which seem more expressive of the researcher's and others' subjectivities. The use of a qualitative method, such as ethnography, does not prescribe any particular ideology or research goals. Ethnographies are not inherently more feminist than surveys or pre/post-treatment experiments. Cooper suggests that the use of objectivist qualitative methods, such as triangulation, leads to questions of "paradigm fit," as researchers may then be furthering the goals of post-positivism rather than radical critique. Yet the matter is not so simple when paradigms are designed to be ideologically freighted. Critics like Carl Herndl and Nancy Blyler are dissatisfied with ethnographers who do not seem committed to critical theory. Blyler is particularly explicit in questioning the motives of those whose work does not explore the preferred ideological ramifications in "Research as Ideology in Professional Communication" (*Technical Communication Quarterly* 4, 1995, pp. 285–313).

Cooper claims that I am demonizing radical critics, just as critical theorists have demonized scientists. I hope I have not. I do argue that some of their claims are wrong. I do argue that objectivist forms of research (qualitative and quantitative) are more conducive to the shared use and cri-

tique of methods that we need to create rich, interconnected frameworks of knowledge. As is common in rhetorical criticism, I also speculate about what values and beliefs underlie the specific claims of specific critics. But I do not make the kinds of assumptions about radical critics that they have about scientists in general—that they are sexist, or racist, or stuck in retrograde ontologies, or implicated in social injustice.

My essay was a response to the observation that in recent years, antagonism to empirical research in our field has broadened from dislike of specific methods to the demonizing of scientific approaches in general. Rather than improving our methods and findings through reuse, critique, and modification, we now denounce them as flawed or ideologically suspect and discourage their use. As a result, our disciplinary preoccupation with the researcher's character (what I call *ethocentrism*) is pulling us away from substantive research on reading and writing—and diminishing our ability to promote effective social action.