Motives, Metaphors, and Messages in Critical Receptions of Experimental Research: A Comment with Postscript

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Must we continue to be plagued by the scientific nemesis? By the specters of averages and standard deviations?

William F. Irmscher

The Experimental community in Composition must be considered a deeply troubled one.

Stephen M. North

Comment

Soon after the publication in 1986 of George Hillocks' Research on Written Composition (RWC), three of the most distinguished voices in composition studies reviewed it for College Composition and Communication, the flagship journal of the discipline. All of them, implicitly or explicitly, derogated experimental research. All of them, intentionally or not, fueled and more firmly entrenched a pervasive discomfort with empiricism in composition studies.1 Here I suggest that the discomfort may be motivated in part by an unconscious desire to "shrink from the weight" of fact—"the vital, arrogant... dominant X" of which Wallace Stevens speaks in his poem, "The Motive for Metaphor." Here I argue that the discomfort is perpetuated by the metaphors and messages embedded in critical receptions of experimental research. Here I offer one analysis worth considering.

The first of the three RWC reviews, written by Stephen P. Witte, was thorough, favorable, and accurate in most of its criticisms. Witte recommended that Hillocks' book be read. But with his concluding criticism—that RWC "contributes to... the controversy over methodologies in research" by "presuming the efficacy of... a particular inquiry paradigm" (206, 207)—Witte, himself a well-known empiricist, ironically contributed to the derogation of empiricism. This passage was key:

The dispute over research methodologies and paradigms, which seems to have in large part motivated Hillocks' inquiry, is largely counterproductive. ... What the field needs is not more unproductive and even counterproductive
bickering about methodologies, but greater recognition of the strengths and limitations of all research methodologies. (206-07)

The message this text sent its readers was clear: Hillocks' RWC, the most current, conspicuous summary, index, and meta-analysis of twenty years' worth of experimental composition research, was motivated by and rooted in unproductive and counterproductive bickering about research methodologies.

Given the power of book reviews written by distinguished scholars for flagship journals, we should consider several questions about the effect of Witte's message. How many teachers, for example, chose not to read RWC because of that message, despite Witte's recommendation? How many still read the book but, post-Witte, read it predisposed to preempt or misread key passages such as the following: "We cannot afford to reject one mode of research in favor of another . . . . We need to use whatever modes of research are useful to learn as much as we can" (Hillocks 246)? How many readers subliminally decoded an implicitly derogatory message in Witte's extended metaphor about trips and terrains, such as this one: "Using meta-analysis in any field is a little like trying to photograph a field of Cenizo or Purple Sage . . . . from an airplane" (206)? How many readers read the Witte review, perhaps quite incorrectly, as negative—particularly in light of the negative review which followed it?

That second review, Richard L. Larson's, was also accurate in several key criticisms. But its thesis—that Hillocks' "fundamental arguments" are presented in language that "the working professional reader . . . cannot grasp" (210)—promoted the destructive notion that experimental research is inaccessible to the mainstream of our profession. This is a fiction denied not only by many college-level writing teachers, but also by many elementary and secondary teachers who quote from their highlighter-penned copies of RWC with ease. For the myth of experimental research's inaccessibility to have been affirmed (or reaffirmed) by one of the most distinguished voices in rhetoric and composition is unfortunate. In fact, it's depressing, especially to us non-number-crunchers who have struggled to develop number-crunching competence because we do view it as mainstream. Larson's review isolated one example of dense prose, a paragraph explaining how to calculate an effect size, to support the contention that "passages such as this" pervade the book, and to send the message that Hillocks' language, the language of experimental research, renders his book inaccessible. In fact, the review stressed this message, arguing that RWC's inaccessible language is "one of the acute ironies of our profession today" (211), that quantitative composition research is, ironically, not usable by active English teachers who teach composition. Again, given the power of such reviews, we should ask certain questions. How many readers, for example, chose not to read the book at all as a result of Larson's argument? How many read the book, but under the influence of
the review's implicitly derogatory metaphors, like the one about "semantic and conceptual thickets beset[ing] the unwary reader" (208)? Its message was, of course, that readers need be wary of some kind of trap.

The third review, the embedded subtext of a "personal perspective essay" by William F. Irmscher, sent powerful messages rooted in key words and troubling theses. Three warrant especially close reading. First, Irmscher wrote, "What may be at the basis of the present crisis is that composition specialists in English departments have deferred to the world view of English-Educationists" (85). There are, of course, two powerful messages here: (1) that there is a crisis; and (2) that composition specialists have deferred to an implicitly lesser world view. Second, he argued, "It is practically a given that one cannot say positively what causes improvement in writing effectiveness or whether a particular teaching approach is responsible for a change" (84). The thesis here is troublesome: since one cannot demonstrate teaching's impact on writing improvement, there is really no reason to conduct research at all, of any kind. Finally, Irmscher said, "I see no reason why dissertations and scholarly articles need to be only barren factual statements. Do we have to abandon the literary and critical values of our discipline in the name of scholarship" (87)? The message here is clear, hostile, and powerfully worded: experimental research abandons the values of our discipline.

Irmsher's proposed model for future scholarly inquiry implicitly denigrated experimental research: scholars "should use as many sources of information as will produce relevant data" (85), but not if they are "barren factual statements" or statistical "specters" and "nemeses" (87). Researchers should "note as accurately as possible what subjects do and say" (86), but not by "disrupt[ing]... the natural setting with... cameras, tapes, and talk-aloud protocols" (85). Researchers should return, Irmscher argued, to metaphor (87).

I claim no special insight into Professor Irmscher's motives. I am reminded, though, of Wallace Stevens' contention that the motive for metaphor is the unconscious desire to shrink from fact, and the reminder prompts me to wonder. Did these key reviews of RWC, the most conspicuous discussion of experimental research available in our time, send subliminal messages rooted in the motive for the metaphor in them? Do some composition teachers shrink from the thought of "vital, arrogant, dominant" facts about the teaching of composition? Do some shrink from experimental research because they fear fact and think experimental research might uncover it? Or do they genuinely doubt that fact exists—at least in composition studies—and, if so, what are the implications of such doubt?

In composition studies, certain fears are understandably associated with fact. And they're powerful fears. They're fears of accountability. If ever we agreed that there were things we knew with certainty about teaching writing—things that work, things that do not—then we would be fully accountable for our own teaching and our preparation of the teachers who
follow us. If ever we agreed that modes and model-excerpts and grammar drills accomplish little in teaching writing, then we would be fully accountable for those courses still based on them. If ever we agreed that there were ways to teach (and not to teach) writing, then we would be accountable for any and all of those composition agendas varying "from nothing to everything" (Kinneavy 1) and based on little more than the notion that composition is "an amorphous pastime that is vaguely good for everyone" (Welch 276).

There are similar fears associated with experimental research itself. If we agreed that experimental composition research were accessible to all of us in composition studies, then we would all have to learn from it and contribute to it. If we knew that we could test our own teaching, as well as the teaching of those whom we supervise, then we would be obliged to do both. If we felt we could grasp others' experimental findings, then we would all be obliged to learn from their work, to confirm it, to challenge it. It is much easier, much more comfortable, to declare experimental composition research inaccessible—to derogate it, ignore it, bicker about it, and then bicker about the bickering.

Postscript

Does this read like just so much more of the latter? Am I guilty of the very infraction for which I cite Witte? Perhaps I should offer some background. I first drafted this comment nearly two years ago after my first exposure to experimental research in composition studies. I had just completed my first experimental study, an examination of collaborative peer revision's effect on audience consciousness. I had relied on Hillocks' pre-test/post-test/effect-size model, as well as the protocol-analysis model popularized by Linda Flower and John Hayes. Having been guided by Research on Written Composition, I was amazed by the lukewarm-to-hostile reviews it had drawn, and I was eager to deconstruct them. Even though I didn't understand many of Hillocks' charts and tables and graphs, I saw his basic message as clear, constructive, and conciliatory: there are lots of ways to do studies, and we may certainly do our own as we see fit; but each would be more valuable if we would only follow some basic rules and then report our work in enough detail so that it could be replicated or aggregated by anyone who might want to do either; if we would all agree to do this, then we would be able to test for the larger truths, the truths which emerge from the recurring patterns.

At the time, I was largely unaware of how pre-paradigmatic we are in the experimental dimension of the discipline. Later, though, after soliciting critical responses to my own experimental work, I began to learn some of those larger truths which emerge from recurring patterns. Here is one of them: many of the people refereeing our journals don't believe in experimental research at all; of those who do, I don't think there are two who agree on how to do it. My referee-dyads, at least, made contradictory claims and offered irreconcilable responses:
My investigation was "interesting" and "useless"; "of great interest" and "of limited interest"; "important" and "already done at Wisconsin."

My design was "intelligent" and "naive"; "appropriate" and "seriously flawed"; "worthy of high marks" and "ignorant of what research is and how it is done."

My literature review was "too brief" and "too extensive"; "too cursory" and "too lengthy"; "overly summarized" and "drags on and on."

During revision I needed to "use a Mann-Whitney" and "include ANOVAs"; "drop the worthless protocol analysis" and "drop the statistics and focus on the rich protocol data"; "drop the appendices" and "expand the appendices."

The work was "really only appropriate for someplace like RTE," and "had [I] tried Written Communication?"

"For all its size," Stephen North recently concluded, "the Experimental community has not exercised anything like a proportionate influence on the field" (144). Is such work, North asked, "unacceptably sloppy" (145), "or are there other factors at work" (146)? Review rhetoric and referee rhetoric have led me to conclude it is the latter.

Relatively little experimental research is published; relatively little of what is published, then, is read. The derogation of experimental research, the myth of its inaccessibility, the absence of consensus about experimental methodology, and the isolation of experimental research to certain journals come together in a powerful, unintentional alliance which serves to suppress a huge body of information about the teaching of composition and, as well, to perpetuate the suppression.

After having spent most of the morning counting, I offer the following unscientific study to suggest the magnitude of the suppression:

- Of the "approximately 2000 pieces" of experimental research in the RWC bibliography, about half are unpublished dissertations; few of those remaining have been published as articles in mainstream composition studies journals.5

- Of the hundreds of articles published in the fifty-three reasonably current issues of the eleven mainstream composition journals I take, three (3!) report work from an experimental study.

I'm fairly certain that many studies are done and that most are submitted for review. The vast majority, though, are never individually published. In this context, the dangers of denigrating, neutralizing, or suppressing Hillocks'
Critical Receptions

Compilation and meta-analysis cannot be overstated. Books like *Research on Written Composition* may be the sole dissemination vehicles for hundreds and hundreds of pieces of work. We need to develop strategies for changing that. The critical receptions of experimental composition studies have served not only to suppress information, but also to perpetuate methodological uncertainty. Whose standards shall we teach in our graduate research methods courses? Whose standards shall we impose on experimental theses and dissertations? There is a powerfully ironic paradox here. Disputes about what constitutes inter-rater reliability are important to statistical researchers. They are beside the point, however, to descriptive researchers, meaningless to those opposed to experimental research, and laughable to those who don’t believe in rhetoric and composition. My work, your work, Hillocks’ work—all are important pieces of a pedagogical puzzle. It’s a puzzle most especially worth solving, most especially worth believing solution is possible, from the standpoint of program direction and teacher training. While I think it appropriate to fear the thought of ever being told how to teach, of ever being asked to subordinate art to science, I do think we can arm teaching assistants with more than roll books, coffee cups, and red Bic pens. I think we can use the lessons learned from experimental research to show them some patterns, trends, and truths.

Notes

1See North, Berkenkotter, and others for speculations about the humanism/scientism split and other forces contributing to this discomfort. It is a widespread phenomenon not limited to these reviews, to this book, or to *College Composition and Communication*. I suspect, for example, that Foster, writing in *Journal of Advanced Composition*, had the same effect as these *College Composition and Communication* reviewers with his contention that “Hillocks and Beach and Bridwell err” in “constricting our view of the real richness of composition inquiry” (37).

2North claims experimental research to be the largest body, “more than that produced by all the other Researcher methods combined” (142).

3An interesting aside is North’s report that his research has “not turned up a single published replication study in Composition” (159).

4One strategy, of course, is to continue the calls like Berkenkotter’s for “epistemological ecumenicalism among the diverse groups that constitute our field,” for “learn[ing] more about one another’s models of knowing” (80). The strategy is ineffective, however, and the learning cannot take place, if certain modes of inquiry are privileged by our journals. In lamenting Hillocks’ exclusion of the non-empirical from his meta-analysis of audience studies, Foster noted, “Fortunately... all readers of composition’s principal journals have had the opportunity to become aware” of key non-empirical audience studies (38). Foster cannot make the same claim about key...
empirical ones.  

Lauer and Asher's new *Composition Research: Empirical Designs* (which encourages multimodal approaches) probably offers the best answers to my questions at this time.

This essay began as a paper at the 1989 convention of the Conference on College Composition and Communication in Seattle. I thank Michael Flanigan for suggesting it, David Mair and Kathleen Welch for believing in it, and my reviewers for helping me improve it.

Works Cited


Call for Papers

*Syntax in the Schools*, a quarterly newsletter (K-college), is seeking papers on new approaches that relate grammar to students' own writing or to the literature we give them to read. For more information, contact Ed Vavra; *Syntax in the Schools*; Shenandoah College; Winchester, VA 22601.