Composing Research

Johaneck, Cindy

Published by Utah State University Press

Johaneck, Cindy.
Project MUSE. muse.jhu.edu/book/9257.

For additional information about this book
https://muse.jhu.edu/book/9257

For content related to this chapter
https://muse.jhu.edu/related_content?type=book&id=261363
I know that nothing pleases an academic more than a defense of the indefensible, an affirmation of the value and truth of what all had come to agree was worthless and false.

*James L. Battersby, 1996*

As rhetoricians, we have a long history of debate and verbal bantering. From Plato’s attack on Gorgias, to Aristotle’s criticism of contemporary handbooks, to Ramus’s arguments against Quintilian, to the nineteenth-century “art vs. science” debate, to our own time in which we debate the kinds of knowledge we value and the kinds of research we should conduct, the very foundations of what we believe is accurate in our field have seemed to shift rapidly from the start. *How we see those foundations, however, how we frame our debates, both past and present, is at issue here. On the one hand, diversity within our field is necessary: diverse theories and scholars work for and against each other in a way that is necessary for our field to enrich our knowledge, to gain a respectable place among scholars in other fields, and to invite new scholarship of the future. On the other hand, especially regarding research methods in our field, we seem to adopt an opposite view: our field is simply divided by different ways of knowing, and we argue which ways are better and which are worse. This latter approach has fueled our debates about research practices in our field, highlighting a perceived incompatibility among them.*

*In order to progress beyond such divisions in the future, we must first understand why they are false, harmful, simplistic, and limiting. So far, for example, several scholars under review here have seen a certain “truth” in narratives and a certain “falsehood” (untrustworthiness) in numbers (e.g., Charney, 1996, p. 582): there is an established*
tendency to see information that is mathematical as somehow automatically decontextualized and reductive, while information that is story-like is seen as somehow able to capture context naturally and automatically in a narrative. The quest for context and our sensitivity to it has advanced our preference for the narrative, a form that we claim has the power to reveal the full complexities of the contexts in which we teach and conduct research—contexts in which we have dialogue, feelings, and problems to solve.

In spite of our tendency to believe that narrative forms capture context better than numbers do—or that narratives are contextualized by their very nature—we must understand that “context” and “narrative” are not synonymous. While numbers might give us only some information, we must reframe our praise for the narrative with the understanding that narratives, too, give us only some information. And, depending on context, the kind of information we seek must vary: when stories are readily available and are informative (or, perhaps, are all we have), we should, of course, share them; when numbers are easily obtained and are informative, we should share them, too (and share them completely and, certainly, without apology). To argue instead that narratives, anecdotes, and stories are always more true than numbers, that numbers are always out of context and narratives are not, that it is always appropriate to share a researcher’s personal voice ignores the very thing to which we claim to be rhetorically most sensitive: context.

Instead of discussing our research with a sensitivity to the contexts from which our work emerges, we have developed a sensitivity to a more simplistic element: form. The narratives we share and the numbers we show are products of inquiry that emerged from some natural process, a context in which we had the desire to know something. When we argue, as we have been, that some forms of research products are more welcome, more interesting, more “true” than others, we ignore the full contexts of research that would naturally produce other valid forms or, especially, contexts that would naturally produce a mix of forms.

When we publish our research in traditional scientific-looking forms, the process of that research often seems to be hidden—never
ignored, but assumed to be understood by the trained eye—and is, therefore, misunderstood by the untrained eyes of our field. The narrative form, in contrast, is readily understood—literary training has enabled us to easily grasp it, relate to it, and extend its meaning. In other words, we forget our own advice that the process of learning and process of writing cannot always be seen in the product alone. As teachers of writing, we are trained, and continue to train ourselves, to look at products from our students as clues to their processes, knowing the process is there and how to look for it. As researchers of writing, we do not look at our research with nearly as much care—or with as much sensitivity to context as we claim to have.

A greater sensitivity and attention to form has instead produced a body of scholarship in composition that defends our preferences for certain forms. Theories of knowledge, as we have come to frame them in our field, have been artificially divided as much by form as by “thinking”—a division that also ignores the varied contexts in which we learn and know. Worse, we sometimes argue that some theories of knowledge, or epistemologies, are sensitive to context while others are not. The context (usually ideological) in which such arguments are made, however, is often ignored.

Because our response to quantitative research methods has been, in part, emotionally and politically driven (as argued in the last chapter), the epistemological arguments we make to defend and to theorize our positions often leap widely from the local to the global. On a daily basis, we don’t live at an epistemological level; we live in the immediate, emotional, political, social world of things we like or are good at (such as form) or things we need to get done at the present moment—that is, for academia, our teaching, researching, and publishing. We construct epistemology abstractly as a means of defending our world—defending our preferred teaching methods and, especially, preferred research methods—but we can’t always “get there from here” without dismissing other worlds, other methods.

As a field of composition, we need to “get there from here” in a more inclusive manner, but a discussion of only competing epistemologies would erroneously make the same leap while maintaining the artificial dichotomy we have already created. Therefore, the
competing epistemologies we have outlined for our field will be briefly reviewed in this chapter—but only for the purpose of illustrating the decontextualized arguments they represent. The remainder of the chapter will present a Contextualist Theory of Epistemic Justification (Annis, 1978), a theory that allows for what Hobson (1992) called an “epistemological mix,” a template for rethinking our research and one that (re)grounds us firmly in the rhetorical principles that have guided our field from the start and that should always guide our research questions, whatever they may be—as shown in the Contextualist Research Paradigm proposed later in this chapter.

Instead of arguing, in other words, about which research method or which epistemological stance is sensitive to context, we must ask instead: In what context does that sort of argument make sense? In what context does such division naturally occur? In what contexts do divided ways of knowing serve us well? In what contexts in other areas of our lives do we make such distinctions?

Divisions among theories of knowledge construct context artificially, after the fact. As a result, we are often coerced by our own field’s scholarship now to reject traditional research methods, opting instead for other methods, regardless of what we want to know, regardless of how best to come to know it. Simultaneously, we propose misguided arguments that only certain methods are sensitive to that same context—contexts we either ignore or construct artificially after our chosen method is comfortably in place. Inattention to the contexts in which we construct such arguments, and ultimately choose one research method over another, has created the unfortunate illusion that the range of research methods available to us somehow stems from incompatible systems of thought— incompatible and, therefore, competing epistemologies.

COMPETING THEORIES OF KNOWLEDGE IN COMPOSITION

Berlin (1987) once noted that

[R]hetoric refers to a diverse discipline that historically has included a variety of incompatible systems. . . . [E]very rhetorical system is based on epistemological assumptions about the nature of reality, the nature of the
The notion of epistemological assumptions was captured by Emig’s (1982) articulation of “Inquiry Paradigms,” in which she asserted the need for five elements in such a paradigm:

1) a governing gaze; 2) an acknowledged, or at least conscious, set of assumptions, preferably connected with 3) a coherent theory or theories; 4) an allegiance to an explicit or at least a tacit intellectual tradition; and 5) an adequate methodology including an indigenous logic consonant with all of the above. (65)

In Emig’s words, “there can be no more than three governing gazes, so it is easy and almost inevitable to regard most of us as one of three kinds of gazers: positivistic, phenomenological, or transactional/constructivist” (65) (see Figure 4.1). In other words, as Emig defined a governing gaze—“a steady way of perceiving actuality” (65)—there can be no more than three, there can be no overlap among the three she has outlined, we can adopt only one of them, as the three are clearly incompatible with each other. For Emig, the most “diametrically different,” and “most fundamentally opposed” (65) were the positivist and phenomenological governing gazes: the positivist focusing on phenomenon stripped of context and ignoring individual interpretation, and the phenomenological focusing on context and the perspective of the individual who is perceiving the phenomenon.

Emig connected the phenomenological gaze, as others have, to Polanyi’s (1964) concept of “personal knowledge,” a concept Emig praised as “steadily useful” (67). Kerlinger (1986), too, found Polanyi’s concept of personal knowledge to be useful, though as a behavioral scientist, in a different way. Borrowing Polanyi’s phrase, “passionate commitment,” Kerlinger outlined the role of personal knowledge for the scientist (those often accused of the narrow, useless governing gaze of positivism). Though the following passage from Kerlinger is quite long, it is this very passage—the first page of his Preface to the third edition of *Foundations of Behavioral Research*—that helped me
begin to rethink our own debates about how we conduct research in our own field—a rethinking that, in part, inspired and formed the context for this work.

Some activities command more interest, devotion, and enthusiasm than do others. So it seems to be with science and with art. . . . It seems a far cry from science to art. But in one respect at least they are similar: we make passionate commitments [Kerlinger cited this phrase as Polanyi’s] to them.

This is a book on scientific behavioral research. Above everything else, it aims to convey the exciting quality of research in general, and in the behavioral sciences and education in particular. A large portion of the book is focused on abstract conceptual and technical matters, but behind the discussion is the conviction that research is a deeply absorbing and vitally interesting business.

It may seem strange in a book on research that I talk about interest, enthusiasm, and passionate commitment. Shouldn’t we be objective? Shouldn’t we develop a hardheaded attitude toward psychological, sociological, and educational phenomena? Yes, of course. But more important is somehow to catch the essential quality of the excitement of discovery that comes from research well done. Then the difficulties and frustrations of the research enterprise, while they never vanish, are much less significant. What I am trying to say is that strong subjective involvement is a powerful motivator for acquiring an objective approach to the study of phenomena. It is doubtful that any significant work is ever done without great personal involvement. (vii)

In contrast to the many in composition who see the subjective and the objective as fundamentally opposed to one another, Kerlinger invited his students, readers of his text, to bring their own personal commitment to their reading and offered two pieces of advice: First, “I would encourage students to discuss, argue, debate, and even fight about research. Take a stand. Be opinionated” (viii). This is the part that composition has done quite frequently, though not in the context of Kerlinger’s second piece of advice: “Later try to soften the opinionation into intelligent conviction and controlled emotional commitment” (viii). In the end, for Kerlinger, “It is doubtful that students can learn much about science, research design, and research methods without considerable personal involvement” (vii-viii).
Such emotional commitment and personal involvement, for Emig and others in composition, are often perceived as absent in what we have framed as “objectivist” or “positivist” epistemology—a perception that has fueled our passionate defense of the personal as more valuable than the scientific. Such a perception has narrowed, especially recently, our potential channels of scholarship: in the current climate of composition research, the personal commitment that brings us to our research must result in an equally personal text/product. Often citing critics of science (as Emig did), rather than scientists (such as Kerlinger), we have latched onto that criticism of science before we have firmly grasped what science is, what a scientific method is. Thus, what Emig called governing gazes and what others have called epistemologies remain artificially contrasted, divided, and separate in our field.

Berlin, especially, outlined a simplified division among epistemological assumptions in our field—three theories of rhetoric (see Figure 4.1): objective theories based on a positivistic epistemology that locates truth in the external, measureable world; subjective theories based on truth residing within the individual, a notion eloquently captured by writers such as Emerson and Thoreau; and transactional theories based on the assumption that truth arises from the “interaction of the elements of the rhetorical situation” (7-17), the basis for current theories regarding the social construction of knowledge.

Berlin outlined three kinds of transactional theories: the cognitive and the classical, which, Berlin argued, virtually ignore the role of language; and the epistemic, which involves language “in every instance of its manifestation” (16). We could rebut Berlin’s assertion that language was not significant in the classical tradition or the cognitivist (16), and we should have difficulty with his use of “truth.” Gradin (1995), for example, found Berlin’s simplification of epistemological stances in our field troublesome, and, frankly, I’ve always been confused that some epistemologies are “epistemic” and others are not. This classification, however, has provided language and frameworks that we have used to solidify the divisions within our field ever since.
Lunsford (1991), for example, went on to identify three kinds of writing centers (see Figure 4.1): the garret center (expressivist) where writers go to think individually and be inspired, the storehouse writing center (objectivist) where writers get information on rules and so on, and the collaborative center—a center based on a social-constructionist point of view and a center that Lunsford argued was best. Murphy and Sherwood (1995) applied a similar three-paradigm model for writing centers in their Preface to *The St. Martin’s Sourcebook for Writing Tutors*. While Murphy and Sherwood argued that “tutorials are rarely, if ever, exclusively the product of any one paradigm” (4), they point out limitations and criticism of only two of them—the current-traditional/objectivist and the expressivist—while upholding the social-constructionist as “dominant” (3), pointing out the strengths of only this model.

Using a collaborative/social-constructionist model, Murphy and Sherwood articulated four “principal ideas” governing the rest of their text and, generally, the tutors’ role: 1) tutoring is contextual, 2) tutoring is collaborative, 3) tutoring is interpersonal, 4) tutoring is individualized (1). Of course, all tutoring is contextual, but the last three principal ideas decontextualize the tutor’s role: not all tutoring is automatically collaborative when direct instruction is sometimes necessary, online tutoring has brought into question our definitions of “interpersonal,” and small-group and in-class tutoring creates contexts in which tutoring might not be individualized. Unfortunately, only the first principal idea can be supported, and in the context of the first, the other three cannot, because they ignore the first—context. Construction of these principal ideas was based on the dominant social-constructionist model of rhetoric, ignoring other theories.

Such divisions among theories of rhetoric have been of concern to some in the field. Gradin (1995), for instance, argued that Berlin’s three-part division for theories of rhetoric was too simplistic (see also O’Donnell, 1996). Of concern to Gradin, however, was the separation between expressivist and social theories, two stances she combined in *Romancing Rhetorics*. Gradin offered a new look at romantic rhetoric through which “we can continue to embrace social theories while
retaining what is most valuable about expressivist doctrine” (165), an argument that rests on the field’s current acceptance of social theories. Of the three-part division that Berlin offered, Gradin, here, sought to join only the transactional and the expressivist. Evidently, the objectivist is dead and buried.

LIMITATIONS OF THE COMPETITIVE EPISTEMOLOGICAL FRAMEWORK

Such artificial divisions among theories of knowledge have led to the artificial dichotomy we have perpetuated in our research. When debating the merits of qualitative and quantitative methods, we connect that debate to competing epistemologies, as our field has outlined them (Figure 4.1), in order to defend our preferences.

Such competitive theories of knowing, however, are stripped from the context of the need to know and are, therefore, false lenses through

Figure 4.1

Governing Gazes (Emig), Competing Theories of Rhetoric (Berlin), Competing Models for Writing Centers (Lunsford), and Current Research Models
which we attempt to define our research, accepting in the process only limited parts of what it means to know something fully. How we see our research has suffered from this self-imposed near-sightedness—a near-sightedness that threatens to move our research even further from the contexts of other work in our field, such as our teaching.

For all of our attempts to construct such competitive theories of knowing, after all, composition teachers and tutors are already keenly aware that elements of all of them, in spite of the competitive nature we have assigned to them, are at work in our field at every moment. Any writing teacher who takes attendance, gives grades, and teaches students how to correct grammatical or structural errors works in the so-called outer, measurable, objective world. Any teacher who has individual conferences with students, small-group work in the classroom, or interaction with a writing center, operates on transactional theories. The same writing teacher who incorporates journals, freewriting, and expressive assignments allows for the subjective to work, too. And when students attend writing centers, the best tutors will know that sometimes students need dialogue to generate ideas, sometimes they need to vent or “talk out” an idea, and sometimes they need direct instruction on writing skills that can’t be coaxed into their minds through questions or discussion.

On a day-to-day basis, experienced and successful teachers and tutors do not flail about when all of these “competing” elements enter our teaching, our offices, our writing centers, our classrooms—as they so regularly do. On the pages of our scholarship, however, pages on which we construct our theories and present our research, we divide these same elements and defend our stances passionately, as if the elements have nothing to do with each other. Appropriately, Connors (1983) specified our research rather than our teaching when he noted, “as a research discipline, we tend to flail about” (10).

Especially for those who advocate qualitative research on the grounds that quantitative methods/researchers ignore context, the decontextualized three-part epistemological structure of our field must be revised and recontextualized. (It should also be blamed in large part for the flailing about that currently plagues us.) To construct competing theories of knowing, as we see them operating
in our research, does no more than suggest that we are incapable of embracing all ways of knowing, and that we refuse to acknowledge the truth of our field: we live in all worlds, in all modes of knowing, but we are trained to understand only some, unable to discuss the “other,” and unwilling to see the narrow channels of scholarship we have imposed upon ourselves.

And what, ultimately, are we saying about ourselves when we construct such arguments? Consider, for example, arguments that support socially-constructed models of knowing because they are more collaborative (feminine) than competitive (masculine): these arguments are also based, directly or indirectly, on a competitive (masculine?) model of epistemological difference (after all, “collaborative vs. competitive” is, in itself, a competitive way of thinking). Indeed, we embrace one epistemological stance by acting through another.

Further, our distrust of numbers—or our misunderstanding of them and our own poor training in how to use them—has led us to distrust the researchers who use them as well. I agree with Charney (1996, p. 583) that we have fallen into a most destructive and inaccurate view of quantitative researchers: if the traditional researcher focuses the context of an experiment in such a way that does not reveal the “gut feelings” that led to the study, does not articulate the full process and trials and tribulations of that research, and does not share emotion, that researcher will be criticized not so much for not sharing it, but for not having that process at all. Add to that our anxiety about statistics that inhibits our understanding of that text, that makes our “eyes glaze over,” and we fall victim to another all-too-human phenomenon: to blame the “other” for what we cannot understand: It is wrong to use numbers (because I don’t like reading them, and I’ve never understood them); it is wrong to use an experiment (because I’ve never conducted one myself).

In the context of our experiment at the bowling alley, we are motivated by your personal belief that your red bowling ball is lucky. Luck, of course, has much to do with “chance,” so it is only fair (and necessary) to determine mathematically the extent to which the data we have gathered are due to only
chance. We have noticed differences in the average scores for each color (green=6.0, red=7.0, and purple=8.0). By eyeballing these means, remember, we could guess that the purple ball is actually “luckiest,” but we also remember that individual scores within each color group varied, too. Chance, then, could be operating in two places: between colors (each color achieved a different mean), and within colors (the 6 scores within each group were not consistent). To determine the extent to which the color differences are not due to chance, we can determine the ratio of the two places where variation occurs: between colors and within colors.

This is what an Analysis of Variance (or ANOVA) does. Stay tuned and relax.

Our anxieties about numbers, experiments, and the statistical analyses they require have moved our field to construct epistemological stances abstractly, as a means of defending our own anxieties, our inadequacies, our lack of training—epistemological stances from which we attack “the other”—in an uncomfortable contact zone (Pratt, 1991) in which two cultures (Snow, 1965) collide, miscommunicate, and remain by our own contention irreconcilably different. Perhaps epistemology does not “construct us” as strongly as we suggest, at least as such inquiry appears on the pages of our scholarship: after all, we were researchers and teachers long before epistemological inquiry appeared in that scholarship. We constructed it. If we created these false divisions and a false theoretical security through our epistemological inquiry, we, too, can change it.

Though we have successfully broadened the scope of our research potential to include more than the older tradition of the quantitative, we swing too far, dismissing the value of that tradition for reasons that do not serve any of our research endeavors well, whatever the contexts might be.

For simplicity here, let’s look at only the red ball and the purple ball. Of course, your red ball was the reason for this experi-
ment, but the purple ball scored a higher average. To see if the difference between the purple score and the red score was due to chance, we need to determine three kinds of variation among the scores achieved by each ball:

1. What is the total amount of variation in the study?
2. Of that total amount of variation, how much of that variation can be explained by a difference in color (the variation that occurred between the color groups)?
3. Of the total amount of variation, how much cannot be explained by a difference in color (the variation that occurred within each group)?

This mathematical procedure will look similar to the procedure for the standard deviation described in chapter three.

First, let's review the raw scores and the group averages we're looking at:

<table>
<thead>
<tr>
<th></th>
<th>Red</th>
<th>Purple</th>
</tr>
</thead>
<tbody>
<tr>
<td>Scores</td>
<td>9</td>
<td>10</td>
</tr>
<tr>
<td></td>
<td>9</td>
<td>9</td>
</tr>
<tr>
<td></td>
<td>8</td>
<td>8</td>
</tr>
<tr>
<td></td>
<td>7</td>
<td>8</td>
</tr>
<tr>
<td></td>
<td>5</td>
<td>7</td>
</tr>
<tr>
<td></td>
<td>4</td>
<td>6</td>
</tr>
<tr>
<td>Totals</td>
<td>42</td>
<td>48</td>
</tr>
<tr>
<td>Avgs</td>
<td>7</td>
<td>8</td>
</tr>
</tbody>
</table>

To determine how much variation was due to differences in color and how much was due to differences in individual trials/scores, we first need to calculate the total units of variation and then we'll determine how many of those units were due to the color of the ball and how many were due to differences in individual scores within each color group.
To do this, we’ll first need to calculate the grand mean, the mean for all scores in this red/purple comparison, by adding all 12 scores and dividing by 12: the grand mean (or M) is 90/12 = 7.5.

Now we return to the three places where we determine how many units of variation we have:

1. the total units of variation will be determined by subtracting the grand mean (M) from each raw score in the study (Y), or: Y-M.
2. the units of variation due to group differences (between colors) will be determined by subtracting the grand mean (M) from each group mean (Ȳg), or: Ȳg-M.
3. the units of variation due to individual differences (within groups) will be determined by subtracting the group mean (Ȳg) from each individual score (Y), or: Y-Ȳg.

This process determines components of differences, allowing us to look at each different score in different ways. To look at these individual scores in the overall study, however, we’ll need to compute in a similar fashion the components of sums of squares.

This computation is similar to the procedure for computing the standard deviation. We’ll need to square each difference we find through subtracting and then sum (∑) those squares.

Our overall formula for Components of Sums of Squares looks like this: total variation = variation between groups + variation within groups \(∑(Y-M)^2 = ∑(Ȳg-M)^2 + ∑(Y-Ȳg)^2\)

The chart below will help clear up confusion. We’ll subtract as outlined above, but remember from computing the standard deviation: if we add up all the results of our subtraction, we’ll always have zero (because we’re subtracting using the mean, and it makes sense that the mean will have an equal amount above and below it). Therefore, we’ll square each result like we did for the standard deviation. Our computations for total units of variation (Y-M) will look like this:
You can practice your own computations to determine how much of this total variation (35.0) is explained by differences between colors ($Y_g-M$, and then square the difference) and how much cannot be explained because it’s due to differences among individual scores within groups ($Y-Y_g$, and then square the difference).

I’ll give you the answers: the units of variation explained by difference in color is 3.0. That is, of the 35.0 units of total variation, 32.0 cannot be explained by difference in color because they were due to differences within colors. Does that mean your results are not statistically significant—due merely to chance and not differences between colors? Stay tuned for the significance testing and find out.

THEORIES OF EPISTEMIC JUSTIFICATION: META-EPISTEMOLOGY

All epistemological inquiry, of course, focuses on the nature of knowledge, how we go about finding or creating it, how and where it exists in the first place, and what can even be known. Alston (1989), however, distinguished between epistemology and epistemic
justification, asserting that there “is a distinction between what we may term substantive epistemology and meta-epistemology”:

Meta-epistemology is concerned with the basic concepts we employ in epistemology, concepts of knowledge, truth, belief, justification, rationality, and so on, and with the methods, procedures, and criteria to be employed in determining how to apply those concepts. Substantive epistemology, on the other hand, consists in our endeavors to use these concepts to arrive at results on such matters as the conditions under which we have knowledge or justified belief of one kind or another, and on what knowledge or justified belief we have. (1-2)

In other words, a meta-epistemology focuses on all of the parts that are at work in an operating epistemology and conducts analyses of how those parts work and definitions of the terms we need to discuss epistemology. Substantive epistemology puts those parts in motion (in practice, so to speak) and prepares us to share our beliefs and knowledge with others. Alston’s book, Epistemic Justification, then, focused on meta-epistemology through an analysis of epistemic justification theories, a look at all “the parts” of our justification for beliefs or knowledge and how those parts work.7

Theories of epistemic justification, when properly understood, will aid a researcher’s understanding of a research process grounded not so much in competing epistemologies, but in an epistemological dynamic that allows us to find the best available means of knowing at a given time, in a given place. Alston explored the nature of justification as both (and necessarily) objective and subjective. The notion of justification, especially epistemic justification, is, on the one hand, subjective and personal:

What confers justification must be “internal” to the subject that she has a specially direct cognitive access to it. It must consist of something like a belief or an experience, something that the subject can typically spot just by turning her attention to the matter. (4-5)

At the same time, epistemic justification involves and seeks the objective, the external world we hope to justify, to know, or to believe:
We typically turn our attention to justification and the like when we fall prey to doubts about the possibility of knowledge, about our capacity to get beyond our own thoughts and experiences to the real objective truth about the world outside our minds. . . . For nothing else would lend a belief some rational credibility when we are radically questioning our access to anything beyond our own consciousness. (5-6)

Alston warned against seeing only the “internal,” personal view here as the driving epistemic force in our inquiry. The interplay between the subjective and objective—the interplay between our own doubts, our own experience, and our world—moves us beyond an “egocentric position” (6).

Our development as researchers who are able and willing to pursue the wide range of research questions we naturally encounter requires a movement beyond an egocentric epistemological state. Though our work is often personal, individualized, and based on experience, the natural interplay of both quantitative and qualitative is necessary for full epistemic justification of our beliefs. We composition scholars, then, could learn much from Annis’s (1978) “Contextualist Theory of Epistemic Justification.”

Annis’s theory focuses on three “parts” of epistemology in particular—three parts that, for composition scholars, will look remarkably familiar. Our field has divided itself by competing epistemologies (objectivist, subjectivist/expressivist, and transactional/social-constructivist), but these, for Annis, are not competing substantive epistemologies so much as they are the necessary “parts” that comprise his theory of epistemic justification based on what Annis called an “issue-context,” as shown in Figure 4.2: 1) the belief sought to be justified, 2) an appropriate objector group, and 3) a believer’s level of understanding.

Annis’s contextualist theory shifts our attention from specific substantive epistemology to a meta-epistemology by asking us to attend more seriously to context and to examine more fully the “parts” that exist in that context before we put them in motion. Instead of arguing, for example, that qualitative or quantitative research methods are best, that one kind of research is natural or another unnatural, or that
all research must include student and researcher voices, a Contextualist Theory of Epistemic Justification draws us to an analysis of context: what do I want to know? why do I need to know it? how can I frame my question in a way it can be answered? and so on.

Such a framework offers a meta-epistemological reflection before the “parts” of the epistemology are put in motion through research—a reflection that might produce surprising answers for those who favor one kind of research method over another, and a reflection necessary for serious inquiry: Annis’s theory rightly assumes, as we should

---

**Figure 4.2**

**Governing Gazes (Emig), Competing Theories of Rhetoric (Berlin), Competing Models for Writing Centers (Lunsford), and Current Research Models**

<table>
<thead>
<tr>
<th>Research Models</th>
<th>Positivist</th>
<th>Transactional</th>
<th>Phenomenological</th>
</tr>
</thead>
<tbody>
<tr>
<td>Emig 1982</td>
<td>focus on phenomenon outside of context; quest for absolute truth</td>
<td>knower and known interact; mutual transformation</td>
<td>focus on personal reality in context; quest for multiple individual truths</td>
</tr>
<tr>
<td>Berlin 1987</td>
<td>Objectivist</td>
<td>Transactional</td>
<td>Expressionist</td>
</tr>
<tr>
<td></td>
<td>relying on external, scientifically verifiable proof</td>
<td>relying on group dynamics and collaboration</td>
<td>relying on one’s internal interpretation of the world</td>
</tr>
<tr>
<td>Lunsford 1991</td>
<td>Storehouse</td>
<td>Collaborative</td>
<td>Garret</td>
</tr>
<tr>
<td></td>
<td>writing center features, handbooks, drills, modules, handouts</td>
<td>writing center features, dialogue; meaning is negotiated by the group</td>
<td>writing center features, individual expression, inspiration</td>
</tr>
<tr>
<td>Research Models</td>
<td>Quantitative</td>
<td>Social</td>
<td>Personal</td>
</tr>
<tr>
<td></td>
<td>relying on numerical data, controlled methods, and statistical analysis</td>
<td>relying on case study, observation and dialogue/interviews</td>
<td>relying on personal narrative, reflection on experience, anecdotes</td>
</tr>
<tr>
<td>Issue-Context</td>
<td>Belief; issue under consideration; observations, proof, data, etc. that relate to the belief in question</td>
<td>Appropriate Objector Group; the culture or community in which beliefs are justified</td>
<td>Believer’s Level of Understanding; personal conviction, experience of an individual</td>
</tr>
</tbody>
</table>

Necessary components for epistemic justification, not competing elements of epistemology
assume in our field as well, that those who engage in inquiry through research are “critical truth seekers” (281), not merely defenders of their own preferences, ideologies, or writing styles.

The issue-context is contextually- and socially-driven, based on the “actual social practices and norms of justification of a culture or community of people” (282). Because composition studies has become a multidisciplinary endeavor, we have access to numerous cultures and communities, including the scientific. If, for example, I believe that using red ink on my students’ papers will cause a negative feeling in my students (a believer’s level of understanding), and if other writing teachers around me (an appropriate objector group) believe the same, we agree enough to accept the belief that red ink is “bad.” However, if I remember that as a student, I never experienced a negative feeling toward red ink when my own teachers used it, I might begin to doubt the belief our group holds and construct a new belief of my own. I might strengthen that belief by using red ink on my students’ papers and then informally asking them for their reactions to it. If the objector group, my colleagues, wish to maintain their belief and reject my new belief, I am now in a position to test or to experiment on my new belief in a way that will either support or refute the objector group. In a final report of this actual study (in chapter six), I will illustrate those rhetorical decisions about events, experience, and data—decisions guided by the context of both process and product.

The Contextualist Theory of Epistemic Justification is grounded in the assertion that all justification of beliefs is a social act. In a social act, we always have 1) individuals with their own individual experiences and beliefs, 2) other people around the individual who may object to or accept the individual’s belief, and 3) issue-related facts, data, demonstrations, and observations that will help refute or confirm both the individual’s belief and the group’s beliefs. Because justification is a social act, the contexts in which it occurs will vary and, in contrast to how some composition scholars have defined a “social act,” some contexts naturally include numerical data or the need to understand probability. Annis outlined an example of a drug being tested to see if it would cure a disease without causing harmful
effects. If researchers are testing the drug’s effect on animals, their concern for adequate proof will not be as high as it would be if they were testing the drug’s effect on humans, so the researcher might require a more stringent significance level in statistically testing the outcome (282). Here, Annis showed the effect that context has on how we analyze data for statistical significance.

For our own test of statistical significance, we return to the units of variation explained by color difference (3.0) and the units of variation not explained by color difference (i.e., Variation due to individual differences in scores within color groups) (32.0). Getting the ratio of explained variation to unexplained variation is what we do when we get an F-ratio.

But first, we have to take into account the “size” of the two places where variation occurred. Variation between color groups occurred only between 2 colors. Variation within color groups occurred among 12 individual scores. It seems unfair, in a sense, to compare 2 things to 12, so we’ll have to factor in the size of these comparisons through something we call degrees of freedom.

The easiest way to explain degrees of freedom is to play a game: If I tell you that a set of 6 scores must add up to 42, and then ask you to randomly start listing those 6 numbers as you wish, you will have 5 numbers that could be anything. Once those 5 are filled in, however, the 6th will have to be the number that makes all 6 add up to 42. Let’s say you pick 4, 5, 6, 7, and 8 as your first five numbers. The 6th number must be 12 in order for all 6 to equal 42. In other words, that last number has no degree of freedom for you to choose what you want. What you had, however, was 5 degrees of freedom or N-1.

Since we only had two color groups, the degrees of freedom are easy to figure out. N-1 (where N is the number of groups) is 2-1, which is 1. For the 12 scores, however, each set of 6 was in its own group. The 6 red scores had to add up to 42, and the 6 purple scores had to add up to 48. That would leave you with 5 degrees of freedom for each group, or N-k, where N equals
the number of scores, and \( k \) equals the number of groups
\( (N-k = 12-2 = 10) \).

Before we get the ratio of between-group differences to within-group differences, then, we’ll “even out” the size of those groups by dividing by their degrees of freedom:

\[
\frac{3.0}{32.0} / 10 = \frac{3.0}{3.2} \quad F = .9375
\]

In most statistics handbooks, we’ll find an “F table,” or a table of values where we can find our own F ratio by the degrees of freedom in both the numerator and the denominator in the above equation. An F table looks like a grid, listing degrees of freedom for the numerator (1, in our case) across the top, and degrees of freedom for the denominator (10, in our case) down the side. Like using a map, we find our degrees of freedom and use them like coordinates to look for the critical value required for our F ratio to be statistically significant.

But before we look up our own F value to determine its statistical significance, we have to decide at what level are we willing to accept some error? The F table will give us different critical values for an F ratio having 1 degree of freedom in the numerator and 10 in the denominator, based on levels of probability: Once out of a hundred (.01)? Five times out of a hundred (.05)? Only once in a thousand (.001)?

The standard level of acceptance for statistical significance (especially in the context of a study as harmless as ours) is five times out of a hundred or .05. If we find our F value to be significant at the .05 level, we can confidently say that the probability with which our results were due merely to chance is less than 5 times out of 100.

According to the ratios presented in the F table, if we have 1 degree of freedom in the numerator and 10 degrees of freedom in the denominator, our F value needs to be at least 4.96 to be significant at the .05 level. Our F value is .9375.
While the purple ball achieved the highest average in our study, you can at least argue that such a result could be due merely to chance. At the same time, however, we need to return to the null hypothesis here: there is no difference in scores bowled by different colors. At this point, we fail to reject the null hypothesis. This suggests, based on the data we collected and analyzed, that we have no reason to believe that one color is luckier than another.

A Contextualist Theory of Epistemic Justification reframes our current view of epistemology-in-competition and constructs instead an epistemological dynamic that emerges naturally from the need to know, from a question arising from a particular context that will, if we examine context fully, lead to the best research method(s) available for answering that question at that moment. If we view our research from this template rather than from one of competing epistemologies, we construct much more than a mere “gray area,” a “happy medium” on which we might agree philosophically but continue our debates practically. In the past, others have attempted to achieve such agreement, but strong division among our researchers, dichotomous language that traps us into “camps,” and a focus on method rather than context has kept us frozen and separate in the same attempts to bring us together.

THE POTENTIAL OF RE-CONTEXTUALIZING OUR EPistemological FRAMEWORK

North, in 1987, proposed methodological egalitarianism: the “I’m OK, you’re OK” approach to methodological diversity in our field. Practitioners are OK, historians are OK, clinicians are OK. To draw an analogy, imagine an integrated neighborhood of mixed race, one in which no one really interacts. You do your thing, and I’ll do mine, and as long as we don’t cross boundaries, we can live happily in our neighborhood. In 1992, Kirsch called for methodological pluralism, a call for the children of this neighborhood to play together. After all, they can learn much from each other, share culture and language and values—if only we encourage them to try. But as long as the adults in
this neighborhood make the public rules, provide the language, and
teach the values, these children might not play together well, might
not play together willingly, and might not play together for very long.

Contextualized epistemological pluralism asks that the adults in
this neighborhood be willing to play together, too—finding common
ground, understanding the wealth of knowledge we have when we
put that whole puzzle together, recognizing the contexts in which we
naturally share common goals, and changing language to reflect that
new value. We must first understand that, though we appear to be dif-
f erent, it is that very difference that makes us necessary to each other
when we desire to fully examine the contexts in which we
work/play/learn—a difference that gives us something to offer each
other. After all, if we were all the same, what could we possibly learn
when we explore our questions?

A Contextualist Theory of Epistemic Justification forces us to
focus not on numbers vs. narratives, but on the questions that moti-
vate us to learn in the first place. A template such as this grounds us in
the things we most value as scholars of rhetoric: context, questions,
knowledge, and a mix of cultures—and the active quest they set in
motion. Much as we teach our students to ask critical questions, to
examine all possible points of view, and to find as many sources that
help them get to the bottom of their own inquiry, a contextualist the-
ory will help us do the same, will help us practice what we preach,
and will ground us again in the rhetorical tradition that shaped the
context for our field in the first place.

One final note before we leave the bowling alley (and, yes,
we are now leaving). This should not be the only study on which
you base firm conclusions. Surely, the results of this study are
worth sharing and thinking about, but don’t forget the context in
which this was done: at only one bowling alley, for only one
bowler, with only three colors, and with only 6 trials per ball. The
fascinating thing about experiments like this is their sensitivity to
context: what will happen when we play with that?

And a personal note: I hate bowling! I am far from being a
bowler or a bowling fan, but I know a good research question
when I see one; my feelings about bowling don’t keep me from asking those questions. I hope the same is true for anyone doing research in our field, as well.

EPISTEMIC JUSTIFICATION AS RHETORIC: DECISION-MAKING IN CONTEXT

Though Annis never framed his theory as Aristotelian in nature, he could have easily done so (and, perhaps, he should have). For composition scholars, Annis’s theory is composed of elements in a rhetorical dynamic similar to what we have come to call the communications triangle, a dynamic inherent in Aristotelian rhetoric.\(^8\) Scholars such as Booth (1963) and Kinneavy (1971) have examined the three-part rhetorical foundation that most of us teach our students in some fashion. How many of us encourage students to examine 1) their persona as a writer in relation to 2) their subject/issue and available information/data related to their subject and 3) their intended audience, all in the rich context of having a purpose or of having a question to explore? How often do our textbooks and our classrooms explore 1) the ethical appeal, 2) the emotional appeal, and 3) the logical appeal, each corresponding to those familiar elements: the writer, the audience, the subject, supported by adequate facts/data/information?

We discourage students’ use of purely emotional appeals, calling on them to ground that appeal in solid examples, data, statistics, facts. Aristotle argued, “it is wrong to warp the jury by leading them into anger or envy or pity: that is the same as if someone made a straight-edge rule crooked before using it” ([1354a], p. 30). Our own scholarship, however, has been guilty of the opposite: we have fallen into an odd, imbalanced rhetorical stance that comes from the stories we tell, stories that appeal heavily to audience emotions but stories that are also uniquely personal to the writer, to which an audience may or may not relate.\(^9\) Annis’s exploration of the three-part theory of epistemic justification is remarkably similar to Booth’s (1963) exploration of why we teach those three parts of rhetoric—together—not to exploit only one or two parts of this human system, but to achieve a natural balance as dictated by context: “the habit of seeking this balance is not the only thing we have to teach under the heading of
rhetoric. But I think that everything worth teaching under that heading finds its justification finally in that balance” (145).

A Contextualist Theory of Epistemic Justification not only re-trains us to seek that same balance in our research, but also grounds us again in a rhetorical tradition in which such a balance was not only sought, but also was the honest thing to do—an honesty and code of ethics explored not only by Aristotle, but by others who followed him, rhetoricians such as Cicero, Quintilian, and St. Augustine, as well as a few scholars of our own time. To balance our justification of knowledge in composition research, we must understand where and how the rhetorical issues and the research issues in which we conduct our inquiry intersect.

A CONTEXTUALIST RESEARCH PARADIGM

In an effort, then, to bypass the dispute between numbers and narratives, we must return to the notion of context and revise our view of divided epistemologies. Numbers as well as narratives naturally occur in most contexts. A Contextualist Theory of Epistemic Justification is a useful template on which to base a new inclusive paradigm, helping us to decide on research methods for a particular project based not on politics or on personal preferences, but on the contexts in which our research questions arise.

Numerous forces shape our research questions and decisions. Those decisions are guided by the contexts in which we work, contexts in which must ask several questions and solve several problems—about method, form, ourselves, our audience, and our evidence. Figure 4.3 presents a Contextualist Research Paradigm for Rhetoric and Composition—a matrix that shows the intersection of the rhetorical issues and the research issues that form varied research contexts. Our use of this matrix should be guided by three simple principles.

1. There are no predetermined answers for any of the questions in the matrix. Researchers must answer these questions in the specific contexts of their own research.
2. Each cell in the matrix, though focused on a particular kind of question, cannot be explored without the others. In other words, no question in any cell can be asked and answered without all of the others
being asked and answered as well. Such is the relationship of evidence, method, form, writers, and audience—dependent on each other.

3. The questions presented in the matrix do not have to be asked in any particular order, as all research could potentially have any starting point, depending on individual contexts.

The questions I’ve placed in each cell of the matrix are general enough to be asked by any researcher. Specific answers and further
questions must be provided in context and will, of course, vary. Each question, however, ties the researcher specifically to the context of the initial research question being asked in the specific context from which the question emerged. Such a matrix, then, keeps us focused on the issue at hand, rather than letting us become embroiled in arguments about competing epistemologies, political defenses of research methods, and an avoidance of quantitative measures that do enhance the knowledge of our field.

In Alston’s words, a matrix such as this helps us move beyond our own consciousness, embracing the interplay of the subjective and objective—an interplay necessary for discovery, for constructing our identities, and for sharing our discoveries. Such a matrix helps us break free of our current anxieties and debates about different research methods so that we are better able to conduct more of the research we—and our students—need, research that will explore what we feel is the truth and what we do or do not believe. Aristotle reminded us, “things related to truth [are greater] than things related to opinion” ([1365a], p. 74).

In this matrix, we see rhetoric and dialectic unfold in Aristotelian terms. In his translation of Aristotle’s *Rhetoric*, Kennedy (1991) retained the original Greek *antistrophos* in the first sentence: “Rhetoric is an *antistrophos* to dialectic. . . . All people in some way, share in both; for all, to some extent, try both to test and maintain an argument [as in dialectic] and to defend themselves and attack [others, as in rhetoric]” ([1354a], pp. 28-29). While current interpretations of *antistrophos* vary (see Green, 1990), the interplay of rhetoric and dialectic was, for Aristotle, determined by context: “Let rhetoric be [defined as] an ability, in each [particular] case, to see the available means of persuasion” ([1355a], p. 36). The pursuit of any research question, then, is based on the dynamic interplay between rhetoric and dialectic and guided by an understanding of contextualist principles.

Phelps (1988), too, applied “contextualist principles” (219) in her own quest to reconstruct “composition as a ‘discipline,’ a human science” (205). Phelps drew heavily from varied theories in an effort to illustrate the potential of drawing together the eclectic epistemic foundations of our field. For instance, Phelps explored the role science
plays when testing our experience but also discussed science-as-background that we fold into our experience. While I agree with much of Phelps’s exploration—and certainly admire her goals—her own context seems unclear, because the text is purely theoretical.

Though Phelps addressed briefly the notion that teachers often object to theory (207), her text doesn’t address that objection directly through concrete examples—an important task in light of her argument that teaching/praxis should be a central issue in our field. Indeed, Phelps’s text is far removed from the contexts of our day-to-day teaching and inquiry. Phelps acknowledged that she gave limited attention to “the fact that students are themselves human subjects in the classroom and the further complication that they are themselves learning to reflect via written language” an element seemingly crucial to her argument. One might argue that Phelps’s reconstruction of composition as a human science—teaching, praxis, integrating varied forms of knowledge with our practice—cannot succeed outside of the context of what we most wish to research: our teaching and our students’ learning.

In contrast, a Contextualist Research Paradigm that focuses on questions (rather than just theory) and that demonstrates how eclectic forms of knowledge could work together in varied contexts (rather than just theorizing that they could) is able actually to release the power of the research process and the actions of the researcher within the specific contexts that produce them.

APPLYING A CONTEXTUALIST RESEARCH PARADIGM

A Contextualist Research Paradigm allows us to see not only the process of our research, but also the products of that research differently. A new lens such as this will enable us to see more clearly the bridges that already exist in the qualitative/quantitative dichotomy. Quite naturally, narratives and numbers often coexist in some fashion in most research contexts. If we truly embrace a wide array of research methods, we will see especially the narrative undercurrent of traditional-looking studies and begin to understand better why researchers make the decisions they do, guided by their understanding of the intersecting rhetorical and research issues present in the context of their work.
To illustrate the matrix at work, chapter five will re-present Eileen Oliver’s (1995) study published in *RTE*, “The Writing Quality of Seventh, Ninth, and Eleventh Graders, and College Freshmen: Does Rhetorical Specification in Writing Prompts Make a Difference?” Inserted throughout the reprint of Oliver’s study are sections of an online interview in which Oliver articulated the story behind her study and her reasons for choosing her methods—a description of decisions made in the context of her desire to answer a research question and to share her discovery. To the traditionally trained eye, such a description will not be a surprise. To those trained only in narrative methods, however, I hope the presentation of these two texts together begins to show how a traditional study can also reflect a “thick description” and—as the matrix illustrates—a natural product of the research and rhetorical processes at work in a scholarly context.

**NOTES**

1. Consider, for example, Enos’s (1996) *Faculty Lives and Gender Roles in Composition*. Enos outlined carefully how she gathered her demographic data—where the numbers came from, how she gathered them, and what they might mean. In other words, she carefully articulated the context from which those numbers emerged. In contrast, not all of Enos’s narratives were so carefully presented. While most stories clearly revealed moments of discrimination, several stories are vague and hard to understand—were out of context—partly because Enos kept the storytellers and their institutions anonymous. Enos, in her introduction, remember, argued that narratives more than numbers tell us who we are (and are more “true”). However, because her numbers are more consistently contextualized, we might find them more “true” than the narratives sometimes taken out of context.

2. The notion of “partial truths” in narrative was articulated by Clifford (1986) in *Writing Culture: The Poetics and Politics of Ethnography*. Clifford argued that ethnographies are fictions in “the ways they are systematic and exclusive. Ethnographic writings
can properly be called fictions in the sense of ‘something made or fashioned.’ . . . Interpretive social scientists have recently come to view good ethnographies as ‘true fictions.’ . . . Ethnographic truths are thus inherently partial” (6-7). Clifford introduced the essays in Writing Culture by emphasizing their attention to the creation of form. Something is always excluded from our stories because “one cannot tell all” (7).

3. In A Mathematician Reads the Newspaper, Paulos (1995) outlined numerous contexts in which the average American pays attention to mathematics, including policy-making in the courts, fat grams in food items, personal finance, presidential polls, and health reports. In other contexts of our lives, in other words, we make numbers important.

4. In a review of five books on science, Selzer (1998) noted that only one of the five, Toumey’s Conjuring Science (1996), “renders science as a benign, trustworthy, liberal, liberating, and admirably self-regulating enterprise that deserves public support” (450). In contrast to the other four books reviewed, “Toumey takes as his given that science is a part of culture, and as such is both a product and producer of it . . . that science is indeed open to humanist scrutiny” (450). Selzer’s complaint about two other books under review, Taylor’s Defining Science (1996) and Gates and Shteir’s Natural Eloquence (1997), revealed our misguided expectations of scientific inquiry and our attention to form: “there is too little textual analysis to satisfy someone like me who understands English studies primarily as the investigation of written discourse” (Selzer, 1998, p. 450).

5. Emig called the scientific method ‘mistakenly named’ (66). Other scholars have similarly doubted the phrase as well: Shapin (1996), Phelps (1988), Ray (1992), to name a few.

6. Bushman (1998) articulated a “Social-Expressivist” writing center based in part on Lunsford’s (1991) three-part division for writing centers. Though Bushman also cited criticism for such divisions, such as Hobson’s (1992) examination of epistemological debates in varied writing center contexts, he defined his terms, as Lunsford and Berlin did, with three ways of knowing—the transactional, expressivist, and objectivist. Like Gradin (1995) and O’Donnell
(1996), Bushman attempted to join only two: the transactional and expressivist.

7. Alston noted that many theories of epistemic justification are not a unified set of theoretical explorations. Indeed, Alston pointed to some epistemic theories’ potential for justifying beliefs even in the face of that belief being clearly incorrect and to some epistemic theories that do not allow for intuition or experience on the part of the knower (3). This, of course, poses a problem for theories of epistemic justification generally, but Alston’s warning here is one of common sense or, perhaps, what happens when common sense is lacking.

8. See especially, On Rhetoric, (1356a): “Of the pisteis provided through speech there are three species: for some are in the character [éthos] of the speaker, and some in disposing the listener in some way, and some in the argument [logos] itself, by showing or seeming to show something.”

9. In spite of the popularity of personal stories and anecdotes in our research, we must examine those times when stories fail to communicate. While I was finishing the first draft of this project, for example, I had numerous conversations with a friend of mine (who has given me permission to relate this), a woman approximately my age who had just started the doctoral program I was then finishing. For reasons I cannot fully understand, she had felt an overall sense of powerlessness in her coursework. She had shared stories with me, trying to give me examples of the lack of power she felt. In turn, I had given her stories in which I tried to share numerous (and similar) times when I have not felt powerless in the same program. Eventually, however, she gained a new sense of power—not through stories, but through reading theories of critical pedagogy. When I asked her why my stories didn’t help her (and why abstract theory did help her), her conclusion was simply that “You’re not me, and I’m not you.” My stories and hers, though they appeared to be contextually similar, were, in fact, different—because they were so personal and could not cross boundaries that two individuals would naturally have.

Briggs (1998) outlined a similar use of narrative—as a writing center director/tutor, Briggs shared a past tale of her own frustration.
with academic norms (an M.A. thesis) in order to help a freshman confront her own frustration in a freshman composition class. Briggs explored why this “narrative as response” worked here, and I have also made such storytelling work with my own students, both in a writing center and in a classroom. But I have more stories that did not work with my students, and in their own words, “But you’re the teacher, I’m a student!” or “You’re an English major!” or, more simply, “Yeah, right, like you can compare your writing to mine!” As much as I might see those connections and try to make my students see them, too, their unwillingness to find what we have in common makes the storytelling fail because the stories are personal and, therefore, different.