Composing Research

Johanek, Cindy

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Our growing defense of qualitative research and storytelling in composition is accompanied by passionate arguments against the older, traditional research paradigm—a passion that, as conversation with others in the field has made clear, makes some of us look the other way or lash out at the “old school” whenever conversation turns to the older tradition. That paradigm, for many, has grown out of a male-dominated tradition, places too much value on mathematics, and is written in a stifling, disinterested style that is unpleasant to read (and write).

More importantly, our abandonment of “traditional” research has been praised for allowing more diverse researchers to express their voices, voices that—as women, people of color, and practitioners—have been relatively silenced until recently. Such shifts bring into focus new epistemological stances that question the traditional ways of knowing, and this epistemological shift has produced attacks aimed at the old research paradigm on two levels: the broader issue of epistemology and the more narrow issue of research methods. Before we review how and why research methods relying on numerical data should remain potentially valuable depending on context, it
is important to address that part of the audience that wants nothing to do with such research.

I focus here on three particular sources of arguments against the traditional research model. While this chapter is divided into three sections, readers will see features and arguments that overlap among the sections: 1) our general anxiety about mathematics and statistics, 2) feminist responses to that older model, and 3) our preference for writing that is more creative and literary than the standard research report. These arguments often relate to each other, but for my purposes here, each deserves its own treatment.

DON’T MAKE ME DO MATH: MATH AND STATISTICS
AVOIDANCE AND ANXIETY

If I could steal a dedication from someone else’s work and use it for my own, I would steal from Paulos’s (1995) *A Mathematician Reads the Newspaper*, dedicated “To storytelling number-crunchers and number-crunching storytellers.” Paulos briefly shared some childhood memories, joked about using Pythagoras and Pulitzer in the same sentence, and speculated on the relationships between mathematics and our daily lives. In the plainest of language, Paulos explained, “The misunderstandings between mathematicians and others run in both directions” (4), but Paulos argued that “number stories” can enhance our understanding of economics and environmental predictions, illuminate our understanding of “crime, health risks, or racial and ethnic bias,” and even eliminate myths surrounding sports figures (4).

For Paulos, mathematics provides insight into popular culture and scholarly pursuits and should not be separated from either, but he understands where much of our anxiety comes from:

[B]ecause of the mind-numbing way in which mathematics is generally taught, many people have serious misconceptions about the subject and fail to appreciate its wide applicability. . . . It’s time to let the secret out: Mathematics is not primarily a matter of plugging numbers into formulas and performing rote computations. It is a way of thinking and questioning that may be unfamiliar to many of us, but is available to most of us. (3)
Indeed, in composition studies, quantifying data is rarely seen as illuminating what Paulos called “people stories” (3) and is seen instead as a separate world, often having nothing to do with people at all, certainly nothing to do with anything pleasant. Phelps (1989) called scientific research “distanced and neutral, sometimes employing elaborate statistical apparatus” (40), suggesting that statistics play a role in separating such research from people stories. Charney (1996), in “Empiricism Is Not a Four-Letter Word,” strongly objected to such distinctions but accurately captured the distaste often expressed toward numbers and the unfortunate stereotype of researchers who use them:

[N]o one likes the way scientists seem to privilege numbers and disparage words—the way numerical and graphic evidence is treated as clean, precise, and solid. . . . misrepresenting the world as manageable, fully determinate, and reducible to clear and accurate formulas. (571)

And if we believe Shea’s (1996) comment in an article on statistical significance in the Chronicle of Higher Education, we would think that everyone in higher education trembles at the mere mention of statistics:

No subject makes the eyes of graduate students in social science glaze over faster, and even many professors view statistics as a necessary bit of drudgery. (A12)

When we bring that “bit of drudgery” into composition research, we sometimes apologize for its presence in people stories, as Lerner (1997) did in “Counting Beans and Making Beans Count,” an analysis of grade improvement among students who visited his writing center and students who did not:

First a caveat: I know that numbers can obscure (and what I’m about to detail does reduce those complex human beings who come to our writing centers down to manageable integers). My own research into writing center settings has primarily used qualitative methods because it’s the processes of interaction, goal setting, teaching and learning that make our work so fascinating. (2)
As if aware of his “anti-bean-counting” audience, Lerner carefully walked his readers through the process of his data gathering and comparison, providing excellent explanations for his choice of methods. He presented his “bean counting” with humor, especially in headings like “Full of beans” and “Bean counters unite.” Lerner praised the “exciting prospect” of numerical data gathered on the National Writing Centers Association website as added proof that “writing centers can and do make a difference” (3). But Lerner captured what many see as the necessary evil of numbers when we direct writing programs or writing centers. After all, we have, for Lerner, a tougher audience to please:

[I]nstitutional mandates, bean-counting administrators, and, ultimately, our professional standing often call for answers. . . . I’ve learned about a whole new level of accountability. . . . I need to anticipate my audience’s needs. College administrators often want numbers, digits, results. (1-2)

Lerner’s justification illustrates Charney’s (1996) assertion that “Compositionists readily assume that disciplines that adopt scientific methods do so for reflected glory and access to institutional power” (576). When we adopt such methods—and their accompanying numbers—for ourselves, then, it is sometimes due to the pressure to gain that same protective power or simply because we feel, apologetically, that we have to. Especially in writing centers, for Kail and Allen (1982), research is necessary for many reasons in writing centers: one reason is to “educate your administration,” and “like it or not, administrators need numbers” (233).

At the bowling alley, we’re gathering numbers to please no one but ourselves. We’re here because we really want to know if your red ball is as lucky as you say. Now that each ball has been bowled 6 times, we have our scores, our data set, showing how many pins out of ten each ball managed to knock down:

Just by eyeballing the raw scores, we note that the red ball seems to have come in second. But don’t panic yet. Thankfully, we have more ways of looking at numbers that can help us out.
For now, you’ve taken the first step: gathering, organizing, and presenting the information you found.

In addition to apologies for numerical data, we have examples of research that gathered such data but surprisingly did not share it. For example, Fitzgerald, Mulvihill, and Dobson (1991), in their work on graduate writing groups in writing centers, conducted a survey of graduate students at their university. They asked graduate students about their preferences for working on theses and dissertations and about the kinds of services the writing center could offer them. The authors referred to the survey (attached as an appendix to the published article) but did not report any of the quantitative data they worked so hard to gather.

The survey asked, for example, if graduate students would prefer multidisciplinary writing groups or discipline-specific writing groups if the writing center offered such services to graduate students. Instead of reporting the answers to that survey question, the authors stated, “the students told [the director], almost unanimously, that they preferred to be in groups with people from other disciplines” (137). Here, the authors preferred—and have given more value to—the testimony of the few students who participated in graduate writing groups, when much more data were readily available about their graduate population generally (data that readers would undoubtedly find useful).

Hunzer (1997) also conducted a survey in a writing center to explore gender stereotypes. After observing that female students preferred working with female tutors because male tutors were “intimidating”
and that male students preferred working with male tutors because female tutors “were not aggressive enough” (6), Hunzer mailed a survey with twelve questions to seventy-four students. She printed three of those questions in her article but did not share any of the responses. While she noted the number of students who responded (39 total, 16 male, 18 female, and 4 anonymous) and the age range of her sample (17-30), she shared results only from the five students who volunteered (and kept their appointments) to be interviewed (7). Responses in these five interviews—not responses on thirty-nine surveys—formed the entire data analysis. While student responses here are interesting to consider in relation to gender stereotypes and student expectations of writing center tutors, more data were available but not given.

Now that you’ve gathered the scores at the bowling alley, we need a way to talk about them. It’s a bit bulky to discuss your raw scores: the red ball scored a 9 and then another 9 and then . . .

Averages or means help us share information with others. We can easily figure the average scores for the bowling balls by dividing the total score by the number of trials each ball had (6).

<table>
<thead>
<tr>
<th>Scores</th>
<th>Green</th>
<th>Red</th>
<th>Purple</th>
</tr>
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<tbody>
<tr>
<td>9</td>
<td>8</td>
<td>6</td>
<td>5</td>
</tr>
<tr>
<td>9</td>
<td>8</td>
<td>7</td>
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<tr>
<td>10</td>
<td>9</td>
<td>8</td>
<td>7</td>
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<tr>
<td>Totals</td>
<td>36</td>
<td>42</td>
<td>48</td>
</tr>
<tr>
<td>Avgs</td>
<td>6</td>
<td>7</td>
<td>8</td>
</tr>
</tbody>
</table>

You still shouldn’t panic. After all, the average, or mean, is only one way of looking at these data. The mean is a measure of central tendency, or a number we can use to summarize the
data somehow, to capture its “flavor,” so to speak, or describe the data in some way—which is why we call it a descriptive statistic. Like adjectives, these numbers describe what we see, so we can more clearly share them with others. Here, “7” is the best descriptor of the red ball’s overall performance.

But there are other measures of central tendency that can describe what we see. Stay tuned. I know you’re going to like one of them.

In “Students’ Reactions to Teacher Comments: An Exploratory Study,” Straub (1997) did present his data, but only descriptively: in a study of 142 students’ ranking of forty teacher comments on a four-point scale (1=definitely prefer, 2=prefer, 3=do not prefer, 4=definitely do not prefer), Straub presented the average score students gave for each teacher comment and some average scores for categories of comments (such as “praise” or “advice”). Straub made comparisons among students’ responses by “eyeballing” the average scores. For example, Straub concluded that

These students were generally receptive to questions, but they were particularly receptive to open questions. . . . The average rating for open questions was 2.08, the third-most preferred mode of commentary in the study, behind advice (1.76) and explanations (1.56). The average rating for closed questions was considerably less favorable: 2.24, only a notch better than imperative comments. (109)

Eyeballing the data to determine differences resulted in Straub’s loose phrasing, such as “generally receptive,” “considerably less favorable,” and “a notch better”—loose comparisons that significance testing would have clarified. Overall, Straub concluded that students “seemed to be influenced far less by the focus of teacher comments than by the degree of specificity of the comments and the modes of commentary” (100). Even though these three variables—focus, specificity, and mode—were a part of this study, Straub presented only one table of data: for mode of commentary.

Statistically, “mode” has a different meaning. While the mean determines the arithmetic average of a set of scores, the
mode describes the same set of scores by looking at the most frequently-occurring score. The mode, of course, does not contradict the mean. It simply gives us another angle from which to view the same thing, as all researchers converge on their data in as many ways as possible to learn as much as possible about it.

Since you want the red ball to win, presenting the mode might help you. Even though the red ball placed second in overall raw score (and, therefore, the mean), you win when it comes to the mode. Of the three balls, the red had the highest mode, or most frequently occurring score: 9. The mode for the purple ball was 8, and the mode for the green ball was only 5.

At this point, the red ball still has hope. While the purple ball achieved the highest average, you could argue that the red ball appears to achieve the highest score more consistently. If you argued only the mean vs. the mode, however, you would never get anywhere. So the difference here between the mean and the mode illustrates two more important points: the mean is not the only number that can describe our data, and we need to look at our data set in other ways in order to understand all of the information it contains.

If Straub crunched his data beyond simple averages, he did not share it. Certainly, looking at the means for each comment helps us describe students’ preferences, and anyone would know that a mean of 2.1, for example, seems different from a mean of 3.1. But how different were they? How much variation is it, exactly? And, for an item that achieved a mean of 3.1, for instance, how varied were students’ responses within that item? Straub didn’t elaborate on his data by discussing or presenting the standard deviation, another descriptive statistic that gives us more information about the averages, except for one comment in a footnote:

The extent of students’ agreement about their strong preference for advice is indicated by the standard deviation on their ratings for the eight advisory comments. It is .84, the lowest in the study, indicating only a minor variation in the students’ preferences for items in the group. (114)
Readers should also be interested in the highest standard deviation in the study, a number that would tell us the extent to which students might have disagreed about their preference for a certain comment, but Straub did not share it. Here, we have a study in which data are presented, but only in part—and data will always contain more than just averages.

At first glance, *standard deviation* looks like an oxymoron, but it is, actually, a very descriptive term. Here, “deviation” refers to how far an individual score deviated (or varied) from the mean. When the green ball scored a 9, for example, we can see that it deviated 3 points from the mean (6.0). Knowing how far each score varied from the mean gives us additional information about those scores and about the mean’s ability to describe the whole data set. But to talk about each individual score, again, gets bulky, so we’ll instead determine the “average” deviation among the individual scores in each set: the standard deviation.

To do that, let’s see how far each score varied from the mean by subtracting the mean from each score. You’ll notice, though, that subtracting the mean from each score will result in some negative numbers. How do we get the average deviated score when we have to add negative numbers? If we add the numbers we get when we subtract the mean from each score, we’ll have zero, as the second column below shows. We solve the problem by squaring that score (multiplying two negative numbers gives us a positive number). Think of it as grammatically correcting a double negative. Later, we’ll have to remember that we squared these numbers.

For now, let’s label our columns to help keep them straight. Let’s work with the red ball as an example, and label the raw score “X,” the deviated score “x,” and when we square that x, we’ll label it $x^2$.

Once we’ve squared each deviated score, we can determine their mean by adding them (22.00) and dividing by the number of scores (6), just like we compute any average.
The average (mean) turns out to be 3.67, but remember we had to square each deviated score to correct for negative numbers. The average, 3.67, then, is the standard deviation squared, not the standard deviation for the green ball’s scores. We can take the square root of 3.67, however, and get that number: 1.91. The scores that the red ball achieved deviated from the mean of 7.0 by an average of 1.91 points.

Calculating the mean of X: \( \frac{42}{6} = 7.0 \)
Calculating the mean of \( x^2 \): \( \frac{22.0}{6} = 3.67 \)
Standard deviation: \( \sqrt{3.67} = 1.91 \)
You can do the math for the green and purple balls.

Similar to Straub’s study, in which he gathered data but did not share a full analysis, was Radencich’s (1998) summary of research done by four of her masters students (coauthors, Eckhardt, Rasch, Uhr, & Pisaneschi). In one of the four studies, which tried to determine a difference in word count in students’ journals when given either teacher-provided or self-selected journal topics, Radencich articulated the data analysis as follows: “Becky computed word counts per journal entry per student and then used an ANOVA to compare those of boys and girls for teacher-provided and self-selected topics. The only difference she found was higher word counts for self-selected than for teacher-provided topics” (88). Here, mere mention that an ANOVA was done seemed sufficient, rather than a

<table>
<thead>
<tr>
<th>X</th>
<th>Subtract the mean</th>
<th>x</th>
<th>x²</th>
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<tbody>
<tr>
<td>9</td>
<td>-7.0</td>
<td>2.0</td>
<td>4.0</td>
</tr>
<tr>
<td>9</td>
<td>-7.0</td>
<td>2.0</td>
<td>4.0</td>
</tr>
<tr>
<td>8</td>
<td>-7.0</td>
<td>1.0</td>
<td>1.0</td>
</tr>
<tr>
<td>7</td>
<td>-7.0</td>
<td>0.0</td>
<td>0.0</td>
</tr>
<tr>
<td>5</td>
<td>-7.0</td>
<td>-2.0</td>
<td>4.0</td>
</tr>
<tr>
<td>4</td>
<td>-7.0</td>
<td>-3.0</td>
<td>9.0</td>
</tr>
<tr>
<td>Totals</td>
<td>42</td>
<td>0.0</td>
<td>22.0</td>
</tr>
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full detailing of the analysis and results. In another of the four studies— one that attempted to measure the effects of different background music (including a no music control) on students’ journal writing—Radencich also ignored numerical data, sharing only the teacher’s observations and students’ commentary. While Radencich’s purpose was to examine her graduate-level course on research, such vague summaries of the research done in the course cannot help us understand the full context in which that research was conducted or what that research might mean.

While other researchers certainly publish the data they find, the above examples illustrate that it is possible to publish research that relies on data without reporting all of that data—or any data at all—or to convert numerical data to a qualitative report. This is a surprising notion, considering how difficult it is to gather that data in the first place: why wouldn’t a researcher want to share the results of such hard work? But perhaps we shouldn’t be surprised to see some researchers ignore the math when research textbooks in our field do the same. Lauer and Asher (1988) explained statistics and measurement in an appendix to Composition Research: Empirical Designs, referring to a list of other suggested readings “for someone without extensive statistical background” (232). In a more comprehensive, sophisticated text, Hayes et al. (1992) reviewed strategies for reading research reports, including the statistics/results section, but suggested to readers who “have trouble reading graphs” that they, too, refer to the list of “additional readings” that can help with such matters (15): “Our objective is not to give you extensive knowledge of subject matter or of statistical methods” (11). And while MacNealy’s Strategies for Empirical Research in Writing (1999) was “intended for novices: those with no background in empirical research and even those who are afraid of math” (ix), MacNealy acknowledged that her introduction to statistical procedures was insufficient: “As you begin to think about possible statistical procedures to use in analyzing your data, you should consult one of the many books on statistics” (x).1

While none of these texts purported to be a statistics textbook, each reviewed several studies and research methods that relied on statistical analysis in order to provide meaning to that research. All
referred readers elsewhere to learn more about statistics on their own, as if it is not the place of the composition researcher to teach stats, or as if readers wouldn’t mind that omission. While these texts offered definitions of statistical concepts, none walked readers through the intricacies of basic procedures to help them understand the logic behind them, in the full context of investigating a research question. Without such help, mere definitions often remain confusing, vague. While these authors acknowledged that some of their readers would need help with (or, perhaps, would have anxiety about) statistical analysis, none took the opportunity to offer full procedural and conceptual help that would ease anxiety or clarify confusion.

Such anxiety is not uncommon in a field more concerned with words than with numbers—with literacy rather than numeracy (Steen, 1990; Snyder, 1990). Steen (1990) argued that, even in careers requiring mathematics, some must overcome “insecurity brought on by their school experience with mathematics” (216). Still others, though often well-educated, “are virtually innumerate; others become ‘mathophobic,’ avoiding tasks or careers that require any use of mathematics” (216) (see also Tobias, 1978, 1987). For Steen, lack of confidence in math or statistics naturally leads to avoidance.

Other forces, of course, have shaped our response to numbers. A Nike advertisement for women’s athletic shoes, printed in popular women’s magazines in the early 1990s (and available now on several websites), captured both a public and a scholarly awareness of the potential harm in a number:

A woman is often measured by the things she cannot control. She is measured by the way her body curves or doesn’t curve, by where she is flat or straight or round. She is measured by 36-24-36 and inches and ages and numbers, by all the outside things that don’t ever add up to who she is on the inside. And so if a woman is to be measured, let her be measured by the things she can control, by who she is and who she is trying to become. Because every woman knows, measurements are only statistics and statistics lie.

We have to be careful, especially in rhetoric and composition, if we believe that statistics lie. Our own field has had to justify rhetoric as
an honorable pursuit, refuting charges that often come with phrases like “mere rhetoric” or “empty bombast”—phrases that suggest rhetoricians, too, can lie. And let’s admit it: words tell more lies than numbers do. After all, we have another way of pointing out liars: “you’re just telling stories.”

FEMINIST RESPONSES TO THE TRADITIONAL RESEARCH PARADIGM: IN SEARCH OF OUR MOTHERS’ VOICES

Science and scientists—and the numerical data and scientific thought accompanying them—have been criticized for years by feminists fighting the combined effect of male domination in science (and in higher education generally) and society’s general acceptance of science as power. There is widespread discussion, of course, of the high number of men over women involved in the sciences, of numerous fields (especially medicine) studying men far more often than women, of differences between men’s and women’s ways of knowing, and of long-standing social expectations of women to engage more fully in the arts or humanities than in the “bolder,” more analytical sciences, often remaining assistants to the men who “do real science.”

Before I proceed, I feel the need to make my own stance clear here. I include a review of feminist arguments about research and research methods only to caution against choosing research methods based only on political ideology or against choosing research methods only because they do not have a male-dominated history. At the same time, several feminists have posed perhaps the most valuable arguments about our research at the epistemological level (producing great changes, of course, in what we hope to know, how we can come to know it, and, most importantly, who can be a valid knower). Still, several others have pushed a feminist ideology stripped of epistemological discussion that could and should include everyone.

Feminists and non-feminists alike may ask feminist or non-feminist research questions, and, of course some questions may be neither. I agree with Harding (1987) that both men and women can engage in feminist inquiry and that research without loyalty to either gender is possible and can be helpful (Harding, 1986). At the same time, however, gender neutrality in research does not always help us
understand women and men when gender is not an explicit variable, and feminist researchers still run the risk of being perceived as “persons who ‘stir up trouble’ over nonissues” (Carter & Spitzack, 1989, p. 1) when they move from gender neutrality to gender studies.

We would make a big mistake if we understood feminist contributions to our research as contributions by and for women only. In spite of some arguments to that effect, I hope instead to present feminist inquiry as aiding our understanding of both women and men and, especially, the unwritten rules of the power structure in which we live. Of special interest here are the arguments put forth about traditional methodological preferences for research. An even bigger mistake than always choosing one method would be to reject a research method only because of its male-dominated history or to prefer some methods because they appear to suit women better. For some, this might sound like an anti-feminist argument that requires more defense.

Like many women, I first came to feminism for survival and for tools for fighting back. In my first semester of graduate school, I had the opportunity in an advanced composition class to write about my own experiences as they relate to a larger social issue. In my essay, I told my favorite shaping stories in an exploration of sexism: my stories about raising pigs when I was in high school so that I could go to college—only to be voted “prettiest pig farmer in town”; about bringing those pigs to market—only to be asked if I was “keeping Dad company”; about working at a farm supply store for a couple of years in college—only to be doubted that I even knew where things were; and about driving a forklift in a freezer during the summer between my undergraduate work and graduate school—only to be told too many times, “let one of the men get it”. (Often, the men who challenged me in these ways “got theirs” eventually, especially when I worked in that freezer—like the men who wouldn’t take my advice to put their tailgate up to hold the 100 pounds of ice they just purchased: I was told not to worry my little head, but I laughed that “little head” off when the ice didn’t make it out of the parking lot, when it started to melt immediately and slid off, and the men were too embarrassed to come back.)

And in my first week as a writing center director, the male professor who asked me if our new furniture arrangement came out of that
same “little head” reminded me yet again that women’s battles—and my own personal ones—are far from over.

I share these stories not only to demonstrate my own fondness for stories and my understanding of what they can reveal, but also to illustrate that my life does not allow me to be anti-feminist, in spite of some questions I have of feminist arguments about our research. Especially in composition, many women are now in positions to make a difference, and we do—for the present and the future. Active research of all kinds will move us forward so that no more histories of male domination are needed to assert our right to the present. Such research will require that we use all available tools to make necessary changes. With this in mind, several texts are well worth reading—with a critical eye for what they mean for all of us.

While I hope we’re ready to move beyond discussions of our male-dominated history, I cannot deny that we still live within the culture that such a history shaped. Go back to the bowling alley and ask yourself if you assumed our bartender to be male or female. Be honest. In the actual text, the bowler has no gender. Your vision of male or female was imposed on the text. If you assumed neither sex, good for you. If you assumed male, it doesn’t mean you’re sexist: you simply live in the same world feminists have been trying to expose and change for decades.

For many, research that relies heavily on numerical data embodies a set of stereotypically masculine values. Relating her experiences as a biochemist, Shepherd (1993) speculated on “the emotional” more often associated with the feminine and “the rational” more often associated with the masculine. In her quest to “unveil the feminine face of science” (as the title of her book suggests), Shepherd traced the male domination of science—as have numerous feminist scholars—and blamed such domination for a decreased public interest in science, for scientific language that has become increasingly cryptic and more separate from the public, and for “the intentional repression of
one such approach, that representing the feminine viewpoint, which has been ignored from the outset” (2).

Science without feeling (in other words, science without the feminine) if taken to its extreme, Shepherd argued, is akin to Nazi scientists conducting experiments on Jews (249)—the extreme result, for Shepherd, of masculine thought that looks only at the objective, at the data and procedures, and ignores what the feminine attends to: the interconnectedness of people and of social responsibility (250). This absence of emotion or of personal involvement in the sciences was addressed immediately by Shepherd’s first sentence of her “Acknowledgments”: “This book is a personal journey, embarked upon to discover and honor the emerging Feminine in myself and in our culture” (vii).

For some, the inclusion of women and women’s issues involves a change not only in research questions and the researchers themselves, but also in research methods. Railing against a male-dominated tradition and its favored research models, we now seek different methods that seem able to embrace and reveal what Shepherd called the “personal journey” and what has become a new epistemological stance. For example, Hawkins (1989) argued that “participant observation, unstructured interviews, and use of personal documents” should be emphasized in research on (in particular) sexual harassment and (in general) any research that recognizes the “reactivity of human beings” (61). Langellier and Hall (1989) argued for interviews as the best method for understanding women’s communication, rather than “sex as a variable measured against male-as-norm” (202), in their research involving women’s personal narratives about food and food preparation.

In composition, Sullivan (1992) also favored such methods for advancing feminist inquiry. While she praised composition for the large number of women who have been pioneers in our field and for not being guilty of studying male populations only (38-39), she curiously proclaimed that women students who enter the male-dominated academy still must learn “modes of discourse that [women] have had little voice in shaping” (40). Sullivan, like Shepherd and others, lashed out at the male dominance in higher education and in the sciences:
Taking gender as the starting point of inquiry . . . is a necessary but not a sufficient condition of feminist methodology, for feminism has as its ideological goal the overturning of patriarchal assumptions and practices that render women’s experiences invisible and undervalued. (50)

For Sullivan, this feminist ideology leads feminist composition researchers to prefer “the cluster of methods that fall under the rubrics of qualitative and naturalistic inquiry” (57), arguing that “traditional methodologies—the research practices and assumptions—of our discipline” continue to allow “men’s discursive practices to define the standard against which women’s writing is judged” (58). Sullivan was highly critical of two texts in particular that have attempted to contribute to composition’s quest for a firmly established research paradigm:

The dominant paradigm, reflected throughout works such as Lauer and Asher’s Empirical Designs and in parts of North’s The Making of Knowledge in Composition, dictates that the researcher must detach herself from the object of inquiry and keep personal bias and values from influencing her observations and analysis if she is to paint an objective and undistorted picture of reality. (55)

Sullivan relied heavily on Harding’s (1987) work, in which Harding questioned the existence of a “feminist methodology” but proposed three important characteristics of feminist inquiry: 1) it should be based on women’s experiences; 2) it should examine phenomena important to women; and 3) it should involve the researcher and his/her experiences and assumptions rather than pretend objectivity through a disinterested stance.

Highlighting the voices of women (including the researcher) seems to be the most important contribution feminist scholars can make to composition. To achieve this goal, Sullivan argued for research methods she called “qualitative and naturalistic”—research that invites a prose style related more to narrative than to the traditional research article:

Techniques such as open-ended interviews and case studies enable researchers to generate descriptions of composing from the point of view
and in the language of the writers they are studying. Participant observation, a defining feature of ethnomethodology, allows researchers to reflect critically on their own subject position, both as researchers and as authors, in the twin sites of the study—in the field and on the page. (57)

While Sullivan concluded that these particular methods would suit a feminist researcher best, she admitted in the same piece that previous case studies—even those on (and by) women writers—have upheld the male-dominated prescriptions of good writers, especially in graduate work. She criticized a case study in her own 1988 dissertation and another presented by North (1987, pp. 37-42) in *The Making of Knowledge in Composition*; both case studies focused on women graduate students who were struggling to succeed. Sullivan illustrated how the lack of a feminist research question and feminist research principles created studies that she later seemed to construe as anti-feminist—in spite of methodologies she argued were naturally suited for feminist inquiry. Both studies, for Sullivan, drowned the voices of the women being discussed and never considered socialization of gender as a potential reason for difficulty in either woman’s writing. In short, while Sullivan argued for case study methods as a means of revealing women’s voices and as an appropriate tool for feminist inquiry (or for inquiry in composition generally), she illustrated how case studies can also distort—through the very subjectivity of the author—that same inquiry. In other words, Sullivan inadvertently demonstrated that method alone cannot determine good or poor feminist scholarship: in spite of her argument that qualitative and naturalistic methods are more suitable for such scholarship, she illustrated two cases in which this was not true. And in spite of her reliance on Harding’s work, which is open to a range of research methods and styles, Sullivan suggested, though indirectly, that any study involving women must be done in a certain way or it will not make a valid contribution (or, conversely, that other, different ways are still acceptable for studying men, with or without their “voices”).

Also relying on Harding’s (1987) question of feminist methodology, Kirsch (1993) constructed and defended her research method in *Women Writing the Academy* according to Harding’s three principles...
of feminist inquiry: focusing on issues important to women, grounding inquiry in women’s experiences, and being personally involved as a researcher. Kirsch presented valuable case studies that examined women’s views of authority and audience in their writing for various disciplines, but, like other researchers, Kirsch defended her method politically and ideologically rather than via her need to answer a particular research question. Even though Harding doubted the existence of a feminist research method, Kirsch adopted Harding’s principles in “method form” and apologized for how traditional that method appeared when written out in chapter two:

The feminist research principles described by Harding informed the design of this study. . . . Although the subheadings of this chapter appear to indicate a rather traditional research report, the discussion within each section, the last section of this chapter, and the overall organization of the book (e.g., the portraits of writers between chapters) all indicate the extent to which this research is shaped by a feminist methodology. (30)

The apology for such a traditional-looking research report suggests the extent to which composition researchers have established their distaste for such research, even for how it looks. In her defense of her chosen method, Kirsch also implied that research in traditional form is never based on experience, never involves the researcher personally, and never examines feminist research questions. Of course, Women Writing the Academy contributes greatly to our inquiry about gender issues and writing and is enlightening and readable. On a less positive note, however, it contributes also to the tension of a false dichotomy—separating (and elevating) one kind of research from (and over) another.

Preferences for case studies and other qualitative forms of research have often been contrasted with what has been called the “masculinization of thought” that requires objectivity, mathematics, and distance on the part of the researcher. A stronger defense of personal narratives and case studies, then, has been developing among several scholars in our field—a field said to be highly feminized (Lauer, 1995; Enos, 1996; Connors, 1995, 1996; Healy, 1995) and, therefore, in a position to be naturally opposed to such masculinization of thought—especially for
feminist scholars who use gender issues as a means of defending such a growing preference. Much in our field has been determined feminine: student-centered and collaborative pedagogies, the nurturing environment of writing centers, and the staffing of most first-year writing classes and writing centers. Changes in the field brought on by women, I believe, are the strongest and most productive new features of our field. It seems natural, then, that preference for research methods mirroring these features—case studies, narratives, interviews, ethnography—are now rapidly following suit.

Even when narratives are combined with numbers in multi-modal research, narratives are sometimes assigned more weight. In Gender Roles and Faculty Lives in Rhetoric and Composition, Enos (1996) presented an extremely valuable study on composition faculty in English departments that gave special attention to gender issues. Enos’s blend of demographic data, survey results, and interviews provided a strong picture of the scope of gender bias in our field, and the stories from many women (and a few men) illustrate all too clearly the often combined struggles that composition faculty still face against literature-dominated departments and that women face in a male-dominated hierarchy. In spite of a large amount of numerical data presented with these stories, however, Enos believed the stories clearly carried more weight: “I believe our stories, more than statistics, tell who we are” (2).

I believe the “power” depends on something besides “reasoned discourse” or statistical analysis. I believe this book’s most powerful use of “data” is the narrative, in the stories that help us define our places in academia so that we can better trace our future. The stories you will hear, more than the “hard data” you will read, use the power of the occasion to make our histories more compelling, more true. (1)

Of interest here is that Enos presented the stories as more true than the statistics she gathered to show the scope of the problem. Without those numbers, however, some value in the stories would be lost (though they would not be “less true”): the numbers and the narratives support each other too well; one without the other would collapse their mutual support on which the power of Enos’s book depends.
Like Enos, who argued that stories are more true for us than statistics, Elbow’s *What is English?* (1990) demonstrated a clear hunger for stories among teachers of writing. Sharing his notes from one session at the 1987 English Coalition Conference, Elbow captured a discussion in which Janet Emig asked other participants, “What are the conditions that all teachers need?” Some participants gave standard responses, such as “smaller classes” and “more time.” Then, according to Elbow’s notes, “Rosalinda Barrera suggests stories” (197).

Stories. Indeed, the blurb on the back cover of Elbow’s book praised its storytelling nature, especially for being “very personal” and for having been written in “a lively and accessible style”—features we would rarely assign to even the best of traditional academic theory or research. Writing teachers naturally gravitate toward methods that not only relieve us of the need to crunch numbers or count beans, but also allow us to share the things we like best: Stories. Stories with style.

For some, stories have provided the foundation for teaching philosophies. Carroll (1997) argued, “English I is about telling stories, about the stories we tell students and the stories they tell us and the stories we construct together. At the same time, it’s all true, not because stories map a unified reality but because stories do have consequences” (932). Welch (1997) articulated the role of stories in her own teaching philosophy:

> I approach composition with the belief that rhetoric and poetics are intertwined, that arguments are underwritten by stories, and that these stories work powerfully as forms of persuasion. . . . I learn from stories. (939)

In a positive review of five books on storytelling and teaching writing, Welch argued, “All five can teach us about the shapes and uses of stories in our field” (940).

Perhaps this passion for stories is what made Rose’s *Lives on the Boundary* (1989) so popular. The subtitle gave it away: “A Moving
Account of the Struggles and Achievements of America’s Educationally Underprepared.” For me, the book was, indeed, moving, especially in those places where I saw stories much like my own. Rose relied on stories—stories from his own life and from others—and, for Rose, there was a reason for that:

The stories of my work with literacy interweave with the story of my own engagement with language. Lives on the Boundary is both vignette and commentary, reflection and analysis. I didn’t know how else to get it right. (xii)

Other researchers also attempt to get it right through stories. For composition studies, storytelling serves as the primary selling point of methods such as ethnography; as Brodkey (1987) so concisely articulated, “All ethnographies begin in stories”:

One needs more, not fewer, ways to narrate experience, for the value of ethnography inheres in neither analysis nor interpretation, but in the researcher’s decision to examine lived cultural experience—to conceptualize it, reflect on it, narrate it, and evaluate it. (32)

Relying on Foucault’s (1977) discussion of authorship (“What Is an Author?”), Brodkey explained that ethnographers, not the method, tell the story. In contrast to experimental replications or repeated explications of the same poem (works that “display methodologies” more than their “authors”), ethnographers are in charge of their “candidly authored” works and construct narratives anew; they are, therefore—for Brodkey and in Foucault’s terms—authors (27).

For those who have explored the place of stories in our scholarship, a new focus on the author (and on authorship) holds power in other ways as well. Gannett (1995), for example, explored the story as not only a means of sharing our lives or reflecting on our lives, but, more importantly, as a means of making our lives, as the title of her essay (in Phelps and Emig 1995) explicitly stated: “The Stories of Our Lives Become Our Lives.” In part, Gannett reviewed, as others have, a male-dominated history that once prevented women from engaging in academic discourse, a restriction that attracted (forced?) women to the set of genres known as journals and diaries (114), a set of genres
that Gannett argued is now entering university discourse: “In the university, women have begun to valorize and reclaim the discourse traditions they have historically found empowering” (124), a discourse tradition based on the personal and social meanings found in stories.4

Storytelling also has the power to construct our identities as classroom teachers. Royster and Taylor (1997) explored the identity of the basic writing teacher through Taylor’s teaching journal5—an identity often lost in the scholarship that attends more to constructing instead the identity of the basic writer. Of importance to Royster and Taylor, however, was not only storytelling, but also a critical look at the nature of storytelling as an inclusive tool with the potential to construct identities for a diverse group of teachers:

In one way, this article is yet another call to story as a very useful methodology for sharing classroom experiences—this time with the gaze on the teacher. Our call, however, is also for a critical step back from our narratives to make them reach out more inclusively and more meaningfully for the general landscape of our work. At this point, our view is that we need to think, not only about ourselves in classroom space, but also about the art of storytelling in terms of its theoretical and political implications. What have we learned about the telling of stories? How do we assign meaning and draw value for the classroom cultures from which our telling comes? (42)

In addition to constructing academic identities, storytellers and authors of ethnographies are able to connect their personal lives/identitites to their academic identities, to “bridge . . . a rather large gap between academic research and real problems” (Moss, 1992, p. 153). For Moss, ethnography and its focus on the everyday activities of communities allowed her to take a personal approach to her research on literacy in the African-American church, an approach in which the story had clear value for Moss:

[Ethnography] was the only research method I had been introduced to that allowed a researcher to tell a story about a community—a story told jointly by the researcher and the members of the community. (154)
Here, Moss reminded us that the author of an ethnography not only constructs the narrative, but she is also a part of the study itself “in more than some abstract ‘researcher’ way” (154; see also Radencich, et al., 1998).

Authorship in research was also important for Newkirk in “The Narrative Roots of the Case Study” (1992). Critical of those who justify case study research by “straddling paradigms” (132)—defending case studies as scientifically rigorous and generalizable while upholding the individualized narrative at the same time—Newkirk presented the need for a case study paradigm that he understood could be perceived as “dangerous” because it is not one “of methodology and objectivity, but of authoring and the cultural values embedded in various narrative plots” (133). Newkirk argued that we have not yet embraced such a narrative-based paradigm because of “the consistent warning in the educational research textbooks. . . . The great god of Methodology is invoked to protect the researcher from charges of storytelling” (133). The value of storytelling rests, however, as Newkirk argued,

. . . on a core of mythic narratives—deeply rooted story patterns that clearly signal to the reader the types of judgments to be made. . . . As readers of these studies, we find them true or convincing, not because of careful methodology (important as that is), or because of wealth of detail, but, I would argue, because of the gratification we get from seeing cultural myths being reenacted. (135-136)

As these cultural narratives are reenacted, the author, more than the method, controls the text, and while Newkirk did not say so, such a shift from method to author allows one important feature to be revealed that authors have and methods apparently do not: emotion. Storytelling, more than statistics, allows our emotions to emerge, an act that, in Newkirk’s terms, brings us gratification.

Paulos (1995), intrigued by the popular need for gratification, pointed out the popularity of emotional appeals (rather than evidence) in the media, law, and business. And while our own scholars put forth their own reasons for storytelling and for the emotional involvement of the researcher, Paulos proposed his own (less favorable, more dangerous) reason for that natural desire:
It’s easier and more natural to react emotionally than it is to deal dispassionately with statistics or, for that matter, with fractions, percentages, and decimals. The media (actually, all of us) frequently solve this problem by leaving numbers out of stories. . . . [W]e all tend to be unduly swayed by the dramatic, the graphic, the visceral. (80-82)

For Paulos, we avoid the mathematical/rational/statistical in part because it is difficult and because the emotional is easier and feels more natural. We can find numerous examples of such emotion in composition scholarship. As Newkirk, for example, shared his own experience with case study, he related his struggle to find the narrative thread on which his final product would ultimately be based:

I had to “intensely consult and intensely ignore,” keeping the data I needed, putting aside the rest, grieving a bit for all I had to leave out.

It is a lonely feeling, and for a while an empty feeling. But I was not totally alone because I had patterns of other narratives to draw on. I could make new stories out of old ones. (150)

Shifting our attention from the method to the writer greatly enhances the emotional side of our research while increasing variability in texts, audiences, and the subjects or stories themselves; indeed, such attention to authorship and narratives, for Newkirk, has been seen as “radicalism” in composition research, going against the grain, so to speak, of traditional research methodologies, when, paradoxically, case studies and ethnography are based on equally long-standing traditions of “enduring narratives” such as the tragedy (136). This radicalism, however, is demonstrated by other scholars who believe that allowing the method and data to speak through experiment or other standard, traditional methods is to allow the traditional genre of academic prose to remain unquestioned, undisturbed.

Indeed, for most storytellers in composition, the value of a story rests mostly in its potential for political resistance to academic tradition. In *Narration as Knowledge*, Trimmer (1997) explored the subordinate role that stories (and the English teachers who love them) have held in the academy:
We became English teachers because we loved stories. We loved reading them, writing them, and talking about them. . . . But as we worked our way into our professional lives, we slowly, almost imperceptibly, changed our attitude toward stories. We lived in a world that did not trust them. Stories were not true. Stories were not reliable. If we wanted to keep stories in our lives, we had to convert them into something else. Something more serious. More scientific. (x)

For Trimmer, English teachers have compromised their love for story under pressure from an academy that values science: we ask students to write stories only to diagnose their errors; we teach them to “dissect plots and theorize themes,” to be analytical rather than creative (x). The analytical dissection of text is expected in traditional academic prose.

Kirsch (1993), too, resisted such traditional academic prose when she refused to traditionally conclude her work on women writing in the academy:

Conclusions demand that an author summarize and unify, make coherent what might be otherwise fragmented, impose order and control on material that might be otherwise out of order, out of control. . . . [C]onclusions can lead to erasing differences, and erasing differences can lead to the silencing of voices. . . . It is that kind of silencing, that kind of concluding I would like to avoid here. (125)

Instead of a traditional conclusion, then, Kirsch reminded us that the stories in her volume were presented without interruption so they may speak for themselves, to “become audible” (126) in an academic system blamed for drowning those voices and stories. For some scholars, then, stories contribute to our scholarship by throwing in a wrench that reminds us that academics and academic research come from anything but a well-oiled, efficient machine. Trimbur (1993) praised this value of stories in his “Foreword” to Kirsch’s work:

The stories women tell about their lives writing in the academy are worth listening to in part because they challenge the conventional view of academic publication as a seamless meritocratic system that recognizes
significant work, rewards talent, and ignores the rest. . . . Their stories also
call into question the genres in which academics write and the reasons
they write in the first place. (x-xi)

The same kind of “breaking free” from traditional academic prose
also formed the foundation for Sullivan and Qualley’s collection of
essays, Pedagogy in the Age of Politics (1994) in which

authors locate their inquiry in their own practices as teachers, scholars
and theorists, writing from their own narratives and not merely from (or
about) the master narratives currently circulating in academe. (xii)

Here, stories allow the individual to “come forth” and combat tra-
ditional modes of inquiry we somehow find limiting or constraining
to our individuality.

In a review of Pedagogy in the Age of Politics, however, Jacobs
(1997) warned that such diverse, individual narratives “contributed
to the diffuseness of the volume and the sense that contributors are
isolated rather than members of a social network” (465). Indirectly,
then, Jacobs warned that such narratives could lead to a collapse of
community: stories are often given value for the sake of the individ-
ual telling a story or for the flavor of the story itself, rather than the
story’s relationship to other stories, to other storytellers, to the field,
or to other kinds of inquiry. Jacobs perhaps sensed the potential for
stories such as these being told outside of a larger context—specific
criticism often reserved for quantitative research (as many scholars
have condemned data-gathering as devoid of context).

SUMMARY

Our researchers have, on the one hand, successfully highlighted
the voices of marginalized groups as valuable contributors to the
field, have critically questioned the status quo of university and
departmental hierarchies, and have produced scholarship to which
the majority of the field can personally relate. On the other hand,
some proposals that “the rubrics of qualitative and naturalistic
inquiry” (Sullivan, 1992) are best suited for such goals have stemmed
from a desire to be different from a male-dominated history and
male-only research and research communities. In addition, other arguments and preferences for these methods have expressed the relief we feel when not required to do math or to write or read those old-fashioned, boring research reports.

We must be careful not to dismiss particular methods—especially those that rely on numerical evidence—as anti-woman, anti-humanist, or anti-creative, for to do so would be to blame the vehicle for having had a lot of bad drivers. Research relying on numerical data is still a dependable vehicle for getting us to some of the places we need to go, and we need all possible vehicles in order to convey the most valuable and diverse body of knowledge possible. Such a vehicle need not be so strangely driven, however: instead, we should all become the best drivers we can, ready for any road.

All research methods and how we teach those methods to others can be done in such a way to include the feminist, to understand math as a storytelling language in its own right, and to include the narrative as a foundation for and an extension of that research in relation to experience and practice.

Chapter four will construct a means through which such an inclusive view of our research might occur. Letting go of dichotomous language, bypassing debates among competing epistemologies, and returning to the roots of a long-standing rhetorical tradition—a Contextualist Theory of Epistemic Justification will help us begin to understand our research needs in the contexts from which they arise, provide us with a more inclusive language, and enable us to further our training in even more diverse research methods.

NOTES

1. In a 1998 survey of subscribers to consortium-l@mtu.edu (a listserv devoted to graduate studies in composition), none of the respondents (N=8) indicated that their composition program included a statistics course. While some graduate programs in composition (such as my own program at Ball State University) require statistics training for their graduate students, such requirements are clearly
unpopular. Permission for this survey is on file at Ball State University, IRB Protocol ID #98-160.


3. Another case study that drowned the voice of the student in question was “The African-American Student: At Risk” (Gill, 1992) published in College English. Gill argued that African-American students face situations that are unique. She proceeded to describe an African American male who lived in poverty in a fatherless home with his mother supported only through welfare; he needed to work full-time in order to supplement his family’s income, and he was the family’s only hope for a college graduate since his older sister had become an unwed mother (225-226).

   While the problems this student faced have been frequently associated with the African American community, not all African American individuals have faced situations similar to this student’s, and non-African Americans face similar problems. Gill chose an extreme scenario to illustrate her point and did not include the student’s voice or texts.

   Unfortunately, Gill’s primary purpose for sharing such a scenario was to argue the benefits of giving African American students positive feedback and praising strengths and improvements in student writing (226). However, she did not illustrate how that kind of reinforcement enabled the student in her case study to succeed, nor did she examine why such reinforcement is uniquely successful with African Americans.

   For a strong case study, see DiPardo’s (1992) “‘Whispers of Coming and Going’: Lessons from Fannie” (Writing Center Journal, 12.2, pp. 125-144). DiPardo focused on a Navajo female student (Fannie) for whom English is a second language and an African American female tutor (Morgan) in her second semester of tutoring. DiPardo’s case study is informed by a sensitivity to language, culture, class, and gender: she related passages from interviews with both the student and the tutor, portions of Fannie’s essays, and excerpts from taped tutorials. She wove these voices
with her own and with scholarship on language, culture, and tutoring/teaching with a unique blend of sensitivity and authority.

DiPardo’s case study included as much writing and talking produced directly by the women she was studying as passages that were her “own.” Further, DiPardo’s case study did not purport to generalize about Navajo students or African American tutors; instead, she used this case study to examine effective and ineffective tutoring strategies and the need for strong tutor training. DiPardo’s case study won the 1993 Outstanding Scholarship Award from the National Writing Centers Association, and the piece was reprinted, most likely for its tutor training value, in Murphy and Sherwood (1995), *The St. Martin’s Sourcebook for Writing Tutors*.

4. While it might be true that the stories women have historically told in diaries and journals ultimately became empowering, Gannett also admitted that men’s diaries were published far more often than women’s and that diaries and personal writing that women managed to publish were most often for the purpose of illuminating the life of some famous man (125). While much discussion and research might portray personal narratives and journals as uniquely feminine, we must not forget that many stories of historical importance were uniquely masculine. For instance, we have few slave narratives from women (Gates, 1987), and most slave narratives were introduced by white abolitionists who attested to the credibility of the slave’s authorship.

Gannett’s argument that personal narratives (especially in journals and diaries) are, by their very nature, empowering requires more discussion. I tend to agree more with bell hooks (1989) that diaries and the personal stories in them have the potential to serve as another silent place where women, especially young girls, are “holding and hiding speech” (7) that does not necessarily empower them so much as it maintains their silence and their status as “seen but not heard.” Indeed, what makes a story empowering is not always the story itself or the nature of narratives, but the changing culture around the narrative that changes how those narratives are perceived.
Taylor’s journal, excerpted in Royster and Taylor’s article, serves as an interesting illustration of the connection between our literary training and our desire to tell stories. Most of Taylor’s headings for her journal entries/stories were framed with literary references: “September 25: Great Expectations,” “September 29: The Outsiders,” “Late October: Invisible Man,” “The Grapes of Rap,” and “Final Portfolios: Grim Fairy Tales,” to name a few.