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The Method Section as Conceptual Epicenter in Constructing Social Science Research Reports

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In this article, the author argues that Method sections in social science research reports, particularly those that employ qualitative methods, often lack sufficient detail to make any results that follow from the analytic method trustworthy. The author provides a brief review of the evolution of the Method section from the 1960s to the present, makes a case for a more robust reporting of research method, and then outlines one way to achieve the end of providing a detailed, specific account of research methods that enable readers to understand unambiguously the means by which data are rendered into results. This consideration includes attention to the reporting of data collection, data reduction, data analysis, and the context of the investigation to make it clear why an illustrative presentation of data supports the claim that it offers.

Keywords: *methodology; research methods; publication; writing research reports; publishing research reports; qualitative methods in writing*

One consequence of being a former journal editor is that for some reason people think that you know what you're talking about. As a result, other editors frequently call on you to evaluate manuscripts submitted to their journals. Before Michael Smith and I coedited *Research in the Teaching of English* (*RTE*) from 1996-2003, I was asked to review a half-dozen or so manuscripts a year. Since our *RTE* editorship ended, my workload has picked up, with the number increasing annually (I reviewed 33 manuscripts in 2007 and usually write about a half-dozen tenure-and-promotion cases as well). Given that during our 7-year term of service with *RTE* we averaged

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over 100 manuscripts a year while also individually reviewing articles submitted to other journals, I've read a heck of a lot of manuscripts over the last dozen or so years, covering a lot of ground. I've reviewed for 28 different journals in the fields of literacy, teacher education, linguistics, composition, communication, anthropology, educational psychology, cultural psychology, cognitive psychology, early childhood education, reading, general education, and other disciplines. If only our merit pay system recognized this work as important. Aside from the articles included in tenure-and-promotion dossiers, these manuscripts for the most part have not been published, suggesting the difficulty that most people have with the genre of the scholarly article.

But impressing you with my workload is not my reason for listing these experiences. Rather, it's to say that I've evaluated about every kind of manuscript you could imagine and have developed a pretty good idea of the sorts of problems people create for themselves when writing scholarly papers. (I wish I could say that I consistently avoid these same problems in my own writing, yet reviewers have been known to fault my papers for the very shortcomings that I see in others' work.) In my view, what can, and perhaps should, serve as the conceptual epicenter of a manuscript is the Method section, which unfortunately, too often, is an afterthought more than a driving force in authors' presentations of research reports. I argue in this article that greater attention to the Method section would strengthen the account of the conduct of the research for the benefit of both author and readers and serve as the nexus for the other sections of the paper's organization and alignment with one another.

A Brief and Incomplete History of the Method Section in Literacy Research

Through my reading of these many manuscripts, I can only conclude that the Method section is not nearly as important to others as it is to me. Perhaps this limited sense of value follows from the olden days of research in education and literacy-related fields, that is, before the mid-1980s when all hell broke loose in terms of methodological pluralism. Braddock, Lloyd-Jones, and Schoer (1963) set the terms for the conduct of research in the domain of English back then, attempting to move the field of composition research into a new and unprecedented era of stature by positioning the experimental study—with variables reduced to frequencies that could be statistically compared to identify effective means of teaching—as the pinnacle of scientific inquiry in composition studies.

Braddock went on to become the founding editor of the National Council of Teachers of English's *Research in the Teaching of English* in 1967. *RTE* served as one of the few outlets for empirical studies of teaching and learning in the academic domain of English. Researchers at that time were not concerned with literacy practices in other arenas: the workplace, the community, disciplines other than English, and other settings outside school. This classroom bias persisted through Hillocks's (1986) sequel to Braddock et al.'s research review. Hillocks himself was a devotee and practitioner of experimental studies and research in school-based writing instruction, even while many in the field had begun to reject such designs in favor of small-sample studies predicated on newly imported theoretical perspectives and beliefs about the value of taking a nonnumerical, in-depth look at fewer participants (see, e.g., the critical reviews of his study by Durst, 1987; Newkirk, 1987).

The reporting of an experimental study's method does not require extensive explication, as evidenced by the brevity of the Method sections in articles published in journals such as *RTE* till the mid-late 1980s. At this point in the field's development, *Written Communication* and other composition journals were founded to address newly recognized needs for studies in workplace and community writing, writing in the disciplines, and other composition production not necessarily linked to formal instruction in school. In experimental research articles, most authors explained the "treatment" they were contrasting with a control group, which often went undescribed, causing Hillocks (1986) to reject most of the experimental studies conducted from 1963-1983 in his meta-analysis of composition research. Experimental researchers further described how particular variables were controlled for in the design, explaining such features as "counterbalancing" to minimize the effects of interventions such as the topics included in writing prompts. These studies additionally included attention to the particular statistical tests applied to the data to produce the results: a chi-square test, a *t* test, an analysis of variance, an analysis of covariance, a multivariate analysis of covariance, and others.

The method, then, was fairly straightforward, requiring little theorization or exegesis regarding the construction of data. Aside from explaining the differences between the comparison groups (e.g., teaching heuristics vs. teaching "traditional grammar" or more vaguely vs. "the control group"), the nature of the data (e.g., the identification of primary traits in student writing), the control of variables (e.g., teacher effects, order effects, etc.), the data collection points (e.g., pretest vs. posttest student writing samples), and the sort of statistical tests run (e.g., a *t* test, an analysis of variance), the author had little other explaining to do. The students themselves were typically presented as generic, a condition later viewed as problematic given researchers' tendency to study White, middle-class students and generalize from them to all populations (for a critique of such sample selections, see the contributors to Cole, Engeström, & Vasquez, 1997).

When language and literacy researchers began to borrow from paradigms outside the experimental approach in the 1970s, methodological explication became more important. First, often the theories and methods invoked were from outside the general reader's experience and so called for clear outlining, as Flower and Hayes (1981 and many other publications) did when importing the investigative method of protocol analysis from cognitive psychology in order to study the recently conceived idea of writing processes. Second, the assumption of researcher objectivity came under fire, suggesting the need for researchers to acknowledge and account for social construction of their data (Smagorinsky, 1995). Third, with greater attention to the relational nature of research, researchers were called on to explain more about the context of the investigation: the social and cultural experiences of the participants; the physical, social, and political setting of the research; the assumptions at work in the environment; the researcher's relationships and interactions with the participants; and much more.

On the whole, then, it became incumbent on researchers to account for far more than they had previously provided in order to explain the conduct of their investigation. Increasing attention to the social complexity of research begat a greater need to implicate method in results, presenting authors with new obligations as they wrote their articles. Meanwhile (and as is still the case), many journals adhered to the page requirements of a previous era, requiring a host of new decisions for authors who needed to account for research method and investigative context and who needed to explain complex data sets in sufficient detail to be persuasive, all the while keeping manuscripts to 20-30 pages. (As one who tends to err on the side of excessive detail in these matters, I'm thankful that *Written Communication* has never followed this rule too closely. And during our editorial term at *RTE*, Michael Smith and I waived the page limit entirely in order to allow authors greater latitude with the Theory and Method sections of their articles, to the satisfaction of some readers and ire of others.)

The Method section, then, has evolved to the point where, in order for results to be credible, the methods of collection, reduction, and analysis need to be highly explicit. Further, the methods need to be clearly aligned with the framing theory and the rendering of the results. Given that the vast majority of manuscripts that I review fall short in these areas, it seems worthwhile to explore what at least one reviewer believes to be important about the construction and presentation of an article's Method section.

Why the Method Section Matters

When journals do allow extra space, authors do not necessarily generate Method sections of greater clarity. In my work as an editor and reviewer, the area that most frustrates me about authors' treatment of method is the absence of detail. Time after time, I run across something such as the following:

> I read the data several times. In the first reading, I generated provisional categories to guide my subsequent readings. Then, I read the data and refined these categories, looking for themes and patterns. Upon further reading I created the ultimate categories used for the analysis. And now, my Results.

I'm all for multiple readings of data and continual refinement of categories but I need to know specifically what the categories are and how they evolved through the process of data reduction and analysis, how they are applied as codes, how they work in relation to the author's framing theory, how they are reduced from a "raw" state to "cooked," and so on. To belabor this culinary metaphor: Imagine reading of a wonderful dish and being told how to prepare it as follows:

> First, select all ingredients that could conceivably go in the dish. Review them carefully, then pick the ones you want to use and put the rest back in the pantry, perhaps saving them for another meal that you will prepare later. Then reconsider the ingredients you've selected and decide which are most important. Do this again just to make sure. Then mix the important ones together and give it a taste, adding other ingredients as necessary. Put them in cookware, heat, and serve.

Based on this set of instructions, I'm not sure whether I'm making a casserole or a pie, whether the spices are Indian or Thai, whether I heat them in an oven or a fondue pot, or much else.

I have the same feeling all too often when reviewing manuscripts for journals: I have only the vaguest sense of what the author is doing with the data in order to render it into results. If I don't know pretty clearly how the researcher is conducting the study, then it doesn't matter much to me what the results are because I have no idea of how they were produced. To me, that's reason enough to recommend that the article not be published. I don't think that replicability is necessary in the conventional sense, that is, of conducting an identical study with identical results, which serves to corroborate the quality and veracity of the study being replicated and the validity of its findings. The "social turn" in literacy studies in the past two decades (see the contributors to Smagorinsky, 2006) suggests that people from different backgrounds (e.g., from different cultural groups, genders, socioeconomic classes, races, ethnicities, religions, and other categories) will not necessarily act in the same way under the same conditions. Indeed, many have argued that particularity, rather than generalizability and replicability in this sense, is a worthy aim of research (e.g., Bloome & Bailey, 1992; Valsiner, 1998). Even if I find the historical sense of replicability to rest on an increasingly fragile foundation, however, I do think that I ought to be able to reconstruct a study's design based on how an author explains it. In most cases, unfortunately, authors are far too nebulous in their account of method for me to have any idea of how to do so.

In this article, I'd like to argue for greater attention to accounts of research method, both for the reader's sake and the writer's. As a reader, I simply need to know how data become results in order to trust the author's claims. But for me as a writer, the Method section plays a pivotal role in the production of a research article. It serves as the core from which radiate the content and organization of each of the other sections of an APA-style research report. I'll next outline how the Method section functions in my own writing, with the hope that an account of this process will be of use to others.

The Method Section: One Person's Primer

Writing research articles is, and should be, difficult. If it were easy, anyone could do it. But the issues are complex, the genre difficult to master, and the analytic work mind- and nerve-racking and enormously time-consuming. In the next section I outline issues that arise in the collection, reduction, and analysis of data; in reporting the context of the investigation; and of problems that I often find as a reviewer in descriptions of these processes in manuscripts that journal editors ask me to evaluate.

Data Collection

Describing a data collection is probably the most straightforward part of accounting for method. Generally, this section includes a description of the data sources and how they were collected: field notes, interviews, audio recordings of discussions, ancillary artifacts, samples of writing, and so on. But merely listing sources in a general way is typically insufficient. As Chin (1994) has argued, simply announcing that data are composed of "interviews" overlooks the fact that interviews may be conducted in many ways, obligating the researcher to be explicit about who conducted the interviews, whether or not multiple interviewers were involved and if so, how consistency across interviewers was achieved (e.g., relying on a uniform interview protocol or set of prompts and providing the text of such scripts), and other factors that help to reveal the specific nature of the data collection. I use interviews here for illustrative purposes; virtually any qualitative research method benefits from explication of this sort.

Limitations and cautions about the data collection procedures also merit attention. Interviews, to return to this example, are not benign but rather involve interaction effects. Rosenthal (1966) examined researcher effects in behavioral research and identified a myriad of characteristics that can affect the relationship between a researcher and participant, in turn helping to shape the data that emerge from the collection process. For instance, female participants tend to be treated more attentively and considerately than men, female researchers tend to smile more often than their male counterparts, male and female researchers behave more warmly toward female participants than they do toward men (with male researchers the warmer of the two), White participants are more likely to reveal racial prejudice to a White researcher than to a Black one, gentile subjects are more likely to reveal anti-Semitic attitudes to a gentile researcher than to one whom they perceive as Jewish . . . the list seems endless. Making some effort to account for these phenomena helps to explain the social construction of data in studies involving researcher-participant interactions.

Further, the report of method should be tied to the study's motivating theory in terms of data collection, reduction, and analysis. For instance, I have used the method of protocol analysis for many studies. Broadly speaking, the collection of a protocol involves the recording of a research participant's verbalization while working to solve a problem, usually one specified by the researcher: to play chess, to work on economic problems, to read a particular kind of text, or to engage with another task of cognitive interest to the researcher (and, one hopes, the participants). The purpose is to generate a verbal text that a researcher can analyze to provide an account of cognitive processes.

Ericcson and Simon (1993) provide a magisterial account of the ways in which protocol analysis was conceived within cognitive psychology's information-processing paradigm so that its terminology and concepts not only categorize data but illustrate and amplify theory. They describe two types of protocol collection: *concurrent*, in which a participant thinks aloud during the process of completing the task, and *retrospective*, in which a participant completes components of the task or the whole task and then is prompted to reconstruct the process from memory. To these two types I would add *stimulated recall* (Bloom, 1954; DiPardo, 1994; Rose, 1984), in which a person is filmed while working on a task and shortly thereafter is recorded while watching the film and reconstructing the cognitive processes for the researcher, often with question prompts to facilitate the generation of the account. I have studied not just individuals but groups whom I have recorded as they talked while or after working using each of these three methods to capture the cognitive and, inevitably, social processes involved in the participants' analytic and interpretive work.

Initially, I followed the guidelines of Ericcson and Simon (1993), locating my research in the information-processing paradigm in which they worked and using my studies to make inferences about student writers' processes; this work was greatly influenced by the approaches taken by Flower and Hayes (1981 and many other publications) and Bereiter and Scardamalia (1986 and others), who themselves were working in the tradition described by Ericcson and Simon (1993). Later, however, I shifted my own theoretical orientation to more sociocultural perspectives grounded in the work of Vygotsky (1987), which is concerned with the ways in which cognitive frameworks are internalized through cultural practice. This shift required a reconceptualization of protocol analysis, which I felt could serve as a useful investigative tool even when removed from its information-processing origins.

But first I had to retheorize and repurpose it so that it made sense to me and to readers as a method that could account for cognition from a more social perspective (see Smagorinsky, 1998). In terms of data collection, this move involved departing the clinical settings employed by Ericsson and Simon (1993) and asking participants to turn on their recorders while writing whenever and wherever they felt comfortable doing so. I thus had to rely on students' self-prompting both to initiate a data collection session and maintain a steady stream of speech as they worked.

Particularly when I first began this shift I had to provide detail in the publications in which I reported the research (e.g., Smagorinsky, 1997) to distinguish my approach from that of my information-processing antecedents. My paradigmatic reorientation thus obligated me to account for my decisions in data collection so that they were aligned with my motivating theory. Even something as seemingly simple as describing data collection procedures, then, is fraught with the potential for reviewer skepticism. Authors need to balance the need to explain these phenomena in detail and the simultaneous needs to stay within page limits and not test readers' patience with excessive detail before even getting to the results. My approach is to err on the side of abundant detail and allow the editor to decide where to draw the line; at least I've made my case for the validity of my approach to my primary gatekeeper. Given that every editor has different priorities, I'll let her or him establish the parameters for detail on method after I've demonstrated my own understanding of the conduct of my investigation.

Data Reduction

Data reduction is a critical part of research method but gets scant attention in publications about the conduct of research. All researchers reduce their data: some to numbers, some to words, some to both. I've often heard graduate students talk about their "thousand pages of data," with which they are most impressed. The researcher's task is to take this amorphous mass of data and reduce it to something comprehensible and useful.

The familiar pattern I've seen so often in manuscripts I'm asked to review—that of providing a general account of data reduction involving reading, finding provisional themes, and developing and refining codes, yet without explicitly naming them—does little to illuminate for readers how a researcher has reduced data from an inchoate corpus to a systematically organized set from which a subset can document representative trends. As a reader, I want to know the principles by which an author has either eliminated data or selected something representative. Simply announcing that something is representative of the larger corpus is not convincing. During our *RTE* editorial term, at times we required an author to tabulate the whole data set in order to demonstrate the representativeness of what was presented as illustrative. It often turned out that the samples presented were more representative of the author's preferred conclusion than what the data actually produced about the focus of the study. Such authors were hardly in the minority; impressionistic data reports often involve selectively chosen data designed more to confirm a researcher's preconceived thesis than to mine the data exhaustively to understand what they suggest or reveal.

Related to the reduction of data so that representative samples are available is attention to disconfirming or discrepant data, that is, data that are unrepresentative of the corpus as a whole and that raise questions about available generalizations. Disconfirming data can be important for several reasons. First, they may disrupt neat interpretations of the trends and complicate conclusions available from the analysis. Second, disconfirming data may serve as a separate focus of analysis. If, within a research site, a person or subset of people perform anomalously, they often merit attention, particularly if they share traits as members of some sort of minority group relative to the whole (as might a few men in a classroom of women, the subset of novices in a larger group of experienced workers, and so on). Attending to anomalies, then, may challenge norms practiced by the majority within a group or serve as a separate focus of attention for a better understanding of what is not representative of the whole.

My reviewing of manuscripts suggests an unwillingness on the part of many authors to think about why their preconceptions might be wrong by interrogating the complete data set through exhaustive, systematic analysis. Explaining the reduction process in detail—not through general descriptions borrowed from methodology textbooks—seems to me to be critical in presenting persuasive research findings, particularly in the sort of qualitative approaches that have dominated the field of late. Part of this account ought to be a juxtaposition of representative data with a tabulated version of the full analysis as evidence that the data that provide the focus of the results do indeed faithfully distill the entire corpus.

Data Analysis

The process of data analysis, like those of collection and reduction, tends to get superficial treatment in the majority of manuscripts that I review. I will next outline a set of considerations that I think are important in both conducting and explaining the means by which a reduced data set gets rendered into a persuasive, evidentiary set of results. To do so I will include attention to my work with doctoral students because my collaborative approach becomes an issue in the sections that follow. A significant portion of my scholarship at this point in my career also serves as an important part of my teaching: I mostly collaborate on my studies with my doctoral students in research apprenticeships that involve them working with me one-on-one for a total of 4 hours each week over two semesters. In a typical year, I work with two students in such apprenticeships.

Taking this approach is good for all of us: I get assistance with my work, my students get experience and course credit for working with me, and we share authorial credit in presentations and publications. I also often credit the participating teacher as a coauthor because my research relies more on description than on intervention and so the teacher's construction of the curriculum and her or his teaching practices constitute a major part of the research design (Gallas, 2000; Smagorinsky, Augustine, & Gallas, 2006).

Coding Data

Coding as the manifestation of theory. I believe strongly in the value of coding data, both for the benefit of the analysis and for the purpose of explaining to readers my orientation to the research and understanding of the data. Not everyone agrees with me on this point; at a recent scholarly gathering, a full professor at a research university stated unequivocally that all coding of data is "positivistic" because it names data segments in ways that can take on the status of certainty. Perhaps coding can be bound by beliefs of its irrevocability, but surely not always.

Viewed differently, coding makes evident the theoretical approach used to analyze the data by applying code names to segments of text (typically, in my work, field notes, interview transcripts, and transcripts of people speaking as they work). In this conception, coding manifests what theory would say about data and makes the researcher's theoretical perspective on the data corpus explicit, without precluding other ways of looking at it. Also, by creating categories in sets and levels, a researcher manifests not only a theory but the principles within that theory and their relations to one another. In that sense, coding establishes the researcher's subjectivity in relation to the data and the framework through which data are interpreted. From this perspective, the codes are not static or hegemonic but rather serve to explicate the stance and interpretive approach that the researcher brings to the data.

To illustrate, my work assumes Wertsch's (1991) extension of Vygotsky (1987), which postulates that the appropriate unit of analysis for the study of the development of human consciousness is volitional, goal-directed, tool-mediated action in social context. Because I have interrogated this axiom and have accepted its explanatory power, I rely on its principles to provide the major categories that I typically employ: I look at the *tools* that mediate thinking, the *setting* in which those tools have gained currency and sanction, and the *goals* toward which people put them to use. These general categories, I have found, provide a framework through which to develop more finely tuned codes that specifically account for the thinking that takes place by the people who participate in my research.

Those codes vary depending, first, on the topic of the study—for my purposes over the past decade, studies of early-career teachers developing concepts about how to teach secondary-school English or elementary Language Arts, or studies of students constructing texts or talking about texts in ways that they find meaningful, using both writing and nonverbal media for their interpretive and representational work. The subcodes also vary depending on the specific problems that the participants are attempting to solve. One early-career teacher, for instance, might be struggling to find ways of teaching writing and come to rely on a formulaic approach such as the five-paragraph theme (Johnson, Smagorinsky, Thompson, & Fry, 2003), whereas another might be attempting a progressive pedagogy within a scripted curriculum (Smagorinsky, Gibson, Moore, Bickmore, & Cook, 2004). Or a group of students might be interpreting *Hamlet* through a pictorial medium (Smagorinsky & O'Donnell-Allen, 1998), whereas another student might be designing a home interior for a class in interior design (Smagorinsky, Zoss, & Reed, 2006) or writing essays in academic, personal, and hybrid genres (Smagorinsky, Augustine, & O'Donnell-Allen, 2007).

The transcripts from these studies suggest the ways in which these three major categories involve subcategories specific to the problem-solving activity engaged in by the research participants. Although there might be overlap across the studies, that overlap follows from commonalities in what emerges from the data rather than a priori categories that I superimpose on the transcripts. For the most part, the kinds of cultural tools employed by participants—either material or psychological—are a function of what they need to do to act on specific problems presented by their environments (Tulviste, 1991). And so while participants in a variety of studies might employ the tool of a narrative—to depict their emotions on masks representing their sense of identity (Zoss, Smagorinsky, & O'Donnell-Allen, 2007), to create pathways for negotiating the premises of a house (Smagorinsky, Cook, & Reed, 2005), to inform their interpretation of a poem (Smagorinsky, Cameron, & O'Donnell-Allen, 2007)—more unique codes come in relation to more specific tool use, such as the economical design of a horse ranch to allow for direct movement around the premises (Smagorinsky, Pettis, & Reed, 2004).

The development of these categories is not, from my theoretical perspective, a vehicle for producing a static representation of reality. Rather, it is to align my analysis with my motivating theory in ways that make my own subjectivity in relation to the data clear and unambiguous. Doing so would not preclude someone else from approaching the data in a different way for different purposes but rather delineates the ways in which my perspective contributes to my construction of the situation. The degree to which I do so persuasively and credibly becomes apparent when my work goes through the review process and my peers judge how effectively I have argued from the data.

Collaborative coding and reliability. Back when I wrote my dissertation (employing protocol analysis of writers before and after writing instruction to contrast the effects of different modes of instruction on writers' processes; see Smagorinsky, 1991), I was expected by my committee to demonstrate the reliability and validity of my investigative method. The standard toward which I worked was for me to code my data, then train a second rater in my coding system and have her or him code 15% of my data with an agreement level of at least 80%. Traditionally, such a result confirms the reliability of the codes in that two independent raters produce roughly the same results when putting the system into effect. Although this method has been questioned by poststructuralists such as Harding (1991) because of the tendency in the field to associate reliability with confirmed truth, it remains a standard measure in much social science research.

My current approach to research within the sociocultural tradition of Vygotsky (1987) has led me to accept neither the traditional notion that agreement equals reliability nor the poststructural view that agreement represents a chimera masquerading as truth. I employ a second coder, yet that coder, a doctoral student, works with me throughout the coding process as we labor through the data and discuss each data segment before agreeing on how to bracket and code it. In other words, we reach agreement on each code through collaborative discussion rather than independent corroboration.

Undoubtedly, there is an uneven relationship when I work with a graduate student because the data are from my collections and I'm more-or-less in charge and more experienced with how to do this sort of thing—that's why I'm the teacher and she or he is the student. You could probably throw gender issues into the mix and claim that my patriarchal approach reproduces the masculine hegemony that has traditionally produced inequities in research, given that almost all of my doctoral students have been women. How, some reviewers of my work have asked, can this work be collaborative when clearly the relationship is fundamentally and inevitably inequitable?

The answer has several facets. One advantage of working with good students is that we have complementary areas of expertise. My work with Cindy O'Donnell-Allen, for instance (e.g., Smagorinsky & O'Donnell-Allen, 1998), took place in her high school English class. Given my outsider status, she understood the setting of the students' work far better than I did. In accounting for the context of the research, then, she took a leading role in terms of knowledge about the school, community, faculty traditions, curriculum, students, and other factors that affected the ultimate shape of the data. This knowledge was valuable both when considering the context of the students' schoolwork and in understanding how they produced texts and interpretations in the setting of her classroom.

At times, the students' expertise is more formal. Michelle Zoss, for instance, studied with me specifically because we shared an interest in the role of art in English classes. Michelle herself is an accomplished artist with considerable academic training in the technical aspects of art, art theory, and educational theories regarding art education. In our studies of students' construction of pictorial texts in both an Interior Design class (Smagorinsky et al., 2006) and in Cindy's English class (Smagorinsky, Zoss, & O'Donnell-Allen, 2005; Zoss et al., 2007) and later in her own independent research (Augustine & Zoss, 2006; Zoss, 2007; Zoss & Jones, 2008), Michelle's considerably greater knowledge of the world of art was instrumental in arriving at the interpretive lens through which we viewed the data and, as a consequence, in the determination of which terminology we used in coding the data.

Collaborative coding thus provides a means through which levels of expertise may emerge through the process of discussion in relation to data. I don't think that one should automatically assume that there is a static, strictly hierarchical relationship between a professor and an accomplished doctoral student who brings in years of valuable teaching experience, is often involved in avocations that can inform research, and is reading and taking courses outside of the professor's areas of expertise. Presumably, the professor has greater experience with the conduct of research that benefits and accelerates, and inevitably provides direction for, the student's trajectory.

In contrast, when working independently, coders must work with a fixed coding system that the second rater either applies in accordance with the initial coding or not. And given that any coding session that falls below 80% agreement can simply be called "training" before a higher rate of agreement is reached, the reification of this percentage is somewhat disingenuous. I regard the flexible and generative nature of the collaborative approach as more likely to produce an insightful reading of the data because each decision is the result of a serious and thoughtful exchange about what to call each and every data segment. In my view, independent coding treats the system as a fixed entity, which denies its potential for negotiation as researchers work through data together.

The collaborative approach also provides abundant teaching opportunities. When discussing data or codes with a student, I often digress to explain how another research paradigm might consider the data (along with suggested readings); or we pause to conduct Internet searches to clarify a point or fact; or we consult a source from my bookshelves; or I provide my knowledge of the personal histories, relationships, lineages, and trajectories of people I know in the field; or the student shares with me relevant teaching experiences or ideas from coursework and related readings; or we educate ourselves in the countless other ways that are possible when two people work together on a problem that they both find interesting and challenging. Because I consider my research to be a critical part of my teaching, I view these sessions as central to the doctoral education of my students and an experience that pays off given the scaffolding it provides them as they move away into their own independent research.

Finally, working collaboratively with my students simply makes my work more enjoyable. It provides me with a smart, interesting, and motivated companion who can push me into new ways of thinking. I continue to get older while my doctoral students typically are about 30 years old, give or take a few years. I thus have an ongoing pipeline of vibrant, energetic, and contemporary thinkers and personalities to keep me from getting too stodgy or fixed in my ways as, much to my chagrin, I age. The vitality that they bring to my office, and the good company that they provide as we work, are invaluable assets that keep my work enjoyable, fulfilling, and energizing.

The Context of the Investigation

In the current era in which subjectivities, attention to culture, and other relational and contextual factors must be taken into account in order to situate research findings, it has become de rigueur for authors to include a section on the context of the investigation. In many manuscripts, the Context section is included in the Method section but I've begun breaking it out as a separate entity in spite of the *Publication Manual of the American Psychological Association*'s (2001) guideline to the contrary. I believe that while context and method are related, context merits its own attention, a belief that follows from my orientation to sociocultural theories of human development that stress the fundamentally social nature of human frameworks for developing concepts (e.g., Cole, 1996; Vygotsky, 1987; Wertsch, 1991).

Because the context of any study is infinitely complex, identifying the aspects of a setting that are relevant to the research can be difficult to sort out, and what matters may not be evident to a researcher who is not intimately familiar with all of the people and places involved in the research. Further, what is germane may be highly confidential and not amenable to being reported, such as the personal histories of research participants with sensitive backgrounds. For instance, in a set of studies I did with John Coppock in his classroom—situated in an alternative school for recovering substance abusers (Smagorinsky & Coppock, 1994, 1995a, 1995b)—some of our focal students were in the federal witness protection program because they had testified against their drug dealers, with their alternative school and recovery program experience tied to the terms of their agreement with prosecutors. Their experiences with drugs and alcohol were relevant to their interpretive work in the research yet could not be included in the publications, as specified as part of our own agreement with the school administrators.

One peril of working in the Vygotskian tradition is its emphasis on a cultural-historical approach. The problem is that there is a whole lot of culture, and a whole lot of history, for each person involved in social science research, and so paring it down to something manageable and relevant, without shortchanging what matters, is a tremendously vexing job. As a school-based researcher, I often provide skeletal statistics about a school's size and demographics and profiles of the primary participants in the research. Yet what I provide is inevitably inadequate in genuinely situating the study in its social context. A major culprit in this dilemma is the page limit provided by the typical academic journal, and yet even for those rare journals that allow free rein, reviewers often become impatient with lengthy accounts of context and urge reductions to allow for a quicker path to the results. Results, however, can only make sense when sufficiently contextualized, at least from the perspective I take on human development.

I wish that I had a tidy solution to this conundrum but I wrestle with it in every research report I write. Not only do I struggle during the initial preparation of the manuscript but I must negotiate the content of the context section with reviewers and editors of the various journals to which I submit my work. This give-and-take with editors produces context sections of different content and detail, even when different studies focus on the same setting and participants; the ultimate shape of the context section is a function of what I'm able to gather and include and what an editor believes to be relevant to the report, within the page limits for both individual articles and the overall annual page maximum that a journal's sponsor allows its editors to produce. And so the context section, like much else about the publication process, is never entirely the product of the author's decisions; rather, it is the result of a negotiated and at times collaborative process with editors and reviewers, all situated within the journal's historical mission and purview, the sponsoring organization's parameters for how much space is available for the journal as a whole, the economics of the publication business, and other factors that contribute to the ultimate shape and appearance of scholarship.

The Epicentric Role of the Method Section

One of the most prevalent problems I find in manuscripts that I review is a lack of alignment across the major sections of the paper (Theoretical Framework, Method, Context, Results, Discussion). That is, an author might invoke a framing theory but not explain how the method involves that theory, or might explain a method without referring back to it in the Results, or might pose research questions that are not addressed in the Results, or might explain results and provide a soapbox speech in the Discussion that does not follow from the analysis. Or all of the above.

Authors often go awry when they either pose no research questions, or pose different questions at different points in the manuscript, or pose questions that are not answerable through the data, or pose answerable questions but present results that appear unrelated to the questions. In my experience, studies work best when an author poses a limited set of answerable questions and then aligns the paper around them, making sure that they are theorized, that the method produces data that serve as evidence for claims, that the results are presented in clear relation to the questions, and that the discussion follows from the analysis.

My experiences as a reviewer and member of dissertation committees suggest that achieving this degree of alignment is extraordinarily challenging for most authors. I believe that one way to promote such alignment is to use the Method section as the epicenter of the paper, that is, the vehicle through which alignment can be if not assured, at least systematically attempted. Because this approach works reasonably well for me, I'll outline some measures I take to achieve the greatest degree of alignment I can in my initial draft of a manuscript.

The linear form of a final APA-style research report is deceptive. I would never recommend writing the Theory section first, Method second, Context third, Results fourth, and Discussion fifth. The process—or mine, at least—involves a lot more recursion in what gets written when (and inevitably, what gets written suggests that other areas need to get rewritten). I always write the Method section very early in the process because most of what I need to consider when writing it affects other parts of the paper.

In particular, the outline of the analytic approach—for me, usually the articulation of a coding system—sets the terms for what I need to talk about elsewhere in the manuscript. If my codes reflect a sociocultural orientation to the data, then I need to frame the study from this theoretical perspective, and the same goes for information-processing theorists, postcolonialists, phenomenologists, and everyone else. Ultimately, I need to ensure that if I claim this perspective, the language that I employ for naming my categories needs to be grounded in the terminology and constructs of the framing theory. For this reason, borrowed coding systems can be highly problematic because they were developed by someone else for, in all likelihood, other purposes and certainly for other data. Rather, codes need to be developed in a dialectic relation among the data, the theoretical framework, and whatever else a researcher brings to the analytic process. (See Bracewell & Breuleux, 1994, for a counterperspective on the value of universal coding systems.)

Just as significantly, codes need to work in clear relation to the presentation of the Results. The absence of such a relation is a feature of most papers I'm asked to review. The utility of this relation serves both the study's final form and persuasiveness and the process of producing it so that the major sections of the manuscript are aligned. In most of my research, I present a tabulated version of all codes. Constructing this table involves more than just taking all the codes, putting them in a table, and including the frequency with which each occurred. While this step is important, it is insufficient. From that point, some means of organizing the codes is necessary in order for them to make theoretical sense. As I've outlined, my major categories of goal, tool, and setting provide one convenient means of categorizing the codes initially. Still, I often find that some codes are redundant, some are so infrequent as to be irrelevant to the study's focus, some are irrelevant to the research questions, and so on.

As a result, further data reduction is in order, forcing my collaborator and me to continue discussing which codes remain relevant to the study's focus, which are not, which need collapsing into single categories, which need renaming, and making other decisions that contribute to a clearly focused, well-documented study. This process in turn suggests much about how to organize and present the results because of the focus it provides for the analysis. With the codes finally tabulated in a way that makes theoretical sense and provides a focus for the analysis, our effort to organize the Results section in relation to both the research questions and the coding system becomes much more logical to us. We can't say that reviewers always find our final decisions immaculate or publishable in the initial round of review but we at least have the basis for work that, in negotiation with our editors and reviewers, has greater potential for eventual publication.

Further, the Results need to invoke the codes so that it's clear how the analysis has produced the findings. When a researcher only provides a general description of coding (I read, I coded, I found themes), we readers never know how the results were produced. But even when a method is clearly explained, an author often proceeds to report results with no further mention of how the analysis produced them. Referring back to the analytic method while reporting results ought to be, I believe, a standard move in published research. Referencing the method while reporting results was a common practice during the era of the experimental study, when such alignment was assumed necessary. An author would say in the Method section that comparative results were produced through the application of analysis of variance, and in the Results, the author references those statistical tests when reporting specific findings. In one comparative study, for instance, Wu and Rubin (2000) report the following:

> The first set of contrasts on the textual variables is the most extreme comparison. It compares Taiwanese students' writing in Chinese with U.S. students' writing in English. To control for spurious errors due to the number of dependent variables, we first conducted a MANCOVA. The multivariate nationality/language effect was significant (Wilks' Lambda = .453, $F(8, 68) = 10.27$, $p < .001$, $\text{Eta}^2 = .55$). There was no significant covariate effect for measured collectivism (Wilks' Lambda = .956, $F(8, 68) = .39$, $p = .92$, $\text{Eta}^2 = .04$), nor any significant effect of topic (Wilks' lambda = .867, $F(8, 68) = 1.30$, $p = .26$, $\text{Eta}^2 = .13$). Nationality/language and topic did not interact (Wilks' Lambda = .899, $F(8, 68) = .96$, $p = .48$, $\text{Eta}^2 = .10$). (p. 164)

This sort of reporting illustrates the expectation for a statistically driven study to refer to the analytic method—the particular tests run to study specific variables—when results of that analysis are presented.

Such procedures have not yet become standard in the reporting of qualitative research and I strongly believe that the linkage between analytic method and results produced ought to be. Doing so requires referencing the coding system or whatever other kind of method is employed to reduce data to a manageable form when using data as evidence for claims.

Final Thoughts

I see the Method section as having an impact in at least two ways. First, for the writer it can serve as the point of origin for the ways in which the other sections of the manuscript find their thrust and organization. A research method requires a theoretical perspective, and so the content of the opening framework for an article is suggested at least in part by the tenets behind the investigative method. Explicitly stated research questions need to be answerable through the methods employed in the research. Results need to be specifically linked to method so that it is clear to readers how results have been rendered from data and how the theoretical apparatus that motivates the study is realized in the way that the data are analyzed and then organized for presentation.

In addition to providing the organizational principles for the author in constructing an evidence-based and warranted argument, a Method section is critical in readers' sense of trust in the claims of the study. As a reviewer I may find an opening theoretical gambit to be compelling, but if I can't reconstruct the author's means of collecting, reducing, and analyzing data, then I will have little faith that the construction of results follows from responsible and consistent treatment of evidence and will not likely recommend the paper for publication.

In 1990-1991, my first year as an assistant professor at the University of Oklahoma, *Written Communication*'s founding editor Steve Witte taught in the Department of English. During one of our many conversations, he told me that he'd never had an article rejected by a journal. I wish I could say the same. I hope that by outlining these considerations, I—along with the other contributors to this issue—can help others improve their prospects for publishing their research. We put way too much time and effort into this enterprise for it only to be read by dissertation committee members or other groups of friends and colleagues. For me at least, paying attention to these issues has given my research a much better chance of finding a larger public audience.

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