Chapter 19. Science Studies as Writing Studies

Teaching academic writing and uses of intertextuality made me wonder how disciplines were organized and how individuals came to engage in academic writing. I was not at first aware of the field of sociology of science, nor had I taken any sociology courses since my first year of college. The interpersonal approach of my therapy, however, had primed me to think about sociality. When at a faculty party I started describing the way I was thinking about disciplines and disciplinary writing to a colleague from the Sociology Department, he suggested I needed to read some sociology of science. He also suggested that I contact Robert Merton, the founder of the field, who was just a subway ride uptown at Columbia University. Merton generously invited me to participate in the graduate seminar he shared with Harriet Zuckerman, starting in 1978. I continued to participate weekly for at least four years. Through the seminar I got to know a number of the rising and established scholars of the field. I also began attending science studies conferences, most regularly the Society for the Social Studies of Science.

Inventing the Analysis of Scientific Texts

At first, I had little idea what studies of scientific writing might look like. While some rhetorical theorists had argued that science might be considered rhetorically, little empirical study of scientific writing had been published. I have a notebook stretching through two years full of ideas trying to think through how I should proceed, what problems I should focus on, what texts I might study, and what modes of analysis I might use. It also contains notes about my presentations at seminars, meetings with other scholars, and responses to provocative readings.

I began by doing what I was most familiar with from my literary training: looking at texts from historical anthologies of exemplary scientific writing from the ancient world through the twentieth century. I started by taking informal notes on selections that looked interesting, without much of an idea what I was looking for. After looking at what I had noted on my first attempts, I developed coding sheets to record parallel observations on each example (including basic information such as length, primary claim, empirical materials, number and use of citations, subheadings, and organization) as well as more interpretive evaluations (such as source of problem addressed, representation of author and audience, representation of methods, and main argumentative strategy). I thought I was distancing myself from literary criticism and its assumptions because I was noticing different kinds of details than were most important in literary texts, yet I

was aware that I was still using my skills in noticing textual details, organization, themes, and stances. I eventually also incorporated analysis of genre, class, social and historical location, and social relations which I had developed in my dissertation. I also drew on what I had been learning in my teaching about how writers were referring to and discussing texts in their fields. Many of the questions and plans in that notebook I now see as continuing through the following decades, including the role of data and data production, the relationship of internal thought and expressed thought, the role of changing textual form and its relationship to structure of arguments, and the formation and communication of knowledge collectives, and the role and evolution of disciplinary literatures.

Since I would not know what a persuasive analysis of scientific texts would look like until I did some, I moved from note taking to writing a full paper and seeing what issues arose within it. I was anxious and uncertain while writing my first analytical presentation of scientific texts for Merton and Zuckerman's seminar because I had to solve many for-me novel problems. In order to highlight disciplinary contrasts, I picked examples from natural sciences, social sciences, and humanities. Since I had no principled way to identify typicality of writing in those fields, I chose highly cited articles for analysis—under the assumption that recognition indicated that the articles met the values and needs of their fields to become certifiable contributions, even though they inherently were not typical in their distinctive success. In order to organize the comparison, I initially drew on the traditional communication triangle that had many incarnations from Aristotle through James Kinneavy, all of which had some variation of the author, audience, and world/subject at the vertices. In my early drafts, however, I struggled with how to organize the discussion of the prior disciplinary literature, which was everywhere, but not well represented in the triangle. Each of the three vertices relied on the literature—as markers of authority and ethos, as shared knowledge of the field, and as establishing relations with the audience. As well the literature established criteria of judgment, methods of inquiry, and markers of argument legitimacy. When I presented an early version of the paper to Merton's seminar, he reasonably pointed out that I seemed to want to make the literature an additional vertex—which would of course allow interaction with the other vertices. When I turned the communication triangle into a triangular prism (at the time I inaccurately called it a pyramid), the problem dissolved, and I wound up using this triangular prism explicitly (and sometimes implicitly) as an analytic heuristic in my work for many years. This helped me see how fundamental the literature was in academic writing. When the term intertextuality came along a few years later, I began using it, though I had a few qualms about the baggage it brought. The term *intertextuality*, as used in literary studies, kept a distance from the other elements, whereas the triangular prism formulation highlighted how interactive the literature was with authorial identities and representation; audience knowledge, expectations, and criteria; and stance towards the phenomena in the world being discussed (see Bazerman, 2004c for a fuller discussion of the different uses of the term).

Figure 19.1. Robert King Merton

When I showed an early version of this presentation to Karin Knorr, an eth-nographer of science, she pointed out to me that I was relying on a number of stereotyped, folk-belief assumptions about disciplines and their relation to each other. This was an important lesson, pushing me to greater precision in char-acterizations and questioning common sense ideas about social phenomena. I became more cautious and specific in my claims, and more careful about theo-retical generalizations. I read more deeply into sociological theory and findings. As I started to draw on other disciplines within science studies, I tried also to re-spect the knowledge and perspective of each of the fields I was drawing on, at the same time as I pursued my own questions derived from writing studies. While I remained confident in my textual analysis of each of the three articles, in the pub-lished version (Bazerman, 1981b) I backed away from any claim of characteristic differences among fields. Rather I characterized the analyses of these articles only as individual spots in a complex landscape we did not yet have a more extensive and detailed understanding of.

This cautiousness also led me to be more intentional and careful in defining corpora and the generalizations to be drawn from them. I remember a comment from one of the other participants in Merton's seminar when asked about the limitations of his sample and analytic methods—he acknowledged the limitations and uncertainties of the empirical materials but then commented that his claims were modest and did not ask a lot from the data. I took this to mean there was not an absoluteness about what methods to use, but rather one should calibrate one's materials, methods, and claims to be adequate for each other. The amount of detail and precision available would affect the nature of what one could claim, and, inversely, the nature of the claim could determine the necessary evidence and precision needed to warrant it.

In Merton's seminar, as typical in sociology, research methods were examined and critiqued. Further, the role of replicable methods in the production of science, the limits of replicability, and the purpose of representation of methods in research articles were explicit topics within science studies. Accordingly, I became more aware of the importance of making methods explicit in a way that was not part of my training in literary studies nor was common within studies of rhetoric and composition. As I began publishing studies of scientific writing, I became as painstakingly explicit in laying out my methods as I could be, even when it involved creative, critical, or interpretive methods. From that time forward I made it a point of specifying methods, in the design of the study, identification of research site, formation of a corpus or other data collection, data representation, and mode of analysis. I also have worked where possible, to make the initial representation of the data distinct from its further analysis; in literary and rhetorical studies often these two steps are conflated and the audiences see the empirical material only through the critic's interpretive vision and critical conclusions.

One particular methodological principle I learned in Merton's seminar and have attempted to employ in my consequent studies has been the idea of the strategic research site. That is, particular research sites display the phenomenon of interest with unusual empirical prominence or clarity; in less favorable research sites, the phenomenon might also be at play, but may be entangled with harder-to-parse variables or may appear with smaller and less visible effects (Merton, 1987). For my first study, the choice of prominent articles from three different fields was strategic for displaying text differences and how those representations are presented for evaluation. It was not strategic for looking at typicality or systematic differences among the fields, which would require larger corpora with careful criteria of inclusion. Nor was it strategic for examining the processes of writing or the actual modes of investigation, which would require ethnographic examination of research and writing practices. I have also come to advocate an inverse of this idea of strategic research site: if I have access to certain materials and data, so what are they good for, what can they show me? At some point I transformed this realization into a question I repeat to students, that every bit of empirical data is evidence of something, but the question is "evidence of what?"

This kind of principle has guided me both to look for places to investigate specific research questions and to identify the potential use of materials or research opportunities that might fortuitously become available to me. This principle has helped me navigate between intention and luck, allowing me to build a coherent view of related concepts grounded in investigations while being flexible in recognizing and responding to opportunities (see also Bazerman, 2008d for a further discussion of method.)

When I shared a draft of this early paper with colleagues from literary and composition studies, they found my formulations complex, unfamiliar, and hard to understand. I first made typical excuses that the concepts were hard to understand because they stood at the intersection of disciplines and theories. But one reader commented that the conceptual complexity increased my responsibility as a writer to make the text as understandable as possible, explaining clearly the concepts from different theories and disciplines along with their relationship. Since I had been advocating this principle for a number of years, I was quickly shocked back to my better revision angels. I became more mindful of how to lead the readers into the ideas without dumping them suddenly into unclearly defined concepts and relationships. While I have been able to find some recognizable, concrete, and familiar ways to connect readers with my ideas, I have not, however, eliminated the strangeness or novelty of the ideas that made them hard to process. Insofar as contingencies, subordinations, relations and other complexities needed to be represented, I have tried to present them sequentially across paragraphs, well-marked by textual signposts. The more I did this, the more readers could follow me on the journey, seeing more of what I was trying to show, and the longer they would tolerate the length and difficulty of the journey. Also, individuals could select the parts they were interested in and prepared to accept and did not have to take the entire intertwined package. Over the years, however, as my conceptual world has become more elaborated, this problem may even have become worse. I, however, have accepted my responsibility as a writer to make the work as accessible as I can.

After all this feedback and the mandates for revision, and after multiple presentations, including at the meeting of the Society of the Social Studies of Science, in September 1979, the article was published as "What Written Knowledge Does: Three Examples of Academic Discourse" in the journal *Philosophy of the Social Sciences* in 1981. While over the years I have been gratified by the reception this article has received, I continue to wonder why this first attempt to sketch out an approach which I have been elaborating and refining for the next forty years still remains more highly cited than some more recent papers which I think are more sophisticated in method and theory, as well as conceptually clearer and easier to read. But I'll take it. This reminds me that readers have their own reasons and interests which determine how they will take up work. The best I can do is offer multiple entry points which can then lead readers more fully into what I view as a consistent research and theory program.

A Next Step into Interdisciplinarity

As I became more familiar with science studies, I felt the need to make sense of the implications of the field for scientific writing. This impulse fortuitously coincided with an invitation to contribute to a volume on scientific and technical writing. I proposed writing a review of literature of social studies of science from the perspective of writing: "Scientific Writing as a Social Activity" (Bazerman, 1983a). This essay synthesized the different perspectives I had become familiar with, both in how they saw the function of writing within social systems of science and more specifically about what they pointed to in the structure, goals, and processes of scientific writing. This synthesis required developing a critical perspective on work from other fields that made claims or assumptions about writing, particularly if the fields were not well immersed in language and writing studies.

In thinking through how I would respond to the invitation, I was starting to learn how to match my research and intellectual interests to the publication opportunities that came my way. Of course, this is something most publishing researchers learn to do, improving throughout one's career. For me, finding that match meant having a sense of the next steps in my own research along with a sense of the audiences, venues, and genres that might be appropriate contexts for those pieces of work. Sometimes when opportunities came along the parts would spontaneously click and I would realize, "oh, this would be exactly the place to move me forward on this part of the project." But sometimes it might take longer to think through what would touch the interests of these editors and their audiences, what kinds of evidences and theories and arguments might be powerful for them, and then how that argument might advance what I was trying to understand. Sometimes the pieces matched almost exactly and I could just propose something I was already working on. Sometimes the opportunity would suggest to me something I had not yet considered, but I would see as a valuable next step. At other times I would see a task that might take me temporarily away from my main line of work, but which would carry me forward in a plausible way that might ultimately be useful. At other times, however, anything I came up with for the opportunity would be too much of a digression from where I wanted to go; then I would give the opportunity a pass—especially since I was by then tenured and no longer felt compelled to take every publication opportunity that would have a quick payoff. In this case, I had not previously thought of doing such a review, but I soon realized how such a review would help me (and my colleagues in writing studies) make better sense of the sociology of science literature and think through the implications for writing. I even began thinking about the review article as the equivalent of writing a graduate comprehensive field exam in sociology of science, though my own doctoral requirements were long past.

Such a review was different than other syntheses of sociology of science as I was applying the work to a different field. Writing this review in fact helped me get my priorities in focus. As I became more engaged with other fields, I

was finding it hard to stay focused on what this material showed me about writing and how it could contribute to writing studies. It was too easy to get caught up in the intriguing problems and research agendas in those other fields. I have recurrently had to pull back to remember what was of importance to writing and the teaching of writing. Ultimately, I was only a tourist or amateur in those other fields. Trained historians, philosophers, and sociologists would be better equipped and more appropriately focused to address historical, philosophical, or sociological questions. That was their work. But I could make sense of and use their products, and what they found might be of interest to colleagues in my field. Even when I felt that as a writing researcher I could contribute to sociological, philosophical, or historical issues, I found there were limits to how much I could engage with people in those fields and persuade them as my perspective, modes of analysis, and evidence were so different from what had become persuasive in their fields. At most, some of the empirical things I and other writing researchers found might gradually become visible in the other fields and gradually work into their view of their fields, but they would have to pursue the inquiries and findings in their own terms. So even as I continued to learn from those fields and translate their work into my own field, I found that there were limits as to how much I could bring my perspectives into the discussions of those other fields.

A Further Step into the Problem of Facticity

In early 1980, writing a review of Latour and Woolgar's recently published *Laboratory Life* helped me sort out further what criteria successful analysis of scientific writing should meet (Bazerman, 1980a). This book was groundbreaking in many ways and would have a large impact. I appreciated many of its perspectives and insights, and found its detailed ethnographic observations around writing production important contributions. But I also found that the authors were not always knowledgeable about the complexity of language or writing processes. The book made a number of questionable assumptions, inappropriate analyses, conceptual conflations, leaps of reasoning, and unwarranted conclusions, which I noted in the review. In particular, the book did not seem careful enough in thinking through the differences of spoken and written language. So while I shared much of the enthusiasm most readers had for the book in the way it showed the centrality of inscription and text production in science, I was one of the few reviewers that questioned some of the book's more radical conclusions based on what I viewed as flawed reasoning and analysis of language. From the point of view of my own development as a writer, this exercise helped me think through the kinds of precision in analytic method and reasoning I would need to make and the standards of argument I would need to meet in order to produce work that I would consider warranted. This review also made me cautious about producing work too colored by a predetermined philosophic stance that might distort selection and analysis of evidence and frames of interpretation.

Substantively, the review focused me on the nature and quality of fact production and fact accounts. Latour and Woolgar had talked about this process as one of forgetting, as data became inscribed and materiality was left behind. As phenomena are reduced to inscriptions, whether graphic, numeric, or verbal, much is lost of the vitality, materiality, complexity, and multidimensionality of the phenomena. Nonetheless, the inscribed aspects of the phenomena are carried forward and thus remembered, although this was not considered by Latour and Woolgar. The inscription choices are intentional and purposeful, focusing attention on the phenomena the researchers are interested in. The question then becomes as much about what is remembered as what is forgotten, why certain things are chosen to be remembered, how that remembering happens, and whether that selection does violence to the phenomena and research questions. These were issues that stayed with me and would be behind a number of my later inquiries (see Chapters 26 & 27).

A number of additional themes introduced in Merton's sociology seminar have continued in my writing. Many of them I have recognized and discussed in some of my later theoretical pieces, and many are implicit in my growth of sociological imagination[8]—that is, seeing phenomena as part of social processes rather than only individual choice. One major theme that stayed with me was citation and codification, that is, how later work evaluates, selects, builds upon, and organizes earlier work. Citation studies were prominent in Merton's world, elaborated by Eugene Garfield and early citation indexing (currently called the Web of Science). Some of Merton's students worked within Garfield's Institute for Scientific Information (ISI) and the seminar had a number of presentations from Garfield and his research team. This work fed my growing interest in the use of sources in academic writing, and how scientific literatures formed emergent networks of knowledge. The work of the ISI helped me think about each individual article being part of a dynamic quasi-stable communal process of knowledge production.

Increased awareness of the emergent and changing nature of disciplinary literatures also focused the purpose of my own writing and publications: to propose persuasive findings and ideas that would carry forward discussion, reorient future work, or enable others to address evolving problems. While persistence of my own claims over time might be affirming, moving the literature as a whole forward became more important to me. This at times might mean, for example, opening up new areas or approaches with claims or ideas that would be rapidly superseded but which nonetheless could suggest directions others might take.

During my time in this seminar the English translation of Ludwik Fleck's book from the early 1930s, *Genesis and Development of a Scientific Fact* (1979) appeared. Merton was one of the editors and we devoted several sessions to this work. This book introduced the idea of thought styles (which in my reading of the book appeared to be representational styles) within thought collectives. Fleck's approach

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8.   The term was introduced by C. Wright Mills, 1959.

connected individual cognition, sociocultural organization, activity, and representational form realized in texts. Further, Fleck argued that culturally shaped activity was passively constrained by material conditions. He added that scientific cultures actively maximized the passive constraints of material experience on human representations of knowledge. This way of thinking gave me a means to formulate epistemological implications of writing processes by connecting research methods, representational methods, and analytic methods in writing in different disciplines; namely, how methods actively worked to constrain claims though the passive demands of evidence of the world. This also led me to think more about the production and use of evidence in my own work and the work of my students, and how those practices intersected with the culture of the field of writing studies. I came to see epistemology as a practical matter, realized in all the practices leading to the ultimate piece of writing. Epistemology, consequently, could be a site for empirical research and not just an abstract philosophic matter.

Fleck's formulations also helped me resolve the tensions between the relativist and empiricist accounts of science and scientific texts. I found much to value in the relativist analyses of scientific texts as human constructions (after all, language and writing are produced by humans, using the human inventions of textual representation, to communicate with other humans). Nonetheless, I still saw value in the empiricist account that scientific representations helped us understand and live more successfully within our material worlds. Fleck's formulations along with Vygotsky's accounts of how we develop our thinking at the intersection of our spontaneous experiences, our disciplined learning, and our interested engagement in the material world helped me formulate positions in the debates during this period, known as the science wars. While my formulations did not get much hearing from people entrenched in the alternate positions, I believe they do quietly persist and define credible research programs, even as the more absolutist positions on either side have softened and lost their vitriol. My struggles with developing and articulating my position, nonetheless, did give me the framework from which to develop my theoretical position in Chapter 11 of *Shaping Written Knowledge*.

Learning to find a middle path in those divisive times further built my skills of synthesizing and integrating radically different perspectives—respecting all sides, avoiding landmines, and recasting or sublating opposing views into a more comprehensive framework. This of course is a standard dialectical process, enacted repeatedly within Vygotsky's writings, which provided me models of how to do it successfully. I have used this discursive strategy not only in moving past the empiricism and relativism divide, as I have tried in the years to integrate psychological, sociological, historical, rhetorical, linguistic, textual, curricular and practical production approaches to writing. This synthetic orientation also guided me as an editor and leader of collaborative enterprises as I tried to bring out the nature and value of each contributor's line of work at the same time as seeing its connections, boundaries, and consequences for different lines of work. In later years, for

example, this helped guide me in creating reference books like the handbook and the reference guides that attempted some kind of order, coherence, and intersection among the different contributions. Even more it was evident in the kinds of syntheses I fostered within collaborations on the lifespan development of writing.

An Introduction to the Rhetorical Tradition, Carnegie Mellon Version

After the first year in Merton's seminar, in the summer of 1979, I also participated in an NEH summer seminar at Carnegie Mellon University led by Richard Young. This was my first systematic introduction to the classical rhetorical tradition and its more contemporary elaborations. At CUNY we had focused on basic writing and open admissions, with a strong emphasis on understanding students and their thinking, but at CMU the focus was on the nature of argument and the early process work, which was framed within the classical canon of invention. This seminar helped me be more attentive to the rhetorical tradition which was looming large in writing studies, and I would draw on it where I found appropriate. But I also found classical rhetoric limited in scope, with many of the concepts and problems closely tied to a limited set of genres within institutional and activity settings derived from classical models (see Bazerman, 1993c). A few of the concepts I was able to apply more broadly, such as *kairos*[9], but I found that not all writing was best understood as argument nor were all the phenomena of interest to me capturable within rhetorical theory as then constituted. Drawing on an increasingly broad set of interdisciplinary concepts and approaches, I have had an uneasy relationship to rhetoric which some see as foundational for writing studies. Only as I was eventually able to redefine rhetorical theory to serve as an umbrella for my more interdisciplinary approach could I cast my work as rhetorical. I discuss this more extensively and explicitly in Chapter 24 and in my two books on *Literate Action* (Bazerman 2013c, 2013d).

My major paper for Richard Young's seminar examined the reflective narratives in the collection *Sociologists at Work* (Hammond, 1964) to understand how research processes were tied to text production, from the early stages of developing background knowledge and theories (largely through engagement with the literature) and problem definition, through research design and data collection, then analysis and writing up. That is, I characterized the entire research process as leading toward research publications. To parse the processes in the narratives I used the TOTE (Test-Operate-Test-Exit) model proposed in Miller, Galanter, and Pribram's 1960 book, *Plans and the Structure of Behavior*, which was one of the core texts in the CMU cognitive world. In my paper I analyzed how the sociological researchers framed their tasks and problems at each stage and by which

9.   But I also saw this as more complex and needing reinterpretation, see "Whose moment?" (appearing as a chapter in Bazerman, 1994b)

criteria they felt they had resolved that level of work and proceeded to the next. This rough seminar paper was heavily weighted down with detailed presentation of the accounts in the volume. The analysis and conclusions were only partially cooked, and I never got to present the paper at the seminar nor received feedback from the other seminar participants or by the seminar leader.

I did, however, present this work at Merton's seminar at Columbia, and got engaged feedback from participants who wondered about the normativity and limitations of the accounts I was using as data. This led me to greater cautiousness in interpreting and evaluating the accounts I might use as data and pushed me towards more intensive studies of distinct cases. As well, several people (including seminar co-leader Harriet Zuckerman) wondered about the assumptions I was making about how directed the research work was towards publication, which led me to greater caution about the teleological directiveness of the research process. Yet the project did get me thinking more about how the earlier parts of the research process were consequential for the publications that would emerge. This extended orientation to text composition or formulation stayed with me and worked its way into a number of my studies afterwards, from my critique of Latour and Woolgar's *Laboratory Life*, to my examination of how Compton came to design his study and record his data, to how Newton characterized his investigative processes in different kinds of documents, and ultimately to how students learned to collect and inscribe data in their projects (see Chapters 20, 21, 27, and 28, among others).

Merton, on his part, while wondering about the value of considering the entire research process from the perspective of text production, did see the importance of the published archive and asked me to look into Karl Popper's World Three, which is the material productions of human knowledge, such as books (Popper, 1972). Popper's consideration of knowledge as a production helped give some shape and robustness to my conceptualization of disciplinary literature and its operations in the material world.

Given the issues raised about this project which made the conclusions I drew questionable, I never revised the paper to submissible form. I was never able to find a good strategy to revise the material into something I could stand behind with confidence. Yet the project still marked a major moment in learning how I could investigate and understand writing more deeply and what criteria my writing would have to address to be credible.

Another Inflection Point

This period posed many new problems and explored nascent solutions that would set a trajectory for my further writing development. I was no longer using writing to figure out what it was I wanted to do; I was figuring out how to do it. I had found my mission in life in the teaching of writing and had identified basic pedagogic imperatives and strategies. Now I was forming approaches to research

and ultimately theorizing—figuring out research questions and empirical methods to pursue them, locating interdisciplinary resources while learning to stay true to my motives and intellectual interests. I recognized that writing studies would benefit from a sociologically oriented research program to supplement the on-going cognitive psychological research program of process studies. I also started to gain the sociological and historical tools to understand how I could support the substantive research along with institutional presence and legitimacy of writing studies (see Chapters 26 and 27, among others).

This inflection point also changed the intertextual field I had been working within. The explicit scholarly literature on writing that I had attempted to contribute to was in a small, marginal corner of the humanities and consisted of only a few books and one journal, *College Composition and Communication*, that regularly published research along with the practice-based articles that dominated the few other journals and newsletters. The journal *Written Communication* did not appear until 1984. As I had discussed earlier, textbooks, by far, were the largest vehicle for sharing ideas about composition, and they were not seen as part of the scholarly literature. So keeping up with the literature and citing sources was not hard—the problem was paucity and not overabundance of resources. But now I was drawing on multiple well-established social science disciplines and specialties each with their journals, books, and intellectual traditions. I needed to learn to select from complex literatures, identify their relevance for my emergent project, and explain their relevance across disciplinary divides. While my own research and pedagogy were helping me to recognize and analyze the role of intertextuality in different disciplines, I had to deal with complex interdisciplinary intertextuality as a practical matter. Having developed ways to work with complex interdisciplinary intertextuality, I no longer felt bounded by the limits of the literature of writing studies, even as the number of journals, publishers, and books in the field increased. As I continued to see how writing was imbricated with so many other aspects of life, I seemed to have no choice but to keep trying to expand the intertextual spaces I found relevant to writing.

Although I was using the literary tools of my prior education, I felt I was inventing new ways of proceeding, to produce work of both practical value and academic credibility, situated in a new interdisciplinary space. This sense of trying to invent new ways of studying writing was to continue in the following years, as my research program in scientific genres started to gain momentum. The train wheels were starting to roll.