# Chapter 20. Studying the Changing Genres of Science and Figuring Out How to Write about It

After my first few attempts at studying scientific writing, I started to get a sense of what kinds of projects I might pursue. One study seemed to lead to the next, along with new writing challenges for each. In the course of addressing these challenges I was inventing the kinds of articles I was writing.

During this next stage in my writing development the leading writing problems I addressed concerned contributing to the advancement of the disciplines I was engaged in. I thought about forming research questions, identifying relevant data and resources, representing and analyzing data, and forming arguments within knowledge fields. Some of the earlier leading problems in writing development were no longer in the front of my concerns: developing sentence and argument structure skills; sorting through troubled emotions; clarifying values, commitments, and affiliations; extending my imagination and deepening my reflective contemplation; and even being communicatively accessible to different audiences. I continued to work on these earlier learned components of writing, but mostly in the context of larger questions which presented bigger problems to solve.

Accordingly, my narrative from this point forward will focus less on writing development at the text manipulation, personal expressive, or audience communicative levels, and will focus on issues such as social organization of activity fields, knowledge organization, inquiry methods, organizational structures, and strategies of texts as interventions within communal knowledge-making. My analyses of my writing choices and writing learning from this point forward will focus more on underlying decisions leading to the positioning and construction of texts than on the final textual forms in which I present my findings and ideas. The fundamental questions directing the project would motivate and guide the work of bringing the text into the world.

The narrative from this point forward may also seem less attached to life issues and emotions and more attached to abstract questions of knowledge investigation and formulation. Yet my commitment to advancing knowledge and practices of writing and writing education had by this point become deeply personal, defining my sense of value and accomplishment. This work was carried out to advance a professional field, rather than resolve personal questions of identity and commitment. My own research and the professional discussions it contributed to also changed my understanding of writing and thus the kinds of questions, observations, and explanations I had about my own writing, making my professional contributions personally meaningful. I hope the narrative to follow may reverberate with the experiences of other writers who find personal value in contributing to disciplines, professions, or other specialized organized social activities. Each will have a different set of experiences and pathways, but each will also find the meaning and value of their writing development within the practices, interactions, and development of their fields.

## A Process Study of Practical Scientific Reasoning

My incomplete study of *Sociologists at Work* convinced me that disciplinary writing processes varied and were tied to inquiry processes. My literary training suggested that drafts leading to published work could reveal the origins and evolution of ideas, along with the explicit concerns of the writer. As I reviewed literary studies for robust examples of how I might proceed, I unfortunately found most draft studies limited to noting the technical details of dating different drafts or focusing on specific themes, rather than trying to understand more comprehensively the emergence of texts. So methodologically, I seemed on my own.

At that time, I was living in New York City where a number of scientific societies were located. When I asked the archivist at the American Institute of Physics about possible files of drafts leading towards major articles, he pointed me toward a microfilm copy of Arthur Holly Compton's papers (the originals were at Washington University in St. Louis where Compton had worked). Within this collection the most complete set of notebooks and papers came from a less-known paper that followed up on his well-known empirical confirmation of quantum theory (through what is now known as the Compton Effect). I had initially naively thought that I would find the drafts of the major 1923 paper which could then be interpretable as a self-contained case. Although any historian of science would know that major discovery papers often appeared within a series of much less well-known papers, I was surprised to learn that texts were part of disciplinary discussions and did not stand alone. Sometimes it is not even clear at first which paper in a sequence would be later identified as the most significant.

Consequently, in order to analyze the lesser-known paper that had the most notebook and draft materials, I needed to place it within a larger history of Compton's research program, as adding a new kind of evidence within an ongoing discussion, using the recently invented bubble chamber. This device indirectly showed the presence and trajectories of particles through photographs of condensation tracks left by particle movements. This method created for Compton a series of problems in selecting, representing, evaluating, and analyzing the data. Compton's notebooks in particular revealed the principles of selection of which photographic plates to use, based on the clarity and distinctness of tracks. All this was prior to his actually producing the article draft. Then within his draft and revision he was attentive to how he represented the world of nature outside the text and how he characterized those representations. In the draft and revisions he controlled the representation of nature by postponing topics in order to insert preliminary information about the equipment that produced the data or about other logically prior data. He extended discussions to make the nature of the data clearer; he fine-tuned the precision of the language; and he controlled the level of specificity and precision appropriate to the argument being made. He repeatedly adjusted the epistemic level of the discussion (whether, for example, he is discussing photographic data of the tracks, the calculated energies and trajectories of particles, or hypothesized theoretical characterizations of the particles and their interactions). He also adjusted the representation of his actions and judgments, and his relationship to the audience. Through his drafting and revision practices, including the timing and use of the abstract, we can see him bringing the inscribed object into the social world of science while being careful to identify exactly how and in what form experience of the material world is brought into the discussion.

I organized this story as one of temporally unfolding constraints from his professional standards, the prior discussions in the literature, his material actions in the laboratory, the recorded data artifacts, and the persuasive expectations of his readers. At the same time these constraints provide him the opportunity to make warrantable arguments through credible empirical evidence to intervene in disciplinary discussions. I argued his actions showed how disciplinary contributions could be produced with adequately reliable representations of nature—or as the original title indicated: "The Writing of Scientific Non-Fiction: Contexts, Choices, and Constraints" (Bazerman, 1984b).[10]

When setting out to do this draft process study, I started out simply to inquire into the processes of a scientific writer, although I was aware of the epistemological problem of representation, as discussed in the previous chapter. Only as I engaged with the materials, however, did it become clear to me that Compton's notebooks and drafts provided evidence of him being attentive in a practical way to the difficulties of precise representation and how empirical experience should be brought into knowledge discussions. He was not an epistemologist, a philosopher, or a science studies scholar, but as a practical experimental scientist he showed how epistemological problems are managed and contingently resolved within the empirical and argumentative practices of his field.

Because I was examining the emergence of a particular paper resulting from the constraints and choices influencing his process, I included in this essay the published version of Compton's article, as I had for the three papers examined in "What Written Knowledge Does" (Bazerman, 1981b). I wanted to make the central data available to an audience that may not have been familiar with them. This grew out of my undergraduate new critical practice of reproducing short poems

10. Later, when I was to incorporate this article as a chapter in *Shaping Written Knowledge,* I was to change the title somewhat and elaborate the discussion to focus on the process of making reference: "Making Reference: Empirical Contexts, Choices, and Constraints in the Literary Creation of the Compton Effect."

that were then examined intently or quoting extensively from them. In my next studies, however, I was to examine larger corpora, so I needed to find new ways of making the larger collections of data available to the readers so they could then evaluate the validity of my analysis and argument.

The Compton study had heightened my awareness of how located each scientific text was in an historical process, both in its general practices of scientific writing and the particular discussion it was part of. This awareness led me to the next question of how scientific articles changed over time. Fleck's concepts of thought collectives and thought styles further reinforced my historical interest—particularly as Fleck's treatment of thought style was tied to changing representational styles which formed shared ways of seeing and characterizing phenomena, mediating between individual and collective thought. Finally, working in the library and archives of the American Institute of Physics I met regularly with its director, the historian Spenser Weart, who helped me think about how science and scientific communication developed historically.

## Historical Studies of Corpora

The AIP library contained a full collection of the society journal *Physical Review* since its founding in 1893, as it grew from a minor regional journal into the world's leading venue for publication in physics. Over this period the journal had proliferated in its editions, frequency, and page count, to well over 150 times its original annual length by 1980. An examination of the journal could provide a window into how scientific writing had changed since the late nineteenth century. However, studying such a corpus created many problems. Because it was so massive, I first needed to identify a sample based on a coherent set of principles and procedures to make any detailed analysis possible. Then, over the period so much had changed in physics—its theories, experimental techniques and devices, even the phenomena examined, many of which were not even imaginable in 1893. Further, the size, organization, and specialization of American physics had changed radically from when it was a marginal backwater of a field then centrally located in Germany. Consequently, I couldn't attribute changes in style, organization or other features of texts simply to changes in ways of writing. In order to minimize confounding factors, I sought a subspecialty that was stable in its work, methods, and arguments. Optical spectroscopy appeared to be among the most stable because its core task remained analyzing the differing wavelengths of light from a source using a prism to identify the composition of the source. I eliminated, however, electron spectroscopy and spectroscopic examination of nuclear events, which opened new directions to the field. Even within optical spectroscopy, theory changed radically. It turned out, in fact, the changes in the writing were most driven by the centrality of newly emergent theories to frame studies and analyze results.

Even this subspecialty, however, offered too many texts to examine them all in detail, so I needed to establish further sampling principles and procedures. This was prior to digitization of archives, so even quantitative work had to proceed by hand counting. Because each of the forms of analysis I eventually carried out required a different level of work, I wound up creating three different selections from the full corpus. The recognition that I needed multiple levels of analysis and multiple selections for those different analyses was itself something I had to work out as I proceeded through the project. For the gross quantitative analysis of article length, I selected all the articles from 1893-1900, then all the articles from every fifth year through 1950, then only the first few issues from each fifth year through 1980. This increasing selectivity reflected the growing size of the journal and number of articles. For the second level of mixed qualitative and quantitative analysis of number of references, graphic features, organization and mode of argument, I focused only on optical spectroscopic articles appearing in 1893 (the first year), 1900, and every ten years after (if less than three appeared in the selected year, I included the next year; if more than six appeared in any year, I only used the first six in the earliest issues of the year)—for a total of 40 articles examined. Then, for detailed examination of syntax and vocabulary, I looked at all the optical spectroscopic selections from 1893-1895, 1920, 1950, and 1980. The time intervals and size of samples of the second and third corpora were influenced by qualitative judgments I made as I looked through the larger corpus based on the trajectory and rapidity of changes that appeared to be emerging. I wanted to be granular enough to capture change.

The quantitative results at all of the levels showed that there were noteworthy changes in length of articles, numbers of references, and sentence syntax, but the meaning of these quantitative results were baffling. Only qualitative analysis could make sense of these and other changes over time. From this I learned that quantitative measures could sometimes locate where there were phenomena worth investigating, but the meaning of the changes could only be teased out by qualitative analysis, which might also reveal the interaction of the several features counted. The article length, for example, had to do with the changing nature of the arguments as well as with the data compaction of theory, which was also revealed in the changing character of the graphic elements and the citation lists. The abstraction and aggregation of the theoretical orientation were counterbalanced by the complexity of theory needing explanation and application to the case. The increasing role of theory in structuring the argument also influenced the different sections and the appearance of unique section headings to highlight the reasoning path, even as generic headings for methods and findings increased (Bazerman, 1984a).

In the course of this study, I found that my understanding of physics was too limited to fully understand some of the more recent, theoretically organized articles, so I hired a graduate student in physics as a specialist informant to walk me through the articles to unpack the ideas and the rhetorical strategies. Since then, for briefer periods, I have often relied on unpaid specialist colleagues of various sorts to guide me through historical scholarship, intellectual property law, liability litigation, information science, and many other areas that my work touched on. I also found it useful for understanding disciplinary cultures and knowledge to present myself as uninformed and asking for guidance.

The study of *Physical Review* showed me that I could turn runs of journals sitting on library shelves into corpora for the study of historical changes in writing. Another lucky break soon led to my next corpus study. I was at the National University of Singapore for a few months in the summer of 1982, accompanying my partner who had a visiting research position. The library, it turned out, had a complete original run of the *Philosophical Transactions of the Royal Society*, the first scientific journal in English. I later found out this copy was acquired when the library was restored after the Second World War. The story goes that Derek de Solla Price was posted there and charged with keeping watch over the journal; he stacked the volumes of the journal by years against the walls of his rooms as the library building was being restored. That's how he claimed he discovered his hypothesis about the exponential increase of science. Since that bit of serendipity, the volumes had been rarely used as many of the centuries-old pages were still uncut when I examined them. The library did not have many restrictions on circulation, and cheap, no-questions-asked photocopying was readily available. In the years before digitization and internet access, this was an incredible opportunity for me to acquire an important corpus for later study. I was able to photocopy all the articles from volume 1, 5, 10, 15, 20, 25, 30, 35, 40, 50, 60, 70, 80, and 90; this gave me a good sample from the journal's founding in 1665 to 1800.

When I returned home, I tried to make sense of what I had. On the face of it, the journal contained a heterogenous collection of genres, especially during the early decades. The first volumes contained mostly letters to the editor, Henry Oldenburg, but the correspondence soon was directed to the society for their regular meetings. Then the journal offered reports of what happened at the meetings, and finally it presented free-standing articles. The journal was becoming over time a venue in itself, separated from more personal interactions or society meetings. The rapidity of change in the early years of the journal meant dividing the corpus by traditional genres would highlight discontinuity and lose the trajectory of the change.

From the very beginning, nonetheless, a number of articles claimed to report on experiments. We would not currently consider many things then called experiments to be experiments, so even the idea of experiment seemed changing and emergent. I formed my corpus of those articles that specifically used the term "experiment." Tracking what counted as an experimental report and how it was reported became the focus of the study.

I counted the number of articles and pages along with the percentage of the journal devoted to such articles. These numbers revealed that experiments were initially only a small subset of articles, but they became increasingly prominent over the next hundred thirty-five years I studied. Given the obvious changes in the articles, I also needed a qualitative analysis of what kinds of accounts were being called experimental. I first informally described the articles to see what they were trying to do, which led to a list of questions I then subjected all the articles to in my more formal analysis (specified in Bazerman, 1988a, Chapter 3, p. 64).

I could identify trajectories of change in the articles along several dimensions. Some articles led the change and others lagged behind. Over the entire period, experiments showed increasing degrees of intentionality and design. They also reported greater procedural detail under more controlled conditions. Witnessing the experiments became more accessible to wider audiences, ultimately through providing instructions for all readers to recreate the phenomena. Results were presented with increasing precision and quantification. Research was more associated with theoretical issues and focused questions, embedded in reasoning, and ultimately within argument. All these developments moved toward modern forms of the experimental article, but prominently missing was the modern use of literature and associated citation practices. This absence left open a question for later research.

As I wrote these studies of corpora of journals over time, I became aware that I was making a different kind of historical argument than was typical in the history of science, which tended to focus on individual actors, their ways of working and thinking, their series of discoveries, and their contacts with other scientists. A few historians also studied scientific organizations and the individuals who formed or influenced them. But I was arguing that the textual forms themselves were a kind of institution, influencing the character, ends, and reportability of scientific work along with creating networks of interchange, critique, and theory. I was making the case that the changing form of texts was in fact part of the history of science, creating infrastructure for contributions and interactions as much as the formation of societies and other institutions that created spaces for individual actors to carry out their work. The few historians who shortly thereafter did start to attend to scientific writing adopted a more traditional historical approach of focusing on local stories of individual actors (for example, Dear, 1991).

## The Effect of Individual Historical Figures on Scientific Writing

In order to speak more directly to historians of science, I began to look into greater detail to the work of individuals and the institutions they created. Again, luck entered in here as a number of trips to Britain for conferences and talks allowed me to examine the archives of the Royal Society in London and the Newton archives at Cambridge University.

In examining the history and archives of the Royal Society along with the recently published correspondence of the first Secretary and first editor of the *Philosophic Transactions*, Hans Oldenburg, I found new roles emerging in relation to the exigencies of producing and evaluating articles for the new journal. With the emergent complexity of the roles and the kinds of problems perceived by the stakeholders, I started to see how people took on multiple roles which had conflicting obligations and pressures. The evaluation criteria for the journal, in turn, interacted with the changing form and contents of the articles. I looked to sociological theory about role, role conflicts, and conflict mediating devices in order to shed light on participants' actions. The parts started clicking in place when I connected role conflict theory with Merton's norms of science, namely communality, universalism, disinterestedness, and organized skepticism. These norms I saw as ways of juggling and resolving role conflicts by creating different stances, commitments, values, and identities for the scientists. These norms fostered greater dispassionateness in evaluating the work of others and allowed scientists to identify with a communal endeavor which buffered the inevitable bumps, bruises, and outright losses resulting from competitive advocacy for findings (Bazerman, 1987b).

Seeing how changing forms of writing evolved in response to role conflicts and changing norms pushed my writing to be able to explain coherently how multiple theories could come together in a complex mechanism of interlocking parts. Multiple pieces and theoretical accounts started to move around in my head and one thought led to another. Over several weeks of lightbulb moments, I had a continuing cascade of thoughts "oh that's why this occurred . . . oh that's how that fits . . . oh, all the parts are here." In its own way this was as extraordinary an experience as my somnambulistic undergraduate paper writing described in Chapter 11. But here I was wide awake, actually in a return visit to Singapore on a visiting professorship, and I remember many of these light bulbs going off as I was swimming daily in the apartment complex pool. As these pieces fit together, they each became more meaningful because they worked with each other and coincided with the historical facts I was putting together. I initially presented this work at the National University of Singapore and then published it as an article before including it as a chapter in *Shaping Written Knowledge* (Bazerman, 1988a).

As I was working on this second study of the early *Philosophical Transactions,* I continued to be intrigued by a series of articles surrounding Newton's optical theory in the earliest years of the journal, which didn't quite fit the evolving patterns I had been noting. This well-known controversy extended over two years in 16 published articles by Newton and his critics (with several more unpublished letters in Oldenburg's correspondence). Historians had studied this exchange as an intellectual disagreement, but they had not looked at from the perspective of rhetoric or writing, that is, how Newton carried out his side of the argument or how the differences of the situations and functions of the texts would have influenced his goals and strategies in each form, potentially explaining his rhetorical strategy that crystalized in the *Opticks*. As I started to look into the case, I became further intrigued to find that Newton had made several explicit statements about his argumentative strategy. Ultimately he rejected journal publication as

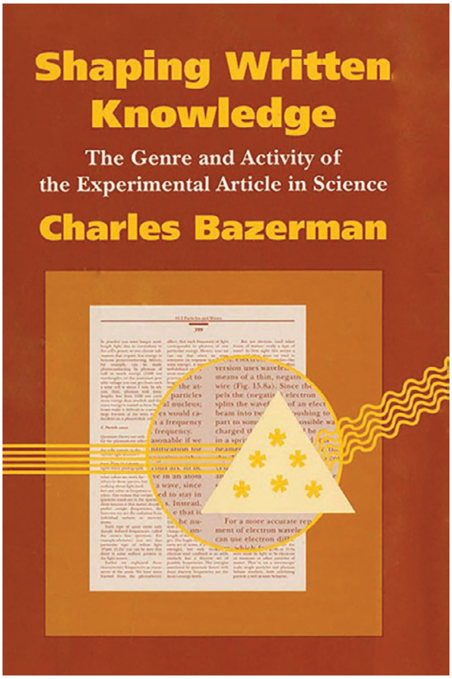


*Figure 20.1: Shaping Written Knowledge*

not being conducive to the kind of extensive theoretical elaboration he needed for people to understand his claims. He found his conducive form of argument 30 years later in his book *Opticks*. I also found out his initial journal article and consequent response articles were not his first attempt to formulate a theory of light, which started in his undergraduate notebooks, continued in his lectures as Lucasian Professor at Cambridge (both fortunately published for the first time in scholarly critical editions shortly before I started this study), and several other unpublished manuscripts (Bazerman, 1988a, Chapter 5).

This study indicated both Newton's atypicality in his initial frustrations at the journal interchange and his rhetorical leadership in forging modes of argument in his book that would come to dominate a century later. By learning how to address the extensive historical research literature on Newton with a detailed study of his manuscripts as forms of writing, I was able to develop a way of talking about the way transformative scientists thought rhetorically about how to make their novel ideas visible and persuasive. In this way they changed not only science, but the way scientists wrote to formulate and argue for their science. I would later find this rhetorical leadership in other figures such as Joseph Priestley and Adam Smith.

## Studies of Contemporary Practices

As I was looking into the history of scientific writing, I continued contemporary studies of how physicists read published scientific studies and how social sciences adopted and adapted scientific genres. Each of these contemporary studies taught me new methods of inquiry and new forms of presentation. In order to understand what readers got from articles and how they went about reading, I had to learn how to carry out interviews and *in situ* observations, triangulating between what a group of physicists told me about their reading practices and what I observed as they actually searched for articles, made decisions about the depth of reading they would engage in, and went about making sense of the articles they read for their own research purposes. These are techniques and writing challenges well known to qualitative, ethnographic social scientists, but for me they were new and made necessary by my interest in how people interacted with texts. My observations were particularly hybrid as I asked think-aloud questions of the physicists while they were interacting with the journal contents; I needed to correlate what was being said with my notes on what the subjects were looking at and the actual texts they were reading. In doing so I came to see how fundamentally their worlds, planning, and work were shaped by historically emerged structures of articles, publication procedures, publishers, and evaluation systems. Articles they read formed what they knew, how they thought, what they contributed to, and what was expected in their contributions. Their readings concretely realized the collective wisdom of the scientists who had come before and contemporaries with whom they interacted (Bazerman, 1985a).

There was a logic in the sequence of my research questions. As I learned how differentiated and emergent scientific writing was, I began to inquire into how embedded the emergent forms of writing were in social and publication arrangements, and how those forms and expectations emerged through the rhetorical, persuasive choices of individual writers, great and small. Genres, though recognizable through conventional forms, were dynamic sets of options forming and reforming within the goals and social organization of their fields. Each of these inquiries led me into different materials, along with different methods of collection and analysis to reveal patterns and processes. Insofar as the work was historical and archival, I needed to locate moments and archives where issues would be robustly evident, and then each study would suggest different forms of analysis. Some of the inquiries, however, would require stepping outside of the archives to gather evidence of contemporary practices of scientists.

One question that started to nag at me was how the social sciences began to write in what they considered scientific genres. As I started to see that scientific writing was historically changing and differentiated among and within disciplines, I wondered when, why, and how social scientific writing evolved from the natural sciences, and with what consequences for current practices. This ill-formed issue came into focus when I was invited to participate in a conference on *The Rhetoric of the Human Sciences,* with papers to be collected in a volume of the same name. When I discovered both Thomas Kuhn and Clifford Geertz would be participating in the conference, I felt even more pressured to find a coherent approach grounded in careful evidence. Textbooks about writing in the disciplines and the interdisciplinarity of my research had made me aware of the great influence the *Publication Manual of the American Psychological Association* (3rd ed., 1983) had throughout all the social sciences. The influence was most visible in citation practices expected at all levels from undergraduate papers through the published research articles in a number of disciplines. The APA publication manual, however, regulated many other areas from pronoun and verb choice, through text organization and use of subheadings, to experimental design and survey subject choices. In Kuhnian terms, it was paradigmatic in regulating the disciplinary matrix of psychology and other social sciences which adopted its standards. I quickly found that, of course, the APA manual had a history. That history was relatively short, little more than a half century (at that point), growing rapidly from a six-and-a-half-page article of advice to graduate students, passing through a number of forms on its way to a separately bound volume with 132 pages in its third edition in 1983 (and now in 2020 appearing in its seventh edition with 427 pages). I compared the contents of the various versions, the length devoted to each of the topics, and the regulatory directiveness of each recommendation. But that was only a part of the story.

Establishing a set of rules hardly means people were already following it or that they would uniformly follow it after the fact. Rules are usually made to control behavior because at least some people are doing otherwise. As a wise institutional mentor told me, "No one puts up 'don't walk on the grass' signs unless people are walking on the grass." I decided to compare the recommendations of evolving versions of the *Publication Manual* to the professional practices within psychological publications from the time of the field's separation from philosophy in the late nineteenth century through the earlier versions of the manual. It turns out that the writing in the psychological journals was more varied and more philosophically intensive than the narrow empiricist formats suggested in the manual, even among prominent behaviorists. The recommendations and then the compulsory rules were not enshrining already established, universally accepted practices of manuscript evaluation and publication decisions. They were, instead, conscious attempts by a small group of disciplinary leaders to impose a particular approach to research and to what would count as a contribution to psychology. This imposed approach embodied particular theories about psychology and psychological science, which further entailed beliefs about the appropriate roles and forms of reasoning available to psychologists, since psychologists themselves were psychological beings. The knowledge they would produce, accordingly, would only be considered reliable if they were guided by the epistemic principles of this version of psychology. So my study of the archival materials moved from comparative content analysis of the regulatory documents to close textual analysis of articles, embedded with an historical contextual analysis of the discipline, its journals, and the field leaders. These analyses then led to an ideological analysis of the intellectual program realized in the various editions of the regulatory manual. The study did not pass judgment on the theory, epistemology, or research practices of this view, but only pointed out the nexus that was intentionally advanced by key players as they came into control of the means of publication. Unsurprisingly, this approach to psychology remained contested, even as holders of alternate views had to accommodate to the publication rules and criteria. The contestation has in fact increased since my study, as the approaches in the field have proliferated and gained strength (while others have been pushed to the margin as not rigorously scientific), though the publication regulation has stayed in place to influence other social sciences. While the style (or parts of the style) may in fact be convenient to other fields for their own reasons, I hope my analysis brought to awareness some of the implications of that style (Bazerman, 1987a).

## Creating Coherence out of Separate Studies

I initially saw the studies described in this and the previous chapter as separate, but after most were written and several published, I began to see how they fit together into a big picture, which would gain meaning and force if I brought them together. Conceiving a book posed the problem of how to sequence and frame the studies, and provide transitions among the parts. It also made me think about and commit to an overall argument and the evidence needed to support it. The result was *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science* (Bazerman, 1988a).

Given my underlying practical motivation in the teaching of writing, I felt obligated to show how these arcane interdisciplinary studies could contribute to improved pedagogy. The first chapter argued why interdisciplinary and historical research was necessary to understand the complexity and specificity of academic writing in order to guide instruction and practice. The second chapter expanded on the value of detailed study of research writing through an adaptation of my initial study showing the radical difference among writing in different fields; this chapter would demonstrate to the readers the warrant for the consequent studies. Over three chapters, I then created a narrative of the emergence and changing form of experimental reports in journals during the 17$^{th}$ and 18th centuries in relation to the changing social arrangements. A next section of three chapters then spoke to 20th century uses and practices of scientific writing in physics, and a further section considered the uptake of scientific genres in the social sciences. The penultimate chapter of the book addressed theoretical issues arising from the studies, particularly concerning how writing mediated between our experience of nature and our organized, inscribed, published knowledge. The last chapter then returned to the practical implications for contemporary science writing and the teaching of academic writing. In this structure I see echoes of my college papers on the role of dramatic prologues and epilogues, bringing the audience from the street into the world of the drama, and then at the end returning them to their everyday lives, but somehow bearing the message of the performance.

As I was putting the finishing touches on *Shaping Written Knowledge*, I still had open questions about the emergence of scientific writing, leaving work for future studies. I already had more than enough material to write this one book and already had taken a number of risks. Rather than having studies focused by easily recognizable problems within a well-structured disciplinary literature, I increasingly found myself inventing new research questions and problems elaborating the theoretical picture I was constructing through the sequence of studies. I kept going further and further out on my own limb. I was making bets on the future that eventually enough people would get what I was doing and that over time the logic of those studies would become more apparent and relevant. This bet, however, pressured me to explain the connections I was making. As I reached towards different kinds of questions than were common in the field of teaching of writing and drew on literatures that were far outside the field, I was imposing on myself an obligation to introduce, explain, and show the relevance of these disciplines to my colleagues, in the hope that they could see why I was wandering so far afield. Perhaps they might also become engaged in some of these literatures. To some degree these were good bets, but even the best bets often lose, so that while some have pursued lines close to mine in some respects, other parts still hang out there, and the total intersection of questions and literatures that I pursued still appear to me to be idiosyncratic. So I still feel compelled to keep explaining myself, like the Ancient Mariner, bending the ears of polite, but impatient wedding guests.