5 LITERATE ACTS AND THE EMERGENT SOCIAL STRUCTURE OF SCIENCE

Elizabeth Eisenstein, in her monumental work, *The Printing Press as an Agent of Change,* details major events in the formation of literate culture, which in turn transformed politics, society, economics, and knowledge. That transformation, although fomented by a single technological invention, was realized only through a nexus of many innovations—linguistic and social as well as industrial. Similar lessons are to be found in Goody; Graff; Havelock; and Scribner and Cole. The history of scientific writing also reveals the many developments necessary to realize literate culture.

In the previous two chapters I have examined the emergence of a linguistic technology that has helped shape modern literate culture. I have associated this linguistic technology with the generic features of scientific experimental communication, which in our time has been associated with certain regularities of form. However, as the change and variation within the pages of the *Transactions* and of Newton's optical writings suggest, the technology and the genre are no simple, rule-determined set of inflexible procedures and forms. They rather represent continuing realizations of social activity within socially structured situations. Industrial, social, and linguistic inventions, such as the inventions of the printing press, the scientific society, and the scientific journal, helped shape the situations out of which the technology emerged and in which the new technology provided the means of social action. The linguistic inventions of this new communication technology, because they themselves embodied social actions, in turn set in motion changes within the structured social situation. Humanly made solutions addressed the immediately perceived problems and provided an environment influencing the perceived structure of future problems.

This chapter looks up from the pages of the texts examined in the previous chapters to observe more directly the interaction between linguistic technology and social structure. In examining how social situations structure communication events and how forms of communica-

tion restructure society, this chapter will foregound sociological theory. Thus the literary analysis (contextualized in a social account) of the last two chapters will here give way to a sociological analysis (based on a literary account). I will be working largely within the view of social structure elaborated by R. K. Merton in *Social Theory and Social Structure*. As Stinchcombe points out in his commentary, in this view social structure lies within the individual's choices of socially structured alternatives. That is, individuals through perception of situation and available alternatives and in their choices make and remake social structure. Through microdecisions individuals both realize and create social macrostructure. In this chapter I argue that this Mertonian position is a contextualized, constructivist one.[1]

The First Editor of a Scientific Journal

In 1665, three years after he had been named secretary of the newly formed Royal Society, Henry Oldenburg founded the first scientific journal in English, *The Philosophical Transactions of the Royal Society of London*.[2] Although not a scientist himself, he saw his mission to advance science through increased communication. Already by the late 1650s he had started correspondence with a number of scientists, be-

1. By *constructivist* I mean simply the position that humans construct their own activities and knowledge. The constructivist position in the sociology of science has been associated with a critique of Mertonian social theory as falsely asserting that people behave according to preexisting abstract norms that seem to contradict the individual's immediate interests and actions (Knorr-Cetina, *The Manufacture of Knowledge;* Collins, *Changing Order;* Mulkay, *Science and the Sociology of Knowledge;* Barnes, *Scientific Knowledge and Sociological Theory*). I neither read Merton that way nor agree with the critique.

Sociological constructivists have favored microscopic studies of individuals' situated actions over studies of larger patterns of regularities in individuals' social behavior. The social belief and apparent social influence of such larger patterns has correspondingly provided a puzzle in constructivist accounts. Attempts to explain the status of apparent macrostructure and the mechanisms by which that apparent macrostructure may be generated from microactions are presented in Knorr-Cetina and Cicourel; and R. Collins. Such accounts are thoughtful, ingenious, and interesting, but would be aided by an understanding of the mechanisms linking microactions and macrostructure already implicit in the Mertonian theory they have largely rejected.

2. The French *Journal des Scavans* first appeared three months prior to the *Transactions*. Various authors still contest which nation shall have the honor of giving birth to the first scientific journal, with the crucial point hanging on the broader character of the French journal.

coming a conduit for exchange of scientific information across Europe.[3] As his correspondence and skills as a correspondent grew, he began to see how increased sharing of information goaded working scientists to produce more and to reveal more of what they were doing. Conflict inevitably resulted as correspondents learned the opinions of others and as Oldenburg synthesized the findings of scientists working in the same area. Oldenburg, although becoming highly skilled at elaborate flattery and social graciousness, did not try to gloss over such differences, but rather encouraged their recognition. From the beginning he sensed that science needed to be agonistically structured, so that each player—seeing the moves of the others—makes countermoves attempting to defend his position and to eliminate his opponents from the field.[4] This is not the exposé of the dirty social underbelly of science—this is the plan for science. As long as such conflict was played out in the semiprivacy of correspondence, it did not lead to serious hostilities (M. B. Hall 187).

The role of correspondent in the persons of Oldenburg and, on the continent, Marin Mersenne, helped bring together a previously dispersed scientific community, which had communicated primarily through books. The slowness of book publication, the limited distribution, and the increasing popularity of vernaculars had kept the scientists' audiences and communicants limited. Moreover, books tend to present self-contained universes, accounts complete in themselves with little opportunity for response, except in the muffled comments of the unsatisfied reader. Communication through books minimizes confrontation, disagreement, discussion, synthesis, and sense of competition.

The reactive social dynamics encouraged by Oldenburg's correspondence were also encouraged more locally by the early scientific societies, the Royal Society of London, the Academia del Cimento, and the Académie des Sciences. Standing between the Royal Society and the rest of the scientific world, Secretary Oldenburg became the center of scientific communication, It is little wonder then, that Oldenburg, needing an additional source of income, created a journal of scientific information and found a ready market. The journal put Oldenburg even

3. Information on Oldenburg's life and works is to be found in the introductions to the nine volumes of *The Correspondence of Henry Oldenburg*, edited by M. B. Hall and R. Hall; in M. B. Hall, "Henry Oldenburg and the Art of Scientific Communication," *British Journal for the History of Science* 2 (1965): 277–90); and in R. Hall, "Henry Oldenburg," *Dictionary of Scientific Biography*.

4. Latour and Woolgar, chapter 6, expresses a similar imagery of scientific research as an agonistically structured game, where each move restructures the game.

more in the center of communications with his correspondence doubling in the first year and tripling again within three years.[5]

Although Oldenburg did not succeed in turning as much profit as he had hoped, he did succeed in turning himself into an editor, the first scientific editor. In the earliest issues he was still very much the correspondent, writing an extended newsletter of all items of interest that had come to his attention: a new book from the continent, a presentation he had witnessed at the meetings of the Royal Society, a report he had received from one of his correspondents. All was filtered through his voice as he selected and focused attention on those aspects he thought his readers might find most newsworthy. Some features of his writing do change from his previous correspondence: the information is selected to be of generally wider interest, and the long passages requesting information and continued correspondence vanish, although they remain in the private correspondence. Nonetheless, important stylistic features remain: the chatty informativeness; the assumption that the readers are knowledgeable about the subject at hand and are therefore only looking for the latest news, which they will largely know how to interpret; and the consistently complimentary tone, aimed at encouraging continued cooperation. In short, although personal business has been eliminated, Oldenburg still treats the readers as correspondents, people who write to him with information in return for the information he provides them.

Editor, Author, and Reader

However, the new social dynamics of a broadly circulated periodical soon necessitated changes in Oldenburg's relationship with his audience and authors. Within scientific correspondence even the distinction between author and reader had hardly been a sharp one. Whereas previously his correspondents both read and wrote letters, now only a small subclass of the readership contributed information for

5. By a count of letters written by him in the published correspondence, which includes all extant items, in the two years prior to his secretaryship, his letters numbered 14 in 1661 and 9 in 1662; in 1663 and 1664, his letters numbered 52 and 59, respectively. In the first year of editorship, 1665, the number jumped to 115; even more strikingly, through April, before the appearance of the first issue, his letters numbered only nine, with nine more added through June, with all remaining 97 letters being written in the second half of the year. In 1666 he wrote 114 extant letters and in 1667, 151. In 1668 the number jumped to 318, and continued at high levels for the next years.

the benefit of the rest of the readership. The contributor becomes a more distinctive and important voice than the newscarrier. Accordingly, Oldenburg increasingly lets the contributors speak for themselves, turning them into authors. He rapidly increases the amount and length of quotations from his sources, until he soon prints entire letters with only a short editorial introduction. Eventually that editorial introduction vanishes as does the form of the letter, leaving freestanding authored articles. In changing from a correspondent, passing on the news through his own perception and personality, to an editor enabling authors to communicate directly with readers, Oldenburg seems to vanish from the pages of the journal, appearing only in the occasional editorial statement. Yet, while the editor is apparently nowhere, he is of course implicitly everywhere, in the appearance, content, style, and personality of the entire enterprise. An editor's voice is a composite voice, comprising all the voices that make up the journal. The quieter the apparent editorial voice, the stronger the corporate one.

In standing between the journal authors and journal readers, the editor helps define not only his own role, but the character of these other two roles. Oldenburg could not keep his journal afloat unless he had authors to fill up the pages. Although at first he could rely on the residual habits of correspondence, the new configuration of editor standing between authors and audience could no longer support the old motivations of information sharing and competitive reaction. Indeed, the new publicness would prove a serious irritant to potential authors. Oldenburg had to offer other lures, such as public exposure of ideas, priority, fame, cooperation of amateur fact-gatherers, and participation in a great universal undertaking. Competitiveness was recast in the threat that the competitor might win these rewards first (for example, see Oldenburg's *Correspondence* 2:439–43; 3:631–33; 4:331–33).

Once Oldenburg enticed a correspondent to share information to be published, he had to keep the contributor satisfied with the results to ensure continued contributions. This we can see in three areas: first, accurate reporting of the information being shared (adding to the pressures for increased use of the author's voice and placing limits on editorial modifications of submitted articles); second, ensuring contributors perceive the benefits they receive from publication (through praise in the editorial voice and in private correspondence with the contributors); and third, protecting the contributor from some of the less pleasant consequences of publication (primarily through ego-stroking and appeals to higher values in private correspondence surrounding an open controversy in the journal pages). These activities to maintain good relations with his contributors potentially conflict with his

responsibility to the communal endeavor of science as embodied in the Royal Society. To resolve this conflict, Oldenburg removed editorial commentary on individual contributions from the pages of the journal, leaving the flattery for the letters. He now stroked his authors in private, not public.

Insofar as authors see the benefits of publication, they start writing for the audience, which has the power of granting recognition, instead of for the editor. The editor becomes an intermediary. Thus contributors write Oldenburg increasingly public, formal letters for publication rather than private communications to be digested by Oldenburg. By anticipating the editorial process, authors gained greater control of how their work would be presented. Thus letters came to have clearly marked expository sections, with private material gathered together in other deletable sections, through time reduced to a few prefatory personal comments. Eventually entirely public letters were written, accompanied by private letters of transmittal. Dropping the pretense of the letter form, authors began addressing readers directly in article form, transmitted with a private cover letter to the editor.

The role of the reader is less visible, the act of reading leaving little physical trace. We do know, however, that the early membership of the Royal Society and the readership of the *Transactions* were far wider than the collectivity of active virtuosi. During this early period society membership and journal readership were dominated by leisured gentry, neither professionally nor personally committed to orderly, extensive, systematic investigation. Rather, as members of a largely urban and educated class, they sought amusement and novelty. They were excited by the new philosophy but not necessarily critical or thoughtful in their appreciation. A few merchants and artisans from fields like mining and lens-grinding supplemented this primary readership, as did a few rural and colonial gentry (Hunter, *Sciences and Society* 70–80). In appealing to this nonprofessional, novelty-hungry audience, Oldenburg took for his domain the wide wonders of the world including earthquakes, medical monstrosities, language education, and foreign journeys.

Contributors to the early journals also wrote for this kind of audience, using the language of curiosity and wonder to create appreciation for new findings and inventions. Contributors used their texts to gain publicity and other forms of support for their work. Newton, for example, presented his optical findings in the *Transactions* to promote his completed book on the subject. More actively, contributors sought support for their investigations by requests for meteorological, oceanographic, naturalist, and anthropological data from travellers.

Public Identities and Role Conflicts

The public presence of the journal and other forms of publicity, such as Sprat's *History of the Royal Society,* established a public identity for the journal, its contributors, the society, and its membership, as standing for a new movement in knowledge. Satires by Samuel Butler, Thomas Shadwell, and Aphra Behn relied for their effect on general public recognition of the social type of virtuoso.[6] For *Transactions* readers as well, the cast of characters and the enterprise started to take on social meanings. The *Transactions* became a point of contact for readers in small cities outside London and aided in the formation of local societies. Part of the purpose of these local societies was to make available copies of the *Transactions* and to imitate the activities reported therein (Hunter 81). Oldenburg, as the center of an increasingly organized communication system, took on a recognized scientific role and identity, even though he himself was not a contributing scientist. Finally, individual scientists, such as Boyle, Hooke, and Newton, became public figures through regular publications; in Newton's case his public presence was only on the rarest occasions supported by actual attendance at a Royal Society meeting.[7]

As public figures, natural philosophers were expected to live up to norms of genteel and politically responsible behavior. But their roles as natural investigators required rather odd behavior, such as looking at the moon and waterdrops, using peculiar contraptions like vacuum chambers and microscopes, and suggesting unorthodox opinions about taken-for-granted objects. Not only did they do this at public meetings, but they wrote about it in the journals so that anyone could read about it. These role tensions and violations provided grist for the satirists. (Sociological role theory emphasizes the importance of publicness as a key factor in role conflict; see Marwell and Hage; Merton, "The Role-Set"; Stinchcombe; and Stryker.)

At first, role conflicts were perceived more outside the nascent scien-

6. Samuel Butler, "Elephant in the Moon" and "On the Royal Society" in *Genuine Remains in Verse and Prose* (London, 1759); Aphra Behn, *The Emperor in the Moon* in *The Works of Aphra Behn,* vol. 3 (London: Heinemann, 1915); Thomas Shadwell, *The Virtuouso* (London, 1676). Shadwell's play in particular shows evidence of the author's extensive readings of the *Transactions* in search of satiric details.

7. Although becoming a member of the Royal Society in 1672, Newton did not attend his first meeting until 18 February 1675. His attendance remained sporadic even after he moved to London and was elected to the society's council at the end of the century. Only with his election as president did he begin regular attendance, after missing his first meeting as chief officer. Richard Westfall, *Never at Rest*: 267–b8, 476, 629.

tific community than in it. Inside this community, members were recognizing a separate professional identity, establishing themselves as their own primary reference group (see Merton and Rossi, and Turner, "Role-Taking"). However, an emerging division within the readership of the *Transactions* soon led to new types of role conflict. Within the largely amateur, uncritical readership was a smaller circle of readers more actively concerned with the advance of knowledge. These would read critically, comparing what they read with what they believed and observed. Of course, critical reading occurs whenever a reader has a stake in the writer's topic, but now the critical reader could criticize in a public forum proximate to the original text. The journal facilitated not only criticism, but the public role of critic. Just as correspondence networks had served to increase the amount and immediacy of criticism, the journal made the critical activity public. And the answer also became public, casting the natural philosopher into the regular role of public defender of his work. The role of the third-party audience became important in the resolution of disputes.

This argumentative situation creates role conflict for the authors, who are caught between publicizing their own work in terms that would most appeal to the general reader and defending their work from the inner circle of specialized readers who have the power to criticize and therefore cast doubt upon work in public. Power begins to flow to a subclass of the readers, those best able to assess or criticize the work being presented, thereby affecting the general public impression of the work. If ali potential critics are satisfied, no debate will ensue and one's work will appear unchallenged. Similarly, if an article avoids the domains of all potential critics, the work will again appear undisputed. However, if one makes claims in an area where others have interests and those claims unsettle those interests, challenge is likely. The article to be successful must then either disarm potential opposition or lay the groundwork for proper public defeat. Thus contributors' interests are best served by developing standards of public argument and adhering to them. The narrative of chapter 3 describing the emergence of the experimental article details both the pressures shaping standards of argument and the consequent standards as embodied in textual practices.

As the articles in the *Transactions* became more concerned with professional argument, other more popular journals (such as *Weekly Memorials for the Ingenious* and *The Athenian Mercury*) filled the gap between professional and popular audience (Hunter, *Science and Society* 55). Since the general audience was no longer the more powerful force for the authors, authors in the primary journals no longer served the needs of the general audience so well. Moreover, the serious natural scientists found the

general audience interlopers. Several attempts were made to control the membership and increase the professionalism of the Royal Society (Crosland; Hunter and Wood; Hunter "Early Problems"; Stimson 147–51). Editors began to eliminate articles of insufficient professional interest and quality. In 1752 referees were introduced to maintain professional interests and quality further.

Thus the author's role conflicts in relating to two separate kinds of audiences in the same public forum led to separation of the two audiences (see Biddle and Thomas; Marwell and Hage; Merton, "Role-Set"; Stinchcombe; Stryker; and Turner, "Navy Disbursing Officer"). This social reconfiguration of the participants in the journal communication process led to further redefinition of roles, new conflicts, and new mediating mechanisms.

Exclusions and Gatekeeping

The reconfiguration relies on the social facts of recognition and authority, both externally and internally, of the Royal Society and its publications. Public recognition of the Royal Society as the primary social institution committed to inquiry increased the prestige of membership and publication in its journal and gave the society sufficient public capital to be exclusionary. Supporting this symbolic power of the Royal Society was the transfer of the *Transactions* from private ownership to the society in 1690, freeing it from private mercantile interests (Stimson 114), although it was not technically the official journal of the society until 1752. The editors (all secretaries of the society until 1751) could now look solely to the ideals of the society for guidance in shaping the journal. These goals now were to be achieved by exclusivity rather than inclusivity, turning the editor from a merchant of knowledge into a gatekeeper. At first keeping out information of only amateur interest, then keeping out work of amateur quality, the editor limited the potential audience and began to monitor the statements made among the professionals.

The exclusion of contributions, however, did put special burdens on the editor. First, the editor needed to establish sufficient authority to have his judgments respected as sound. Since this particular institution was founded on scientific inquiry, only the judgments of a respected natural philosopher would carry intellectual weight. An administrator

secretary and editor from 1695 to 1713, and later as president of the society (Stimson 143).

Second, in order to retain authority and trust of the professional community, the editor must be perceived as fair and unbiased. However, since the editor has his own research interests and competences, he cannot remove himself from accusations of bias and/or selective incompetence. Moreover, insofar as the editor exercises authority by making judgments, he inevitably creates injured parties. No matter how much participation in the public discussion of a journal appears a desired good to members of a community, an atmosphere of unfairness and distrust, especially attributed to the chief interlocutor (the editor), will poison the atmosphere and destroy the communication.

Indeed, such a conflict took place in the early 1750s. At that time antiquarians' and historians' interests had become dominant in the society, and those interests were represented by the secretary/editor Cromwell Mortimer. John Hill took up a campaign of ridicule against the *Transactions*, pointing to the triviality and foolishness of many reports published therein. His criticism heightened after he was not elected to membership in the society. The response of the society to his satire was to take responsibility for the journal out of the single hands of the secretary editor and place it in those of a committee, which would review and select manuscripts to be published (Stimson 140–45).

Through this innovation, the Royal Society established the role of editorial board cum referee. The editorial function was maintained and strengthened by removal of the responsibilities from any one individual's hands. In order to maintain authority, the editor cannot be perceived as exercising it, but rather must take a distanced stance on all decisions which might be likely to be perceived as injurious to others. The invention of editorial boards to handle issues of general policy and of referees to handle issues concerning individual contribution not only helps the editor maintain authority and trust by assigning responsibility to other individuals, but it further allows the journal to establish a corporate identity, representing the field as a whole. Perceived scientific eminence of editorial board members and referees, as well as distribution among the various subcommunities of the larger scientific community, help maintain the authority of the journal as an institution through the appearance of fairness and generalized competence.[8]

8. Maintenance of the appearance of fairness is important to the maintenance of authority in bureaucratic settings; this generalization has been taken as almost axiomatic in the literature on bureaucracies since Weber. For a seminal discussion on the relationship between gatekeeping, critical criteria, and the maintenance of communal trust, see R. Merton and H. Zuckerman, "Institutionalized Patterns of Evaluation in

Group Formation and Integration

Stringent gatekeeping only works when individuals so wish to enter gates that they are willing to satisfy the gatekeeper. The early motives to publish in the *Transactions*—publicity before mixed audiences, priority, possible cooperation of amateurs—were at best peripheral to the activity of carrying on natural investigations at that time. Even the lure to participate in the great universal undertaking, Bacon's Salomon's house, appealed more to ideals than to the realities of research. However, as the character of scientific communication changed from the late seventeenth century to today, publication became essential to research and integrated the working scientists into a communications network. Increasingly, one could only play the game by stepping onto the playing field, and stepping onto the playing field drew one into the social organization of the game players.

An early step in this process of group formation occurred when publication in the journal became a recognized identity-granting social activity. Presenting work before the Royal Society and being mentioned in the pages of the *Transactions* identified one as a natural philosopher. The success and prestige gained by the journal then accrued to whoever published therein. Perhaps more importantly, this prestige lent legitimacy to the work itself. It is one thing to mix chemicals in the back shed at the estate; it is another to be in contact with a secretive brotherhood of suspect alchemists; and it is quite another to participate in open demonstrations as part of a prestigious social institution.

Although at first criticism may have seemed a rather irritating byproduct of public exposure, particularly within such a motivatedly critical crowd, this too became seen as a necessary, though unpleasant medicine. Statements acknowledging the usefulness of criticism appear in a variety of articles and letters in the seventeenth and eighteenth centuries, even from the notoriously intolerant Newton.[9] Only serious professional criticism could broaden the individual scientist's narrow view

Science," in *The Sociology of Science*. For other accounts of difficulties of early editors see Sherman Barnes and of modern editors see Fox, chapter 1.

9. Newton's grudging recognition of the benefits of communal cooperation and criticism can be seen in the closing lines of his article "New Theory of Light and Colours," and in his dubious compliment to Pardies: "In the observations of Rev. F. Pardies, one can hardly determine whether there is more of humanity and candour, in allowing my arguments their due weight, or penetration and genius in starting objections. And doubtless these are very proper qualifications in researches after truth" (*Philosophical Transactions* 6:4014. Translation from *Newton's Papers and Letters on Natural Philosophy* 106, ed., I. B. Cohen).

and could separate personal conviction from universal truth; the professional forums of publication offered this criticism most readily and reliably.

Gradually researchers start to recognize the cooperative interlinking of their work. The shoulders of giants commonplace turns during the late seventeenth century from a resource in the ancients vs. moderns struggle to a recognition of one's near contemporaries (Merton, *On The Shoulders of Giants*). Informal and irregular recognitions of debt occur throughout the eighteenth century, and in the nineteenth century modern citation practices start to develop. Citations began only as a recognition of debt, but developed into a close interlinking of the current work with the on-going research and theory which formed a codified network of the literature.

In these ways, researchers recognized that their work meant more for being part of a socially legitimated, critical, socially interactive, and cumulative communal process centered on publication in socially recognized forums, screened by gatekeepers, facing public criticism, being cited by others, and being accepted into a codified literature. These elements form the core of most contemporary accounts of the current communication of Science (see, for example, Garvey; Meadows, *Communication;* Ziman, *Public Knowledge*). Group integration as represented in journal publication has become so much the hallmark of modern science that Kuhn takes it as the primary indicator of mature science.

Yet we must not idealize the integration as a simple vanishing of the individual into the group processes. This is the error of Salomon's house, science by bureaucracy, and the ill-fated French Royal Academy attempt to declare science from the outset to be an anonymous, joint endeavor (Hahn 26–28). Integration only worked as an integration of individuals who see personal interests and identity expressed through the group activity. The individual must not only identify with the community as a whole, but must see that his own contribution to the group endeavor will raise his own standing within the community, allowing him to contribute more fully.

Persuasion, Witnesses, and the Representation of Events

A fundamental change in group identification and individual assessment occurs when a contributor presents his work for the

scrutiny of his peers as well as for their enlightenment. He no longer can adopt the pose and authority of the expert informing the uninitiated. He must rather establish the authority on communally accepted grounds beyond himself. Thus empiricism, which for Bacon was a mode of investigation, now becomes a mode of persuasion (Dear; Shapin, "Pump and Circumstance"; B. Shapiro; Hacking, *Rise*). To persuade someone of something you must show them what you have found. That is, an event in nature is not an empirical fact with scientific meaning until it is seen, identified, and labelled as having a particular meaning. Moreover, although it may be a fact to the person who first locates it, it is not a fact to other researchers until they have been satisfied that that event has occurred. Only by making the fact communal can one claim discovery of that fact for oneself and reap the rewards of it.

In the early Royal Society, persuasion of facticity was accomplished directly by public demonstration before the assembled members, then recorded in the notice published in the *Transactions*. The persuasion occurs at the public demonstration; the publication does not persuade, but rather only reports the fact of public persuasion. As the particulars of demonstrations become recognized as crucial to the outcome, not all members could witness all trials, so representative witnesses (sometimes of royal or other nonscientific status) came to stand in for the general membership. With time, as it became evident that one needed expertise to view and judge the event appropriately, witnessing was limited to recognized scientists. That is, as events become treated as more particular, and more difficult to interpret properly, witnessing became less and less a public matter. Finally, witnessing devolved on a single witness, the researcher himself. This had to do with the change in research from finding brute facts into inquiring into the meaning of difficult to understand facts—troublesome events had to be investigated by a series of observations and experiments that served as part of an intellectual path of inquiry for the researcher. This meant that persuasion depended not on the presentation of selected, displayed brute events to others, but on the symbolic representation of events in the published report.

How does one convince a critical audience that something happened when they didn't see it? One rhetorical strategy is to establish ethos; that is, that the author/observer is a credible witness, following all proper procedures thoughtfully and carefully. Newton attempts this in his early article "A New Theory of Light and Colours" where he first presents himself as a proper Baconian stumbling across a natural fact before then asserting the bulk of his results categorically. Similarly, in the latter half of the eighteenth century, writers commonly presented themselves

as representative scientists by showing their reasonable path of inquiry. This strategy of establishing general credibility fails, as it did in the above examples, when other scientists get different results and come to different conclusions. Academic credentials today serve something of the same general function of lending credibility, but only in the most general union-card manner. That is, credentials permit one to present results, but the results must stand on other grounds (Cole and Cole).

With the failure of ethos as the primary means of validating results unwitnessed by others, the burden of persuasion fell on detailed accounts of each individual experiment—that is, on the representation: to establish proper procedure (that is, the experiment is done as any scientist might have done it), to specify all the conditions and procedures (that is, replication instructions), and to indicate how the experimental procedure answers potential objections. As findings and theory develop, consistency of results with other results aids in the persuasion. Anomalous findings raise more objections, requiring more vigorous counterarguments and powerful demonstrations. Seriously anomalous findings are also likely to undergo more serious attempts at replication than anticipated findings.

Consequently, while representation replaces immediate empirical experience of the audience/witness, the representation must appear experienceable by the audience. The representation must appear plausible to readers having expertise and experience similar to the author's, must seem so proper and controlled as to answer all objections and must offer an apparent replication recipe promising any trained scientist the possibility of experiencing the reported event. Although the replication instructions may in fact be incomplete, requiring additional craft knowledge to make the experiment work (Mulkay, *Science and the Sociology of Knowledge;* Collins, "Sociology of Scientific Knowledge"; Collins, *Changing Order*), the account must be consistent with replication procedures, whether or not the experiment is precisely replicated, for all future attempts at related findings serve as indirect replications. Thus authority now comes not from one's sources, nor from one's good person, nor even from a publicly witnessed fact, but from a representation of events, hewing closely enough to events and defining the events so carefully as to answer all critics, seem plausible to readers with extensive knowledge and experience with similar events, and to hold up against future attempts to create similar events.

As gatekeepers gain in power, restricting access to publication, the representation of empirical events becomes even more important. An editor or referee reading through a manuscript must judge plausibility and soundness solely on the written account. The longer term judg-

ments based on consistency with future results cannot enter into the short-range publication decision; the writer must present the results so that they appear to have happened.

Authority deriving from the representation of events devalues the immediate standing of the individual, institutions, and traditional teachings. Within the network of scientific communication, even kings, nations, and sacred texts lose power before those representations of nature identified as empirical facts. Within the scientific article, authors adopt humility before the facts, putting their empirical findings and derivative generalizations in the central rhetorical positions. On the other hand, those individuals, institutions, and beliefs which have the power of facts behind them gain the authority of empiricism. This leads to a curious conflict. As science gained general social prestige, individual scientists took on the roles of public spokesmen, adopting the mantle of authority from science. This external role, representing science to the wider social and political worlds, was far from the humility before nature demanded internally in science. Similarly, social institutions and belief systems claiming to be based on empirical fact took on a power and attitude of power quite in contrast with the tentativeness required within scientific work. Even within science, an individual convinced of his empirical evidence may assume an arrogance with respect to his colleagues out of keeping with his "scientific" role as an inquirer after the facts of nature. When a scientist's sense of self grows from one of these public, nonscientific sources, his scientific credibility not uncommonly wanes.

Role Conflicts and Differentiated Audiences

Such conflicts between self-assertion and humility are classic conflicts within the role set. That is, an individual filling a status such as scientist or editor has a number of different role partners, with each set of role partnerships incorporating different norms and behaviors; insofar as the partnerships remain mutually invisible, one's behavior can respond only to the partnership at hand. But when the behavior becomes visible to other role partners, conflicts arise (Merton, "Role-Set"). A policeman can be a mean character on the beat, a good guy to school children, and a regulation-follower in the patrol house; however, a school child witnessing a drug bust or a police investigation unit looking into procedural violations on the beat presents the policeman with a conflict as to how to behave.

Scientific publication, by definition, is a public act, hard to keep se-

cret from selected role partners. Moreover, journals provide a public forum and not just a public platform. Thus we would expect the public performance of journal publication to foment role conflicts and foster consequent mediating mechanisms. Further, since we have seen that the new institution of journal publication proliferates social roles, we would expect the opportunities for conflicts to increase with time. Finally, since the role behaviors we are most concerned with here are communicative behaviors, which are just where the conflicts are being publicly displayed, we would expect these conflicts to affect the writing.

As we have already seen, the changing social configurations of scientific communication created conflicts for contributors, who resolved these conflicts by addressing those segments where they perceived power to lie. At first power resided in the scientific correspondent for he controlled the return of useful news. With the journal, power began to rest with the readers who could grant recognition and spread of one's work. With the rise of criticism, power began to flow to the professional part of the readership who had to be satisfied to maintain credibility. The growth of gatekeeping placed the gatekeepers before all; and finally the development of cumulative science gave the last word to the readership of working scientists, for they held the key to incorporation.

However, these last three powerful partners did not displace each other: each retained power. To this day a successful publication must satisfy gatekeepers to get published, must defend itself against critics to maintain credibility, and must appear useful enough to readers to be cited and incorporated in future work. It is not easy to dance to all three masters, as evidenced by the many articles that get published and avoid criticism, yet never are cited, or articles that get published but then become the objects of controversy from which they do not emerge whole.

Conflict Mediation

The complex social configurations in contemporary scientific communication and community also present social complications for gatekeepers, critics, and readers, but for simplicity's sake the remaining analysis will primarily be from the perspective of the contributor. In particular, we will consider how four features of the contributor's role partnerships provide conflict-mediating opportunities. First, publication role relationships do not occur until near the end of the knowledge-production process, allowing extended areas of prepublication privacy and semiprivacy to develop problems, claims, arguments, and evidence. Second, the proliferation and differentiation of publication

venues allow the contributor to limit his visibility to selected sets of gatekeepers, critics, and readers. Third, since communication with gatekeepers occurs chronologically prior to communication with critics and readers, and since the three role partnerships hold different rewards, the contributor may make strategic choices among the role partnerships. Fourth, contributing scientists usually fill the gatekeeper, critic, and reader roles. While this aids the contributor by creating certain uniformities in the audience—uniformities that the contributor himself shares, in the long run this creates more conflicting demands on the working scientist. Nonetheless, this integration of all the roles within the single working scientist allows an overriding identification with the entire enterprise of science. The manifold conflicts on the working scientist may then be finally mediated by a set of institutional ideals and goals that distance the scientist from particular conflicting roles and that absorb the various affronts and setbacks.

A closer look at these conflict-mediating processes reveals that many additional features of the social structure of contemporary science can be seen as responses to exigencies created by the communication system. To start, as scientific communication becomes liable to increasingly organized scrutiny by gatekeepers, critics, and research-motivated readers, the preparation of publishable statements retreats more deeply into private and semiprivate workspaces.[10] The primary empirical event (increasingly observable and interpretable only by the specialist) moves out of public sight into the experimenter's laboratory, with the public presentation becoming only the claim-maker's representation. This substitution of representation for presentation allows the claim-maker added selectivity and control—in planning and executing the empirical events (that is, experiments); in reporting only successful experiments and eliminating false leads, distractions, and unworkable experiments; and in presenting a cleaned-up account of the experiment, without bad trials, fuzzy data, or slips of the hand.

Such representational control does open the door for unconscious and conscious distortions, ranging from seeing only what one expects to see to outright fraud (Hanson argued first for observations being theory-laden; Shapin, "History of Science," reviews the studies demonstrating observations as interest-laden). This necessarily is a matter of concern, and procedures have developed to hold individual scientists

10. Here I am ignoring more recent issues that have arisen from the grant process which has brought gatekeeping in a new way into the early stages of work. See Greg Myers, "The Social Construction of Two Biologists' Proposals," for an illuminating study of how the funding process helps shape the direction and focus of proposals and work.

accountable for what they report as happening in the laboratory. The extent and effectiveness of these procedures have from time to time come under question, particularly when major instances of fraud come to light. Here, however, we need note only that the systems of accountability are a result of the privacy of statement-making. The scientific community must assure itself that the writer of research is not a fiction writer, that the laboratory consists of more than a typewriter.

The scientist may also maintain a degree of privacy over work in progress by sharing early formulations of the work only with selected colleagues in informal settings—in the coffee lounge, in correspondence, or at closed seminars. These early exchanges help shape the ultimate public argument (for example, Latour and Woolgar, chap. 4). In some tightly structured specialties the less formal communication channels may be primary for the core group, with the published article only for the record and peripheral audiences. Once the informal communication in this tightly organized group passes the approval of the inner circle, then it is as good as published (Menzel; Price and Beaver; Crane). But not every informal communication passes that test to become approved, publishable material. Claims found faulty within the small group are unlikely to surface in reputable publications.

The emergence of validated claims from small research groups resembles the negotiation process that occurs among authors, referees, and editors before an article appears in the journal (Myers, "Texts as Knowledge Claims"). This semiprivate correspondence, shrouded by confidentiality, aims at transforming the private work into the most publicly acceptable form, although authors may not always see it that way and the semiprivacy raises questions about unintentional and intentional abuses (Mitroff and Chubin). Some accountability procedures have developed around the gatekeeping system to exercise control over the privacy.

Another kind of opportunity for conflict mediation through lessened visibility has been created by scientific specialization. The proliferation and specialization of scientific journals have preserved the publishing scientist from facing the judgment of the entire scientific community. In the evolving discussion of specialized research questions, local criteria for the acceptability and significance of work develop. These local criteria may be neither obvious nor superficially consistent with criteria of other specialties. On those limited occasions when specialized work strikes interspecialty issues clearly and forcefully enough to warrant more general presentation, contributors can seek publication in more widely distributed journals. Again, a negotiation process between author and gatekeepers may determine the level of generality at which a

claim may be presented and the proper form and place for such a presentation (Myers, "Texts as Knowledge Claims").

Third, the differentiation of audience into three separate kinds of role partners offers the contributor strategic choices in appealing to partial audiences. An untenured junior researcher, needing publications more than public recognition, will likely be most concerned with meeting the criteria of the gatekeepers. Other researchers, humbled by an ideology of cumulative science rather than by the employment system, may be satisfied to contribute a careful, small piece of work, paying most attention to the critics. On the other hand, if one feels wider ambitions thwarted by entrenched gatekeepers and critics, one may attempt to bypass them by beginning a new journal or creating a less conventional channel of communication to the readers. Self-declared revolutionaries and communally declared crackpots may both follow this route; this dichotomous naming indicates the gamble of this procedure.

Finally, and most significantly for the social structure of science, all the communication roles of contributor, gatekeeper, critic, and reader may be taken at various times by a single scientist. Every scientist is trained to read the literature critically and habitually searches the literature for new findings to build on (see chapter 8). As careers develop, scientists then get to referee and perhaps edit journals. By adopting these various role perspectives, quite literally taking the part of the other in communication partnerships, the research scientist learns to understand, accept, and meet audience expectations and demands. Once you act as a referee, for example, you know better how to satisfy referees. This psycho-social integration into the entire process of scientific communication acts as accumulation of advantage that accrues to successful scientists just as much as the more tangible advantages of grants and large laboratories (Merton, "Matthew Effect"; Cole and Cole).

Role Unification and the Norms of Science

All these communicative roles were only gradually integrated into the single status of scientist. Prejournal critics included clerics, kings, and philosophers. The first editor was an administrative organizer rather than a working scientist. Readership was quite wide. Even the interim role of witness, later incorporated into the role of the scientist himself, was at first widely held, then more narrowly held by people of prestige derived from a variety of social institutions. Only the referee role, the last of the roles created in this process, was born requiring that it be filled by a working scientist.

This gradual unification of roles results from empiricism replacing all other forms of authority in institutions concerned with natural knowledge. If authority lies in nature, those best capable of administering that authority are those who have the most intimate and rigorous contact with nature. At first, artisans and craftsmen held some authority because of their practical contact with nature, but this limited authority vanished with the rise of detailed, documented representations of nature replacing direct experience as the relevant form of knowledge. The intimate practical contact without the proper way to talk about it in public granted little prestige and no authority (Ochs; M. B. Hall, "Technology"). Not surprisingly, all the separate scientific roles, shared by the same set of individuals, became embued with similar norms and values. The shared value system of science was made possible by a common source of authority and a unified prestige system. Nobel Prize winners become editors and heads of labs and have their critical opinions taken most seriously. In fact the recognized quality of their work often leads to these other forms of authority long before prizes add worldly recognition and worldly authority to the previously established authority of empirically grounded research.

This unification of prestige, authority, and multiple roles in the single status of the scientist, however, presents the individual scientist with further role conflicts. Not only must the contributing scientist please a three-tiered audience, that same scientist when acting as reader, critic, editor, or referee must avoid irreparable breaches with those same individuals. In this situation conflicting role demands cannot be kept separate, as all the actors take all the roles. A contributor wanting findings to be accepted, but also having a critical role to fulfill, might hesitate alienating a significant potential reader or referee. Nor will the potential contributor accept without suspicion an editorial rejection that might be attributed to the editor's or referee's interests as potential contributors themselves. The possible conflicts and perceived violations are legion.

These conflicts become particularly omnipresent because the whole communicative system is based on conflict, a way of organizing the criticism that emerged with a public forum of communication. Critics are set against contributors, gatekeepers do make harsh choices, readers do select which material to build on and evaluate what they read—and the entire process brings the agonistic interactions into a form of public debate and discussion.

Very strong mediating devices are needed to hold this agonistic social structure together. Some of these devices are to create pockets of privacy within this rather public system. As we have seen, the editor was able to slough off some of the more sensitive conflictful choices to editorial boards and referees so that he might maintain good relations with con-

tributors and readers. Similarly, anonymity surrounds refereeing to allow for "objective judgment." But these devices have only limited power. An editor must take responsibility for journal policy, assignment of referees, and thus the content of the journal. Anonymity in refereeing, often only a transparent veil, can at best hide only personalities and not intellectual commitments. The much stronger conflict mediating devices lie in the distancing values of science.[11] Commitment to organized criticism, communalism, universalism, and objectivity allow individuals to absorb individual strains, conflicts, and violations in the name of the communal endeavor. In this way the overall status of scientist is more than just an umbrella for the many roles taken by the individual; it is a crucial identity adopted by the scientist that allows him or her to rise above the conflicts and strains within particular roles adopted as part of this overall identity.

This overall integration of values and identity does not mean that all individuals equally identify with all parts of the system. Neophytes necessarily have limited experience and socialization. Individuals become alienated or remain marginal for many reasons. Cynicism, manipulation of the system, and fraud may appeal to individuals on the margin or individuals who are expected to fulfill demands beyond their legitimate means. Whole groups and scientific communities may develop other structures as they respond to different social/political/ or belief pressures. These qualifications notwithstanding, the general thrust of the development of the communication system of science has been to structure science in much the terms described by Merton.

The Social Construction of Social Structure

Thus a constructivist analysis of the social structure of scientific communication, examining actors' situated strategic micro-

11. Rose Laub Coser, "Role Distance, Sociological Ambivalence, and Transitional Status Systems," specifies Goffman's concept of role distance (from *Encounters*) as arising either in situations of ambivalence resulting from conflicting role expectations made by a single role partner or from transitions to new roles. The conflict situation I have described as occurring in scientific communication has elements of both situations arising from the complex multiple interactions with single role partners and from the expectation of critical skepticism which keeps creating distance between role partners. In this situation, adherence to the more abstract norms of science and identification with the generalized status and goals of scientist create role distance helping to resolve conflicts and ambivalences.

choices, gives a picture of scientific structure consonant with more traditional macroanalyses.[12] This should hardly be surprising. What individuals who constructed the scientific community constructed was the scientific community.

Yet this inquiry has been more than tautological, for we have seen how the scientific community developed around the engendering and management of conflict. We have seen how the conflict-based interaction shaped the means of communication and its regularized channels. We have seen how the structuring of communication helped establish the role set of the scientist.[13] We have seen how norms of behavior and self-representation emerged out of the need to manage the conflicts and relieve the role tensions created by the structured activity of scientists. We have seen how commitments to a communal project beyond oneself help distance a scientist from personal strains and create the collectivity as a social fact.

Science, responding to its own dynamics and activities within its particular social circumstances winds up structured differently than other social systems, equally constructed out of their situations and activities, and developing their own appropriate symbolic systems. As a sociolinguistic system science has emerged through the socially contexted language choices of language users.

Finally, we gain an appreciation of how complex a social activity empiricism requires for its realization. It is not, as Swift's parody in *Gulliver's Travels* would have it, a group of men mutely gathering in a chamber and inarticulately pointing at one object and then another. Although perhaps some early members of the Royal Society might have had opinions not far removed from such parodies, the social realization of the empirical program soon pushed all participants to far more com-

12. Warren Handel, "Normative Expectations and the Emergence of Meaning as Solutions to Problems: Convergence of Structural and Interactionist Views," presents a similar analysis of the compatability of sociological frameworks by considering negotiated meanings as a means of resolving structured conflicts and thereby restructuring the perceived situation and the symbolic means of interaction. The protean restructuring of the sociolinguistic system embodied in scientific communication can best be seen in such a light. The evolving symbolic center of the interaction embodied in scientific texts constantly remakes social structure in ways that require renegotiation of what the scientific text should be.

13. Joseph Ben-David, *The Scientist's Role in Society*, also offers an account of the emergence of the role of scientist, but Ben-David's account concerns the broader social perception of what a scientist was, rather than what it meant to be a scientist within a scientific community. Ben-David provides an enlightening account of how the emergence of the public category of scientist shaped the possibilities of science in various periods. In this chapter, however, I have tried to provide an account of the emergence of the structured relations and activities of the scientist within the activity of science.

plex social behaviors. Yet this recognition of social complexity of human behaviors does not deny that the project is empiricist. Our contemporary Brobdingnagian microscopic examination of modern science need not convince us that it is a Grand Academy of Lagado, nor a petty world of Lilliput. The scientific community is what we have made of it.