CHAPTER 9

Scientific Writing as a Social Act: A Review of the Literature of the Sociology of Science

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It is generally recognized that writing is a social act – a communication between individuals and within groups; moreover, implicit in every writer's concern for audience is the realization that skilled writing relies on knowledge of the social context and intended social consequences of the writing. Yet sociological thought and knowledge have rarely been used to aid the study of writing, except for the politically important sociolinguistic studies of the difficulties particular dialect groups have in mastering the standard literate code [1–5].

Scientific and technical writing particularly lend themselves to sociological study because they serve limited functions within distinct communities. The discipline of sociology of science has, furthermore, done much to map out the structure of the scientific community and its activities. This essay will review the literature in the sociology of science to explore what light it may shed on scientific and technical writing and to see what questions it may raise for future study.

In its early years (1935 to 1960) the sociology of science was primarily concerned with the relations between society as a whole and the social institution of science: the effects of science on society, the social conditions under which science prospered, the attitudes with which society viewed science. Much of the seminal work was written by Robert Merton; his *Science, Technology, and Society in Seventeenth Century England* examines the interplay

among Puritan values, economic expansion, military needs, and the growth of science [6]. "Science and the Social Order" [7] and "Science and Technology in a Democratic Order" [8] explore in more general terms the social attitudes that aid or impede the development of science. Bernard Barber's *Science and the Social Order* follows in this tradition by placing the social institution of science against other social institutions, such as the political order, business, and the university, in order to provide a macrocosmic view of the social role of science [9]. DeGré similarly examines science as an institution within the wider society [10]. In more recent years such interests have been channeled into policy studies – recommending, evaluating, and analyzing government initiatives in science and examining the extent and nature of sex discrimination within science.

Just prior to 1960, however, sociologists began to look into the social structure of the scientific community itself. Research focused on issues such as the values of science and their enforcement; the rise, structure, and interaction of specialties; rewards and competition; and the relationship between cognitive structure and social structure in scientific fields. These and similar issues bear directly, as the following pages will attest, on our understanding of scientific communication. This review will proceed in two parts. The first section will examine the major models of scientific activity and community in order to define the role that communication takes in each. Each model points to different features and functions of scientific writing. The second part of the review will apply specific concepts and findings of the sociology of science to well known issues in the study of writing: the writing process, textual form, the dissemination process, and audience definition and response.

THEORIES OF SCIENTIFIC ACTIVITY WITH IMPLICATIONS FOR WRITING

Even the traditional view of scientific writing, stemming from Bacon [11] and fostered by the founding of the Royal Society [12], implies a social theory. According to Bacon, the social factors that inevitably impinge on the individual scientist and form the circumstances of his work are impediments to the search for objective truth and need to be eliminated through the procedures of scientific practice and communication. One must free one's mind and one's language from what Bacon calls the idols that inhabit them, so that reality may be impartially observed and accurately reported in a language that is transparent and unproblematic. Persuasion is extraneous, for any statement can be tested empirically. One should not write for a particular audience, but should rather attend to the objects under consideration, for all of humankind is capable of recognizing the truth clearly stated. The book of nature is open to all; by meticulously transcribing it the scientist is able to rise above the limitations of self and society.

The moment, however, that one thinks of language as something other than a transparent conveyor of accessible realities independent of the personality and consciousness of writer or reader, the sociology of the scientific paper becomes more complex. In "Is the Scientific Paper Fraudulent?" for instance, Medawar argues that the scientific paper distorts scientific thought because it does not at all represent the process of discovery [13]. Although the scientific paper gives the appearance that a scientist gathers objective observation until a conclusion rises inductively, Medawar argues that all observation is prejudiced and that discovery is distinctly different from proof, which is after-the-fact and deductive. Nor are the guesswork and groping that go into the formulation of a scientific idea represented in the crisp hypothesis, results, and conclusions of the formal paper. In comparing observations of laboratory work and published reports, Knorr and Knorr find that the formal report not only omits all the false leads and unsuccessful procedures but does not even discuss the factors that resulted in the choice of problem and the final set of procedures [14]; moreover, the report does not even provide enough information for another scientist to replicate the successful procedure. In addition, Toulmin has argued that, except in formal logic and mathematics, scientific writing does not follow the canons of formal deductive logic [15]. Finally, studies of diffusion, resistance, and evaluative judgment suggest that the claims of a scientific paper are accepted neither uniformly nor promptly [16–20], indicating that forces beyond the proof presented on the page act on the audience of the scientific community. If a scientific paper is not a complete account of a scientist's observations and doings, nor a tightly argued deductive proof of claims, nor an unproblematic conveyor of claims to be objectively evaluated fairly and promptly by a professional audience, what indeed is the scientific paper communicating, and to whom?

Contemporary accounts imply answers to these questions by examining the way scientific knowledge is created and modified through social processes. Ziman, for example, sees scientific knowledge as defined by the consensus of scientists [21]. Each new publication or statement is based on the existing consensus and strives to become accepted into that consensus. Ziman explains that dependence on prior consensus accounts for the heavy use of citation and a strong continuity of language from previous literature on the subject. On the other hand, impersonality of voice and technical language can be seen as attempts by the new contribution to appear as if it has already been accepted into the body of agreed-upon knowledge. The scientific paper makes the would-be contribution public, open to the evaluation and judgment of the scientist's peers. After a period of evaluation – and being ignored is as much part of the process of judgment as explicit critiques or citation in the review literature – the work may become actively accepted consensual knowledge, cited and used in future work in the area.

The communal evaluation of scientific publications is considered from another perspective in Popper's concept of objective knowledge [22]. By *objective knowledge* Popper does not mean the knowledge produced by impartial

scientists, for Popper recognizes that each scientist's subjectivity affects the claims he or she makes. Rather, Popper suggests that a scientific statement, once written, itself becomes an object upon which critical operations can be performed. Just as the spider web, once made by the spider, has an existence independent of the spider, so do scientific statements, once made, exist independent of the subjectivity of the maker. These statements, now objects in the world, can be examined according to the criterion of falsifiability, suggested by Popper in an earlier work [23]. That inspection by critical tests is crucial to the scientific endeavor. Because the body of all statements exists, for Popper, as something other than nature itself and other than the subjective consciousness of scientists, he calls the scientific literature the "third world," or knowledge that exists "without a knowing subject." The role Popper perceives for writing and publication in opening thought up for critical inspection beyond the circumstances of the statement's utterance is similar to the role literacy has in developing culture, as noted by the anthropologist Goody [24, 25] and the classicist Havelock [26, 27]. Similar also is Eisenstein's historical evaluation of the social and intellectual consequences of the invention of printing [28].

The continuous evaluation and reevaluation of statements by the scientific community, as Ziman suggests, lead to a changing consensus about what comprises accepted scientific knowledge. The state of that evolving consensus is important for what a scientist both says and sees, for in affecting the terms and manner of description, a change in the consensual understanding also affects the perception and conception of the phenomena being studied. As Hanson argues in *Patterns of Discovery* [29], all observations are theory laden, all perceptions are filtered through assumptions about the world. Thus each scientist's reports of observations must be understood as the product of explicit and implicit theory, the larger part of which is not the scientist's own invention, but which is received from the consensually shared knowledge of the time and the discipline. Lakatos, Toulmin, Kuhn, and Fleck each present models of how scientific statements are embedded within such received knowledge.

Lakatos suggests that a scientific community shares a research program consisting of methodological rules which define what is and is not valid and promising research [30]. A negative heuristic (the rules that point out what not to pursue) forms the "hard core" of the program, limiting the infinite possibilities for work into a coherent field with coordination of results and theory among members of the community. A positive heuristic gives the scientists guidance in wending their ways through the confusions and anomalies even within the limited field. The positive heuristic can evolve, resulting in a problem shift (or change of the scientific community's focus of attention) while still remaining within the research program defined by the negative heuristic. Indeed the vitality of a research program depends, according to Lakatos, on its ability to generate new problems for investigation. The research program will persist despite anomalies as long as the program keeps suggesting new research questions;

the anomaly itself may be considered as a new problem within the program. As inconsistencies add up, however, the program will either shift through attempts to rationalize anomalous findings or will wither as scientists chose to formulate their work according to more promising programs. Thus each scientific contribution is to be understood against the background of the existing research program, the problems the program proposes, and the evolution of the program in response to new findings. Consequently, in order to communicate the point and value of new work, the scientific writer would be well advised to understand how his or her new contribution fits within the continuity of the problems of the relevant research program. If Lakatos is right, adherence to accepted theory is not so necessary for an article's gaining acceptance as is adherence to the current research program. Even empirical data need to be sorted through the structure of the field's problems. Thus an article that expands a field's problems or redirects the research program is more consequential for the development of a science than the critical experiment that would presumably falsify one theory and verify a competing theory.

Toulmin proposes a Darwinian evolutionary model of knowledge [31]. Competing concepts proliferate; those best adapted to their time survive to be developed and modified in succeeding work, and those less well adapted fall into desuetude. At times one strong line of theory will come to dominate an area, but a change in conditions – whether intellectual, social, economic, or historical – may lead to a new proliferation of competing concepts. Science shares the foregoing with all branches of knowledge, but most scientific disciplines also fall into the more limited class of compact disciplines, characterized by five features.

1. The activities involved are organized around and directed towards a specific and realistic set of agreed collective ideals.
2. These collective ideals impose corresponding demands on all who commit themselves to the professional pursuit of the activities concerned.
3. The resulting discussions provide disciplinary loci for the production of "reasons," in the context of justificatory arguments whose function is to show how far procedural innovations measure up to these collective demands, and so improve the current repertory of concepts or techniques.
4. For this purpose, professional forums are developed, within which recognized "reason-producing" procedures are employed to justify the collective acceptance of novel procedures.
5. Finally, the same collective ideals determine the criteria of adequacy by appeal to which the arguments produced in support of those innovations are judged [31, p. 379].

Toulmin's view suggests that all knowledge-bearing documents, including scientific writing, should be understood within the conditions and goals of the period as well as against the competing contemporary claims. Further, a text

should be seen as only one articulation of an evolving concept struggling to survive. The writer of knowledge-bearing texts needs to be aware of the current climate of conceptual competition and evolution as well as the history of the concept at issue. Furthermore, in writing for scientific as well as other compact disciplines, one should understand the continuity between the work at hand and other work in the discipline. More concretely from the writer's point of view, the writer must know the problems of the field, the ideals and ethos of the field, the accepted justificatory arguments, the institutional structure in which the knowledge is to be communicated, and the criteria of adequacy by which the innovative work will be judged.

According to Kuhn, under conditions of "normal science," a scientist's work and statements are dominated by contemporary assumptions about what science is and how one does it [32]. To describe the myriad shared features that define the accepted science of a period, Kuhn used the term *paradigm*, but changed its meaning from a model or exemplar used to conceive of phenomena to the entire complex of shared habits rarely raised to the level of explicit rules. Masterman has pointed out that Kuhn used the term *paradigm* in at least twenty-one different ways in *The Structure of Scientific Revolutions* [33]; Kuhn has since proposed the alternative terms *disciplinary matrix* (to indicate that what a discipline shares is more complex and something other than adherence to a particular theory [34]) and *exemplar* (to indicate concrete problem-solutions that determine tacit preferences). Scientific writing, then, in periods of normal science must be seen as the manifestation of the many particular habits of the time, such as typical modes of perception and problem definition, common formulations, earlier models of problem solutions, and styles of speculation. The scientist writing within a disciplinary matrix at a time of normal science seems to follow very closely in the footsteps of his colleagues. Moreover, because the shared features of a disciplinary matrix often lie below conscious articulation, writing within each discipline can only be fully understood by those who share the matrix. Communication between participants in separate disciplinary matrices is rife with misunderstanding and unresolvable conflict unresolvable because there is no neutral terminology that will allow for determination of mutually acceptable criteria of adjudication. Thus, periods of revolutionary science, when no one view of what is proper science holds sway, are marked, according to Kuhn, by a breakdown in scientific communication, and scientists start to argue "like philosophers." [35] Such arguments are not resolved by evidence and one side's admission of defeat, but only by the emergence of a new generation of scientists with a marked preference for one of the matrices.

The consequences of Kuhn's theory for the nature of scientific writing in revolutionary periods are manifold. If Kuhn is correct, there should be clearly identifiable differences between the writing within two competing matrices. At the height of revolution, writing should take on a markedly argumentative, persuasive character. There should be clear evidence of miscommunication between members of the two matrices. The character of writing within a disciplinary matrix should also change as it loses or gains hold, either entering or leaving a period of revolution. Finally, if Kuhn is correct, a writer at a time of revolution would be wise to direct comments not so much at his opponents as at uncommitted third parties, such as young scientists entering the field; the argument should proselytize rather than attempt a definitive answer to the opposition.

Fleck's *Genesis and Development of a Scientific Fact*, first published in German in 1935 and obscured by the turmoil of the period until translated recently, anticipated many issues raised by current writers about the social influence on the context of scientific statements [36]. Fleck finds thought of any period dominated by a characteristic style emerging out of the contemporary "thought collective." The socially shared elements – or active elements, because they are the live carriers of the common culture – constrain what any scientist may find and determine the manner in which the scientist will express findings. However, in pursuing stylized intellectual work in accordance with the dictates of the thought collective, the scientist will run up against the resistances of empirical discoveries. Fleck calls these empirical resistances the passive elements of knowledge, because they are in a sense passively waiting for the scientist to chance into them. A passive element once discovered also becomes a constraint on scientific statement. A scientific fact is, indeed, the expression of such a passive resistance in the stylized terms actively determined by the contemporary thought collective. Fleck cites the example of the atomic weights of oxygen and hydrogen: no matter what one thinks hydrogen and oxygen to be and no matter what one perceives atomic weight to be (active elements in knowledge), once one assigns the atomic weight of sixteen to oxygen (also active), inevitably the atomic weight of hydrogen must be 1.008 (passively constrained). The passively determined ratio of the two weights is expressed as a fact in the stylized terms of modern chemistry. Until such empirical resistances are discovered, thought may be capricious, for the thought style may not be able to adjudicate among equally plausible claims, but a fact once discovered and expressed gives the scientist a solid point against which to fix an argument. The mark of modern science is its active pursuit of passive constraints, maximizing empirical experience to minimize thought caprice.

Fleck analyzes medical texts and diagrams from several different periods and cultures to illustrate how particular scientific statements may be viewed as the products of thought styles coming up against and contending with empirical resistances. Fleck's discussion suggests that a writer must rely on contemporary modes of statement while using new empirical experience as a heuristic for developing new forms of statement. Fleck also implicitly provides a method for reading scientific texts outside one's current thought collective, by distinguishing between the consequences of the thought style and those of the empirical resistances. Fleck's work, like Kuhn's, suggests the interesting program of identifying the particular features of discourse that characterize any particular thought collective. Finally, Fleck discusses the effect of popularization of science, noting how language, and consequently thought, becomes more concrete and definite as audiences outside the core scientific community are addressed.

These philosophers, Lakatos, Toulmin, Kuhn, and Fleck, suggest ways social processes shape the form and content of scientific statement. In contrast, a group of sociologists discuss the social structure and social mechanisms that allow and encourage the production of scientific statements. This group includes Merton [6, 37] and those who have followed his lead (including, among others, Cole and Cole [38], Cole [39], Gaston [40], Hagstrom [41], Storer [42], and Zuckerman [43]). Typical topics for Mertonian analysis include the value system (or ethos) of science, the extent and nature of deviance from that value system, the reward system and the importance of priority in the allocation of awards, the institutions of evaluation (e.g., organized skepticism and gate-keeping), social stratification and its function within the scientific community, and the accumulation of advantage (i.e., the process by which successful scientists gain the means to be even more successful). Many of these studies provide important insights into the context in which scientific writing takes place. Mertonians find that the chief reward of science is recognition for original and important work. In this claim we find a motive for persuasiveness in scientific papers, which must establish the priority and significance of the claims they present. However, other factors in the ethos and evaluative systems of science serve to restrain the desire for recognition and the tendency towards persuasion. Merton and his followers have turned to countable features of texts, particularly citations, in an attempt to gain quantitative indicators of social structure; their citation counts and other quantitative data provide substantial clues as to what actually is happening in the text, as will be discussed later.

Although Merton clearly recognizes that social issues inside and outside the scientific community affect the cognitive content of science, he is careful to keep a sharp distinction between social and cognitive aspects of science. On the other hand, some sociologists of science, particularly in Britain, are increasingly willing to see social issues dominating, if not explaining, the cognitive content of science. The differences between the British and American schools of sociology of science are discussed from the British perspective by Mulkay [44] and from the American perspective by Ben-David [45].

Taking the most radical epistemological position among the British sociologists of science is Barnes, who argues that a complete sociological account can and should be given of how scientific beliefs are developed and maintained, just like the complete sociological accounts of other belief systems [46]. The truth or falsity of a scientific claim should not affect the kind of sociological account needed to explain the claim. Barnes does not see the ethos of science as a mechanism encouraging the finding of truth [47]. He, indeed, accepts the full relativistic implications of a totally sociological explanation of scientific belief and claims that scientific knowledge has no more certain hold on truth than any other form of culturally determined knowledge [46]. Bloor takes the less radical position that although many scientific statements may be explained through logic and empirical testing, at crucial junctures logic and empiricism do not provide guidance, and crucial issues in dispute are settled by cultural preferences [48]. Drawing examples from mathematics and logic, Bloor demonstrates that major issues are answered by social negotiation rather than scientific reason. Least radical because they assume the truth of the findings of the field they investigate, Edge and Mulkay demonstrate in a detailed case study of the development of radioastronomy in Britain that the social context (both scientific and non-scientific) and the social structures that develop within a scientific field help shape the development, progress, direction, and knowledge of that field [49]. Elsewhere, Mulkay has criticized the view that scientific knowledge is privileged and has proposed a more thorough sociological approach to scientific knowledge [50].

What Bloor calls the "strong programme" in the sociology of science would lead to the view that social negotiation and the advancement of individual interests determine the nature of scientific communication. Working with this program and influenced by French phenomenology, some researchers have developed economic models of scientific activity in which the scientists shape the scientific paper to enhance its prospects in a market place that will assign it a value. Clearly the persuasion here is not tempered by ethos or critical evaluation. Among these researchers, Knorr considers the past literature as a set of scriptures, a source of cultural capital [51]; Latour and Woolgar [52], taking up the suggestion of Bachelard [53], consider even the equipment in the laboratory a reification of earlier literature, and consequently the result of social negotiation and a form of cultural capital.

SCIENTIFIC WRITING FROM A SOCIOLOGICAL PERSPECTIVE

The first half of this article has explored theoretical constructions of the social structure of science in order to suggest how a scientific paper might be conceived of in sociological terms. In all these theoretical constructions, no matter what their ultimate epistemological positions, the scientific statement is recognized as a social act within a social context. The remainder of this article will explore what the sociology of science tells us about the social context of scientific writing and the concrete influence of that context on the writing process and product. This exploration will focus on four main issues: the writing process, textual form, the dissemination process (including publication and audience structure), and audience response.

The Writing Process

In order to understand the process of scientific writing, we need to examine the relationship between writing and research activities. From one point of view, the two are coextensive.

The writing process may be said to begin long before the writer sets pen to paper; the moment one focuses attention to a topic with the hope that thinking and data-gathering will lead to a written statement, one starts to engage in activities that will shape what finally appears on the page. In the sciences this pre-writing stage is particularly long, while the actual writing-up stage is frequently very rapid. In *The Double Helix,* for example, Watson reports that almost two years were spent from the time he and Crick turned their attention to the structure of DNA until the time they were ready to write up their findings, while only little more than a week was needed for actual writing and revisions [54]. Pressures to establish priority lead to a great rush to print, cutting down the time available for writing up results, except in special circumstances (such as the case of Jodrell Bank, described by Edge and Mulkay [49]). Thus, to find the events, choices, and focusing of thought that shape the scientific paper, one must look to that long pre-writing stage of laboratory work.

Latour and Woolgar, working from observations of a biochemical laboratory, have suggested, in fact, that the entire laboratory activity is a process of inscription, gradually turning the materials under study into the words and symbols that appear in an article or other scientific communication [52]. They view the laboratory as a kind of factory; the raw materials of biological specimens, human power, electrical energy, the morning mail, and the like, are processed through experimental apparatus, equipment for extraction, labeling procedures, analytical machines, computers, and the word processing equipment of the front office in order to produce the marketable products called scientific communications which are shipped out in the afternoon mails. At each stage of this process of inscription, the product gets further and further from the raw object of study and more heavily encoded in symbolic languages. Latour and Woolgar are much concerned with the forgetting of the real object at each stage, but one could as well be concerned with what remains and how it is transformed to language at each stage.

Latour and Woolgar, following Garfinkel with modifications [55], also explore how in conversations scientists on a team construct tentative formulations of their subject in anticipation of what is likely to satisfy the colleagues who will be evaluating their work. Thus, laboratory conversation appears much like an early drafting or rehearsal procedure with the intent of creating a persuasive argument. Audience considerations appear to enter early and often into laboratory work.

Despite the close relationship between research activity and writing, Knorr emphasizes that the entire research process is not presented in the written

ɪroduct [51, 56]. Working from observations of research activities in a
echnological laboratory studying plant protein, she has found what she calls
process of constructive tinkering. With no fixed idea of what they are looking .
or to guide them in the design of experiments and observations, scientists are
ɪrone to tinker, that is, make a moment-by-moment "progressive selection of
/hat works through what has worked in the past and what is going to work
nder present, idiosyncratic circumstances." [51, p. 673] Out of this tinkering,
he scientist discovers an asset, something the scientist perceives as giving an
dvantage or handle in considering a problem; only then is the specific problem
o be addressed selected and defined. Then the scientist moves backward in
rder to "make the stuff work." [51, p. 677] Such assets include what catches
ne's eye as a striking new idea when read in a paper. However, in another
nalysis Knorr and Knorr find that virtually nothing of the laboratory process of
inkering is carried over into the final report of findings [14] : the report of
rocedures is so incomplete as to be useless in replication, and all the
nsuccessful probings and tinkerings are never mentioned. The asset or bright
lea is taken as the given rather than the product; the only other item carried
ver from laboratory to article is the statistical chart of data. The article is
onstructed on different grounds, to be discussed below.

Bazerman, on the other hand, working from first-person accounts of
ɔciological work, has found indications that formulations of research problems,
ypotheses, data, and interpretation are made throughout the research process;
ɔme earlier formulations, in fact, linger to become part of the final statement
57]. From first recognition of a research problem to the final report there are
ıany intermediary documents which, although not reaching closure on the
ɔlution, establish the terms of the problem under investigation, the procedures
) be followed and later reported on, the selection of data, and the preliminary
ɔnclusions.

If the sociology of science has only recently gotten into the laboratory to
otice the correlation between activities and final statement, the field has long
een interested in problem selection and the focusing of attention. These are,
ı effect, the writer's first choice: what topic shall I write on? The sociology of
:ience has considered how the choice of research topic is influenced by both
ıe socio-economic factors apparently external to science and the cognitive and
ɔcio-economic factors internal to science.

Merton's early and continued interest in how research priorities are
:tablished forms a basis for understanding the intellectual background that
ıapes problem choice [58–61]. Only with an appropriate shared framework
knowledge substantially developed through prior discoveries will it become
ident that a particular future discovery is conceivable, possible, and within
ach, so that a priority race may begin with several aspirants aiming to be the
rst to reach the well-defined unknown. For example, the well known priority
ce for the structure of DNA, as recounted by Judson [62], depended on

established theories of genetics, biochemistry, and molecular physics, as well as specific knowledge of proteins and X-ray diffraction techniques. Moreover, contemporary knowledge may so clearly point toward problems to be tackled and so fully provide ideas and information that may be pieces of the solution, that several scientists may come to the same or similar solutions at close to the same time, as in the case of Darwin and Wallace. According to Merton, multiple discoveries and near-simultaneous discoveries are frequent phenomena.

Zuckerman, in an extensive review of the literature on problem choice in science, explores in depth the types of cognitive assumptions that lead scientists to certain problems and away from others, including assumptions about theory, terminology, accepted scientific laws, and the riskiness of reputedly error-prone areas of investigation [63]. As a result, she says, scientific knowledge accumulates selectively. In an earlier study, for example, Zuckerman noted how greatly research had been constrained by the misnaming of bacteria as *schizomycetes* (i.e., reproducing only through asexual splitting) [64].

Other sociologists have given a variety of accounts of how problems are selected. Crane, in defining fashion in scientific problem selection, distinguishes between those cases where scientists flock to new problem areas for scientific reasons and those cases where social or economic factors influence the migration of attention; her analysis is tied to the formation of invisible colleges, to be discussed below [65]. Fell recounts the fashions, some of them recurring, in cell biology [66]. Stehr and Larson have found generational differences in areas of sociological specialization, indicating that the shared experience of each age cohort influences problem selection and distinguishes each cohort from all others [67]. Edge and Mulkay notice that the problem selection of radio-astronomers is influenced by, among other things, the equipment available and decisions about technological strategy, administrative styles of the research team, and the receptivity of different audiences [49]. Cozzens explores how reviews of the literature, in giving shape to the knowledge of a field, serve to identify problem areas for future investigation [68]. Gieryn discusses those considerations which would lead a scientist to continue with one line of research; he suggests that in general scientists shift attention to new problem areas only gradually [69]. Finally, Sullivan, White, and Barboni attribute the differences in problem selection they found among particle physicists of different nations to a kind of economic consideration based on potential recognition [70]: given current technology, knowledge and other resources, what significant findings is the team likely to achieve with priority? Similar economic calculations are discussed by Latour and Woolgar [52], Knorr and Knorr [14], and Knorr [51].

Because writing and research are part of the same overall process, which begins with attention and problem selection, the values (or ethos) governing the conduct of research have implications for writing. However, in applying the sociological concept of ethos to the production of scientific texts, we must remember that the sociological definition of ethos as a set of institutionally realized moral imperatives is not the same as the rhetorical definition of the term as the appearance of the author's character in the text as a persuasive element. There may well be a relationship between the two forms of ethos, but no simple correspondence can be assumed. In the seminal work on the topic, Merton finds a scientist's activity defined by four moral imperatives [8]: universalism (that knowledge claims and individual advancement be judged on impersonal cognitive criteria), communism (that knowledge be shared, even as recognition is given to the discoverer), disinterestedness (that conclusions be reached and advanced impartially, under threat of institutional sanction), and organized skepticism (that all claims be systematically judged according to current standards and knowledge of the field). Since this original formulation, other authors have suggested modifications [9, 41, 42, 71, 72], and some have challenged the basic conception by suggesting that deviance goes unpunished [73], that the norms exist only as after-the-fact justifications [45, 73], that the norms are irrelevant to the operation of science [45, 73], that each norm is balanced by a diametrically opposed counter-norm [74], that the norms do not differ from the norms of other fields [47, 75], and that the norms refer only to pure science and not applied [9, 42, 47]. Stehr reviews the history of the controversy and suggests a possible resolution [76]. Miller raises the issue of how the sociological concept of ethos might be applied to technological writing, but only after the scientific ethos is carefully distinguished from the technological [77].

Textual Form

The close relationship between writing activities and research activities suggests the value of a detailed analysis of scientific texts to determine how they function. However, serious study of the features of scientific texts has begun only recently. For a long time, texts were seen as the historical markers of discovery, as the method by which findings are made public for consensual evaluation, or as the measure of a scientist's career. The extent of textual inquiry was the common-sense questioning, like that by Weinberg [78], of the turgidity of scientific prose. Even though the theories of science as a social activity discussed earlier in this article had direct implications for the understanding of scientific texts, only Fleck was led to the detailed analysis of texts, in order to give substance to the concept of thought styles. He finds striking examples of how different patterns and habits of expression result in different theories and conclusions [36]. Because Fleck's book was unnoticed until recently, no tradition of textual studies has developed from his work, and it has been left to the recent observers of laboratory activity to be drawn to the analysis of texts through their bafflement about how these texts relate to laboratory practice.

As part of their observation of activities in a biochemical laboratory, Latour and Woolgar closely examined the scientific texts produced therein [52]. They see these texts as moves in a game. In the process of establishing credibility and

gaining credit, the tactical moves of a scientific paper help the scientist establish a position from which to continue the game. In particular, Latour and Woolgar consider two features of the texts. First, they characterize scientific statements as to how closely the statements appear tied to the circumstances of a particular laboratory, for such particularity makes the statement appear less fact-like and thus less credible. The statements which gain the most credit are those that seem to rise above the circumstances of their discovery in order to appear generally true. Latour and Woolgar analyze several texts to expose the strategies by which the scientists attempt to decrease the particularity of their own statements. Second, texts are examined to show how they change the rules of the game as they go. By introducing new criteria for credible work, an article is able simultaneously to discredit older work retrospectively, to promote the value of the work pursued by the article's authors, and raise the stakes for competitors so that certain researchers may be forced out or prevented from playing the game. Each paper is part of the evolution of a scientific specialty, with each contributor trying to redefine the game to his liking and favor. Latour and Woolgar analyze the research literature on thyrotropin releasing factor (TRF) in detail in order to show the tactics by which each research team has pressed its own version of what constitutes credible work.

Woolgar elsewhere analyzes a different kind of scientific text, the autobiographical account of discovery. In one study, he notes great variation in the discovery accounts of different participants in the same field [79]. After isolating some of the causes of the variation, Woolgar discusses insights into the discovery process revealed by the differences. In another study, he analyzes a single account closely to reveal the practical reasoning by which the scientist creates "a picture of the discovery process as a path-like sequence of logical steps toward the revelation of a hitherto unknown phenomenon." [80, p. 263] To do this the scientist must give the reader preliminary instructions on how the text is to be read and must employ externalizing, pathing, and sequencing devices.

Knorr and Knorr find that a text, rather than being an accurate summary of laboratory work, is a persuasive document intended to establish the value of the scientist's research within a particular market [14]. To do this the paper must first reconstruct the market, define the needs of the market, and identify the research being reported as the proper vehicle for the satisfaction of those needs. The paper must then fulfill the mandate it has constructed by demonstrating that its solution to the market needs was in fact achieved in the laboratory. There is no need for a complete and reproducible account of the work because detailed procedural instructions are communicated by personal contact, and methods will in any event be modified by later workers. The report need only present a plausible account of general events to establish that the solution has been realized.

Bazerman considers each text as mediating among four poles: the writer, the audience, the object under study, and the prior literature on the subject [81].

Features of the text can be explained in relation to one or more of these poles. In comparing texts from molecular biology, sociology, and literary studies, he finds various techniques by which the biological text subordinates its representation of writer, audience, and prior literature to its representation of the object under study. Although certain features of the scientific paper are directed toward persuasion of the audience, demonstration of the originality of the author's conclusions, and the reconstruction of prior literature, all these features are harnessed in the service of creating a symbolic representation of the object of study. The final criterion of all the features is the fit between object and language. In the sociological and literary texts other configurations of the four elements are achieved.

Qualitative citation studies shed light on the persuasive and argumentative uses scientists make of references and citations in advancing their own statements. Gilbert explores a variety of possibilities for strategic referencing, from displaying allegiances to borrowing capital from authoritative previous work [82]. Small investigates how in chemistry well known papers come to stand for specific concepts and procedures; reference to these works invokes a narrow meaning and stands in place of more complete explanation [83]. Moravcsik and Murugesan catalogue citations in theoretical high energy physics, finding, among other things, that about 40 per cent of references are perfunctory and that references tend to be affirming rather than negational by a 6:1 ratio [84]. Chubin and Moitra, using similar data from high energy physics, find varying citation patterns depending on the type of article or letter [85]. Spiegel-Rosing, in examining a sociological specialty, finds that the largest number of references are used to substantiate a statement or point to further information; only an almost miniscule number of references (0.4%) make negative evaluations [86]. Cole's taxonomy of citation types, also based on data from sociology, more fully explores the number of ways a reference can be used to substantiate a new argument – from use in the formulation of research problems to the interpretation of results [87].

The Dissemination Process

Once research activities have been completed and the written text has been given form, the social processes affecting scientific writing are far from over. The routes by which a scientific text reaches its readers are by no means straightforward; neither is the ultimate configuration of its audience, both inside and outside the author's specialty, nor are the mechanisms by which work will be evaluated. It isn't even evident whether the text will contribute to the overall evolution of science, and if so, how. Nonetheless, it is useful to consider how texts contribute to the continuing discourse of science, both for the interpreter of science and for the scientific writer trying to frame a statement that will wend its way through the intellectual labyrinth of the evaluation of his peers. Indeed every writing scientist must be an interpreter of the scientific literature, for the cumulative nature of science assumes that the new depends on the old. Even the old saw on the subject of intellectual debt, usually attributed to Newton, has a long and intricate history which Newton capitalized on, as Merton discusses in the amusing study *On the Shoulders of Giants* [88].

There are two major routes by which scientific knowledge is disseminated. The first is formal publication in print, which will be discussed later. The second, informal exchanges among scientists, is less publicly visible, but nonetheless important. As Menzel noted as early as 1959, scientists frequently gain important information through personal contacts, often in an unplanned, accidental manner [89]. Price then suggested that these informal communications among scientists actually form organized networks of researchers active in closely related areas; to describe this network phenomenon, Price revived a term from the time of the founding of the Royal Society – "invisible colleges." A study by Price and Beaver suggests that invisible colleges consist of the most productive workers closest to the research front surrounded by a floating membership of less productive workers [90]. Large groups of collaborators play a crucial role in communicating information, although non-collaborating scientists still have access to most of the information. Gaston documents the crucial role of informal communication in high energy physics because of the rapid change in the research front [40]; Gaston notes that older scientists seem to rely more heavily on informal communication than younger ones, and experimentalists more heavily than theorists. Other structural features of the invisible college in high energy physics are also described. Crane has provided an extensive study of the characteristics of information networks in science, noting among other things different styles and extents of participation in invisible colleges [17]. Griffith and Mullins note that invisible colleges tend to be more highly organized if the group is formulating a radical conceptual break with the rest of its field [91]. In addition to being associated with theoretical breaks, such highly organized networks tend to have acknowledged intellectual and organizational leaders, geographical centers, and a brief period of intense activity.

The most permanent and public method of disseminating scientific information is through formal publication in print. The following discussion of this method considers two issues: the referee system, which determines which texts will enjoy publication, and the configuration of the audience, which actually uses published texts. Generically, the referee system is a form of gatekeeping (whether the metaphor is drawn from St. Peter or Kafka depends on the studies you read). Zuckerman and Merton describe the history, rational function, and variations of the referee process throughout the sciences and humanities [92]; they note that even from the beginning of journal publication with the *Transactions of the Royal Society* there has been some attempt to control quality through the use of referees. Ben-David examines the role of national academies and other intellectual institutions in helping to establish standards of scientific work and in developing consensus on promising problems and methods [93]. Where national academies have developed strong methods of quality control, such as in Germany in the early nineteenth century, science flourished. Only when monopoly conditions later developed did German science lose ground to French. Zuckerman and Merton also find, as of 1967, a differential pattern in rejection rates depending on discipline [92]: in those fields with strong consensus as to what constitutes significant work competently produced (such as physics, geology, and linguistics), up to 80 per cent of the manuscripts are accepted for publication; in fields with low consensus, such as history, language and literature, and philosophy, up to 90 per cent of manuscripts are rejected. Also, in different fields there seem to be different editorial policies: in high rejection fields editors state they would rather run the risk of overlooking some good work than of publishing inferior work; in high acceptance fields, where shared standards are likely to prevail, editors are more willing to publish borderline work.

There is some disagreement about whether the review process is biased. Zuckerman and Merton's examination of the archives of *Physical Review* revealed that no identifiable bias appeared in the review process [92]. Crane, on the other hand, does notice a correlation between prestige of referees and prestige of authors in journals in sociology and economics [94]. Crane interprets this correlation as a result of similarities in training rather than the influence of personal ties. Abramowitz, Gomes, and Abramowitz evaluated the effect of cognitive bias on psychologists acting as referees [95]. The psychologists were asked to evaluate an empirical study contrasting the psychological well-being of student political activitists and non-activitists; all versions of the paper were identical, except that in half the copies references to activists and non-activists were switched in the findings and discussion sections. When asked to evaluate specific features of manuscript quality such as methodology and writing, the referees exhibited no significant bias; but when asked to consider the statistical analyses, conclusions, overall manuscript quality, and publishability, the referees exhibited strong bias in favor of the article that supported their own political leanings.

The referee process used in the evaluation of grant proposals submitted to the National Science Foundation has also been the subject of conflicting studies. Mitroff and Chubin review the debate [96], focusing on the conflicting studies of Hensler [97], who finds significant biases in the peer review process, and Cole, Rubin, and Cole [98], who find the process on the whole fair.

A number of studies of stratification in science suggest some of the possible influences in the review process. One cause may be the accumulation of advantage. The seminal work on this subject is Merton's discussion of the Matthew Effect [99], named after the passage in the Gospel of Matthew which describes how "unto every one that hath shall be given, and he shall have abundance." In a more recent essay, Merton gives an anecdotal account of how

accumulation of advantage has worked in the career of Thomas Kuhn [100]. Allison and Stewart have also found statistical evidence of accumulation of advantage in several fields [101]. The most comprehensive study by Cole and Cole finds that the most rewarded and recognized scientists are indeed those who have contributed the most, that those physicists who have gained most prestige are in a position to receive more communication and thus are able to proceed in their own work in a more informed and efficient manner, that important papers are recognized quickly no matter what the status of the author, and that almost all significant work gains recognition over a period of several years, although the middle-range work of more prestigious authors is likely to gain more rapid initial recognition than equal work of less well known colleagues [38]. Finally, Cole and Cole find no obvious signs of significant ethnic or sex discrimination. In a more recent study, Cole does locate a number of points where sex discrimination enters science [39]. Other major studies on stratification include Zuckerman's study of Nobel Prize winners [43], Zuckerman's consideration of the function of stratification [102], Zuckerman and Merton's study of the role of age in the structure of the scientific community [103], Mulkay's analysis of the role of the scientific elite [104], and a recent article by Hargens, Mullins, and Hecht which suggests that the differing structures of research areas affect the role and extent of stratification [105].

Although the gatekeeping system prevents many texts from getting published, the number of those that are published seems to grow exponentially. Several authors, including Price [106], Crane [17], Storer [42], Weinberg [78], Ziman [21], and the Committee on Scientific and Technical Communication of the National Academy of Science [107], have discussed the effects and proposed solutions to the so-called knowledge explosion. One obvious effect is that scientists must select the texts they read; they cannot read them all. Their collective decisions determine the configuration of the audience for a published scientific text.

Citation studies, some of which are discussed below, suggest that most scientists attend to a limited set of articles that tend to correspond to the structure of their specialty and their network of professional and personal contacts. The implications of who reads and cites whose research are large. Consider the case of radioastronomy, as documented by Edge and Mulkay [47]. Radioastronomy developed out of military radar groups during World War II. After the war two major research groups were established in Britain at Jodrell Bank and Cambridge to investigate celestial phenomena the military groups had incidentally noted. Early publications were in journals of technical radio engineering. The work of radioastronomers was virtually ignored by optical astronomers, nor did the radioastronomers attend much to the more long-standing literature of optical astronomy. It was as if there were two heavens – the radio and the visual – which had nothing to do with each other. The single xception of some early interchange over meteors occurred only because at the me optical astronomers generally left observations of meteors to amateurs. ecause of the lack of interchange, the application of radio techniques to stronomical questions was for a time stunted and radioastronomers spent ıbstantial time rediscovering things long known to optical astronomers. loreover, even within the radioastronomy community, each group seemed to roceed on different tracks, paying greater attention to its own findings, with lf-citation rates being particularly high. Storer has discussed the difficulties ıat lead to the low degree of transfer of information among different scientific sciplines [108].

Studies of citations (which papers refer to which other papers) and co-citations vhich two papers are repeatedly cited together in third documents) can aid in scribing the configuration of the audiences that uses scientific texts. The ırlier statistical work by Price observes that in tightly structured fields a large ımber of references are to a limited number of very recent articles, which seem represent a research front [106, 109]. In different fields there are different nounts of scatter of citations and different citation half-lives for articles. A ore recent study by White, Sullivan, and Barboni explores the interdependence theory and experiment at a time of revolutionary change (the discovery of rity violation) in the physics of weak interactions [110]. Co-citation studies, ch as those by Small [111]; Small and Griffith [112]; Griffith, Small, onehill, and Dey [113]; and Garfield, Malin, and Small [114] have begun to ıp out how the social structure of specialties changes in the wake of blications reporting discoveries that reshape the field. They have measured mmunications within and between a wide number of specialties, both those at are rapidly changing and those that are more slow moving. Through -citation mapping they have been able to create graphic representations of the ucture of specialties over time. Moreover, a study by Mullins, Hargens, Hecht, d Kick [115] shows that the networks of communication revealed by -citation closely resemble networks revealed by other measures of interaction, ch as personal contact and awareness of each other's work. A recent article Lenoir has suggested using co-citation clustering in conjunction with ckmodeling techniques to explore further the relationship between specialty ucture as revealed in print and as revealed in personal contact and awareness 16]. Most of the citation and co-citation studies have been made possible by data of the Institute for Scientific Information, publishers of the *Science ation Index, Social Science Citation Index,* and *Humanities Citation Index.* e prefaces to these indexes by editor Garfield frequently point out features the social structure revealed by the citation data; these prefaces have been lected [117].

The communication patterns within technological fields differ from patterns the sciences. An early article by Marquis and Allen finds that for many sons, including obligation to an employer, technologists are less likely to rely

on and disseminate findings through public print sources [118]. Price further explores the "papyrophobic" character of technology to discover ways in which diffusion of technology and the interaction between science and technology can be increased [119]; he recommends rapid turnover and transfer of personnel so that young technologists will bring the newest scientific findings and the older technologists will carry their expertise with them to the new work site. Ben-David, on the other hand, believes that the way to increase technological use of scientific findings is to encourage entrepreneurial opportunities for technologists, who are likely to know best which scientific knowledge is most applicable [120]. The Price essay discussed above is part of a volume called *Factors in the Transfer of Technology,* edited by Gruber and Marquis [119]; it also includes an article by Toulmin, extending his ideas on scientific evolution [31] to show how scientific findings and innovative techniques are diffused to technology; and one by Allen, generally showing that technological information is transferred poorly, that literature is not used as a primary channel of communication, that translation problems are common in transferring information across informational boundaries, and that the better performing groups rely on the resources of their own laboratories. Allen's comprehensive study on the subject, *Managing the Flow of Technology*, focuses largely on communications within a single organization, considering such factors as organizational structure and architecture; although the limited and troublesome roles of literature and communication among organizations are fully documented, all his recommendations concern internal organization [121].

Audience Response

When a scientific text has reached an audience, through direct or circuitous channels of dissemination, the audience has the opportunity to respond in several ways. Until recently, studies of reception have been limited to cases where there has been initial resistance to or rejection of ideas later accepted [16–20], the implicit assumption being that other instances of acceptance and rejection were based on rational judgments and further experimental evidence. Latour and Woolgar's investigations into the microprocesses through which judgments are developed and expressed, however, suggest that there is much to be learned about how readers, particularly scientific readers, form judgments about their reading, both upon immediate reading and upon long-term development of beliefs about their specialty [52]. In more general terms, Gilbert speculates about the process by which judgments regarding research findings are rendered [122]: when claims are accepted, they serve as models on which to base new research, thereby becoming temporarily adopted as scientific knowledge. At this moment it remains unclear how much social negotiation and reconstruction of the literature – in the manner described in Berger and Luckmann' *The Social Construction of Reality* [123] – actually takes place in science.

A number of historical studies have examined how the formulation or claim of one scientific text can emerge to dominate a field for some time. In his book on the Copernican Revolution, Kuhn emphasizes the collapse of prior beliefs through the accretion of anomalies and the emergence of the new theory out of a period of confusion [124]. Elkana notes the confusion in terminology that prevented the discovery of conservation of energy until appropriate terminology lent precision to the concept of energy [125]. Cole observes the role of the theory in producing puzzles to be solved [87], in legitimating and interpreting empirical work, and in generating further theoretical innovations; a theory that proves useful in all these ways may outlast anomalous empirical findings. Crane finds that the main factors affecting how a theory was received in theoretical high energy physics were its breadth and testability – how many kinds of observations and with what level of testable predictiveness the theory covered [126].

One feature of acceptance first noticed by Merton is that although accepted claims are at first explicitly recognized through citation, as the claim grows older it is no longer explicitly referred to, but rather is implicitly incorporated into the argument of other scientific texts, becoming an assumption rather than a specific source [127]. Messeri has examined this process of obliteration by incorporation by using case material from the acceptance into standard geological knowledge of plate tectonics and sea-floor spreading [128].

One final case study should remind us that however we conceive scientific writing, scientific texts, and the processes of dissemination and reception, our conception must always be grounded in an understanding of the contemporary social and intellectual conditions that surround any act of statement making. Mendel has long been cited as a legendary example of a scientist whose work was ignored because it did not fit the scientific orthodoxies of his time. Brannigan, however, now finds that Mendel was far from originally ignored or rejected in the 1860's [129]. His work was recognized and well cited by his contemporaries as a substantial, although far from revolutionary, contribution to the field of hybridization. Moreover, from Mendel's comments, his limited publishing ambitions for the work, and his satisfaction with the reception, it appears that the scientific reception matched his own estimation of his work. Only later, in a turn-of-the-century debate on evolution, was Mendel reinterpreted as making a major contribution to the theory of genetics. Only in retrospect, in light of this new interpretation, did Mendel's contribution appear to be ignored.

CONCLUSION

The consequences of any scientific paper for our understanding and control of nature, for future work in science, and even for our retrospective reconstruction of the knowledge and history of a discipline are the result of complex social

processes we have barely begun to explore. In the same way, how a scientific text emerges from a social web of human motivations, intentions, and actions holds many mysteries. We can see that intentions and consequences meet through the printed text, but until we sort out the web of social action that surrounds the text, we cannot know fully what the piece of scientific writing is or does.

Postscript

After this review was written, but before it went to press, several essays relevant to scientific discourse appeared. Knorr-Cetina has brought together in a book length essay her theories on the constructed nature of scientific statements and knowledge [130]. Garfinkel, Lynch, and Livingston have closely analyzed the talk identifying an astronomical discovery [131]; Morrison has examined "telling-order designs" in texts of didactic inquiry [132]; and Yearley has analyzed the persuasive elements in an early nineteenth century geological text [133]. Bazerman has discussed the problems arising from political science's attempt to institute an idealized version of the scientific paper [134] and has examined the forces and choices shaping some articles by the physicist A. H. Compton [135]. Gilbert and Mulkay have released the first results of their research program examining discourse practices in a biochemical research area [136–141]; and Mulkay has argued for the importance of discourse studies for an understanding of science [142].

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