The Role of the Scientific Paper in Science Information Systems

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Abstract

Studies of scholarly communication among scientists agree that the journal article is peripheral to research-front science. This conclusion derives from a cognitivist, science-as-knowledge model that construes scientific work as a conceptual activity in which information plays a central role. But according to recent studies of scientific practices, scientific work is not primarily conceptual but instead consists in stabilizing complex networks of heterogenous elements. These studies suggest that discursive elements and practices, such as writing and using journal articles, contribute to the stability of networks. Thus the importance of the article to research-front science need not consist in its role in the communication of information. An especially important analytical category of studies of scientific practices is that of an *objectifying resource.* This paper argues that the journal article is central to research-front science because it is among science's objectifying resources. The argument proceeds by exploring the implications for science information systems of three historical analyses of science: Sir Francis Bacon's model of state-organized science; Robert K. Merton's analysis of journals as systems of credit and reward; and Robert Boyle's literary technology for warranting scientific facts. Some conclusions of recent studies of the relationships between contemporary laboratory practices and the discursive practices resulting in the production of the journal article strengthen the argument.

Introduction

Ever since J. D. Bernal's controversial proposal to the Royal Society's Scientific Information Conference of 1948 for central distribution of scientific papers (Royal Society, 1948), the scientific journal article has occupied a precarious position in studies of science information systems. Journals have been called "the most important source and medium of scientific information" (Mikhailov, Chernyi, & Giliarevski, 1984, p. 198). Indeed, "scientific documents are a form of science. Without them, science cannot exist" (p. 147). Yet the exponential growth of scientific documents, rather than signifying a corresponding growth in scientific communication, signals to some the collapse and ruin of the entire system. In a typical assessment, written almost a decade before his Royal Society study, Bernal lamented of "the chaos of scientific publication" that "the burden of this vast mass of [journal publication] is in itself a great handicap to scientific research" (1939, pp. 118, 119). Even worse, the significance of the journal article has been challenged apart from its rapid proliferation. Restating a commonplace observation, Robert Hayes concluded that "natural scientists are focused on the acquisition of new data rather than the analysis of existing records. For them, the records of the past are peripheral to research" (1992, p. 6). Price's index (Price, 1970; reprinted in Price, 1986) gave mathematical expression to a recurrent conclusion of studies of science information systems that the archive of "the records of the past" is consulted with such a low frequency that other, and most typically, informal channels of scientific communication are regarded as central to research-front work.

This paper argues that the paradox of a document form both essential and marginal to science arises from a cognitivist conception of science, which Andrew Pickering calls the science-as-knowledge model. It interprets science as a unified conceptual field whose structure is determined by scientific method:

> For the logical empiricist, say, scientific culture consists in a field of knowledge and knowledge claims, and scientific practice consists in the appraisal of conceptual knowledge claims against observational knowledge, an appraisal ideally governed by some logic or method. (1992, p. 3, note 2)

In such a model "scientists figure as disembodied intellects making knowledge in a field of facts and observations" (Pickering, 1995, p. 6). It has been criticized as an "algorithmical model" of knowledge, one "very much in accord with the view of the information scientist," who "views knowledge as the sort of information that enables a computer to carry out its programmer's intentions" (Collins, 1992, p. 75). When knowledge is seen as "a set of formal instructions, or pieces of 'information' " (p. 57), the salient activity of science becomes *information processing*. Scientific method becomes the program that generates propositions from scientific information. Insofar as the production of scientific knowledge is communal and cumulative, information must be communicated among scientists. Documents enter the picture as vehicles for the communication of information, which is interpreted as the epistemic content of the documents' statements. The problem for the study of science information systems is to analyze traffic flow in epistemic content, or "information," in order to maximize knowledge production.

Given this analytical framework, the role of the journal article becomes paradoxical. One response to the paradox is to accept the science-as-knowledge model, but to locate the article's importance in a social system of credit and reward. This response is problematic because it uncouples the labor of scientific writing from laboratory work and knowledge production. Another response is to reject the model, locating the article at the center of scientific labor, but at the expense of the centrality of information flow to scientific work. This paper argues, by three historical examples, for the second response. first, the historical antecedents of the science-as-knowledge view are briefly indicated in Sir Francis Bacon's model of the communal scientific enterprise. Second, the mid-twentieth century response in terms of credit and reward is located in the work of Robert K. Merton. finally, it is argued that the concept of an objectifying resource, which is central to contemporary studies of scientific practices, suggests a more plausible analysis of the journal article's role in scientific work. The historical roots of this concept reach back to Boyle's contribution to the development of a literary technology that helps transform local laboratory results into phenomena of a shared, objective world.

Documents and Information in Baconian Science

Bacon's model of science, emerging from the fragments of his projected *Instauratio magna*, the "great instauration," or renewal, of the sciences in the early seventeenth century, anticipates some important aspects of modern notions of science information systems. Although the origin of the scientific journal is usually dated some forty years after Bacon's death in 1626, he anticipated science's literary technology by placing a system of written records at the heart of knowledge production.

In his "Plan of the Great Instauration" Bacon (1960, p. 17) insists that its first part must be a "Division of the Sciences." He describes this as "a summary or general description of the knowledge which the human race at present possesses" (1960, p. 17). In other words the Division of the Sciences is to be a written record of what is currently known. Its most fundamental division reflects "the absolute chasm which exists between the truths given in revelation through the Word of God and axioms discovered by the powers of man and, secondly, through distinctions among the human faculties" (Anderson, 1948, pp. 148–149).[1] The main classes corresponding to the three main human faculties—memory, imagination, and reason—are history, poetry, and philosophy. History is divided into natural and civil. Natural history records and organizes the phenomena of nature. It is a classification of the epistemic content of records, which, once written out according to strict rules designed to purge them of anything other than what may be derived through observation and experiment, provides the basis for inductive generalizations. In Anderson's gloss natural history is a "delineation of the sort of experimental history which is suitable for the building of a philosophy" (Anderson, 1948, p. 259). Because "it provides the materials on which the understanding is to operate" (p. 260), this part of the classification is so necessary "to the Instauration that, if it cannot be provided, the scheme cannot become operative and the whole project for the reform of knowledge may as well be given up" (p. 259).

In a statement clearly expressing his science-as-knowledge approach, Bacon says of his Division of the Sciences: "However, I take into account not only things

[1]The details of Bacon's classification must be pieced together from several writings, since the Division of the Sciences was never completed. Bacon offered in its place a Latin, reworked version of his much earlier *Advancement of Learning* (1605), titled *De dignitate et augmentis scientiarum*. For an account of the texts and the details of Bacon's classification, see chapters 13 and 14 of Anderson (1948).

already invented and known, but likewise things omitted which ought to be there" (1960, p. 18). His classification can include things "which ought to be there" because its principles do not derive from literary or cultural warrant, but from the structure of knowledge itself. Its logical foundation means that it is hospitable to the inferences drawn from existing records according to Bacon's proposals for reasoning correctly and generating higher-order conclusions from "things already invented and known." Since the progress of thought is from natural history to natural philosophy, the classification's hospitality to the things "which ought to be there" creates a class for records in the third part of the Great Instauration, "The Phenomena of the Universe, or a Natural and Experimental History of the Foundation of Philosophy."

The relationship between Bacon's classification and its literature inverts that of nineteenth- and twentieth-century systems.[2] In Bacon's system a classificatory position does not derive its warrant from the literature but from the organization of the natural world as represented by scientific knowledge. Because new information is generated from previous information by scientific reason, new scientific records have a position already guaranteed for them in the classification of documents, corresponding to the position of their epistemic content in the organization of knowledge.

In *New Atlantis*, Bacon's utopian fable of state-organized natural science, the knowledge of the natural world is produced only through a highly structured social system. The "things already invented and known" are collected for inclusion in the Division of the Sciences principally from books and other written records. This document collection activity is divided among several different ranks of scientific worker: "Merchants of Light" (those who sail to distant lands to collect and make reports of experiments), "Depredators" (those collecting local experimental reports), and "Mystery-Men" (those collecting reports of the mechanical arts). The information gathered and recorded by workers in these first three divisions of scientific labor is then processed by those in the three following divisions. The "Compilers" re-present previous experiments in "titles and tables," displaying observations perspicuously, thus allowing axioms to be more easily drawn from them. Reasoning from the records of the Compilers, and consulting with other scientific colleagues, the "Lamps" suggest new experiments to advance knowledge by building upon current and previous work. The experiments are performed by the "Inoculators," whose reports are then submitted to the "Interpreters of Nature." The Interpreters generate higher-order axioms to guide further observations. Finally, the "Dowry-Men," or "Benefactors," apply the knowledge gained to useful inventions.

Bacon's description of the scientific enterprise as a set of collaborative, socially organized activities of gathering, producing, processing, classifying, and applying written records constitutes what we would today call a science information system. He recognized that science does not develop merely by thought, experiment, and observation, but requires a literature.[3] For him, a scientific record contributes a unit of scientific knowledge. Knowledge does not advance merely by an increment in the number of its constituent units, but by the organization of the units through the inferences and generalities—axioms—drawn from them, such that new observations can be made and further experiments devised. The proper classification of recorded units of knowledge is not merely heuristic, allowing higher-order axioms to be drawn from them; it is also representational. The differentia of the subclasses of natural history and natural philosophy are categories that represent the structure of the natural world. Categories of this kind achieve the goal of the classification, which is to facilitate generalizations that increase our knowledge of nature.[4]

What would the structure of scientific literature look like, given Bacon's view of the scientific enterprise? Journal articles would be organized by a classification system reflecting the levels of generality of the axioms derived from the experimentally generated observations reported in them. The structure is hierarchical, culminating in a set of articles containing high-level generalizations. The organization of documents mirrors the structure of knowledge, because the imperatives of document classification derive from the inductive inferences holding between classes of recorded information.

Bacon's model of scientific activity resists many of the reductivist tendencies of science-as-knowledge models. For Bacon, science is the product of much more than merely cognitive activities. He emphasizes the social organization of collective labor, strict rules for writing

[2]For a brief but illuminating recent discussion of nineteenth- and twentieth-century classifications, see Miksa (1998).

[3]The importance of documents, their collection, organization, and the social structure required for the production of knowledge from them, is emphasized in Martin's study of the relationships between Bacon's view of science and his design for the British imperial state (Martin, 1992).

[4]See Anderson's gloss on the categories of the natural philosophy class (1948, pp. 154–156).

scientific documents, and applications for the production of machines, instruments, and other technological devices. Yet the organizing principle of scientific activity derives from a science-as-knowledge model. Scientific documents communicate information, in the form of observations, that provides raw material for new results. Since observations support inductive generalizations only as members of a class of observations, they must be combined with other observations. Since many of these observations derive from the literature, the epistemic content—we would say, the "information"—conveyed by documents is as directly implicated in the production of new results as the information generated through experimental work. Science is a conceptual field, a systematic organization of information and the propositions derived from them. It consists of immaterial, conceptual entities: information in the form of observations, concepts, and propositions. The essentials of Bacon's model are threefold: 1) Information is identified with the epistemic content of documents; 2) document classification mirrors the classification of information, and both are based upon the organization of knowledge; and 3) the communication of scientific information is achieved by the system of scientific document production, organization, and use. In this model classifiers map and mirror the structure of knowledge. They labor alongside experimental scientists as coworkers in the production of knowledge.

Cognitive Contamination: Merton's Norms

Although Bacon's science information system implicates a complex social organization responsible for the production, organization, and circulation of documents, it is governed by the cognitive imperatives of science-as-knowledge models. But for Merton, the father of the sociology of science, the structure of scientific knowledge is not sufficient to regulate a science information system. He sees science as a social order whose cohesiveness, stability, and systematic advance depend not only on the epistemic value of scientific information but also on shared values based on adherence to specific norms. Since Merton's norms are treated in detail in a voluminous literature,[5] his original four are simply listed here: organized skepticism (scientists are expected to evaluate new knowledge critically and objectively); disinterestedness (their findings are not expected to be used in a self-interested fashion; they are expected to maintain an attitude of emotional neutrality toward their work); universalism (scientific merit should be evaluated independently from the personal or social qualities of the individual scientist); and communalism (since scientists do not own their findings, secrecy is forbidden, and open communication is prescribed).

Since Merton's social norms—what he called "the ethos of science"—build moral imperatives into the heart of scientific activity, knowledge production comes to depend on more than adherence to cognitive and technical standards. The scientist not only follows rigorous methodological precepts, such as those Bacon took great pains to elaborate, but also works at "fashioning his scientific conscience" by cultivating an ethos, "that affectively toned complex of values and norms which is held to be binding on the man of science" (Merton, 1973b, pp. 268–269). However, Merton also recognized that since abiding by the ethos of science is not its own reward, scientists need to be acknowledged for observing the norms. Whereas Bacon's system fails to build rewards into the social structure of science, Merton's system embeds reward in science's information system. The most important kind of reward for scientific work is "eponymy, the practice of affixing the name of the scientist to all or part of what he has found." The most highly prized rewards are therefore titles, such as the Copernican system, Hooke's law, and Halley's comet, but

> The large majority of scientists, like the large majority of artists, writers, doctors, bankers and bookkeepers, have little prospect of great and decisive originality. For most of us artisans of research, getting things into print becomes a symbolic equivalent to making a significant discovery. (Merton, 1973c, p. 316)

Not all publications, however, have equal value, since, as Merton points out, "for a published work to become a genuine contribution to science, it must, of course, be visible enough to be utilized by others" (1973a, p. 332). Yet for the great majority of scientists, mere publication becomes the chief form of eponymous recognition and reward because the mechanism of publication—the referee system—is an "institutionalized pattern of evaluation."[6] Since journal referees bestow or withhold the imprimatur of science, they administer one of science's most important reward systems. Referees are

[5]For a short and accessible introduction to the "scientific ethos debate," see Toren (1983); a useful list of references may also be found in Bazerman (1983, p. 168).

[6]Garvey also emphasizes the reward inherent in mere publication, through the "use of journal articles as the primary source to establish

"an example of status-judges who are charged with evaluating the quality of role-performance in a social system . . . Status judges are integral to any system of social control through their evaluation of role-performance and their allocation of rewards for that performance" (Merton & Zuckerman, 1973, p. 461).

If science's social system is structured by normative standards, then how is compliance with its cognitive and epistemic standards to be guaranteed? Merton's answer is that abiding by the ethos of science advances knowledge because the norms flow from scientific method:

> The institutional goal of science is the extension of certified knowledge. The technical methods employed toward this end provide the relevant definition of knowledge: empirically confirmed and logically consistent statements of regularities (which are, in effect, predictions). *The institutional imperatives (mores) derive from the goal and the methods* [emphasis added]. The entire structure of technical and moral norms implements the final objective. The technical norm of empirical evidence, adequate and reliable, is a prerequisite for sustained true prediction; the technical norm of logical consistency, a prerequisite for systematic and valid prediction. The mores of science possess a methodologic rationale but they are binding, not only because they are procedurally efficient, but because they are believed right and good. They are moral *as well as* technical prescriptions. (Merton, 1973b, p. 270)

If Merton's norms are connected as he claims with science's cognitive and technical imperatives, then insofar as rewards are distributed through science's formal information system, the Mertonian model places great stress on the epistemic value of the scientific journal article. Reward through publication recognizes work of epistemic value only if journal articles reflect scientific work accurately and are used to further scientific knowledge. If they do neither, then the reward system becomes uncoupled from epistemically valuable activity. Merton often expresses his agreement with both points by assuming that formal publications are used directly in knowledge production:[7]

> The system of monitoring scientific work before it enters into the archives of science means that much of the time scientists can build upon the work of others with a degree of warranted confidence. It is in this sense that the structure of authority in science, in which the referee system occupies a central place, provides an institutional basis for the comparative reliability and cumulation of knowledge. (Merton & Zuckerman, 1973, p. 495)

If journal articles have the epistemic value Merton assumes, then his model is compatible with Bacon's. Given the imperfections of mortals, a system for distributing rewards for submitting to the discipline of scientific method is needed. A nice solution is Merton's: Embed the reward system in science's formal information system. With the norms in effect the circulation of journal articles not only communicates the information required for the performance of advanced scientific work, as Bacon's model requires, but also distributes rewards to information of genuine epistemic value, thereby satisfying Merton's model.

The problem, however, is that the epistemic value of a reward system embedded in the formal channels of science's information system is held hostage to the question of whether journal articles contribute information used directly in the derivation of new results. Yet studies of scholarly communication in science show that they only rarely convey the information required for research-front work. The possibility that Merton's ethos of science can become unhinged from the epistemic value of the information conveyed by the communication system in which it is embedded therefore introduces a destabilizing element into his analysis. Rewards through publication institutionalize the norms of science only if publications actually convey the information used in the derivation of new knowledge. Otherwise, the reward system floats free, as it were, from epistemic value. The system may continue to function even if journal articles are used only by referees or status-judges to bestow the reward of publication upon articles whose value as information for deriving new knowledge has dropped to

priority" (1979, p. 75): "In almost every scientific discipline today, the socially accepted medium for establishing priority is the scientific journal article" (p. 69).

[7]The "information-recognition exchange model of scientific organization" developed by Hagstrom (1965), Merton's student, according to which the "organization of science consists of an exchange of social recognition for information" also exhibits the same dependency of the integration of moral and cognitive value on the logical role of the information exchanged for social recognition. When manuscripts are given as "gifts," in Hagstrom's analysis (pp. 12–23), in exchange for the social recognition granted through publication, information of epistemic value to scientific knowledge is thereby rewarded only if the gift *is* information, that is, if it is logically related to the derivation of new knowledge. Hagstrom makes this explicit in his comments on another form of recognition operating through the formal channels of scientific communication, the practice of citing the publications of others: "It is usually necessary, even obligatory, for them to recognize previous work, *for the validity of their own contributions depends logically on the earlier work*" (p. 24; emphasis added).

zero.[8] Thus Garvey, for example, can assert, without violating the Mertonian project, that the information communicated in journal articles is not useful at the research front where new knowledge is generated; yet its mere publication constitutes reward by establishing priority and ownership.[9]

A strength, albeit unintended, of Merton's model is its capacity to explain the importance of documents largely useless in the production of new scientific knowledge.[10] But this explanatory power is gained only at the expense of the communicative and informational value of the overt content of the scientific journal article, which played such a central role for Bacon. The destabilizing element of Merton's analysis relocates the informational value of the article in a latent content, which helps position its author in a social hierarchy of status and prestige.

The Journal Article as an Objectifying Resource

Science-as-knowledge models privilege the role of information because they emphasize such activities as data generation and processing, constructing and testing hypotheses, and theory building. They marginalize the role of the journal article in science information systems because studies of scientific communication show that articles are not the source of the information required for the production of research-front knowledge. Furthermore, content analyses of journal articles show that they do not represent the process of scientific discovery, but present after-the-fact proof, omitting false leads, unsuccessful efforts, and the factors resulting in both the choice of problem and the final set of procedures. Not only do they typically fail to provide enough information for the replication of successful procedures, but also the very possibility of replication has been challenged (Collins, 1992).[11] These problems are nicely condensed in Bazerman's question:

> 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? (1983, p. 158)[12]

Contemporary studies of scientific practices have rejected science-as-knowledge models. Pickering (1992, p. 6), for example, asks whether "analytic repertoires developed in the service of a problematic of knowledge can serve as the primary basis for understandings of practice." He concludes that "most scholars who have taken it as their task to get to grips with scientific practice in some detail have found that they cannot." Modeling science as a conceptual field "does not offer much purchase upon the complexities evident in the nearest laboratory" (p. 5). Studies of scientific practices emphasize instead the "patchiness," or the "motley" of science rather than conceptual homogeneity or unity: "Scientific culture is made up of all sorts of bits and pieces—material, social, and conceptual—that stand in no necessary relation to one another" (p. 8). The varieties of scientific practices and the complexities of scientific culture bring into sharp relief the false assumption of science as a unified, conceptual field: "Scientific culture is *disunified, multiple, and heterogeneous*" (Pickering, 1995, p. 70). From this view the goal of scientific work is not the pro-

[8]In an exchange between Harnad and Fuller on electronic journal publishing, Hanad argues that the "esoteric" literature, i.e., scholarly journal articles, has no market: "Esoteric serial publishers will learn that their real clients are esoteric authors (actually, their institutions and granting agencies) rather than readers" (Harnad, 1995b, p. 311); the "captive audience" of the journals "is not the readership of the journals, it is the institutional library that must have the entire journal in hand for the few, if any, who ever consult any particular article" (p. 317). In his response to Harnad (1995a), Fuller also emphasizes the noncognitive function of the journal literature: "The communication of results, the allocation of credit, and the creation of an archive all reflect the *publicity* function of journals" (Fuller, 1995a, p. 300). For the final word on the exchange, see Fuller (1995b).

[9]Garvey notes that his studies with N. Lin and K. Tomita "raise some questions about the function of current journal articles: Can the journal article any longer be regarded as a vehicle which effectively communicates current scientific information? If not, can the journal article be reworked to function more efficiently in the capacity of integrating scientific information into a larger framework?" (1979, pp. 223–224.) These are the same questions posed by Bernal forty years earlier.

[10]Even this strength may be challenged. If scientists use the results of others before they get into print, then their knowledge of priority and ownership does not depend upon journal publication.

[11]Hacking notes that experiments do not generally replicate previous work in order to refute theoretical conjectures: "Folklore says that experiments must be repeatable . . . roughly speaking, no one ever repeats an experiment" (1983, p. 231).

[12]Bazerman's paper is a useful introduction to some of the pre-1980 literature on the sociology of science and its implications for scientific and technical writing. Bazerman is one of the few who have made the role and function of the scientific paper a distinct research topic (1988). See also Knorr-Cetina (1981); for actor-network approaches to scientific writing, see Callon, Law, and Rip (1986); Latour and Woolgar (1986); and Latour (1987).

duction of a conceptual field but the stability of networks consisting of many heterogeneous elements.

Bazerman (1994, p. 118) has suggested extending the idea of networks, or the "notion of system . . . to include all kinds of symbolic representations, relationships, practices, and objects that must be brought into alliance for any technology or scientific knowledge to take hold." If the notion of system is extended in the way he suggests, then such discursive elements as the journal article belong to the "motley" of the natural sciences. The problem of their role in scientific activity then shifts to their contribution to stabilizing networks. Alternatives to information must be found among the concepts that explicate this role. Chief among them is the concept of an objectifying resource, which suggests that the article aids stability by its contribution to the construction of the objectivity of the natural world.

The historical origin of the article as one of the most important *discursive* objectifying resources of science may be traced to Boyle's contribution to the "literary technology" devised in the seventeenth century for reporting scientific results. According to Steven Shapin and Simon Schaffer (1985, p. 76), it was one of the "three technologies . . . involved in the production and validation of matters of fact: material, literary, and social." Boyle recognized the importance of discipline in the development of the experimental report as a particular literary form. Among its important rhetorical features were "virtual witnessing" and a moral posture of "modesty." To borrow a term from Donna Haraway, "modest witness" is an apt name for this literary style.[13]

Virtual witnessing was the literary equivalent of the careful staging of scientific experiments. Once a pertinent phenomenon was produced in the laboratory, it was reproduced before a highly select group of witnesses. Such demonstrations "were a routine feature of the meetings of the Royal Society, and a *Register-Book* was provided for witnesses to testify their assent to experimental results" (Shapin, 1996, p. 107). Such direct witnessing, although important to the constitution of matters of fact, was a limited way of propagating a new and highly disciplined form of experience that was to legitimate scientific assent.[14] Boyle therefore sought to multiply witnesses through "the production in a *reader's* mind of such an image of an experimental scene as obviates the necessity for either direct witness or replication" (p. 60). In order to achieve such virtual witnessing, a specific literary technology had to be devised, "a technology of trust and assurance that the things had been done and done in the way claimed" (p. 60). Boyle realized that if "one wrote experimental reports in the correct way, the reader could take on trust that these things happened. Further, it would be as if that reader had been present at the proceedings. He would be recruited as a witness and be put in a position where he could *evaluate experimental phenomena as matters of fact*" (p. 63; emphasis added). For Boyle, this took the form of a literary style characterized by an "ornate sentence structure, with appositive clauses piled on top of each other," in order "to convey circumstantial details and to give the impression of verisimilitude" (p. 63). This ornate, rather than succinct, style was required to present simultaneously, in one snapshot, as it were, all the details required for virtual witnessing. "Elaborate sentences, with circumstantial details encompassed within the confines of one grammatical entity, might mimic that immediacy and simultaneity of experience afforded by pictorial representations" (p. 64).

Neither direct nor virtual witnessing were forthright presentations of the highly localized and contingent laboratory circumstances that contemporary studies of scientific practices have revealed as typical elements of scientific work. Boyle's "circumstantial style" was designed as the prose version of the *staged* experimental scene, one already purged of the local context, contingencies, situatedness, and opportunistic reasoning involved in the actual production of the laboratory phenomenon. Thus the experimental report, carefully designed for virtual witnessing, is the discursive correlate of a theatrical strategy of objectivity. Since the point of the literary technology is to substitute for replication and direct witnessing, its circumstantial details must be as routinized and standardized as those of the staged event. The experimental report is written to present an objective phenomenon of the natural world, not a local phenomenon arising from the kind of contingencies

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[13]Haraway's use of this expression is ironic, as it serves to articulate her criticism of the gender blindness of Shapin and Schaffer's work (Haraway, 1996).

[14]The extent of this limitation is evident in the following observation: "For practical reasons alone the number of direct witnesses for experimental performances was always limited: in Boyle's laboratory that public probably consisted of at most three to six competent colleagues, and audiences for Royal Society trials rarely exceeded twenty and were typically much smaller" (Shapin, 1996, p. 107).

encountered by modern ethnographers who study laboratory work.[15]

The second important rhetorical feature of Boyle's literary technology is "the modesty of experimental narrative":

> It was the burden of Boyle's literary technology to assure his readers that he was such a man as should be believed. He therefore had to find the means to make visible in the text the tokens of a man of good faith. . . . Thus the literary display of a certain sort of morality was a technique in the making of matters of fact. A man whose narratives could be credited as mirrors of reality was a *modest man*; his reports ought to make that modesty visible. (Shapin, 1996, p. 65)

To strike a posture of modesty through scientific writing consists, first, in eschewing grand, natural philosophical systems in favor of the piecemeal work characteristic of the scientific journeyman satisfied with the limited goals of experimental reports. "Those who wrote entire systems were identified as 'confident' individuals, whose ambition extended beyond what was proper or possible. By contrast, those who wrote experimental essays were 'sober and modest men,' 'diligent and judicious' philosophers, who did not 'assert more than they can prove' " (p. 65). And proof in experimental matters required that all traces of personal style be purged from the writing so that the facts could appear to speak for themselves. Thus a

> technique for showing modesty was Boyle's professedly "naked way of writing." He would eschew a "florid" style; his object was to write "rather in a philosophical than a rhetorical strain." This plain, ascetic, unadorned (yet convoluted) style was identified as *functional*. It served to display, once more, the philosopher's dedication to community service rather than to his personal reputation. (p. 66)

To pursue similarities between contemporary scientific writing and Boyle's literary technology of virtual witnessing is not to suggest that the experimental report has not changed since the seventeenth century. But even today grand schemes, typically published in books, have a lower epistemic status than journal articles. Furthermore, the decontextualized style of the Methods and Materials and the Results and Discussion sections of the contemporary journal article also consists in a flat, unadorned, recitation of events (Knorr-Cetina, 1981). Perhaps the most striking similarity between Boyle's and contemporary presentations of facts with sufficient stability, as he put it, to "make their own way," is the inscription of a discursive opposition between matters of fact and the speculations in which they are embedded. In Boyle's literary technology, there "were to be appropriate moral postures, and appropriate modes of speech, for epistemological items on either side of the important boundary that separated matters of fact from the locutions used to account for them: theories, hypotheses, speculations, and the like" (Shapin, 1996, pp. 66–67). For matters of fact "a confident mode was not only permissible but necessary" (p. 67). As for the experimental report's proper style for venturing speculations or hypotheses, or what Boyle calls "opinions," here is Boyle's advice to his nephew:

> In almost every one of the following essays I . . . speak so doubtingly, and use so often, *perhaps, it seems, it is not improbable*, and other such expressions, as argue a diffidence of the truth of the opinions I incline to, and that I should be so shy of laying down principles, and sometimes of so much as venturing at explications. (p. 67)

Boyle's distinction between a confident style for statements of fact and a hesitating style for speculative and interpretive statements is mirrored in the contemporary journal article's contrast between the interpretive problem setting of its introduction and the plain speaking of the methods and materials section and the reluctance to draw conclusions in the results and discussion (Knorr-Cetina, 1981). In both early modern and contemporary writing facts must be discursively stabilized. For Boyle the "separation of moral modes of speech and the ability of facts to make their own way were made visible on the printed page" (Shapin & Schaffer, 1985, p. 67). His " 'naked way of writing,' his professions and displays of humility, and his exhibition of theoretical innocence all complemented each other in the establishment and protection of matters of fact" (p. 69). Remarking on the rhetoric of the contemporary journal article, Knorr-Cetina notes that it "is well suited to the stereotyped image of science as presenting the 'facts' which others may use in making decisions" (1981, p. 123).

[15]On the reporting of circumstantial detail, Shapin and Schaffer write: "It is, however, vital to keep in mind that in his circumstantial accounts Boyle proffered only a *selection* of possible contingencies. There was not, nor can there be, any such thing as a report that notes *all* circumstances that might affect an experiment. Circumstantial, *or stylized*, accounts do not, therefore, exist as pure forms but as publicly acknowledged moves towards or away from the reporting of contingencies" (pp. 64–65; emphasis added).

In describing Boyle's objectifying project, Shapin and Schaffer ask: "If the obligation to assent to items of knowledge was not to come from human coercion, where did it come from?" The answer is the same today as it was in the seventeenth century:

> It was to be nature, not man, that enforced assent. One was to believe, and say one believed, in matters of fact because they reflected the structure of natural reality. . . . *Yet the transposition onto nature of experimental knowledge depended upon the routinization of these technologies and conventions.* (p. 79; emphasis added)

The continuity from Boyle's day to our own of the literary style of the "modest witness" is one of the most telling emblems of the necessity of such routinization. Facts must be inscribed in scientific writing so that they can "make their own way":

> The matter of fact can serve as the foundation of knowledge and secure assent insofar as it is not regarded as man-made. Each of Boyle's three technologies worked to achieve the appearance of matters of fact as *given* items. That is to say, each technology functioned as an *objectifying resource.* (p. 77)

Since the seventeenth century, "the objectivity of the experimental matter of fact [has been] an artifact of certain forms of discourse" (Shapin & Schaffer, 1985, pp. 77–78). Whatever other changes the experimental report has undergone in almost 350 years, many similarities attest to "modest witness" as an enduring literary style of scientific writing. Science's literary technology continues to construct the reader as a witness to a world of facts and phenomena of a natural world.

Contemporary studies of scientific writing show that the natural world represented in the journal article is not the same as the world of the laboratory. Knorr-Cetina shows how the scientific paper functions as an objectifying resource through its discursive construction of an alternate world. She calls this transformation of laboratory work through writing a "conversion of reason":

> We have observed a conversion into another currency, a transmutation into the totality of another language game. This conversion was itself a process. It started long before the paper was written, through the production of measurement data and other written traces of laboratory work, and continued with the collective enterprise through which these traces became caught, identified, and finally preserved within the double-threaded web of argumentation that distinguishes the finished paper. (Knorr-Cetina, 1981, p. 131)

The transformation is *from* the localized, contingent, opportunistic, highly situated, analogical, and practical reasoning governing laboratory resource selection, *to* the abstract, decontextualized space of the scientific paper. The transformation is at the same time a recontextualization, relocating possible decisions and possible conclusions on a stage of facts which "make their own way" in an objective, natural world, purged of all traces of human intervention.[16] The contingencies of actual scientific labor are transformed into an abstract, cognitive space, in which information from previous work and information produced in the laboratory are processed according to the rules of scientific rationality, thereby producing new information contributing to science's collective project of faithfully representing an objective, natural world.

Given the disequivalence between laboratory reason and its discursive reconstruction in the scientific paper, "the link between the laboratory and the scientific paper cannot be established by rules of cognitive transformation. The scientists who write a manuscript do not recall the research process and then proceed to summarize their recollections" (Knorr-Cetina, 1981, p. 130). Thus the paper is not a vehicle for the communication of information. Instead, the paper is a particular *discursive resource*, different from the laboratory's material setups, but no less an outcome of scientific labor. The erasure of particularity, situation, locality, and contingency represents the discursive fulfillment of its objectifying function. Since the paper stages a witnessing, not of the actual laboratory but instead of the "facts" of an objective, natural world, its witnessing is virtual in a double sense. Not only are the witnesses absent from the scene, but the scene itself is a discursive construct.

[16]Knorr-Cetina elaborates on this recontextualization: "In the transition from laboratory work to the scientific paper, the reality of the laboratory changed. We have seen the situationally contingent, opportunistic logic of research replaced by a generalized context of present and possible worlds, and the interest negotiations of particular agents transformed into a projected fusion of interests of technology, industry, the environment and a human population needing protein. We have seen the reasoned selectivity of laboratory work overruled by formulaic recitations of the doings which emerged from this selectivity, and the measured results of these doings purged of all traces of interdependency with their constructive creation. We have seen the indeterminacy of the laboratory reduced to the careful expression of scientific doubt which the paper allows" (1981, pp. 130–131).

The scientific paper's virtual witnessing stages a *simulacrum as witnessed,* through the process of which it becomes a phenomenon of the natural world. Insofar as knowledge of an experiment comes to depend upon its reconstruction by the scientific journal article, the particularity of the real laboratory situation is forever erased:

> The instrumental mode of production which results in laboratory measurements involves an almost total decontextualization, relieved only by the rationales found in the scientists' written notes. The literary mode of production which results in a published paper offers a recontextualization, but as we have seen, not one which brings back the memory of laboratory work. The transition is, at the same time, a conversion of the written traces themselves. Except in the memory of those who were present during the process, *it is an irreversible transition.* (p. 130; emphasis added)

The transformation of laboratory reasoning found in the scientific paper is typical of scientific resource conversion. Scientific work aims at launching resources used in other research contexts. The continuity of scientific practices does not arise from the logical coherence of an information space whose *telos* is the completeness of its representation of a natural world, but from the labor of resource conversion among scientific fields and resource extensions to transscientific fields. When the complex hybrids of the laboratory are taken up by others and used as resources in their own projects, "they undergo a recontextualisation and reconstruction similar to what we found in the writing of the paper" (Knorr-Cetina, 1981, p. 132).

Although the resource conversions of the journal article are characteristic of scientific products generally, there are also important differences. A literary technology's products are *discursive* objectifying resources. To standardize and routinize discursive decontextualizations and recontextualizations through a discipline of scientific writing creates the objectifying resources for the discursive construction of objectivity. In other words science's literary technology creates resources for the articulation of objectivity, nature, scientific truth, and scientific knowledge. Formal writing is crucial to establishing the documentary techniques for the institutionally authorized enunciation of scientific truth. Studies of scientific practices therefore imply that the journal article is central to such practices, not because it conveys information but because of the centrality of objectifying resources to the cultural phenomenon we know as natural science.

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