Scientometrics, Vol. 1. No: (1978) 65–84

STUDIES IN SCIENTIFIC COLLABORATION

PART I. THE PROFESSIONAL ORIGINS OF SCIENTIFIC CO-AUTHORSHIP

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(Received January 3, 1978)

From a historical and sociological perspective, this essay presents and develops the first comprehensive theory of scientific collaboration: collaborative scientific research, formally acknowledged by co-authorships of scientific papers, originated, developed, and continues to be practiced as a response to the professionalization of science. Following an overview of the origins and early history of collaboration in the 17th and 18th centuries, a study of the first professionalized scientific community, that of Napoleonic France, confirms that, as the theory predicts, collaboration is a typical research style associated with professionalization. In the early 19th century, virtually all joint research was performed by French scientists; collaborative research only appeared much later in England and Germany when they, too, underwent professionalization. That historical finding, which constitutes a puzzling anomaly for any other view of scientific teamwork, here conforms to theoretical expectation. Several other predictions of the theory are presented, to be taken up in subsequent studies.

Introduction

Although collaborative research is an important characteristic of the contemporary organization of scientific inquiry, no one has yet viewed available data on its incidence, patterns, functions, and value within a comprehensive framework. In this and subsequent essays we will develop such a framework, by demonstrating that scientific collaboration represents a response to the professionalization of science. Moreover, we will show that, because scientific teamwork did not originate in the twentieth century, a sociological analysis of collaboration must begin with its past history. An examination of the historical roots of co-authorship, particularly in the early nineteenth century French scientific community, will provide a necessary background for research in this field.[1]

Current studies on scientific collaboration can be divided into three categories: those concerned with its statistics, its quality, and its social character. Each characteristically and implicitly emphasizes the recency of collaboration by focusing on the twentieth century, especially the post-World War II period. Statistical accounts are concerned primarily with demonstrating that the percentage of research produced by teamwork, albeit highly variable according to scientific field, exhibits a monotonic increase for the past four or five decades.[2] Studies of quality, less concerned with statistics, but equally impressed by teamwork's apparent recency and its rate of growth, attempt to evaluate its significance for science. Such accounts are strongly polarized between those which see teamwork as bane and those which see it as boon. Both views are stereotypical, reflecting different assumptions about the nature of scientific activity and discovery.[3] A few discussions, however, transcend the limitations of statistical and polemical accounts, and attempt to gain balance and insight by analysing and explaining collaboration in terms of more general theories in sociology or the sociology of science. These represent the closest approach to a general treatment of teamwork.[4] However, even these treatments prove to be far more descriptive than analytic; they remain inadequate to the task of providing a comprehensive picture of collaboration.

Collaboration: a response to professionalization

While scientists are usually thought of as members of a community and their membership in the communities and subcommunities of science has been explored on this basis, few people have analyzed the *professional* nature of the scientific community and its consequences.[5] We use the word "professional" here with some apprehension, for the word is static and can only be used to describe the product of the *process* of professionalization at a specific time. "Professionalization" refers to a dynamic organizational process which led to a revolutionary restructuring of what had been a loose group of amateur and full-time scientists into a scientific community. "Professionalization" redefined how science was done, who did it, where it was done, who paid for it, what its practitioners wanted, and how they became scientists.[6] It continues to affect structural changes within the scientific community as well as the community's relationships with the outside society.

Thus professionalization can best be viewed as a process which organizes a group of individuals along a set of attributes – attributes which are both inclusive and exclusive. That is, professionalization defines the rules, rights, and rites of access to the group, what holds the members of the group together, and what sets them apart from other individuals in the larger society. Furthermore, professionalization structures the obligations and benefits of the group's members while defining their relationships with outsiders.[7]

The basis for the process of professionalization of science lies in the scientific community's ability to lay claim to support from the outside society and the society's ability to provide it. Such claims are based, in turn, on the capability of certain members of the society to set themselves apart as a group by setting various internally-sanctioned and -controlled signs and symbols (for example, Nobel prizes, awards, society memberships, academic degrees) and more importantly, by avowing their possession of a unique and special body of knowledge. When viewed by the larger society as esoteric but useful, a body of knowledge and the rights to its exploitation may serve to make the specialized community autonomous – if, first, the specialized knowledge insures the profession's claims to societal support, and, second, the authority for deciding how this support will be distributed remains within the profession. Historically, the process of professionalization has simultaneously structured – and thus institutionalized – both societal support and internal mechanisms for its allocation and use.[8]

Continued reliance on support from the outside society implies the need, periodically, to justify such support, as well as to distribute the proceeds within the scientific community. Thus the community develops and presents to the larger society a unified and cohesive picture of its work and indicates the benefits that society can expect to derive from their association. Internally, the scientific community deals with certain problems arising from the distribution of social support by setting standards for performance and training; it also defines "scientifically valid" goals and problems – and their solutions.[9]

Internally, also, the encouragement of continued social support requires that some members of the scientific community act as representatives to the larger society. Such positions are usually filled by the "acknowledged winners" in the competition of research (in both revolutionary and normal times) or their intellectual progeny. By virtue of their role as spokesmen, these scientists not only receive societal support themselves, but they also become responsible for its reallocation within the community. Moreover, as the "acknowledged leaders" of the scientific community, they become the foci for the allocation of its own intellectual resources. This part of the scientific community can be seen as the elite.[10]

The foundation of the National Academy of Sciences in the United States in 1863, illustrates how the relationship between a professionalizing scientific community and the outside society may, with increasing societal support, lead to increased centralization of authority and control within the scientific community.

> Behind this seemingly innocent legislation (creating the Academy) was Alexander Dallas Bache and his devoted band, the Lazzaroni. Since at least 1851 they had worked for an organization of the leading scientific savants- a select group in marked contrast to the American Association for the Advancement of Science, which was open to all friends of science, whatever their actual attainments in research . . . The Lazzaroni were concerned with standards of professional competence in order to squeeze out and to put in their proper place those they considered inadequate as scientists. The Lazzaroni also sought a relationship with the government that would provide funds for science. Their beloved chief, Alexander Dallas Bache, naturally thought of federal funds since he was the best example of how a scientist might use such funds.[11]

Evidence of BACHE's authority, as seen in the following letter to J. F. FRAZER, also makes it clear that professional recognition leads to more than status within the profession: It is the route to power.[12]

> I have been obliged to admit in reply that there are some men too mean to bring into our Academy thus slightly intimating that I so class Geo. P. Bond and Spencer F. Baird. I have had favorable opportunities for inductions upon them in parts of their lives, and have come to distinct conclusions. The venerable Smithson would I think have had both in and R. E. Rogers out. That nomination seems to be generally looked upon with disfavor . . .[13]

Thus recognition is not merely a passive phenomenon. During both revolutionary and normal periods of scientific growth, it is a rite of passage which confers the right to recognize others: it represents a source of power in the scientific community.[14]

To sum up the process of which recognition is a part, professionalization gives coherence to the nature of the scientific community: externally, by making other members of the outside society incompetent to judge its internal problems (social as well as theoretical) and internally, by necessitating the selection of representatives to the outside society. The financial benefits derived from this increased coherence – and the coherence itself–lead to increases in the degree of stratification and domination within the community.[15]

Not all scientists can be responsible for the allocation of funds, nor can all scientists edit journals, act as referees, or teach at major universities. Some scientists become more important than others. This is not to say that in science there is a single hierarchy, or even a single pre-eminent community, for communities in science exist at numerous levels[16] and the stratification system is unstable. Changes in scientific paradigms bring with them changes in the degree of domination of scientific communities both with respect to each other and with respect to their members. But the situation stabilizes once a normal period in science has been reached.[17]

Within this system of stratification and domination, collaboration becomes a mechanism for both *gaining* and *sustaining* access to recognition in the professional community. Collaboration provides a means of demonstrating one's ability to those already in a position to "recognize" others as well as keeping up one's output from such a position. Thus collaboration acts as a social regulator: it provides possible avenues of mobility for those who seek recognition; it also maintains and solidifies recognition for those who have received it.[18]

To put it briefly, partially through the increased access it provides to sources of information, support, and facilities, collaboration increases both visibility to the elite and productivity. It allows the choice of eponymity or anonymity, depending on the quality of research. The division of labor makes it difficult for others within the profession to assess responsibility; the ensuing tendency is to give credit, but to withhold blame. Meanwhile, collaborators derive benefits from their increased visibility through increased access to the intellectual resources of the community. This may lead, in turn, to more productivity, additional recognition, and professional advancement.

Just as collaboration becomes a means of advancement in the professional hierarchy of science, so it simultaneously advances research. Thus it affords both personal and mundane satisfactions, as well as those provided by "science for science's sake."

Advantages of the hypothesis

Other treatments of collaboration attempt to delineate various patterns and modes of teamwork – describing and classifying the motives which lead researchers both to form and to terminate associations, distinguishing the relative roles and contributions made by team members – and to relate their findings to a number of socio-theoretical concepts. But even the best of these reduce to a descriptive list, in bewildering variety, of the motives which seem to underlie scientific collaboration. The following is a summary of such motives.

Although distinctions between formal and informal collaboration supply a limited preliminary schema, such distinctions fail either to comprehend or to provide a theoretical framework for the multiplicity of explanations advanced above.[19]

The most frequently advanced explanation for teamwork attributes it to the specialization of science.[20] According to this thesis under such conditions scientific vision becomes ever more specialized and narrow, and collaboration thus becomes necessary when scientists deal with problems which cross disciplinary bounds. Because

Table 1
Motives for collaboration: a summary

access: to special equipment of facilities
to special skills
to unique materials (e.g. chemical compounds)
to visibility
recognition

efficiency in: use of time
use of labor

to gain experience

to train researchers

to sponsor a protege

to increase productivity

to multiply proficiencies (thereby increasing access to sources of support, visibility, recognition)

to avoid competition (thereby forestalling loss of priority, i.e., recognition)

to surmount intellectual isolation

need for additional confirmation of evaluation of a problem

need for stimulation of cross-fertilization

spatial propinquity

accident (serendipity)

to the historically blind, both specialization and collaboration appear to be of rather recent origin, they tend to use a false correlation from current history to support a plausible but only partially valid argument.

Despite its clear association with some of the motives for teamwork, specialization is simply too limited a concept to serve as a general explanation. One of the puzzles that it leaves unsolved is the large variation in the incidence of collaboration by field. For example, chemists and biomedical researchers virtually always work in teams, but mathematicians rarely do.[21] We may explain some – but not all – of the difference by referring to the fact that experimentalists tend to collaborate more than theoreticians. However, using specialization to explain collaboration in this instance requires a belief that, compared to chemistry, mathematics is relatively unspecialized. This is not the case.

Complementary to the specialization hypothesis are those hypotheses which depict collaborative relationships as advantageous for accumulating and advancing scientific knowledge. This view usually characterizes collaborative work as a fit and rational organizational means for advancing science for science's sake. But that assumes that the scientist would enter collaboration altruistically, sacrificing his individuality to the advancement of science, and assessing teamwork as the appropriate means toward this end.[22] Occasionally, some studies of collaboration do mention personal motivations and satisfactions, but they remain subordinate and incidental.[23] Given the immense importance ascribed to recognition, to the eponymic property rights in scientific discovery, and the historical frequency of polemical disputes over priority, one would not expect scientists to opt unselfishly for anonymity over eponymity.[24]

A final misleading characteristic of the literature on collaboration is its implicit emphasis on the recency of collaboration. The lack of historical perspective is so great that most published statistics indicate that collaboration only began in the twentieth century. Moreover, the Anglo-American tradition of research characterizes its past predominantly in terms of the "lone wolf", the great individual, or the fathers or heroes of science. That historical past, a commonly-held heritage derived from the introductory pages of scientific textbooks, constitutes another predisposition to consider collaboration as a quite recent mode of research in the scientific community. Those who hold this view discount as exceptions the well-known collaborations of KEPLER and BRAHE, of HOOKE and BOYLE, of LAVOISIER and LAPLACE, of DULONG and PETIT, and of GAUSS and WEBER. Because in this framework, collaboration represents an anomaly in scientific research, widespread acknowledgement of its practice can only be a recent development. In fact, as we will show, collaborative investigation traces its roots to the birth of modern science – the scientific revolution of the seventeenth century. Ignorance of the historical past of teamwork constitutes one factor hampering the development of a proper analytical framework for treating it.

The hypothesis that professionalization accounts for the growth of collaboration suffers from none of the foregoing disabilities. It is a more comprehensive explanation than any of the motives listed on Table 1. It holds for both the historical development and the current growth of collaborative research, in contrast to specialization, which represents both a more recent phenomenon and a partial response to professionalization. Furthermore, since collaboration represents a means to both professional advancement and increased knowledge, the hypothesis provides for a more complete range of motivations, from psychological to intellectual. Finally, the hypothesis may provide a framework for explaining other phenomena related to collaboration.

Early collaboration

Although teamwork appeared during the Scientific Revolution as early as the seventeenth century, it cannot really be considered as the prototype of modern collaboration. For several reasons, the joint work of scientists – or, more appropriately, natural philosophers and virtuosi – at that time, as recorded in the *Saggi* of the Accademia del Cimento, the major publications of the Academie des Sciences, the *Histoires,* and various "histories" of the Royal Society, differs from present practice.

In England, the Royal Society tradition was predominantly individualistic; at its meetings, members demonstrated experiments or showed curiosities to the assemblage. Its "histories" were not creatively research oriented, but Baconian inventories of extant knowledge, compiled in committee fashion, as necessary preludes to research, or "experiments of light." In contrast, French and Italian academies carried on group investigations (e.g., "The Committee of the Whole") but such investigations lacked the freely chosen associations of current teamwork. (The closest modern approximation of the French and Italian pattern occurs in the industrial research team or the large accelerator laboratory.)

In neither the research styles nor the modes of publication of early seventeenth-century scientific societies and academies can we find a model for contemporary collaboration. Their usual publication form, a book or an extended essay, does not reflect the brevity, the immediacy, or the particularity of the "modern" research paper. The latter first appeared in the 1660s, in editions of the first scientific journals, the *Philosophical Transactions* and the *Journal des Scavans.* By the same token, the *Encyclopédie* of DIDEROT and D'ALEMBERT stands as a political rather than a scientific landmark. In a tradition dating back to Roman times, the *Encyclopédie's* collaborative character derives not from direct mutual labor on the same subjects, but from pasting together the work of many individuals. Furthermore, the *Encyclopédie* was not concerned with advancing the research front of science.

Collaboration in the 17th and 18th centuries

A logical place to look for the origins of collaboration is in the new organs of communication created in the scientific revolution: scientific journals and their contents, that is, scientific papers. Within a century of their beginning, scientific periodicals as a means for the rapid dissemination of new research had nearly become the dominant mode of reporting investigations. They are the direct antecendents of present day research communications.

Although this approach requires an inspection of such journals, comprehensive libraties of scientific papers produced before 1800 are rather rare, so direct bibliographic research would require considerable time and labor. Fortunately, a comprehensive bibliography of such papers exists as a convenient and acceptable alternative to direct inspection. Of the approximately 20000 or so scientific papers written in the period, a 10% random sample was taken for certain research fields. Table 2 displays the statistical results.

The earliest collaborative paper found was published in 1665 and attributed to HOOKE, OLDENBURG, CASSINI, and BOYLE. Only six of the forty-seven collaborative papers produced before 1800 in the sample date from the seventeenth century. The remaining forty-one collaborative papers were written in the eighteenth century, and twenty-six of these (or 55% of the *total* sample and 65% of the eighteenth-century collaborative papers) were written between 1760 and 1800. Thus; although collaborative work apparently represented a relatively constant fraction of the scientific literature until the middle of the eighteenth century, it increased dramatically after that. During the last four decades of the eighteenth century, French scientists produced 54% of the collaborative papers in the sample.

However, only twelve of the forty-seven collaborative papers can be considered more than astronomical or meteorological observations; of these, eight involve chemical analyses. It appears that teamwork even in the seventeenth and eighteenth centuries exhibits a disparity between "experimental" and "theoretical" research. Collaborative

Table 2
Collaborative papers: 1665–1800

Subject	Number of research papers			
	In all	Collaborative		Collaborative, %
		1665–1760	1760–1800	
Natural History and Zoology	447	1	2	0.7
Botany and Mineralogy	428	2	2	0.9
Chemistry	224	0	5	2.2
Physics	448	1	7	1.8
Astronomy	554	17	10	4.9
Total	2101	21	26	2.2

Data from a 10% count of the papers for the subject headings shown and listed in J. D. REUSS, Repertorium commentationum a societatibus litterariis editarum, 16 vols., Göttingen, 1801–22.

work seems to center about routine matters of technique, of taking and recording observations, or about the observations themselves. Joint research reflecting attempts to use data in a theoretical context to advance knowledge, work of a conceptual kind, or even of an "experimental" as opposed to an observational kind is extremely rare.

The most striking facet of these data is the high degree of joint authorship in astronomy, where 24 of the 27 papers are observations of planets, stars, and eclipses. Mathematical astronomy, dependent on observational data, was closest to being a professionalized science in the 18th century. Major countries supported astronomical observatories, and until midcentury offered substantial prizes for a satisfactory method of determining longitude at sea (in which astronomers were the major participants). In fact, the state's recognition of the importance of astronomy gave the field a quasi-professional status: the state willingly supported research and gave astronomers a considerable say in the disposition of this support. This is reflected in prototypes of the twentieth century IGY, the expeditions equipped and sent forth by every major scientific country for the transits of Venus in 1761 and 1769.[26] Perhaps it is not too surprising that astronomy, of obvious practical interest to eighteenth century society, exhibits the strongest degree of collaborative endeavor.

In short, we have established that the origins of collaboration are closely connected with the origins of modern science and with professionalization's early stages. Collaborative work is found in the journals of scientific societies, which were aimed at a particular audience. Furthermore, in the field of astronomy, where professionalization is most advanced, collaborative work occurs frequently.

The nineteenth century: professionalization of science

Throughout the eighteenth century and the first decades of the nineteenth century, the scientific communities of both England and Germany were taking the first steps toward professional status through the establishment of scientific societies, journals which published scientific papers, and state-supported observatories and museums, and through the pre-emption of amateurs in some subfields of science. However, it was France during the revolutionary and Napoleonic eras which "rushed science to professional status as part the reconstruction of society as a whole."[27]

The French scientific community differed from those of its two main rivals (the German and English scientific communities) particularly in its achievement of general social acceptance and support from the larger Franch society. Moreover, the French scientific community was able to exercise autonomy over its use of this support.

Unlike German and English science, which had little or no state support, French science used such aid, in the last decade of the eighteenth and first decades of the nineteenth centuries, to legitimize and institutionalize itself on a grand scale. France fostered the world's leading scientific institutions of the time: the École Polytechnique, the École Normale et Supérieure, the Muséum d'Histoire Naturelle, and the Institut de France. The existence of such well-supported institutions encouraged the teaching of science as "valuable in itself, and not merely as auxiliary to some other professions."[28] Though the French scientific community was more concerned with the training of an elite corps of researchers than with producing large numbers of scientists[29] (a practice which may partially account for the rapid decline of French science after 1840), the institutions mentioned above did provide employment for French scientists. Other scientists occupied state-supported positions and some "became part of the official elite during the last years of the Revolution, and maintained this status under Napoleon."[30] Consequently, through employment within scientific institutions and integration within the official elite, the French scientific community enjoyed considerable state support and a considerable degree of autonomy from the larger society.

In short, conditions in France provided the roots for the growth of formal scientific training, the modern research laboratory, specialization, and hence, above all, professionalization.[31] In this regard France became a model for others to emulate.[32]

Both England and Germany recognized the successful components of French scientific growth, especially the need for educational reform and state support.

> It is no accident that Liebig after leaving the laboratory of Gay-Lussac in Paris was so impressed by the importance of laboratory study that he established a new method of teaching chemistry in the laboratories set up at the University of Giessen. But the innovation which was probably more important was the difference in the type of men engaged in teaching . . . the leaders of their day in research . . .[33]

As the century progressed, both Germany and England developed professional scientific communities, though they were several decades behind the French. University reform, in Germany, though not initially favorable to the natural sciences, had, by the 1830's led to the beginning of "a flowering of the natural sciences and of the experimental approach in general, giving Germany a lead over England.[34] Furthermore, OKEN's organization of the Gesellschaft Deutscher Naturforscher und Ärzte in 1822 testified to the growing "strength" and self-consciousness of the German scientific community. It also stimulated British emulation. In 1831, the British Association for the Advancement of Science was founded. British scientists had increasingly grown dissatisfied with the state of British science. Their complaints received widerspread public notice in 1830 with the publication of Babbage's *Reflec-*

tions on the Decline of Science in England. The central focus of their dissatisfaction and concern is reflected in Babbage's complaint that "Science in England is not a profession, its cultivators are scarcely recognized as a class."[35] The Royal Society, England's most prominent scientific institution, scarcely represented science; its scientist members were a minority in 1830. Not until mid-century did the rough balance between scientists and non-scientists begin to tip in favor of the former. As one historian of science has commented,

> English science lacked organized support, few English scientists held university posts, little attention was being paid to education in the sciences, and English men of science lacked financial security.[36]

In England professionalization had made little progress: amateurs vitiated pre-emption, education in science was weak, social and state support virtually non-existent, and professional autonomy a dream.

The French character of collaboration

If we are correct in considering collaboration as a response to the professionalization of science, then we should find that: (1) collaboration occurs as a frequent or typical style of research in the first scientific community which became professionalized, that of France in the Napoleonic era, (2) collaboration during the first decades of the nineteenth century is mainly limited to the French scientific community, and (3) teamwork represents a typical and useful method of research among those who stand out as the most professional French scientists of the time.[37]

In order to investigate the extent to which professionalization was tied to collaboration, we decided to see if networks of collaboration would adequately reflect the French professional scientific community. Using a snowball sample, we chose an example of a professional scientist of the time and attempted to generate a sample through his collaborators.[38] Such a sampling technique has the advantage of ascertaining the importance that professional scientists placed on the collaborative style of research. It immediately identifies those respondents who are collaborators and establishes the extent to which these respondents are linked by their collaboration within the professional community. If the sample tends to generate a map of the professional scientific community, then we may infer that the members of the community regard collaboration as an important style of research. Furthermore, this indicates that a positive attitude toward collaboration is more or less generally held throughout the community (depending on the extent to which the community is linked). This must be so, for the basis of the mapping is the collaborative linkage itself.

JEAN BAPTISTE BIOT (1774–1862), who during the first 20 years of the nineteenth century established a reputation as one of the leading scientists in France, seems to exemplify the career of the professional scientist. For this reason he was chosen to generate the sample. BIOT began his rise within the scientific community when he wrote LAPLACE offering to proofread the pages of *Mécanique Céleste* as they came off the press. LAPLACE accepted, and BIOT's career began through the patronage of LAPLACE. By the end of his career he was a member of the Paris Académie des Sciences and the Royal Society of London, a professor at the College de France and the Faculté des Sciences, an adjoint of the Bureau des Longitudes and a member of the editorial boards of several journals in the physical sciences. His original research covered almost every area of the physical sciences. In a sense, BIOT's career offers a midpoint between those of LAPLACE and BERTHOLLET, whose scientific lives began before professionalization occurred in France, and those scientists who, like GAY–LUSSAC and DULONG, began their careers in the era of professionalization.

The initial choice of BIOT led to the generation of a large group of collaboratively-linked scientists. The group eventually reached a size of approximately 900 members before no new collaborative linkages could be found. The sample, through succeeding generations of collaborative linkages, spans the entire period of the *Royal Society Catalogue* from 1800 to 1863. It includes as collaborators scientists representing the nationalities of more than six European countries. Furthermore, it reflects the non-specialized character of most science during this period, for it covers publications in all fields of scientific work: chemistry, physics, biology, technology, earth science, biochemistry, medicine, etc. Its linkages suggest interactions between scientists on and between all levels of eminence.

Because we intend to show the linkage between collaboration and professionalization, we shall concentrate our comments on that portion of the sample which includes only those members who began their scientific careers within or before the period when the French scientific community became professionalized, namely, 1799–1830.

The extent to which collaboration occurs as the French style of research during this period is indicated in Tables 3 and 4.

As Table 3 shows, during the thirty-year period from 1799 to 1829, approximately 53% of all collaborative papers produced were written by members of the sample.[39] Furthermore, nearly 75% of those papers (i.e., those written by members of the sample) were the product of two or more French scientists, as can be seen from Table 4. The remaining 25% of the papers which can be credited to members of the sample reflect a mixture of national inputs. Nonetheless, they also evidence a French predilection for collaboration. Some of these papers are produced in collaboration with at least one French author. Still others, albeit the product of two or more authors who are not French, indicate a "French connection" because they are linked in turn through one of their authors (at one or more removes) to a member of the French community.

Further, it is especially noteworthy that despite occasional collaboration by German and English scientist, chains of collaborative linkages (subgroups of collaborators within the sample) are not generated by members of the English and German scientific communities during the first decades of the nineteenth century. Rather, the formation of such chains of collaborators depends on the French community's tendency toward collaboration. This is indicated in the following two figures. (The direction of the arrow indicates the direction of the linkage [by time]. A single arrow indicates all other collaboration took place after the linkage depicted. A double arrow indicates there were collaborative associations both before and after the indicated linkage).

Table 3
Cumulative collaboration by decade

Decade	Percentage of Sample Group's total collaborative output which had been published by the end of the decade	Percentage the Sample Group produces of cumulative total of all collaborative papers published by end of decade
1800–09	8	87
1810–19	14	69
1820–29	28	53
1830–39	43	47
1840–49	66	43
1850–59	89	37

Table 4
Cumulative percentage of collaborative papers produced by the Sample Group, by nationality of authors, by time

Period	Nationality					
	French	German	English	Swedish	Other	Total
1800–09	87	2	–	6	5	100
1800–19	81	6	2	8	3	100
1800–29	75	12	4	5	6	102*
1800–39	70	16	8	3	3	100
1800–49	70	15	10	3	2	100
1800–59	69	14	9	3	5	100
1800–63	68	14	10	3	5	100

*Figures add to more than 100% because of rounding upwards.

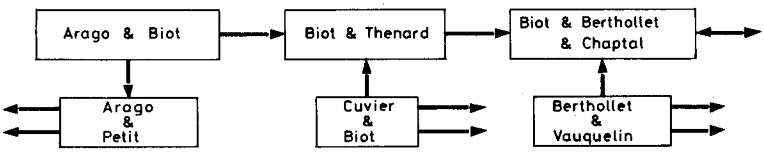

Fig. 1. French patterns of collaboration

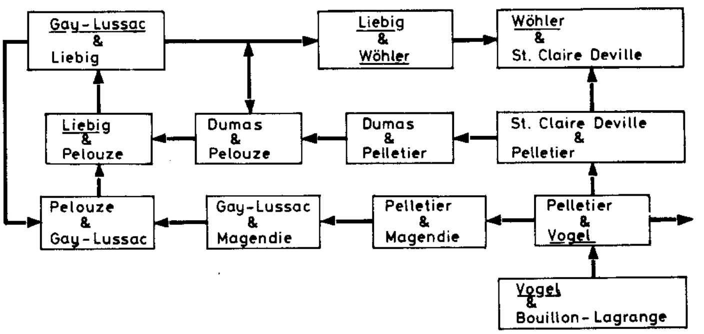

Fig. 2. French interaction with other nationalities (underlined names are non-French scientists)

Although these results support the hypothesized relationship between collaboration and professionalization, it is necessary to examine the possibility that they are an artifact of the sampling procedure.[40] Consequently, a random sample of French, British, and German scientists who were contemporaries, but not members of the BIOT group, was drawn from a preliminary listing of scientists for the *Dictionary of Scientific Biography.*[41] The sample was stratified into two categories, (highly eminent and eminent, as judged by the DSB editors on independent grounds) as an additional safeguard against any bias present in the earlier sample.

The results not only confirmed, but strengthened, the earlier conclusions about the French predilection for teamwork.

Table 5 demonstrates that collaboration within the time period 1800–1830 is restricted mainly to the French. While 73% of the French scientists in our independent sample collaborate, only 13 and 22% of British and German scientist, respectively do so. At the time French science accounted for slightly less than half of all research. Though the frequency for all of the above groups (with the exception of the English, who seldom collaborate) is unusually high (the average for the time is approximately 11%[42]), it is the French who display an exceptional propensity for this style of research. Furthermore, French scientists demonstrate a tend-

Table 5
Extent of collaboration by nationality, for scientists not belonging to sample group

Rank	Percent who collaborate		
	French	English	German
Highly Eminent	60	15	25
(N =)	(15)	(20)	(4)
Less Eminent	77	11	21
(N =)	(26)	(18)	(28)
Overall	73	13	22
(N =)	(41)	(38)	(32)

ency to expand the collaborative style of research by forming chains – rather than pairs – of collaborative linkages. For example, many of the French scientists in this sample formed chains of five or more members. Such linkages indicate the general acceptance of collaboration as a style of research within the French community; furthermore, they suggest a closeness between its members and hence a communal cohesiveness.

On the other hand, English scientists within the sample never formed more than dyadic relationships with other scientists when they collaborated. Occasionally, a German scientist linked to a French scientist would generate an additional collaborator, but this was not such a general tendency as it was with the French.

Moreover, eminence alone cannot be viewed as a factor of collaboration. As we can see from Table 5, equally brilliant British and German workers tended to work in far more isolation than their French counterparts.

The factor accounting for this difference in research style between the French, on one hand and, on the other, the German and English, is the community nature of French science – which is the product of its professionalization. In addition, then, to its other aspects, collaboration can be seen as a formal acknowledgement of a community's existence. The more often collaboration links researchers together, and the longer such linkages can be extended, the more integrated is the community and the more mutually dependent are its members.

Conclusion

We have thus established, as predicted by the theory, that when France was the only country with a professional scientific community, its scientists produced the bulk of co-authored research. Furthermore, while that was a normal research pattern in France, it was not in other countries. The predominantly French character of that pattern, which otherwise would appear anomalous and somewhat peculiar, represents a natural consequence of the process of professionalization.

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The authors wish to express their grateful appreciation for the most generous assistance and helpful advice of Prof. D. de SOLLA PRICE of Yale University.

The inception of this study was partially supported by an Assistant Professor Research Grant from the University of Missouri-Kansas City.

Notes and references

1. In this and the two subsequent essays, we are primarily concerned with the empirically researchable topic of formal acknowledgement of scientists' collaboration in research by a co-authorship of published work. Because of this we use the following terms interchangeably: "collaboration", "teamwork", "mutual, cooperative, or joint research", "joint or co-authorship". We recognize that these terms have distrintly different implications in other contexts, but we have not dealt specifically with collaborations between scientists and non-research personnel, such as supporting technicians, nor with internalist historical criticism of large and expensive installations and the scientific and economic pressures these produce towards collaborative work. Such subjects must be very differently treated, and we are not aware of any empirical statistical investigations in such areas.
2. See, for example, M. SMITH,, The Trend Toward Multiple Authorship in Psychology, *American Psychologist,* 13 (1958) 596–599, J. P. PHILLIPS, The Individual in Chemical Research, *Science,* 121 (1955) 311–312; W. R. UTZ, *American Mathematical Society Notices,* 9 (1962) 196–199; B. L. CLARKE, Multiple Authorship Trends in Scientific Papers, *Science,* 143 (1964) 822–824; D. de SOLLA PRICE, *Little Science, Big Science,* Columbia University Press, New York, 1963, p. 87–90.
3. For a comprehensive review of these positions, see H. ZUCKERMAN, Nobel Laureates in the United States: A Sociological Study of Scientific Collaboration, (Unpublished Ph. D. dissertation, Columbia University, 1965), Chapter 1. (Revision, without extensive co-authorship statistics, published as *Scientific Elite, Nobel Laureates in the United States,* Free Press, New York, 1977).
4. The best of these are: H. ZUCKERMAN, op. cit., and two works by W. D. HAGSTROM, Traditional and Modern Forms of Teamwork, *Administrative Science Quarterly,* 9 (1964) 241–263, and *The Scientific Community,* Basic Books, New York, 1965, Chapter III.
5. Although the sociologist R. MERTON has been most influential in studying science as a community, his approach tends to obscure certain important factors. First, his postulation that the scientific community is organized around four norms (organized skepticism, universalism, communality, disinterestedness), either denies the existence of other motivations in a scientist's career or downgrades them by making them only isolated deviations normally to be shunned by scientists. More significantly, MERTON's work has oriented the sociology of science toward explaining the structure of the scientific community in terms of these four norms and consequently influenced others toward the view that scientists' behavior can be

explained as either conforming to or deviating from the norms. Finally, reliance on this normative ideology, especially when priority or recognition is involved (two case which are statistically significant events in the scientific community) has led sociologists of science to treat status as a passive phenomenon (not leading to power which is tabooed by the norms: such a case in which power or authority was involved would be treated as an isolated deviation). In this view status seeking is a permissible goal only when it can be explained as the natural result of conflicting norms. Thus this provision eliminates the need for explanations based on other non-positivistic possibilities. For an extended discussion of MERTON'S influence on the sociology of science see: M. D. KING, Reason, Tradition, and the Progressiveness of Science, *History and Theory*, 10 (1971)

6. E. MENDELSOHN, The Emergence of Science as a Profession in Nineteenth-Century Europe, in: *The Management of Scientists*, K. HILL, (Ed.), p. 4.
7. The nature of this process seems to differ when the profession exists mainly outside the academic community. Although the non-academic professional is also required to commit himself to the strictures of his professional community (for example, he cannot practice without the community's sanction), relationships with the community are defined less closely, and success in a professional career such as law or medicine depends at least as much on client relationships and referrals as on collegial approval. Within the scientific community, especially that part of the community dedicated to pure science, the clients, in a sense, are other scientists. Consequently, success in a scientific career is more strongly structured by professional institutions and relationships, on which scientist depends. Furthermore, the unanimously professional nature of the scientist's clientele helps discourage the evaluation of his work by "non-professional" criteria.
8. G. DANIELS offers a possible historical view of professionalization as progressing through four distinct but overlapping stages: pre-emption, institutionalization, legitimation, and professional autonomy. According to DANIELS, pre-emption marks a declaration of independence, that only those who are specially educated and trained in science are fit to contribute to science and to evaluate scientific contributions. Consequently, no longer can science be considered a province for convenient dabbling by the well-educated and well-meaning amateur. Scientific research is reserved for the practitioner, and a zealous defense and preservation of that domain underlies the xenophobic and chauvinistic character of pure science.
   The formation of scientific societies, journals, observatories, museums, research laboratories, foundations, and social educational institutions or curricula for science marks the emergence of the second stage, institutionalization. The third step, legitimation, signifies social acceptance of the previous two stages, through acknowledgement and support of the newly formed social system of science. Professional autonomy, the final and most crucial stage, is that in which the professional leaders of science gain the ability to control and direct social resources in support of science without significant interference on the part of non-scientists. Although this schema offers a context in which to place the historical growth of professionalization, we prefer to view the process as open-ended, without a "last" stage. See, G. DANIELS, The Process of Professionalization in American Science: The Emergent Period, 1820–1869, *Isis*, 58 (1967) 151–166.
9. T. S. KUHN, *The Structure of Scientific Revolutions*, Chicago Univ. Press, Chicago, 1962, p. 37, 40, and M. D. KING, Reason, Tradition, and the Progressiveness of Science, *History and Theory*, 10 (1971)) 20, 27–28.
10. This does not necessarily imply a self-conscious group, although it does not exclude it. In *The Power Elite*, C. W. MILLS noted that the "power elite is composed of men (who) . . . are in positions to make decisions having major consequences. Whether they do or not make such decisions is less important than the fact that they occupy such pivotal positions . . . They occupy the strategic command posts of the social structure, in which are now centered the effective means of the power and wealth and the celebrity which they enjoy."

11. N. REINGOLD, *Science in Nineteenth Century America,* Hill and Wang, New York, 1964, p. 200.
12. In science there is a complex interplay between professional position (indicating power and authority) and intellectual disputation. KUHN recognized that intellectual grounds are not always the deciding factor in scientific disputes. He notes, on the contrary, that during revolutions in science there is no intellectual grounding for deciding in favor of one paradigm or another. Only *after* the decision has been taken can an intellectual justification be generally accepted. At such times power within the professional community might be less important than in times of normal science, when disputes are less challenging to the professional power structure. Then they are more amenable to professional injunctions rather than community vote. During a period of normal science the power structure is more easily able to exercise control through positive inducements, such as grants, editorial policy, and appointments to institutions, committees, or societies.
13. N. REINGOLD, op. cit., p. 206.
14. As KING has noted, because those who write about the sociology of science have treated property as an emblem of status rather than a source of power the concept of recognition which bestows the right of property to a scientist for his discovery has consequently been dealt with as a passive phenomenon: it merely confers prestige. However, in the case of priority of discovery "what is at stake is not simply the prestige of a 'father', but his authority, and the intellectual commitments of his heirs." (KING, op. cit., p. 20)
15. This process can be viewed as a social image of what KUHN describes in the *Structure of Scientific Revolutions.* The transition from pre- to post-paradigm periods within scientific communities is similar to the effect of professionalization in that it gives rise, through the need to select a single paradigm for the community (subcommunity), to new hierarchies and increased coherence.
16. T. S. KUHN, op. cit., p. 177.
17. For an extensive treatment of such hierarchical social structure in science, see J. R. COLE, and S. COLE, *Social Stratification in Science,* The University of Chicago Press, Chicago, 1973.
18. Not only directly, through the output of research, but also indirectly by providing recognition to the next generation of elite scientists who will acknowledge their intellectual debts. Consequently, collaboration provides both an indirect means of advancing one's authority in the scientific community and a direct path of personal productivity.
19. D. CRANE, Collaboration, Communication, and Influence: A Study of the Effects of Formal and Informal Collaboration among Scientists, mimeo, 1969, and W. O. HAGSTROM, Traditional and Modern Forms of Teamwork, cited above.
20. For example, see J. JEWKES, D. SAWERS, R. STILLERMAN, *The Sources of Invention,* St. Martin's Press New York, 1959, p. 161–162, and G. P. BUSH, L. H. HATTERY, Teamwork and Creativity in Research, *Administrative Science Quarterly,* 1 (1956) 361–362.
21. H. ZUCKERMAN, op. cit., p. 79, 85.
22. This is similar to the critique which N. STORER levels against the genealogy of scientific norms proposed by R. K. MERTON, B. BARBER. Cf. N. W. STORER, *The Social System of Science,* Holt, Rinehart and Winston, New York, 1966, p. 83–86.
23. For example, personal motivations and satisfactions appear in protocols in the work of ZUCKERMAN and HAGSTROM, but they remain incidental and subordinate. Cf. H. ZUCKERMAN, op. cit., and W. O. HAGSTROM, *The Scientific Community,* loc. cit.
24. R. K. MERTON, Priorities in Scientific Discovery: A Chapter in the Sociology of Science, *American Sociological Review,* 22 (1957) 635–659, and E. G. BORING, Eponym as Placebo, in: *History, Psychology and Science: Selected Papers,* R. I. WATSON, D. T. CAMPBELL, (Eds), Wiley, New York, 1963, p. 5–25.

25. J. D. REUSS, *Repertorium commentationum societatibus litterariis editorum,* [etc.], Göttingen, 16 volumes, 1801–21.
26. H. WOOLF, The Transits of Venus, Princeton Univ. Press, Princeton, N. J., 1959.
27. E. MENDELSOHN, op. cit., p. 7.
28. R. GILPIN, op. cit., p. 88.
29. R. GILPIN, *France in the Age of the Scientific State,* Princeton Univ. Press, Princeton, N. J., 1969, p. 86.
30. J. BEN-DAVID, *The Scientist's Role in Society,* Englewood Cliffs, N. J., Prentice-Hall, 1971, p. 97.
31. R. GILPIN, op. cit., p. 94.
32. E. MENDELSOHN, op. cit., p. 14.
33. Ibid., p. 11.
34. J. BEN-DAVID, op. cit., p. 117.
35. C. BABBAGE, *Reflections on the Decline of Science in England,* p. 10–11, as quoted in MENDELSOHN, op. cit., p. 22.
36. E. MENDELSOHN, op. cit., p. 31.
37. We here defer treatment of the third consequence to a forthcoming essay, in which it is shown to be the case: See Scientific Co-authorship, Research Productivity and Visibility in the French Scientific Elite, 1799–1830: Studies in Scientific Collaboration II, *Scientometrics,* in press.
38. For information about the statistics of such sampling, see L. GOODMAN, Snowball Sampling, *Annals of Mathematical Statistics,* 32 (1961) 148–170.
40. The problem with the snowball sample concerns a feeling that succeeding "nominations" carry a bias introduced at the original starting point(s). With respect to this sample this has both positive and negative aspects. On the positive side, the sampling procedure helps trace the linkages within the scientific community. On the negative side, it may have led to an overrepresentation of eminent French scientists within the sample because of the initial choice of BIOT: BIOT, himself an eminent French scientist, may in turn have only picked eminent French scientists, and so on with succeding nominations.
41. A printout listing all the scientists to be included in the *Dictionary of Scientific Biography* 1970–), rank ordered each scientist by assigning an article length corresponding to his importance. This list served as the universe from which the several samples for comparison with the Biot group were chosen at random.
42. From the *Royal Society Catalogue,* it can be estimated that between 1800 and 1863, approximately 36000 scientists contributed papers, of whom about 4000, or 11%, collaborated at least once.