Enclaves of Anarchy: Preprint Sharing, 1940-1990

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ABSTRACT
The purpose of this paper is to provide a timeline of the gradual organization of preprint sharing of traditional research articles and its consolidation as the preferred means of connection among scientists engaged at the forefront of increasingly narrower sub-specialties that emerged after World War II. At mid-century, the flood of research that scientific journals faced created a burden on the traditional system of publication. The social dynamics involved in the production of high energy physics research, in particular, proved to be the main arena of scientific communication that contributed to the advent of preprint exchange as an alternate system of publication during the 20th century. Increasing sophistication of information and communication technology relative to cost and convenience of use were factors that ultimately led to the online deployment of arχiv in 1991. The history of preprint sharing in scientific publishing is a relatively unexplored area of study, and subserves the larger need to understand the immediate circumstances surrounding the move to digital publication by scientific journals. In particular, the challenge presented by preprint sharing to the formal system of peer review that print journals have traditionally offered was an over-riding concern throughout the course of the changes described here.

Keywords
Preprints, scientific communication networks, 20th century, scholarly publication, peer review.

INTRODUCTION
The title of this paper is a phrase lifted from questions raised by sociologist Herbert Menzel (1967) at the height of the scientific community's frustration with the formal publication system: "Will the advances in information engineering make informal scientific communication obsolete? Or is person-to-person communication so ingrained or so indispensable that it will perpetuate enclaves of anarchy in a well-planned landscape?" (57). Preprint sharing raises questions regarding formal and informal communication that have historically had great salience for approaches to scientific information. However, the development of preprint in the 20th century has never been the object of a general historical study within LIS. This study is an incipient effort at synthesis of the historical evidence.

The research presented here has taken the form of a broad historical survey of preprint practices in certain scientific communities between the 1940s and the 1990s and of the attitudes of various stakeholders toward these practices. This project was initially conceived as a timeline that could expose the sociological progression of prepublication practices that led up to the online deployment of arχiv. This paper, then, is not meant to be an exhaustive account, but one that provokes thinking about the evolution of various segments of the scientific community's information practices during the mid-20th century. The method I used in the collection of the information presented here is typical of most historical investigation. The primary and secondary sources accessed were publications that appeared during the five decades in question. Particular attention was given to statements made by editors, scientists, librarians, and documentalists regarding prepublication sharing of research. No attempt is made here to provide a history of technology or explain technological systems in any detail. The timeline ends at 1990 as a general point at which publication fundamentally shifted to digital publication. The focus here is on the fate of the traditional scientific research article as the systems involved with sharing information changed to meet the needs of certain scientific research communities.

CONFERENCES AND ANXIETIES: THE 1940S
In this decade, the perception of a scientific information crisis was in its early stages due to the end of World War II. During the war, many scientists worked on military projects that, for security reasons, were kept secret; once these obligations expired, a steady outpouring of publications ensued J. D. Bernal, renowned crystallographer and pioneering writer on the sociology of science, complained in 1948 that the world wars had "wrecked" the scientific communication system (McNinch, 1948). Subsequent to these upheavals, there was a great amount of pressure for the publication and distribution of single articles, known as "separates." In the face of such efforts at obviating the traditional system of publication, conservative forces within the scientific community struggled to maintain the place of the journal and the journal editor as prime forces in scientific information exchange (Phelps and Herling, 1960).

In 1945 a proposal was made to reform journal publication by federating existing scientific societies so that preprints

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could be easily distributed to all their members (Phelps and Herling, 1960, 65). One manifestation of this effort at reform was the foundation in 1949 of the Medical Sciences Information Exchange. This was meant to be a clearinghouse for ongoing research supported by federal and private sources. Its purpose was to prevent redundancy by enabling researchers to be aware of ongoing projects similar to their own. Eventually relocated to the Smithsonian, the Exchange sought to institutionalize the preprint exchange process on behalf of the American scholarly community, arguing that this would not only save cost to the individual author but would also be more democratic in that scholars not known to the author could also access copies. At the time of its inception, the Smithsonian's Science Information Exchange was strongly opposed by many senior scientists because it tacitly awarded recognition to unrefereed work.

Maintaining the established convention of peer review was a primary concern as systems of sharing scientific information alternate to formal publication tentatively emerged. In 1946 at the Royal Society Empire Scientific Information Conference, J. D. Bernal submitted a suggestion for periodical reform that included "provisions for the refereeing of papers by panels of members of the scientific societies before publication" followed by the distribution of "separates" to libraries and journals upon request (Phelps and Herling, 1960, 65). Two years later, Bernal more formally proposed at the Royal Society Empire Scientific Information Conference that the scientific community of the UK needed a "National Distributing Authority," and was in need of a centralized publication and distribution hub. His proposal was that "instead of distributing copies only on request, copies would be distributed immediately to all libraries and individuals who had registered their interest in certain fields," and also that "abstracts would be distributed on cards rather than in journals" (64). He withdrew the idea, however, admitting that since "studies had shown that distribution of separates…of scientific information in any form direct from publishers to individual scientists played only a minor role in the communication of scientific knowledge" (67).

JOURNAL POLITICS AND THE COLD WAR: THE 1950S

At the start of the 1950s there appeared to be "a sort of unconsciously cooperative groping by all parties toward workable solutions" that, though not formally organized, constituted a system of sharing research that had not yet been published (Clapp, 1954, 2). During this decade the U.S. dollar was established as the dominant world currency (Hoole, 2006, 89), thus securing the central place of the United States in the development of "Big Science," and also in the management of the distribution of scientific research. It was in these years that more calls for "separates, either printed or on microform" continued to be made, but the idea of separates came to be seen more as a means of bypassing journal publication than for any other reason (Phelps and Herling, 1960, 65). It was in the early 1950s that the Belgian particle physicist Leon Rosenfeld complained of "almost drowning in the flood of new results of experiments with particle accelerators" and observed that because "physicists are sending preprints to colleagues following the same trail…periodicals no longer properly fulfill their function" (Andriesse, 2008, 133-134). Statements such as these heralded the advent of a quasi-formal preprint exchange system in the burgeoning field of high energy physics.

The beginning of the 1950s also saw a marked growth proliferation in the publication of scientific journals, a development that, according to Hoole (2006), reflected the unleashing of peacetime scientific activity at the end of World War II (88). It appears that politics within the journal publishing business itself played a large role in the rise of preprint exchange as a preferred means of exchange among scientists. For example, Ukrainian-born British MP and entrepreneur Robert Maxwell was a "dishonest competitor" in the world of scientific publishing according to Andriesse (2008). His Pergamon Press (today an Elsevier imprint) sought to capitalize on the mid-century publishing crisis with the preemptive, initial publication of the *Journal of Nuclear Energy* (1954), an action which effectively undermined the successful launch of the journal *Nuclear Physics* (1956) by North-Holland Publishing (also currently an Elsevier imprint) under the guidance of Maxwell's competitor, Dutch physicist Daan Frank (Hoole, 2006, 132-133, 137). Such commodification of journal publication by large publishers contributed to the growing inclination by physicists to bypass it altogether.

During the early 1950s, the push for a system of preprint sharing had not yet fully focused on the problem of peer review. In 1954 calls began to come from authors working in various sub-disciplines of psychology for a more centralized distribution of separates. At the same time, these calls were not aimed at negating the established assumption that journals should maintain their authoritative editorial function by continuing to serve as the primary and formal hubs for distribution (Phelps and Herling, 1960, 66). Indeed, somewhat polemically, American librarian Mortimer Taube of Documentation, Inc. claimed in a 1953 lecture entitled "Implications for Professional Organization and Training" (1953) that "these publishers of unpublished reports threaten to invade the field of librarianship and steal its most sacred jewel, its mark of professional status, namely, the responsibility for organizing specialized information" (123).

The launch of Sputnik in 1957 stimulated a flurry of governmental activity regarding the effectiveness of scientific information exchange. *Inter alia*, the International Conference on Scientific Information, sponsored by the National Science Foundation, the National Academy of Sciences, and the American Documentation Institute, met in order to address the state of affairs regarding publication of research. The result was a two-volume, 1600-page document that included various findings and proposals by noted pioneers in the information field. Among these findings, the mention of preprints in several places indicates its central role in information exchange between scientists by this time. For example, "Review Literature and the Chemist" contained the suggestion that "documentation centers staffed by specialists, equipped with electronic computing machines and in telecommunication with university and industrial laboratories, would make a good deal of printed information unnecessary," presaging arχiv by three decades (Phelps and Herling, 2006, 67). Other presentations included perspectives on how the library field was experiencing the sudden importance placed on prepublication sharing of work. For example, the introduction of marginal or "grey" literature into the acquisition process had become a problem for special librarians, according to one report (Cleverdon, 1958, 695). As well, the "Aslib Education Committee" addressed the issue of saving unpublished research presented at scientific gatherings and proposed a syllabus for post-graduate training in which up to 48 of 166 total lecture and practical working hours would be required with "preprints and offprints" (Agard-Evans and Farradane, 1958, 1489-1493).

At the close of the decade, in 1959, the United Nations Educational Scientific and Cultural Organization (UNESCO) undertook a long-term study of "the increasing inadequacy of the scientific research periodical as a method of communication." The study found that a number of suggestions for alternative forms of dissemination had been ongoing for several decades. Among these were proposals that:

- special radio and television stations could broadcast reports,
- tape recordings could be used as an effective means of dissemination,
- the publication of microcards or photographs of entire articles was needed,
- and that the deposit of lengthy articles and tables not fit for periodical publication could be placed in central, internationally-located depositories (Phelps and Herling, 1960, 62-63).

The authors of the report conclude that their own "experience in the Engineering Societies Library with preprints of our societies shows that the man-hours required to receive and prepare separates for use are many times the number of man-hours required to handle the same number of papers received as papers in periodicals" (Phelps and Herling, 1960, 71). In this same year, MIT's Artificial Intelligence Lab instantiated a series of research articles entitled "AI Memos." Some of these memos became conference papers, journal articles, or book chapters, but many remained research manuscripts without subsequent publication in other forums (Kling, 2003).

APA, NIH, and HEP: THE 1960S

Proposals for organized hubs of preprint sharing began in earnest on a group rather than individual level during this decade. In a 1960 issue of *ASLIB Proceedings*, J. D. Bernal encouraged the inception of "a ganglion system of communication" that would be ancillary to the regular system of publishing. Such a communication system would abet the discovery of international research through "a complete system of reports" and "a directory of all the scientists concerned and their special interests" that could be accessed by "using modern information transmission methods" to "enable people to get the information they want in an extremely quick way" (Bernal, 1960, 437). Bernal clearly expected a system like Ginsparg's arχiv to eventually arise as a way to meet the snowballing need to streamline information transfer: "I anticipate the building up of an information service in the future on a logical basis as a piece of communications engineering" (438).

Psychology (APA)

In 1961 Derek de Solla Price's *Science since Babylon* was published with a "highly influential footnote" that preprint exchange was one of the markers of a "new invisible college" comprised of scientists who "substitute personal contact for formal communication among those who were really getting on with the job" (Griffith, 1969, 233; Griffith and Miller, 1970, 126). Price's thesis found striking expression during this decade in a brief but important experiment with systems of preprint exchange that arose in some newly developing, highly specialized areas of psychology. This was documented and analyzed in Garvey and Griffith's well-known project on scientific information exchange with the American Psychological Association (APA) that began in 1961. The study showed that preprint exchange by the producers and consumers of information in the field of psychology was one means of creating a new channel of communication in an attempt to improve the performance of the publishing system, which had, at the time, an onerously and counter-productively protracted publication lag (Garvey and Griffith, 1967, 1011-1012).

The formation of these new avenues of communication became an indication to Garvey and Griffith that a new specialization was forming, and it was often followed by the inception of a new journal. Furthermore, this "process of formalization may continue to evolve until someone realizes that an institution has emerged which has most of the characteristics of an archival journal: a large and increasing input of manuscripts, an existing gatekeeping group, an eager and expanding audience, and growing economic problems. And thus a new journal—and possibly a new scientific society—is born" (Garvey and Griffith, 1967, 1012). The study also revealed that "those who need preprints most—young scientists, workers at small institutions, and researchers in less developed countries—are frequently not the recipients" (1014). As an alternative to journal publication, preprints were seen as essential to the dissemination of research prior to APA conventions because they facilitated and made more effective the in-person, informal exchanges that occur at such meetings (1015).

Griffith also undertook a solo project with the APA that focused on their innovation of publishing lists of manuscripts that had been accepted by long publication lag journals. The *1965 Proceedings* of the APA convention became the "early public announcement of research completed and accepted for publication [and] was expected to make it possible for a variety of interested persons—many of whom would have been unable to do so—to contact authors nine months to over a year prior to publication" (Griffith, 1969, 3). The study found that:

- Requestors were generally very young and did not usually have access through the "invisible college" (Griffith, 1969, 19).
- "Grey literature" was differentiated from "preprints" in that convention papers were seen as outside of the formal (journal) and informal (preprint) system, and so received "some screening for quality;" they were generally considered as "interim reports of work" which would later be "published in some archival form" (Griffith, 1969, 21).
- Therefore, as a form of peer review, the *1965 Proceedings* had an important effect upon the submission of manuscripts: about 25% of authors delayed or decided against submission of a manuscript to an APA journal based on reception of their convention presentation (Griffith, 1969, 42).

Another way in which the *APA Proceedings* arguably comprised an informal system of review and served as a means for one's peers to enact the important work of reviewing and editing is that over two-thirds of conference attendees reported current or former activity in the same area of work as the presenter of a lecture they attended. In fact, a section of the reports entitled "Modifications of Scientific Work Resulting from Convention Presentations and the Interactions Surrounding such Presentations" indicated that the conference "findings are of interest because, in general, the "Author" respondent groups are the most actively engaged in the research and take a most active part in the convention interaction, or, at least, are likely to have a great number of queries directed toward them whatever their own participation may be" (Griffith, 1969, 110-111).

Information Exchange Groups (NIH)

The National Institutes of Health (NIH) also began its Information Exchange Group (IEG) experiment in 1961. The IEG was a "continuing international congress by mail," a characterization that was printed on the front cover of all the scientific memos that were exchanged (Albritton, 1965, 13). The program was provoked by the recognition that a single scientist often has research he wants to share with all other scientists who are working in same area. The information exchange between the seven groups that were formed for the study functioned in this way: "It may be a sentence, a paragraph, or even a complete research paper, ready to submit to a journal or, indeed, already submitted and perhaps accepted. […] There is no 'review,' no editing or abstracting. The material that is sent on its way to each of the members is a photographic replica of the material received from the author. In this respect the IEG, in operation, is a group of scientists engaged in a research-area-wide, worldwide private professional correspondence" (2).

Each IEG dealt with an extremely narrow specialty and differed radically from the Physics Information Exchange (PIE) groups that were started in the following year. Though they all related to the domain of biology, each IEG consisted of individual biologists, not of groups operating through a library. The first six IEGs had a combined membership of almost 800 by 1965, and the seventh IEG, which was provoked by an article in a 1964 issue of *Science* regarding the success of the experiment with the first six groups, was expected to increase that number to about 1,000 individual scientists (Albritton, 1965, 6-7). According to the author of the report prepared for the Office of Research Accomplishments, Division of Research Grants of the NIH, "Approximately 80% of these memos "were full research papers destined for publication" (11). Regarding priority, any research findings that were shared using the IEG were treated as "personal communication" between professional colleagues and were to be given due credit (9).

Although the report noted that scientists typically trust colleagues they know personally and keep one another informed via private correspondence of research findings as they come to light, the matter of priority was a prime concern when the first IEG was organized because membership involved stepping onto the "untested terrain" of revealing unpublished findings to hundreds of other scientists at once: "For the first scientists who agreed…to run this risk, the venture is comparable to what was faced by the first pilot who broke through the sonic barrier. [Yet] only one complaint of 'failure to give credit' has occurred. Hardy indeed would be the individual who would attempt to claim credit for another man's discovery, with a jury of up to two or three hundred of his peers frowning down upon him" (Albritton, 1965, 10-11). The NIH report was also very careful to assure its readers that the commercial periodical structure retained its power of editorial gatekeeping. To preserve the egalitarian ideal of sharing, the IEGs allowed membership to "any scientist who is a bona fide researcher in the field." All members received updated membership lists, and they all received the numerous photocopied memos which were circulated, about 90% of which were preprints of papers that were meant to be published, eventually, "with or without change..." (Price and Beaver, 1966, 1011-1012).

Theoretical High Energy Physics (HEP)
The primary centrifuge for the emergence of a formalized system of preprint exchange in the sciences can be located within the information exchange priorities and initiatives of the high energy physics (HEP) community during the 1960s. The Stanford Linear Accelerator (SLAC) Library was founded in 1962 with the charge by its director W. Panofsky to actively and promptly acquire preprints in high energy physics, to catalog preprints full and promptly, and to include every author regardless of how many there were. Luisella Goldschmidt-Clermont of CERN—probably the first preprint librarian in history—served as advisor and mentor to this project from the beginning (Addis, 2002). At the same time, the Deutsches Elektronen-SYnchrotron (DESY) in Hamburg began compiling the published and unpublished HEP research literature it received and hired physicists to assign keywords for them (Gunnarsdóttir, 2005, 551). Additionally, the *Directory of Published Proceedings*, one of which was devoted exclusively to "Science, Engineering, Medicine, and Technology," was published by InterDok Corp. as a means of dealing with two to three year publication lag that had started occurring with formal journal publication for many fields in the early 1960s (Kronick, 1985, 36). Finally, Charles Gottschalk of the U. S. Atomic Energy Division proposed the PIE in 1965, as well (Moravcsik, 1966, 62).

In 1966 a study of *The Role and Distribution of Written Informal Communication in Theoretical High Energy Physics* that had been conducted for the U. S. Atomic Energy commission by M. J. Moravcsik of Lawrence Radiation Laboratory, Livermore in response to Gottschalk's proposal the previous year was published. In this study, written informal communications (WICs) in theoretical high energy physics were "loosely referred to as 'preprints' since most of them appear to be drafts and manuscripts of papers intended for eventual publication," (Libbey and Zaltman, 1967, 1). The report recognized the advent of "preprint librarians" as a distinct class because "much of the information needed was not the kind a person could be expected to know without spending some effort to gather and organize the necessary data" (8-9). The findings were based on interviews conducted at the 13th International Conference on High Energy Physics at Berkeley (13) and a couple of surveys that were sent to "36 U.S. and 47 foreign institutions identified as having a preprint collection for the use of theoretical high energy physics" in 1966 (Maguire, 1967, C-1).

The findings of the report included the following observation: "Whether a person is working in one of the so-called 'classical' areas in high energy theory or in a new, or a 'hot' area may well determine how concerned he is with the timeliness of the information reaching him" (Libbey and Zaltman, 1967, 27). Despite the claim that physicists would object to a flood of written informal communication, the report stated that they wanted to be on as many distribution lists as possible in order not to miss anything, and that "there is every reason to believe than an intelligently designed and conducted selective dissemination scheme would eventually be able to satisfy its users that they would receive the documents of greatest interest to them so that in the long run they would elect to receive the smaller number of higher relevance documents" (70). Additionally, the surveys found that the priority claims were not an issue to physicists participating in preprint exchange (71). The report included a cost estimate that lends insight to an essential problem with preprint distribution in the pre-digital age: the estimated number of "written informal communications" per year among the high energy physicists studied was 2300 at a total cost of over $113,000 for printing and mailing from within the U.S. (73).

Moravcsik also engaged in a debate in the pages of *Physics Today* with the editor of the *Physical Review*, Simon Pasternak, on the subject of preprint exchange in the same year the report on informal sharing in HEP was published. (Libbey and Zaltman, 1967, 3, 6). A key point of contention in this debate was the question of whether journals were becoming obsolete due to the rise of preprint exchange. As an editor, Pasternak was staunchly against the rise of preprints. He described the job of journal editing as involving the selection of referees who are not only experts, but are also specifically chosen for the review of specific papers. The unique service that reviewers provide is pointing out "misleading claims, omitted details, ambiguous statements, minor errors in argument, overlooked pertinent references, unrealized implicit assumptions, unrecognized limitations to the conclusions, obscurity, and discursiveness," claimed Pasternak (1966, 41).

Only formal journal publication ensures that an article will be more reliable than the original preprint, he maintained. In his view, the danger of preprint circulation was that "impersonal distributions" essentially constituted the publication of unedited, unproofread papers, which were then open to citation. Furthermore, in more active fields, those who did not have access to the report literature were at a considerable disadvantage in terms of access, as were libraries that would have to strain to track which preprints had been published (Pasternak, 1966, 41-42). Additionally, because preprints were free they also constituted unfair competition to journals. Pasternak believed that if preprint dissemination were allowed to prevail journals would become "mere archival depositories" that would be supplanted in the future by a central machine depository: "Their only virtue would be neater printing. The refereeing system would be destroyed" (42).

In rebuttal, Moravcsik pointed out that preprints grew out of "the age-old custom among scientists of writing letters and other informal communications" and that they have never been outright substitutes for journal articles. According to Moravcsik, the main advantage of the preprint was speed;

another advantage was control over distribution—preprints reached those who are interested in their content, which is a very important asset "in an age of information explosion" (Moravcsik, 1966, 62). He conceded to Pasternak the point that the lack of a formal referee system left pre-published work open to error, but stated that this flaw is inherent to all scientific communication, whether communicated orally or in published print. He used the NIH's IEG experiment as a point of comparison for the PIE experiment to show how costly the service of production and distribution of preprints was (Moravcsik and Pasternak, 1966, 65). Moravcsik concluded by suggesting that preprint exchange actually encourages submission of research to journals because the author has already had a chance to "crystallize" his findings by having communicated them informally with chosen colleagues (Moravcsik, 1966, 67).

He also referred to Swanson's recently published article "On Improving Communication among Scientists," which, in his words, "makes it amply clear that the sole object of improved communication services is improved communication among those who use them. Also mentioned by Moravcsik, was Swanson's "Scientific Journals and Information Services of the Future," which addressed the "extensive use of informal communication channels" that raised "serious questions as to the future of the scientific periodical" and proposed that a new kind of 'information center' be created" to deal with the problem (1966, 1005). The solution, in his opinion, rested in part on the "selective dissemination" of research to the "clusters" of scientists that are of "especial importance" because "informal communication by its very nature is restricted to relatively small groups of scientists" through "personal correspondence, and the mailing of preprints and reprints" (1006).

Finally, the close of the decade saw the community-wide distribution of SLAC's weekly list of new preprints, Preprints in Particles and Fields (PPF) beginning in 1969. Hundreds of physicists paid an annual subscription fee to receive PPF weekly by international post. Those in faraway places complained that actual copies of the preprints on the list were not available and that they tended to arrive very late. PPF continued this distribution of hardcopy publication until the fall of 1993. "Dubious and hostile" journal editors were mollified by the publication of a PPF section called "Anti-preprints," that listed journal references for recently published preprints (Addis, 2002).

STRENGTH OF WEAK TIES: THE 1970S

Building on his earlier work with information exchange in psychology, Griffith co-authored a comparative study of the degree of interaction of researchers in various sub-disciplines with their disciplinary peers. Griffith and Miller (1970) found that in the field of psychoacoustics, the two most important and influential researchers were not members of society meetings (the Acoustical Society of America) despite the strong influence they exerted through formal information channels: "An interesting observation is that no preprint exchange group developed among the core persons in this field. Interviewees stated that key researchers maintained such close contact with one another's work and were so familiar with the techniques being used that raw data or conclusions alone were sufficient to keep researchers fully informed of one another's work" (132).

In another area, Griffith and Miller observed: "There is no preprint exchange group within neuropsychopharmacology, although preprints are sent to specific people working on related problems. The group that does research on the behavioral analysis of drug effects…once had a preprint exchange group but such exchange has now ceased to exist. Information needs seem to be mainly satisfied by formal means, especially journals [which] generally have very low publication lags" (1970, 132). Griffith and Miller's observation regarding the strength of ties within sub-disciplines consisting of small numbers of peers predated by only three years what Granovetter (1973) would call "the strength of weak ties." Of main interest here is that an absence of preprint exchange was used as a measure of closeness between members of these communities throughout the study.

The 1970s saw rapid innovation in the information technologies that would abet the consolidation of systems necessary for effective preprint sharing by high energy physicists. During this decade research utilizing the particle accelerator at CERN involved collaborations of more than 50 physicists per project (Knorr-Cetina, 1999, 161). In 1971, Electronic bulletin boards (EBBs)—a rapid means of sharing research findings in some cases months before they would be printed in a journal—became possible due to the creation of File Transfer Protocol (FTP) (Lucas-Stannard, 2003, 2-3). The following year the Science Information Exchange was incorporated as the Smithsonian Science Information Exchange (Smithsonian Institution, 2011). In 1974, the Stanford Physics Information Retrieval System for High-Energy-Physics (SPIRES-HEP) database began. It is estimated that this contained up to 5,000 bibliographic records at a time at the height of its use. SLAC was responsible for annotating the bibliographic records in the HEP electronic database and printed card catalog while also weeding out published or "dead" preprints (Addis, 2002). Moreover, SPIRES and DESY had been collaborating, and by this time physicists in both Europe and North America were subscribing to the regular listings of new preprints in the HEP community that were being generated from a single database (Gunnarsdóttir, 2005, 551). By 1975, the SLAC library was receiving an average of 70 preprints per week (Addis, 2002).

By the mid-1970s librarians at the National Radio Astronomy Observatory (NRAO) were responsible for tracking papers by staff and visitors in order to produce an annual bibliography and to provide additional information for the compilation of statistics on telescope use. This bibliography, known as the "RAPsheet" (Radio Astronomy Preprints), was a biweekly list of new preprints the NRAO received and was heavily used. In response the library staff made available a weekly printout of the entire database, which contained annual lists of preprints organized by institution, and a list of all unpublished papers that had been identified. Online searching of the database was not publicly available because the FORTRAN code in which it was written was too cumbersome to facilitate custom searches (Bouton and Stevens-Rayburn, 1995, 149-150).

By the late 1970s, at least 10,000 scientific conferences were held per year, of which two-thirds published proceedings (Kronick, 1985, 33). In 1977, the *Journal of Chemical Research* was established as a joint venture by the British, French and German chemical societies as a "synopsis journal" that published 1-2 page summaries of reports (Science Reviews 2000, 2013). To close the decade, Latour and Woolgar's 1979 laboratory study found that only 50% of written scientific communications produced in the course of research were being published in journals (Kronick, 1985, 37).

THE STRENGTH OF STRONG TIES: THE 1980S

While other scientific domains continued to struggle with formal publication and review, the 1980s saw the field of high energy physics overtly embed preprint exchange as the main system by which research was shared. By 1980, the SLAC library was receiving an average of 97 preprints per week and in 1982 it became the first at Stanford University to throw out its card catalog (Addis, 2002). In 1981, the Smithsonian Science Information Exchange was unable to support itself from its revenues and was disbanded. Responsibility for the database was transferred to the National Technical Information Service (Smithsonian Institution, 2011). A study by King, McDonald, and Roderer regarding the use of scientific journals in the United States found that somewhat over half the authors of scientific articles were distributing preprints at this time, "with an average of slightly less than one for each author" (Kronick, 1985, 84).

In 1982, due to concern with the integrity of peer review practices across all scientific domains the *Behavioral and Brain Sciences* journal undertook an investigation of referee evaluations of submitted manuscripts. Twelve articles that had been previously published by prestigious academics in renowned journals with high rejection rates were formally resubmitted with fictitious identifying information to the same 38 editors and reviewers of the journals that had originally published them. Of the nine that were not detected as re-submits, eight were rejected (Peters and Ceci, 1982, 187). Following the eight-page report of this study that was printed in the journal, an additional fifty pages were devoted to "Open Peer Commentary," in which the function and importance of preprints relative to a formal review system was introduced by a couple of thoughtful contributors.

Historian Donald Beaver commented in the early 1980s that a problem with formal journal publishing was that editors and reviewers did not know the published literature as thoroughly as members of the invisible colleges. Individual members of each research specialty communicated with reasonably efficiency with one another through preprints and other more personal exchanges at this time (1982, 199). Physicist Michael Moravcsik, long-time advocate of preprint exchange in high energy physics (see above) observed that the peer review system in physics was consistent enough that the internal inconsistencies suggested by the experiment with psychology journals were unlikely to develop in physics publishing. In responding to the commentary, Peters and Ceci noted that preprints of accepted *Brain and Behavioral Science* articles were circulated to a large population of potential commentators that had been selected from a wide variety of sources, including editor and referee recommendations (1982, 226). They added that worldwide circulation of this journal's preprints could be made easier with machine readability and telecommunications, both of which would also make the "transition to an exclusive soft copy format" possible, "if and when the archival scientific literature ever actually elects to do so" (234).

In 1983 the SPIRES-HEP database won an award from the Special Libraries Association, the first time an online database was chosen for this award (Addis, 2002). A tape of the NRAO database was installed at the Space Telescope Science Institute that same year. Named the "STEPsheet" (Space TElescope Preprints), it functioned using a VAX (virtual address extension) system that allowed online searching and, therefore, instant access from any terminal in the institute. Thereafter, NRAO replaced their FORTRAN code with Inmagic information and library services software (that had become commercially available in 1983) and their IBM mainframe with a VAX (Bouton and Stevens-Rayburn, 1995, 150). All astronomy journals, the astronomy sections of general science journals, and meeting proceedings were constantly checked against the database. Newly published papers were pulled and any unpublished preprints more than a few years old were removed and retained off-list. In the mid-1980s, STEPsheets and RAPsheets began to be exchanged by e-mail, which allowed each of the preprint librarians to edit, proofread and append the lists (151).

By 1985 the SLAC library was receiving an average of 116 preprints per week and the SPIRES-HEP catalog held a total of nearly 12,000 records (Addis, 2002). A program called QSPIRES was developed by the SLAC database systems developer George Crane which allowed physicists to search the SPIRES database using e-mail. For the first time users were able to immediately find all papers by a particular author or institution or find out how often their own work had been cited. Moreover, the PPF list could also be sent by e-mail (O'Connell, 2000). The use of e-mail access spread quickly to more than 40 countries, though it was still impossible to receive an actual copy of a preprint or published paper through this medium.

By the end of the 1980s, however, a scientific typesetting software program became available to members of the HEP community (TeX) that made it possible to produce preprints of presentational quality equal to that of published papers (Gunnarsdóttir, 2005, 552). Nonetheless, preprints continued to be sent by ordinary mail service until the early years of the 1990s. In 1987 Paul Ginsparg approached the SLAC librarian in charge of SLAC-SPIRES, Louise Addis, to consider maintaining an electronic depository of full texts in TeX, but the SLAC at the time did not have the resources to pursue this (Gastel, 2002, 42). By 1988, the NRAO RAPsheets distribution list consisted of 85 internal and outside addresses, all of which received paper copies of preprints. In July of 1988 the database contained 3207 preprints, 1602 of which had not yet been published (Bouton and Stevens-Rayburn, 1995, 152).

ARχIV: THE 1990S

In 1990 the SLAC library was receiving an average of 143 preprints per week (Addis, 2002). It was clear that there was a need to innovate in order to more effectively provide access to users. In this same year, Tim Berners-Lee, a physicist at CERN, created the first web page and Paul Kunz, a physicist at SLAC, brought the idea to the US, in effect creating the World Wide Web (Lucas-Stannard, 2003). Ginsparg became a research staff member at the Los Alamos National Laboratory (LANL) in 1990, and in August of the following year he set up an automated e-mail repository for full text with an alert system that sent new abstracts out daily with instructions for receiving the full articles via an automated request. Meant to serve "friends and colleagues," the original plan was to store roughly 100 full-text articles for three months each "until the existing paper distribution system could catch up. By popular demand, nothing was ever deleted" (Ginsparg, 2011, 145). In Ginsparg's own words: "The original bulletin board was engineered to level the research playing field. It is hard to imagine now, but considerable time and effort was once spent printing, photocopying and posting preprints to a privileged few friends and colleagues, before publication in formal journals" (2011, 146). By the end of 1991 a few thousand people were accessing preprints in arχiv, and in 1993 the project started receiving NSF funding. In 2008 the database surpassed 500,000 records (Gastel, 2002, 42).

DISCUSSION

In 2011 Paul Ginsparg commented, "The idea that print journals had outlived their usefulness was already in the air in the early 1990s. David Mermin memorably wrote in *Physics Today* in 1991: 'The time is overdue to abolish journals and reorganize the way we do business'" (Ginsparg, 2011, 147). The forces at work over the course of the 20th century regarding the need to transfer research quickly between colleagues culminated in the sentiment that the standard means of exchange no longer suited the way in which research in the sciences occurs, particularly in HEP. Each of the scientific disciplines that tried to obviate traditional journal publication by developing alternate systems for exchanging pre-published material between 1940 and 1990 emerged from the 20th century with different cultures of communication. The work of Garvey and Griffith in the 1960s with the APA showed that in the case of the field of psychology the problem of journal lag time had become just as much a problem as it had in the case of high energy physics. In the case of HEP, however, due to the emergence in the 1960s of large number of participants working on a single project, the exchange of preprints became indispensable and has evolved into a formal system that is now embedded in the way HEP research is conducted.

Much ink has been devoted by information professionals and scientists alike to the changing concept of how to deal with the ephemeral and less formal products of research, and, unsurprisingly, the idea of what constitutes a preprint saw many manifestations between 1940 and 1990. The submission of a manuscript to a scientific journal in which pre-published work was shared with a wide range of people not only entailed stages of informality that appeared to constitute publication, there were also written reports of presentations that were given at scientific conferences that made their way into printed proceedings and were sometimes not otherwise published. By the 1960s it was clear that these forms of sharing were not suited to formal publication, not only because journals had limits regarding format, but also because longer work was no longer suitable for formal publication. In the health sciences, preprint sharing did not become as organized and persistent by 1990 as it was in HEP, though NIH's IEG experiments of the 1960s preceded a system of "deposits" that began in the 1970s in which pre-published manuscripts were reviewed and formatted for publication. The British Library Document Supply Center PubMed Central (PMC) is now NIH's digital journal archive and offers public access to papers.

While PMC is not truly a system of preprint sharing, it does point to the main criticism that was leveled at the efforts to share one's research outside of the conventional print system during the mid-20th century—that a system of formal journal publication was essential to ensure quality of research through a system of peer review. The ongoing dispute regarding the relative merits of formal and informal sharing of research that began as a call for the publication of individual articles during the years of World War II and culminated in the advent of online journal publication at the end of the 20th century reveal that, at the very core, the issues around peer review were pitted against the timeliness of the information's availability. During the 1960s and 1970s, when various experimentations with systems of preprint exchange were underway in psychology, physics, and health-related fields, sociologists of science began to offer theories regarding the means of constructing knowledge based on what could be observed in the scientific community. Menzel (1967) saw the scientific community as having fragmented into multiple publics and that alternate systems of sharing had arisen to meet the needs of these new groups. These alternate means were the way in which important research would not be lost. According to a zoologist he interviewed, "it is not enough to put things in a journal. It needs to be pushed… [I]t takes accidents, redundancy, and other imperfections in the communication system to bring about…beneficent results" (62).

In 1981 Knorr-Cetina saw the published paper as "not a final product in any reasonable sense of the word. A published paper is stabilised in print, but not in the discourse into which it is inserted and by which writing is sustained" (106). Preprints, then, became part of the essential process of scientific discourse and part of the "methodical construction" of research in the mid-20th century (Knorr-Cetina, 1981, 121). Reliance on the formal system of exchange was no longer a reasonable expectation because of the way in which scientific research is conducted in particular fields. Knorr-Cetina has referred to the informal mechanism of exchange between researchers as "the personal discourse of (technical) *gossip*...a kind of mangle though which all significant events and entities within an experiment and its relevant surroundings are put" (Knorr-Cetina, 1999, 201). It is this informal "gossip" composed of personal exchanges at conferences, of comments on drafts of papers, and so forth that began to function as a new form of review, despite decades of worry to the contrary.

Of great interest, then, is what is happening at the present time regarding the social organization of non-print, online journals and preprint databases in terms of a system of peer review. One manifestation of this issue is that the traditional structure of the research article has evolved into newer forms of information sharing, via hyperlinks that facilitate, for example, links to data visualizations or of bits of code and other sorts of digitally-produced innovations. There is a pressing need for work in this area, which is beyond the scope of what is offered here. No attempt has been made to offer any theory as to why any of these changes have come to pass, only to represent what key figures have observed and what actions they have taken. As well, no attempt has been made to deeply examine differences between the various scientific disciplines represented here, only that each of them played a significant role in the development of new means of preprint sharing in various disciplines during the five decades which are the focus of this timeline.

REFERENCES

Addis, L. (2002). "Brief and Biased History of Preprint and Database Activities at the SLAC Library, 1962-1994." Last modified: 13 Jan 2002. http://www.slac.stanford.edu/spires/papers/history.html

Agard-Evans, A. B & Farradane, J. (1958). Training the scientific information officer. In *Proceedings of the International Conference on Scientific Information: Two volumes*. National Research Council (Ed.), Washington, D.C.: The National Academies Press. Retrieved from http://www.nap.edu/catalog.php?record_id=10866

Albritton, E. C. (1965). *The information exchange group: An experiment in communication*. Washington, D.C.: Office of Research Accomplishments, Division of Research Grants, National Institutes of Health. Retrieved from http://saltworks.stanford.edu/assets/py379dm4170.pdf

Andriesse, C. D. (2008). *Dutch messengers: A history of science publishing, 1930-1980*. *Library of the written word, volume 7.* Boston, MA: Brill.

Beaver, D. D. (1982). On the failure to detect previously published research. In Peer review practices of psychological journals: The fate of published articles, submitted again, D. P. Peters and S. J. Ceci (Eds.), *The Behavioral and Brain Sciences 5*(2): 199-200. doi: 10.1017/S0140525X00011183

Bernal, J. D. (1960). Scientific information and its users. *ASLIB Proceedings, 12*(12): 432-438. doi: 10.1108/eb049779

Bouton, E. N., & Stevens-Rayburn, S. (1995). The preprint perplex in an electronic age. *Vistas in Astronomy*, *39*(2), 149-154. doi: 10.1016/0083-6656(95)00068-X

Cleverdon, C. (1958). The evaluation of systems used in information retrieval. In *Proceedings of the International Conference on Scientific Information: Two volumes*. National Research Council (Ed.), Washington, D.C.: The National Academies Press. Retrieved from http://www.nap.edu/catalog.php?record_id=10866

Clapp, V. W. (1954). The problem of specialized communication in modern society. In M. E. Egan (Ed.), *The Communication of Specialized Information: Papers Presented before the 17th Annual Conference of the Graduate Library School of the University of Chicago, August 11-15, 1952*. Chicago, IL: University of Chicago Press.

Garvey, W. D. (1979). *Communication: The essence of science*. New York, NY: Pergamon Press.

Garvey, W. D., & Griffith B. C. (1967). Scientific communication as a social system. *Science 157*(3792): 1011-1016. doi: 10.1126/science.157.3792.1011

Gastel, B. (2002). From the Los Alamos preprint archive to the Arχiv: An interview with Paul Ginsparg. *Science Editor*, *25*(2), 42-43. Retrieved from http://www.councilscienceeditors.org/files/scienceeditor/v25n2p042-043.pdf

Ginsparg, P. (2011). Arχiv at 20. *Nature*, *476*(7359), 145-147. Retrieved from http://www.nature.com/nature/journal/v476/n7359/full/476145a.html

Granovetter, M S. (1973). The strength of weak ties. *American Journal of Sociology, 78*(6): 1360-1380. doi: 10.1086/226707

Griffith, B. C. (1969). *Reports of the American Psychological Association's Project on Scientific Information Exchange in Psychology, volume 3* (NSF Grant GN-547). Washington, D.C.: APA, NSF Office of Science Information Service.

Griffith, B. & Miller, J. A. (1970). Networks of informal communication among scientifically productive scientists. In *Communication among scientists and engineers*. C. E. Nelson and D. K. Pollock (Eds.), Lexington, MA: Heath Lexington Books.

Gunnarsdóttir, K. (2005). Scientific journal publications: On the role of electronic preprint exchange in the distribution of scientific literature. *Social Studies of Science, 35*(549), 549-579. doi: 10.1177/0306312705052358

Hoole, D. (2006). The changing geography of an industry and a debate. In A. de Kemp, E. H. Fredriksson, B. Ortelbach (Eds.), *Academic publishing in Europe: The role of information in science and society*. Amsterdam, Netherlands: IOS Press.

Kling, R. (2003). The Internet and unrefereed scholarly publishing (Working paper no. 03-01, draft V 7.1R). Prepared for *Annual Review of Information Science and Technology, volume 38*, B. Cronin and D. Shaw (Eds.) Retrieved from: http://scholarworks.iu.edu/dspace/bitstream/handle/2022/166/WP03-01B.html

Knorr-Cetina, K. (1981). *The manufacture of knowledge: An essay on the constructivist and contextual nature of science*. (Pergamon International Library of Science, Technology, Engineering and Social Studies). Oxford, UK: Pergamon Press.

Knorr-Cetina, K. (1999). *Epistemic cultures: How the sciences make knowledge.* Cambridge, MA: Harvard University Press.

Kronick, D. A. (1985). *The literature of the life sciences*. 1985. Philadelphia, PA: ISI Press.

Libbey, M. A., & Zaltman, G. (1967). *The role and distribution of written informal communication in theoretical high energy physics*. *Report No. NYO-3732-1 (Rev.)*. New York, NY: American Institute of Physics, U.S. Atomic Energy Division.

Lucas-Stannard, P. (2003). Preprints in scholarly communication: Lessons from high energy physics. Librarian, NASA Glenn Research Center. Unpublished paper retrieved from http://paigestannard.blogspot.com/2006/06/publications-and-writing-samples.html

Maguire, C. (1967). Research in informal communication among high energy physicists: Study of existing network. In *The role and distribution of written informal communication in theoretical high energy physics*. Report No. NYO-3732-1 (Rev.). New York, NY: American Institute of Physics, U.S. Atomic Energy Division.

McNinch, J. H. (1948). The Royal Society Scientific Information Conference, London, June 21-July 2, 1948. *Bulletin of the Medical Library Association, 37*(2): 136–141.

Menzel, H. (1967). Planning the consequences of unplanned action in scientific communication. In *Communication in science: Documentation and automation*. A. de Reuck and J. Knight (Eds.) London, UK: J. & A. Churchill Ltd.

Moravcsik, M. (1966). A debate on preprint exchange—rebuttal: Some comments on Pasternak's criticism. *Physics today, 19*(6): 71-73. doi: 10.1063/1.3048306

Moravcsik, M. J. (1982). Rejecting published work: It couldn't happen in physics! (Or could it?). In Peer review practices of psychological journals: The fate of published articles, submitted again, D. P. Peters and S. J. Ceci (Eds.), *The Behavioral and Brain Sciences 5*(2): 228-229. doi: 10.1017/S0140525X00011183

Moravcsik, M. & Pasternak, S. (1966). A debate on preprint exchange. *Physics Today, 19*(6): 60-69. doi: 10.1063/1.3073756

O'Connell, H. B. (2000, February). *Physicists thriving with paperless publishing*. SLAC-PUB-8357 Invited talk at the AAAS annual meeting and science innovation exposition, Washington D.C. Retrieved from http://www.slac.stanford.edu/pubs/slacpubs/8000/slac-pub-8357.html

Pasternak, S. (1966). Is journal publication obsolescent? *Physics today, 19*(5): 38-43. doi: 10.1063/1.3048275

Peters, D. P. & Ceci, S. J. (1982). Peer review practices of psychological journals: The fate of published articles, submitted again. *The Behavioral and Brain Sciences 5*(2): 187-255. doi: 10.1017/S0140525X00011183

Phelps, R. H., & Herling, J. P. (1960). Alternatives to the scientific periodical: A report and bibliography. *Unesco Bulletin for Libraries, 14*(2), 61-75.

Price, D. D. S. & Beaver, D. D. (1966). Collaboration in an invisible college. *American Psychologist, 21*(11): 1011-8. doi:10.1037/h0024051

Science Reviews 2000. *Journal of Chemical Research*. (2013). Retrieved from http://www.sciencereviews2000.co.uk/view/journal/journal-of-chemical-research

Smithsonian Institution. SIA RU000482, Smithsonian Science Information Exchange, records, 1946-1983. (2011, July 11). Retrieved from http://siarchives.si.edu/collections/siris_arc_217048

Swanson, D. (1966). Scientific journals and information services of the future. *American Psychologist, 21*(11): 1005-1010. doi: 10.1037/h0024061

Taube, M. (1953). Implications for professional organization and training. *American documentation, 4*(3): 122-125. doi: 10.1002/asi.5090040307