

## Publication and the Future of Knowledge

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It is a great pleasure to address you this morning, and I thank the program committee for the invitation. I want in the next 50 minutes to do three things. The first is to give an overview of the scholarly knowledge system over the last eighty years. The second is to give a similar overview of the scholarly publication system. The third is to distill from these two histories some issues that face us today. I should warn you that I shall argue that our major problems antedate the current technological revolution.

Electronic wizardry is the occasion of our disease, not its cause.

Let me begin with some definitions and caveats. By scholarly knowledge I mean the body of work produced by professional academics and other full-time researchers. By scholarly publication I mean books, journals, monograph series, and electronic postings produced by and consumed by these people. In terms of areas, I shall talk principally about the humanities and the social sciences, what I will call HSS for short. I set this limit for two reasons. First, it is in these fields that university presses are most dominant. Second and more important, HSS knowledge evolves by different rules than does natural science knowledge. It is not clear, for example, what cumulation and progress mean in fields like sociology and anthropology, much less in fields like

literary studies or philosophy. That being the case, we should not cram HSS knowledge into the ill-fitting framework of science, much less into the impoverished notion of scientific knowledge that obtains in much of today's apocalyptic about publishing, libraries, and digitization. Apparently those authors have not read Thomas Kuhn. The citation indexes tell us that Kuhn's book has been cited 14,796 times as of sixteen days ago. That works out to one citation every 28 hours for the last forty-six years. I suppose the book wasn't in Current Contents, so the doomsayers must have missed it.

## I

Let me turn, then, to my first topic, the evolution of the scholarly knowledge system. There is little social scientific study of how HSS knowledge is actually produced. The historians have chronicled various periods of various disciplines, but the sociologists who have so ruthlessly anatomized natural science have devoted almost no attention to the humanities and social sciences. There have been only a few well-meaning surveys by information scientists, who have been horrified to discover that HSS researchers - at least as the surveyors see it - take out books in a random order, read them haphazardly, and then write vague personal reflections about them.

Given this dearth of evidence, I have myself spent some time prospecting in the historical record, so I can summarize for you a large paper I have just completed on the history of scholarly practices among HSS scholars in the United States over the last century. I apologize for the omission of Canada,

but comparable data are not available. You will note that there is a good deal of demographic information in my story; demography - it should be no surprise - is the central force in the evolution of scholarship in the twentieth century.

In the years before the First World War, the entire PhD cadre of HSS researchers in all fields probably numbered about a thousand. The HSS fields were overwhelmingly library-based, with a few outliers like archeology and anthropology drawing on other kinds of data. Most research was done in a handful of universities, all of them in or near the great library cities of Boston, New York, Washington, and Chicago. Professors and graduate students did their research side by side in departments that had their own office space and, most often, departmental libraries immediately at hand. Acquisitions for these departmental collections were in faculty hands.

Although we often think that our present electronic tools are uniquely powerful, these scholars worked in a surprisingly rich reference environment. In periodical bibliography, the Readers Guide and its scholarly equivalent the International Index date from this period. As for books, although book bibliography was hindered by the lack of a national classification standard, most American libraries had rejected the continental model of shelving by acquisition numbers and had followed Dewey's lead in relative - that is, subject-driven - shelving. This meant that physical browsing by subject - a research advance quite as revolutionary as Google - was possible throughout American university libraries. As for archives and document bibliography, US

government documents had better indexing then than they would at times later in the century, and comprehensive lists of special collections were already available.

In the interwar years, this scholarly world changed dramatically. First of all, it expanded to well over ten thousand by the Second World War. However, disciplines themselves remained small enough - typically numbering about 1500 to 2000 - for faculty to know virtually everyone in their fields. They could know all dissertations going forward across their discipline should they wish to, and, indeed, could in practice read all new work in their entire discipline should they so desire. It was this expanding generation of scholars that stabilized the modern scholarly publication system, about which I will be speaking in a few minutes.

This newly strong and confident academia, however, saw a transformation of research habitus. HSS faculty lost their bid to retain departmental libraries, defeated by centralizing university librarians armed with the twin rhetorics of efficiency and generalism. Departmental libraries survived but only in centralized settings, where they were of much less use to faculty. To balance this loss, the librarians produced a number of new tools. In periodical bibliography, the Union List finally appeared, guiding scholars to periodical sources nationwide. For books there came the first stirrings of the NUC, the regional depository catalogs, and an interlibrary loan system. For archives and documents, LC began a regular census of manuscript collections, the PRO finally issued a serious guide to its holdings, and the Document

Catalog continued as a solid index to American gov docs.

But the big story of the interwar was the explosion of specialized bibliographies and tools, generated by scholars - or sometimes scholarly librarians - for research use. The immense London Bibliography of the Social Sciences, the AHA Guides to Historical Literature, and the MLA annual bibliographies are all examples. The similar PAIS Bulletin was to be sure produced by librarians, but they were special librarians in a research library setting and their product was aimed largely at the research market. All these tools in fact bypassed things like the Reader's Guide and the International Index, which in effect became tools for general readers rather than scholars. This period thus produces the first clear evidence of a division between the scholars and the librarians, the scholars favoring specialized tools and departmental libraries, the librarians universalist tools and centralized libraries. Moreover, in this period the social sciences began emancipating themselves from libraries altogether, turning increasingly to ethnographic, survey, and quantitative methods.

After the Second World War, the system changed again. Academia ballooned, doubling twice in two decades. It lost its human scale and, in most cases, also began to lose touch with its past; a world in which so much research appeared so fast inevitably forgot older work overnight. Specialization grew rapidly as a means of dealing with this flood of material. It might still be possible to know most of the scholars in one's specialty, but not in the discipline. Similarly one could know about dissertations in one's specialty

but not discipline-wide. Predictably, specialist journals flourished.

As for general information tools, it was in the 1950s that abstract journals finally began to appear in the social sciences and even the humanities, although the latter were not very successful. A massively expanded Union List in the mid 1940s no doubt helped scholars locate unusual materials, and the publication, at last, of a version of the NUC meant that the interlibrary loan process became somewhat easier. In the late 1950s the National Union Catalog of Manuscript Collections finally began to provide systematic guidance on archival holdings, although as with the national list of dissertations that had arrived in the 1930s, the tool was too little too late; historians had long since turned their graduate students towards local topics and local archives. US Government documents meanwhile limped along under the miserable Monthly Catalog. In summary, the universalist side of the reference system expanded, but with mixed to negative reception among scholars.

Meanwhile, specialized reference tools continued to explode. A typical example is the UNESCO-sponsored *Current Sociology*, of which each monthly issue comprised a massive review essay and an equally massive bibliography, both produced by a specialist researcher. Such tools clearly dominated research practice. And by this time (the 1950s), we have hard evidence that researchers had deserted the librarians' general purpose tools; surveys showed they got their bibliographical references from hearsay and from other people's bibliographies and reference lists. Scholars, that is, had no interest in the

universal indexes that librarians touted as the answer to the knowledge explosion. They might occasionally use the Union List and the printed NUC in detailed work, but their preliminary bibliographical work and a good deal of their focal library research work was done with specialty tools, many of which they would have owned personally through subscription or purchase. It is also in this period that the paperback book emerged, which enormously increased the ability of scholars to own both current and classic texts, with their rich bibliographies. By this time many social scientists and not a few humanists were nearly independent of libraries for most of their basic research materials: between personally gathered data, subscriptions to major journals, and substantial paperback collections of research monographs and classic literature, they had most of what they needed in their own offices, just as their predecessors had had before the First World War.

In the postwar era library research thus became a yet larger enterprise, and HSS scholars completed their emancipation from the librarians' core reference and bibliographical tools, which they henceforth used only when absolutely necessary. The further evolution of those tools was thus in many ways irrelevant to their enterprise. As a result, when the 1970s brought the social science and arts and humanities citation indexes from ISI, library research scholars were not particularly interested. These were truly universal indexes, quadrupling the coverage of the Wilson bibliographies and replacing non-specialist human indexing with automated KWIC indexes in a kind of race to the bottom. But most library research scholars used these indexes seldom if

ever. They had long since decamped to specialist tools, many of which they owned themselves. When they needed more bibliography, they went to major recent monographs or to specialist bibliographies. The librarians' core system was a tool only of last resort.

But the post-1970s world brought a transition to the specialist tools as well. Half of the HSS dissertations ever written in the history of American academia have been written since 1982, and a third of them since 1995. As a result, the specialized tools and techniques earlier evolved to deal with the overwhelming quantity of research began to fail, because even within specialties the mass of material was now too overwhelming. It was easy enough to simply ignore everything published in third-rate journals, and clearly many scholars began to do that. But even such a tool as other people's reference lists was no longer as good a source of bibliography as it once had been.

The evidence for this is curious, but very strong. We know, first, that the number of references in a typical social science article has gone up by a factor of about five in the last fifty years. Perhaps more important, a set of solid studies from the 1950s tells us that across most fields, at that time, a third of references were to a single page in the cited source and another third to some page range. Today, probably less than ten percent of all references mention any specific page or range at all, even in sources hundreds of pages long. The vast majority of the expanded citation list of the modern article, that is, does not comprise substantive references at all; some of the new citations are there to preempt a reviewer's anger, some to signal

membership in this or that in-crowd, some simply as decorative Christmas balls. All of this destroys the scholarly utility of other people's reference lists, which lay precisely in their selectivity and substance. A similar problem emerged in the immense "bibliographical essays" in the back of all too many monographs, essays which seem to list every book the author has ever thought about or indeed ever heard of.

I should also point out another implication of the decline of specific page citation, one that is crucial for our future. It means - and we need to be frank about this - that there has been over the last fifty years a substantial decline in the seriousness with which scholars are reading each other's work.

More broadly, there appeared in many fields in the 1960s and 1970s a new structure supplementing specialization as a strategy for dealing with the sheer mass of scholarship. I have elsewhere called this structure "generational paradigms," by which I mean specialty-groupings, within disciplines and subdisciplines, that take a particular view of the substantive, methodological, and philosophical debates in their field, and then pursue that view to the exclusion of other approaches. In my own field of sociology, generational paradigms like the labeling approach to deviance and the new sociology of scientific knowledge are common. Across HSS more generally, the best example is the gender paradigm, which - among many other virtues - has legitimated the curt dismissal of whole generations of earlier work and the rewriting of the entire HSS corpus on a new basis. We are even

seeing such things in economics, which is calmly reinventing psychology under the name behavioral economics.

Such generational paradigms are characteristic of most HSS fields. They permit scholars to set aside the huge, unknowable mass of prior work, freeing them for the tasks of reading what they want and of making what is - at least in their own eyes - serious progress. Generational paradigms thus allow scholars to make careers based on novelty in a system in which genuine novelty has become difficult if not impossible for anyone willing to carefully search and receptively read the prior scholarly record.

Generational paradigms typically last for about twenty-five years, starting from a few major statements, then flowering in empirical work as the students of the founders begin to specify the details, then dying in a mass of routine as the intellectual grandchildren of the founders keep going through the meaningless motions. In effect, such paradigms are signs that - as in many systems of abundance - a dynamic of fashion has begun to dominate academic work in the humanities and social sciences. It may well be that these fields are, in some sense, mature to the point of decadence.

The crisis in the HSS disciplines thus long antedates the internet and the digital library. It grows out of processes continuous since the 1920s - the rapid expansion of the academic population and the equally rapid development of location and access tools. The librarians counted on indexing to save the day and guide the investigator through the welter that comes with these increasingly powerful tools. Their central metaphors have always been

scientific, their poster children for success have always been the natural sciences, and their aim has been to make of the library a universal identification, location, and access machine. The digital information world is in that sense simply the latest version of a quite familiar librarian program, one that HSS scholars rejected eighty years ago and that has consequently played a surprisingly small role in the triumphant march of HSS scholarship in the past century.

By contrast, the HSS researchers, although they relied to varying degrees on libraries, started withdrawing from this universalist project in the 1920s and gradually created specialty tools and specialty research practices that enabled them to bypass the hugely inefficient searches that were the only possibility under the universal bibliographical system. By the 1950s and 1960s this alternate system of specialty tools and practices was mature. It could therefore survive the race to the bottom that culminated in the ISI databases on the one hand and WorldCat on the other. But after 1970, it too became the victim of its own success and began to degenerate from scholarship into fashion. Generational paradigms began to dominate HSS scholarship, as the crisis of abundance led to loss of direction. This process was crowned by the anti-canonical debates, which papered over the real problems with a rhetoric of democratization and, quite often, anti-intellectualism. Interestingly enough, the same rhetoric of democratization would become central to the discourse about the digital informational world, which has also its own strong strain of anti-intellectualism.

## II

So much, then, for the basic history of HSS scholarship as an enterprise. What about the scholarly publication system in which that enterprise recorded its output?

The story of the scholarly publishing system in the last century is a story on the one hand of great stability and on the other of great change. I start with the stabilities, for given the rapid expansion and transformation of HSS academia, these are quite surprising.

First of all, since the 1920s, when the scholarly publication system took its present institutional form, it has had pretty much the same cast of characters doing pretty much the same kinds of things. Scholars still figure in the system as authors, referees, and readers. Authors still send manuscripts to editors with varying but usually minimal degrees of encouragement and solicitation. Editors still choose among manuscripts on the basis of quality, market, and - in the case of books - expense. Editors still take advice from unpaid referees. Two principal types of publication still dominate - the journal article and the freestanding book. Scholarly societies still sponsor many journals, which are printed on consignment - and sometimes formally published - by commercial or university presses. Both kinds of presses still also publish independent journals and both kinds of presses still publish scholarly books.

This stability in actors and roles in scholarly publishing is quite

surprising given that university presses were as of 1920 a largely untried innovation, only recently evolved out of a plethora of prior publishing arrangements. A whole variety of book-related tasks - printing, distribution, purchasing, sales, editorial preparation - were all floating around in the institutional stew of the early universities, partially out in highly diverse ways among presses, bookstores, libraries, and departments. But by the mid 1920s, the exchange and purchasing functions had settled into the library (and occasionally the bookstore), while the rest settled into the university press in its full-blown, imprint-editorial-printing-distribution model. By 1927 roughly a quarter of scholarly periodicals were published by university presses outright and many of the rest were printed by university presses on consignment. Of scholarly books, universities published about 40%, to commercial publishers' 25% and about 10% each from government, scholarly societies, and research institutes.

In addition to these stable characters and roles, two other, very important aspects of this system have stayed the same since 1920. One of them is the rate of publication by scholars. Most of us probably believe - I certainly did before looking at the evidence - that scholars publish much more today than in the past. But while there are indeed more articles than ever before, there is no clear evidence that the average American HSS scholar today is publishing more in a scholarly lifetime than scholars did eighty years ago. A wide variety of evidence suggests, rather, that we are publishing the same amount as our forebears.

I won't rehearse the details. But the broad evidence comes from data on the numbers of journals issued and books published. In 1927, University of Chicago Press Director Donald Bean found that the typical HSS field had about one journal per 100 to 150 members in that field's principal scholarly society. There was a new scholarly book for about every 20 to 30 members. J. W. Bowker undertook a similar investigation in the late 1940s and found the same figure: one journal per 100 to 150 society members. In 1975, using ISI figures for journal numbers and World of Learning for society numbers, one finds the following figures for scholars per journal in the various fields: 124 anthropologists, 99 sociologists, 131 political scientists, and 155 economists. In the humanities, we find 63 philosophers, 85 historians, and 128 literature professors. And indeed these figures are about the same today. The book figures are comparable, with perhaps a slight increase.

If we want broader evidence, Carol Tenopir and Donald King - the data mavens of the "scientific information" literature - replicated in 1997 King's 1975 study of the entire "scientific" literature, which included the social sciences as part of the sciences more broadly. Even across the whole of the sciences the overall ratio of the total number of papers to the total number of scientists remained flat. Surveys of scientists, however, found that scientists CLAIM to be writing almost twice as many papers in the 1990s by comparison to those surveyed in the 1970s. How much of that is due to changes in sample frame, how much to the spread of coauthorship, and how much to wishful thinking is not clear.

So the overall fact is that even in the sciences there is no firm positive evidence for an increase in productivity per scholar per unit time. The glut of publishing is a glut of people, not a speedup of production.

So the cast of characters and the rate of publication have stayed the same. A third and quite fascinating stability of the scholarly publication system is that the complaints about it have been constant - indeed virtually identical - for the last eighty years. Let me start again with Donald Bean's 1927 report, which searched the literature and convened a conference of editors and scholars, finding in both places a litany of complaints.

Overproduction of second-rate material (31) was condemned, as was excessive specialization. (32). At the same time there were complaints about inability to publish important scholarly work with small audiences. Scholars also complained about the periodical format itself - they received too many

irrelevant and bad articles for one or two of importance (They wanted JSTOR, you see). Finally, they also complained about timeliness of publication.

Indeed, Bean argued that the emergence and consolidation of university presses in the 1910s and 1920s was essentially a response of universities to the overburdening of the earlier scholarly publication system, which had been based on societies and a few national institutions like the Smithsonian, and which had failed to keep up with increasing scholarly production in the late 19th century (78).

I must say that I was tempted to spend my entire time with you focusing on the stupefying, hilarious stability of these complaints. Here are a few

tidbits. Chester Kerr studied university presses in the late 1940s. What did university presses complain about? That faculty took trade books to commercial publishers and that faculty writing was almost deliberately aversive to the general reader. Sound familiar? What did people think about journals - that were too many journals for the amount of material to be published in them. (187). Have we heard that before?

Ten years later, Rush Welter did a broad survey of views on scholarly publishing. What did he find? University press editors complained about bad writing and poorly structured manuscripts. Authors complained about inadequate review and biased referees. Editors of journals complained about getting too few good manuscripts and too many long, badly written ones. Scholars had their usual complaints, but stressed that journals published a huge amount of bad research. Indeed, Welter remarked somewhat caustically that authors (in his words):

displayed an indifference to much of what is published in their fields of specialization that often seemed to reflect rather an exaggerated trust in what they themselves were doing than an objective evaluation of the many kinds of research that contribute to effective scholarship.

Mind you, these authors who complained about bad referees and hostile editors had two-thirds of their book manuscripts accepted for publication, indeed a whopping 88% of the non-dissertation manuscripts. They had submitted a couple of articles apiece in the last two years and of those articles 87% had eventually been accepted.

In the late 1970s, the ACLS sponsored another major survey of scholars.

The complaints were exactly the same. Authors complained about unfair and delayed reviewing. They squawked about their unpublished work, but their publication numbers suggested that such failure was in practice minimal. Although readers were satisfied with much of what they did read, they nonetheless complained that there was too much bad research published. As for presses, they were looking back to the 1950s and 1960s as a lost golden age and complaining about the small market for scholarly work - the same complaint about unworldly scholars writing unpopular books that appears in all prior reports. At scholarly journals, editors complained bitterly of a glut of bad manuscripts. This seems particularly surprising because many of these were new journals and one would think that new journals - typically founded by insurgent specialists who feel shut out of standard venues - would be happy with their submissions. But nonetheless the complaint was universal.

Such evidence as there is for the present tells the same old story. But I don't need to tell you about the present crisis, because, after all, most of us believe pretty devoutly in a crisis in scholarly publishing. And of course, it's the same old crisis we always have. The university presses are panicked that there aren't enough good, commercially viable manuscripts. The acquisitions editors think academics write long, unreadable, needlessly boring books. The journal editors despair of finding even a handful of good papers. The authors resent the terrible reviewing they get, chafe at delays in publication, and search the literature in vain for work as good as their own. The current crisis is the same old crisis right down to our sense - as editors

or authors or readers - that we "really know that, actually, now, there really IS a crisis." The fact of the matter is that these complaints are structurally given in the nature of the system and are probably signs of health rather than illness.

So three things have stayed the same about scholarly publishing: the cast of characters, the rate of individual publication, and the structurally given set of complaints. Now for what has changed.

To begin with exogenous changes - changes from outside scholarly publication itself. By far the most important is the huge expansion of the population of academics. That the publication system remained more or less the same despite the tenfold increase in academia between the 1920s and the 1970s is amazing. There were also some important technological changes. By erratic jumps, printing became steadily cheaper over the century. The popularization of microfilm in the 1930s ended the earlier practice of routinely publishing dissertations and provoked a brief euphoria - very much analogous to the present one - of utopian expectations about universal accessibility of published material. Perhaps more important, the rapid deployment of paperbacks in the 1950s not only enabled scholars to own a far larger number of books, but also gave presses a cheap way to market backlists, a very happy result for both sides until the emergence of an effectively national second-hand market via the online world began to sour it for the presses.

Within the system itself the various trends are best understood in terms of the actors involved. Among university presses the most obvious change was

the steady increase in sheer output as well as in the share of scholarly books printed. Even by the 1930s, university press titles had moved up to 7% of all new non-fiction titles. By 1950 new university press books were 50% of new scholarly books. Over the last fifteen years, in my own university's acquisitions, books from the top five university presses outnumber those from the top five commercial presses by about 30%.

Another change in university presses was the rise of the search or acquisitions editor in the 1950s, part of an increasing competition AMONG university presses for both manuscripts and staff. Although often seen as a sign of weakness in the system - search editors were to ferret out those very few good manuscripts - in fact the subsequent record makes it clear that this was a sign of growth and health. A correlative change was the move away from local faculty as authors and sponsors of books, another sign of robust growth. A final change in university presses, again one that took off after the Second World War, was international marketing. By the late 1950s, international sales were running 15% of total sales at the bigger American presses.

For journal editors, the century also brought major changes. The most important is the steady founding of new journals. In the 1930s and 1950s the chief mechanism appears to have been specialization: most new journals represented specialized constituencies. Later on - once specialization had run its course to the point, sometimes, of absurdity - new journals more often reflected new methodologies or new paradigms, as I noted earlier. The perennial editorial complaint about bad manuscripts also called forth various

innovations. Submission fees spread steadily from the 1960s onward. Page charges were often discussed but never implemented. More commonly, editors simply rejected some bad manuscripts without review, although it is not clear whether this has become general.

Authors also changed their behavior in important ways over the century. One change was the move away from local presses as book publishers, partly in pursuit of broader choice, partly to signal status. Another major change was the rise of the edited volume. Edited volumes became very useful for the launching of generational paradigms, because the presses' demand for thematic unity outweighed the potential readers' demand for uniformly high quality. For senior authors in particular, edited volumes bypassed the tediousness and unpredictability of journal-based peer review.

Authors changed strategies toward journals as well. Senior authors promoted research assistants to the coauthor list, in order to help the latter with their job search. But HSS authors do not seem to have followed - at least until very recently - the practice now dominant in the biological sciences of slicing articles into smaller and smaller pieces to increase the sheer numbers of items on their vita.

I have said little so far about reviewing although reviewers are a crucial part of this system. Indeed, it is here, I think, that we may indeed face a new kind of crisis, although given my argument so far, that's a rash thing to say. At first quite informal, reviewing became formal - indeed normative - by the 1970s. This change meant that reviewing began to consume an

immense amount of professional time. At one journal per 100 to 150 people and at an average of from 100 to 150 submissions per year per journal and a median of three readers per manuscript, the average scholar is submitting one paper a year and reviewing three others. The book trade was the same. Even using numbers from the 1960s, one finds that roughly one in six academics was reading at least one full book MS each per year. Given that reviewing for both journals and books was in practice quite concentrated at the top of the prestige hierarchy, the numbers were no doubt much, much higher for leading scholars; consulting editors at major journals today review fifteen to twenty articles per year, and many senior scholars review three or more book manuscripts a year. Peer review for publication thus placed a quite substantial burden on the scholarly time of the best scholars. In practice, people are increasingly refusing to do it, in large part because refusing is so easy on email: at the *American Journal of Sociology* we are now routinely asking seven to ten people to get two to three reviews of an article. Five years ago, by snail mail, four to five requests would produce two to three reviews. So much for the utility of running your journal electronically.

We have seen then that certain things about the academic publication system have stayed roughly the same: the cast of characters, the rate of publication per scholar, the types of complaints by various actors in the system. And we have seen that certain other things have changed steadily. University presses raised their overall output and their market share of scholarly work, developed internecine competition, moved from local authorship

toward general acquisition, and built international markets. Editors and boards emerged to found journal after journal, but editors deluged by bad work resorted to submission fees, rejection without review, and other damage control strategies. Authors sought greater prestige in book publishers and moved towards patron/client coauthorship relationships. They pioneered the edited volume both as a refuge from peer review and as a means of sub-paradigm advancement. Reviewers - these same authors under another name - took up the immense task of refereeing all this material as informal strategies for manuscript flow control were delegitimized and the publication system began to take up what became in many cases a professional education function.

Given that some parts of scholarly publication changed rapidly while others did not, there must have been some slip-clutches that permitted an articulation between change and stability.

The first such clutch mechanism involved the university presses, and here, of course, I am not telling you anything you don't know much better than I. It is a simple fact that throughout the period, university presses both came into and went out of existence on a fairly regular basis. Short of such extreme measures, presses could change their subsidy requirements or lessen royalties. Or they could and indeed did shift costs like indexing, permissions, and even - in effect - typesetting onto authors. They could decrease or outsource services like copy-editing. Some of the copy-editing that remained could be implicitly shifted onto authors via the word-processing revolution. As for printing, presses were almost out of the printing business

altogether by the 1950s, and indeed, a large number got out of the fulfilment business later in the century.

Thus, much of what looks from the outside like constancy in university presses conceals a shrinkage of the actual organizations to a hard kernel of acquisitions, bare-bones editorial function, and imprint. But presses have also expanded in good times. Royalties have sometimes gone up. Major projects aimed largely at reputation have been undertaken. New journals have been taken on or old ones released to other publishers or even stopped. Thus, one of the ways presses survive is by growing or shrinking their functions and obligations as the conjuncture moves. This has been a longstanding and regular process, and if it seems to incline towards shrinkage that is not its only direction. This kind of constant adjustment will no doubt continue.

Journal editors have not the same kinds of latitude. They have to produce a product every month or so. Like presses, journal editors have adopted proactive policies. They routinely troll meetings when they are short of papers. Indeed, the standard recourse of desperate journal editors is the special issue, which in effect fills an empty journal by enticing some organized group with a chance to by-pass standard peer review and substitute a more focused and hence more reader-friendly process.

As for authors, unlike presses and journals, they are not corporate entities but individuals, and cannot hunker down to wait for better times. Their strategy for dealing with the erratic nature of the publication experience has been two-fold. First, as I have noted they have through the

century developed a variety of strategies for evading what emerged as classical double-blind peer review: putting together edited volumes or relying on the desperation of editors by selling a group of papers as a special issue. More simply, they have relied on multiple submission. If you roll the dice enough times, you will throw double-six eventually. There is a home for every article and a publisher, believe it or not, for every book.

This phrase captures what is my own main conclusion about scholarly publication. It's actually a matching system, where books and articles are trying to find publishers and journals. Sooner or later everybody gets hitched, but they have some rough romances on the way. In my view, the scholarly publishing system has been extremely robust in historical terms. It has increased output by full order of magnitude without any fundamental transformation of its structure. It manages ups and downs with entry and exit on the periphery, or with trimming or shifting of functions. Although, as I shall say in a few moments, there are some rough patches ahead, the system seems to me quite likely to lurch along for a good while yet.

### III

Let me then try to pull this together into a sense of where we are today. The main conclusion of my first section is that the humanities and social sciences are in their own crisis quite independent of anything publishers can or cannot do. They are basically being crushed by their own success. In any given area, there is - and long has been - far more work, and indeed far more

GOOD work, than anybody can possibly read. Now this is also true in the natural sciences, but the logic of the natural sciences is different. In the natural sciences knowledge is felt to cumulate, and so new work is thought to subsume old work in such a way that there is no need to look at it. All the useful information from the past is thought to be carried forward. To be sure, this is probably not true, but since the scientists all genuinely believe it to be true, the sciences work that way in practice, because as my Chicago predecessor W. I. Thomas once said, if men think situations are real, they are real in their consequences.

But that Frank Kermode didn't write about gender in *The Sense of an Ending* doesn't really mean that scholars should evermore disregard the book as if it believed in phlogiston. Nor does the fact that Levi-Strauss made outrageous simplifications in *The Savage Mind* mean that that book should be consigned to the rubbish heap along with geocentric models of the universe. In the humanities and in much of the social sciences, we don't really think that we are getting closer and closer to some truth, the way scientists think, nor even that our knowledge is somehow steadily improving, replacing bad old knowledge. (To be sure, I won't speak for the economists, whose knowledge, at least according to many of them, is perfect already.)

So the crisis in HSS knowledge is a fundamental one. How ought we to organize and make our way through the fields of knowledge if it is indeed the case that there is unthinkably too much to read and that we are not cumulating, but evolving in some other way? As I said earlier, we got through

the twentieth century with two expedients - specialization and generational paradigms. My own sense is that scholars are finally beginning to realize that even the latter don't work, in the sense that generational paradigms are not really a legitimate or valid way to deal with the problem of abundance. They are just ad hoc agreements to ignore some huge body of material.

But you are yourselves no doubt more interested in what I think are the implications of this crisis of abundance for scholarly publishing. Here I can only speculate.

A first implication seems clear; one of the things that overload promotes is star systems. That is, one way to reduce the amount everybody has to read is to agree that there are stars and that their work is all we need to read. I earlier mentioned the book that is probably the most influential single work in HSS in the last fifty years - Kuhn's *Structure of Scientific Revolutions*. It's a great book about scientific knowledge but then so is Michael Polanyi's *Personal Knowledge*, and the latter has been cited only three thousand times that is, once for every five citations to Kuhn. But any serious reader will tell you that these are both superb books, that it is not the case that Kuhn's is better, much less five times better. Yet Kuhn is the star.

Now obviously, if the academics need or use a star system to manage their overload, that has implications for publishers. And indeed, it was probably publishers' own belief in stars that drove them to create acquisitions editors in the 1950s. But the belief in the home run doesn't actually lessen your need to invest in singles. Look at the drug companies - their bottom lines are

driven completely by star drugs, but this just leads them to invest more and more heavily in huge brute force searches of likely suspects - the analog of publishing all those wacky monographs on the hope that one of them will turn out to be *Montaillou* or *The Lonely Crowd* or *Political Man*.

So the academics' use of a star system just forces publishers to prospect more and more broadly. The same thing, by the way, obtains for journal articles. Over 10% of all citations to the *American Journal of Sociology* in any given year are to five classic articles out of the more than seven thousand that have appeared in the journal's history. Since nobody knew at the time those articles were published that this would happen to them, we had to publish all those others just to roll the dice enough to get a few major hits.

A second aspect of the abundance crisis with implications for publishing involves the balance between publishing and other forms of presentation. The decreasing importance and impact of the single publication, along with a number of other more general shifts in the culture, seem to me to be shifting much of academics towards a performance ethic. This means that the article or book is regarded as a performance of some idea or ideas that are in repertoire, so to speak, both the repertoire of the author and more broadly of his discipline or subdiscipline. Less and less do we think that the idea is fixed in place, once for all, by the act of writing, as it would be in the natural sciences, unrepeatable now that someone has done it once. Particularly if the ideas involved are relatively general or abstract, they are likely to be quite familiar, to be old ideas that are being performed in new contexts

and in new ways, like repertory plays.

This performance ethos implies that the actual performance of the text -

that is, the talk or lecture or seminar - may well be seen as more important

than the publication of the final version. I note a number of signs of this.

In economics, for example, publication is now the last, rather uninteresting

phase of an article's life. If it is any good, everybody in the discipline has

long since seen it on a website, heard it as a talk, and probably joined in a

discussion of it online. At my own journal, we sometimes find authors not

bothering to send us the final manuscripts of their papers because the only

thing they really cared about was the acceptance to put on their vita.

Publication, that is, is merely about signals of achievement, not actually

about communication. Communication happens in performance.

Well if that is actually becoming true, then we are indeed in a new world. As long as the career system relies on peer-reviewed publication as its final measure of achievement, those publications - and if necessary the subsidies to maintain them - have to continue, one way or the other. But if achievement were to begin to be measured by some other performance system - say an online approval voting system - the journals would be in trouble in a hurry, not because subscriptions would disappear, but because SUBMISSIONS would disappear. Put another way, it is at present not at all clear that the communication function of the journals would sustain them independently of their achievement-rating function, particularly in a world where the main ideas are held in common and what matters most is the performance of them.

This argument brings us back, of course, to the problem of good knowledge and how it is to be judged going forward. So we are back at my original point: HSS academia is in a crisis, quite independent of what is happening in publication. Solving that internal crisis must come first. We must know what we imagine good knowledge to be before we can imagine a future publication system for it.

Focusing on that particular crisis means that I have ignored other crises in the offing. I would like to close with very brief mention of three other crises that could - indeed probably will - wreak havoc in both the knowledge and publications systems.

The first of these is the culture's science binge. Public veneration of science in America is reaching heights unseen since the 1950s. The biomedical research budget dwarfs all other research funds. The immediate impact has been to shift library acquisition monies from mainstream HSS monographs to highly esoteric and staggeringly expensive scientific journals. This is the so-called serials crisis, which is purely a science crisis, and could be solved tomorrow by separating out the science journal budgets from the main library budgets. One worries, however, that in the current frame of mind such a division might in many universities lead to even greater disinvestment in libraries and HSS monographic holdings. Indeed, my own sense is that the science mania has not yet passed its zenith, and that this crisis will continue.

Another shift, far more dangerous, is the underlying seizure of our intellectual patrimony by capitalists who intend to claim ownership of it and

then resell it to those of us who created it in the first place. Historically, the world of knowledge has been a communist world. We contribute to it as we can, we take from it as we need, but the holdings are essentially public. Especially is this true in the humanities and social sciences where the great ideas are common ideas. But the knowledge world is shifting rapidly toward claims of ownership. Today it may be algorithms and chemical formulae, but tomorrow it could well be interpretations of Locke or theories of religion, if someone could figure out how to make those things produce rental income. A culture whose greatest intellectual achievements include financial instruments that bet on people's future beliefs about even further future housing prices will not blink at the prospect of renting out theories of democracy for use in undergraduate textbooks or charging fees for diagrams of the argument in *The Critique of Pure Reason*. Given the more than one trillion dollars spent on education in America, the attempt to move all of knowledge onto a fee-for-service, rental basis will be extremely strong. It is here, indeed, that the real battle between communism and capitalism will be waged.

Finally, I should mention, as a fellow of the joint computation center of the Argonne National Laboratory and the University of Chicago, that we are rapidly approaching the automation of knowledge. Scholars are already designing algorithms to troll the biomedical and pharmaceutical literatures for faint regularities across thousands of papers, looking for promising leads for further investigation. The extension of such methods to the humanities and social sciences will not be far behind. We are probably not more than thirty

or forty years from having algorithms that can, quite literally, write articles. That being the case, it is time to ask ourselves what is the uniquely human, individual contribution to knowledge itself. Answering this question is indeed the challenge that I think will finally wake the humanities out of the exhausted sleep that seems to have followed the canon wars.

With that apocalyptic note, I will leave you. But I cannot sit down without acknowledging, as a scholar and representative of the academic profession, the great obligation that we owe you who have built and maintained the wonderful publishing system on which we so much rely. It is the university presses that have allowed scholars to continue aiming at the unusual, the novel, and the unremunerative. It is the university presses that have produced the lion's share of the specialty research tools on which we depend. And I am confident that as we face the extraordinary uncertainties of the near future, it will be the university presses that will publish - whatever that is going to mean - the most essential results of our common enterprise. Thank you.