The Slowdown of the Economics Publishing Process

Glenn Ellison

Massachusetts Institute of Technology and National Bureau of Economic Research

Over the last three decades there has been a dramatic slowdown of the publication process at top economics journals. A substantial part is due to journals' requiring more extensive revisions. Various explanations are considered: democratization of the review process, increases in the complexity of papers, growth of the profession, and cost and benefit arguments. Changes in the profession are examined using time-series data. Connections between these changes and the slowdown are examined using paper-level data. There is evidence for some explanations, but most of the slowdown remains unexplained. Changes may reflect evolving social norms.

I. Introduction

Thirty years ago papers in the top economics journals were typically accepted within six to nine months of submission. Today it is much more common for journals to ask that papers be extensively revised,

I would like to thank the National Science Foundation (SBR-9818534), the Sloan Foundation, the Center for Advanced Study in the Behavioral Sciences, and the Paul E. Gray Undergraduate Research Opportunities Program Fund for their support. This paper would not have been possible without the help of a great many people. I am very grateful for the efforts that a number of journals made to supply me with data. Many of the ideas in this paper were developed in the course of a series of conversations with other economists. I would especially like to thank Orley Ashenfelter, Susan Athey, Robert Barro, Gary Becker, John Cochrane, Olivier Blanchard, Judy Chevalier, Ken Corts, Peter Diamond, Bryan Ellickson, Sara Fisher Ellison, Frank Fisher, Drew Fudenberg, Joshua Gans, Edward Glaeser, Daniel Hamermesh, Lars Hansen, Harriet Hoffman, Jim Hosek, Alan Krueger, Paula Larich, Vicky Longawa, Robert Lucas, Kathleen Much, Wally Mullin, Paul Samuelson, Ilya Segal, Karl Shell, Andrei Shleifer, and Kathy Simkanich without implicating them for any of the views discussed herein. Lesley Chiou, Richard Crump, Simona Jelescu, Christine Kiang, Nada Mora, and Caroline Smith provided valuable research assistance.

[*Journal of Political Economy*, 2002, vol. 110, no. 5]
 0022-3808/2002/11005-0002$10.00

and on average the cycle of reviews and revisions consumes about two years. The change in the publication process affects the economics profession in a number of ways: it affects the timeliness of journals, the readability and completeness of papers, the evaluation of junior faculty, and so forth. Most important, the review process is the major determinant of how economists divide their time between working on new projects, revising old papers, and reviewing the work of others. It thus affects how productive the profession is as a whole and how enjoyable it is to be an economist.

This paper has two main goals: to document how the economics publishing process has changed and to explore why it has changed. On the first question I find that the slowdown is widespread. It has affected most general-interest and field journals. Part of the slowdown is due to slower refereeing and editing, but the largest part occurs because journals require more and larger revisions. On the second question I attribute portions of the slowdown to a few changes in the profession but find that the full magnitude of the slowdown is hard to explain.

Although the review process at economics journals has lengthened dramatically, the change has been gradual. Perhaps as a result it does not seem to have been widely recognized (even by journal editors). Section II provides a detailed look at how review times have grown and where in the process the changes are happening. What may be most striking is that in the early 1970s most papers got through the entire process of reviews and revisions in well under a year. If we go back another decade or two, almost all initial submissions were either accepted or rejected: the noncommittal "revise-and-resubmit" was reserved for exceptional cases.[1]

In the course of conversations with journal editors and other economists, many potential explanations for the slowdown have been suggested to me. I analyze four sets of explanations in Sections III–VI. Each of these sections has roughly the same outline. They begin with a discussion of a set of related explanations, for example, "A common impression is that over the last 30 years change X has occurred in the profession. For the following reasons this would be expected to lead to a more drawn-out review process." They then present two types of evidence. Time-series data are used to examine whether change X actually occurred and to get some idea of the magnitude of the change. Cross-section data at the paper level are then examined for evidence of the hypothesized connections between X and review times. In these tests, I exploit a data set containing review times, paper characteristics, and

[1] An anecdote I find revealing is that a senior economist told me it looks odd to him to see young economists' resumes trumpeting that papers have been returned for revision. When he was young he never would have listed a revise-and-resubmit on his resume because he would have been embarrassed that something was wrong with his initial submission.

author characteristics for over 5,000 papers. The data include at least some papers from all of the top general-interest journals and nearly all post-1970 papers from some of them.

In the explanations discussed in Section III, the exogenous change is the "democratization" of the publishing process, that is, a shift from an "old-boys network" to a more merit-based system. This might lengthen review times for a number of reasons: papers need to be read more carefully, mean review times go up as privileged authors lose their privileges, and so forth. I find little or no support for such explanations. Time-series data on the author-level and school-level concentration of publications suggest that there has not been significant democratization over the last 30 years. I find no evidence of prestige benefits or other predicted patterns in the cross section.

In Section IV, the exogenous change is an increase in the complexity of economics papers. This might lengthen review times for a number of reasons: referees and editors will find papers harder to read, authors will need more help to get things right and will not be able to get it from colleagues, and so forth. Some simple tests support this view. Papers have grown substantially longer and are more often coauthored. Longer papers and coauthored papers take longer in the review process. Together, these effects may account for a couple months of the slowdown. Other tests of complexity-based explanations provide no support. If papers were more complex relative to economists' understanding, I would expect economists to have become more specialized. I do not find such a trend in data on top-journal publications. In the cross section there is little evidence of the other links between complexity and delays. For example, the publication process is not faster for papers handled by editors with more expertise.

In Section V the growth in the economics profession is the exogenous change. There are two main channels through which growth might slow the review process at top journals: it may increase the workload of editors and it may increase competition for the limited number of slots in top journals. Explanations based on increased editorial workloads are hard to support: submissions have not increased much. The competition story is more compelling. Journal citation data indicate that the best general-interest journals are gaining stature relative to other journals, and some top journals are publishing fewer papers. Looking at a panel of journals, I find some evidence that journals tend to slow down as they move up in the journal hierarchy. This effect may account for three or four months of the observed slowdown at the top journals.

Section VI discusses a couple of additional simple arguments. One is that journals may be asking for more revisions because improvements in computer software have reduced the cost of revisions. Another is that more revisions are optimal because the information dissemination role

of journals is less important. One piece of anecdotal evidence that is problematic for these explanations is that the slowdown does not seem to have been intentional.

While I find evidence to support a few explanations, the biggest impression I take away from Sections III–VI is that it is hard to attribute the majority of the slowdown to observable changes in the profession. A common theme of the results seems to be that the economics profession today looks a lot like the economics profession in 1970. This will make it hard to argue that today's review process must be so different.

An intriguing alternative hypothesis is that there may not be any fundamental cause: the slowdown could reflect a shift in arbitrary social norms. Just as papers without any data on technologies attribute unexplained changes in the wage structure to "skill-biased technological change" and other papers attribute unexplained differences in male-female or black-white wages to "discrimination," one could characterize Sections III–VI as showing that the largest part of the slowdown is due to changes in social norms. Such an "answer" to the question of what caused the slowdown is unsatisfying: it recasts incompleteness in our understanding of the slowdown as incompleteness in our understanding of why social norms changed. In Ellison (2002; this issue), I attempt to provide some content to the explanation of changing social norms by developing a model in which social norms would evolve in the direction of emphasizing revisions. Section VIII presents some general evidence on social norms and examines one aspect of this model.

There is a substantial literature on economics publishing. I draw on and update its findings at several points.[2] Four papers that I am aware of have previously discussed submit-accept times: Coe and Weinstock (1967), Yohe (1980), Laband, Maloney, and McCormick (1990), and Trivedi (1993). All these papers but the first make some note of increasing delays: Yohe notes that the lags in his data are longer than those reported by Coe and Weinstock; Laband et al. examine papers published in the *Review of Economics and Statistics* between 1976 and 1980 and find evidence of a slowdown within this sample; and Trivedi examines lags for econometrics papers published in seven journals between 1986 and 1990 and notes that there is a trend within his data and that lags are longer in his data than in Yohe's. Laband et al. also examine some of the determinants of review times in a cross-section regression.

[2] I make particular use of data reported in Yohe (1980), Laband and Piette (1994), and Siegfried (1994). Siegfried (1994), Hudson (1996), and Laband and Wells (1998) provide related discussions of long-run trends in the profession. See Colander (1989) and Gans (2000) for overviews of the literature on economics publishing.

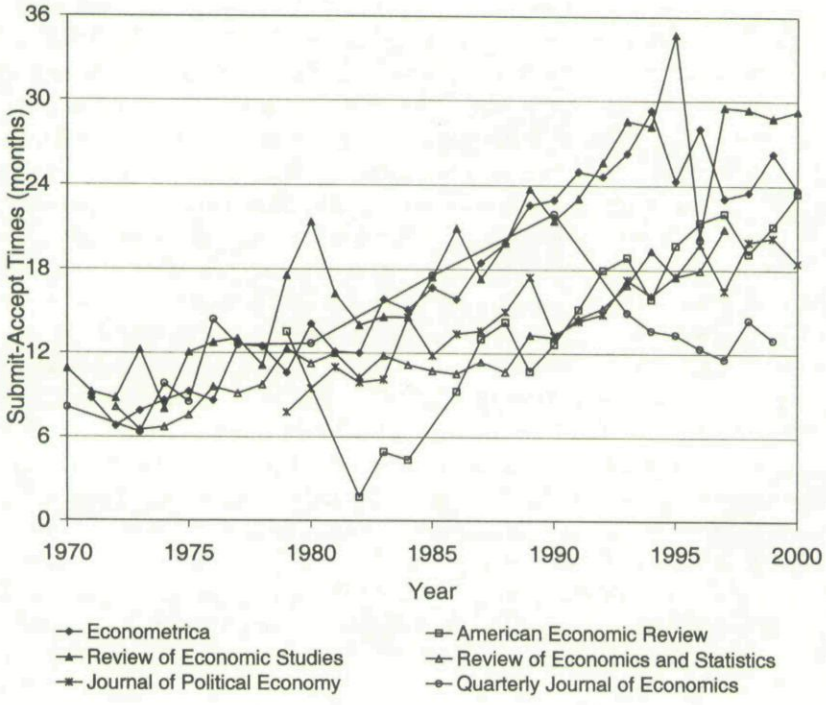

FIG. 1.—Mean submit-accept times for papers in top general-interest journals

II. The Slowdown

This section documents the gradual but dramatic increase in the amount of time between the submission of papers to top economics journals and their eventual acceptance. A large portion of the slowdown is due to journals' requiring more and larger revisions.

A. *Increases in Submit-Accept Times*

Figure 1 graphs the mean length of time between the dates on which articles were initially submitted to several journals and the dates on which they were finally accepted (including the time authors spent making required revisions) for papers published between 1970 and 1999.[3]

[3] The data for *Econometrica* do not include the time between the receipt of the final revision of a paper and its final acceptance. The same is true of the data on the *Review of Economic Studies* for 1970–74. Where possible, I include only papers published as articles and not shorter papers, notes, comments, replies, errata, etc. The series from the *American Economic Review* and the *Journal of Political Economy* are taken from annual reports and

The data cover six general-interest journals: *American Economic Review* (*AER*), *Econometrica, Journal of Political Economy* (*JPE*), *Quarterly Journal of Economics* (*QJE*), *Review of Economic Studies* (*REStud*), and *Review of Economics and Statistics* (*REStat*). The first five of these are among the six most widely cited journals today (on a per article basis), and I take them to be the most prestigious economics journals.[4] I include the sixth because it was comparably prominent in the early part of the period.

Most of the year-to-year changes are fairly small, but the magnitude of the increase over the 30-year period is startling. At *Econometrica* and *REStud,* review times lengthened from six to 12 months in the early 1970s to 24–30 months in the late 1990s. My data on the *AER* and *JPE* do not go back nearly as far, but I can still see submit-accept times more than double (since 1979 at the *JPE* and since 1986 at the *AER*).[5] The *QJE* is the one exception to the trend. Its review times followed a similar pattern through 1990, but with the change of the editorial staff in 1991, there was a clear break in the trend, and mean total review times have now dropped to about a year. I shall discuss below the ways in which the *QJE* is and is not an exception to the pattern of the other journals.

The slowdown of the economics publishing process is not restricted to the top general-interest journals. Table 1 reports mean total review times for various economics journals in 1970, 1980, 1990, and 1999.[6]

B. What Parts of the Review Process Are Slower?

A common first reaction to the data on submit-accept times is to imagine that the story is a breakdown of standards for timely refereeing. It appears, however, that slower refereeing is only a small part of the story. Figure 2 graphs the mean time between submission and the sending of

---

presumably include all papers. For 1993–97, I also have paper-level data for these journals and can estimate that in those years the mean submit-accept times given in the *AER* and *JPE* annual reports are 2.2 and 0.6 months shorter than what I would have computed from the paper-level data. The *AER* data do not include the *Papers and Proceedings* issues. The means for other journals were tabulated from data at the level of the individual paper. For many journal years, tables of contents and papers were inspected manually to determine the article-nonarticle distinction. In other years, rules of thumb involving page lengths and title keywords were used.

[4] The ratios of total citations in 1998 to publications in 1998 for the five journals are *Econometrica,* 185; *JPE,* 159; *QJE,* 99; *REStud,* 65; and *AER,* 56. The *AER* is hurt in this measure by the inclusion of the papers in the *Papers and Proceedings* issue. Without them, the *AER*'s citation ratio would probably be approximately equal to the *QJE*'s. The one widely cited journal I omit is the *Journal of Economic Literature.*

[5] The *AER* data include three outliers. From 1982 to 1984, Robert Clower ran the journal in a manner that was substantially different from the process before or since. I do not regard these years as part of the trend to be explained.

[6] While I focus exclusively on economics in this paper, similar trends exist in psychology, computer science, linguistics, statistics, and some other fields. I present some data on these broader patterns in Ellison (2002; this issue).

TABLE 1
MEAN SUBMIT-ACCEPT TIMES AT VARIOUS JOURNALS

JOURNAL	MEAN TOTAL REVIEW TIME IN YEAR			
	1970	1980	1990	1999
	Top Five General-Interest Journals			
AER		13.5*	12.7	21.1
Econometrica	8.8†	14.0†	22.9†	26.3†
JPE		9.5	13.3	20.3
QJE	8.1	12.7	22.0	13.0
REStud	10.9†	21.5	21.2	28.8
	Other General-Interest Journals			
Canadian J. Econ.		11.3*		16.6
Econ. Inquiry		3.4*		13.0
Econ. J.		9.5*		18.2†
Internat. Econ. Rev.	7.8†	11.9†	15.9†	16.8†
REStat	8.1	11.4	13.1	18.8
	Economics Field Journals			
J. Appl. Econometrics			16.3†	21.5†
J. Comparative Econ.		10.3†	10.9†	10.1†
J. Development Econ.	5.6†‡	6.4†	12.6†	17.3†
J. Econometrics		9.7†	17.6†	25.5†
J. Econ. Theory	.6†	6.1†	17.0†	16.4†
J. Environmental Econ. and Management		5.5†	6.6†	13.1†
J. Internat. Econ.		8.7*		16.2
J. Law and Econ.		6.6*		14.8
J. Math. Econ.	2.2†‡	7.5†	17.5	8.5
J. Monetary Econ.			11.7†	16.0†
J. Public Econ.	2.6†§	12.5†	14.2†	9.9†
J. Urban Econ.		5.4†	10.3†	8.8†
Rand J. Econ.		7.2*	20.0	20.9
	Journals in Related Fields			
Accounting Rev.		10.1	20.7	14.5
J. Accounting and Econ.		11.4†	12.5†	11.5†
J. Finance		6.5*		18.6
J. Financial Econ.	2.6†‡	7.5†	12.4†	14.8†

* Date from Yohe (1980) pertain to 1979 and probably do not include the review time for the final resubmission.
† Does not include review time for final resubmission.
‡ Data for 1974.
§ Data for 1972.

an initial decision letter at the top five general-interest journals.[7] At *Econometrica,* the mean first-response time in the late 1990s is virtually identical to what it was in the late 1970s. At the *JPE,* the latest figure is about two months longer than the earliest; this is about 20 percent of the increase in review times between 1982 and 1999. The *AER* shows about a one-and-a-half-month increase since 1986; this is about 15 per-

[7] The precise definition varies from journal to journal. Details are given in the figure legend.

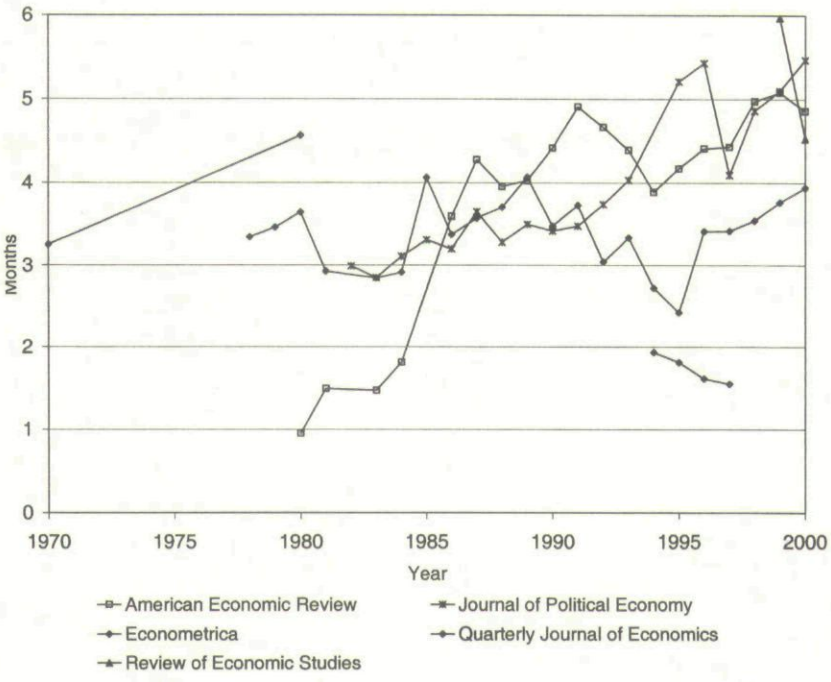

FIG. 2.—Mean first-response times at top journals. The *Econometrica* data are estimates of the mean first-response time for all submissions (new submissions combined with resubmissions). They are derived from data in the editors' reports on papers pending at the end of the year. The year t estimate reflects response times for submissions arriving between July 1 of year $t-1$ and June 30 of year t. Figures for the *AER* are estimated from histograms of response times in the annual editor's reports and also refer to papers arriving in this time period. Figures for the *JPE* are obtained from annual reports. They appear to be the mean first-response time for papers that are rejected on the initial submission in the indicated calendar year. Figures for the *QJE* are mean first-response times for papers with first responses in the indicated year. The 1970 and 1980 numbers were estimated from a random sample of papers. Figures for *REStud* are taken from the journal's web site and reflect first-response times for submissions received in a fiscal year starting March 1.

cent as large as the increase in submit-accept times over the same period.[8] The pattern at the *QJE* is different from that of the others. The *QJE* experienced a dramatic slowdown of first responses between 1970 and 1990, followed by an even more dramatic speed-up in the 1990s. This difference and reviewing many revisions quickly without using referees account for the *QJE*'s unique pattern of submit-accept times.

[8] Again, the figures from the Clower era are not representative of what happened earlier and are probably best ignored.

TABLE 2
FIRST-RESPONSE TIMES FOR ACCEPTED AND OTHER PAPERS

SAMPLE OF PAPERS	MEAN FIRST-RESPONSE TIME IN MONTHS									
	1970	1980	1985	1990	1992	1993	1994	1995	1996	1997
QJE:										
Sent to referees	3.3	4.6					3.5	3.2	2.9	2.7
Accepted	4.8	5.8	7.2	9.0			4.8	3.7	3.2	3.7
JPE:										
Rejected			3.3	3.4	3.7	4.0		5.2	5.4	4.1
Accepted					6.9	6.7	6.9	8.4	10.3	7.8

NOTE.—The first row gives estimated mean first-response times for papers with responses in 1970 and 1980 and the actual means for all papers with first responses in 1994–97 from the *QJE*. (The later means are computed omitting data on papers rejected without using referees.) The second row gives mean first-response times for papers that were eventually accepted. For 1970–90, the means pertain to papers published in the indicated year. For 1994–97, numbers are means for papers with first responses in the indicated year and accepted before August 1999. The third row gives mean first-response times for papers that were rejected on the initial submission by the *JPE* in the indicated year. The fourth row gives means for papers with first responses in the indicated year that were accepted before January 1999.

The data in figure 2 could be misleading if first-response times are different for accepted and rejected papers. Table 2 compares the first-response time conditional on eventual acceptance to more standard "unconditional" measures at the *QJE* and *JPE*. At the *QJE* the two series have been about a month apart since 1970. There is no trend in the difference. At the *JPE* the differences are larger. While only recent data are available, longer first-response times are clearly a significant part of the overall slowdown. For papers published in 1979, the mean submit-accept time was 7.8 months. This includes an average of 3.3 months papers spent on authors' desks being revised, so the mean first-response time conditional on acceptance could not have been greater than 4.5 months and was probably at least a month shorter. For papers published in 1995, the mean submit-accept time was 17.5 months and the mean first-response time was 6.5 months. Hence, the lengthening of the first response probably accounts for at least one-quarter of the 1979–95 slowdown.[9]

There is ample evidence of a second component of the slowdown: journals now require more extensive revisions than they did in the past. The clearest evidence I can provide on the growth of revisions is a time series on the *QJE*'s practices I put together by reading through old index card records. The first row of table 3 illustrates that the slowdown at the *QJE* began around 1960 following a couple of decades of constant

[9] For papers published in 1997, the mean submit-accept time was 16.5 months and the mean first-response time was 9.8 months; the majority of the 1980–97 slowdown may thus be attributed to slower first responses. It appears, however, that 1997 is an outlier. One editor was very slow, and the journal may have responded to slow initial turnarounds by shortening and speeding up the revision process. Note that the figures in the text differ from those in the table because the table categorizes papers by first-response year rather than by publication year.

TABLE 3
REVISIONS AT THE *QJE*

	YEAR OF PUBLICATION								
	1940	1950	1960	1970	1980	1985	1990	1995	1997
Mean submit-accept time (months)	3.7	3.8	3.6	8.1	12.7	17.6	22.0	13.4	11.6
Mean number of revisions	.6	.8	.6	1.2	1.4	1.5	1.7	2.2	2.0
Mean number of revisions before acceptance	.4	.1	.2	.5	.8	1.0	1.7	2.2	2.0
Mean author time for first preaccept revision (months)	1.4	2.1	2.0	2.1	3.0	4.2	3.6	4.1	4.7

submit-accept times.[10] The second row of table 3 shows that the mean number of revisions authors made was roughly constant at around 0.6 from 1940 to 1960 and then increased steadily to a level of about 2.0 today. The *QJE* used to categorize responses to initial submissions into four groups rather than three: "accept-but-revise" was a separate category that was more common than "revise-and-resubmit." Before 1970 "revise-and-resubmit" seems to have been used only in exceptional cases. For example, only five of the papers published in 1960 had received a revise-and-resubmit.[11] The third row of table 3 illustrates that the increase in revisions is even more dramatic if one does not count revisions made in response to accept-but-revise letters.

The sketchy information I have obtained on revisions elsewhere suggests that the *QJE*'s pattern is not atypical. The unpublished 1960 *Econometrica* annual report reveals a process similar to the 1960 *QJE*'s: 45 acceptance letters were sent in 1959, and only four papers were returned for revision.[12] Marshall's (1959) discussion of a survey of the editorial policies of 26 journals never mentions the possibility of a revise-and-resubmit but does mention that authors are frequently asked to revise papers upon acceptance. As for the *QJE*'s current practices being typical, I know that articles published in *Econometrica* in 2000 were, on average,

[10] The fact that it took only three to four months to accept papers in the 1940s seems remarkable today given the handicaps under which the editors worked. One example is that requests for multiple reports on a paper were done sequentially rather than simultaneously: there were no photocopy machines, and the journal had to wait for the first referee to return the manuscript before sending it to the second.

[11] Twelve papers were accepted on the initial submission and 11 initially received an accept-but-revise. The 1970 breakdown was three accepts, 12 accept-but-revises, nine revise-and-resubmits, and one reject (which the author protested and eventually was overturned on his third resubmission).

[12] The four revise-and-resubmits in 1959 followed four in 1958 and two in 1957. In 1955 and 1956, however, the average was 12 per year.

revised 2.04 times prior to acceptance. The available data on the *JPE* and *AER* are less clear, but it appears that multiple revisions are less common at the *JPE*.[13]

Submit-accept times at the top finance journals provide another illustration of the increase in revisions. Although the *Journal of Financial Economics* is rightfully proud that its median first-response time in 1999 was just 34 days (as it was when first reported in 1976), mean submit-accept times have risen from about three months in 1974 to about 15 months in 1999.[14] Similarly, the *Journal of Finance* had a median turnaround time of just 41 days in 1999, but its mean submit-accept time has risen from 6.5 months in 1979 to 18.6 months in 1999.[15]

A final factor contributing to the increase in submit-accept times is that authors are taking longer to revise their papers. The best data source I have on this is again the *QJE* records. The final row of table 3 reports the mean time in months between the issuance of a "revise-and-resubmit" letter in response to an initial submission and the receipt of the revision for papers published in the indicated year. The time spent doing first revisions has increased steadily since 1940. Authors were about one month slower in 1980 than in 1970 and about one and a half months slower in the mid 1990s than in 1980. This could reflect that authors were asked to do more in a revision or took longer to do similar tasks. The fact that authors of the 1940 papers took only 1.4 months to revise their manuscripts (including the time needed to have them retyped and waiting time for the mail in both directions) suggests that the revisions then must have been less extensive. Various *JPE* annual reports indicate that the total time that authors spent making all revisions increased from 4.1 months for 1980 publications to 6.6 months for 1999 publications. How much of this increase is due to the increase in the number of revisions is unclear.

In summary, submit-accept times at top journals have increased by 12–18 months over the last 30 years. The majority of the slowdown

[13] One way to estimate the frequency of multiple revisions is to note that the *JPE* received an average of 86 revisions per year in 1993–98 and published an average of 49.5 papers. This implies that the average number of revisions per paper cannot be greater than 1.7. Only about 10 percent of the papers published in 1998 have notes in the *JPE*'s database indicating that they were revised more than once, but these data may be unreliable: the database design did not anticipate the possibility of multiple revisions, and data are sometimes overwritten. In this period *JPE* editors often asked for additional revisions on accepted papers. These are not counted in either data source.

[14] The *Journal of Financial Economics* reports only submission and final resubmission dates. The mean difference between these figures was 2.6 months in 1974 (the journal's first year) and 14.8 months in 1999. Fourteen of the 15 papers published in 1974 were revised at least once.

[15] The distribution of submit-accept times at the *Journal of Finance* is skewed by the presence of a few papers with very long lags, but the median is still 15 months. Papers in its shorter papers section had an even longer lag: 23.2 months on average.

reflects the growth of revisions. One-quarter of the slowdown may occur because journals take longer to conduct initial reviews. A smaller part occurs because authors take longer to carry out revisions. Anecdotal evidence suggests that today's revisions are much more extensive than those of 30 years ago.[16] It is unclear whether this fully accounts for the fact that it takes longer to produce an initial decision and revisions take longer to make or whether authors, editors, and referees also take longer to perform comparable tasks. The main lesson I take from this section is that when one is trying to think about what changes in the profession might have caused the slowdown, it will be useful to think about factors that might contribute to more extensive revisions.

III. Democratization

This section and the three that follow examine changes in the economics profession that may help explain the slowdown.

A. *The Potential Explanation*

I use the term "democratization" to refer to the idea that the economics publishing process has become more open and meritocratic. There are a number of reasons why such a change might lead to a slowdown. First, carefully evaluating journal submissions is a demanding task. If journals used to rely more on the author's reputation, then decisions could have been made more quickly. Second, a democratization could lead to higher mean submit-accept times by lengthening review times for some classes of authors. For example, authors who formerly enjoyed preferential treatment might face longer delays. Third, democratization might change the composition of the pool of accepted papers. For example, it might allow more authors from outside top schools or from outside the United States to publish. If these authors have longer submit-accept times, then the compositional change could increase the mean submit-accept time. Authors who are not at top schools might have longer submit-accept times because they have fewer colleagues who can help them improve their papers before submission and are less able to tailor their submissions to editors' tastes. Authors who are not native English speakers may have longer submit-accept times because they need more help to improve the readability of their papers.

[16] For example, even though the *JPE* asked for what I would regard as a relatively small revision on this paper, the editor's letter and reports contained over 3,800 words of advice and commentary. A search of my colleagues' files found correspondence for a nonrandom sample of three old papers. One (unusually for 1975) was accepted with no revisions. The initial correspondence on the other two papers (from 1960 and 1972, respectively) contained 351 and 755 words of advice and commentary.

TABLE 4
AUTHOR CHARACTERISTICS FOR ARTICLES IN THE TOP FIVE JOURNALS

	DECADE				
	1950s	1960s	1970s	1980s	1990s
Author-level Herfindahl			.00138	.00142	.00135
Percentage from top eight schools	36.5	31.8	27.2	28.2	33.8
Harvard share of *QJE*	14.5	12.3	12.7	6.4	12.5
Chicago share of *JPE*	15.6	10.6	11.2	7.0	9.4
Non-English name share			26.3	25.2	30.6
Percentage female			3.5	4.5	7.5

B. *Has There Been a Democratization? Time-Series Data on the Characteristics of Accepted Papers*

The first place I shall look for quantitative evidence on whether the review process has become more open and meritocratic since 1970 is in the composition of the pool of accepted papers.

One natural prediction is that a democratization of the review process, especially in combination with the growth of the profession, would reduce the concentration of publications.[17] The top x percent of economists would presumably capture a smaller share of publications in top journals since other economists are more able to compete with them for scarce space, and economists at the top N schools would presumably see their share of publications decline. The first row of table 4 presents a Herfindahl index of authors' "market shares" of articles in the top five general-interest journals in each decade; that is, it reports $\sum_a s_{at}^2$, where s_{at} is the fraction of all articles in decade t written by author a.[18] There was actually a small increase in author-level concentration between the 1970s and the 1980s and then a small decline between the 1980s and the 1990s. Despite the growth of the profession, the author-level concentration of publications in the 1990s is about what it was in

[17] Of course this need not be true. For example, it could be that the elite received preferential treatment under the old system but were writing the best papers anyway, or that more meritocratic reviews simply lead to concentration of publications in the hands of the best authors instead of the most famous authors. A possibility relevant to school-level concentration is that the hiring process at top schools may have become more meritocratic and led to a greater concentration of talent.

[18] I use all articles that appeared in the *AER*, *Econometrica*, *JPE*, *QJE*, and *REStud* between 1970 and some time in 1997–98. Each author is given fractional credit for coauthored articles. I include only regular articles, omitting where I can shorter papers, notes, comments, articles in symposia or special issues, presidential addresses, etc.

the 1970s.[19] The second row of table 4 reports the weighted fraction of pages in the *AER*, *QJE*, and *JPE* written by authors from the top eight schools.[20] The numbers point to an increase in school-level concentration, both between the 1970s and the 1980s and between the 1980s and the 1990s.[21] I include the 1950s and 1960s figures because they suggest a reason why a belief that the profession has become more egalitarian since the "old days" might be widespread: there was a substantial decline in the top eight schools' share of publications between the 1950s and the 1970s.

One might also look for evidence of a decline in favoritism in the prevalence of articles from a journal's home institution. Rows 3 and 4 piggyback on Siegfried's (1994) work to illustrate trends in the page-weighted share of articles in the *JPE* and *QJE* written by authors at each journal's home institution. In each case the substantial decline between the 1970s and the 1980s noted by Siegfried was followed by a substantial increase between the 1980s and the 1990s.

The final two rows of table 4 contain estimates of the fraction of articles in the top five journals written by women and non–native English speakers. They were obtained by classifying authors on the basis of their first names. Each group has increased its share of publications, but as a fraction of the total author pool the changes are small.

My conclusion is that it is hard to find much evidence of a democratization of the review process in the composition of the pool of published papers.

C. *Evidence from Cross-Sectional Variation*

The data I use in all the cross-section analyses contain submit-accept times for most papers published in *Econometrica*, *REStud*, and *REStat* since 1970, papers published in the *JPE* and *AER* since 1992 or 1993, and papers published in the *QJE* in 1973–77, 1980, 1985, 1990, and since 1993. The data end at the end of 1997 or the middle of 1998 for all

[19] Recall that the 1990s data do not include most 1998 and all 1999 papers. I would expect that a Herfindahl index computed from a full data set would be smaller.

[20] My data do not include author affiliations for pre-1989 papers. I thus computed figures for the 1990s that can be compared with figures given in Siegfried (1994) for earlier decades. Some of the numbers in Siegfried's paper were in turn directly reprinted from Cleary and Edwards (1960), Yotopoulos (1961), and Siegfried (1972). Pages are weighted so that *JPE* and *QJE* pages count for 0.707 and 0.658 *AER* pages, respectively. One departure from Siegfried's paper is that I assign authors to their first affiliation rather than splitting credit for authors who list affiliations with two or more schools.

[21] Most of the increase between the 1980s and 1990s is attributable to the increase in the top three schools' share of the *QJE* from 15.7 percent to 32.2 percent. The increase from the 1970s to the 1980s, however, is in a period in which the top eight schools' share of the *QJE* was declining, and there is still an increase between the 1980s and 1990s if one removes the *QJE* from the calculation.

TABLE 5
SUMMARY STATISTICS FOR PAPER-LEVEL DATA

	Sample					
	1970s (N=1,564)		1980s (N=1,154)		1990s (N=1,413)	
Variable	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Lag	300.14	220.27	498.03	273.84	659.55	360.90
AuBrookP	.07	.45	.06	.30	.09	.35
AuP&P	.19	.49	.27	.70	.35	.78
AuTop5PubsLast5	.80	1.37	.93	1.09	.79	.99
AuTop5Pubs70s	2.20	2.51	1.02	1.77	.42	1.34
SchoolTop5Pubs	...	...	...	...	35.47	32.07
EnglishName	.65	.45	.67	.43	.66	.41
Female	.03	.16	.04	.17	.07	.22
UnknownName	.09	.29	.02	.15	.01	.11
NumAuthor	1.39	.60	1.49	.64	1.73	.71
Pages	13.09	6.37	17.43	7.52	24.20	8.72
JournalHQ	...	...	...	...	.08	.27
NBER	...	...	...	...	.17	.37
Order	6.81	4.08	6.40	3.70	5.39	3.15
log(1+Cites)	2.52	1.31	2.90	1.27	2.33	1.03
EditorDistance					.81	.25
AER	.00	.00	.00	.00	.18	.38
Econometrica	.41	.49	.56	.50	.26	.44
JPE	.00	.00	.00	.00	.18	.38
QJE	.09	.28	.06	.23	.18	.38
REStud	.24	.43	.38	.49	.20	.40
REStat	.26	.44	.00	.00	.00	.00
Sample coverage	51%		44%		74%	

journals. I include papers in *REStat* in the 1970s regressions, but not in analyses of subsequent decades. The sample includes only standard full-length articles, omitting (when feasible) shorter papers, comments, replies, errata, articles in symposia or special issues, addresses, and so forth. Summary statistics on the sets of papers for which submit-accept times are available are presented in table 5.[22] Note that data are available for about three-quarters of the articles in the top five journals in the 1990s and for about half of the articles in the earlier decades. I shall not give the definitions of all the variables here, but shall instead discuss them in connection with the relevant results.

The first thing I shall do with the cross-section data is to investigate a question relevant to the discussion of "has there been a democratization?" Specifically, I examine whether papers by high-status authors

[22] I have omitted summary statistics on the dummy variables that classify papers into fields. See Sec. IV*B*2 for more on these.

that were accepted used to (or still do) make it through the review process more quickly.[23]

The dependent variable for the regressions in this section, Lag, is the length of time in days between the submission of a paper and its final acceptance (or a proxy for this).[24] I include a number of explanatory variables to look for evidence that papers by high-status authors are accepted more quickly. The first two, AuBrookP and AuP&P, are the average number of papers that the authors published in *Brookings Papers on Economic Activity* and the *AER*'s *Papers and Proceedings* issue in the decade in question.[25] Papers published in these two journals are invited rather than submitted, making them a potential indicator of authors who are well known or well connected.[26] Estimates of the relationship between publication in these journals and submit-accept times during the 1970s can be found in column 1 of table 6. The estimated coefficients on AuBrookP and AuP&P are statistically insignificant and have opposite signs. They provide little evidence that high-status authors enjoyed faster submit-accept times. The results for the 1980s and 1990s in columns 2 and 3 are qualitatively similar. The estimated coefficients are always insignificant, and the point estimates on the two variables have opposite signs.

Another potential measure of status is publications in an earlier period. In each regression I have included a variable, AuTop5PubsLast5, giving the number of articles the author published in the top five journals in the preceding five years. Besides reflecting status, the variable might also proxy for an author's ability and motivation to write clean papers and revise them promptly. The coefficient estimate from the 1970s regression indicates that authors who had "high status" by this measure got their papers through the review process a little more quickly. The coefficient from the 1990s regression is similar but is not statistically significant. The test thus provides little evidence of a declining status benefit. In the 1980s and 1990s regressions, I also include

[23] The most important question on status bias is whether papers by high-status authors are more likely to be accepted when paper quality is held fixed. I cannot address this question, however, because I have little data on the pool of rejected papers.

[24] Because of data limitations, I substitute the length of time between the submission date and the date of final resubmission for papers in *Econometrica* and for pre-1975 papers in *REStud*. The 1973–77 *QJE* data use the time between submission and initial acceptance (which was not infrequently followed by a later resubmission).

[25] More precisely, author-level variables are defined first by taking simple counts (not adjusted for coauthorship) of publications in the two journals. Article-level variables are then defined by taking the average across the authors of the paper. Here and elsewhere I lack data on all but the first author of papers with four or more authors.

[26] To give some feel for the variable, the top four authors in *Brookings* in 1990–97 are Jeffrey Sachs, Rudiger Dornbusch, Andrei Shleifer, and Robert Vishny, and the top four authors in *Papers and Proceedings* in 1990–97 are James Poterba, Kevin Murphy, James Heckman, and David Cutler. Another justification for the status interpretation is that both AuBrookP and AuP&P are predictive of citations for papers in my data set.

TABLE 6
BASIC SUBMIT-ACCEPT TIME REGRESSIONS

	SAMPLE		
VARIABLE	1970s (N=1,564) (1)	1980s (N=1,154) (2)	1990s (N=1,413) (3)
AuBrookP	10.1	−28.7	−24.4
	(.77)	(.93)	(1.15)
AuP&P	−8.3	14.4	15.5
	(.66)	(.99)	(1.22)
AuTop5PubsLast5	−8.6	2.4	−9.5
	(1.92)	(.27)	(1.00)
AuTop5Pubs70s	...	.2	4.4
		(.04)	(.64)
SchoolTop5Pubs	...	...	−.5
			(1.37)
EnglishName	1.2	3.1	−2.2
	(.09)	(.16)	(.10)
Female	−35.8	−54.8	56.8
	(1.05)	(1.20)	(1.28)
UnknownName	4.7	−8.7	−.4
	(.22)	(.16)	(.01)
NumAuthor	−19.1	17.6	31.0
	(2.10)	(1.39)	(2.31)
Pages	5.6	5.0	5.3
	(5.57)	(3.90)	(4.28)
JournalHQ	...	...	11.6
			(.33)
NBERQJE	...	...	−22.5
			(.59)
Order	1.8	4.9	8.8
	(1.25)	(2.08)	(2.76)
log(1+Cites)	−21.7	−12.2	−40.7
	(4.91)	(1.70)	(3.86)
Journal dummies	yes	yes	yes
Journal trends	yes	yes	yes
Field dummies	yes	yes	yes
R^2	.11	.10	.19

NOTE.—Absolute values of t-statistics are in parentheses. The dependent variable, Lag, is the length of time between submission of a paper to a journal and its acceptance in days. The sample is subsets of the set of papers published in the top five or six general-interest economics journals between 1970 and 1998 as described in the text. All regressions include journal dummies, journal-specific linear time trends, and dummies for 17 fields of economics.

the number of top five journal articles the author published in the 1970s, AuTop5Pubs70s.[27] The coefficient estimates for this variable are small, positive, and insignificant in the two decades.

The second thing I would like to do with the cross-section data is to evaluate arguments that mean submit-accept times may have increased because of compositional effects. I noted earlier that mean submit-

[27] These variables give fractional credit for coauthored papers; do not count short papers, comments, papers in symposia, etc.; and are averages across the coauthors of papers with two or three coauthors.

accept times would go up if a greater fraction of articles were written by authors not at top schools and non–native English speakers and these authors had longer submit-accept times. The time-series data indicated that such explanations cannot be very important: there has been only a slight increase in publications by non–native English speakers, and the very top schools, at least, have been increasing their share of top-journal publications. Nonetheless, I shall complete the analysis here by looking at whether "outsiders" do have longer submit-accept times. First, to examine the school story, I include in the 1990s regression a variable, SchoolTop5Pubs, giving the total number of articles by authors at the author's institution in the 1990s.[28] The estimated coefficient on SchoolTop5Pubs in column 3 of table 6 indicates that authors from top schools had their papers accepted slightly more quickly but that the differences are not statistically significant. The coefficient estimate of −0.46 is small: such an effect would allow economists at the very top schools to get their papers accepted about one and a half months faster than economists from the bottom schools.[29] Second, I included in all regressions a variable indicating whether the authors of a paper have first names suggesting that they are native English speakers, EnglishName.[30] The estimated coefficients on this variable are extremely small and insignificant in each decade.

To conclude, I essentially find nothing in the time-series or cross-section data to indicate that the economics publishing process is more open and meritocratic than it was in 1970. I also find no support for the idea that mean submit-accept times may have increased because a more democratic review process has increased the share of papers by outsiders.

[28] This school-level variable is defined using my standard set of five journals, giving fractional credit for coauthored papers, and omitting short papers, comments, papers in symposia, etc. Each author is regarded as having only a single affiliation for each paper, which I usually take to be the first affiliation listed. Many distinct affiliations were manually combined, but some errors surely remain, especially at foreign institutions. Different academic units within the same university are also combined.

[29] The variable is about 100 for the top three schools. Authors from other elite schools would have a substantially smaller advantage. The value of SchoolTop5Pubs is above 35 for only five other schools, and only 14 schools have values of 20 and 35. The top 10 schools in the ranking are Harvard, MIT, Chicago, Northwestern, Princeton, Stanford, Pennsylvania, Yale, and University of California at Berkeley and Los Angeles. The second 10 are Columbia, University of California at San Diego, Michigan, Rochester, the Federal Reserve Board, Boston University, New York University, Tel Aviv, Toronto, and the London School of Economics. To the extent that there is a relationship between SchoolTop5Pubs and submit-accept times, it looks linear.

[30] Here again I take an average of the authors' characteristics for coauthored papers. Switching to an indicator equal to one if any author has a name associated with being a native English speaker does not change the results.

IV. Complexity and Specialization

A. *The Potential Explanation*

It is a common impression that economics papers have become increasingly technical, sophisticated, and specialized over the last few decades. There are at least three reasons why this might lead to a lengthening of the review process.

First, referees and editors may take longer to read and digest papers that are more complex.

Second, it may make it necessary for authors to get more input from referees. One story would be that increased complexity reduces authors' understanding of their own papers, so that they need more help from referees and editors to get things right. Another is that in the old days authors were able to get advice on their papers from colleagues. With increasing specialization, colleagues are less able to provide this service, and it may be necessary to substitute advice from referees.

Third, editors may be forced to change the way they handle papers. In the old days, this story goes, editors could easily understand papers and digest referee reports. This let them clearly articulate what improvements would make a paper publishable and check for themselves whether the improvements had been made on resubmission. With increased complexity, editors may be less able to determine and describe ex ante what revisions would be sufficient. This leads to multiple rounds of revisions. In addition, more rounds must be sent back to referees, lengthening the time required for each round.

B. *Has Economics Become More Complex and Specialized?*

For a couple of reasons I do not regard it as obvious that economics has become more complex over the last three decades. First, by 1970 there was already a large amount of very technical, inaccessible work being done, and the 1990s have seen the growth of a number of branches with relatively standardized, easy-to-read papers, for example, natural experiments, growth regressions, and experimental economics. To take one not-so-random sample of economists, the Clark Medal winners of the 1980s were Michael Spence, James Heckman, Jerry Hausman, Sandy Grossman, and David Kreps, whereas the 1990s winners were Paul Krugman, Lawrence Summers, David Card, Kevin Murphy, and Andrei Shleifer. Second, what matters for the explanations above is not that papers are more complex, but rather that they are more difficult for economists (be they authors, referees, or editors) to read, write, and evaluate. The game theory found in current industrial organization theory papers might be daunting to an economist transported here from the 1970s, but it is second nature to researchers in the field today. In its February

1975 issue, the *QJE* published articles by Joan Robinson and Steve Ross. The August issue included papers by Nicholas Kaldor and Don Brown. To me, the range of skills necessary to evaluate these papers seems greater than that necessary to evaluate papers in a current *QJE* issue. In this section, I develop some empirical evidence on complexity.

1. Some Simple Measures

A couple of trends that have been noted by other authors might reflect increasing complexity. First, today's papers are substantially longer (Laband and Wells 1998). At the *AER*, *JPE*, and *QJE*, articles are now about twice as long as they were in 1970. At *Econometrica* and *REStud*, articles are about 75 percent longer. Whether longer means more complex is less clear: one reason today's papers are so much longer is that they have longer introductions, spend more time surveying the related literature, provide detailed summary statistics, and do other things that are supposed to make papers easier to read. Another fact that seems incongruous with the idea that length reflects complexity is that prior to 1970 there was a 70-year long trend toward shorter papers (Laband and Wells 1998).

Second, more papers today are coauthored (Hudson 1996). In the 1970s only 30 percent of the articles in the top five journals were coauthored. In the 1990s about 60 percent were coauthored. In the longer run the trend is even more striking: in 1959 only 3 percent of the articles in the *JPE* were coauthored. This trend could reflect an increase in complexity if one reason that economists work jointly on a project is that one person alone would find it difficult to carry out the range of specialized tasks involved. A fact that seems incongruous with the idea that coauthorship reflects complexity is that coauthorship is now less common at the *REStud* and *Econometrica* than at the *AER*, *QJE*, and *JPE*.

2. Measures of Specialization

The relevant notion of complexity for the stories told above is complexity relative to the skills and knowledge of those in the profession. My thought on developing evidence on such complexity is that I can examine whether economists have become more specialized. If an increase in complexity has made it more difficult for authors to master their own work, for colleagues to provide useful feedback, or for editors to digest papers, then it seems reasonable to expect that economists would have responded by becoming increasingly specialized in particular lines of research.

To measure the degree to which economists are specialized, I use the index that Ellison and Glaeser (1997) proposed to measure geographic

concentration.[31] Suppose that a set of economics papers can be classified as belonging to one of F fields indexed by $f = 1, 2, \ldots, F$. Write N_i for the number of papers written by economist i, s_{if} for the share of economist i's papers that are in field f, and x_f for the fraction of all publications that are in field f. The Ellison-Glaeser index of the degree to which economist i is specialized is

$$\gamma_i = -\frac{1}{N_i - 1} + \frac{N_i}{N_i - 1} \frac{\sum_f (s_{if} - x_f)^2}{1 - \sum_f x_f^2}.$$

Under particular assumptions discussed in the Ellison-Glaeser paper, the expected value of this index is unaffected by the number of papers that are observed and by the number and size of the fields used in the breakdown. The scale of the index is such that a value of 0.2 would indicate that the frequency with which pairs of papers by the same author are in the same field matches what would be expected if 20 percent of authors wrote all of their papers in a single field and 80 percent of authors wrote in fields that were uncorrelated from paper to paper (drawing each topic from the aggregate distribution of fields).

I first apply the measure to look at the specialization of authors across the main fields of economics. Largely on the basis of *Journal of Economic Literature* (JEL) codes, I assigned the articles in the top five journals since 1970 to one of 17 fields.[32] Table 7 gives the fraction of papers in each field in each decade.

Table 8 reports the average value of the Ellison-Glaeser index (computed separately for the 1970s, 1980s, and 1990s) across economists having at least two publications in the top five journals in the decade in question. I have calculated the index in two ways. The top row reports on a calculation in which I made my best effort to assign each paper to the appropriate field. For post-1990 papers, my data contain both pre-1990 and post-1990 JEL codes for papers, and I believe that the latter allow me to classify papers more accurately. The second row reports on a calculation in which I ignore the new JEL code information and classify all papers using an unchanging algorithm. The absolute level of specialization in all three decades seems fairly low relative to the common perception. Depending on which series one looks at, one could argue that there has been either a slight increase in specialization or a slight decrease in specialization. The difference between the 1990s

[31] The analogy with Ellison and Glaeser (1997) is to equate economists with industries, fields with geographic areas, and papers with manufacturing plants. See Stern and Trajtenberg (1998) for an application of the index to doctors' prescribing patterns similar to that given here.

[32] In a number of cases the JEL codes contain sets of papers that seem to belong to different fields. In these cases I used rules based on title keywords and in some cases paper-by-paper judgments to assign fields.

TABLE 7
FIELD BREAKDOWN OF ARTICLES IN THE TOP FIVE JOURNALS

	PERCENTAGE OF PAPERS		
FIELD	1970s	1980s	1990s
Microeconomic theory	25.8	29.5	22.7
Macroeconomics	16.8	15.5	21.2
Econometrics	10.5	9.1	8.7
Labor	9.8	9.0	8.6
Industrial organization	7.3	11.0	8.3
International	6.9	5.4	5.6
Public finance	6.1	5.3	5.3
Finance	3.2	5.3	7.7
Development	3.8	1.4	1.6
Experimental	.4	1.3	2.5
Urban	2.2	.7	1.1
History	1.1	1.9	1.0
Political economy	1.1	.6	1.9
Productivity	1.4	1.2	.9
Environmental	.4	.4	.8
Law and economics	.3	.3	1.0
Other	3.1	2.3	1.2

values in the two rows is as would be expected. Random misclassifications will tend to make measured specialization decrease.[33]

The results above concern specialization at the level of broad fields. A second relevant sense in which economists may be specialized is within subfields of the main fields in which they work. To construct indices of within-field specialization, I viewed each field of economics in each decade as a separate universe and treated pre-1990 JEL codes as subfields into which the field could be divided. I then computed Ellison-Glaeser indices exactly as above on the set of economists having two or more publications in the top five journals in the field (ignoring their publications in other fields). I restrict the analysis to the seven fields for which the relevant sample of economists exceeded 10 in each decade and for which the subfields defined by JEL codes gave a reasonably fine field breakdown: microeconomic theory, macroeconomics, labor, industrial organization, international, public finance, and finance.[34]

The results presented in table 9 reveal no single typical pattern. In four fields—microeconomic theory, industrial organization, labor, and public finance—there is a trend toward decreasing within-field specialization. In two others, macroeconomics and finance, there is a drop

[33] A related bias is that it may be easier for me to divide papers into fields in the 1990s because my understanding of what constitutes a field is based on my knowledge of economics in the 1990s.

[34] The number of economists meeting the criterion ranged from 19 for finance in the 1970s to 264 for theory in the 1980s. The additional restriction was that I included only fields for which the Herfindahl index of the component JEL codes was below 0.5.

TABLE 8
SPECIALIZATION OF AUTHORS AT LEVEL OF MAJOR FIELD

CLASSIFICATION OF 1990s PAPERS	MEAN SPECIALIZATION INDEX		
	1970s	1980s	1990s
Best possible	.33	.33	.37
Consistent use of old JEL codes	.33	.33	.31

NOTE.—The table reports Ellison-Glaeser concentration indexes reflecting authors' tendencies to concentrate their writings in a few major fields.

from the 1970s to the 1980s followed by an increase from the 1980s to the 1990s. International economics has the opposite pattern.[35]

Overall, I interpret the results of this subsection as indicating that there is little evidence that economists have become more specialized.

C. *Links between Complexity and Review Times*

In this subsection I discuss a few pieces of evidence on whether an increase in complexity would slow the review process if it were occurring.

1. Simple Measures of Complexity

I noted earlier that papers have grown longer and that coauthorship is more frequent. Although it is not clear whether these characteristics should be thought of as measures of complexity, their relationship with submit-accept times is certainly of interest. Two variables in the regression of submit-accept times on paper and author characteristics in table 6 are relevant.

Pages is the length of an article in pages.[36] In all three decades, this variable has a positive and highly significant effect.[37] The estimates are that longer papers take longer in the review process by about five days per page. The lengthening of papers over the last 30 years may then account for two months of the overall increase in submit-accept times. Alternate interpretations for the estimate can also be given. For example, papers that go through more rounds of revisions may grow in

[35] The calculations reported in table 9 classify post-1990 papers into major fields using the new JEL codes. An improvement in the major field classification of papers would be expected to bias the results toward a finding of reduced within-field specialization. When the 1990s papers are classified using only the pre-1990 JEL codes, measured within-field specialization increases for all fields, but the ranking of 1970s vs. 1990s specialization is unchanged.

[36] Recall that the regression includes only full-length articles and not shorter papers, comments, and replies.

[37] This contrasts with the results of Laband et al. (1990), who report that in a quadratic specification the relationship between review times and page lengths (for papers in *REStat* between 1970 and 1980) is nearly flat around the mean page length.

TABLE 9
SPECIALIZATION OF AUTHORS WITHIN SUBFIELDS OF EACH MAJOR FIELD

FIELD	INDEX OF WITHIN-FIELD SPECIALIZATION		
	1970s	1980s	1990s
Microeconomic theory	.38	.32	.31
Macroeconomics	.27	.17	.23
Industrial organization	.35	.30	.19
Labor	.28	.22	.11
International	.25	.35	.27
Public finance	.50	.28	.18
Finance	.29	.20	.42

NOTE.—The table reports Ellison-Glaeser concentration indexes reflecting authors' tendencies to concentrate their writings in a few subfields.

length as authors add material and comments in response to referees' comments, or longer published papers may tend to be papers that were much too long when first submitted and needed extensive editorial input.

NumAuthor is the number of authors of the paper. In the 1990s, papers with more authors had longer submit-accept times.[38] Under the assumption that the rise in coauthorship is due to the greater complexity of papers, this is another connection between complexity and the slowdown. The magnitude of the effect is small, however. The cross-section estimate implies that the shift from 1.4 authors per paper in the 1970s to 1.7 authors per paper in the 1990s would account for only about 10 days of the slowdown.

2. Specialization and Advice from Colleagues

My idea for providing evidence on whether increased complexity would slow the review process by making authors more reliant on referees is that the stories for why this should be true suggest that advice from colleagues is important and helps authors get papers through the review process more quickly. Economists at top schools are more likely to have colleagues with sufficient expertise to provide useful feedback than economists in smaller or less active departments. Hence, the finding of Section III*C* that authors at top schools had only a small statistically insignificant advantage in submit-accept times makes these stories seem unimportant.

[38] A puzzling result is that the opposite was true in the 1970s despite the presumably inferior communication technology.

TABLE 10
EFFECT OF EDITOR EXPERTISE ON SUBMIT-ACCEPT TIMES

	DEPENDENT VARIABLE: SUBMIT-ACCEPT TIME		
INDEPENDENT VARIABLE	(1)	(2)	(3)
EditorDistance	−67.9	−143.9	−23.9
	(1.4)	(3.4)	(.5)
Editor fixed effects	yes	yes	no
Field fixed effects	yes	no	yes
Journal fixed effects and trends	no	no	yes
Other variables from table 6	yes	yes	yes
R^2	.29	.28	.19

NOTE.—The table reports the results of regressions of submit-accept times on the distance of a paper from the editor's area of expertise and the other independent variables used in the regressions of table 6. Absolute values of t-statistics are in parentheses.

3. Specialization and Editor Expertise

My idea for examining the editor-expertise link between specialization and submit-accept times is straightforward. I construct a measurement, EditorDistance, of how far each paper is from the editor's area of expertise and include it in submit-accept time regressions such as those in table 6.

The approach I take to quantifying how far each paper is from its editor's area of expertise is to assign each paper i to a field $f(i)$; determine for each editor e the fraction of his papers, s_{eg}, falling into each field g; define a field-to-field distance measure, $d(f, g)$; and then define the distance between the paper and the editor's area of expertise by

$$\text{EditorDistance}_i = \sum_g s_{e(i)g} d(f(i), g).$$

When the editor's identity is not known, I evaluate this measure for each of the editors who worked at the journal when the paper was submitted and then impute that the paper was assigned to the editor for whom the distance would be minimized.[39] The construction of the field-to-field distance measure is based on the idea that two fields can be regarded as close if economists who write papers in one are likely to write in the other. Details are reported in the Appendix.

Table 10 reports the estimated coefficient on EditorDistance in regressions of submit-accept times in the 1990s on EditorDistance and the variables in the regressions of table 6. To save space, I do not report

[39] The data include the editor's identity for all *JPE* papers and for recent *QJE* papers. All other editor identities are imputed.

the coefficient estimates for the other variables.[40] The specification in column 1 of table 10 departs slightly from the earlier regressions in that it employs editor fixed effects rather than journal fixed effects and journal-specific trends. The coefficient estimate of −67.9 is inconsistent with the hypothesized link between editor expertise and delays: it indicates that papers that are farther from the editor's area of expertise had slightly *shorter* submit-accept times.[41] The effect is not statistically significant. I do not find this result implausible. Indeed, one editor remarked to me that he felt the review process for the occasional international trade paper he handled was less drawn out than for papers in his specialty. He could always identify a number of ways in which papers in his specialty could be improved, but with trade papers, if the referees did not have many comments, he would just have to make a yes or no decision.

The regression in column 1 includes editor and field fixed effects (for 17 fields). Including the fixed effects may obscure potentially interesting sources of variation. For example, the *AER, QJE, JPE,* and *Econometrica* have all had labor economists on their boards for a substantial part of the last decade, whereas none of the 42 editors is an international economist. Any information contained in differences in the mean submit-accept times for labor and international papers is ignored by the estimates with field fixed effects. Similarly, if editor expertise speeds publication, then editors who handle fewer papers outside their area should, on average, be faster. Estimates with editor fixed effects do not exploit this.

Column 2 of table 10 reports estimates from a regression that is like that of column 1, but with the field fixed effects omitted. The coefficient estimate for EditorDistance is now −143.9, and it is highly significant. Apparently, fields that are well represented on editorial boards have slower submit-accept times. Column 3 of table 10 reports on a regression that omits the editor fixed effects (and includes journal fixed effects and journal-specific linear time trends). The coefficient on EditorDistance is somewhat less negative in column 3 than in column 1, but the difference is far from significant.

To conclude, the two pieces of support I have been able to provide for complexity-based theories are that longer and coauthored papers have longer submit-accept times (and that papers are growing longer and are more likely to be coauthored). This may count for a couple months of the slowdown. The evidence on specialization, however, sug-

[40] Most estimates are very similar to those in col. 3 of table 6. The most notable change is that the coefficient on log (1 + Cites) increases to 67.2 and its *t*-statistic increases to 6.79, whereas the coefficient on Order becomes smaller and insignificant. The interpretation of these variables will be discussed in Sec. VIII*C*.

[41] The standard deviation of EditorDistance is 0.25.

gests that perhaps economics has not gotten more complex relative to the understanding of economists. I have also been unable to find evidence to support any of the other mechanisms by which increased complexity might slow the review process.

V. Growth of the Profession

A. *The Potential Explanation*

The exogenous change behind the explanations discussed in this section is the growth of the economics profession. There are a number of ways growth might slow the review process.

First, in the "old days," editors may have seen many papers before they were submitted to journals. Current editors may see a smaller fraction of papers before submission. Unfamiliar papers may have longer review times.

Second, growth may increase editors' workloads. Busier editors may be more likely to return papers for an initial revision without having thought through what would make a paper publishable and thereby end up requiring more rounds of revisions. They may also be more likely to use referees to review resubmissions, which can lead to more rounds and longer times per round.

Third, growth may lead to more intense competition to publish in the top journals. This change would lead to an increase in overall quality standards. To achieve the higher standards, authors may need to spend more time working with referees and editors to improve their papers.

B. *Has the Profession Grown?*

As Siegfried (1998) notes, the U.S. academic economist population grew rapidly before 1970 but has grown slowly since then. Membership in the American Economic Association more than doubled in the 1940s and grew by more than 50 percent in both the 1950s and 1960s, but it was only 10 percent higher in 1998 than in 1970. This should not be surprising: the great increase in college enrollment occurred in the pre-1970 period.

Growth, if it has occurred, must then be coming from the average economist's being more intent on publishing in the top journals or carrying out more research. It is a common impression that more universities in the United States and abroad are emphasizing journal publications. Direct evidence that this is affecting the top journals, however, is elusive. I noted earlier in discussing democratization that the school-level and author-level concentration of top-journal publications has not decreased. Another relevant fact is that foreign institutions have

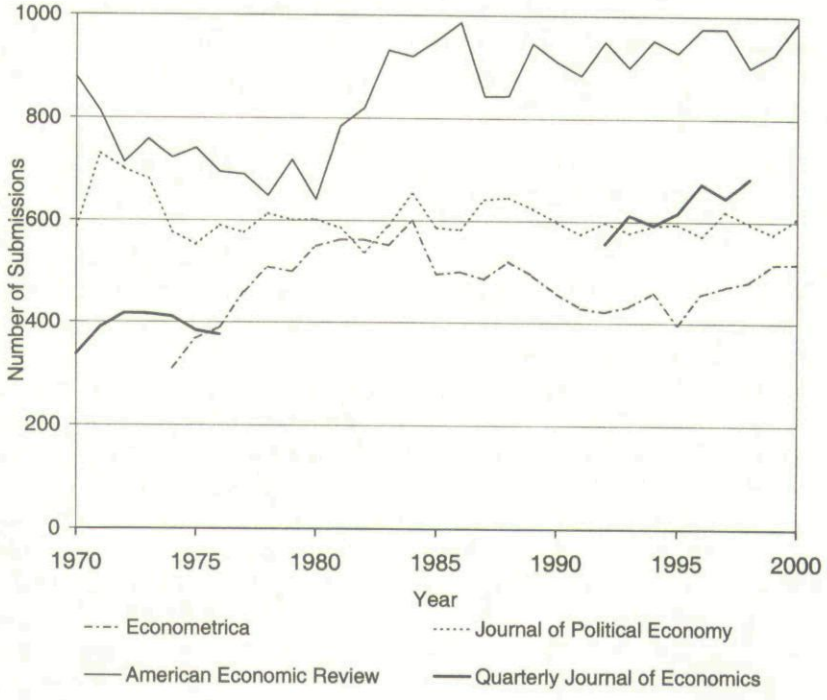

FIG. 3.—Submissions to top journals

not realized any gains. In 1970, 27.5 percent of the articles in the top five journals were by authors working outside the United States.[42] In 1999 the figure was only 23.9 percent.[43]

The impact of growth on editors' workloads is fairly easy to estimate. Editors' workloads have two main components: spending a small amount of time on the large number of submissions that are rejected and spending a large amount of time on the small number of papers that are accepted. Figure 3 graphs the annual number of new submissions to the *AER*, *Econometrica*, *JPE*, and *QJE* since 1970. It clearly shows that there has not been a dramatic upward trend in submissions.[44] Submis-

[42] Each author of a jointly authored paper was given fractional credit in computing this figure, with credit for an author's contributions also being divided if he or she lists multiple affiliations (other than the NBER and similar organizations).

[43] The percentage of articles by authors from outside the United States dropped from 60 percent to 41 percent at *REStud* and from 34 percent to 28 percent at *Econometrica*. There was little change at the *AER*, *JPE*, and *QJE*.

[44] A similar figure would highlight the dramatic growth of the economics profession before 1970. For example, between 1960 and 1970, *AER* submissions went from 276 to 879. *Econometrica* received just 90 submissions in 1959.

TABLE 11
NUMBER OF FULL-LENGTH ARTICLES PER YEAR IN TOP JOURNALS

JOURNAL	NUMBER OF ARTICLES PER YEAR		
	1970–79	1980–89	1990–97
AER	53	50	55
Econometrica	74	69	46
JPE	71	58	48
QJE	30	41	43
REStud	43	47	39

sions to *AER* dropped between 1970 and 1980, grew substantially between 1980 and 1985, and have been fairly flat since (which is when the observed slowdown occurs). Submissions to *JPE* peaked in the early 1970s and have been remarkably constant since 1973. Submissions to *Econometrica* grew substantially between the early 1970s and mid 1980s and have generally declined since. Submissions to *QJE* increased at some point between the mid 1970s and early 1990s and have continued to increase in recent years. To estimate the change in the work editors need to do on rejected papers, one would want to adjust submission figures to reflect changes in the difficulty of reading papers and in the number of editors at a journal. Articles are now 70–100 percent longer, and a larger fraction of current submissions are articles (as opposed to notes or comments). At the same time, however, there have been substantial offsetting increases in the number of editors who divide the workload at most journals: the *AER* went from one editor to four in 1984, *Econometrica* went from three to four in 1975, the *JPE* went from two to four in the mid 1970s, and *REStud* went from two to three in 1994. Hence, the rejection part of editors' workloads should not have increased much. The acceptance part of the workload should have been reduced because journals are not publishing more papers (see table 11) and more editors divide the work. I would conclude that any explanations based on the premise that growth in the profession has increased editors' workloads cannot be supported.[45]

The level of competition to publish in top journals will depend on the amount of high-level research that is being conducted, the number of papers the top journals publish, and economists' preferences for publishing in the top journals. Increases in the size of the profession must make the level of research at least moderately higher. A second competition-increasing effect is that the number of top-journal articles published each year has decreased. *Econometrica* and the *JPE* have not

[45] Note that I have taken no position on whether today's editors actually spend more or less time on their jobs. The fact that editors are trying to guide more extensive revisions may have made the job bigger. I would, however, regard any explanation based on this effect as not having growth in the profession as the root cause.

increased the number of pages they publish in proportion to the length of their average article and thus publish fewer papers now than in the 1970s. Table 11 reports the average number of full-length articles published in each journal in each decade.[46]

There have been a number of changes in the journal market since 1970. To explore the effects on competition, I use data from the Institute for Scientific Information's *Journal Citation Reports* and from Laband and Piette (1994) to compute the frequency with which recent articles in each of the economics journals listed in table 1 were cited in 1970, 1980, 1990, and 1998. Specifically, for 1980, 1990, and 1998, I calculated the impact of a typical article in journal i in year t by

$$\text{CiteRatio}_{it} = \frac{\sum_{y=t-9}^{t} c(i, y, t)}{\hat{n}(i, t-9, t)},$$

where $c(i, y, t)$ is the number of times papers that appeared in journal i in year y were cited in year t, and $\hat{n}(i, t-9, t)$ is an estimate of the total number of papers published in journal i between year $t-9$ and year t.[47] The data that Laband and Piette used to calculate the 1970 measures are similar but include only citations to papers published in 1965–69 (rather than 1961–70).[48] Total citations have increased sharply as the number of journals has increased and the typical article lists more references. To compare the relative impact of journals at different points in time, I define a normalized variable, $N\text{CiteRatio}_{it}$, by dividing CiteRatio_{it} by the average of this variable across the top five general-interest journals.

Table 12 reports the values of NCiteRatio for each journal along with averages for a few groups of journals. The data reveal a striking pattern that I have not previously seen mentioned. There has been a dramatic

[46] There is no natural, consistent way to define a full-length article. In earlier decades it was common for notes as short as three pages and comments to be interspersed with longer articles rather that being grouped together at the end of an issue. Also, some of the papers that are now published in separate sections of shorter papers are indistinguishable from articles. For the calculation reported in the table, most papers in *Econometrica* and *REStud* were classified by hand according to how they were labeled by the journals, and most papers in the other journals were classified using rules of thumb based on minimum page lengths. I varied these rules slightly over time to reflect that comments and other short material have also increased in length.

[47] The citation data include all citations to shorter papers, comments, etc. The denominator is computed by counting the number of papers that appeared in the journal in years $t-2$ and $t-1$ (again including shorter papers, etc.) and multiplying the average by 10. When a journal was less than 10 years old in year t, the numerator was inflated under the assumption that the journal would have received additional citations to papers from the prepublication years with the ratio of citations of early to late papers matching that of the *AER*.

[48] In a few cases in which Laband and Piette did not report 1970 citation data, I substituted an alternate measure reflecting how often papers published in 1968–70 were being cited in 1977 (relative to similar citations at the top general-interest journals).

TABLE 12
RECENT CITATION RATIOS: AVERAGE OF THE TOP FIVE JOURNALS NORMALIZED TO ONE

JOURNAL	VALUE OF *N*CiteRatio			
	1970	1980	1990	1998
	Top Five General-Interest Journals			
AER	1.01*	1.02	.73	.64
Econometrica	.86	.95	1.71	1.00
JPE	.81	1.69	1.11	1.23
QJE	.94	.61	.74	1.37
REStud	1.38	.74	.71	.76
	Next-Tier General-Interest Journals			
Econ. J.	.65	.78	.49	.33
Internat. Econ. Rev.	.53	.53	.26	.20
REStat	.95	.65	.36	.29
Average for group	.71	.65	.37	.28
	Top Field Journals in Major Fields			
J. Development Econ.		.28†	.30	.16
J. Econometrics		.49†‡	.53	.36
J. Econ. Theory	.78†§	.69	.40	.21
J. Internat. Econ.		.35	.38	.26
J. Law and Econ.	.71	1.26	.87	.51
J. Monetary Econ.		.87†	.81	.45
J. Public Econ.		.56†	.34	.19
J. Urban Econ.		.61†	.28	.24
Rand J. Econ.		1.11	.78†§	.31
Average for group	.75	.69	.52	.30
	Some Other Economics Journals			
Canadian J. Econ.	.34†§	.24	.18	.06
Econ. Inquiry	.26	.44	.29	.15
J. Appl. Econometrics			.32†	.26
J. Comparative Econ.		.38†‡	.24	.16
J. Environmental Econ. and Management		.46†	.21	.16
J. Math. Econ.		.42†‡	.28	.10
Average for group	.30	.39	.25	.15
Mean CiteRatio for top five journals	...	1.46	2.59	3.99

* Value is computed as a weighted average of values reported in Laband and Piette (1994) for the regular and *Papers and Proceedings* issues.

† The journal began publishing during the period for which citations were tallied, and values are adjusted in accordance with the time path of citations to the *AER*.

‡ Data pertain to 1982.

§ The value was not given by Laband and Piette, and data instead reflect 1977 citations to 1968–70 articles.

decline in the rate at which articles in the second-tier general-interest journals and in field journals are cited relative to the rate at which articles at the top general-interest journals are cited. In 1970 and 1980 the top field journals and the second-tier general-interest journals typically received about 30 percent fewer citations than the top general-interest journals. Now they typically receive about 70 percent fewer citations.

Citation counts are typically interpreted in one of two ways. First, they can be thought of as reflecting paper quality. With this interpretation, one would conclude that top general-interest journals have raised their quality threshold (relative to the other journals), which suggests that there is now more competition for their space. Second, they can be thought of as reflecting what journals economists read and pay attention to (potentially independent of quality). With this interpretation, one would conclude that the benefit from publishing in top journals has increased, which suggests that there should be more competition for top-journal space.[49] The citation results, the growth in the number of active economists, and the reduction in publications by top journals all work in the same direction and lead me to conclude that there probably has been a substantial increase in the competition to publish in the top journals.

C. *Links between Growth and Review Times*

Given the results of the previous section, editor workload explanations for the slowdown will not work. I focus in this subsection on the two other stories, exploring whether decreased familiarity with submissions or increased competition would slow the review process.

1. Familiarity with Submissions

To examine the idea that in the old days editors were able to review papers more quickly because they were more likely to have seen papers before they were submitted, I included two variables in the 1990s submit-accept times regression of table 6. JournalHQ is a dummy variable indicating whether any of a paper's authors was affiliated with the journal's home institution.[50] I do not find that it has a significant effect on submit-accept times.[51] The variable NBERQJE is a dummy variable indicating whether a paper published in the *QJE* had previously been an NBER working paper.[52] Again, I find no significant effect on submit-accept times. There could be confounding effects in either direction for both

[49] One reason why top journals might now receive relatively more attention is that the growth of working paper distribution and seminar series may have reduced the time economists spend reading journals, and economists may stop reading the lowest-ranked journals first.

[50] I regarded the *QJE* as having both Harvard and MIT as home institutions and the *JPE* as having Chicago as its home. Other journals were treated as having no home institution because editors at the other journals generally do not handle papers written by their colleagues.

[51] Laband et al. (1990) report that papers by Harvard authors had shorter submit-accept times at *REStat* in 1976–80.

[52] About one-third of all 1990s *QJE* papers are identified as having been NBER working papers. This is about twice as high as the fraction in any of the other top five journals.

variables—for example, editors may feel pressure to subject colleagues' papers to a full review process, they may ask colleagues to make fewer changes than they would ask of others, they may give colleagues extra chances to revise papers that would otherwise be rejected, and so forth—but I feel the lack of an effect is still fairly good evidence that the review process is not greatly affected by whether editors have seen papers in advance.

2. Competition

My thought on developing evidence on the effect of competition on review times was that it may be informative to look at how review times are related to journal prestige in a sample of journals. In a simple cross-section regression of the mean submit-accept time of a journal in 1999 on the journal's citation ratio (for the 23 journals listed in tables 1 and 12), I estimate the relationship to be (with *t*-statistics in parentheses)

$$\underset{}{\text{MeanLag}_{i99}} = \underset{(8.9)}{14.6} + \underset{(2.0)}{6.0}\,\text{NCiteRatio}_{i98}.$$

The coefficient on NCiteRatio indicates that as a group the top general-interest journals have review processes that are about six months longer than those at journals almost never cited. The *QJE* is an outlier in this regression. If it is dropped, the coefficient on NCiteRatio increases to 11.3 and its *t*-statistic increases to 3.4.

The panel aspect of tables 1 and 12 lets me examine how submit-accept times at each journal have changed as the journal moved up or down in the journal hierarchy. Table 13 presents estimates of the regression

$$\begin{aligned}\text{MeanLag}_{it} - \text{MeanLag}_{it-\Delta t} = {} & \alpha_0 \frac{\text{NCiteRatio}_{it} - \text{NCiteRatio}_{it-\Delta t}}{\text{NCiteRatio}_{it-\Delta t}} \\ & + \alpha_1 \text{Dum7080}_{t\Delta t} + \alpha_2 \text{Dum8090}_{t\Delta t} \\ & + \alpha_3 \text{Dum9098}_{t\Delta t} + \epsilon_{it},\end{aligned}$$

where i indexes journals, and the changes at each journal over each decade are treated as independent observations.[53] In the full sample, I find no relationship between changes in review times and changes in journal citations. The 1990–98 observation for the *QJE* is a large outlier in this regression. It may be contaminated by endogeneity: one reason why the *QJE* may have moved to the top of the citation ranking is that

[53] Where the 1990 data are missing, I use the 1980–98 change as an observation.

TABLE 13
EFFECT OF JOURNAL PRESTIGE ON SUBMIT-ACCEPT TIMES
Dependent Variable: $\Delta \text{MeanLag}_{it}$

	SAMPLE	
INDEPENDENT VARIABLE	Full ($N=45$) (1)	No *QJE* 98 ($N=44$) (2)
$\Delta \text{NCiteRatio}_{it} \div \text{NCiteRatio}_{it-\Delta t}$	1.0	5.6
	(.4)	(2.5)
Dum7080_{it}	5.7	6.6
	(3.1)	(4.0)
Dum8090_{it}	5.5	6.4
	(4.9)	(6.4)
Dum9098_{it}	2.4	4.4
	(2.2)	(4.1)
R^2	.55	.65

NOTE.—t-statistics are in parentheses.

its fast turnaround times helped it attract better papers.[54] When I reestimate the difference specification dropping this observation (in col. 2 of the table), the coefficient estimate on the fraction change in the normalized citation ratio increases to 5.6 and the estimate becomes significant. Hence, I have once again identified both a change in the profession and a link between this change and slowing review times.

How much of the slowdown over the last 30 years can be attributed to increases in competition for space in the top journals? The answer depends both on which regression estimate one uses and on what one assumes about how much of the increase in the relative position of the top journals reflects increased competition for space in the top journals and how much reflects decreased competition for space in the other journals. On the high side, one might argue that there is just as much competition to publish in, say, *REStat* today as there was in 1970. If one then estimates the effect by multiplying the coefficient estimate from the regression that omits the 1990–98 *QJE* change by the sum of the three proportional declines in the NCiteRatio of *REStat*, then one would estimate that about five months of the top-journal slowdown is due to increased competition.[55] This, however, is probably an overestimate because my guess is that it is easier to publish in *REStat* (or the *Journal of Economic Theory*) now than it once was.

To conclude, I would estimate that increases in competition probably account for three or four months of the slowdown at the top journals.

[54] In part because the data are not well known, I think that the reverse relationship is not very important for most journals.

[55] This is also what one would estimate from just noting that *REStat* has slowed down by about 10 months since 1970, and the slowdown at the non-*QJE* top journals probably averages about 15 months.

I see no evidence to support any of the other hypothesized mechanisms by which growth in the profession may have slowed the review process.

VI. Costs and Benefits of Revisions

A. *The Potential Explanation*

In this section I discuss two simple arguments about how the increase in revisions could be an optimal response to exogenous changes in the costs and benefits of revising papers. The exogenous changes are improvements in computer technology and changes in how economic research is disseminated.

Thirty years ago there were no microcomputers. Rudimentary word processing software was available on mainframes in the 1960s, but until personal computers developed and word processors became common in the late 1970s or early 1980s, revising a paper extensively usually entailed having it retyped. Running regressions was also much more difficult. The earliest forms of SPSS and TSP were written by graduate students in the late 1960s, but statistical packages did not become widely available commercial products until the mid 1970s.[56] The first spreadsheet, Visicalc, appeared in 1979. Statistical packages for microcomputers appeared in the early 1980s and were adopted very quickly. These technologies reduced the cost of revising papers. It seems reasonable to suppose that journals may have increased the number of revisions they requested as an optimal response. This might or might not lead to an increase in the time authors spend revising papers depending on whether the increased speed with which authors can make revisions offsets their being asked to do more. Journals would spend more time reviewing the extra revisions.

Thirty years ago most economists would not hear about new research until it was published in journals. Now, with widely available working paper series and web sites, journals may be less in the business of disseminating information and more in the business of certifying the quality of papers. This may make timeliness of publication less important and may have led journals to slow the review process and evaluate papers more carefully.

B. *Evidence*

The stories above seem plausible. Unfortunately, I have less hard empirical evidence to present in this section than in the preceding ones.

The stories above portray changes in the review process as the result

[56] The companies SPSS and TSP incorporated in 1975 and 1978, and the SAS Institute was founded in 1976.

of optimizing decisions by journal editors. My first thought on developing evidence on the theories was that I could look for evidence of such decisions by talking to editors and reading their writings. I discussed the slowdown with editors or former editors of all of the top general-interest journals and a number of field journals. None mentioned to me that increasing the number of rounds of revision or lengthening the review process was a conscious decision. Instead, even most long-serving editors seemed unaware that there had been substantial changes in the length of the review process. A few editors indicated that they feel that reviewing papers carefully and maintaining high quality standards is a higher priority than timely publication and this justifies current review times, but this view was not expressed in conjunction with a view that the importance of high standards has changed.

I looked at annual editors' reports as a source of contemporary written records on editors' plans. At the *AER*, most of the editors' reports from the post-Clower era just say correctly that the mean time to publication for accepted papers is about what it was the year before. There is no evident recognition that when one aggregates the small year-to-year changes they become a large event. No motivation for lengthening the review process is ever mentioned. The unpublished *JPE* editors' reports include a table giving a three- to five-year time series on mean submit-accept times. Perhaps as a result the *JPE* editors did notice the slowdown (although not its full long-run magnitude). The editors' comments on the slowdown do not suggest that it was planned or seen as optimal. For example, the 1981 report says that

> the increase in the time from initial submission to final publication of accepted papers has risen by 5 months in the past two years, a most unsatisfactory trend. ... The articles a professional journal publishes cannot be timely in any short run sense, but the reversal of this trend is going to be our major goal.

The 1982, 1984, and 1988 reports express the same desire. Only the 1990 report has a different perspective. In good Chicago style it recognizes that the optimal length of the review process must equate marginal costs and benefits but takes no position on what this means in practice:

> Is this rate of review and revision and publication regrettable? Of course, almost everyone would like to have his or her work published instantly, but we believe that the referee and editorial comments and the time for reconsideration usually lead to a significant improvement of an article. A detailed compar-

ison of initial submissions and printed versions of papers would be a useful undertaking: would it further speed the editors or teach the contributors patience?

The one thought I had for providing quantitative evidence on the importance of cost and benefit explanations is that I can compare trends for theoretical and empirical papers. Since revising empirical papers has been made easier both by improvements in word processing *and* by improvements in statistical packages, one might expect that the growth of revisions would be more pronounced for empirical papers. To examine this hypothesis, I had research assistants inspect more than 2,000 of the papers in my data set and classify them as theoretical or empirical.[57] For the rest of the papers I created an estimated classification by defining a continuous variable, Theory, to be equal to the mean of the theory dummies of papers with the same JEL code for which I had data.[58]

Authors of theoretical papers now clearly face a longer review process. In my 1990s subsample I estimate the mean submit-accept time for theoretical papers to be 22.5 months and the mean for empirical papers to be 20.0 months. This should not be surprising: *Econometrica* and *REStud* have longer review processes than the other journals and publish a disproportionate share of theoretical papers. To examine how review times differ within each journal, I included the Theory variable in submit-accept time regressions such as those given in table 6. This produces no support for the idea that the slowdown should be more severe for empirical papers: the Theory variable is insignificant in every decade.

For several reasons I do not find the lack of empirical support for the cost and benefit explanations surprising. First, the results presented earlier on the increasing concentration of citations in the top journals suggest that the decline of the information dissemination role of top journals has been overstated.[59] Second, it seems unlikely that the incremental improvements in word processors and statistical packages over the last 15 years have had enough of an impact on costs to make journals alter their behavior. Finally, I realize that I do not have a particularly good way to get at these theories empirically. My feeling is that the cost and benefit stories are not very important but probably account for some portion of the slowdown that I have not found a good enough way to estimate.

[57] The subset consists of most papers in the 1990s and about half of the 1970s papers.
[58] On average, 83 percent of papers in a JEL code have the modal classification.
[59] The period studied in this paper effectively precedes web-based paper distribution.

VII. Review of Results Explaining the Slowdown

Over the last 30 years, submit-accept times at most top journals have increased by 12–18 months. My goal in Section II was to provide enough details on the slowdown to guide the search for explanations, and my goal in Sections III–VI was to see whether I could identify changes in the profession that could account for the slowdown.

I have found solid support for three explanations. Papers are getting longer, and longer papers have longer submit-accept times. More papers are coauthored, and coauthored papers have longer submit-accept times. There appears to be more competition for space in the top journals, and journals' review processes seem to get more drawn out as a journal's position improves. My best estimates are that the first accounts for two months of the slowdown, the second for a week or two, and the third for three or four months. I do not think of the findings of Section II that referees may be slower to write reports and authors slower to make revisions as additional partial explanations for the slowdown because they are not separately connected to underlying changes in the profession. Indeed, part of what I have found in Sections III–VI may be reflecting differences in referee and author response times. For example, the result that submit-accept times are longer for longer papers must reflect in part Hamermesh's (1994) observation that longer papers take longer to referee. Hence, I regard the analysis so far as leaving well over half of the slowdown unexplained.

I have noted a number of other potential explanations. In some cases, for example, with the cost and benefits arguments, it may be that there is an effect and I just have not found a good way to estimate it. In other cases I am fairly confident that there is not much to an explanation, but obviously cannot rule out some very small effect. It could be that adding up a large number of such small effects can account for another substantial piece of the slowdown. The results suggest to me, however, that there is a substantial missing piece to the puzzle.

VIII. Changes in Social Norms

A. The Potential Explanation

I use the term "social norm" to refer to the idea that the publication process may be fairly arbitrary: editors and referees could simply be doing what conventions dictate one does with submissions. Such social norms need not reflect economists' preferences about the review process or its outcomes. For example, it seems plausible to me to imagine that in a parallel universe another community of economists with identical preferences could have adopted the norm of publishing papers exactly

as they are submitted, figuring that any defects will spur academic discourse and reflect on the author.

The simple statement that a shift in social norms could account for the slowdown is undeniable but also unsatisfying. It immediately leads one to wonder why social norms would have shifted in the direction of increasing revisions.

When I have mentioned the social norm idea, many people have suggested supposed changes in the economics profession that could have led to a change in social norms. My view, however, is that this may not be the best way to think about shifting social norms. To explain changes in fashions from year to year, for example, I think it is more fruitful to explore how social dynamics may lead fashions to continually evolve in the absence of changes in preferences than to look for changes in preferences or economic conditions that could account for what becomes fashionable each year.

I discuss a model of social norms for academic publishing along these lines in Ellison (2002; this issue). The model illustrates a reason why norms may gradually evolve in the direction of emphasizing revisions. In the model a community of referees try to learn and follow the prevailing norm. Papers have multidimensional quality: q-quality reflects the clarity and importance of a paper's main contribution and r-quality reflects other dimensions that are often the focus of revisions, for example, robustness, exposition, extensions, and so forth. How the dimensions are weighted is an arbitrary social norm. Referees learn this norm from reading journals and from their experiences as authors. Economists' inflated opinions of their own work lead to a perpetual puzzle: "Why am I being asked to do so much when the journal is full of mediocre papers?" An endogenous allocation of research effort and attempts to rationalize what is going on lead social norms to evolve in the direction of emphasizing r-quality. My idea in applying the model is that an increased emphasis on r-quality may be what we are seeing in the submit-accept time data.

B. *General Evidence on the Social Norm View: Are Review Times Field-Specific?*

How does one test whether review times are determined by arbitrary social norms? My thought here is that if one believes that arbitrary social norms develop within academic communities, then because economists mostly referee papers in their field and receive reports written by others in their field, one would expect that norms would be somewhat different across fields.[60]

[60] Differences between fields of economics, however, would presumably be limited by economists' working in multiple fields and learning about norms by talking to colleagues.

TABLE 14
MEAN SUBMIT-ACCEPT TIMES BY FIELD IN THE 1990s

Field	Number of Papers	Mean Submit-Accept Time
Econometrics	148	25.7
Development	24	24.7
Industrial organization	108	23.2
Theory	356	22.9
Experimental	35	22.5
Finance	117	21.6
Labor	105	20.8
History	12	20.6
Macroeconomics	282	20.4
International	69	19.3
Political economy	30	18.6
Public finance	60	17.9
Productivity	15	16.2
Environmental	10	15.5
Law and economics	13	14.5
Urban	13	14.4

Table 14 lists mean submit-accept times for papers in various fields published in the top five journals in the 1990s. It indicates that economists in different fields have different experiences with the publication process.[61] There is, however, limited overlap in what is published across journals, and in our standard regression with journal fixed effects and journal-specific trends, the differences across fields are not jointly significant.[62] Hence, these data alone cannot distinguish field-specific differences from differences in practices across journals.

A second source of information on the field specificity of review times is the mean review times of different field journals. If one compares table 14 with table 1, it is striking that the fields that have the longest review times in general-interest journals also have long review times in their field journals. For example, the slowest field journal listed in table 1 is the *Journal of Econometrics*. There are 11 fields listed in table 14 for which I also have data on a top field journal. For these fields, the correlation between the review times in the two tables is .81. I take this as clear evidence that there are field-specific differences in submit-accept times.

One could explain field-specific differences in review times without resorting to arbitrary social norms by attributing the differences to inherent differences in the complexity of papers in different fields. My thought on potentially distinguishing field-specific norms from com-

[61] These differences are jointly highly significant in a comparison of means. Trivedi (1993) observed that econometrics papers published in *Econometrica* between 1986 and 1990 had longer submit–final resubmit times than other papers.

[62] The p-value for a joint test of equality is .17.

TABLE 15
MEAN SUBMIT-ACCEPT TIMES FOR THEORY SUBFIELDS IN THE 1990s

Field	Number of Papers	Mean Submit-Accept Time
General equilibrium	34	27.9
Game theory	82	26.3
Unclassified	32	22.3
Decision theory	28	22.1
Price theory	59	21.9
Learning	13	21.6
Contract theory	59	21.2
Auction theory	13	19.3
Social choice	19	19.0
Welfare economics	17	16.9

plexity effects was that complexity differences are a less likely explanation for differences between more finely divided fields. Table 15 lists mean submit-accept times for papers belonging to each of 10 subfields of microeconomic theory.[63] The differences between the theory subfields are large, and they are also statistically significant at the 1 percent level in a regression with journal fixed effects, journal-specific trends, and our explanatory variables. I take this as suggestive that field-specific differences in review times are not entirely due to inherent differences between fields.

C. *Evidence on the* q-r *Theory*

As described above, I suggest in Ellison (2002; this issue) that the slowdown of the economics publication process can be thought of as part of a broader shift in the weights that are attached to various aspects of paper quality. To assess the applicability of the model, we can look for two things: evidence that journals make a q-r trade-off and evidence that the way in which the q-r trade-off is made has shifted over time. The idea of this subsection is that review times may indicate how much effort on r-quality is required of authors and other observables may proxy for q-quality. By putting these proxies on the right side of a submit-accept time regression, we can examine whether (and how) journals make q-r trade-offs.

The regressions in table 6 include two explanatory variables intended to reflect q-quality. Order is the order in which an article appears in its issue in the journal; that is, for example, one indicates that a paper was the lead article, two the second article, and so forth. The variable

[63] As above, the means pertain to papers published in one of the top five journals in the 1990s, and the *Econometrica* data are actually submit–final resubmit times rather than submit-accept times.

log (1 + Cites) is the natural logarithm of one plus the total number of times the article has been cited.[64]

The regression results provide fairly strong support for the idea that journals make a q-r trade-off. In all three decades, papers that are earlier in a journal issue spent less time in the review process. In all three decades, papers that have gone on to be more widely cited spent less time in the review process.[65] Several of the estimates are highly significant.

The regressions do not, however, provide evidence to support the idea that there has been a shift over time to increasingly emphasize r. Comparisons of the regression coefficients across decades can be problematic because the quality of the variables as proxies for q may be changing.[66] The general pattern, however, is that the coefficients on Order and log (1 + Cites) are getting larger over time.[67] This is not what would be expected if journals were placing less and less emphasis on q-quality.

IX. Conclusion

One consequence of the slowdown of the economics publishing process is that economists spend a significant portion of their "research" time revising papers. Ellison (2002; this issue) notes that trends similar to those discussed in this paper can be seen in many academic disciplines. The thought that this effort may be neither necessary nor valuable is sobering. Robert Lucas (1988, p. 5) has said of economic growth that "the consequences for human welfare involved in questions like these are simply staggering: Once one starts to think about them, it is hard to think about anything else." Journal review processes have a large effect on how much progress growth economists make. They also affect the productivity of all other social and natural scientists. One could thus argue that they are an even more important research topic.

Why has the slowdown occurred? There are a large number of ways in which it is supposed that the profession has changed over the last 30 years. I find it interesting that my empirical analyses direct attention

[64] The citation data were obtained from the on-line version of the *Social Sciences Citation Index* in late February 2000.

[65] Laband et al. (1990) had found very weak evidence of a negative relationship between citations and the length of the review process in their study of papers published in *REStat* between 1976 and 1980.

[66] For example, total citations as of 1999 may be a good indicator of the ultimate importance of a 1970 paper but a poor indicator of the importance of a 1997 paper. I also know that the relationship between the order in which an article appears and how widely cited it becomes has strengthened over time. This suggests that Order may now be a better proxy for q-quality.

[67] The increase is less pronounced if one thinks of the effects as a fraction of the total review time.

toward a couple of changes in the profession that are not as widely recognized. First, today's economics papers are almost twice as long as the papers that were being written 30 years ago. Second, there appears to be increased competition to publish in the top five journals in part because the profession has grown, but more because the number of slots in the top journals has decreased and the top journals have become more prestigious.

The two explanations noted above account for only about one-third of the slowdown. Many other potential explanations have been suggested to me. Perhaps there are simply fewer important ideas waiting to be discovered. Perhaps referees are more insecure or spiteful. Perhaps economists today have worse writing skills. Perhaps it is an echo of the profession's growth in the 1950s and 1960s and the tenure bulge. Perhaps it is a multiple equilibrium phenomenon: we spend so much time revising because authors, cognizant that they will have to revise later, send papers to journals prematurely. I am sure that such behavior is widespread but think that my data on the slowdown *understate* the true increase in polishing efforts. In comparison with published papers from the early 1970s, it seems that even the first drafts of today's papers have been rewritten more times, have more thorough introductions with more references, consider more extensions, and so forth.

What I find most striking in the data is how hard it seems to be to find substantial differences between the economics profession now and the economics profession in 1970. The profession is not much larger. It does not appear to be much more democratic. I cannot find the increasing specialization that I would have expected if economic research were really much harder and more complex than it was 30 years ago. Although it is possible that the slowdown is the result of a large number of minor changes, the fact that the profession looks similar in so many ways suggests to me that it may be difficult to provide any standard equilibrium explanation for why publication should be so much slower.

The potential explanation for the residual slowdown I find most intriguing is that perhaps there is no reason why economics papers must now be revised so extensively. Perhaps the changes reflect a shift in arbitrary social norms for what journals are *supposed to* ask authors to do and what published papers *should* look like. The one piece of support I can provide for the arbitrary social norm view is that the length of the review process varies from field to field within economics.

What future work do I see as important? First, there are many other data sources to explore. The experiences of different disciplines could be compared. The content of referees' reports and editors' letters could be analyzed. Papers could be read. On the theory side, there is surely much more to be said on why social norms might change.

The most important unresolved issue is surely the welfare consequences of the journal review process. If the social norms model is correct, there may be great scope for altering the current system. It might be possible to go back to a system like the one the *QJE* and *Econometrica* had in 1960, where journals simply decide whether or not to publish authors' submissions and questions about papers can be debated in the literature. Alternatively, three or more rounds of even more extensive revisions could become the standard. Studies of the value of revisions based on blind readings could certainly enlighten this debate. More than any study, however, it seems that an active discussion among economists could reveal a lot about whether the current system maximizes the utility of those involved or whether an alternative system might make economists' lives more enjoyable and research more productive.

Appendix

The idea of the field-to-field distance measure is to regard fields as close together if authors who write in one field also tend to write in the other. In particular, for pairs of fields f and g, I first define a correlation-like measure by

$$c(f, g) = \frac{P(f, g) - P(f)P(g)}{\sqrt{P(f)[1 - P(f)]P(g)[1 - P(g)]}},$$

where $P(f, g)$ is the fraction of pairs of papers by the same author that consist of one paper from field f and one paper from field g (counting pairs with both papers in the same field as two such observations), and $P(f)$ is the fraction of papers in this set of pairs that are in field f. I then construct a distance measure, $d(f, g)$, which is normalized so that $d(f, f) = 0$ and $d(f, g) = 1$ when writing a paper in field f neither increases nor decreases the likelihood that an author will write a paper in field g by[68]

$$d(f, g) = 1 - \frac{c(f, g)}{\sqrt{c(f, f)c(g, g)}}.$$

I classified papers as belonging to one of 31 fields (again using JEL codes and other rules). The field breakdown is the same as in the base regression except that I have divided macroeconomics into three parts; international, finance, and econometrics into two parts each; and theory into 10 parts. See table A1 for the complete list. To get as much information as possible about the relationships between fields and about editors with few publications in the top five journals, the distance matrix and editor profiles were computed on a data set that also included notes, shorter papers, and papers in three other general-interest journals for which I collected data: the *AER*'s *Papers and Proceedings* issue,

[68] The assumption that within-field distances are zero for all fields ignores the possibility that some fields are broader or more specialized than others. I experimented with using measures of specialization based on JEL codes like those in Sec. IV*B*2 to make the within-field distances different, but cross-field comparisons like this are made difficult by the differences in the fineness and reasonableness of the JEL breakdowns, and I found the resulting measure less appealing than setting all within-field distances to zero.

TABLE A1

CLOSEST FIELDS IN THE FIELD-TO-FIELD DISTANCE MEASURE

Field	Three Closest Fields		
Micro theory:			
Unclassified	Industrial organization	Micro: welfare economics	Micro: general equilibrium
Price theory	Micro: unclassified	Micro: welfare economics	Micro: decision theory
General equilibrium	Micro: unclassified	Micro: welfare economics	Micro: game theory
Welfare economics	Micro: unclassified	Public finance	Micro: general equilibrium
Game theory	Micro: learning	Micro: contract theory	Micro: social choice
Social choice	Political economy	Experimental	Micro: welfare economics
Contract theory	Micro: learning	Micro: game theory	Micro: unclassified
Auctions	Experimental	Micro: contract theory	Industrial organization
Decision theory	Micro: price theory	Micro: unclassified	Micro: game theory
Learning	Micro: game theory	Micro: contract theory	Finance: unclassified
Macro:			
Unclassified	Finance: unclassified	International: international finance	Macro: growth
Growth	Productivity	Development	Macro: transition
Transition	Finance: corporate	Law and economics	Development
Econometrics:			
Unclassified	Econometrics: time series		
Time series	Econometrics: unclassified		
Industrial organization	Micro: unclassified	Micro: contract theory	Micro: auctions
Labor	Urban	Public finance	
International:			
Unclassified	International: international finance	Development	Macro: growth
International finance	International: unclassified	Macro: transition	Macro: unclassified
Public finance	Micro: welfare economics	Environmental	Urban
Finance:			
Unclassified	Micro: learning	Macro: unclassified	Finance: corporate
Corporate	Micro: unclassified	Macro: transition	Micro: contract theory
Development	Macro: transition	Macro: growth	International: unclassified
Urban	Labor	Law and economics	Public finance
History	Productivity	Development	Other
Experimental	Micro: auctions	Micro: social choice	Micro: game theory
Productivity	Macro: growth	Industrial organization	History
Political economy	Micro: social choice	Law and economics	Macro: transition
Environmental	Public finance	Development	Micro: welfare economics
Law and economics	Political economy	Urban	Macro: transition
Other	History	Political economy	Urban

Brookings Papers on Economic Activity, and *REStat.* Papers that were obviously comments or replies to comments were dropped. All years from 1969 on were pooled together.

To illustrate the functioning of the distance measure, table A1 lists for each of the 31 fields up to three other fields that are closest to it. I include fewer than three nearby fields when there are fewer than three fields at a distance of less than 0.99. The working paper version of this paper (Ellison 2000) contains an additional table illustrating the EditorDistance variable and the accuracy of the editor imputations for a sample of papers.

References

Cleary, Frank R., and Edwards, Daniel J. "The Origins of the Contributors to the A.E.R. during the 'Fifties." *A.E.R.* 50 (December 1960): 1011–14.

Coe, Robert K., and Weinstock, Irwin. "Editorial Policies of Major Economic Journals." *Q. Rev. Econ. and Bus.* 7, no. 4 (1967): 37–43.

Colander, David C. "Research on the Economics Profession." *J. Econ. Perspectives* 3 (Fall 1989): 137–48.

Ellison, Glenn. "The Slowdown of the Economics Publishing Process." Working Paper no. 7804. Cambridge, Mass.: NBER, July 2000.

———. "Evolving Standards for Academic Publishing: A q-r Theory." *J.P.E.* 110 (October 2002): 994–1034.

Ellison, Glenn, and Glaeser, Edward L. "Geographic Concentration in U.S. Manufacturing Industries: A Dartboard Approach." *J.P.E.* 105 (October 1997): 889–927.

Gans, Joshua S., ed. *Publishing Economics: Analyses of the Academic Journal Market in Economics.* Cheltenham, U.K.: Elgar, 2000.

Hamermesh, Daniel S. "Facts and Myths about Refereeing." *J. Econ. Perspectives* 8 (Winter 1994): 153–63.

Hudson, John. "Trends in Multi-authored Papers in Economics." *J. Econ. Perspectives* 10 (Summer 1996): 153–58.

Laband, David N.; Maloney, Michael T.; and McCormick, Robert E. "The Review Process in Economics: Some Empirical Findings." *Rev. Econ. and Statis.* 72A (1990): v–xvii.

Laband, David N., and Piette, Michael J. "The Relative Impacts of Economics Journals: 1970–1990." *J. Econ. Literature* 32 (June 1994): 640–66.

Laband, David N., and Wells, John M. "The Scholarly Journal Literature of Economics: A Historical Profile of the *AER, JPE,* and *QJE.*" *American Economist* 42 (Fall 1998): 47–58.

Lucas, Robert E., Jr. "On the Mechanics of Economic Development." *J. Monetary Econ.* 22 (July 1988): 3–42.

Marshall, Howard D. "Publication Policies of the Economic Journals." *A.E.R.* 49 (March 1959): 133–38.

Siegfried, John J. "The Publishing of Economic Papers and Its Impact on Graduate Faculty Ratings, 1960–1969." *J. Econ. Literature* 10 (March 1972): 31–49.

———. "Trends in Institutional Affiliation of Authors Who Publish in the Three Leading General Interest Economics Journals." *Q. Rev. Econ. and Finance* 34 (Winter 1994): 375–86.

———. "Who Is a Member of the AEA?" *J. Econ. Perspectives* 12 (Spring 1998): 211–22.

Stern, Scott, and Trajtenberg, Manuel. "Empirical Implications of Physician Au-

thority in Pharmaceutical Decisionmaking." Working Paper no. 6851. Cambridge, Mass.: NBER, December 1998.

Trivedi, Pravin K. "An Analysis of Publication Lags in Econometrics." *J. Appl. Econometrics* 8 (January–March 1993): 93–100.

Yohe, Gary W. "Current Publication Lags in Economics Journals." *J. Econ. Literature* 18 (September 1980): 1050–55.

Yotopoulos, Pan A. "Institutional Affiliation of the Contributors to Three Professional Journals." *A.E.R.* 51 (September 1961): 665–70.