Willful Blindness: The Inefficient Reward Structure in Academic Research

Stan J. Liebowitz

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Stan J. Liebowitz is on the faculty of the Naveen Jindal School of Management at the University of Texas at Dallas
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Stan Liebowitz*
liebowit@utdallas.edu

University of Texas at Dallas
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Abstract:

The only reward structure that provides authors an incentive to choose the most efficient sized research teams is strict proration of author credit. Nevertheless, survey evidence indicates that the reward structure used in Economics for at least the last thirty years only incompletely prorates authorship credit, which should lead to inefficiently high levels of coauthorship. A possible reason for incomplete proration is the self-interest of economists with above average levels of coauthorship, a group disproportionately populated by senior economists and thus disproportionately influential. The well-documented increase in coauthorship over the last fifty years, both in economics and academia, may be better explained by the lack of proration than by any shift toward arcane technique or complex statistical analysis. Fictitious authorship, although of dubious ethical status, has the perverse impact of improving the efficiency of authorship when proration is incomplete. Grossly excessive coauthorship, which threatens to make a mockery of authorship itself in several other academic disciplines, may be the path down which Economics is headed if the reward structure is not altered.

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Some academic research is thought to be of great social value, implying that we should care whether it is produced in an efficient manner. Among the most important determinants of the type and quantity of research produced are the rewards, professional and financial, that are given to researchers. All academic departments, when assigning credit for research, must determine, either implicitly or explicitly, the value of each individual research paper and the distribution of credit if there are multiple authors. In spite of the apparent importance of academic research—it is thought to play a major role in advancing economic growth—there are few economic analyses of how the reward structure influences the efficient creation of academic research. In this paper I examine how economics departments determine rewards for the authors of research papers and whether the reward system promotes the efficient creation of research.

This article begins with a demonstration that an efficient reward structure requires strict proration of articles so that the shares of rewards to coauthors sum to the reward that would be provided to a single-authored paper. Survey evidence is then provided to indicate that the reward structure used in Economics for at least the last thirty years incompletely prorates coauthorship, which is consistent with the self-interest of senior faculty who are noted to engage in above average levels of coauthorship. The consequences of incomplete proration should include excessive coauthoring, or at least the appearance of excessive coauthoring, since false authorship is also a rational response to an inefficient reward system. Although these survey results technically are based on data for departments of economics, I believe the results are likely to hold more generally in academia.

Next, the manner in which departments establish the research value of articles is examined. Survey evidence indicates that economics departments prefer to use an *ex ante* measure of quality, the journal of publication, as the main indicator of success even though more direct *ex post* measures, such as citations, are available. This will tend to over-reward workmanlike papers and under-rewards more transformative research. This decision, too, appears to be consistent with the self interest of departmental decision-makers wishing to prevent unsolicited outsiders from influencing the disposition of departmental research rewards.

The causes of increasing coauthorship levels, noted in Economics and throughout much of Academia during the last half century, are then scrutinized. Coauthorship patterns fail to conform with the leading explanation for increased coauthorship, greater specialization and complexity in research, particularly when coauthorship patterns in related disciplines are examined. Instead, the *increase* in coauthoring may be caused, at least in part, by a very slow movement in norms toward the excessive *level* of coauthoring induced by the reward structure.

I then return to the subject of false authorship, which has been noted as a problem in other fields. False authorship allows the included authors to profit from the reward structure’s lack of proration without incurring the diseconomies that would arise from actual excessive coauthoring. Ironically, although false authorship is considered a form of academic dishonesty, false authorship could make the production of research more efficient than it otherwise would be under a reward regime that promotes excessive coauthorship.
A. Full Proration of Coauthored papers is required for Efficient Teamwork

The nature of research is that we hope to learn something new about the world from each correct research paper, although some papers provide more new knowledge than others. Quality differences can be thought of as differences in the amount of some, in principle quantifiable, characteristic of the paper, the measurement of which will be examined in Section C.

The simple model in this section examines only the quantity of papers, implicitly assuming that quality can be converted to quantity. This does not restrict the generality of the analysis as long as quality differences can be compared cardinally across papers. If so, then all papers can be compared in terms of a common denominator, allowing quality differences to be translated into quantity differences, a process that seems to fit the accretive model of research knowledge.¹

Papers can be produced by individuals or teams of various sizes, where some papers are best produced by an individual or small team and other papers are produced most efficiently by larger teams. Is there a reward structure that aligns incentives of academic researchers to choose the most efficient sized team? The answer is very simple: rewarding individual authors for their share of any research paper, no more and no less, will induce efficient team formation and maximize the number of papers written by a given sized research community.

A simple example, assuming that coauthorship is harmful (although the logic holds equally well when coauthorship is beneficial) can illustrate the point at issue. Assume that you have a faculty of 4 individuals writing papers. Assume further that if they write separately they will jointly produce 16 papers a year (4 papers each). Assume also that if they coauthor (2 to a paper) they will jointly produce only 12 papers a year (each pair of authors produces 6 papers). Clearly, it is inefficient for them to coauthor in this case since the total number of papers drops from 16 to 12.

Yet, with coauthoring, each of these individuals can list six coauthored papers on their vita whereas if they write solo, they each have only 4 lines. If the reward system does not prorate vita lines, then the reward for 6 lines will be greater than for 4 lines, and the researchers in this example will be better off coauthoring even though it reduces their joint published output. If, by way of contrast, there is complete proration of papers, then these authors will choose not to coauthor because the prorated value of the six coauthored lines is 3, which is less than the 4 lines they will get credit for if they do not coauthor.²

¹ For example, light bulbs can be compared in a single dimension (light services), say lumens per lifetime of bulb, where longer lasting or brighter bulbs can be thought of as higher quality than shorter lasting or dimmer bulbs. If ‘quality’ is multidimensional, and those dimensions cannot be compared, as might be the case for ‘beauty’, then there is no way to compare different qualities and each quality level is suī generis.

² I assume in the text that authors are equally productive in a coauthored paper, so that the shares that sum to one are equal in size. If authors have unequal contributions, the analysis of efficient proration merely needs to adjust the relative shares of authors to match their contribution, as long as the shares sum to one.
It might be argued that if the proration is incomplete, having each coauthor, say, getting credit for slightly less than 2/3 of a full paper, then the reward system will still achieve the efficient result of inducing solo authoring.\(^3\)

Note that if coauthoring is beneficial, full proration still provides the correct incentives. Change the numbers in the above example so that coauthoring leads to 20 papers per year, compared to the 16 brought about by single authorship. Strict proration credits each author with 5 papers, which is greater than their solely authored 4 papers. Strict proration, therefore, provides authors the correct incentive to coauthor when coauthorship is efficient. As I show immediately below, when the relationship between the number of authors and the number of articles is more flexible and realistic, the lack of complete proration leads to too many authors whereas complete proration leads to the correct number of authors. In other words, the only way to get authors to correctly decide whether to coauthor or not, no matter how the numbers might change in the above example, is to have 100% proration.

More generally, let \(P\) be the total number of publications and \(L\) be the total number of vita lines summed over all curricula vitae.\(^4\) Let \(N\) be the number of authors per paper (assumed to be the same for each paper). In this case:

\[
(1) \quad L = P \cdot N
\]

An efficient reward structure is one that maximizes the number of publications, \(P\), for a given level of research effort. If, however, the reward structure pays a fixed amount for each vita line, independent of how many authors are on each paper (no proration), then researchers (authors) will each try to maximize their lines and this will lead to the maximization of \(L\).

Assume that increased coauthorship increases the number of papers written up to some amount of coauthorship but then decreases the number of papers; in other words, diminishing returns to coauthorship sets in at some point.\(^5\) One simple formulation is to assume that the sum of papers written is a simple quadratic function of the number of authors per paper. Letting \(P\) be the number of papers written, we can represent \(P\) as:

\[
P = \alpha N - \beta N^2.
\]

The change in of publications with respect to the number of authors per paper is

\[
\frac{dP}{dN} = \alpha - 2\beta N
\]

This can be done imprecisely by putting names out of alphabetical order, or more precisely, with a footnote remarking on the relative shares.

\(^3\) Because slightly less than 2/3 of 6 lines is less than the 4 lines they get credit for if they do not coauthor.

\(^4\) I had thought at one time that this conclusion was intuitively obvious, but after decades of dealing with other economists I now believe a more formal demonstration is needed.

\(^5\) Even in the case where the number of publications increased without limit as the number of coauthors increased so that efficiency would require that every author be a part of every paper, complete proration would still be efficient as it would lead to all authors writing all papers.
Publications increase with N as long as N<α/2β. Beyond that, publications fall with additional authors. The number of publications is maximized when N=α/2β. The number of vita lines, from (1), is simply

\[ L = \alpha N^2 - \beta N^3 \]

the slope of which is

\[ \frac{dL}{dN} = 2\alpha N - 3\beta N^2. \]

The number of vita lines is maximized when N=2α/3β. Diagrammatically, this situation looks like Figure 1.

When some level of coauthorship is efficient, meaning that increasing the number of authors beyond 1 increases the total number of publications, the failure to completely prorate papers will always lead to too much coauthoring because the peak of L will always lie to the right of the peak of \( P \). This is because 2α/3β>α/2β.

The simple way to fix the problem of excessive coauthorship is to completely prorate L by the number of authors. This comes directly from (1). Divide both sides by \( N \) and you get

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6. To ensure that the optimal number of authors is greater than 1, we also need to assume that \( \alpha > 2\beta \).

7. This should be clear since when the slope of the function relating number of papers to number of authors per paper is zero (the maximum number of papers), the number of vita lines is still increasing (at the same rate as the number of authors per paper).
Maximizing \( \frac{L}{N} \) is the same as maximizing \( P \) and with this full proration, authors have the correct inducements to produce articles using teams with the most efficient number of authors. This is the simplest and most direct demonstration that complete proration leads to efficiency.

Once this is understood, then it is clear that any reward system short of complete proration provides an incentive for researchers to coauthor beyond the point where it is not efficient for them to do so. Complete proration never leads to too little, nor to too much, coauthoring.

The same analysis holds for the measurement of citations. To the extent that citations are used as a measure of research success, a topic discussed in Section C, they should be prorated so as to not provide incentives to excessively coauthor.

This is a very simple idea. It is merely the avoidance of double counting. It is merely paying the marginal product. These are things we teach our beginning students. Yet, as I show below, the vast majority of departments do not completely prorate publications nor do they come very close to doing so.

**B. In Economics, the Whole Is Less than the Sum of Its Parts**

This potential problem with proration has only been obliquely noted in the literature. Hudson (1996) observed:

> Finally, any net advantage of collaboration may disappear altogether if some individuals combine even though the sum of what each could achieve working alone exceeds their combined efforts. This may occur if an economist can achieve a greater gain in academic reputation from multi-authored rather than single authored papers…it is my impression that at least for work done with only one or two coauthors, an equivalent number of multi-author papers do count for more than one single-authored paper. [P. 115]

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8 There is always the potential for an external effect, such as greater collegiality that might come from coauthorship, which could be a positive factor for coauthorship independent of coauthorship’s impact on the amount of research. Such an external effect would imply that the optimal amount of coauthorship is greater than the level that maximizes the amount of research. Alternatively, it is possible (but highly unlikely) that departmental decision-makers know the true level of ideal coauthorship whereas most faculty members underestimate that level, requiring decision-makers to over reward coauthorship to induce efficient coauthorship levels. In my discussions with senior faculty these possibilities almost never come up so I will ignore them for the rest of the paper.

9 Too little coauthoring would take place if proration went beyond complete proration and awarded a sum of rewards that was less than one. For example, if each author of a dual-authored paper received less than 50% of the total value of the paper this is more than complete proration.

10 Hollis (2001) is one of the few articles to explicitly suggest that the lack of complete proration might be the cause of his finding that coauthorships were largely productivity decreasing. Recently, Card and DellaVigna (2013) observe that coauthorship might be increasing because proration is incomplete.
Hudson’s impression is correct: when economics departments reward research, they do not fully prorate credit for coauthored papers. The first examination of this topic of which I am aware is a working paper I authored with John Palmer in 1983. That working paper reported on a survey asking department chairs the degree to which they prorated coauthored papers. Unfortunately, the material regarding the survey of proration methods never made it into the published versions of the paper (Liebowitz and Palmer 1984, 1988) and I recently threw out the raw material behind that survey thinking, incorrectly, that it must have been included in the published material. Fortunately, both Barnett et al. (1988, p 451) and McDowell and Smith (1992, page 77) report the Liebowitz and Palmer survey to have found that, on average, department chairmen gave each author of a paper with two authors about 70% of the value of a single authored paper instead of a fully prorated 50%.\textsuperscript{11}

Schinski et al., (1998) report on a survey of faculty members in Finance, where respondents reported very similar results: each of two coauthors received, on average, about 70% of the value of a single authored paper, but with wide variations in perceptions. Institutions granting doctoral degrees were more likely to prorate than were institutions without doctoral programs.

In the spring of 2012 I decided to conduct a new study of department chairs asking questions about departmental policies regarding the proration of articles, the proration of citations, and how the quality of articles was determined.\textsuperscript{12} I emailed 56 department chairs, mainly from major departments, with ten outside the U.S., asking if they could categorize how their department treated coauthorship. After two rounds of requests, I heard back from 23 department chairs, all but one of whom were located in the U.S. Two department chairs said they were unable to even roughly approximate whether their departments prorated coauthored articles or not.

Of the 21 department chairs providing numbers, 33\% (7) said their departments did not prorate at all for coauthorship.\textsuperscript{13} Only one department completely prorated papers. The average amount of proration was lower than in the previous studies. For all departments, including those that did not prorate at all, each author of a 2-person authored paper received, on average, 88\% of the value of a singly authored paper. For departments that prorated, each of two coauthors received 82\% of a single authored paper. When proration occurs, it appears to be at only about one third the complete proration amount.

Certainly, there is no evidence that full proration is becoming more common over this 30 year period; instead we find the opposite. We should expect, if these surveys are accurate, that coauthorship in Economics is likely to be above the efficient level.

Inefficiently excessive collaboration among economists is exactly the result found in Hollis (2001). Hollis concluded that coauthorship, holding the quality of the publication constant, led to a decline in the total number of papers written. Although Hollis’ results

\textsuperscript{11} There is no mention of, nor do I remember, how large the sample was for that survey, or how variable the responses were.

\textsuperscript{12} The questionnaire is found in the Appendix A.

\textsuperscript{13} There was no discernible difference in proration based on the quality of departments or whether the department was in a private or public university.
might have been surprising in isolation, they are entirely consistent with the conclusion that the reward structure in Economics (and most likely in other fields as well) induces too much coauthorship.

It should be noted that surveys are not the only method for estimating the reward structure. It is possible to try to infer proration policies by looking at salaries and curricula vitae. Sauer (1988) finds that the relationship between coauthored papers and salaries was consistent with the complete proration of papers and used an ‘outside offer’ rationale to explain why his results might not be inconsistent with the surveys (although the decisions on outside offers are likely to be made by the same people and process used for internal decisions). By way of contrast, Moore et al. (2001), performing an analysis similar to that of Sauer, found that there was no evidence for any degree of proration. Similarly, McDowell and Smith (1992) estimated the relationship between the quantity of publications and the chances of being promoted to Associate or Full Professor and concluded that there was no evidence of proration in these decisions. Ellison (2012) also concludes that proration is far from complete, using a citation based measure to estimate how the degree of proration of citations is reflected in a researcher’s academic position.

My questionnaire also asked whether citations were prorated when they are used in promotion or tenure decisions. There was considerably less proration in the assignment of citation credit than there was in assignment of publication credit. Only one department said that it prorated citations at all.

C. Irrationality in Assessing Research?

It is puzzling that almost no departments of economics fully prorate coauthored articles when economists, by training, should be aware that full proration is the efficient reward mechanism. Admittedly, there is some calculation cost to prorating articles (and citations). But this cost is not high and prorating articles and citations is now automated in Harzing’s “Publish or Perish” program for those who might find the arithmetic too daunting. Further, it seems quite ironic, that at a time when hard-nosed quantitative analysis by the likes of economists is lauded in movies and books about the sports world (e.g., Moneyball), that such analysis is eschewed by economists and other academics in assessing their own performance? Why should that be?

There are two hypotheses, based on simple self-interest, that might explain the lack of proration. A straightforward explanation is that the half of the profession that is above the median level of coauthorship rates will prefer to not prorate articles since the lack of proration raises their relative standing. Even if some faculty members with above median coauthoring levels would forgo personal advantage and agree to rules that maximize efficiency, other faculty interested in advancing their careers will argue against proration. Because of an asymmetry preventing faculty members with less than median coauthorship rates from seriously arguing in favor of more than complete proration, a

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14 Since I am arguing that economists are influenced by self-interest I should mention that although a glance at my record would show that I coauthor less than average and that my citations are stronger than my publications, I am not arguing from self-interest. In the early 1980s, before I had citations and when my coauthorship levels appeared close to average, I proposed full proration of articles and expressed a preference for the use of citations over publications.
compromise position will have less than complete proration. Further, it has been documented that senior faculty members engage in more coauthoring than do junior faculty members, meaning that a majority of the senior faculty will find it in their self-interest to not prorate articles. Since important departmental decisions are generally made by the senior faculty, this factor will further skew the reward system toward zero proration. The normal countervailing factors limiting such self-interest in markets, the attempt by firms to limit the reduction in profit that inefficient behavior entails, are likely to be especially weak here because these are not-for-profit organizations, and the decisions are made largely by the employees, the senior faculty.

A second explanation for the underuse of proration is that the detailed counting of articles and citations that would be needed for complete proration may seem distasteful to some academics. The counting of articles might seem to indicate that there are mechanical cutoff points and boundaries that can be used in judging research output. Holistic methods, where the rendering of decisions are based on reading papers and forming decisions, may seem more scholarly and less accountant-like. It also decreases the likelihood of successful lawsuits for wrongful termination.

Once a policy on proration is set, the value of the paper itself will need to be determined so as to allocate credit to the authors. My survey of department chairs asked how seasoned papers were evaluated when Associate Professors come up for promotion to Full Professor. The reason that the question was limited to Full Professor decisions was so enough time would be available for the number of citations to mature and have relevance.

Respondents were asked to give the relative importance among three frequently used measures of article quality: (a) the journal in which it was published; (b) the number of citations the paper received; and (c) estimates of the quality of the paper determined by department members who read it. Each method has strengths and weaknesses.

The journal in which a paper is published depends on the decision of a handful of editors and referees. These individuals try to make sure that the papers they publish are novel and free from error. They try to publish papers of interest to the journal’s readership. The editors and referees, being human, prefer papers that confirm their own political, methodological, and economic opinions. They also prefer papers that advance the careers of friends and students. Of course, some journal editors may try their best to be fully impartial. But, everyone is human and the last two factors obviously do play a role in the real world.

15 It is difficult to argue, with a straight face, that coauthored papers should be penalized and given less credit, for all the authors summed, than a sole authored paper. But I also thought it would be difficult to argue for less than complete proration and apparently it is not.

16 Conley et al. (2011), and McDowell and Melvin (1983) find that coauthorship activity for a typical faculty member declines for the first few years after the granting of the doctoral degree, bottoms out, and then start upward in a trend that continues, apparently, until retirement.

17 Criticisms and analyses of the peer review process abound. See Cole et al (1981) who find that chance explained 50% of the probability of grant acceptance or Peters and Ceci (1982) who found that psychology journals would often reject articles they had recently published when they were resubmitted with different titles and authors.
Even with complete impartiality, the best editorial decision is merely a hunch about whether the research community will find the paper to be of value—an *ex ante* estimate. A few years after publication, however, it is possible to determine how well a paper is actually doing. Are researchers reading it? Is it on graduate reading lists? Has it made an impact? Counting citations is a simple way to gauge a paper’s impact.

Citations measure a form of *ex post* success. Citations generally measure whether other researchers working on related subjects found a paper to be worth referencing. Papers that successfully question or overturn conventional wisdom will eventually receive many citations although they may have trouble being published in good journals since editors and referees are often the creators of the conventional wisdom. The number of citations is also less likely to be as strongly influenced by personal bias than would be the case for journal publication, since the entire research community is involved with citations and personal influence is not likely to extend to the entire community.\(^\text{18}\) It is true that a paper may be cited as an example of a mistake, or for reporting on data not available to others, or to curry favor with editors, referees, senior professors, or friends. Still, most references are not sarcastic or critical, and although the extent of gratuitous citations is not clearly understood it is possible to go through the citations to try to weed out such cites. Although it is clearly inappropriate to compare citation rates for articles in different disciplines, comparisons within a single discipline should be informative.\(^\text{19}\)

It is important to note that there will generally be a very strong correlation between total citations received by authors and the number of publications those authors have in top journals, so it often will not much matter which metric is used. Articles in better journals are usually above average in quality, are more likely to be noticed, and tend to generate more citations, which is why those journals are highly regarded in the first place. But in those cases where the metrics differ, the citation numbers are likely to be far more informative about the success of a paper than the journal of publication.

Nevertheless, as a measure of research success for senior faculty, department chairs, on average, rely most heavily on the journal of publication, with a weight of 40%. Citations, by way of contrast, were in third place, receiving a weight of 26%, with the reading of articles in between, at 34%.\(^\text{20}\) Further, only 13% of departments give an article’s citations more weight than the quality of the publishing journal. This is analogous to a sports team picking its starting lineup based on the *ex ante* performance of players on athletic tests

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\(^\text{18}\) Gratuitous citations might be an important component of total citations for papers with a relatively small numbers of citations. For more successful papers, however, the importance of these gratuitous cites is likely to be swamped by the citations from relative strangers.

\(^\text{19}\) Different fields can have very great differences in the frequency with which articles referencing other articles. For example, among business journals, articles in strategy and management contain about twice as many references as do articles in economics, according to ISI Journal Citation Reports data.

\(^\text{20}\) One chairman wrote that his department didn’t pay attention to citations because it took several years for citations to reveal themselves and the department cared only about recent research. An extreme case, apparently, of ‘what have you done for me lately?’ It is hard to take such a claim seriously since it doesn’t allow for the very strong autocorrelation that occurs in a person’s publishing history. How someone’s papers have done in the past provides useful information about how they are likely to do in the future. This department’s logic would, by analogy, lead a baseball team to only start players who have had a good batting average for the prior week instead of the players with a history of good performance.
intended to predict success on the field, instead of using the ex post actual performance in games.

Again, there are other self-interested goals pursued by departments and schools that might partly explain this choice of weights. Departmental decision-makers, for example, may be loathe to give up some of their power to the entire research community which is essentially what happens if citations are used to gauge research success. By way of contrast, judging the papers “on their merits” provides departments with the maximum flexibility. Departmental readers of a paper often act as if they recognize the quality of a particular paper better than the overall research community. And in some cases they do, if the department members are of higher quality and more familiar with the subject matter of a paper than the typical authors of papers discussing related subject matter. That still leaves open the question of why departments are willing to allow the decisions of journal editors and referees to play such a large role in the reward structure, since doing so would also seem to remove control from the department. One possibility is that the department needs some ‘objective’ measure of quality with which to help convince university administrators of the soundness of departmental decisions, but because the department gets to decide which journals to count, it can still control, to some extent, the relative rankings of departmental members.

D. The Relentless Increase in Coauthorship

The tendency for coauthorship of journal articles to increase over time has been noted both within the economics literature and for many other academic disciplines. Hudson (1996) examined eight general interest economics journals from 1950 until the early 1990s and found a very low level of coauthorship in the 1950s where generally about 10% of papers were coauthored; then a slow increase in coauthorship rates until 1965, up to about 15%; and after that a much more pronounced increase that continued through the end of his data, 1993. By that time, more than half of the papers in his sample were coauthored. McDowell and Melvin (1983) had previously found, for the same set of journals, that coauthorship increased from less than 5% to over 30%, from 1946 until 1976. Sutter and Kocher (2004) report that, for a sample of fifteen top economics journals, the proportion of coauthored papers increased from 29.7% in 1977 to 53.8% in 1997.

In Table 1, I have calculated, over a somewhat longer time period and using a slightly different measure of coauthorship—the average number of authors per paper—coauthorship levels at ten year intervals. I begin my data with the two years 1940 and

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22 Construction of the data is described in Appendix B. The methodology I have chosen to create this table is low-cost but only generates a sample of articles published by these journals in the period, although I believe that the sample is likely to be unbiased. Briefly, it uses Harzing’s “Publish or Perish” program to provide a list of articles in a journal for specific years. Harzing’s program uses Google Scholar and calculates the average number of authors per article for those articles I chose to be included in the sample. The Google Scholar algorithm makes mistakes and the Harzing program picks up duplicates articles with typos and other problems which I tried to manually remove. The results are not strictly replicable since as Google’s search algorithm changes, the results would change slightly. I try to exclude comments and
1941 and include values for eight general interest journals that were established before 1940 (upon which the average is based) and one specialty journal in a field that is presumed to be less quantitative than most (economic history).  

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These results are mainly consistent with those already in the literature. The growth in coauthorship between 1940 and 1960 is so small as to easily fit within the realm of measurement error but the growth after 1960 is very clear. Coauthorship seems to have been essentially in a static equilibrium, at a very low rate (about ten percent of papers were coauthored), for a twenty year period (and most likely for considerably longer since the coauthorship level is already so small that it could not have been much smaller in earlier years).  

After 1960 an upsurge in coauthoring began. The average number of authors per paper in this sample is currently close to 2 which is almost double the original number of authors per paper during the 1940-1960 period. Note that this increase occurred for all eight of the general interest journals, and that the variation between journals is rather small. Note as well that this increase occurred, although to a slightly lesser extent, in the Journal of Economic History, and as we will see, in other journals outside of Economics as well.

What is the cause of this tremendous increase in coauthorship? Various explanations have been proposed and there have been several attempts to test them empirically.
E. The Leading Explanations for Increased Coauthorship.

There could well be multiple reasons behind the increase in coauthorship. I provide some discussion of the two leading hypotheses\textsuperscript{26} and then discuss how the reward structure hypothesis fits the data.

i. Easier Communication

One explanation for the increase in coauthorship is that it has become easier over time to collaborate on research (Hudson 1996, Sutter and Kocher 2004). Increased coauthorship is an expected and rational response to communications efficiencies. As Hudson states:

Technological developments such as direct dialing, the floppy disk, word processing packages, fax and e-mail have increased the ease of collaborating with colleagues in other departments and other countries. It appears reasonable to suppose that the more individuals an economist has to choose from as potential collaborators, the greater is the probability of finding someone with whom to strike up an effective working relationship. [Hudson, p. 156]

A problem with this explanation, however, is that it is not clear that these costs of coauthorship fell in an important way until about 1990, after a great deal of the increase in coauthorship had occurred. For example, mail and telephone communication have been in continuous operation both before and after the 1960s. It is true that long distance rates fell, particularly after the breakup of AT&T, but the breakup did not occur until 1984. Further, universities often paid for the telephone calls of its faculty members so it is not clear that telephone call expenses would have been an important deterrent to coauthorship.

Floppy discs and word processors moved the typing and production of manuscripts from secretaries to faculty members in the 1980s, but it is not clear that these technologies would have lowered the costs of coauthoring by a great amount. The use of computers prior to the Internet would still require coauthors at different institutions to send discs back and forth. This doesn’t seem like much of an advantage over the pre-PC environment of sending sheets of paper back and forth.

By way of contrast, the costs of coauthorship should have changed dramatically after the adoption of email, at which time (the late 1980s and early 1990s) the back and forth movement of manuscripts and data would have been much faster. At the same time, group authoring tools were added to word processors (e.g., revision marking modes), making working together much easier. The results in Table 1 do somewhat conform to this view since the increase during the 1990s is about twice as great as any other period. Nevertheless, the great majority of the overall increase in coauthorship over the last fifty years would appear to have been due to some other factor(s).

\textsuperscript{26} Although papers in this literature sometimes list more than these two hypotheses, most of the others are usually variants of these two.
ii. Specialization (division of labor)

The leading explanation for increased coauthorship, mentioned in virtually every article on this topic regardless of academic discipline, is based on a presumed increased specialization among researchers. It seems rather self-evident to many commentators that specialization has increased, requiring more coauthorship, although there is little hard evidence to support this view.

Since ‘the division of labor is limited by the extent of the market’ we might expect greater specialization to be related to the number of economists. The field has certainly grown in size since 1940, whether measured by the number of practitioners or by the number of journals, although the latter have grown much more than the former. Coauthors are presumably needed when individual economists become so specialized that they are not well suited to write all the components of an article by themselves.

As attractive and universal as this hypothesis appears to be, however, it is not without problems. A first difficulty is seen in an examination of Figure 1, which lists the number of members of the American Economic Association (AEA), presumably a proxy for the number of economists writing academic articles. Note that the large increase in economists occurred between 1940 and 1970, with membership remaining in something of a holding pattern since 1970. Nevertheless, most of the coauthorship increase has taken place after 1970, including the last 20 years when there has actually been a decline in the number of AEA members. It is, of course, possible that the number of research active economists has increased even while the number of AEA members has remained constant. It is also possible that a lag in authoring norms has held back by decades the new coauthorship equilibrium brought about by the greater number of economists.

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27 These data come from the AEA page http://www.aeaweb.org/AboutAEA/demo_info.php. Data on the number of new doctorates awarded in economics, available from the same source, reveal a similar pattern.

28 Participation rates in the AEA are of potential importance. But from 1962-1985 the percentage of faculty who were AEA members was in the range of 75-80%, and this only dropped to 73% in 1996, the last year for which I can find such data (see the source listed in footnote 27).

29 Card and DellaVigna (2013) note that the number of papers submitted to the top journals has been increasing in the last 20 years and particularly so in the last 10 years even as the number of economists
This division of labor explanation also suffers from the fact that journals and articles have presumably also become more specialized. The number of journals has grown relative to the number of economists, and many new journals that have come into existence seem to cover a narrow and specialized set of topics, as opposed to most of the oldest journals which are mainly general-interest journals.\(^{30}\) If the subject matter contained in journals is increasingly specialized, as seems to be the case, and if this leads to increasingly specialized articles, this would call into question the claim that modern publication requires more knowledge of multiple specialties than was the case decades ago.

Of course, it is possible that the specialization is not one based on fields, but instead is based on technical concerns. Indeed, almost all fields within Economics became more technical after World War II, as suggested by Blaug (2003) who notes that the movement toward formalism in Economics accelerated greatly after the 1950s. Perhaps less technical economists must increasingly join with more technical economists to produce publishable papers.

Similarly, on the empirical front, it is often claimed that modern empirical papers require expertise in finding or creating data, in manipulating data and performing statistical analyses of the data, and in writing up and presenting the theory that is being tested in the empirical paper, whereas in earlier times data sets were small and required less specialized expertise. I will examine these claims in Section F.

Finally, there are other possibilities for specialization, such as having one author with English as a native language now that a majority of economics doctorates from American universities are no longer given to Americans. But this appears likely to be, at most, only part of the story.\(^{31}\)

### iii. The Reward Structure

A reward structure based on incomplete proration provides an incentive to coauthor, even when it is inefficient to do so. Note, however, that there should be a natural limit to coauthorship rates due to diseconomies of production that would seem to be inevitable as the number of authors writing a single paper becomes large enough. In other words, with does not appear to have increased. Further support for increased research intensity of economists seems to come from the fact that in the last few decades the number of journals has been increasing and the number of papers categorized in EconLit has been increasing, although the number of economists has not increased. It is unclear, however, that these facts demonstrate an increased number of papers written since it is possible that a higher percentage of written papers are sent to top journals and that more economists now send failed papers to new journals whereas they used to file those papers away as lost causes.

\(^{30}\) The increased number of journals has been clear, in spite of a lack of growth in the field. I did a quick check by looking at the journals listed in the March 1999 issue of the Journal of Economic Literature where more than half (139 of 233) the journals listed were less than thirty years old (ignoring most business journals and journal titles that included the name of less developed countries).

\(^{31}\) The Survey of Earned Doctorates shows that the major portion of the decline in the share of U.S. citizens earning economics doctorates at American universities began in 1980, a decade and a half after coauthorship had begun its steep climb. Until then Americans earned about 70% of U.S. economics doctorates but that percentage had fallen to about 25% by 2006. For business management, however, half the doctorates were awarded to Americans in 2006 even though there was a larger increase in coauthorship in management journals over this period, as discussed in Section F.
a high enough level of coauthorship, the number of papers produced should fall sufficiently fast due to coauthorship diseconomies that the number of vita lines no longer increases when coauthorship increases.

Why, under these circumstances, would an excessive level of coauthorship manifest itself as a continuous increase spanning a half century, with no end in sight? There are several possible ways in which the excessive coauthorship brought about by an inefficient reward system might be compatible with the almost fifty years increase in coauthorship levels.

First, it is possible that the degree of proration in the reward structure have been falling over time, leading to increasing levels of coauthorship. Although my survey results are consistent with this hypothesis, I am somewhat loathe to conclude that proration has continually declined over the last fifty years and additional evidence on this point is clearly required.

Second, it might take a long time for the researchers to fully adjust to the coauthoring incentives brought about by incomplete proration. Faculty often do not want to break publication ‘norms’ with respect to the number of authors and so might only slowly increase the number of coauthors. An economics article with eight coauthors, for example, would certainly raise the eyebrows of most economists today, although it would appear completely ordinary to medical researchers. Norms change slowly and thus reaching long run equilibrium in this instance really might follow Keynes’ dictum of taking longer than any of our (academic) lifetimes.

If the movement in norms is slow enough, those authors most interested in feathering their nests by engaging in above average coauthoring will write papers with an above average number of coauthors. In a (slow) race to the bottom, other authors catch on to the practice. As the norms changes, the most aggressive authors increase their coauthorship levels some more. If the movement in norms is slow enough, the self-limiting aspects of coauthorship might not be reached for many decades.

Third, it is possible for faculty members to remove the self-limiting diseconomies of coauthorship while keeping the gains in vita lines. The diseconomies from coauthorship arise only when all the authors of a paper actually are involved with its creation. Avoiding the paper-writing diseconomy merely requires having the paper be written by fewer individuals than the number of listed authors. The implication is that the authors resort to a form of false authorship. The only limit to the growth of coauthoring in these circumstances are professional norms and ethical issues.

Ethical problems for these authors can probably be limited, both within the group and externally, by forming teams that work together publishing articles where different members play lead roles for different papers. All group members can talk about the paper and lend some ideas, but the heavy lifting, where diseconomies would most likely come in to play, could be left to a subset of authors, changing for each paper. By avoiding the actual diseconomies of coauthorship through a form of false authorship, there is no limit to how much ‘coauthoring’ can take place. It is even possible, in an attempt to remain free from ethical lapses, that economists devise papers that require different types of expertise, so that multiple authors can be used more efficiently. This type of coauthoring is likely to be particularly sensitive to publishing norms, so as to not attract too much attention. It is also possible that some portion of any greater breadth in styles or
techniques that has occurred in economic articles over the last few decades might be endogenously determined as a response to the reward system, as opposed to the needs of the research.

In Medical Research and in Physics, which have gone much further than has Economics in terms of coauthorships, it is clear that many “authors” have nothing to do with the writing of the papers on which their names appear, as I discuss in Section G. The result appears to be that no one pays attention to most listed authors on these papers and the location of an author’s name becomes crucial, although such information may not be available on the researchers’ vitas. Ethicists in those fields are upset, as they should be, and it appears that some core researchers are upset as well, but these practices continue.

Is there a cost for engaging in false authorship other than possible guilt? I think not. If there has ever been a case where economists (or other academics) were punished for inappropriately having their name included on a paper, I am not aware of it (except for the very different problem of plagiarism).

Left open is the question of why coauthorship began its increase in the 1960s. One possibility is that research first became an important component of compensation for a large number of faculty members at that time, although evidence on this point is obviously called for. Another possibility is that the movement away from writing books towards writing articles, caused by the invention of the photocopier in 1959 according to Liebowitz (1985), might have kick-started the appeal of coauthoring since it might have been easier to share credit for small pieces of work (articles) than for large works such as books.32 A third possibility is that proration might have been more complete prior to the 1960s. But further research appears necessary to answer this question.

F. A Broader Examination of Coauthorship Activity

Hudson hypothesized that high powered theory or empirical work would require a greater division of labor and more coauthorship. He found support for this belief in a subset of journals with higher coauthorship rates that he considered to be more quantitative than his full group. Barnett et al., (1988) performed a test of the division of labor hypothesis using a data set of articles in the American Economic Review over a twenty five year period. After classifying papers as being theoretical, empirical, or both, they found that purely theoretical papers were less likely to be coauthored than papers classified as having an empirical component, a result partly in conflict with Hudson.

I examined a group of theoretical journals containing little or no empirical work and found them to have coauthorship rates that are slightly lower than those for the general interest journals.33 The results were muddied because these journals had an above

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32 The great economics works in the in the period prior to the 1960s were often sole-authored books by the likes of Knight, Keynes, Schumpeter, Hicks, Fisher, Chamberlin, Patinkin, Robinson, and Samuelson among others, although there were also a few influential coauthored books as well.

33 The journals are Journal of Economic Theory, Economic Theory, and Journal of Mathematical Economics which have, respectively, an average number of coauthors of 1.9, 1.88 and 1.75 in 2010-11.
average number of short articles.\textsuperscript{34} I do not believe any firm conclusions can be drawn, but theoretical work did not seem to be a driver of above normal coauthorship.

To further his hypothesis that non-technical work required less coauthorship, Hudson provided evidence that \textit{Economic History Review}, a journal he described as “less quantitative and technical,” had a degree of coauthorship only one fourth that of his other journals. This large differential in coauthorship seemed to provide a compelling hint to the coauthorship riddle. Nevertheless, Hudson’s results are only partially supported in my data. The \textit{Journal of Economic History} experienced an increase in coauthorship that is similar to the other journals in Table 1, although to be fair to Hudson, he was looking at data only up to the early 1990s and the growth in coauthorship in the \textit{Journal of Economic History} was lagging at that time. Looking at three other economic history journals reveals coauthorship rates similar to the \textit{Journal of Economic History}.\textsuperscript{35} Therefore, it does not appear that coauthorship patterns in economic history journals are different enough from other fields of economics to be an important datum in helping us understand the cause of increased coauthorship.

I examined other economics journals and journals in other fields to see whether coauthorship patterns could be discerned. Looking at some lower rated and less mainstream journals (less mainstream even than economic history journals!), I examined the \textit{Journal of Economic Education} for 2010-11 and found the average number of coauthors to be only slightly below average (1.87). The most extreme example of a coauthorship outlier that I could find was the \textit{European Journal of the History of Economic Thought} (1.45) which is edited by a hardy band of individuals who are defiantly swimming against the conventional wisdom that economists do not need to learn any history of economic thought, and whose papers have about half the number of coauthors as the typical economics journal (and whose impact factor is ranked 288 out of 321 journals in economics). Even lower numbers were found for the \textit{Journal of the History of Economic Thought}, although I was dissatisfied with the quality of the data for that journal.\textsuperscript{36} Whether the low level of coauthorship in ‘history of thought’ journals is because it is difficult to find coauthors interested in these topics, or because coauthorship of these articles provides little professional benefit (meaning that there is no gain from these vita lines), or because there is little room for combining different research

\textsuperscript{34} Barnett et al. found that notes had fewer coauthors than regular papers. I examined \textit{Economic Letters}, a journal composed entirely of short articles. It has a coauthorship rate of 1.80 in 2010-11 which is slightly below the average of all journals and weakly supports a view that notes and short articles have fewer authors.

\textsuperscript{35} The journals are \textit{Cliometrica}, \textit{Explorations in Economic History} and \textit{Economic History Review}. The average number of authors in 2010-11 for these journals, in order, is 1.88, 1.84, and 1.65. The lowest of these is \textit{Economic History Review}, the journal chosen by Hudson, which may help explain his results. The \textit{Journal of Economic History} includes many book reviews and shorter papers which I tried to remove by looking at the title of articles. Book reviews were fairly easy to spot but notes were not.

\textsuperscript{36} Coauthorship numbers for \textit{The Journal of the History of Economic Thought} were heavily infected by book reviews although it appears that the number of coauthors is extremely small even after attempting to remove the book reviews.
specializations, is unclear, but the editors of these journals seemed to think there was some truth in each suggestion.\textsuperscript{37}

Casting a slightly wider net in the hopes of finding a useful pattern, I examined leading business school journals, some of which are closely related to the subject matter in economics. One clear pattern emerged: all leading business journals have more coauthoring than the economics journals in Table 1. This includes journals with virtually no quantitative empirical analysis, such as the \textit{Academy of Management Review} (2.39) or its somewhat more statistically-oriented sibling, the \textit{Academy of Management Journal} (2.61). Leading journals in Operations Research, Marketing, and Information Systems all seem to be clustered in the high range of 2.6 to 2.7. Accounting and Finance journals are closer to journals in Economics but still above them, in the 2.3-2.4 range.\textsuperscript{38} The \textit{Journal of Business Ethics} which would seem to have little or no quantitative or statistically based empirical analysis has more coauthorship than most economics journals (2.06), but less than other business journals. What specialized innovations in research methodology could have led to the increase in coauthorship for the \textit{Journal of Business Ethics}, from 1.22 in 1982/83, when it began, to the current level?

The ‘complex (statistical) analysis’ explanation seems to founder in these waters. More generally, if you look at the few journals from these business fields that were extant in the 1950s and 1960s, coauthorship levels look very similar to those in Economics (except for Operations Research, which was higher). Yet coauthorship grew at a greater rate in the business school environment than in economics departments. One explanation could be that authors in these business fields needed the expertise of authors in other fields in order to write research papers, leading to greater coauthorship. This would be worth testing, although my experience is that most authors in a business field write with other authors in the same business field and the extent to which they cross fields doesn’t seem much different than field crossing within Economics. Perhaps the greater pecuniary reward for research in business schools is more of a prod for excessive coauthorship. Perhaps there is even less proration in business schools than in economics departments. Or perhaps the norms changed at a faster rate in business schools. Comparing coauthorship patterns of economists in business schools with economists in economic departments might be a useful exercise to test this norms hypothesis.

I am not comfortable further widening the net to include science or medical journals since I am largely unfamiliar with those fields although Chew (1988) notes that the average number of coauthors in Radiology grew from 1.8 in 1950 to 4.4 in 1985, which is considerably higher than science as a whole where Stephan lists it at 2.86 in 1985. We do know that the seemingly excessive level of coauthorship in medical and science fields seems to be considered an important ethical problem.

\begin{footnotesize}
\footnotetext{37} This conclusion comes from correspondence with Evelyn Forget, editor of the \textit{Journal of the History of Economic Thought}.

\footnotetext{38} The leading business journals that I looked at are found here: http://jindal.utdallas.edu/the-utd-top-100-business-school-research-rankings/list-of-journals/ .
\end{footnotesize}
G. Disciplines where Coauthorship has Run Amok

Coauthorship, or more particularly false authorship, has gotten badly enough out of hand that some fields have adopted ethical rules for when someone should be listed as an author. Here is the rule from the American Physical Society (APS):

Authorship should be limited to those who have made a significant contribution to the concept, design, execution or interpretation of the research study. All those who have made significant contributions should be offered the opportunity to be listed as authors. Other individuals who have contributed to the study should be acknowledged, but not identified as authors.

And from the International Council of Biomedical Journal Editors (ICBJE):

Authorship credit should be based only on 1) substantial contributions to conception and design, or acquisition of data, or analysis and interpretation of data; 2) drafting the article or revising it critically for important intellectual content; and 3) final approval of the version to be published. Conditions 1, 2, and 3 must all be met. Acquisition of funding, the collection of data, or general supervision of the research group, by themselves, do not justify authorship.

I am unaware of any such guidelines in Economics.

These guidelines exist because most analysts in the science and medical areas seem to agree that some nontrivial share of authorship in those fields is fraudulent, meaning that some listed authors had little or nothing to do with articles that bear their names.

For example, Tarnow (2002) reports that physicists believed that on their then most recent paper, 23% of authors beyond the first two did not meet the requirements for authorship based on the less restrictive definition from the APS. The respondents also believed that 67% of the authors after the first two failed to meet the more restrictive definition of authorship from the ICBJE. Further, 59% of the respondents thought that authors after the first two failed to meet a simple criterion of whether they “directly contributed” to the paper.

These are shocking statistics given that a majority of physics articles contain more than two authors (Tarnow reports the median number of authors in his sample of several thousand papers was 3 and the average was considerably higher). If these survey results are accurate, it would mean that a majority of physics authors are engaging in unethical authorship. Virtually everyone in Physics seems to know this. Yet unethical authorship continues. Similar concerns abound in other fields, particularly medical research.39

There is much wailing and gnashing of teeth among journal editors and ethicists in these fields, but they do not seem to fully understand the nature of their problem. Berk (1989), an editor of a leading radiology journal, almost gets it right, but then fails to finish the job in the last sentence of the following quote.

The best solution to the problem would be to devaluate the currency; that is, to decrease the value of coauthorship. This will occur when department chairmen and promotions committees ignore the number of papers published by an individual when considering promotion and the allocation of resources. Only the candidate’s best papers should be considered. [Berk, page 720]

Berk is correct that radiology should ‘devalue’ coauthored papers. The simplest way to do this, however, is to prorate credit for the papers.\textsuperscript{40} Proration will impose a cost on the real authors of the paper who, under the current regime, do not give up credit when they award free authorships to others. With full proration the actual authors would find the value of their own contributions declining when they awarded authorships to others and that cost should deter them from providing most gift authorships.

Economics does not yet seem to have authorship problems of these magnitudes. But I would suggest that it appears to be heading in that direction.

\textbf{H. The Efficiency of Fictitious Coauthorship}

It is clear that measured coauthorship has been increasing over the last five decades in many academic fields, including Economics. The normal presumption in Economics is that this measured increase in coauthorship reflects an actual increase in coauthorship. But, as already mentioned, increases in nominal coauthorship may not be real.

In a world where coauthorship is over-rewarded, we expect excessive amounts of coauthorship, but the excessive coauthorship can take the form of real coauthorship or false coauthorship. The possibility of false authorship makes it conceivable that actual coauthorship might not be rising, or might not be rising to the extent that measured coauthorship is rising.

In such circumstances, social efficiency implies that false coauthorship might be preferred to actual coauthorship even though the former is considered unethical and the latter is not. This may seem counterintuitive, but the idea is very simple.

False authorship does not change the total number of papers published from what it would be under an efficient reward mechanism. False authorship allows authors to use the most efficient teams to produce the research even if those true authors then game the system to extract extra rewards to peripheral team members by providing false authorships. False authorship provides rewards to undeserving individuals but it is unclear how damaging this is to total research productivity.\textsuperscript{41}

\textsuperscript{40} His solution, to only consider the “best papers” presumably means papers where the ‘candidate’ is the lead or second author. This solution has similarities to proration by assigning complete authorship to ‘main’ author or authors while providing zero value to many coauthors. In this case the sum of the shares should be much closer to 1 than is the default of counting all the authors, but is still not as precise as pure proration which assures that the shares sum to 1.

\textsuperscript{41} I am not trying to minimize potential harm from false authorship but merely to point out that the benefits from false authorship might outweigh the harms.
In a world of incomplete proration, false authorship may be a response that allows the production of research to remain efficient in the face of a reward system that promotes excessive and inefficient coauthorship. Jawboning authors to reduce false authorships, by ethicists and academic administrators, to the extent it succeeds, may actually be harmful to the research enterprise. The harm from excessively large authoring teams may be greater than the harm from the claiming of false authorships, in which case false authorship is to be preferred to the alternative.

Of course, the simple and far better way to take care of this problem is to fully prorate articles in the reward structure.

**I. Conclusion**

The efficient production of research is not likely to take place if the reward structure encourages inefficient team production. Because most economics departments fail to fully prorate coauthored articles, there will be an excessive amount of coauthorship, or at least an excessive amount of nominal coauthorship.

Due to the fact that the main decision makers within economics departments tend to be senior faculty, and because senior faculty members tend to coauthor more than junior faculty members, self-interest may be responsible for the current reward system’s over-rewarding of coauthored papers. Self-interest in protecting the prerogatives of local faculty may also explain the under reliance on citations in the rewards being given to more senior faculty.

This excessive coauthorship brought about by the failure to completely prorate authorship credit may be responsible, at least in part, for the increase in coauthorship that has occurred over the last fifty years. Other hypotheses that have been given to explain this widespread increase in coauthorship do not seem particularly consistent with the data on historical coauthorship trends.

Although it is not clear what started the movement toward increased coauthorship, the reward system bias in favor of coauthorship, combined with norms restricting the speed of change in coauthorship, seem capable of explaining much of this steady increase. The serious diseconomies that seem likely to transpire as the number of coauthors grows, may be avoided if nominal coauthorship increases although real coauthorship does not—in other words, if false authorship arises to take advantage of the skewed reward system. With false authorship, there is no economic diseconomy limiting the growth in coauthorship. False authorship, although considered a growing problem in some fields, may actually ameliorate the diseconomies that otherwise would be incurred by excessive coauthorship.

In some academic fields, excessive coauthorship now threatens to make a mockery of authorship itself. Economics may be headed down that path. Although more research on these topics is clearly called for, the subject matter of economics teaches us how to efficiently reward coauthors so as to avoid these problems. We should follow our own teachings.


Ellison, Glenn “How Does the Market Use Citation Data? The Hirsch Index in Economics, working paper December 2012


Sutter, Matthias & Martin Kocher (2004); “Patterns of co-authorship among economics departments in the USA.” *Applied Economics*, 36:4, 327-333.

Appendix A: The Survey Instrument

Dear Department Chair:

I am writing to ask if you can take a few minutes out of your busy schedule to answer some questions regarding measurement of academic achievement. I am conducting an informal survey of department chairs to see how their department measures the publication output and the citations of faculty members coming up for promotion and tenure. If you feel that you cannot speak for your department policy, then please provide your own opinion. The results for individual departments will of course remain confidential.

This is something of a follow-up to my work of almost thirty years ago, some of which was published (with John Palmer) in the *Journal of Economic Literature*. If you participate in the study I will be happy to send you the results.

Here are the questions:

**Publications**

When you evaluate research accomplishments of your faculty:

1. Do you discount coauthored articles relative to solely authored articles, everything else equal? (yes or no)

If the answer to question 1 was “yes” (otherwise go to question #4)

2. Do you completely discount by the number of authors, so that the author of a single authored paper gets twice the credit that each of two authors gets for a dual authored paper, three times as much as for a tri-authored paper, and so forth? (yes or no)

If the answer to question #2 is “no”

3. On a scale from 0 to 100, where 100 represents complete division by the number of authors and 0 represents no division, what would you estimate the division to be in the promotion and tenure decisions in your department?

**Citations**

4. When you examine the research productivity of **full professors**, for relatively mature publications, how much importance (summing to 100%) do you give to (put a % next to each category)
_____ Where (which journal) the articles were published.
_____ The citations generated by those publications.
_____ Your opinion regarding the quality of the articles independent of location of publication or number of citations.

5. Do citations play a role in your tenure decisions? (yes or no).

6. Do you perform any discount of citations by the number of authors for each paper, either by hand or using a program such as Harzing’s Publish or Perish? (yes or no)

If the answer to question #6 is “yes”

7. On a scale from 0 to 100, where 100 represents complete division (of citations) by the number of authors and 0 represents no division, what would you estimate the division to be in the promotion and tenure decisions in your department?

8. Finally, do the opinions above reflect your department policy, or your own opinion? Feel free to explain.
Appendix B: The Methodology of Generating the number of coauthors in journals, over time

Harzing’s “Publish or Perish” program has a tab for ‘Journal Impact’. Type the title of the journal, in quotes, excluding the words “comment” and “reply” in the box for exclusions. Set the years for the two years of interest and click the “lookup” button. You are then faced with a list of articles where the last two columns are “publication” and “publisher”. Google Scholar, the database underneath the Publish or Perish façade, has many errors where names or titles are misspelled. To avoid incorrect listings I ordered both the “Publication” and “Publisher” columns. After doing so, you can remove (uncheck) all listings with the wrong journal (i.e., Scottish Journal of Political Economy instead of the Journal of Political Economy) or incorrect publisher.

I only used listings that included the title of the journal and the website of the journal. Sometimes articles appear in JSTOR as well as the publisher’s web page, in which case I included both. You can arrange the articles by the title to check for double counting although my experience is that an article is likely to show up in one or the other locations but not both. I exclude all articles at SSRN or Repec.

The “exclude” line seems somewhat touchy. Although I routinely put “comment” and “reply” in the exclude line, putting in more terms, such as “Scottish” in the JPE example above, often seemed to remove many more articles than just those with the term you are trying to exclude. I generally avoided putting any term other than comment and reply in the exclude line, but perhaps my experience was not typical.

The articles with the most cites usually have the correct publication and publisher information. Sometimes there are more erroneous articles than correct articles. In this case hit the “uncheck all” button and manually check those articles which look correct.

Once you have limited the articles in this way, take a look at the titles of the articles paying particular attention to articles with a very low number of cites (usually zero) to look for “notes” or memorials, or book reviews, or symposia introductions, and the like. Uncheck the articles that do not appear to be research articles.

When you have limited the listings to those you have confidence in, look at the ‘results’ section of the program and the “authors/paper” listing to find the average number of authors per paper for those papers remaining in the sample. It is also possible to save the results of any search with the “copy” button.