

Are Two (or Three or Four . . . or Nine) Heads Better than One? Collaboration, Multidisciplinarity, and Publishability

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ABSTRACT Although collaborative research has become much more common in the social sciences, including political science, little is known about the consequences of collaboration. This article uses papers submitted to the *American Political Science Review* to assess whether the widely acknowledged benefits of collaboration produced papers that were more likely to be accepted for publication. The results indicate that collaboration per se made little or no difference, but that the disciplinary configuration of the authors did result in differences in the success of these submissions.

Research in the social sciences, which, like the humanities, have traditionally been refuges of individual scholars toiling alone, has increasingly become an activity engaged in by two or more researchers working together. Although collaborative research is more widespread in some social science disciplines (most notably, psychology and economics) than in others (including political science and sociology), all the social sciences are following the lead of the natural and formal sciences, where collaborative research has long been the norm (see especially Endersby 1996; on political science in particular, see Baum et al. 1976; Fisher et al. 1998).

Numerous explanations have been offered for this trend, including the advent of “Big Science,” the expansion of research literatures to magnitudes beyond the grasp of individual researchers, and the trend toward greater quantification (see, e.g., Barnett, Ault, and Kaserman 1988; De Maio and Kushner 1981; Moody 2004; Price 1963). The comparative advantages that teams of researchers enjoy have often been asserted. For example, it is frequently argued that collaboration enables researchers to divide their labor in ways that expand the range of pertinent literatures with which any of them is familiar, bring new methodological skills into play, and produce synergies even when a

team consists of researchers with very similar backgrounds and skill sets.

Still in short supply, though, are empirical assessments of the consequences of collaboration. To be sure, there is evidence that teamwork makes researchers more “productive,” in the sense it enables them to turn out more research papers collectively than they could have done by themselves (Durden and Perri 1995). On the other hand, it should not be assumed that the benefits of collaboration outweigh the costs. For one thing, collaboration involves compromise and may therefore reduce risk taking and innovation, yielding “more technically proficient papers than in the past, but at the cost of the imaginative leap forward . . . in a new direction or . . . fresh impetus to an old subject area” (Hudson 1996, 157). Collaboration may also introduce *diseconomies* of scale. Whereas the rationale for collaboration is summarized by such staples of conventional wisdom as “two heads are better than one” and “many hands make light work,” another piece of conventional wisdom, that “too many cooks spoil the broth,” conveys the opposite idea. Thus, for example, “a multi-authored paper may be more likely than a single-authored one to end up as a patchwork of text lacking a direction or theme” (Hudson 1996, 157).

In short, speculations—many of them contradictory—abound about the consequences of collaboration. Unfortunately, there is currently little to go on except speculation, for reliable evidence is in short supply. Although it has been widely speculated that multi-author articles are cited more frequently than single-author ones, the available evidence is decidedly mixed (see Bridgstock 1991 for

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a dated but still useful review of this research). Moreover, these citation studies are confined to *published* papers, leaving open the question of whether multi-author papers are more likely to pass the prior hurdle of withstanding a rigorous review process and being deemed worthy of publication in the first place.

In this article, I probe the impact of collaboration upon the publishability of papers submitted to a major political science journal, the *American Political Science Review*. Before proceeding any farther, I must caution against any tendency to equate a paper's publishability with its quality. For reasons too numerous to go into here, no journal's review process can realistically be expected to single out for acceptance only the best papers it receives, nor can it be expected unfailingly to eliminate all those of lesser quality. Thus, in analyzing the consequences of collaboration, I refer to a paper's publishability rather than its quality. It is also worth noting at this point that my focus is on a discipline-based journal rather than on an interdisciplinary or a multidisciplinary one. It seems highly likely, as explained below, that this distinction has an important bearing on the results reported here.

PRIOR RESEARCH ON COLLABORATION AND PUBLISHABILITY

Insofar as I am aware, the publishability of multi-authored versus single-authored papers in the social sciences has been the subject of only two data-based comparisons to date. In one such study, Laband and Tollison (2000) documented an acceptance rate of 12% for collaborative papers submitted during the mid-1980s to an economics journal, the *Journal of Political Economy*, as compared to 10% for single-authored papers. In an earlier study, Presser (1980) examined the fate of papers submitted to an interdisciplinary journal, *Sociometry* (later retitled *Social Psychology Quarterly*) during a one-year period in the late 1970s. Of the multi-author papers, 23% were accepted for publication, a higher rate than the 18% for single-author papers; this pattern persisted when outcomes for papers submitted by members of different disciplines (primarily sociology and psychology) were analyzed separately.

Although the results that Laband and Presser reported are intriguing, they are now dated and subject to certain limitations. For example, Laband paid no attention to the disciplinary affiliations of the authors who submitted papers to the discipline-based journal on which his analysis focused. Perhaps the *Journal of Political Economy* received so few submissions from non-economists that that consideration was irrelevant, but the question remains of whether a paper submitted to a discipline-based journal by a team of researchers, at least one of whom is an "outsider," might fare better than a paper submitted to the same journal by two members of the journal's home discipline. That is, it may be the disciplinary configuration of the co-authors that matters rather than multiple authorship per se. That possibility follows naturally from the purported benefits of collaboration, most of which center on the advantages of bringing different perspectives and skills to bear; on average, these attributes should vary more between scholars trained in and identified with different disciplines than between members of a single discipline. Nor did the impact of multidisciplinarity come into clear focus in Presser's analysis. Presser did take authors' home disciplines into account, but he did so by distinguishing between papers by sociologists and those by social psychologists. Perhaps *Sociometry* received so few submissions from mixed-discipline teams that Presser need not have considered them. Even so, his results did not speak to

the possibility that multidisciplinary teams may have enjoyed greater success than single-discipline ones. Indeed, even if some papers had been submitted by mixed teams of sociologists and social psychologists, such teams could not be regarded as having included a true "outsider" because sociologists and social psychologists jointly formed *Sociometry's* core constituency.

In sum, it is not clear at this point whether collaborative social science papers tend to enjoy greater success than single-authored ones, as judged by their acceptance or rejection by the journal to which they are submitted. Although prior research points in that direction, it leaves some important issues unresolved—issues that are addressed in the analyses reported below.

RESEARCH QUESTIONS

The issues driving the analyses reported below are as follows:

- Is there an overall difference in acceptance rates between single-authored and collaborative papers submitted to the *American Political Science Review*, a highly selective journal that is often described as the "flagship" journal of political science?
- If not, would such a difference emerge when the subfields in which papers are anchored are taken into account? In many disciplines, a paper's chances of being accepted for publication vary from subfield to subfield. For example, a paper chronicling some particular episode in the history of the American economy probably would not be a strong candidate for publication in the *American Economic Review*. By the same token, there may be substantial differences among the subfields of a discipline in the frequency of collaborative research. To stay with the same example, economic historians tend to work alone, while experimental economists are much more likely to work in teams. Such subfield differences could camouflage genuine differences in the impact of co-authorship on outcomes.
- Does the impact of collaboration depend on the disciplinary configuration of a research team? Assume for the moment that two political scientists working as a team have a greater chance of having their work accepted by a leading political science journal than does a lone political scientist. Does it follow that a paper submitted by a team consisting of, say, two sociologists would also have the advantage over a paper submitted by a single political scientist? And what about a paper submitted by a political scientist and a sociologist? Would it fare better than a paper submitted by two political scientists, or by two sociologists, or for that matter by either a political scientist or a sociologist working alone? To answer these questions, we must consider the consequences of collaboration in tandem with those of multidisciplinarity.

DATA AND METHODS

As noted above, I address these questions via analysis of the outcomes of the review process for papers submitted to the *American Political Science Review* (APSR) over the six-year period (fall 2001 through summer 2007) during which I served as the editor. During that period, the APSR received 3,252 first-round submissions, not a single one of which was accepted without revision. As a matter of editorial policy, I issued invitations to revise and resubmit papers for further consideration only to those for which the initial round of reviews made ultimate acceptance highly likely.

In a handful of cases, authors who had been invited to submit revisions did not do so, and one or two papers per year for which I had issued revise-and-resubmit invitations were subsequently rejected; the few papers in these two categories were not included in the analyses reported below. Over the six-year period, only about 7.5% of the submitted papers were ultimately accepted.¹

For each of the 230 papers that were accepted for publication over the six-year period, I recorded the subfield of political science with which a paper was most closely associated and the number of authors of the paper, drawing on information in the files that the editorial office had compiled in logging in each submission. In most instances it was easy for me to determine the disciplinary affiliation of each author, but some cases required judgments or even simple guesswork.² To gauge the impact of the combination of collaboration and multidisciplinary, I divided these papers into five categories: those submitted by a lone political scientist; those submitted by a lone member of a different discipline (an “outsider”); those submitted by two or more political scientists; those submitted by two or more outsiders; and those submitted by a combination of at least one political scientist and at least one outsider.

Recording parallel information (especially that pertaining to disciplinary affiliations) for the 3,022 rejected submissions would have been an extremely labor-intensive process. Fortunately, when one is trying to account for occurrences or non-occurrences of a “rare event”—a description that surely fits the low probability (.075) of having a paper accepted for publication in the *APSR*—one need not have all the data on non-occurrences in hand. King and Zeng’s (2001) rare-events logit procedure provides a means of estimating a logit model for rare events with the full set of the rare “successes” (here, acceptances) and a subset of the much more common “failures” (rejections). Accordingly, I drew a random sample of 500 rejected submissions from the population of 3,022 and recorded the same information for them as for the 230 accepted papers.³

RESULTS

The attributes of these papers are described in Table 1. Overall, approximately 55% of the papers submitted to the *APSR* were by a single author, with approximately 30% being by two authors and 20% or so being apportioned in decreasing numbers among progressively larger research teams, up to a maximum of one paper with nine co-authors. The most common subject matter of these submissions was American politics, followed by comparative politics and at a somewhat greater distance by international politics and political theory; only about 5% of the submissions belonged to either the methodology or the formal theory category (the latter being reserved for formal papers that could not be placed in one of the other categories). (This extrapolated subfield distribution conforms closely to the distributions presented in my

annual editorial reports.) The authors of approximately half of the papers were political scientists working alone, and another third of the papers were submitted by teams composed exclusively of political scientists; that left relatively few submissions from outsiders, whether singly or collaboratively, or from mixed teams consisting of at least one political scientist and at least one outsider.

The top portion of Table 2 shows the logit coefficients (*b* or slope terms) and the 95% confidence intervals for each predictor in four different models. Because logit coefficients convey no direct information about the magnitudes of the estimated effects, the results of several follow-up simulations of these effects are shown in the bottom portion of the table. For each model, these estimates were aggregated from 1,000 simulation runs based on the logit coefficients; in Models 2 and 4, American politics served as the omitted or reference subfield, and political scientists working alone played the same role for the authorship configurations in

Table 1
Frequency Distributions of Attributes of Submitted Papers

ATTRIBUTE OF PAPER	REJECTED PAPERS	ACCEPTED PAPERS	ALL PAPERS
Number of authors			
1	54.8	55.7	54.9
2	30.8	28.3	30.6
3	1.0	13.0	1.3
4	3.4	2.6	3.3
5	0.6	0.4	0.6
6	0.2	0.0	0.2
7	0.0	0.0	0.0
8	0.0	0.0	0.0
9	0.2	0.0	0.2
Subfield of paper			
American politics	35.0	29.1	34.6
Comparative politics	27.4	22.6	27.0
International politics	15.2	18.7	15.5
Political theory	13.2	19.6	13.7
Methodology	4.8	5.7	4.9
Formal	4.4	4.4	4.4
Disciplinary configuration of author(s)			
Single political scientist	46.8	53.5	47.3
Single outsider	7.4	1.3	7.0
Multiple political scientists	34.0	32.6	33.6
Multiple outsiders	6.2	5.2	6.1
Multiple mixed	5.6	7.4	5.8
Number of cases	500	230	3,252

Note: The entries in this table are percentages that total, for each attribute, 100±rounding error. For example, according to the first row 54.8% of the rejected papers were submitted by a single author, 55.7% of the accepted papers were submitted by a single author, and 54.9% of all papers were submitted by a single author. The counterpart percentages for papers submitted by two authors are 30.8, 28.3, and 30.6, respectively. Reflecting the fact that the 500 rejected papers analyzed here are a random sample of 3,022 rejected papers, the figures for all papers are extrapolations based on the observed percentages for rejected and accepted papers and on the percentages of all submissions that were rejected (92.4) or accepted (7.5).

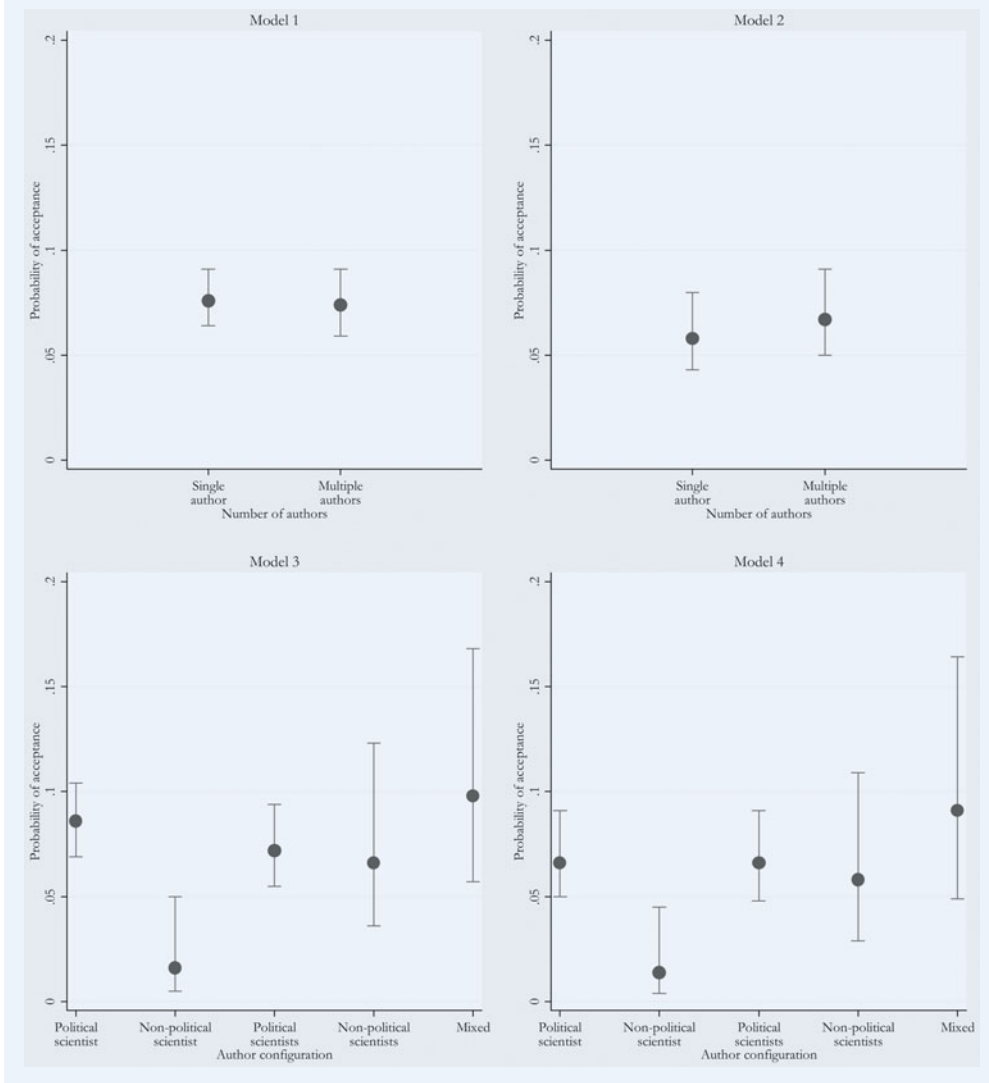
Table 2
Rare Events Logit Estimates and Simulation Results

RARE EVENTS LOGIT COEFFICIENTS AND ROBUST STANDARD ERRORS				
CONFIGURATION OF PREDICTORS	MODEL 1	MODEL 2	MODEL 3	MODEL 4
	B (95% c.i.)	B (95% c.i.)	B (95% c.i.)	B (95% c.i.)
Multiple authors	-0.04 (-0.35 to +0.28)	+0.15 (-0.19 to +0.49)		
Comparative politics		-0.01 (-0.43 to +0.41)		-0.03 (-0.46 to +0.40)
International politics		+0.42 (-0.05 to +0.88)		+0.44 (-0.03 to +0.90)
Political theory		+0.65 (+0.15 to +1.15)		+0.57 (+0.07 to +1.07)
Formal theory		+0.19 (-0.61 to +0.98)		+0.28 (-0.54 to +1.09)
Methodology		+0.39 (-0.34 to +1.12)		+0.41 (-0.32 to +1.13)
Single outsider			-1.71 (-2.90 to -0.53)	-1.67 (-2.86 to -0.48)
Multiple political scientists			-0.17 (-0.52 to +0.17)	-0.01 (-0.38 to +0.37)
Multiple outsiders			-0.28 (-0.98 to +0.42)	-0.15 (-0.86 to +0.56)
Multiple mixed			+0.16 (-0.48 to +0.79)	+0.34 (-0.33 to +1.00)
Constant	-2.49 (-2.70 to -2.29)	-2.77 (-3.11 to -2.44)	-2.37 (-2.59 to -2.16)	-2.64 (-2.98 to -2.30)

SIMULATED PROBABILITIES OF ACCEPTANCE	
CONFIGURATION OF PREDICTORS	PROBABILITY OF ACCEPTANCE (C.I.)
Model 1	
Single author	.076 (.063 to .091)
Multiple authors	.075 (.059 to .091)
Model 2	
Single author, American politics	.059 (.043 to .082)
Multiple authors, American politics	.067 (.050 to .088)
Single author, comparative politics	.059 (.042 to .084)
Single author, international politics	.086 (.060 to .121)
Single author, political theory	.105 (.076 to .146)
Single author, formal theory	.069 (.034 to .141)
Single author, methodology	.086 (.045 to .148)
Model 3	
Single political scientist	.085 (.069 to .103)
Single outsider	.016 (.005 to .056)
Multiple political scientists	.073 (.055 to .094)
Multiple outsiders	.066 (.036 to .124)
Mixed	.098 (.055 to .159)
Model 4	
Single political scientist, American politics	.066 (.050 to .091)
Single outsider, American politics	.014 (.004 to .045)
Multiple political scientists, American politics	.066 (.048 to .091)
Multiple outsiders, American politics	.058 (.029 to .109)
Mixed, American politics	.090 (.049 to .164)
Single political scientist, comparative politics	.065 (.045 to .093)
Single political scientist, international politics	.099 (.068 to .142)
Single political scientist, political theory	.113 (.079 to .155)
Single political scientist, formal theory	.085 (.041 to .180)
Single political scientist, methodology	.100 (.053 to .171)

Figure 1

Simulated Acceptance Probabilities and 95% Confidence Intervals



Models 3 and 4. To conserve space, I present the simulated probabilities of acceptance only for submissions from the American politics subfield, but the very same pattern of results held for the other subfields as well.

The results for Model 1 indicated that the mean probability of acceptance was .075 for single-authored papers and .074 for collaborative ones. Although neither of these estimates was extremely precise, with 95% confidence intervals extending from .063 to .091 for the former and from .059 to .091 for the latter, the two estimates were virtually identical and their confidence intervals overlapped almost in their entirety. Thus, for all practical purposes there was no difference in the success rates that single-authored and multi-authored papers achieved. See Figure 1.

As noted earlier, the possibility could not be dismissed out of hand that the publishability of single- and multi-author papers might differ across subfields. For example, collaboration was especially common among specialists in American politics but relatively rare among political theorists. Thus, what initially appeared to be the non-effect of collaboration may instead have been attributable to subfield differences in both the frequency of

co-authorship and the underlying publishability of the submissions. Gauging the impact of collaboration with that possibility in mind required taking potential subfield effects into account.

The results for Model 2 indicated, first, that acceptance rates did differ across the various subfields, even with the effects of single or multiple authorship held constant. Although subfield differences are not the main focus of this analysis, it is worth noting that, notwithstanding the *APSR*'s reputation for being especially welcoming to American politics-oriented research (and particularly quantitative or formal analyses thereof), it was political theory and international politics submissions that stood out in terms of the success they enjoyed in the review process. More to the point, controlling for subfield differences did little to enhance the effect of collaboration. The simulated acceptance rate for single-authored American politics papers was just one percentage point lower than the rate for their multi-authored counterparts (.057 versus .067)—hardly a wide enough gap for collaboration to be hailed as a boon to publishability—and, as indicated above, the gaps between

single- and multi-authored papers in the other subfields were of similar magnitudes.

To judge from the results of Models 1 and 2, then, whether a paper had just one author or two or more co-authors had little bearing on its chances of being accepted for publication. Even so, these results left open the possibility, noted above, that what really mattered was the disciplinary configuration of authors and co-authors rather than co-authorship per se.

Model 3 poses an initial test of that possibility. Unlike Model 1, which distinguished between single-authored and collaborative papers, Model 3 distinguished among various types of single-authored and collaborative papers. Among single-authored submissions, the author's disciplinary affiliation mattered a great deal, as can be seen by contrasting the simulated probability of acceptance for an American politics-oriented paper by a lone political scientist (.085) and an American politics-oriented paper by an outsider (.016). To be sure, a .085 chance of success would hardly constitute grounds for optimism, but a .016 chance is dangerously close to a guarantee of failure. For co-authored papers, the worst odds were for papers by two or more outsiders, whose probability

of success (.066) fell slightly below those for papers by two or more political scientists (.073) and somewhat farther below those by mixed teams of political scientists and others (.098). Overall, the differences among the three types of collaborative submissions were less pronounced than the difference between the two types of individual submissions.

When subfield differences were taken into account in Model 4, the results for the authorship configurations were essentially unchanged from Model 3. Four of the five configurations were closely bunched, ranging from the highest acceptance rate for papers submitted by mixed teams of political scientists and outsiders to the lowest for teams composed exclusively of outsiders. Leading the field were papers by multidisciplinary teams with at least one political scientist, followed by the other types of papers in which political scientists were involved either singly or collaboratively. Trailing badly (with a 95% confidence interval that overlapped only that of the non-political scientist teams) were papers submitted by a single outsider, with a simulated acceptance rate of just .014.⁴

CONCLUSION

What do these results tell us about the impact of collaboration on the acceptance or rejection of papers by an extremely selective, discipline-based journal? Two basic findings stand out.

- Overall, a paper's chances of being accepted for publication were virtually unaffected by whether it was submitted by a single author or by two or more authors.
- More specifically, single-authored papers fared no worse than multi-authored ones as long as at least one of the authors of the latter was a political scientist. That is, papers submitted by a single outsider fared much more poorly than papers submitted by a single political scientist, by multiple political scientists, or by mixed teams with at least one political scientist.

The fact that the most successful papers were submitted by a combination of political scientist(s) and outsider(s) suggests that incorporating perspectives, skills, and familiarity with research literatures from more than the journal's home discipline can improve a paper's chances of success. The absolute magnitude of this difference should not be exaggerated, but neither should it be dismissed as inconsequential. Given the APSR's very low overall acceptance rate (7.5%), a two- or three-percentage point increase in the probability of success amounts to a substantial boost. Thus, even though multidisciplinary collaboration often involves costs as well as benefits and can in some instances produce more of a mishmash than an integrated product,⁵ the benefits of such collaboration appear to outweigh the costs.

By contrast, the least successful papers, by a wide margin, were those submitted by a single outsider—typically an economist. Economists are often criticized for their “imperialistic” tendency to march into neighboring disciplines without making much effort to acquaint themselves with those disciplines' research literatures. Shortchanging contributions from outside of one's own discipline might matter little when a paper is being considered by a journal in one's own discipline. But seeking acceptance of one's work without paying due heed to prior research in the journal's own discipline—indeed, in a journal generally regarded as the “flagship” of its discipline—is likely to self-defeating.

As noted above, the patterns documented here may not hold for interdisciplinary and multidisciplinary journals, where discipline-based criteria are less likely to come into play. That limitation of these results, however, actually reinforces the major point that emerges from these results: that despite its purported benefits, collaboration per se does not appear to be a boon to a paper's publishability. ■

NOTES

1. The information in this paragraph is taken from the unpublished final report of the APSR editor during this period.
2. For one thing, some political scientists are employed outside of political science departments. Should scholars with a Ph.D. in political science who work in, say, public administration programs or business schools be grouped with other political scientists or with members of the programs in which they are employed? I approached such issues on a case-by-case basis and undoubtedly introduced some error—random error, I strongly suspect—into the categorization. Similarly, some papers simply listed the institution with which an author was affiliated, leaving unspecified the program with which he or she was primarily associated. In such cases, I tried to secure additional information via Web searches, but some of these proved unavailing—hence the need for some guesswork.
3. This data set is available from the author upon request.
4. Not shown in Table 2 are the results of a model featuring interactions between the single- or multiple-authorship of a paper and the subfield of the paper. That specification was tested in order to determine whether collaboration was beneficial in some subfields but inconsequential or even detrimental in others. The results of this more complex specification closely resembled those of its simpler, non-interactive counterpart, casting doubt on the interactive interpretation.
5. For example, in a study of 62 collaborative projects funded by the National Science Foundation, Cummings (2005) found that projects involving more disciplines were no more likely than projects representing fewer disciplines to report positive outcomes.

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