Correlates of Publication Success: Some AJPS Results

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Publication in the American Journal of Political Science (AJPS) is highly valued, largely because of the journal's scholarly reputation. In a recent survey, AJPS placed second among general political science journals. [Looking at their top 20 social science journals, American political scientists gave the following quality ranking, from No. 1 to No. 6: World Politics, American Sociological Review, American Political Science Review, American Journal of Sociology, AJPS, Journal of Politics; see Crewe and Norris (1991, 525, Table 1).] This reputation for quality helps account for the great number of submissions (an annual average of about 265 papers, for the years 1991-92).

Of these submissions, only about one in ten receives initial acceptance (another one in ten receives a revise-and-resubmit, the remaining eight a rejection). Despite these heavy odds against acceptance, some authors overcome them. What predicts publication success in AJPS? Below, we assess what does not help predict it, and what does. These findings, we conclude, lay bare "the paradox of editorship."

Poor Predictors of Manuscript Acceptance

The following five hypotheses are commonly advanced for publication success.

H1: Past Success. (Those who published before are much more likely to be accepted again.)
H2: Field. (Certain fields such as American Politics are favored; certain others such as Political Philosophy are not favored.)
H3: School. (Scholars from prestige schools do better.)
H4: Timing. (The volume of submissions is cyclical, so submission in heavy seasons works against acceptance.)
H5: Turnaround. (The faster the decision letter comes back, the more likely it will be a rejection.)

To test H1, on Past Success, we...
gathered authorship data on AJPS articles (N = 545) published from 1974-92. We found that the overwhelming majority (70%) of authors (defined as the only, or first, author) failed to publish again in AJPS. Moreover, among the 165 authors who got a paper accepted another time, the bulk of them (102) appeared only once more across the 19-year period. At the high end, just 11 authors had more than four publications, 1974-92. Obviously, past success does not give you "a lock on it." On the contrary, a naive prediction would be that, if you have published in AJPS, you probably will not repeat that performance. More realistically, these data on recidivism merely point up the highly competitive market.

To test our other hypotheses, we analyzed data from a current (1991-92) sample (N = 323) of AJPS manuscript submissions. Consider H2, the Field hypothesis. Papers were classified according to whether they were in Political Philosophy, Formal Theory, Methodology, Public Policy, Comparative Politics, American Politics, International Relations, or Political Psychology. Percentage differences in rejection rates across six of these eight fields were generally trivial—from 71% to 83%. Rejection rates outside (and below) this range were found for Political Philosophy (57%) and Methodology (42%), implying that these areas are more likely to have success. However, this implication is misleading; the cell entries are too small (8 and 10, respectively) to infer that these are favored specialties. Rather, the overall conclusion is that papers from different fields fare about equally.

With respect to H3, the School hypothesis, there is a small correlation (r = .17) between author affiliation with a Top 20 Political Science Department (0 = no, 1 = yes) and editorial decision (-1 = reject, 0 = revise and resubmit, 1 = accept). (We reviewed Ph.D.-granting institutions, and assigned a top 20 rank on the basis of the scholarly quality of the political science faculty and graduate program.) This correlation suggests, as expected, that the quality of the department makes some difference. (This would be the expectation, if only because of the greater research resources available to those in Top 20 programs.) However, what is noteworthy is how little difference it makes.

With regard to H4, on Timing, there might be a little seasonality. At the extremes, 27% of the papers submitted in the fall (N = 64) received a revise-and-submit or an acceptance, whereas in the spring (N = 117), the comparable figure was just 17%. Still, this ten percentage point difference is not statistically significant at conventional levels. In reality, an author submitting in any season would face odds that were essentially the same.

Finally, we address H5, on Turnaround. With these 1991-92 submissions, authors received a decision, on average, just under two-and-a-half months after the manuscript was logged in at the Iowa office. (The standard deviation around that value was one month.) When turnaround time (in months) is correlated with the editorial decision, r = .02, there is virtually no linear relationship. Thus, for the expectant author, early news should not be considered more likely to be bad (or good) than later news.

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We expect these rating scores to reflect, strongly if imperfectly, the assessments in the reviewer's extended substantive comments on the manuscript. Therefore, since the editor relies on these substantive comments, we also would expect actual editorial decision outcomes naturally to correlate strongly with these ratings. And they do. That is, for Publish Scale, r = .54; Contribution, r = .52; Advice, r = .51; Breadth, r = .29. Let us explore the first three, the strongest correlates, in more detail.

With regard to the Publish Scale (the 0-10 point rating), once papers pass the "7" mark (an average over the usual three reviewers), the
chances are better than 50-50 that the paper receives either an accept or a revise-and-resubmit. At the other end, if the paper rates no more than a “3,” chances are about 90% or better that it is to be rejected. Similarly, with the Contribution variable, 53% of those scoring “high” were accepted, while none of the papers scoring “none” were accepted. This is strong evidence that, as we had expected, reviewers’ opinions make an important difference.

The impact of reviewer opinion is seen clearly in Table 1, where the last (N) score for each paper. (If the average was less than .5, then it was coded “no” or “not accepted,” otherwise, the reviewers voted “yes” or “accept.”) Similar advice is given if we count as an “accept recommendation” an average value on the 0-10 Publish Scale variable of 6 or greater.) Each issue, then, would be about triple in size, and costs would multiply accordingly. Obviously, nowhere near that much journal space is likely to become available soon. Thus, the AJPS editor, like the editors of other major political science journals, is forced by page constraints alone to make yet another quality cut through the papers, reading over the reviews and manuscripts again before making a final decision.

**TABLE 1**

<table>
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<tr>
<td><strong>Decision</strong></td>
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<tr>
<td>Reject</td>
</tr>
<tr>
<td>Revise and Resubmit</td>
</tr>
<tr>
<td>Accept</td>
</tr>
<tr>
<td>(N)</td>
</tr>
<tr>
<td>(213)</td>
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*For each paper, the responses to the dichotomous vote item (see text, “yes” = accept; “no” = reject) were averaged for the (typically) three reviewers, giving a single vote score for each paper. (If the average was less than .5, then it was coded “0” for the paper as a whole; if the average was .5 or more, then it was coded “1” for the paper as a whole.)

As potent as reviewer opinion is, the table implies that it is not invariably followed. Most notably, 45.5% of the papers where, on balance, the reviewers voted “yes” were actually rejected. This finding lends support to H7, suggesting the editor may exercise some independent judgment. It is tempting to take a normative stance on this issue—should an editor exercise his or her own judgment? For the editor, however, this independence is foremostly practical, due to the exigencies of page restrictions.

The marginals of Table 1 reveal the practical problem. If the editor always followed reviewer advice, as captured in this particular measure, that would mean acceptance of one out of three papers. (That is, overall the vote was to publish 110 of these 323 papers. This advice is not an artifact of the “forced choice” nature of the vote item. Similar advice is given if we count as an “accept recommendation” an average value on the 0-10 Publish Scale variable of 6 or greater.) Each issue, then, would be about triple in size, and costs would multiply accordingly. Obviously, nowhere near that much journal space is likely to become available soon. Thus, the AJPS editor, like the editors of other major political science journals, is forced by page constraints alone to make yet another quality cut through the papers, reading over the reviews and manuscripts again before making a final decision.

**Conclusion**

Reviewer opinion, as expected, is a good predictor of publication success at AJPS. In following reviewer advice, the editor acts as a “delegate,” making decisions on the basis of constituency opinion. However, the editor cannot act solely as a “delegate” of the reviewers. First, reviewers are only part of the constituency. (Besides, there are journal readers, Midwest Association members, researchers, and students, many of whom may not do reviews.) Second, even if reviewers are considered the primary constituency, the standard sample of three reviewers per paper cannot be counted on to estimate perfectly the opinion parameter in the reviewer population. (No matter how carefully the editor sifts substantive reviewer comments, the tiny N problem persists. And, significantly increasing the number of reviewers poses its own difficulties.) Third, supposing reviewer opinions were representative estimates, the journal’s space constraints demand that the editor exercise independent judgment. In exercising this independence, the editor acts as a “trustee,” carrying out decisions under the burden of responsibility the term implies. [Of course, in the political science literature, the “delegate-trustee” distinction can be traced back to the classic of Wahlke et al. (1962).]

There is, then, a “paradox of editorship.” On the one hand, an editor serves as a reviewer "delegate," on the other, as a “trustee” for the larger political science community. As we have seen, these two roles do not always pull the editor in the same direction. [The recent *AJPS* paper by Sigelman et al. (1992) nicely articulates this paradox at the level of elected officials.] Whether the paradox finds a satisfactory collective resolution seems to depend, ultimately, on the quality of the decisions made. That is to say, how good are the papers that are published? The measurement of publication quality is no easy task. However, *AJPS* supporters should take heart, for the journal consistently performs at or near the top of the various quality ratings that have been conducted. [In addition to the recent Crewe and Norris (1991), see Garand (1990), Giles et al. (1989), and Lester (1990).]

**References**


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**About the Authors**

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