Multiple Presentations of "the Same" Paper: A Skeptical View

by
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An increase in the number of political science papers that are presented at multiple academic conferences does not, as Nelson Dometrius recognizes, constitute one of the major existential crises of the twenty-first century. Indeed, I doubt that it would even make it onto anyone's list of the top hundred issues currently confronting the discipline of political science. Nonetheless, it does warrant more attention than it has received in the past (which, insofar as I am aware, is approximately none at all).

For some lesser conferences, program chairs and section heads campaign aggressively to be assigned a large number of panels, only to find themselves later having to beat the bushes in order to scare up enough participants to fill the panels for which they fought. For major conferences, though, the demand to appear on the official program typically outruns the supply of available panel slots, sometimes by a wide margin. The competition for places on the program peaks at the American Political Science Association Annual Meeting, for which the rejection rate for paper proposals now tops 50%.

Why do so many political scientists want to present papers at conferences in the first place—at least at major conferences? The reasons include a desire to receive useful reactions to one's work (alas, a hope all too often dashed in practice), the opportunity to "network" with those who are working on related issues, the chance to get one's name and work into broader circulation, the positive effect that convention papers may have on one's prospects for promotion, a taste for the discipline that a deadline imposes on one's work habits, and—last but by no means least—a recognition that only if one's name appears on the program are one's travel and lodging costs likely to be reimbursed. It is because so many political scientists are motivated by one or more of these intangible and tangible benefits that the demand for panel slots outpaces the supply at major conferences. Of course, when more of what is valued—"goods" for which demand exceeds supply—is allocated to some aspirants, fewer are available to the others. It is largely due to the relative scarcity of panel slots at major conferences that multiple presentations of the same paper can become problematic. If, counterfactually, the supply of panel slots outpaced the demand, then no one would think twice about the willingness of some political scientists to present the same paper at more than a single conference.

But perhaps they should. Consider two different scenarios that share certain features of multiple paper presentations.

The first scenario involves the simultaneous submission of the same paper (a slippery concept that I have more to say about below) to two or more journals. In some scholarly fields, this is standard operating procedure. It seems not to be unusual, for example, for a potential law review article to be under consideration at dozens of journals at the same time. Similarly, within political science a single research proposal may be under consideration at several potential funding agencies, and several potential publishers may have the same book manuscript out for review. In such cases, the scholars who engage in such carpet-bombing do so free of any taint of impropriety. However, in political science, as in most of the disciplines immediately surrounding it, an author who simultaneously submits a paper to more than one journal at a time has violated a basic professional norm. Now, ask yourself why multiple simultaneous submissions of the same paper are not deemed problematic in law but are condemned as unethical in political science. Then, working from that point, ask yourself why, in political science, multiple submissions of a paper to a journal are considered unethical, but multiple presentations of a paper at different conferences are widely held to be unobjectionable.

A key to answering the second of these two questions may be that *serial* submissions of a paper to several political science journals would not be inappropriate; that is, what is objectionable is the simultaneity of a multiple journal submission. Moving a paper down the journal food chain until one finds one willing to publish it is perfectly acceptable.

Well, okay. But that brings me to the second scenario. Suppose that in a course you are teaching on the presidency during the spring semester, a student seeks your permission to submit, for full credit, a paper on the veto power for which he or she already received credit in a course on Congress during the fall semester, or perhaps a somewhat reworked version of that paper. Would you grant the requested permission? I am betting that you wouldn't. Indeed, if you "caught" a student doing what this student has sought permission to do, you may even bring him or her up on plagiarism charges. The question then becomes how this second scenario differs from reaping the rewards (helpful deadlines and criticism,

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networking opportunities, fame and fortune) that one derives from giving the same paper at more than a single conference. Bear in mind that we are now talking about serial, not simultaneous, presentations. Now, I am not accusing those who deliver the same paper twice of plagiarism. However, I do invite you to ponder why you presumably would immediately object to the student's request but you would feel no ethical qualms about doing something similar yourself. Indeed, in one important sense, giving the same paper at different conferences may be *more* objectionable than submitting the same paper twice for course credit. The awarding of grades in a course is typically not a zero-sum game; especially in an age of grade inflation and particularly in a graduate course, the supply of good grades does not necessarily outrun the demand for good grades. By contrast, at a top professional conference like the APSA Annual Meeting, when one's paper proposal gets accepted, someone else's gets denied.

Of course, presenting papers at more than a single conference poses less of a problem if the papers are in fact different. (It doesn't resolve *all* the problems, though; those whose paper proposals repeatedly get turned down tend to get irritated when they see panel slots being hogged by the same people year in and year out and when they hear the same arguments and findings that they heard at last spring or summer's meeting.) But if it is important to distinguish between presenting "the same" paper or a "different" one, then it is also important to recognize the difficulty, in practice, of distinguishing "the same" from "different."

Must a paper remain absolutely unchanged from Time 1 to Time 2 in order to be considered "the same"? At the other extreme, must it be completely different to be "not the same"? There is no simple dividing line or bright-line rule. Imagine, for example, that an author submits a paper to a journal, has it rejected, revises it, and then turns around and submits it, uninvited, to the same journal that has already rejected it. This is not a hypothetical situation, for it happened many times during my stint as editor of the *American Political Science Review*. When it did, I had to make a judgment call about whether the second submission was reviewable—about whether it was really a "new" paper or a "revised" version of the original one. In the former instance, it should go through our review process, like any other new paper we received. But in the latter instance, it should be returned to the author unreviewed, for a rejection letter is supposed to be just that, not an invitation to revise the paper for further consideration; that is, "revise and resubmit" invitations are supposed to be generated by editors, not authors. In making such determinations, I tried to apply the following rule of thumb: Would the original and the reworked versions of the paper be independently publishable, or would publication of the latter render the former obsolete and/or superfluous, in whole or in large part? Only if the second submission appeared to be so different that I would consider it legitimate for the author to try to get both of them published would I regard it as a "new" and "different" paper. In my experience as editor, that was rarely the case. In sum, my rule of thumb was that a revised and, one hopes, improved version of a paper is just that—a reworking of the same paper, not a new paper. Others may or may not like my rule of thumb, but if they want to deal meaningfully with the issue of multiple submissions of "the same" paper, whether to conferences or journals, it is an issue they are going to have to address. In sum, at least by my standards, in the great majority of cases the "It's not the same paper" rationale simply won't wash as a means of sidestepping the potential problems induced by multiple presentations of a paper.

The other commentators on Nelson Dometrius's paper seem to agree that the increasingly common practice of presenting what I would consider the same paper at multiple conferences is not a problem and, indeed, is even laudable. I concede that presenting a paper more than once can be helpful to those who are permitted to do it, for the reasons I noted earlier. What the other commentators have failed to take sufficiently into account, however, is that this practice exacts costs as well as conferring benefits. I tend to doubt, for example, that the marginal utility to A of getting a second round of criticisms of the same paper generally outweighs the utility to B of getting a first round of criticisms—but if A's multiple presentations preclude B's initial presentation (as inevitably occurs at one of the leading conferences), then that will be the consequence. While reiterating that I don't view this as a major issue, my general view is that this is a practice that the political science profession should not be encouraging and should, where feasible, be trying to discourage.

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