

To the Editor:

This letter is being published in *PS* although it pertains to the editorial policy on *The Review*, because the Managing Editor of *The Review* and the President of the Association believe that *PS* is the proper medium for such letters.

To begin, I should like to refer to the Editorial Comment in the December 1972 issue of *The Review*. There the Managing Editor wrote: "Idiots. Irate author writes in and wants to know what recourse he has when the managing editor picks idiots to referee his manuscript and they don't like it and on their advice the managing editor turns the manuscript down." I have some points to make concerning this passage.

It seems a little incredible that any author would write in these terms. My feeling is that there must be something more to the complaint than is presented to us by the Managing Editor. Besides, is it not rather unfair to the complaining author or authors not to let us read what they actually wrote? The Managing Editor for aught we know may have yielded to temptation and presented us with an over-simplified and rather ridiculous version of the author's argument so that he can score an easy victory and appear to be a patient, much-abused man plagued by unreasonable authors trying to force on him manuscripts patently not worthy of publication in the discipline's top journal.

As can be seen my sympathies tend to be on the side of authors because I think editors (and referees) have very great power and there is very little in the way of a check on the abuse of such power. An editor can, and this one does, even refuse to publish letters complaining about his policies while freely availing himself of his editorial space to lash out at his critics.

The Managing Editor of the Review says in the same editorial comment that there is adequate safeguard against error of judgment and injustice towards authors in the existence of other journals in the discipline. This is not altogether true. Great as are the blessings of competition and having alternative vehicles for carrying one's articles authors can pay a considerable price in time and effort going through the long and uncertain processes of refereeing. Much of this can be avoided by more reasonable editorial policies in the matter of refereeing.

I have in mind one particular policy of the Managing Editor and it concerns the treatment of articles that have been revised according to the suggestions of referees. I had the experience of writing an article critical of current aggregate data techniques in the testing of empirical theory. After ten months and a protest I got the article back. The Managing Editor said that the two readers had "found many merits in your analysis" and he extended "an invitation to undertake revision and try us again."

I thought I did a conscientious job of following the referees' suggestions. But the article was rejected with the Managing Editor's regrets. He had sent the revised article to new referees who had no knowledge of the previous referees' views and suggestions. One of them thought that with more amendments it could be published. The other thought it absolutely without merit.

It is my contention that it is quite unfair to an author to have him revise his article according to the suggestions of one set of referees and then to send the revised article to an entirely new set of referees, especially when they are not apprised of the views of the first group. The publication process is hazardous enough under normal conditions, but double jeopardy and a critical article to boot, biases the game altogether too much against the author.

The whole process of refereeing my article from initial submission to final rejection took twenty months. The time factor would have been cut by half and the author dealt with more fairly if the Managing Editor had read the revised article himself to see if the author had carried out the revisions recommended. If the Editor is overworked, this is cause for having an assistant editor not for sending out the revised article as a new article to new referees.

I was a little put out when the article came back and I wrote a letter and asked the Editor of the Review to publish it. He refused because I had quoted from the referees' remarks and he said that publication would be a breach of confidentiality. So I rewrote the letter omitting the referees' remarks. He still refused, suggesting I send it to *PS*. So here it is.

I suppose I must be an eccentric, pugnacious and cranky character to get into a hassle with an editor and to risk acquiring a reputation as a difficult person to deal with. It would be some comfort to me if I could believe in the Editor's "irate author" who calls the referees "idiots." Then I would be able to feel that by contrast I look almost docile and peaceful. Be that as it may, I think my letter may at the very least do a service to authors by warning them of the double-jeopardy policy and hopefully may result in some modification of it if other members of the Association feel the same way about it.

Tom Truman
McMaster University, Canada

To the Editor:

Tom Truman's letter probably requires very little in the way of further comment from me. Our records show that we received an article from him on February 25, 1971. We invited, successively, six people to referee it; two declined, and two neglected to answer us at all.

This caused some delay. Ultimately, two readers did read the manuscript, and the burden of their response was, I thought, that the version we had in hand was unacceptable, but that a revision might make the grade. I communicated this to Professor Truman in a letter dated November 18, 1971. Three days earlier, my assistant had written in response to an inquiry of Professor Truman's explaining the reasons for delay, and apologizing. I should comment that it is not our policy to delay a report for nine months. In this, as in much else, we are at the mercy of our referees; at any rate, our view was, and is, that in this particular Professor Truman had just cause for complaint.

On December 20, Professor Truman replied with a long letter, the burden of which was that he would revise. (I am leaving out some juicy parts.)

I wrote back on January 4, 1973 saying that I would be glad to see his revision and adding "I hope we will be able to get word to you more promptly this time despite the fact that it will be refereed in the normal way. This should take three months." It did: on June 30 we received Professor Truman's revision, and in a letter of October 13 I rejected it, enclosing referees' comments.

A little finger-counting will reveal that Professor Truman has, in his current letter, charged against the *Review* the six months he had his manuscript in his own custody. The initial nine months' delay was bad enough, I should have thought.

I also thought we gave fair warning that his revision would be refereed "in the normal way." When Professor Truman wrote complaining of double jeopardy, I replied:

Revisions are always read by referees, and usually by new referees. As I see it, for a manuscript to be worthy of *APSR* publication, it is not too much to ask that it satisfy fresh readers, when the editor deems it desirable or necessary to enlist their aid. The potential audience for an *APSR* article is, after all, larger than that embodied in two referees. There are many other reasons why new referees are often necessary or desirable. Sometimes original readers are simply not available when a revision arrives. Sometimes I am not satisfied with the quality of the originally proffered advice and want to check the judgment of original referees against other judgments. Sometimes I think a referee is hopelessly prejudiced against an article. Sometimes I fear that reader fatigue has set in and a referee will be perfunctory in his address to a revision.

This, I think, is the best defense I can make of the policy of frequently assigning new reviewers. In some cases it is a necessity, and in many it may be a virtue.

I do not know if this response will give readers adequate data to judge if Professor Truman is "eccentric, pugnacious and cranky," or if I am "a patient, much-abused man plagued by unreasonable authors." A Talmudic scholar of my

acquaintance has asked me how many unreasonable authors constitutes a plague; I am looking forward to a *PS* symposium on this subject.

Nelson W. Polsby
Editor

American Political Science Review

To the Editor:

Peter Sackmann's note on the length of time required to earn a Ph.D. in political science, which appeared in the 1973 Winter issue of *PS* is an interesting attempt to provide useful information to students involved in the field. Unfortunately his findings do not provide what he promised because the criterion used is an inadequate measure of the time required for completing a doctoral program in political science. Sackmann uses as an indice for measuring the time required for completing a doctoral program the number of years elapsed between graduation from college with a bachelors' degree and the awarding of the Ph.D.

Two observations about this criterion may be made. The first is that it reveals a traditional conception of the attainment of faculty status since it implies a progression toward the Ph.D. without interruption of graduate status. Second, and much more important, is the suggestion that such a pattern of progression is the norm since, despite Sackmann's comments in note 4 in which he suggests that "many" students may interrupt their graduate studies, the implication is that such an interruption is the exception rather than the rule. His comments that "even if such cases were not the exceptions from the rule, it may draw an interesting light upon the actual state of the discipline" is difficult to interpret since if such exceptions were the rule his findings would be worthless.

Yet such interruptions may indeed be the norm if the findings derived from a very limited, and admittedly unscientific, survey recently undertaken are prevalent. The survey of three departments of political science (N=27) shows that nearly half of the members of the departments contacted interrupted their graduate education, or did not attain graduate status, until some time after graduation from college for a variety of reasons including the fulfillment of military obligations, service in the Peace Corps, and simply because the Ph.D. was not perceived as necessary when the individual began his teaching career. In addition, two faculty members switched to political science from other professions and thus did not *begin* their graduate education until several years after graduating from college. Overall, the interruptions, or time elapsed between graduation from college and the beginning of the doctoral program, ranged from 1 to 15 years. Yet these years would be included in Sackmann's computation of graduate standing and would therefore inflate the total time required to complete the doctoral program in political science. Indeed, based on rapidly changing social and professional career

patterns, it is likely that more people will interrupt their educations, and that more switching from one profession to another will occur, for example, from individuals in business, and women dissatisfied with the status of housewife.

As it stands, Sackmann's data are at best open to serious criticism and may represent, at worst, a total distortion of reality. What is needed in the future, for a valid determination of the amount of time required to complete all doctoral requirements in political science, is the inclusion of the year in which individuals *begin* their Ph.D. programs. As for the past and present, it might be useful to survey departments of political science in a scientifically acceptable manner in order to determine the extent to which members interrupted their graduate education, or did not attain graduate status, until several years after graduation from college.

Jean P. Richert
Assistant Professor of Law
and Political Science
Richard Stockton College

To the Editor:

This letter hopes to address itself to some of the more immoral and obscene hiring practices carried on in the name of political science departments of this country. This is an intemperate letter. But this subject is one which is too important to hold for forty-eight hours and wait for blandness to set in. The hiring methods of a number of departments has explained to me why, almost by definition, this type of economic market becomes filled with perverts who will use its advantages to any ends, at any costs, no matter the ethical considerations.

Being on the job market, or meat market as it is fondly called by my colleagues, has been both frustrating and humiliating. It is becoming apparent that hiring, by a number of departments, is more and more a game with less and less consideration for the candidate.

Let us get into the specifics of the immorality. Practices like interviewing two or more candidates at the same time; were they supposed to arm wrestle for the position? The unfair practice of demanding an answer immediately for a job position or making the candidate pay his own way to the interview. An even more pressing consideration has become the geographic closeness of the candidate; this blamed on administrative tightening of the budgets.

Probably the most galling practice is the way the APSA Newsletter is used by hiring institutions. The American Political Science Association adopted a policy not long ago in which they said that the most ethical way to deal with hiring was to have *all* institutions who are planning to have job openings announce all positions in the Newsletter. This was designed to give each candidate at least an equitable chance. The hypocrisy of the Association is manifest here. If any simpleton compared the "New Appointments" in *PS* with jobs adver-

tised in the Newsletter, they would get the definite impression that there were twice as many unannounced positions as announced ones.

The other common practice among institutions, which in a way is even more obnoxious than not announcing the position at all, is the placing of an advertisement in the Newsletter after a candidate has already been hired. I, personally, have received announcements that a position has been filled *within* one week after the Newsletter has been received and a letter sent. Some institutions should at least have the decency to cover up their deceit with a short waiting period before mailing rejection letters.

I am not saying that all institutions have employed this practice. Some have given the serious sort of professional consideration which any candidate has a right to expect. But, there are enough institutions employing these practices, that at least some blame must fall on the Association for complicity in these dishonest acts. The least a candidate can expect is an honest evaluation of the job situation from both the institutions involved and the Association. Job hiring for those who have to think about eating next year is not a matter to be taken lightly. It is simply time to get the pigs out of the meat market.

Stuart C. Gilman, ABD
Miami University

[The following is a letter which the author wrote for publication in the Wall Street Journal in response to an editorial but which the newspaper chose not to publish. The Editor of *PS* believes the letter may be of interest to readers of *PS*.]

Are Scholars Always Obligated to Identify Informants?

To the Editor:

Your editorial on Popkin, November 30, does overlook a vital necessity of scholarship. Scholars are constantly faced with the issue of "confidential sources." For example, scholars in the field of the family or sex (look at Kinsey) may get a good deal of information and background on behavior, which would be embarrassing for (or lead to criminal action against) those who provide it, if they are identified. For example, criminologists if they study the dynamics, social and psychological, of crime, must be able to reassure their interviewees that they can be sure confidences about robbery, embezzlement, etc., are not revealed. For example, a good many community studies, undertaken by social anthropologists, in modern countries, go to some pains to disguise the names of the communities, in order that reporting on internal conflict, ill-will, and hostility, may be more honestly done.

As I think without even looking anything up, about three dozen current works come to my mind where the subjects were not identified, that is sources were confidential. Certainly, the reporters for the Wall Street Journal

sometimes refer to "a" something or other as a source, without identifying him in such a way that his mates or colleagues know who he is? The more the Journal deals with crime or local politics, the more necessarily it will have to do this.

In the study, *American Business and Public Policy*, Atherton-Aldine, 2nd edn., 1972, we, my co-autors and I, interviewed a number of members of Congress about the internal dynamics of Congressional action on reciprocal trade legislation. As I recollect, it was typical at that time (1953-5) for members to insist on a promise that they would not be identified; and we made a considerable effort to protect their identity. In consequence, we (and a number of reviewers and commentators) think we learned a good deal about Congressional infighting *as a pattern* which we might not have learned otherwise. We also interviewed several hundred businessmen and lobbyists, and learned a good deal which was at the time regarded as original by scholars, about the relationships between business firms and trade association-lobbying groups, etc. In even more cases than in Congress, the businessmen or lobbyists insisted, sometimes with apprehension, that they not be identified. Since we were concerned with the pattern, not the individual actors, we, of course, so promised. In my study, several times republished on "Congress and the Formulation of Military Policy," (most recently in R. Wolfinger, *Readings on Congress*, 1970), the members of the Armed Services and Appropriations Committees often insisted on anonymity — they did not want the then Chairman or the Pentagon or other Congressmen to know what they felt, and because they trusted me, I think they were more honest than they would have been otherwise.

I myself found this obligation of confidentiality sufficiently important so that I simply refused to tell the members of my dissertation committee who some of the Congressmen I quoted were, because it included men whose discretion I could not be sure of* and I had promised these Congressmen complete anonymity. Were the matter to come to a jury, I hope I would do the same. (In other studies, men in business and politics have told me in some detail about actions which I knew (but they apparently didn't) involved violations of criminal law; clearly, as a scholar, I had an obligation to respect their anonymity there, too.

There is, of course, a possibility under these circumstances that some scholars may invent quotations, may invent interviews, for that matter; and this is a problem as you rightly point out. But the solution, difficult as it is to find, appears to me to lie in the direction of the replication of patterns by different investigators, working independently — anyway the solution does not lie in the direction of revealing who informants are. Then, many informants (especially important executives, etc.) will tell you what they think they ought to be expected to have done and felt; and consequently we will find it even more difficult than it is anyway to find out how things actually happen or are perceived by important participants.

All this may or may not be related to Popkin; but it is directly significant for your editorial.

Lewis A. Dexter
University of Maryland
Baltimore County

*Since PS is chiefly for persons associated with political science departments, I believe I should add that only one member of my committee, by chance, was generally labelled as a political scientist. He did not ask me for this information (merely confining himself to theoretical issues which I found more objectionable!), and I would have trusted his discretion; the issue was in part resolved by the committee's arranging for Ithiel Pool, my collaborator, to sit in on the examination and in effect verify I had not invented anything in the way of data.

P.S. Vice versa, in some situations ANONYMITY can NOT be guaranteed. What I think is far superior to anything else I have ever written, bearing on political science, as generally interpreted, is a study of local politics in Watertown, Massachusetts — I knew that anything I reported would bitterly hurt one of the leading participants in the conflict studied in the very core of his self-image. I knew also that other participants would be irritated and exasperated. Awareness of these points was a factor in leading me constantly to postpone and delay and drag my feet on publication and revision; and it is only now after the most important figure from this standpoint is dead, and after (I hope) time will have made the matters of less account to other participants, that I have faced up to revising it and trying to get it published. This meant that I waited fourteen years. Of course, the tendency to "produce" offsets such abstention, but I think it needs to be part of our ethical code. (I don't claim any great credit here, because this was not a major effort when first started and I was writing a lot of other things and I had other reasons for delay; but I would be ashamed if I had published it when I was first supposed to, in 1960-1. As it turned out, it worked for the best; as the years of reflection let me see far more in the case).