Communications

To the Editor:

"The Great Quota Debate" seems to have developed when affirmative action was extended to cover women as well as minorities. Apparently the large number of women and of women with the necessary employment credentials represents such a potential for change that a number of eminent political scientists have expressed publicly their concern that academic standards will be lowered if universities are required to hire "qualified," women political scientists instead of "the best qualified" political scientists. An important element in this debate is a widely held belief that women are now not discriminated against in the job market, and that, indeed, they may actually enjoy preferential hiring. Because available evidence does not demonstrate this, political scientists should examine placement data carefully before they conclude that the problem is one of reverse rather than of plain discrimination.

In his report for the APSA Committee on Human Resources: Task Force on Placement (*PS* Fall 1972) Thomas E. Mann creates a most unfortunate impression when he states "clearly, women with Ph.D.s in hand are the most successful of all groups seeking positions." One flaw lies in the fact that his data makes no comparison between male and female placements but only between women, blacks, and all candidates. Second, as he himself states, in 1972, the year studied, women represented 12% of the firm job candidates and received a not-out-of-line 12.6% of the placements. Third, while women may have achieved employment approximately equal to their availability, this statistic gives no indication of the kind of placements women received, nor does it suggest whether or not their placements were equivalent to those obtained by men with similar credentials.

Ruth Scott of the University of Utah and I did attempt to answer the crucial question "do men and women of equal caliber achieve equally good placements?" Our data* present quite a different picture from that painted by Mann. Specifically, we compared male and female placements from seven of the "top ten" and three major political science departments for the five year period 1966 through 1970. We found that:

64% of the men and only 56% of the women received full time employment after completion of their prelims;

13% of the men and 30% of the women were not placed at all;

14% of the men and 7% of the women were placed at one of the "top ten" institutions;

98% of male Ph.D.'s and 71% of female Ph.D.'s received full time employment;

51% of male appointments received three-year commitments while 57% of female appointees received only one-year commitments;

42% of male ABD's and 32% of female ABD's were appointed to rank of Assistant Professor.

In short, our study showed that in the recent past women have consistently received poorer placements than have men from the same graduate departments whether the measure was time of placement, kind of institution, level of employment, length of commitment or full as opposed to part time employment.

Our data, of course, does not "prove" discrimination. It may be that women graduates were consistently less competent than the men. However, our attempts to use an objective measure for comparative excellence did not show this. Indeed, no objectives comparative measures analagous to Phi Beta Kappa on the undergraduate level seemed to exist. Sponsorship of individuals by individuals was the usual pattern.

To know whether or not nondiscriminatory employment decisions are being made will require better data than we now possess. If quotas, goals, and data are to be used to make this assessment, at least as many questions as those posed in the Stiehm-Scott study will have to be answered. Others concerning such items as two-career families may also have to be asked.

If results are to be ignored in selecting "the best" new employee, the fairness of the training and hiring process will have to be newly scrutinized. The effect of undergraduate quotas which work for men and against women at institutions such as Harvard, Princeton, Yale, and even Oberlin, the first co-educational college, will have to be examined. The use of employment interviews

* A limited number of copies of our study are still available.

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which personnel studies show to be almost uniformly unreliable and invalid will have to be evaluated. Even intra-department bargaining and trade-offs will have to be controlled so they do not affect job candidates unfairly. It is difficult to be fair and one must look to the data not one's own perception, just as one must not assume the objectivity of long-continued practices.

Judith Stiehm
University of Southern California

To The Editor:

I hesitate to respond to Professor Stiehm's letter since I agree completely with her plea for the collection of new data that directly address the placement problems faced by women political scientists. The discussion of factors contributing to *de facto* discrimination against women included at the end of her paper with Ruth Scott ("A Comparative Study of Placement of Male and Female Ph.D.'s in Political Science," delivered at the Annual Meeting of the American Political Science Association, Washington, D.C. 1972) is an excellent statement of items that we must begin to measure in imaginative ways. Certainly there is no disagreement over what constitutes an important research agenda in this area or that serious problems exist.

What bothers me and I suspect others who have read Professor Stiehm's letter and article is the manner in which inferences are drawn from the available data. The Stiehm-Scott study is based upon a research population defined to include only 427 political scientists over a five year period. My rough estimate suggests this is only about ten per cent of the political scientists coming onto the market. Furthermore, a response rate that varied significantly by sex (female 85%; male-65%) lowered the usable responses to 290. In some cases these were supplied by departments, in others by individuals. Although the nature and quality of placement was crucial to their design, no information on placement *preference* (full time/part time; size of institution; geographical location; timing of placement) was included. Given these constraints, I am very hesitant to place much stock in the differences they report.

The 1972 placement survey I reported in the Fall 1972 *PS* unfortunately included very little information on women political scientists. The limited information, however, was unambiguous and seemed to justify my statement quoted in Professor Stiehm's letter. The comparison between male and female placements that she notes is missing can be computed easily from Tables 4 and 7. The results are as I suggested earlier:

Ph.D.		*ABD*	
Women	*Men*	*Women*	*Men*
92.3%	78.0%	66.1%	63.8%

Obviously this tells us nothing about the quality of placement nor of the comparable placement success of women in earlier years. Nor does it alter the fact that only 12 percent of new political science teachers are women.

Perhaps the next important step in understanding the nature of discrimination against women in placement practices is to conduct intensive personal interviews with a limited, though representative sample of women, at the same time placement figures are being obtained from all Ph.D. departments.

Thomas E. Mann
American Political Science Association

To The Editor:

Martin Shapiro's essay ("From Public Law to Public Policy") in the Fall 1972 issue of *PS* strikes an emphasis that needs to be made, particularly at a time when some opinion leaders in the field persist in clinging perversely to the rubric of "public law" long after it has ceased either to describe what is on-going in political science, or to symbolize goals appropriate for the guidance of future research and teaching. To use Shapiro's example, *The Study of Public Law* (1972) by Walter Murphy and Joseph Tanenhaus, I have the gravest of doubts whether Robert E. Cushman or C. Herman Pritchett (to whom the work jointly is dedicated) would be or is flattered by homage — if that is what it be — that invokes an image of golden sunsets instead of radiant dawns. But I do wish to record my wholehearted support for the general thrust of Shapiro's argument, which is that political scientists should investigate the processes and substance of political policy making, no matter where this may lead them in terms of the traditional ways of cutting up social, governmental, and academic

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pies — pointing as he does in directions that I have thought wise, and that I have tried to follow in my own work, throughout at least the past two decades. (See, for example, my analysis of both state and federal workmen's compensation policy, "Judicial Politics in Michigan" and "The Certiorari Game," pp. 129-142 and 210-254, in my *Quantitative Analysis of Judicial Behavior* [Glencoe: The Free Press, 1959]. See also Arthur S. Miller, "The Impact of Public Law on Legal Education," *Journal of Legal Education,* 12 [1960]: 483-502.) If I had been in a position to advise Shapiro prior to the appearance of his recent article in *PS,* my chief suggestion would have been that he consider amending his subtitle ("or the 'Public' in 'Public Law' ") to read instead: ". . . or Getting the 'Public' out of 'Public Law.' "

Although Shapiro says (p. 413) that political scientists have felt blocked off (by their perceptions of disciplinary boundaries) from studying private law court decisions, in the absence of justifying statutory imprimaturs, certainly Shapiro himself has not behaved as though he felt that way, judging from his publications during the past decade; and neither have I. What *is* paradoxical is that the more traditional public lawmen in political science *always* have foraged off into "private law" issues in their teaching, often while using law school casebooks: thus was I utilizing the Dowling-Patterson-Powell casebook on legal method some nineteen years ago, teaching the judge-made changes in manufacturer's liability through Cardozo's opinion in *MacPherson* v. *Buick Motor* and related state cases. I felt neither inhibited by what law professors might think if they should learn of my encroachment, nor concerned about whether my students were learning public (to say nothing of constitutional) law. And it was out of such unconcern that more than one man's interest in judicial process and behavior began to emerge.

I am mentioned by name in the opening sentence of Shapiro's essay, after it had had been edited, but not as it was originally written, in such a manner that I appear to be assimilated to the position on this issue of Murphy and Tanenhaus who *are cited in the very next sentence following;* and I feel obliged to dissent from the reference to me in that context, on both empirical and theoretical grounds. On the empirical point, the same sentence states that in my own bibliographical summary in the Winter 1972 issue of *PS,* I "employ the phrase 'public law' as roughly synonymous with the legal concerns of political science." My own tabulation fails to support that assertion: out of a total of less than a dozen instances in which the words "public law" appear anywhere in the ten pages of my essay: (1) two appear in quotations of the title of an earlier essay in *PS,* by Robert G. Dixon, Jr.; (2) five are in the context of castigating references to traditional public law work done by persons other than contemporary political scientists ("the older generation [of traditional public law scholars]," "the dogmatic character of the critique emanating from the public law and law school traditionalists during the sixties," "the negative bias toward quantification of public lawyers"); while (3) the remaining four are descriptive of public law, as distinguished from judicial process and behavior ("The traditional method of public law has been and remains case analysis," "A concern for normative theory had long been a hallmark of the traditional approach in public law," and "administrative law [is] another component of traditional public law"). I believe that the only fair inference, from these data, is that in that particular essay (1) I said very little about "public law" as such; (2) what little I did say was pejorative toward public law, or else merely declarative of its peculiar characteristics; and (3) it was no part of my intention in that essay (or in anything else that I have written, so far as that goes) to associate the phrase "public law" with the body of research literature that I discuss in the essay, or with earlier research in judicial process (or allied fields). However inadvertent the remark doubtless was, it is simply false to state that I continue to employ the phrase "public law" as synonymous (roughly or otherwise) with the contemporary legal concerns of political science. Very much to the contrary. I have sought to abolish it from political science curricula and from our working vocabulary as political scientists (except in discussions about the history of our discipline, such as my article "The Future of Public Law," George Washington Law Review, 34 [1966]: 593-614); and it was I who proposed, in remarks at the closing session of the Shambaugh Conference in Iowa City in the fall of 1967, that we renounce public law as the name of a subfield of political science in favor of the rubric "judicial policy, process, and behavior." A majority of the persons there and then assembled could not bring themselves to agree to such a radical innovation — but it will come, probably at about the same time that we as a profession finally are willing to give up our schizophrenic compulsion to persist in referring to the history of political philosophy, as "political theory." I do not, however, believe that such matters of academic ideology are now, or will be in the future, resolved "absentmindedly," (p. 413). For some persons such conceptualization takes place subconsciously, no doubt; but for most, the issue is a facet of the very down-to-earth and gutsy matter of defining public policy for and within the discipline; and in my experience, most colleagues are quite self-consciously aware of just what they are up to when they speak and act in regard to what they consider to be their own baliwick.

Glendon Schubert
University of Hawaii

To the Editor:

This is to protest the misrepresentations, false attributions, and generally slippery nature of the letter circulated by the Ad Hoc Committee, October 12, 1972. While the Nixon Administration has surely familiarized us all with these tactics, and while I have never considered the APSA to be a refuge, I am surprised to observe these strategems in this new context.

The Ad Hoc Committee's letter alleged that the CAUCUS advocates the transformation into "a political action group" and that it urges "the support of partisan political positions" and that the CAUCUS has sought to make the APSA "the political instrument of a few members." It urges members of the profession to repudiate a candidate, H. Mark Roelofs, nominated by the APSA nominating committee (and the CAUCUS) to the Executive Council, without mentioning him by name. Apparently, Professor Wahlke has not troubled himself to read the paper delivered by Professor Roelofs, "Political Science and Political Commitment," delivered at the most recent APSA meeting, in which the above aims are specifically rejected. The refusal of Professor Wahlke to connect the name of Professor Roelofs to the crimes which he is alleged to have committed bears a disturbing resemblance to the tactics of the Nixon Administration. Whatever the source, what can possibly be the justification of alleging specific crimes to a person whose name is never revealed? It appears to be a phenomenon of the times that those who pay the greatest lip-service to the virtues of liberal-democracy, to the vitality of the electoral process, and to the raising of standards of scholarly discourse, are those most likely to desert these standards when their partisan political interests are at issue.

Frank M. Coleman
State University College, Geneseo

To the Editor:

In the Summer issue of *PS* (p. 389), Heinz Eulau set out to test the proposition that "the same crowd, year in and year out, dominated paper giving at the annual meeting." He asked the question: What proportion of the contributors are repeaters? His answer for the period 1956-1969 was 22 percent.

In the Fall issue of *PS* (p. 497), I argued that the wrong question had been asked. The right question would be: What proportion of the papers at the annual meetings are produced by the repeaters? I made a simple calculation, using Eulau's data, and came up with about 40 percent as the answer.

Also in the Fall issue, same page, Eulau comments on my approach. He shot down my simple answer, but then went on to obfuscate the issue of which question should be asked, and provided no further answer to either. He did, however, produce some new data which do something to clarify the difficulties of the calculation. The problem, it appears, is the number of papers with multiple and overlapping authorships.

In his revised data, Eulau counts a total of 1,380 papers presented during the years 1956-1969, of which 1,271 were single author papers. Of these, 780 were by authors who appeared only once and 491, or 38.6 percent, were by authors who appeared more than once. Teams of multiple authors did not appear as repeaters as often as single authors, but the single author repeaters undoubtedly made up parts of teams in many instances, as Eulau's illustrations make clear.

Probably the only way to get a complete answer to the question I posed would be to split the co-authorships into fractional papers for each contributor. On this basis, for example, a member of an authorship team of three would receive credit

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for one-third of a paper rather than a whole paper. It seems clear, however, that the proportion of the papers provided by the repeaters is much greater than the proportion of the repeaters among the population of paper givers. Mathematically, it has to be.

Paul T. David
University of Virginia

To the Editor:

I was depressed yesterday when a colleague informed me he had turned to a firm of specialists for assistance in writing a research proposal. It was not that they and not us would skim off the overhead that every institution can use. It was rather that an internationally reknown scholar of great distinction no longer feels secure in submitting a proposal written in his own words with material he alone considers relevant. Have we indeed reached the melancholy state where form dominates substance in the development of research proposals?

I felt the need to protest. But to whom? To the foundations governments and other beneficent institutions who set the standards for the design of research proposals? Or perhaps to the researchers themselves who conform to and thus perpetrate the absurd formalities that have come to dominate grantsmanship? As a starter I settled on my own professional association partially because the misanthropy of grantsmanship has political implications. For as the situation now stands, skill in writing research proposals commands excessive power in establishing research agendas, and thus in producing the related policy options that frequently research turns up.

I have sat on both sides of the grantmaking fence. Frequently, as a proposer or a disposer I have participated in and witnessed a game played according to a standard set of rules. The essence of a proposal is stated in a few pages, which more often than not provides enough information on which to base a judgment. The proposal then is "fleshed out" with voluminous and marginal material, frequently pulled out of the files of previous proposals, mocked up to fit the new proposal, and then pasted together. Everybody knows what this filler consists of — real or imagined track records, biographic data from the collaborator or consultant file, publication lists, and the rest. It all adds up to compiling a lot of weight and volume which too often bears an inverse relationship to merit. But this is how the game is played. And this is why there is a productive function for those who specialize in playing this sort of game.

Where does it all stop or at least turn around? How about a single strike in which the workers refuse to conform to the more absurd demands of management? But this recourse is utopian because they have us by the jugular, i.e., we need their money to survive.

Perhaps the provisioners should be more rigorous in monitoring their own performance on the basis of a sort of industry-wide compact establishing reasonable standards. Here there is some cause for hope. The foundation I worked for never was snowed by volume and placed a premium on substance rather than form in its review of proposals. Although the U.S. Government appears to be the biggest culprit among the provisioners, there is even evidence of some light at the end of that big tunnel.

Recently the Interdisciplinary Communications Program of the Smithsonian Institution called for proposals for its new population analysis program by refreshingly saying in effect: "Tell us what you want to do in any way you want to tell it to us."

Let us hope this reasonable and sensible precedent catches on.

Elihu Bergman
Assistant Director
Center for Population Studies
Harvard University

To the Editor:

Faculty who have recently or are currently involved in war/peace or peace education courses, majors and programs (undergraduate and graduate) are asked to send copies of outlines and descriptions to Dr. Sandi E. Cooper, Richmond College — C.U.N.Y., Staten Island, New York, 10301.

The purpose is to provide an up to date listing

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and survey of current offerings in these areas. This study is undertaken on behalf of the Conference on Peace Research in History. It ought to be available in 1973.

Sandi E. Cooper
Richmond College, CUNY

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