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## VIEWS AND OPINIONS

### ON PROFESSOR NEUMANN'S "COMPARATIVE POLITICS"

Though he might not accept this phrasing, I should say that Professor Neumann has raised in this paper the two fundamental questions with which comparative government must concern itself in our time — or perhaps in any time. The first is the problem of what we may expect to learn about politics by the comparative method and the second, closely related to the first, is how we may best set about learning it. What distinguishes this paper from many others is the author's ability to view these questions in a rich historical perspective. Indeed his essay is most welcome in these days when political science is worrying the methodological problem like a slaving dog with a juicy new bone. His paper might well be called "The history of efforts by students of comparative government to define for themselves the scope and method of their discipline" or perhaps, "Comparative politics: is it a discipline?" It is helpful, and even sobering, to be reminded that these questions are not new, even though our answers to them may differ from those of our predecessors. Dogs have been delighted by bones for a long time and the bones have been pretty much the same even if the newest one does seem to differ somewhat in contour and succulence.

Neumann's historical classification is both interesting and illuminating. I was fascinated by his description of the conditions that helped give form to the early studies of the state in Germany and by his showing how this special character of German political science shaped the work and biases of political study in America when it was formally inaugurated by German-trained American scholars. But the significance of his classification, it seems to me, lies in his portrayal of the evolution of the nature, goals, and techniques of political study America. He points an unerring finger at the movement from the normative, descriptive effort of the early years into the disillusioned postwar period with its more modest goal of "the concrete and detailed study of material forces." With

this I can find no quarrel.<sup>1</sup> And he now suggests that this second period of "material positivism" has given way to greater or at least different ambitions. Perhaps it has. Certainly comparative government is now a-bubble with new ideas, which indeed have led us into such self-consciousness that we are no longer content to study "comparative government" but insist that our province is that of "comparative politics," as witness the title of Professor Neumann's article.<sup>2</sup>

But whether we are now undergoing so profound a transformation as this essay suggests, I am not sure. The difficulty is that any such historical classification must be to a large extent arbitrary, as Professor Neumann would doubtless be the first to agree. Clearly there were empirical, positivistic studies being conducted before the First World War; just as clearly they are still being conducted today. Utopianism and normative analysis did not entirely disappear with the disillusionment of the thirties. And many of these things are still with us today. History, even of "scientific" disciplines, does not fall into such careful patterns, or the work of their practitioners into such neat categories. The article does not, of course, rest on the assumption that they do; Mr. Neumann is pointing to changes in emphases and he has done this with skill and insight. But the most elusive insights and the most difficult analyses are often those concerned with one's own age and it is here that Mr. Neumann leaves the greatest room for suggestion.

Our discipline is nowadays undergoing a searching self-analysis, so ambitious in some quarters that foundation grants have been procured to abet the effort. And the upshot of it is that we are discontented; we are fearful that comparative government has been concerning itself all these years with the wrong things and using the wrong means of accomplishing its purposes. We have seen our colleagues in other social sciences glorying in their new preoccupation with human behavior and their efforts to apply quantitative methods to its explanation. Inevitably our own concern has turned to the fundamental and ever-fascinating question of method and there is now much breast-beating about our de-

<sup>1</sup>One can follow some of the same disillusionment in Jasper B. Shannon's entertaining "An Obituary of a Political Scientist" in *Journal of Politics*, XIII (1951), pp. 3-18.

<sup>2</sup>Witness also a volume by E. A. Freeman entitled *Comparative Politics*, which was published in 1873.

ficiencies on this count. Perhaps surprisingly we seem to be fairly well agreed on what these deficiencies are. I should think there would be little challenge to the assertion that comparative government has been too narrowly descriptive, has concentrated too much on form and structure to the exclusion of actual political behavior and the dynamic processes of politics, has made too little use of the techniques and findings of other social sciences, has concerned itself too little with politics in the non-Western world, and has been insufficiently grounded in political theory.

But if these are the errors of the past (or present) we must not assume that merely reversing all these bad habits will provide us with uniformly good ones. Many of these things that we complain of we shall have to keep on doing; otherwise our effort will become as sterile as we say it has been in the past. For example, comparative government will never be able to abandon the attempt to describe institutions, for our descriptions and analyses must serve at least as the raw data of our comparisons; and our descriptions can never be adequate, for as time passes and institutions change, the data that have been collected lose their relevancy. As old institutions are transformed and new ones appear, new analyses and new descriptions must be provided.

Secondly, it would be a mistake to allow the present enthusiasm for the study of processes and behavior to carry us to the point of denying the importance of form and structure. These, too, are part of the stuff of politics and we ignore them at our peril, for it is in relation to them that the dynamics of political life often find their meaning. I should concede (and even insist) that our turning toward behavior will provide a more realistic interpretation of politics and a better framework for comparative study. But we must not recoil so strongly from the felt evils of the past that we throw ourselves too far in the other direction. Perhaps we should acknowledge the possibility that the present emphasis on behavior is gratifying in part because it provides a vantage point for criticizing our predecessors and thus proving our own superiority. Indeed it may be that a few years will bring another disillusionment and we will (without abandoning our interest in behavior) readmit to respectability the ancient studies of public law and constitutional taxonomy.

One reason for this greater emphasis on dynamics and behavior

may well derive from the determination to ally ourselves more closely with our sister social sciences, and especially with sociology, psychology, and anthropology, which are the real arenas of the new behaviorism. We are quite properly urged to draw upon them not only for their findings but for their techniques as well. To this one can scarcely object: clearly we must all pursue the truth in common cause. Indeed, I should be the first to proclaim the necessity of recognizing the economic, social, historical, psychological, and cultural determinants of any system of politics. But it seems to me that what is most often meant when our methodologists adjure us to learn from the other social sciences is that we should concentrate upon the use of quantitative techniques for the measurement of political phenomena. Indeed this seems to be a far-reaching concern of political scientists today and here I find myself in an apparent disagreement with Professor Neumann. If I read him correctly, he seems to see a waning of the positivism of his second stage; he finds that our fact finding is now "directed by fundamental questions reflecting the researcher's aspiration." I hope this is so, and I concede his examples, but I still need convincing. It seems to me that in our ardor to be scientific, in our determination to study not forms but behavior, in our commitment to the research techniques of other social sciences, we are still in danger of dwelling so exclusively on quantitative measurement that we stultify our effort to enlarge our horizons and enrich our understanding. I agree most strongly with Professor Neumann's caveat concerning these techniques. If, as some appear to demand, we devote ourselves exclusively to those things that can be measured quantitatively, we risk spending all our efforts on matters that are of less than prime significance—and, what is even more dangerous, we risk assuming that these *are* the matters of prime significance. Obviously where these techniques are appropriate they should be used; accurate measurement is certainly worth more than sloppy guessing. But let us not distort the field of our inquiry by restricting it unnaturally to the things that can be so measured. I do not plead for less than accurate assessment but I should argue strongly that there is still opportunity and need for hypothesis and theory that embrace more than the phenomena appropriate to quantitative measurement; there is still plenty of room for reflective analyses, for insights and interpretations, that do not depend on charts and tables.

The fourth item in our *mea culpa* is the assertion that we have paid too little attention to areas outside the continuum of western culture. Clearly this is so and clearly we are now doing something important about it. A theory of politics that ignores half the globe is manifestly insufficient. And yet a caveat seems appropriate here as well. I hope we shall not turn enthusiastically toward these new areas with the feeling that we have spent enough time on Western politics and that the only real challenge lies beyond these new frontiers. The truth is that the West is by no means exhausted as a field of productive political inquiry and, as a community of scholars, we must not abandon our continuing examination of Western institutions, however laudable and badly needed is the effort to study those of the "non-West." We have not yet been able to formulate an adequate system of generalization about politics in the traditional areas of inquiry; how much more difficult it will be therefore to spread the field of our study over these other tremendously broader and more varied areas which differ in so many fundamental features from the political patterns of the West.<sup>3</sup> More than ever before we shall be confronted with a massive array of variables awaiting analysis and comparison. Certainly we must make the effort but let us not assume that results will come easy. I do not contemplate the early appearance of any general theory or even of any very significant system of generalizations that will serve all areas and all political systems. The differences in the determinants of political behavior and institutions are still far more impressive than the similarities, even in the West. The non-West displays even more variables; confronted by its obvious importance, perhaps we are too ready to assume a homogeneity or a sub-stratum of common (and comparable) elements where none exists. Comparative government must doubtless shift its forces to deal with these new areas and problems, but shifting its forces does nothing to improve its weapons. We shall not improve the prospects of a science of comparative politics by assuming that the non-West possesses a homogeneity that we have never found even in the West. This necessary extension of our frontiers, while greatly increasing our opportunities, also greatly multiplies our problems and all the more urgently de-

<sup>3</sup>A tentative catalogue of fundamental differences between Western and non-Western politics may be found in George McT. Kahin, Guy J. Pauker, and Lucian W. Pye, "Comparative Politics of Non-Western Countries," *American Political Science Review*, XLIX (1955), pp. 1022-41.

mands from us a new conceptualization of our task and a reorientation of our political theory.

Which brings us to what may be the most important part of Professor Neumann's article, namely its demand for a "theoretical reorientation of the whole field." Such a summons must, of course, be addressed to all of political science, but it appears to me that the need is most critical in comparative government. Our need here may be said to lie in two different planes, the one methodological and the other more properly theoretical. By the methodological I mean that research in comparative politics must always be informed by a sound conception of what one is seeking to explain. A lack of such orientation results merely in the massing of random and unrelated data of little use in comparisons and of no use in building a theory. The student must begin with some kind of suppositions or hypotheses about his area of inquiry in order to inform his activity, to guide it meaningfully through the maze of phenomena available for study, and to give form and substance to his conclusions. But in the other, and more important plane, what we shall need is a new effort to contrive or perceive a theory or general explanation of these phenomena, a need that is made critical by the reshaping of the world in our time and by our own new emphases upon behavior and upon the non-West. The old categories of thought, the old classifications of institutions and processes, the old nomenclature impregnated with traditional values—all these must give way to a new kind of comparison, to new labels and categories, to the perception of new relationships, to new explanations; in short, we must look to a conceptual retooling of the discipline. Our world has suddenly grown much larger and we must equip ourselves to deal with it. Theories and explanations that were satisfactory for comparative government are not good enough for comparative politics.

All this makes an ambitious program. It will require the kind of original thought of which most of us are not capable. It behooves us therefore to move somewhat cautiously, taking care to preserve what we know and need while searching for the new. Let us, by all means, lift up our eyes "unto the hills," but let us also keep our feet solidly on the ground. One of the great virtues of Professor Neumann's article is that in it he combines an enthusiastic ambition for his discipline with a cautious judgment of its expecta-

tions. It is his own kind of thinking that will lead the way to the new orientation of which he speaks.

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One has no difficulty in accepting the two major premises of Sigmund Neumann's admirable "tentative appraisal" of the course of political science in the United States in the last half-century. These I take to be (1) that the historical dimension has a peculiar significance for political science, and (2) that the comparative method is essential to any political science worthy of the name. I wish only to bring certain aspects of the historical perspective that Neumann has provided into sharper relief and to emphasize the point that comparison requires criteria for differentiation as well as for the discovery of similarities.

For the purposes of this discussion, I assume that political science concerns itself with those power relationships among men that are involved in the ways in which communities cope with the challenges of community life. The most superficial examination of the circumstances of communities in different times and places indicates that these challenges are not always the same, that the ways of meeting them cannot be reduced in description to a common pattern, and that the components of the power relationships involved vary significantly. This is why any science which essays to comprehend the phenomena of politics systematically must use the method of comparison. This is what Neumann says more impressively in the pungent remarks: "By its very nature, political science is embedded in time and space," and, later, "Social concepts evolve by stages, remaining necessarily fragmentary and tentative and at best present merely a useful working hypothesis for a deeper penetration into an ever-changing reality."

The conceptual tools used by any scientist must be suited to and are, to some degree at least, determined by the phenomena or configurations of phenomena to which he directs his attention. But the phenomena which come under the scientist's observation