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DISCUSSIE

POLITICAL SCIENCE VERSUS POLITICAL ADVOCACY:
 COMMENTS ON THE ARTICLE BY MARVIN SURKIN AND ALAN
 WOLFE *

by Arend Lijphart

In their eloquent critique of what they consider the 'dominant trend' in American political science, Professors Surkin and Wolfe raise a number of highly important issues¹. Unfortunately, the picture of American political science that they present is a thoroughly inaccurate and misleading one. Of course, American political science and American political scientists are by no means perfect. The discipline has indeed not always been sufficiently relevant; some of its practitioners have indeed not always adhered sufficiently to high standards of objectivity and impartiality; some of its predictions have been quite inaccurate; and its professional organization, the American Political Science Association, has indeed not been sufficiently democratic. But these weaknesses, are not serious enough to warrant an indiscriminate indictment of the entire discipline (p. 43). And the alternatives proposed by Surkin and Wolfe constitute a distinct threat to the development of political science.

It is not inappropriate that these issues are discussed in a non-American journal of political science because of their intrinsic and fundamental importance for political science everywhere, but also because of the great influence that American political science has had on the discipline in other countries. Perhaps especially outside the United States, the approaches and products of behaviorism have been accepted too readily, because they represented the most modern and attractive developments to the largely underdeveloped discipline abroad. Let us not make the same mistake again by an uncritical acceptance of the newest development in the form of the antibehaviorist challenge².

The most disturbing aspect of the article by Surkin and Wolfe is that they make extremely serious accusations based on entirely inadequate, distorted, and unrepresentative evidence. First of all, the alleged high degree of methodological and political consensus among American

* Belangstellende lezers worden verder verwezen naar de in dit nummer opgenomen tekst van het 'presidential address' van David Easton.

¹ Marvin Surkin and Alan Wolfe, 'The Political Dimension of American Political Science', *Acta Politica*, Vol. 5, No. 1 (October 1969), pp. 43-61.

² Many of these issues were discussed earlier in the excellent article by A. de Swaan, 'Theoristen versus behavioristen: Enkele aspecten van een tegenstelling binnen de wetenschap der politiek in de Verenigde Staten'. *Acta Politica*, Vol 4, No. 2 (January 1969), pp. 125-138.

scientists, on which much of their argument is based, simply does not exist. If we use the very broad definition of behaviorism provided by Robert A. Dahl ('the behavioral approach is an attempt to improve our understanding of politics by seeking to explain the empirical aspects of political life by means of methods, theories, and criteria of proof that are acceptable to the canons, conventions, and assumptions of modern empirical science'³), there is no doubt that behaviorism has become the major trend. But even in this respect, American political science is by no means monolithic. In the important field of International Relations, for instance, the debate between 'behaviorists' (in the sense of Dahl's definition) and 'traditionalists' has not been concluded.⁴ And in all other fields, too, there is continuing controversy over such issues as the applicability of quantitative methods. There are only too many political scientists, including many leading behaviorists, who share Surkin and Wolfe's opinion that a great deal of research is nothing but 'sophisticated numerology' (p. 54).

On political issues, there is really no consensus at all among American political scientists.⁵ Surkin and Wolfe cite a series of analyses which argue that certain American institutions perform valuable functions (pp. 46-47), but these are not representative examples. It would be quite wrong to say that such favorable evaluations represent a consensus among American political scientists, or even a majority opinion. For instance, some political scientists may favor the electoral college, but my estimate is that the clear majority of their colleagues are opposed to it and would support the constitutional amendment to abolish it. Similarly, if American political science were polled, I think that a majority would turn out to hold negative opinions about malapportionment, the seniority system in Congress, the Central Intelligence Agency, etc. During 1968, very many political scientists supported Senator Eugene McCarthy's bid for the presidency — certainly not

³ Robert A. Dahl, 'The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest', *American Political Science Review*, Vol. 55, No. 4 (December 1961), p. 767. 'Behaviorism' can also refer to the study of political behavior; in that case, it signifies one of the fields of political science rather than a general methodological orientation.

⁴ See, for instance, Klaus Knorr and James N. Rosenau (eds.), *Contending Approaches to International Politics* (Princeton, N. J., Princeton University Press, 1969).

⁵ I am very reluctant to discuss this point, because the quality of a scholar's work in political science should be judged on its own merits and not on its author's political preferences. A man's political science may be wonderful, while at the same time his politics may be terrible; and vice versa, the politics of a bad political scientist may be quite admirable. Therefore, I enter this discussion only because I am forced to do so in order to respond to Surkin and Wolfe's allegations.

an indicator of a conservative political stance in view of the far-reaching changes that McCarthy's 'new politics' would have brought about. Behaviorist Dahl was a member of McCarthy's brain trust, and he also ran (unsuccessfully) as a McCarthy' delegate in the Connecticut primary. Therefore, it is difficult to understand how Surkin and Wolfe can describe American political science as 'a political science which justifies everything in the American political system as unique and workable and condemns any attempt to change it' (p. 46).⁶

Surkin and Wolfe are right, of course, in criticizing the political bias in the work of at least some American political scientists. The goal of scientific objectivity is hard to achieve in the social sciences, and the problem of bias becomes particularly serious and disturbing when secret work is done for government and military agencies. Surkin and Wolfe mention the example of 'the APSA executive director and treasurer (who) were simultaneously connected with the CIA' (p. 45). But what are the facts in this case? These two APSA officers, Evron M. Kirkpatrick and Max M. Kampelman, also happened to be president and vice-president of Operations and Policy Research, Inc., a non-profit research organization financed by grants from government, business, and foundations. Some of OPR' grants were received from foundations which, as it turned out, had received some funds from the CIA. No CIA funds were accepted directly and knowingly by OPR, and all foundation grants were used for completely unclassified research'. To casually call this a 'connection' with the CIA is quite unfair. And, in general, to speak of a 'new alliance between the military-industrial complex and academia' (p. 43) is a wild exaggeration.

Political scientists should maintain constant vigilance against the influence of subjective opinions on their scholarly work. Although complete objective may be difficult or impossible to achieve, it should always be approximated as closely as possible. But this is not accepted as a legitimate goal by Surkin and Wolfe. They are against the supposedly conservative bias of American political science, because it is (in their opinion) conservative, *not* because it is biased. In fact, they are clearly in favor of bias, as long as it is a bias with which they happen to be in agreement. They state, for instance, that

⁶ Moreover, is it fair to condemn the analyses of national conventions by Aaron Wildavsky and of politics in Chicago by Edward Banfield and Martin Meyerson published in 1961 and 1962, on the basis of events that took place in 1968? (See pp. 47-49).

⁷ Moreover, as soon as the incident became known, the APSA immediately conducted a thorough investigation. See 'Report of the Executive Committee', *American Political Science Review*, Vol. 61, No. 2 (June 1967), pp. 565-568.

they are only interested in those kinds of research which promote social change (p. 53). And they reject pluralism on the same political grounds: 'So long as pluralism was conservative, we rejected it. If it were possible for pluralism to support social change... we would accept it' (p. 54).⁸

I want to state emphatically that I disagree with these views expressed by Surkin and Wolfe only on scientific and epistemological grounds, not on political grounds. As a matter of fact, I believe that my own political sympathies do not diverge very much from those of Surkin and Wolfe, although I question their tendency to interpret politics in terms of moral dichotomies. For instance, they express strong disapproval of studies that found advantages in the political system of Chicago under Mayor Daley, the committee system in Congress, and the electoral college (pp. 46-47). Although the Chicago city government certainly has major weaknesses, is it totally evil? Similarly, the system of congressional committees undoubtedly needs reform, but does it really perform no valuable functions at all at the present time? Even the archaic electoral college system may not be entirely without merit: for one thing, it tends to favor the voters in the big cities, and thus gives to Negroes some badly needed political leverage.⁹

Their call for greater relevance in political science raises both similar and different issues. It is understandable that the present grave domestic and foreign problems facing the United States provoke demands for a concerted effort by social scientists to solve these problems and impatience with less directly applicable research. But exclusive emphasis on immediately relevant scientific work would be a mistake. Political science should concern itself both with short-range and with long-range problems. Perhaps political scientists could perform a valuable service by paying greater scholarly attention to the Vietnam war, but in the long run they can probably make a more valuable contri-

⁸ Elsewhere, they state that a 'radical political science' does not mean the process of 'proving' the ideas of the young radicals. Instead, it calls for 'an examination of the truth or falsity' of such hypotheses (p. 58). But if empirical evidence should disprove, the hypotheses, would such evidence have a chance of being accepted, or would it be rejected as necessarily based on the incorrect bias and as 'irrelevant'? Their overriding concern with social change also leads them to accept the following definition of politics: 'any activity aimed toward social change' (p. 59). They regard this as a broad definition, but it is actually a rather narrow one: for instance, is conservatism not to be considered a political phenomenon?

⁹ Total moral disapproval is also implied in their scornful description of the national conventions of 1968 (p. 47). But what is so evil in the fact the delegates were 'chosen under fifty different practices', and that the conventions are institutions 'never recognized by law'?

bution by investigating the general question of the causes of wars and the conditions of peace. Even such an abstract and not directly relevant intellectual activity as model building is not necessarily irrelevant in the long run. Surkin and Wolfe are quite unfair to the model builders, when they state that these models are meant to be 'irrelevant to reality — the more irrelevant the better the model' (p. 51). That may be their judgment of model building (although, incidentally, they do not adduce any evidence to support it), but it is certainly not the aim of the model builders themselves.¹⁰

The demand for relevance contains not only the danger of overemphasizing contemporary problems but also the equally serious danger of encouraging parochialism. If immediate relevance to the contemporary crises in the United States is accepted as the proper criterion for research, American political science should become the study of American politics exclusively. This parochialism can already be discerned, for instance, in the endorsement by Surkin and Wolfe of 'the creation of a research-action political science focusing on criticism of *American* institutions and analyses of alternative social priorities' (p. 60, italics added).

In short, a judicious balance should be maintained between research that is, and research that is not directly relevant. Of course, political scientists can and do legitimately disagree on what is the proper balance. In my own opinion, directly relevant and policy-oriented research has indeed not received sufficient scholarly attention. But I think that it goes much too far to speak of the 'general irrelevance (of political research) to the major social and political problems of the day' (p. 43). In fact, virtually all of the studies which Surkin and Wolfe cite as objectionable works, appear to be highly relevant to contemporary American politics. Studies of Congress by Richard F. Fenno Jr., and Nelson W. Polsby, of national conventions by Aaron B. Wildavsky, of the city government of Chicago by Edward Banfield and Martin Meyerson, and of the Vietnam problem by Samuel P. Huntington (pp. 46-47, 48, 52, 55-56) all seem eminently relevant.¹¹ The explanation of this apparent contradiction is simple, according to Surkin and Wolfe: the term 'relevant' does not mean what it

¹⁰ It is interesting to note that after condemning model building, Surkin and Wolfe turn out to have the ambition of becoming model builders themselves. The 'new political forms' they want to discover and the 'radical alternatives' they want to create (p. 58) are certainly also 'models'.

¹¹ Similarly, in the fields of comparative politics and International Relations (which Surkin and Wolfe hardly mention), the many studies of the Cold War, totalitarian government, and the politics of the developing areas were highly relevant at the time they were written, and some of these (particularly the political development studies) obviously are still completely relevant today.

normally means. Political science is irrelevant, they argue, 'if it is uncritical of society' (p. 55). In other words, political research is only 'relevant' if it is based on or supports the 'correct' political bias.¹² In line with their aim of transforming political science into political advocacy, Surkin and Wolfe support the goal of the Caucus for a New Political Science to politicize the American Political Science Association (p. 60). This issue does not require further comment: all those who favor an objective and impartial political science will oppose this goal. There is one point, however, that does deserve more attention: the CNPS also ostensibly favors democratization of the APSA (p. 60). This is a most worthy aim, and the CNPS must be given credit for providing the impetus behind a series of proposals aiming at democratic reform. At the APSA convention in September 1969, new rules were adopted for the election of the officers of the association and for amending its constitution: in the future, all members of the association will be able to vote by mail ballot for alternative candidates running for the various APSA offices and to vote on proposed constitutional amendments. These changes were adopted *in spite of the vigorous opposition by supporters of the CNPS*. The CNPS clearly preferred the continuation of the old rule, which allowed such important decisions to be made by the small fraction of the total APSA membership who happened to attend the convention — and which could therefore be used by a determined minority to seize control of the APSA. Politicization is the overriding objective of the CNPS, and democratization is at best only a secondary and subordinate goal. In this respect, it is perhaps not insignificant that Surkin and Wolfe describe voting as a 'relatively insignificant event' (p. 53).¹³ Both in the United States and in other countries, there is much in

¹² They add that political science is irrelevant if it 'assumes the values and social priorities of corrupt bureaucracies, powerful elites or unjust social practices instead of using its skills and knowledge to rectify social ills or support alternative social priorities' (p. 55). By using such loaded terms, they make it very hard for anyone to express disagreement. Who can possibly be in favor of 'corrupt bureaucracies' or 'unjust social practices' and against efforts to rectify 'social ills'? But, of course, the dichotomy they propose here is a false one, because it rules out research on important contemporary problems which tries to be as objective and unbiased as possible. A similar dubious dichotomy is proposed when relevance is defined as meaning 'scholarly activity in the service of truth and humanity instead of in the service of power and social status' (p. 45). Are those who wield power necessarily enemies of humanity?

¹³ Their tolerance of opposing views is also disturbingly low. They express their disapproval of the fact that the APSA accorded the same open and liberal treatment to an anti-CNPS group as was given to the CNPS itself (p. 57). If the CNPS faction should succeed in gaining control of the APSA, such even-handed and fair treatment can probably not be expected any longer.

political science that deserves criticism. But let us try to overcome the weak aspects of our discipline and our profession by strengthening the scientific study of politics, not by abolishing political science and substituting political *advocacy*.

RESPONSE TO PROFESSOR LIJPHART

by Marvin Surkin and Alan Wolfe

In his critique of our essay, 'The Political Dimension of American Political Science,' Professor Lijphart notes that he is 'reluctant' to discuss the political ideals and opinions of political scientists, but that he is 'forced' to because we brought it up. That one statement, more than any other in his critique, is indicative of the difference in the traditions out of which we write. Professor Lijphart is defending one tradition, the one of which we are so critical. To him that tradition is to be preferred (values do creep into all of our work) while the other one involves 'distinct threat to the development of political science'. In a sense he is correct, for it was exactly our intention to challenge a certain type of political science. One must distinguish between the science of politics — to which we are all hopefully committed — and particular approaches to political science about which we may differ.

Professor Lijphart's defense of one particular approach to science must be seen as a defense of 'his' approach. The sense of urgency surrounding his critique (defense) may be due to the increasing feeling that his approach to the study of politics has outlived its usefulness. Thomas Kuhn, in *The Structure of Scientific Revolutions*, has described what usually happens in such situations:

— Because he (the student for membership in the particular scientific community) there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals. Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e., for the genesis and continuation of a particular research tradition. — In our paper we have elaborated and criticized the 'normal' science of American political science. We have done so precisely because of our claim to be a value-free, neutral science while manifesting conservative political and ideological biases and, more importantly, using 'science' as its mode of political advocacy. Professor Lijphart persists in

¹ *Acta Politica*, 5 (1969/70); pp. 43–61.

calling American political science 'science' and defending it. We call it political advocacy and ideology of a kind we largely cannot defend, intellectually because it is dishonest to science and politically because it tends to support and reinforce attitudes, policies and governments we cannot embrace. Therefore, when Professor Lijphart writes that we are replacing political science with political advocacy he is guilty of all of the shortcomings of his paradigm, especially the inability to contemplate alternative research, theory or political practice. Therein lies the political shadow of his own biases.

The best example of the shortcomings of Professor Lijphart's approach to science is his attempt to understand, presumably as a political scientist, certain events at the 1969 meetings of the American Political Science Association. He states that reforms of the Association, designed to make it more democratic, were opposed by the supporters of the Caucus for a New Political Science. In his analysis of these events, he has been trapped by his own ideology. The events of a particular political period are taken at face value, with no attempt to uncover either their symbolic significance or the motivations of the actors involved. Had either of these things been considered, any observer would have discovered that the so-called reform proposals, those criticized by the Caucus, were, in reality, simply moves on the part of certain members of the Association to perpetuate an undemocratic situation, in a way which David B. Truman referring to the referendum has fully described in *The Governmental Process*. To take a reform proposal as simply a reform proposal without considering its ability to be used as a simple political ploy is to practice poor science. Hence we come back to our main point. A truly scientific study of politics must go beyond the collection of data about political events to account for the real interests at work behind any political proposal. Without taking such interests into account, the political scientist has indicated that he will be satisfied with incomplete understanding. Many of the American political scientists we have criticized seem to desire such result. In his critique, Professor Lijphart obviously wants to join them.

ONDERZOEKSPROBLEMEN

VERSCHILLEN IN STEMGEDRAG TUSSEN TWEDE KAMER-FRACTIES TIJDENS HET KABINET CALS

door M. van Tijn—Koekebakker, W. Brinkman, W. Koomen

1 INLEIDING

Tussen politieke partijen bestaan verschillen. Een voor de politologen relevante vraag is die naar de dimensies waarop politieke partijen van elkaar verschillen. Deze dimensies plus de posities van politieke partijen in de door die dimensies beschreven ruimte vormen de structuur van het partijenstelsel.

Eén van de manieren om bedoelde structuur vast te stellen is het toepassen van multidimensionale schaal-methoden op een $n \times n$ afstandsmatrix (D-matrix). De cellen van deze matrix bevatten een kwantitatieve aanduiding van de afstand tussen elk paar politieke partijen (in totaal n partijen).

Voor het begrip „afstand tussen twee politieke partijen” zijn talloze operationalisaties denkbaar. Zij zijn in twee brede categorieën onder te brengen. In de eerste plaats kan men een aantal operationalisaties baseren op z.g. *beoordelings-data* (men kan b.v. individuen uit al dan niet a-selecte steekproeven vragen het volgens hen bestaande verschil tussen elk mogelijk tweetal partijen aan te geven op een 10-puntschaal — hoe groter het subjectief gepercipieerde verschil, hoe groter de score — en het gemiddelde van de voor een specifiek paar aangegeven verschillen als indicatie voor de afstand nemen).

In de tweede plaats kan men indicaties voor de afstand tussen twee politieke partijen ontleenen aan *gedrags-data*, d.w.z. aan observaties van concrete gedragingen van (leden van) politieke partijen.

Wij kozen het stemgedrag van politieke partijen in de Tweede Kamer als uitgangspunt voor een operationalisatie van het begrip „afstand tussen partijen”, en wel omdat (a) dit gedrag relevant en belangrijk is, en (b) omdat waarnemingen van dit gedrag gewoonlijk betrouwbaar geregistreerd worden (n.l. in de Handelingen).

Paragraaf 2 van dit artikel bevat een beschrijving van de procedure die leidde tot een D-matrix en van de daarop toegepaste analyse-methode.

Paragraaf 3 geeft een korte weergave van de resultaten, terwijl paragraaf 4 enkele commentaren en suggesties voor verder onderzoek bevat.