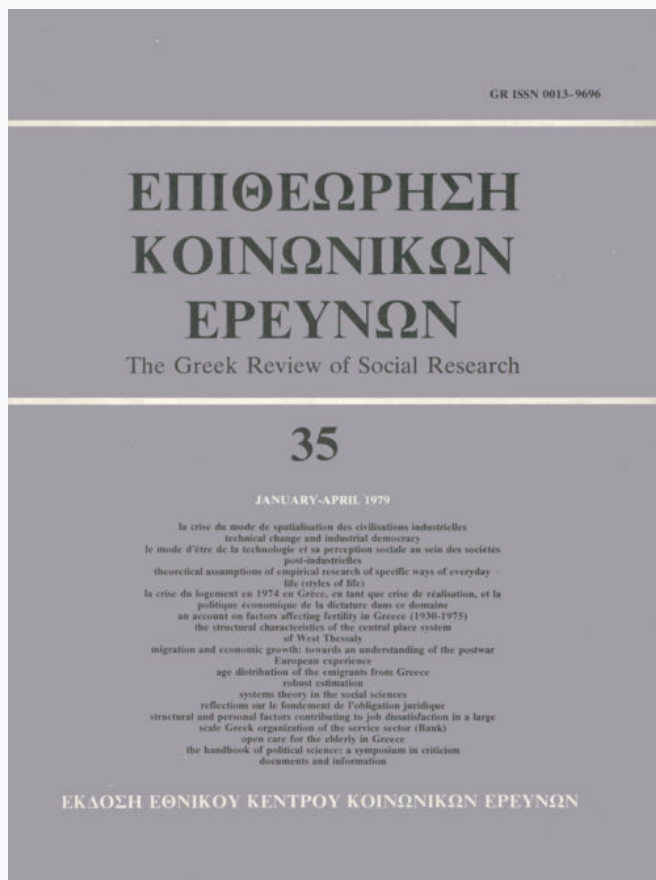


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The handbook of political science: A Symposium in Criticism

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the handbook of political science:

A Symposium in Criticism

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Edited by Paschalis M. Kitromilides

The publication of a Handbook of Political Science might be taken to represent a claim on the part of its editors and their collaborators who have co-authored the eight volumes of the project, that there can be a canon of wisdom in the discipline defined as political science and a perspective of orthodoxy on the content of its various branches into which the Handbook could be an initiation. In fact, such a claim is not made. One has a feeling that there is an implicit assumption motivating the compilation of the Handbook, an assumption informed by a positivist perception of cumulative growth in empirical science that has finally been achieved in the study of politics and has, therefore, rendered political science mature and capable of codification and standardization. Since this objectionable thesis is not explicitly argued one cannot engage in shadow boxing with it, but the attempts at codification and standardization contingent on the very idea of a Handbook, certainly invite comment and assessment. What the Handbook basically consists of is a series of extensive and for the most part quite competent essays which survey the central issues and discuss the basic literature in different subfields and areas of research conventionally included in political science. As such the several volumes of the Handbook are primarily relevant to the concerns of those who want to explore the territory of political science. The Handbook maps this territory out quite well. There may be disagreements over the classification and choice of subjects, the arrangement of the several volumes, the assignments to individual contributors. Overall, however, the project as a whole is an open-ended survey of intellectual and methodological developments and trends in political science that gives a fairly good idea of the condition, achievements and problems of the discipline. This is a considerable accomplishment on the part of the editors, two seasoned political scientists widely respected in the profession for their competence and important contributions in their particular fields of research.

The Handbook has already been critically assessed in a series of reviews by professional colleagues of authoritative judgement in their fields of expertise.* Besides this judgement, however, there is need for another assessment on the part of those who may be considered as the Handbook's most natural audience—*younger political scientists who are newcomers to the field.* It is to them primarily that the Handbook purports to convey a sense of what political science is all about. In the present collective critique, therefore, four young political scientists with the experiences of graduate school still quite fresh in their

* See the reviews of the eight volumes in *The American Political Science Review*, Vol. LXXI, No. 4 (December, 1977), pp. 1621-36.

minds, offer their own assessment of the relevance, usefulness and pitfalls of the Handbook. Each of us represents one of the broadly defined areas of political science, as practised in the United States—political theory, American government, comparative politics and international relations—and offers his own judgement as to how the pertinent volume of the Handbook contributed to his initiation in political science. This critical symposium, therefore, is presented here as still another contribution to the dialogue invited by this attempt at a self-definition and self-discovery in contemporary political science.

Handbook of Political Science, Volume I: Political Science: Scope and Theory, ed. by Fred I. Greenstein and Nelson W. Polsby, Reading, Mass., Addison-Wesley, 1975. 414 pp.

by Paschalis M. Kitromilides
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The first volume of the *Handbook of Political Science* embodies an attempt to define the nature and scope of political discourse. This arduous task is discharged by six authors in five essays which in their different perspectives, modes of argument and foci of analysis—as well as their qualitative unevenness—bring out quite graphically the multi-dimensional character of the systematic study of politics. In the opening essay Dwight Waldo discusses political science as a discipline and as a profession. This essay basically consists of an historical profile of the development of political science in the United States since the nineteenth century and of a survey of the state of the profession in the mid-seventies. Although the initial effort of the essay to relate contemporary political science to the great tradition of political thought, classical and modern, remains rather uninteresting in its crudeness, the main body of the text itself is a useful informative piece. The political scientist who is interested in the roots of the discipline will find in it many details concerning the factors that have determined the historical evolution, over several decades, of what is today the mainstream version of American political science which moreover, as an academic export, has increasingly modified and is becoming to a significant degree the predominant mode of political research in the rest of the world as well.

J. Donald Moon contributes a highly successful and readable essay on the logic of political inquiry. In it he offers a lucid, effective and well informed reconstruction of the two alternative modes of political

inquiry around which has centered a far-ranging methodological debate in political science. The essay presents an outline of the basic tenets, assumptions and claims of the scientific ideal that seeks to explain political phenomena in terms of general laws and causal theories on the model of the natural sciences and alternatively of the interpretative or hermeneutic approach that tries to explain political life by reference to human purposes and contextual meanings. By elucidating the nature of each methodological approach and by subjecting both to systematic criticism, Moon manages to point out quite persuasively that their mutually exclusive claims are fundamentally untenable and that in fact the two approaches complement each other.¹ On the basis of this demonstration he proposes a synthesis of the two contending approaches premised on a conceptualization of human nature and human activity—a model of man. Moon pursues this ambitious project with impressive conceptual rigor and clarity and a thorough command of the voluminous and difficult literature on the methodology of the human sciences.

Although my own work in the history of political and social ideas moves practically in its entirety in the sphere of hermeneutic interpretation, I find Moon's intellectual position and arguments theoretically convincing and thoroughly fruitful for the purpose of transcending the lamentable pitfalls of crude scientism and naive empiricism which plague most of standard political science on the one hand, as well as the theoretical agnosticism and idiosyncratic argumentation of much of the more «traditionally» minded work in political analysis and political history. There is only one suggestion that I might offer to Moon in connection with his effort to liberate political inquiry from the fetters of exclusive methodological paradigms. I would urge a greater degree of attention to the theoretical discussions on the nature and method of the human sciences (Foucault, Gusdorf) as well as to the interdisciplinary approaches to historical and social research (*Annales* school) developed in France, as practical guides and as sources of theoretical inspiration that might enrich the new synthesis that is proposed to American political science.

The essays by Felix Oppenheim and Brian Barry-Douglas Rae on the language of political inquiry and political evaluation respectively, constitute well reasoned and successfully argued explorations of central dimensions of political analysis and at the same time represent very good examples of analytical political theory in themselves. Felix Oppenheim dis-

1. A similar position has been argued a few years ago by Samuel H. Beer, «Political Science and History» in *Essays in Theory and History*, ed. by Melvin Richter, Cambridge, Mass., Harvard University Press, 1970, pp. 41-73.

usses the exigencies of linguistic and terminological usages in political science, identifies the current distinctions and conceptual problems involved in the effort at a careful and systematic definition of the terms that compose the vocabulary of politics and concludes by pointing at the logical difficulties faced by the positivist position on the fact-value distinction. This is a remarkable conclusion to emerge from this rigorous logical discussion of the language of political inquiry which is completely aware and employs to full advantage the entire conceptual and analytical apparatus of behavioral political theory.

This theme is amplified in the essay on political evaluation by Rae and Barry that appropriately concludes the volume. The authors point to the centrality of evaluation and normative discourse for the pursuits of political inquiry and discuss the logical requirements of evaluative arguments. After positing, quite plausibly I think, a conception of human well-being as the ultimate basis of political evaluation they discuss six evaluative criteria and the problems contingent on the application of each of them. Finally, the essay briefly discusses five political principles—the public interest, justice, equality, freedom, democracy—which are central to normative political discourse and are often used as standards of political evaluation. The seriousness with which these issues are explored represents a real and encouraging contribution of this volume. It is clear that contemporary political science has passed beyond the crude positivist militancy of its behavioral phase and has found both the courage and the intellectual maturity to direct its conceptual rigor to the discussion of really significant problems beyond empirical trivialities.

The most problematic aspect of the volume has to do with Dante Germino's essay on the contemporary relevance of the classics of political philosophy. This essay might be of some use to those completely uninitiated in political philosophy who are in need of a very elementary sense of the issues and some basic bibliographic information. Beyond this introductory orientation however—and some political theorists may question even this minimal recognition of its relevance—this essay can be of very little use. The student of political philosophy and its history could conceivably voice many objections and reservations concerning the presentation of the content and task of political philosophy in this essay. And the uninitiated, let's say the convinced empiricist political scientist who looks at the *Handbook* to broaden his awareness of the tasks of other branches of his discipline, might leave this essay with a—justifiable—sense of vindication of his or her contempt for that indeterminate pursuit known as political theory.

The shortcomings of the essay on the classics of political philosophy appear in a way accentuated in

light of the generally good quality of the three analytical essays which discuss the nature of political discourse. So in a rather peculiar way the first volume of the *Handbook of Political Science* illuminates quite adequately and to a large extent with considerable originality the several metatheoretical issues in political inquiry—the issues that refer to the conceptual and methodological character of the concerns of the student of politics—but treats the central and enduring questions—the questions that are fundamentally raised in the classics of political philosophy—only tangentially. This may be to some extent indicative of the overall intellectual climate prevailing in political science and it certainly reflects the strengths and weaknesses of the discipline as a whole. But to ignore or to treat political philosophy and its history superficially does not really reflect the true state of things in contemporary political science. An earlier fear that political philosophy as an unfolding tradition of reflection and commentary on the fundamental problems of public life, might be dead, has proved untenable and recognized as such by those who first voiced it.² The temporary retreat that followed the crisis of conscience precipitated by the age of the World Wars and the silence contingent on the self-examination invited by the combined pressures of the advent of linguistic philosophy, logical positivism and behavioral militancy that constituted the intellectual vogue of the post-war era, have given way to a revival of political theory. This revival has come in the form of a renewed interest in the classics of political thought and in the history of political philosophy as well as in the shape of attempts at original reflection on the perennial problems of the public realm that the classics have kept alive.³ This reawakened concern with political philosophy has obviously been the combined outcome of the problems created by the tensions of social change experienced in the 1960s in the industrial societies of the United States and Europe as well as in the rest of the world with the end of colonialism, and of the intellectual impasse in which militant and normatively agnostic behaviorism had left the social sciences.⁴ The revival

2. See *Philosophy, Politics and Society*, ed. by Peter Laslett, Oxford, Blackwell, 1956, p. vii and generally the editor's introduction, pp. vii-xiv. The subsequent three volumes brought out under the same title, identified as second, third and fourth series and edited by Laslett in cooperation with W.G. Runciman and Quentin Skinner (1962, 1967, 1972) have contributed significantly to the revival of contemporary political theory.

3. A major example of the latter kind of political and moral theory is John Rawls, *A Theory of Justice*, Cambridge, Mass., Harvard University Press, 1971, and more recently Michael Walzer, *Just and Unjust Wars*, New York, Basic Books, 1977.

4. For a view of the response of political theory to these challenges, cf. Judith N. Shklar, «Facing up to Intellectual Pluralism» in David Spitz, ed., *Political Theory and Social Change*, New York, Atherton Press, 1967, pp. 275-95.

of political theory is probably the most remarkable intellectual development in political science in the last two decades and an assessment of its significance should have been a major concern of this volume of the *Handbook*. In addition special attention should have been devoted to what has been described as the restructuring of social and political theory under the impact of developments in continental European philosophy, particularly phenomenology and critical theory.⁵ The discovery of these alternative perspectives by Anglo-Saxon social science is likely to have a major impact on the future direction of the revival of political theory.

An assessment of the state of political theory, therefore, did not necessarily call for a disquisition concerning the relevance of the classics to all cultured persons concerned with the important questions of public life. What might have been much more pertinent would have been a discussion of the methodological problems that have punctuated the recovery of interest in the classics and the questions they raise. Beyond elucidating its wider political and intellectual background and constraints, it might have been quite useful to identify the stakes in the methodological debates going on within political theory today and their political underpinnings—as for instance the broad disagreement between those who believe that political thought should be studied historically and those who see it as a system of esoteric knowledge whose understanding is accessible to only a limited number of initiates.⁶ A discussion of contending approaches and their respective merits could have pointed at the possibilities of a rapprochement—precisely as it is done elsewhere in the volume in connection with other branches of political science. A consideration of how political philosophy and its history is, could, and should be studied⁷ and an assessment of the merits of contending approaches and alternative perspectives, would have performed for political theorists the same service that this volume of the *Handbook* renders to a considerable extent successfully to political scientists generally: increase their intellectual self-awareness and make them conscious of at least some of the central issues, stakes, and alternatives in the several modes of political inquiry.

5. Cf. Richard J. Bernstein, *The Restructuring of Social and Political Theory*, Philadelphia, University of Pennsylvania Press, 1978.

6. The intensity of this debate is characteristically brought out in the exchange on the interpretation of Machiavelli between Harvey C. Mansfield, Jr. and J.G.A. Pocock, in *Political Theory: An International Journal of Political Philosophy*, Vol. 3., No. 4 (November, 1975), pp. 372-405.

7. On these questions cf. the perceptive postscript in Judith N. Shklar, *Men and Citizens: A Study of Rousseau's Social Theory*, Cambridge, University Press, 1969, pp. 215-231.

Handbook of Political Science, Volume 3: *Macropolitical Theory*, ed. by Fred I. Greenstein and Nelson W. Polsby, Reading, Mass., Addison-Wesley, 1975. 648 pp.

by Maurizio Vannicelli
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The «travails of democracy» is perhaps the unifying theme of volume 3 of the *Handbook of Political Science*.¹ Different in their methodological approach, scope, and focus of analysis, the six essays comprising this volume do a brilliant, though uneven, job at illustrating the obstacles and pitfalls that a nation encounters on the road to modernity.² The very act of nation-building, marking the transition from a feudal to a modern form of state organization, ignites a process of adaptation and change which, more often than not, has the effect of diverting the developmental trajectory of a nation from its intended goal of creating the political, economic, and social institutions typical of the modern polity. In virtually no country has the onset of modernity brought about a clean break with the pre-modern past.³ Almost unmanageable in their resilience, the elements of unmodernity always permeate, shape, and condition the search for modernity.⁴ Rather than escaping the bondage of unmodernity, the task of nation-builders should, therefore, imply coming to terms with its existence in order to overcome or limit its ramifications into the modern world and to build a political system which is both stable and modern.⁵

1. Samuel P. Huntington and Jorge I. Dominguez, «Political Development», pp. 1-114; Robert A. Dahl, «Governments and Political Oppositions», pp. 115-174; Juan J. Linz, «Totalitarian and Authoritarian Regimes», pp. 175-412; Michael Taylor, «The Theory of Collective Choice», pp. 413-482; Charles Tilly, «Revolutions and Collective Violence», pp. 483-556; Arthur L. Stinchcombe, «Social Structure and Politics», pp. 557-622.

2. For a brilliant discussion of the impact that the transformation of societies has on the individual, see T.H. Marshall, *Citizenship and Social Class*, (Cambridge, England: Cambridge University Press, 1950); also Ralf Dahrendorf, *Society and Democracy in Germany*, (New York: Anchor Books, 1969).

3. See Samuel H. Beer, «Modern Political Development», in Sam Beer et al., *Patterns of Government. The Major Political Systems of Europe*, (New York: Random House, 1973; first edition published in 1958), pp. 1-120; David E. Apter, *The Politics of Modernization* (Chicago: University of Chicago Press, 1965); S.N. Eisenstadt, *Modernization: Protest and Change*, (Englewood Cliffs, N.J.: Prentice-Hall, 1966); Daniel Lerner, *The Passing of Traditional Society* (New York: Free Press, 1958); Robert L. Sinai, *The Challenge of Modernization* (New York: Norton, 1964).

4. A classical description of the features of feudal societies is presented by Marc Bloch, *Feudal Society*, 2 vols. (London: Routledge and Kegan Paul, Lt., 1961).

5. See Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, Conn.: Yale University Press, 1968); Edward Shils, *Political Development in the New States* (The Hague:

Despite some methodological flaws, such as the proclivity to stress processes and outputs at the expense of inputs, the authors of the various essays show how arduous it is for a political system to achieve a condition of structural stability, while allowing mass participation in the political process, and to reconcile the requisites of political order with the yearning for social justice and economic equality characteristic of the modern man. Under the combined pressure of modernization, industrialization, and the opening up of politics to the masses, stability is often achieved through resort to authoritarian or, at least, un-democratic measures and solutions, the qualitative essence of the polity is subordinated to the pursuit of quantitative levels of economic growth, and social disharmony tends to become the leitmotif of the political system.⁶

While the purpose of the essays of this volume is to analyze differences among political systems; both in their present complexion and in an evolutionary sense and, in some instances, to account for such differences, a conceptual defect transpires throughout the *Handbook*, one of which the foreign reader in particular should be aware. Common to most of the authors is the tendency to view the democratic model of political life and institutional organization as the ideal to be used in order to appraise other political systems, as the frame of reference to which the historical experience of other countries should be reduced, and, by implication, as the goal towards which all nations should strive, irrespective of their historical peculiarities, structural conditions, and contextual constraints. Some of the analytical models presented in this volume are, quite obviously, filtered through the prisms of a latent «Americanism», meaning a tacit belief in the success and virtue of the American experience with democracy. Hence, a basic rule of the comparative approach to politics is violated: that political systems should be analyzed on their own merits, with the purpose of understanding the reasons that make them different rather than judging them on the basis of their proximity to the ideal of bourgeois democracy. The massive amount of cross-national research on which most essays are constructed makes up only in part for the damage that this tendency to impose ready-made blueprints on

other countries inflicts on the overall quality of this volume of the *Handbook*.

I. The Three Dimensions of Comparative Analysis

Once the decision is made as to why to compare, the crucial question facing the comparativist is what to compare.⁷ Two different, though not mutually exclusive, strategies present themselves. On the one hand, the thrust of the analysis can be directed towards the system as a whole, by taking the system as it is and comparing it with the system of another country. The opposite approach is to isolate particular aspects or institutions or processes of a system from the whole, and to compare them with their counterparts in another political system. (A third, and by far more challenging, way of comparing political systems is what might be defined as the «good-life-approach»; and here the emphasis should be on the quality of the political and social life unfolding within a system, as well as on its political beliefs and cultural-political values.)⁸ While both the systemic and the particularistic approaches recommend themselves for their capacity to lend precision and schematic forcefulness to the comparative endeavor, they suffer from the same weakness. In fact, they can indeed describe differences, enumerate them by devising long laundry lists of the factors on which political systems converge and diverge, but the explanations they offer for such differences are speculative at best.

What is needed, therefore, is to focus on the third question which a comparativist should ask, How To Compare?⁹ This is a question which has to be addressed by keeping in mind a basic truth which, in spite or because of its almost tautological correctness, is often neglected. That is to say, systems are neither static, and this applies both to their developmental and structural dimensions, nor uniform in the language, manifestations, and quality of their political life.

7. For attempts to systematize the comparative approach to politics, see David Easton, «An Approach to the Analysis of Political Systems», *World Politics*, vol. 9, no. 3 (April 1957), pp. 383-400; Dunkwart Rustow, «New Horizons in Comparative Politics», *World Politics* (July 1957). For a brief historical sketch of comparative politics, see Harry Eckstein's essay in David E. Apter and Harry Eckstein, eds., *Comparative Politics. A Reader* (New York: Free Press, 1963), pp. 3-42.

8. An excellent book dealing with all factors influencing political systems is Peter H. Merkl, *Modern Comparative Politics* (New York: Holt, Rinehart, and Winston, Inc., 1970).

9. Among the most noteworthy books on comparative politics the reader should consult Roy C. Macridis and Bernard E. Brown, *Comparative Politics* (Homewood, Ill.: The Dorsey Press, Inc., 1961); Roy C. Macridis, *The Study of Comparative Government* (New York: Doubleday, 1955); Richard I. Merritt and Stein Rokkan, eds., *Comparing Nations* (New Haven, Conn.: Yale University Press, 1966).

Mouton, 1962); Aristide Zollberg, *Creating Political Order* (Chicago: Rand McNally, 1966). Also two volumes from the series *Studies in Political Development*: Joseph LaPalombara and Myron Weiner, eds., *Political Parties and Political Development*, vol. 6 (Princeton: Princeton University Press, 1966), and *Crisis and Sequences in Political Development*, vol. 7 (1965), containing essays by Binder, Coleman, LaPalombara, Pye, Verba, and Weiner.

6. See Seymour M. Lipset, «Some Social Requisites of Democracy: Economic Development and Political Legitimacy», *The American Political Science Review*, vol. LIII, no. 1 (March 1959).

Change and diversity are the two elements which, intertwined in a mutually reinforcing cause-effect relationship, give an almost elusive quality to political systems, one which makes the task of the comparativist particularly laborious.

Above all, the goal of the student of comparative politics should be to offer explanations, and not just descriptions, about the differences which exist among political systems, and about the reasons that induce or force political systems to choose different paths and regimes in their evolutionary development.¹⁰ It is then imperative that a methodological approach be selected which takes into account three dimensions which, while interacting with one another, influence the system, both in its entirety and in its particular aspects: the vertical, the horizontal, and the inner dimension. The stages of the historical development of a political system should be dealt with by the vertical dimension of the comparative framework, with particular attention to the time factor; more precisely, to the point in time when a country began to break away from its pre-modern past. Of equal importance should be the issues concerning the initial stimulus (historical, institutional, or economic) which started the process of modernization, and the modality (change from above, from within, and from below) of such a process.¹¹ The purpose of the vertical dimension is simple enough: to collocate a country's break with the past and beginning of its modernization process inside a definite historical stage, which should allow the creation of a developmental continuum on which countries can be compared. A different continuum, one characterizing political systems on the basis of the nature of their political institutions, economic organizations, and quality of political life, should be at the heart of the horizontal dimension of the comparative framework. Finally, the inner dimension should devote itself to the grammar of the political life of a system (What is the degree of competition allowed and sustainable by the system?), the stakes (What do political actors fight about?), the means (What political, economic, and institutional means do political actors use in the pursuit of their aims), and the processes (How are decisions made? How are policies formulated and implemented?).

It is at the intersection point of these three dimensions that the key to the explanatory appraisal of the differences among political systems lies. And it is by focusing the comparative framework on this intersec-

tion that the twin fallacies of statism and uniformity can be avoided, with the result that crossnational comparisons will deal simultaneously with the inputs, processes, and outputs of different political systems, explaining rather than describing the causes and expressions of such differences.

II. *The Vertical Dimension: The Sisyphian Tasks of Political Development*

A dual aim informs the essay by Samuel P. Huntington and Jorge I. Domínguez on political development: to throw some clarity on a field which has been characterized so far by a multiplication of theories and by few attempts at creating coherent, all-encompassing paradigms, and to determine the exact link between political democracy, stability, and socioeconomic modernization.¹² The first aim is certainly accomplished by the authors. Through a detailed review of the enormous literature on the subject, they develop an analytical paradigm consisting of five categories (phase, factors, environment, timing, sequences), which represents a substantial step forward in the schematization of the study of political development. They show, for instance, how qualitative or quantitative increases in the level of mass mobilization and political participation do not automatically and necessarily lead to increased integration and stability of the polity. Among other things, political participation may in fact reinforce class distinctions, while mobilization can sharpen pre-existing political or religious cleavages. Hence, the process of political development is often caught between conflicting pulls which are not always easily reconcilable. The spiral of the process of development may pull the system towards certain institutions which stand in dialectical opposition to the system's forms of political life and to its political culture. As the essay makes clear, the outcome of a process of political development will be successful if it proceeds slowly and by degree, keeping the rates of institutional, societal,

12. Among the major works by Huntington on macropolitical theory, see «Political Development and Political Decays», *World Politics* (1965), 17: 386-430; the above mentioned *Political Order in Changing Societies* (1968); «The Change To Change: Modernization, Development, and Politics», *Comparative Politics* (1971), 3:283-322; (with Clement H. Moore, eds.), *Authoritarian Politics in Modern Society* (New York: Basic Books, 1970); (with Joan M. Nelson), *Socio-economic Change and Political Participation* (Cambridge, Mass.: Harvard University Press, 1976).

For other works by Jorge I. Domínguez, see *Social Mobilization, Traditional Political Participation and Government Response in Early Nineteenth Century Spanish America*, Unpublished Dissertation, Harvard University, Cambridge, Mass. (1972); «Revolutionary Values and Development Performance: China, Cuba, and the Soviet Union», in Harold Lasswell and John Montgomery, eds., *Values in Development*, (Cambridge, Mass.: MIT Press, 1975); *Cuba: Order and Revolution* (Cambridge, Mass.: Harvard University Press, 1979).

10. See Gabriel A. Almond and Bingham G. Powell, Jr., *Comparative Politics: A Developmental Approach* (Boston: Little, Brown, 1966).

11. A masterful book discussing the three forms of transformation is Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy* (Boston: Beacon Press, 1966).

and economic growth in a condition of constant equilibrium with the political culture on which the systems rest.

But, and this is probably the major tension in the essay by Huntington and Dominguez, what their analysis of the interaction between the two variables (socioeconomic modernization and outcomes) overlooks, or at least downplays, is that such an interaction takes place in a condition of mutuality; that is, certain political institutions—to offer a clear example—are not only shaped by the process of socioeconomic modernization, but they also shape it by conditioning its pace and direction. The problem is fundamentally methodological, one which reflects the authors' focus on the impact that socioeconomic modernization has on political culture, political participation, political institutions, and political integration, and their downplaying the impact that the latter can and does have on the former.¹³ The outcome of the process of development, that is, the kind of polity which is created, depends on the mutually reinforcing interaction between modernization and the institutional, social, and political aspects of the system.

III. *The Horizontal Dimension: The Failures of Democracy*

Typical to most interpretations of totalitarian regimes is the tendency to think of them, and to define them, in terms of a pre-conceived notion: totalitarianism is the negation of democracy, and its very existence implies a total rejection of the democratic ideal and of democratic forms of rule and political life. This notion, which was prevalent in the United States in the heyday of the Cold War, ignores a fundamental lesson which should be drawn from Talmon's often forgotten book, *The Origins of Totalitarian Democracy*: that democracy contains two streams, liberal democracy and a totalitarian type of

democracy, which date back to the 18th century.¹⁴ Hence, rather than as a rejection of democracy, the emergence of totalitarianism, in all its forms and vestiges, should be regarded as a failure of the democratic experiment.

Coupled with the realization that there exists neither pure democracy nor pure totalitarianism, the acceptance of the duality of democracy is the point of departure of Juan J. Linz's pathbreaking essay.¹⁵ By introducing into his analysis the notion that systems differ in terms of degree rather than of essence, Linz develops a typology of non-democratic regimes which allows the reader to draw a series of important conclusions: that not all non-democratic regimes are similar, and it is, therefore, incorrect to lump them together under the misused label of «totalitarianism»; that all non-democratic regimes, irrespective of their degree of totalitarianism, are degenerations of the democratic ideal, not mere rejections of it; and that the term totalitarian covers too much, which calls for the creation of a continuum on which totalitarian regimes can be collocated according to the degree of terror they use, the amount of mass participation they allow, and the extent of centralization of power they display.

The major contribution that Linz's essay makes to the understanding of the heterogeneous quality of totalitarianism is, however, the distinction between totalitarian regimes and regimes which have been often labeled, both in the popular and even in the scholarly literature, as totalitarian—regimes which he calls bureaucratic—military authoritarian. For Linz, the fact that some authoritarian regimes adopt the symbolism, themes, and verbiage of fascism does not make them totalitarian. Limited pluralism, controlled participation, tendency towards political apathy of most citizens and toleration or encouragement of such apathy are what characterize this non-totalitarian forms of regime, of which Spain under Franco represents a prime example. In practical terms, this means that some liberal institutions have been introduced but no true party system; the party does not play the same essential «integrative» and «mobilizing» functions of the totalitarian party; qual-

13. Two major works on political culture are Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Princeton: N.J.: Princeton University Press, 1963), and Lucian W. Pye and Sidney Verba, eds., *Political Culture and Political Development* (Princeton, N.Y.: Princeton University Press, 1965). An excellent, though somewhat dated, book on political participation is Lester W. Milbrath, *Political Participation* (Skokie, Ill.: Rand McNally, 1965). For works on institutions, see S.N. Eisenstadt, *Essays on Comparative Institutions* (New York: Wiley, 1965); Robert C. Fried, *Comparative Political Institutions* (New York: Crowell-Collier-MacMillan, 1966); Arnold J. Zurcher, ed., *Constitutions and Constitutional Trends Since World War II*, 2nd ed. (New York: New York University Press, 1955). The issue of political integration is masterfully discussed by Karl W. Deutsch, *Nationalism and Social Communication* (Cambridge, Mass.: MIT Press, 1953); see also Claude Ake, *A Theory of Political Integration* (Homewood, Ill.: The Dorsey Press, 1967).

14. Hannah Arendt, *The Origins of Totalitarianism* (New York: Harcourt, 1966); Carl J. Friedrich and Zbigniew Brzezinski, *Totalitarian Dictatorship and Autocracy* (New York: Praeger, 1965).

15. For other works by Linz, see «An Authoritarian Regime: The Case of Spain», in Erik Allard and Yrjö Littunen, eds., *Cleavages, Ideologies, and Party Systems* (Helsinki: Westermack Society, 1964); «Opposition in and under Authoritarian Regimes: The Case of Spain», in Robert A. Dahl, ed., *Regimes and Oppositions* (New Haven, Conn.: Yale University Press, 1973); «Some Notes towards a Comparative Study of Fascism in Sociological Historical Perspective», in W. Laqueur, ed., *A Guide to Fascism* (Berkeley: University of California Press, 1976).

itative participation on the part of certain segments of the citizenry is preferred to that mass mobilization which is the *raison d'être* of totalitarian regimes; a premium is placed on popular apathy, in contrast to totalitarian regimes which require of the people enormous levels of activism; some pre-existing institutions, such as the Church and its corollary organizations, are not swept away or perceived as antagonistic to the regime but co-opted and often used as training-ground for future elites; the significance of ideology, both as a means for legitimizing the regime and as a catalyst for mobilizing the masses, is substantially inferior to the one detectable in totalitarian regimes; and, finally, while the political life of the regime is marked by high levels of terror, there is not the same institutionalization of terror typical of totalitarian regimes.

While Linz's essay provides useful definitions for appraising non-democratic regimes on the basis of a differentiated typology, his own definition of totalitarianism is somewhat too restricted. For Linz, a definition of totalitarian systems should consist of three elements: the importance of ideology, the existence of a single mass party and other mobilizational organizations, and the presence of concentrated power in an individual or group that cannot be displaced from power by peaceful means. The problem is that, while he carefully specifies the functions of totalitarian ideology, he is strangely silent about what such ideology is supposed to consist of. Because of this, Linz's definition of totalitarianism suffers from the same tension of «Cold-War-interpretations» in that it does not make a convincing, and necessary, distinction between the Bolshevik brand of totalitarianism and the totalitarianism of National Socialism and Fascism. In fact, even the differences that Linz sees between Nazism and Fascism are attributed to different levels of terror and to the unique role of the fascist party, which in Italy was subordinated—because of Mussolini's ethical or Gentilean conception of the state—to the existing state institutions, and not to different ideological «essences».

Therefore, what is needed is a distinction between three dimensions of ideology: ideology as a propellant for seizing power, as a source of legitimation, and as a vision of what the «ideal society» should look like. On the first two criteria obvious similarities can be ascertained between Fascism/Nazism and Bolshevism; but, and this is a crucial difference, the vision of society entertained by the Bolsheviks differed substantially from the one of the Nazis and the fascists. This does not imply, of course, that one form of totalitarianism is qualitatively preferable or better than the other. It only means that fascist and Bolshevik forms of totalitarianism are *both* similar and different.

IV. *The Inner Dimension: The Dilemmas of Democracy*

In the concluding paragraphs of his essay, Juan Linz makes a telling remark: «Competitive democracy seems to be the result of quite unique constellations of factors and circumstances leading to its inauguration and stability.» It is due to this almost fortuitous combination of factors which, while it produces a democratic system, carries within itself the seeds of tension, notably those deriving from two conflicting needs of democracy: the need to have sufficient levels of conflict in society since the absence of conflict would debilitate the forces of progress on which democracy feeds itself; and the need to keep conflict within manageable limits, to institutionalize it in order to prevent its potentially explosive nature from breaking apart the foundations of the democratic order. In practical terms, these two sets of needs translate themselves into dilemmas which every democracy is forced to confront. (And the remaining four essays of this volume of the *Handbook* deal with some of these dilemmas.)

By far the most important is the dilemma between competition and stability. As Dahl puts it in his essay, even in the most highly hegemonic regimes this dilemma does not cease to exist, since foci of opposition survive repression and terror.¹⁶ Needless to say, the dilemma is most acute in polyarchies, which Dahl perceives—in simple words—as the best kind of regime that can be achieved, or the closest approximation to a democracy that a political system can accomplish. While the barriers to opposition in a polyarchy are inherently low, the political interests of large segments of the population remain unsatisfied. Polarization of the political system on the one hand, and inequalities in political resources on the other, create a condition in which a polyarchy is constantly tantalized by the dilemma facing mixed regimes: repression or explosion?

The development and maintenance of a pluralistic social order is the solution that Dahl offers, an order in which access to violence and socioeconomic sanctions is either dispersed or denied both to opposition and government, and in which no ethnic group or subculture is indefinitely denied opportunity to participation in the government. An issue which Dahl does not address concerns, however, the problem as to whom or which institutions should control the process of dispersion of sanctions, the avenues to the means of violence, and the circumstances under

16. For a systematic, though not entirely successful, effort to define Nazi ideology, see Ernst Nolte, *The Three Faces of Fascism* (New York: Holt, Rinehart, and Winston, Inc., 1966). Also, Karl Dietrich Bracher, *The German Dictatorship* (New York: Praeger Publishers, 1970).

which excluded groups are integrated into the political system. In short, one cannot disagree with Dahl's statement that polyarchies can survive only if the process of promoting a pluralistic social order occurs through rather slow evolutionary changes.¹⁷ But the issue concerning the direction, pace, and content of the process of change remains, which points to the key question facing a pluralistic system. Will change keep up with the increasing demands that ethnic and socioeconomic subcultures make on the system? A positive answer to this question means that a regime can remain both diverse and truly competitive, while a negative answer is bound to push a regime towards repressive directions, with competitive politics likely to be displaced by hegemonic rule. And one possible consequence of a regime's sharp turn towards hegemonic forms of control and rule is collective violence, which—as Charles Tilly¹⁸ phrased it in his essay— «tends to cluster around entries into the polity and exits from it». In the short run, the establishment of an hegemonic rule raises the level of repression, if not outright terror, in society; in the long run, it engenders two pre-conditions for a revolutionary, or at least violent, challenge against the regime, since it makes the regime illegitimate among substantial sectors of the citizenry, and thereby creates multiple sources of sovereignty, and makes them willing to accept alternative claims to power.

V. Conclusion

Over the years, a monumental amount of literature on comparative politics has been produced by American political scientists, historians, and theorists of various inclinations. More often than not, the tendency has been to place works dealing with more than one country under the catch-all category of «comparative politics». The facility with which this category has been used denotes, above all, the rudimentary stage at which this discipline still finds itself—and it is debatable whether «comparative politics» can be considered as a distinct and definable discipline within the broader field of political science. Increasingly urgent is the need to develop precise criteria and conceptual/organizational paradigms which will make future work on crossnational analysis truly comparative. In this respect, this vol-

17. See *Who Governs?* (New Haven, Conn.: Yale University Press, 1961); *Democracy in the United States: Promise and Performance* (Chicago: Rand McNally, 1967); *Polyarchy: Participation and Opposition* (New Haven, Conn.: Yale University Press, 1971); *Regimes and Opposition* (New Haven, Conn.: Yale University Press, 1973).

18. Among Tilly's other works, see «European Statemaking and Theories of Political Transformation», in Charles Tilly, ed., *The Formation of National States in Western Europe* (Princeton: Princeton University Press, 1975).

ume of the *Handbook of Political Science* represents an auspicious start in the right direction. On the other hand, comparative politics should not limit itself to gaining legitimacy as an autonomous field by creating frameworks of analysis which are coherent and all-encompassing; its essential task is to help students of politics overcome the dichotomy between «us» and «them» which has colored much of the work on foreign political systems done in the United States.

Handbook of Political Science, Volume 5: *Governmental Institutions and Processes*, ed. by Fred I. Greenstein and Nelson W. Polsby, Reading, Mass., Addison-Wesley, 1975. 459 pp.

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This volume illustrates both the limitations and the special contributions of political science by scholars from the United States. Research on US political institutions has too seldom benefited from a comparative perspective, and it has not always confronted the context of political economy within which institutions work. And yet the phenomenal output of US scholarship has produced a truly cumulative understanding of political institutions, one which any serious theory of policy-making, including theories that normally underplay the importance of institutions, must reckon with.

I. Need for Comparative Studies

For the non-US reader, some chapters in this volume will seem a bit parochial. All of the writers except one (Anthony King of the United Kingdom) are citizens of the US. Students of US politics are often criticized by their colleagues in other fields for not making more use of comparisons with other countries. Only two chapters here make full use of such comparisons—William Riker on Federalism and Martin Shapiro on Courts. Partly as a result, these two chapters are the best in the volume. Anthony King's chapter on Executives does not present as ambitious a theoretical argument, but his task was greater, because virtually no one has written comparatively about executives before. King provides an indispensable guide to existing research worldwide, and shows the gaps in each topic and each country. Nelson Polsby's chapter on Legislatures looks mainly at the US Congress. Mark Nadel and Francis Rourke in their chapter on Bureaucracies summarize the US

literature, but make little use of foreign comparisons, even where some research is available. Harvey Wheeler on Constitutionalism offers an interesting personalized history of the Anglo-American tradition, with nothing on other countries.

One understandable reason this *Handbook* volume is weak on comparison of US institutions with those of other countries is simply that less research has been conducted elsewhere. For example, Anthony King remarks that the US Presidency is one of the few national executives that has been studied intensively. Indeed, the first study of important aspects of the British Prime Minister's office was conducted not by a Briton, but by a US scholar whose previous work was on the Presidency.¹ Perhaps US scholars have aided in the study of other countries' political institutions, but now the US, to better understand itself, needs foreign scholars to return the favor. After all, non-US scholars contributed much in the past. Still the greatest study of US politics ever written is Alexis de Tocqueville's *Democracy in America* (1840), a profoundly comparative work that could only have been done by an outsider. Sadly, this tradition has not continued. Fortunately, the comparative perspective was nevertheless kept alive by waves of immigrant intellectuals, who helped to revolutionize US social science in this century.² Now that this great migration has stopped, the US must develop its own indigenous movement to compare US politics with other countries.³ And yet this movement is only beginning. No two countries could be as ripe for comparison as Canada and the US, but political scientists in both countries have made virtually no use of this perspective. The best comparative studies of the two countries have been by sociologists. Perhaps scholars outside Canada and the US can more easily see the decisive advantages of such comparative study.

II. Institutions Do Matter

Should analysis focus on how political institutions behave, or can the impact of outside pressure on

them be so taken for granted that research should examine not the institutions but rather the pressures themselves? The «power elite» and «pluralist» interpretations of US politics that clashed in the 1950's had fundamentally different views of the shape of the pressure system.⁴ But they were peculiarly united in their view that political institutions were simply the battlegrounds for such pressures. Institutions did not have a life of their own. In his chapter of the *Handbook*, Nelson Polsby chronicles a movement away from this view in legislative research. After 1960, researchers began to discover that, while political pressures set definite boundaries on the behavior of US Congressmen, these legislators were enmeshed in institutions which shielded them somewhat from outside control while also subjecting them to internal norms or constraints.⁵ Douglas Ashford has noted a similar trend in comparative public policy to an identification of political structure as an «independent variable». For example, my own research has emphasized that political institutions make a difference in how a country debates policy in aid to the poor.⁶

Marxian observers for the most part do not concede political institutions much independent influence on policy-making. As Nicos Poulantzas has argued, «the capitalist state is characterized, today just as in the past, by a specific internal unity of its apparatuses, which is simply the expression of the interests of the hegemonic fraction, and of its own role as the factor of cohesion of the power block».⁷ Mainstream US scholars have accepted some elements of the Marxian critique of US politics, but in a way that attributes considerably more autonomy to political institutions.⁸ Marxian treatments of US politics often speak of «the state» as a single entity. It is difficult to reconcile this view with the lush differentiation and fragmentation of institutions so striking in the US. If US institutions are indeed consistently the agents of monopoly capital, then it may be correct to speak of the American «state» in a theoretical sense. But em-

1. Anthony King, «Executives», pp. 173, 224; Richard Neustadt, «White House and Whitehall», *The Public Interest*, II (1966).

2. Two sources on the far-reaching impact of immigrants on American intellectual life are: Donald Fleming and Bernard Bailyn, eds., *The Intellectual Migration: Europe and America, 1930-1960* (Cambridge, Mass.: Harvard University Press, 1969); and Robert Boyers, ed., *The Legacy of the German Refugee Intellectuals* (New York: Schocken, 1972).

3. Leading efforts to put US politics into comparative perspective are: Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace, 1955); Samuel P. Huntington, «Political Science, American Area Studies, and Their Paradigms of American Politics», in Lucian Pye, ed., *Political Science and Area Studies: Rivals or Partners?* (Bloomington, Indiana: Indiana University Press, 1975); and Seymour Martin Lipset, *Why No Socialism in the United States?* (forthcoming).

4. For selection of viewpoints in this controversy, see C. William Domhoff and Hoyt B. Ballard, eds., *C. Wright Mills and the Power Elite* (Boston, Mass.: Beacon Press, 1968).

5. Nelson Polsby, «Legislatures», pp. 285-88.

6. Douglas Ashford, «The Structural Analysis of Policy or Institutions Really Do Matter», Ch. 4 of Ashford, ed., *Comparing Public Policies* (Beverly Hills, Calif.: Sage Publications, 1978); Christopher Leman, *The Collapse of Welfare Reform: Political Institutions, Policy, and the Poor in Canada and the United States* (Cambridge, Mass.: Massachusetts Institute of Technology Press, 1979).

7. Nicos Poulantzas, *Classes in Contemporary Capitalism* (London: NLB, 1975), p. 164 and passim; see also his *Political Power and Social Classes* (NLB and Sheed and Ward, 1973), p. 303ff.

8. See: E.E. Schattschneider, *The Semi-Sovereign People: A Realist's View of Democracy in America* (New York: Holt, Rinehart, 1960); and Charles Lindblom, *Politics and Markets* (New York: Basic Books, 1978).

irical verifications of that view must necessarily study US institutions as they are, showing concretely how economic interests influence each institution in turn. Some of the leading Marxian critiques of US policy-making are surprisingly vague about how US political institutions work.⁹ To be persuasive, a radical critique of US national policy-making must come to terms, for example, with the extensive reporting on policy debates available in the *Congressional Quarterly* and the *National Journal*, two weekly magazines published in Washington, D.C. I hope that these publications are available to scholars abroad.

Faced with immediate evidence about US political institutions, Marxian critics would probably concede institutions at least a limited independent role. It is quite possible for political institutions to shape class alignments instead of the other way around. As early as 1778, Turgot warned that the division of political authority into several branches in the new American state constitutions (an approach also adopted later in the US Constitution) would become a «source of divisions and disputes».¹⁰ If it would be incorrect to describe American society simply as a pluralism of groups, still it is fair to describe US government as a pluralism of institutions. This political structure in turn affects political outcomes. The importance of political structure is brought out by William Riker in his *Handbook* chapter on Federalism. Riker warns against reducing federal politics to social and economic pressures, arguing that it is essentially a political bargain among elites. A curious illustration of this point is a recent collection of Marxian scholarship on Canadian politics; it sheds little light on the current constitutional crisis of that country.¹¹

III. Roots of the Status Quo: The Next Assignment

If Marxian analyses sometimes overlook the autonomy of political institutions, institutional analyses such as those recounted in the *Handbook* are equally guilty of overlooking the economic interests satisfied by existing institutional arrangements. American

political science is still reverberating from the warning of Bachrach and Baratz that empirical studies often overlook the «hidden face» of power. A lobby may be so powerful that it rarely has to exert its influence on political institutions; anticipation of strong opposition can prevent an issue from arising in the first place.¹² Few studies of US policy-making have adequately confronted this challenge, even though surprisingly few empirical studies have been undertaken to clinch the argument of Bachrach and Baratz.

A complication in the debate over «non-issues» is that the social and institutional conditions that promote them are not always deliberate. Those who benefit from the silencing of debate may be innocent of any role in suppressing opposing views.¹³ Many of the institutional patterns discussed in the *Handbook* are not explicitly aimed to further certain class interests at the expense of others, nor is support for the continuance of these patterns restricted to the favored groups. Of course, an institution is not immutable, and one way to test who benefits is to poke it and see who yells. In the US conflicts over issues have often been transformed into conflicts over institutional reform.

Political culture clearly favors the existing economic system and yet assessing responsibility for this situation is even more difficult than explaining the role of political institutions. Attempts to portray US schooling as indoctrination and to attribute the content of the media to corporate manipulation miss the complexity of the question. Remarkably little research has been done on political culture, whether from a Marxian or other perspective. The best existing study of a major segment of US business culture is now more than twenty years old.¹⁴ No volume or chapter of the *Handbook* focuses on political culture. If any topic seems to be central to the debate over the relation of capitalism to US politics, it is this topic. Yet without more basic, thoughtful research, the argument must remain unresolved.

It is the unique contribution of the research summarized in Volume V of the *Handbook of Political Science* to show the complexity and autonomy of US political institutions. Any serious treatment of US policy-making must come to terms with these findings. At the same time, it behooves both critics and defenders of the status quo to ascertain why these

9. See, e.g.: Richard C. Edwards, Michael Reich, and Thomas Weiskopf, eds., *The Capitalist System: A Radical Analysis of American Society* (Englewood Cliffs, N.J.: Prentice Hall, 1967); Ira Katznelson and Mark Kesselman, *The Politics of Power: A Critical Introduction to American Government* (New York: Harcourt Brace, 1975); Milton Mankoff, «Power in Advanced Capitalist Society: A Review Essay on Recent Elitist and Marxist Criticism of Pluralist Theory», *Social Problems* (Winter 1970); Ralph Miliband, *The State in Capitalist Society* (New York: Basic Books, 1969); James O'Connor, *The Fiscal Crisis of the State* (New York: St. Martin's, 1973).

10. Robert Jacques Turgot, Letter to Dr. Price, March 22, 1778.

11. Leo Panitch, ed., *The Canadian State: Political Economy and Political Power* (Toronto: University of Toronto Press, 1977).

12. Peter Bachrach and Morton Baratz, «Two Faces of Power», *American Political Science Review* (December, 1962); see also Raymond Wolfinger, «Nondecisions and the Study of Local Politics», in *American Political Science Review* (December 1971).

13. Raymond Wolfinger, *op. cit.*, p. 1074.

14. Francis X. Sutton, Seymour E. Harris, Carl Kayser, and James Tobin, *The American Business Creed* (Cambridge, Mass.: Harvard University Press, 1956).

institutions persist and why Americans like them. Comparative, empirical research seems most likely to shed light on this question, and I believe that non-US researchers can contribute much and uniquely to this effort.

Handbook of Political Science, Volume 8: *International Relations*, ed. by Fred I. Greenstein and Nelson W. Polsby, Reading, Mass., Addison-Wesley, 1975. 450 pp.

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In the first sentence of the editors' preface contained in each volume of the *Handbook of Political Science*, Fred Greenstein and Nelson Polsby note that «the fledgling political scientist learns that his discipline is ill-defined, amorphous, and heterogeneous». Readers of volume 8 of the *Handbook* will find this description especially apt when applied to the subfield of international relations. The picture of international relations is of a field fragmented and in disarray. Little general agreement can be found on a common conceptual framework—exacerbating the concomitant controversies over proper techniques for the accumulation of data, over which data are indeed useful, and over the methods and criteria utilized in its evaluation.

Faced with such a complex field, Greenstein and Polsby have wisely chosen to divide the subject matter along fairly traditional lines and parcel the duties of analysis among seven distinguished authors. Their contributions focus on these topics: «Theory of International Relations», by Kenneth Waltz; «Research Frontiers in the Study of International Politics», by Dina Zinnes; «The World Political System», by George Quester; «National Security Affairs», by Richard Smoke; «International Interdependence and Integration», by Robert Keohane and Joseph Nye; and «International Law», by Leon Lipson. For the most part, the essays do a good job of outlining the contours of contending theoretical approaches and techniques. George Quester, for example, summarizes the controversy between «idealism» and «realism» in writings on international politics, and Keohane and Nye contrast «integration theory» and analysis of transnational actors with state-centric «realist theory» in the tradition of Hans J. Morgen-

thau. Kenneth Waltz systematically compares «reductionist theories», e.g., economic and social explanations of imperialism, with systems theories and, not surprisingly, finds both approaches wanting.

The emphasis on summarizing alternative approaches, however, does not constrain the authors from building cases for particular points of view. After exposing the presumed deficiencies of other approaches, Waltz argues for his version of balance of power theory as an analytically sound and testable framework, which provides the best way to account for interactions between structure and units and to trace causes at different levels. The criticisms of other approaches are strong and well-conceived. The argument for balance of power theory as a solution, however, is less convincing. Keohane and Nye unabashedly attack the traditional description of the state of nations as a state of war, and they question the «basic tenet of conventional analysis: that students of world politics should limit their focus to nation-states and their interactions». In light of growing international interdependence and the expansion of transnational institutions, particularly involving a host of nonsecurity issues, Keohane and Nye argue persuasively for the usefulness of integration theory in providing insights into the wider spectrum of activities that often defy the limits of traditional analysis with its focus on nation-states, acting from self-interest, and dominated by concerns with security. This does not mean, as Keohane and Nye correctly warn, that integration theory («shorn of its teleological and regional orientation») is deemed superior to or can replace traditional analyses. What it does mean is that the reality of complex interdependence at a variety of levels, while applauded by some and jeered by others, cannot be ignored by serious students of international politics.¹

Dina Zinnes is notably less committed than the other contributors to outlining the broad parameters of their topic. Zinnes' presumed analysis of «research frontiers in the study of international politics» is instead a lengthy (111 pages) brief for one category of approaches: «quantitative international politics» or, as she prefers, the «scientific study of international politics» (SSIP). Zinnes' approach, however, does not go unanswered. Waltz devotes a portion of his essay to address the limitations of «the spell that numbers have cast over so many students of our subject» with the result that «the quest for ever more information now typically takes the form of accumulating coefficients of correlation»:

1. Keohane and Nye further elaborate on these issues in their recently published book, *Power and Interdependence: World Politics in Transition* (Boston: Little, Brown and Co., 1977), esp. parts I and II.

Recently the rage for data in the form of numbers has seemed to feed on itself without regard for the requirements of disciplined inquiry. It may be that scholars of a previous generation gained a spurious authority for their pronouncements through philosophical allusions, impressionistic appeals to history, and displays of literary grace, though that view is considerably exaggerated. The point is not that any particular method or emphasis is suspect, but rather that, whether one is offered a quotation from Plato or a number, one wants to know what it is for. How does it buttress one's logic? How does it establish a connection? How does it serve as evidence?

For Waltz there has been an overbearing concern with methods and less concern with logical utility. The first priority should be to develop and adopt a proper conceptual theory and methodology and then the choice of specific methods becomes essentially a tactical matter.

Beyond summarizing approaches and offering a number of claims and preferences, volume 8 contains some illuminating discussions on a host of other less contentious but interesting subjects. Richard Smoke's essay is a superb survey of national security affairs, emphasizing the period since World War II and tracing developments in the areas of technology, policy and strategy, and scholarly inquiry. Certainly not the least significant of Smoke's contributions is his great care in explaining the unending flood of jargon applied to the instruments, policies, and strategies that have accompanied the nuclear age. Smoke, unlike many of his colleagues, recognizes that in analyzing nuclear proliferation, for example, one need not at the same time contribute to the proliferation of typologies and obscure jargon.² Furthermore, in the process of clarifying concepts and reviewing the national security literature (at least in the context of the United States and its allies), Smoke successfully demonstrates the validity of the major theme of his presentation—what he calls «the context-sensitivity» of national security issues and analysis:

A distinctive characteristic of the field... is the peculiarly high degree to which most of its questions are *sensitive to context*: the international context and/or the technical context, either of which may change rapidly and in major ways. The national security specialist must possess—or develop—both a sense of international power relationships and change therein, and a sense of how technology works and how it evolves. (A sense of the ebb and flow of domestic politics is also necessary.)

Leon Lipson also makes an impressive contribution analyzing international law. Lipson begins with the most central issue of all: the uneasy suspicion that the «reality» of international law is highly problematical. In addressing that issue, Lipson avoids the rosy

2. For a short and handy glossary of some of the current weapons terminology, see *Bulletin of the Atomic Scientists*, 31 (April, 1975), p.13.

picture of the burgeoning practice of international law («the standard work of hundreds of lawyers in scores of firms and businesses») as indicative of either the health of the discipline or as a response to the cynics, who are drawn primarily from the academic world. Instead, Lipson correctly focuses on the substance, performance, successes, and limitations of international law as the yardstick by which its actual significance can be assessed. Final assessments are well-founded only when «derived from observation of institutions, constitutional structure, and ways of thought». Lipson proceeds to present his analysis of these institutions and structures and, with great care and wit, outlines the contours of prevalent perspectives on international law, which he labels: «wistful universalism», «disdainful positivism», «boiled realism», and «revolutionary moralism». The futility of assuming that any view will dominate is amply demonstrated. Lipson begins the last part of his essay by noting the small gains made in systematic theory-building in international law in the first half of the twentieth century. In spite of his attempt to distinguish two contemporary conceptions, one Soviet and the other American and both of which make claims to theoretical rigor and comprehensiveness, it is evident that in this area we are still firmly entrenched in the horse and buggy days.³

Finally, let me return to Kenneth Waltz's important essay, *Advocates*. On the one hand, of uncritical eclecticism or, on the other hand, of grand theory are well-advised to examine carefully Waltz's analysis. Waltz meticulously explores the meaning of «theory», the requirements of sound theory, and the necessity of explicitly defining terms. Only after an understanding of these crucial considerations has been established should one commence the admittedly difficult and tortuous process of formulating and testing theories. The steps involved in this process are thoroughly detailed by Waltz. One may quarrel with some of Waltz's definitions or tests or his demands of international relations theory, but the attempt to promote greater rigor and avoid the recklessness of generating spurious causal connections and unreliable explanations and predictions is absolutely essential to the development of the truly «scientific» study of international relations.⁴

3. For an attempt at presenting the rudimentary outlines of a specific theory of international law, see Stanley Hoffman, *The State of War: Essays on the Theory and Practice of International Politics* (New York: Frederick A. Praeger, Publishers, 1965), chap. 4.

4. The problems of defining and developing theories in the study of international relations are explored in numerous essays in the following collections: Herbert Butterfield and Martin Wight, eds., *Diplomatic Investigations: Essays in the Theory of International Politics* (Cambridge, Mass.: Harvard University Press, 1968); Klaus Knorr and James N. Rosenau, eds., *Contending Ap-*

The advanced undergraduate and graduate students that the *Handbook* addresses will surely profit from traveling through the pages of volume 8. What is covered is generally well-presented and illuminating. Reflective of the literature in the field as a whole, there are some pieces that are incisively analytical, e.g., Waltz and Keohane and Nye, and others that are rambling and unfocused, e.g., Quester. An important

asset is the references listed at the conclusion of each essay. They are generally thorough (Smoke, Waltz, and Keohane and Nye are superb) and, taken as a whole, are excellent sources for further elaboration and study.

In summary, anyone searching for an orderly, well-defined field of inquiry will be dismayed with the current state of international relations. The student will quickly discover that within this amorphous heterogeneity lies both the bane and the vitality of the field. This volume does not bring order to the chaos. It does, however, give a sense of the broad parameters of the field and provides a good basis for determining where and why the controversies exist and are destined to persist.

Approaches to International Politics (Princeton, N.J.: Princeton University Press, 1969); Klaus Knorr and Sidney Verba, eds., *The International System: Theoretical Essays* (Princeton, N.J.: Princeton University Press, 1961); and Raymond Tanter and Richard H. Ullman, *Theory and Policy in International Relations* (Princeton, N.J.: Princeton University Press, 1972).