

## *Chapter Seven*

# Beyond Positivism<sup>1</sup>

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Some intellectual eras come to an end with a bang. Others end with a whimper. Those that end suddenly are swept out by the rapid acceptance of a new theory or a major finding that makes the continuation of old beliefs impossible. Most intellectual eras in the social sciences may end with a whimper – with a vague sense of unease that not much progress has been made for all the efforts of able and hardworking scholars. My personal sense is that we are coming to an end of an era in political science, a slow, whimpering end.

The signs of the demise of an old era include a level of disquietude among the practitioners of a discipline. This volume contains essays written by some of the more able practicing empirical theorists in the country. Most of them indicate some dissatisfaction with the current status of political science. [G. R.] Boynton (Chapter Two) is concerned that the ‘most glaring weakness in our program has been the discontinuity between theory and research’. [J. Donald] Moon (Chapter Six) indicates that ‘one of the major shortcomings or problems of political science is the apparent confusion and diversity of standards in our field’. [Roger] Benjamin (Chapter Three) argues that ‘as soon as we begin to achieve some success in apprehending an important political process, diachronic change occurs in such fields as modernization to render our models obsolete’.

Signs of dissatisfaction with the discipline are not new, however. Since World War II, major confrontations have occurred within political science. Some have occurred in the literature (MacIntyre 1967; Winch 1958; Feyerabend 1970; Falco 1973; Goldberg 1963; Laslett *et al.* 1972).

However, many of the arguments have been fought as departments made recruitment and curricular decisions. The ‘quantitative versus nonquantitative’ dispute has surfaced in many departments each time a new faculty member is hired as colleagues debate about the appropriateness of the approach taken by candidates. Designing undergraduate or graduate curricula is another process likely to invoke a similar conflict. Those taking the antiquantitative position have argued that studying relationships using quantitative data does not provide an adequate understanding of the political world. Advocates of a more quantitative approach to political science have argued that without careful measurement and

1. Introductory Chapter, ‘Beyond Positivism: An Introduction to this Volume’, in Ostrom, E. (1982) *Strategies of Political Inquiry*, edited by Elinor Ostrom, 11–28, Beverly Hills, CA: Sage Publications.

the use of analytic techniques, one does not have a basis for knowing anything. In some subdisciplines of political science, specialized fields of inquiry have been identified by their stance in the quantitative versus nonquantitative dispute quantitative international relations is one example.

While strong feelings have been triggered, the quantitative versus nonquantitative dispute did not generally touch fundamental issues of importance to understanding political phenomena. When couched as a fight between those who quantify and those who do not, the dispute does not touch the central question of what are the basic processes occurring in different political systems in different eras. Rather, the dispute has centred on the way variables are coded and the type of statistics used as criteria to establish that a relationship between one variable and a second variable exists. It has been, to a large extent, an intellectual confrontation over how to represent descriptions of the world, rather than an argument about different organising principles for understanding the world. Both camps have remained fundamentally descriptive in their approach to the study of political phenomena.

The disquietude reflected in the essays in this volume differs from the discontent articulated by those scholars pushing for a more quantitative approach to political science. The concern here is that after several decades of far more rigorous empirical research, the hoped for cumulation of knowledge into a coherent body of theory has not occurred. This is a lament from those who have been, as Boynton phrases it, 'in the research camp'.

The central theme of the essays in this volume is the need for the development of theory as the basis of our discipline. Benjamin (Chapter Three) concludes after assessing the state of comparative political inquiry that 'we should be concerned again with opening up new concepts, reworking old questions, developing new puzzles, juking old theories and developing new ones, rather than developing and applying methodologies'. While political scientists have been asserting that theory should be the basis of our discipline throughout the post-war era, the definition of what constitutes proper theory is used differently here from the definition of theory articulated by logical positivists and accepted by many scholars in the discipline.

To some extent the heavy emphasis on descriptive, empirical, quantitative work may have resulted from the naive acceptance of a particular school of philosophy of science. The books that had the most influence on our conception of what theory should be were for many years those by Brodbeck (1968), Hempel (1965), Nagel (1961) and Rudner (1966).

At the time these authors accepted the logical positivist position on the type of scientific method to form the foundation of the social sciences. In a critique of rational choice theories from a covering law perspective, Moe (1979: 216) aptly describes the central belief system of the logical positivist's approach.

There is a fundamental unity of scientific method across the natural and social sciences; in both, the purpose of science is the explanation of events and the nature of explanation is nomological. An event is explained when a statement of its [...] occurrence is deductively subsumed under lawlike statements

and statements of initial conditions; and these logically prior statements, the theoretical premises, must be well-confirmed by the available evidence, since it is the presumed truth of the premises that logically justifies the expectation of the event's occurrence. Scientific progress hinges on the discovery of well-confirmed lawlike statements and on their integration into increasingly general and comprehensive theoretical structures.

If in order to have a theory admitted to the hallowed halls of science, the theory had to be based upon well-accepted empirical laws, then indeed the major effort in the discipline had to be to go out and discover empirical laws. Since scholars were trying to find empirical regularities, the major focus for political scientists doing research was on questions of method – of how to operationalize variables adequately and of the proper kind of statistical test to use to assert relationships between variables. These are essential questions of an empirically based political science. But their dominance during the past several decades places the questions of how to describe political relationships in a quantitative manner above how to gain an adequate understanding of the processes involved in the relevant world of inquiry.

Further, the positivist perspective on the necessity of building political science anew, based on a rock bottom of quantitative empirical research, dominated the instruction given to graduate students in our discipline. Given the recruitment pattern into the discipline, entering students have frequently evinced shock when presented with this perspective on the nature of the discipline. High school students are rarely exposed to anything called political science until their junior or senior year. By that time most students have already taken mathematics, science, history, literature, and languages. Many students select a college major early from one of the subjects they took in high school and found interesting and challenging. Students who enjoy abstract thinking and mathematics may have already selected mathematics or a science that allows them to pursue these interests prior to any introduction to political science (see Lave and March 1975).

The first introduction to political science qua high school civics does not attract the more theoretically and mathematically inclined students either. The focus on current events in high school civics courses associates political science with the study of current events for many students. This association is reinforced in college. At many universities large numbers of freshmen take an introduction to American government course where the emphasis is on the current structure and political behavior of American national government. Given the clientele and texts available, most of these courses are descriptive and include a heavy dose of current events.

Students who are interested in power struggles and learning the inside story related to current world crises are attracted. Students who are interested in more abstract intellectual endeavours are turned off. Majoring in political science does not alter the picture. Students can major in political science and never be introduced to political philosophy or the rigorous development of any type of modern political theory. Few of our undergraduates take more than minimal requirements in logic, philosophy, mathematics, or economics; and few take statistics as undergraduates.

Students entering graduate school with this type of background react to logical positivism in an extremely bimodal fashion. Some students vehemently reject the position. They either drop out of political science during their first year or simply develop a graduate program that does not require them to use theory rigorously or to learn quantitative methods. These students tend to ally themselves with faculty who are still fighting the antiquantitative war. Other students may have accepted the position too naively and wholeheartedly. If the position is accepted totally, what does this mean to a young colleague in political science?

The belief system includes the following propositions. For a discipline to be a science, it must have certain types of theories. For a set of logically connected statements to be a theory, they must start with a well-known empirical law. For a statement to be considered an empirical law, quantitative data must be gathered and analysed, and clear-cut and consistent patterns must repetitively be found. For students exposed to this position early in their careers, the import of their new scientific religion was that political science had no theory. David Easton (1953: 4), for example, asserted that:

Clearly, if political science could arrive at [...] at general theory, the understanding of political life that it would give would be both profound and extensive. There is no need consequently to point out that such a theory would be desirable because of its utility. The only thing that is not apparent, however, is that the formulation of such a theory is a possible and necessary step along the road to reliable and perceptive knowledge about politics.

No such theory is visible on the horizons of political research in the United States today.

For some political scientists, political science itself began in the 1950s. In a paper I read this past spring, for example, repeated references were made to the classical work in the field. All of the works cited were written after 1960!

With the 'rock bottom' approach, as Popper (1965) calls it, scientific theories could not be constructed until political science had undertaken substantial hard-nosed empirical work to find the empirical laws to become the bedrock of the discipline. With missionary zeal, several generations of young colleagues went forth to collect data so that a political science could be constructed using their empirical findings as the foundation. Many of the early generations in this revolution were armed for the foray with minimal statistical training and no training in mathematics or logic. Changes slowly appeared in our journals. First, a few cross-tabulations were interspersed among what previously had been predominantly textual material. Correlation matrices appeared next. Multiple regression techniques were then introduced. More recently, a whole spectrum of advanced multivariate statistics has been displayed.

As methods have become more sophisticated, graduate students of more recent times faced even more perplexing problems. Those who did not reject the mainstream approach realized that they must acquire substantial methodological training just to read the major journals. Given that our recruitment pattern into the discipline has not fundamentally changed, most entering graduate students

still have no philosophical, logical, mathematical, or statistical training as undergraduates. Consequently, in order to gain minimal levels of quantitative training, many graduate students take heavy course loads in statistics and methods and fewer courses where they might be exposed to the development of systematic substantive theories.

The combined effect of this recruitment process interacting with this type of socialization may have produced a 'know nothing' era in the discipline. Many scholars who presumed they were building our new empirical foundation did indeed know very little about substance and about the relationship of the statistical languages they used to the absence of theoretical models to which the language of data analysis should have been related. The criteria for what would be accepted as 'facts' became a significant correlation coefficient or a high R<sup>2</sup>, even when it meant the acceptance of nonsense or the rejection of long-established knowledge.

It is, of course, hard to characterize political science as being of one piece. Contained within the same discipline are so many individuals following such different approaches. However, during the past several decades, a number of young political scientists considered themselves to be in the vanguard of the discipline. They saw themselves creating the empirical foundation for the final development of a science. They rejected the work of those who had gone before them. It is their work that largely dominated our major journals.

During the 1960s many political scientists accepted, for example, the frequently repeated statement that 'political structure doesn't matter'. As Dye (1966: 297) expressed it, 'political variables do not count for much in shaping public policy'. The fact that political variables accounted for a small proportion of the variance in government expenditure levels after economic and social variables had first been entered in multiple regression equations was taken as 'proof' that institutional variables did not matter and should not be the subject of a mature science (see Dawson and Robinson 1963; Hofferbert 1966, 1972).

The way a process is conceptualized should affect the analytical techniques to be used for estimating statistical parameters in empirical models of that process (Wright 1976; Stonecash 1978; Johnston 1972; Hanushek, Jackson 1977). Multiple regression techniques were first developed to examine the independent effect of land, labour, and fertilizer in agricultural productivity (see Ezekiel and Fox 1959). Since each of these variables was conceptualized as *independent* and its effect on productivity was *additive*, the general linear model underlying multiple regression was the appropriate theoretical language for stating how these variables would be related to a dependent variable. I seriously doubt that one could find many statements in Hobbes, *The Federalist*, or Tocqueville that conceptualized the effect of institutions in a manner similar to that of fertilizer added to labour and land to produce corn (see V. Ostrom 1980, 1982). In the last decade, as models of how political structure affects the pattern of relationships among economic, social, and technological variables have been formulated and tested, scholars have found important structural effects (see Carmines 1974; Wright 1976; Frey and Pomerene 1978; Summers and Wolfe 1977; Phillips and Votey 1972; E. Ostrom and Whitaker 1973; Parks 1979; Parks and E. Ostrom 1981).

The languages of data analysis and method have dominated the languages of theory construction during much of the past two decades. As Sprague points out in Chapter Four, reliance on certain methods, because they are perceived to be more scientific, can coerce political scientists to ignore important theoretical questions. He is particularly concerned with the overreliance on *national* probability samples. Without adding contextual variables to individual records of a national sample, scholars can only examine how individuals, who have been plucked from their environments, acquire political attitudes. Important theoretical questions, relating political learning to context, cannot be pursued using most of the massive data sets already collected about the American electorate.

It is essential for empirical researchers to learn the languages of data analysis and to learn them early in their careers. Our undergraduate programs could all be strengthened by advising students of the importance of mathematics and statistics to an undergraduate major in political science. But a central task of the coming era is to reverse this domination so that the development of theory precedes the choice of appropriate methods to test a theory.

Fortunately, many scholars in the discipline, including the authors of chapters in this volume, are taking major steps to reestablish the priority of theory over data collection and analysis. Moreover, theory has also come to mean for many political scientists a set of logically connected statements without the requirement that assumptions used in a theory have themselves *already* been established as empirical laws. The covering-law perspective has not been replaced with another dominant philosophy of science. Among the important alternative views are those of Kuhn (1970), Lakatos (1971) and Habermas (1973). Among the authors who have provided useful overviews of the different traditions and important attempts at synthesis are Moon (1975), Toulmin (1977) and Shapiro (1981).

The essays in this volume reflect the subtle change occurring across political science and the sister disciplines of economics, sociology, and psychology. The first essay in this volume, by G. R. Boynton, is inspired by the work of Susanne Langer. Boynton argues that scientific advances have come when theorists have found principles by which they can order events that appear incommensurable and relate these events to one another. This places the focus for scientific advance on theoretical breakthroughs. The most important part of a theoretical breakthrough, according to Boynton, is an organizing principle. Terms are defined for the purpose of the relevant organizing principle. Proper names are no longer used. If events that were thought to be incommensurable are included within the same theoretical classification, it may also be the case that empirical references that share similar names may not be included in the same set when looked at from a theoretical perspective.

Boynton's chapter can be viewed as an inquiry into what is the 'right type of law' for social scientists. The right type of law, he argues, is highly specific and relates a limited number of variables to each other under stated conditions. The conditions of a theory state the values of other variables that must be closely approximated for the posited theoretical relations to hold among explanatory variables. The 'other' variables condition the type of relationships among the

explanatory variables stated in the theory. By stressing the importance of stating the essential conditions for an organizing principle to operate, Boynton urges social scientists to try to understand the logic of relatively contained situations where the conditions structuring a situation are specified. His notion, therefore, of theory pertains to the organizing principles used to understand particular types of situations structured in specific ways (*see also* Barry, Chapter Five).

The importance of structuring conditions is central to the argument made by Roger Benjamin concerning the historical nature of social-scientific knowledge. Benjamin argues that the relationships found *within* political systems may vary across historical time periods. Different historical periods are for Benjamin what the concept of a laboratory is for Boynton. Within a laboratory the scientist structures an experiment to study dynamic change while carefully controlling other variables. Whenever the scientist changes the fundamental structure of the experiment, the scientist produces diachronic change in the laboratory.

As Boynton points out, the concept of a laboratory is a mental frame. Social scientists, studying the on-going stream of events within real political systems, cannot set the parameters of variables that structure political processes. In times of relative political and social stability, the 'experimental conditions' remain unchanged. However, in times of major societal development, the level of diachronic change may be so great as to make the study of dynamics futile until the consequence of change in structure can be examined.

Benjamin compares the prevalent theories used to explain political processes within industrial societies in the 1960s, with the theories used in the 1980s. He reasons that new theories will be needed to understand the macro- and microeconomic forces at work in post-industrial as contrasted with industrial systems. Four theories are currently evolving that Benjamin (Chapter Three) feels will be the source of our understanding of behavior in the remainder of the twentieth century. The four theories are (1) Mancur Olson's recent theory of stable societies and economic decline, (2) the product cycle theory, (3) the collective goods theory, and (4) critical theory. Benjamin concludes from his review of these diverse theories that 'it is not business as usual in post-industrial societies'. This leads him to argue that political scientists must understand the changing macro political-economic structure before they can explain regularity in microbehaviour. Further, he argues the regularity in microrelationships in one historical period may be dramatically different in another.

John Sprague (Chapter Four) also examines the relationship between broader social structure and microanalysis. However, while Benjamin is interested in international political economies as they have an impact on political relationships within nation-states, Sprague is interested in the structure of neighbourhood-level political attitudes as they may affect the determinants of individual political attitudes. Both ask how microrelationships can be examined within a macrostructure; however, the levels of their analyses differ dramatically.

The fundamental theoretical question that Sprague addresses is, 'What are the mechanisms that connect microenvironments with individual political behavior? Or, more boldly put, how does social structure coerce individual behavior?' This

is an interesting question for Sprague to address, because he has been interested for many years in macro processes. Sprague develops an organizing principle to answer the puzzle of how the environment impinges upon relationships among individual political-attitude variables. His organizing principle differs from the model of the individual using the traditional assumptions of neoclassical economics. Rather, he posits a model of information processing in the individual that structures the processes through which individuals acquire political attitudes. His work exemplifies a nonrational choice model that is based on a foundation of methodological individualism.

While Boynton draws on the work of Susanne Langer for inspiration about how to develop coherent theory in political science, Brian Barry (Chapter Five) relies upon Mill. Mill, according to Barry, stresses the problem that social scientists face given the multiplicity of causes simultaneously affecting the processes of interest. By isolating some relatively simple cases, in a process similar to the mental laboratory that Boynton posits, Mill argues that one may be able to ascertain how some small systems operate when isolated from the impact of other confounding variables. However, such findings about one micro-system may not hold over time and place if structuring variables change dramatically. While we may be able to establish empirical trends, ‘we cannot use them with any confidence as a basis for prediction unless we have reason to expect the underlying conditions to remain unaltered’ (Mill, quoted by Barry, Chapter Five). Benjamin’s stress on the importance of history is also consistent with Mill’s analysis.

The cumulative effect of the chapters by Boynton, Benjamin, and Barry should lead to a sense of humility on the part of social scientists concerning what it is possible to do. All three scholars argue that when conditions are well specified and isolated, it is possible to develop rigorous theory for how some variables interact with others. However, the number of variables in the conditioning requirements may be large. Gaining a hold on how all those variables interact, if major diachronic change occurs, may be beyond our capabilities. Barry argues that this may leave a limited role for the social scientist, but he also argues that it would be ‘absurd to reject it in the pursuit of something more ambitious but actually useless’.

The advantage of economics, according to Barry, has been its focus on a confined part of social life where institutions tightly structure the set of available options for participants and the range of effects. Given some simple behavioural laws about how individuals value outcomes, it is possible to develop relatively well-supported theory about the behavioural tendencies, given the nature of immediate situations. Barry then asks what kind of theory may enable social scientists to understand nonmarket processes. He turns to the work of Mancur Olson and shows how it has been applied by Popkin to a different setting with considerable fruitfulness. He also illustrates how Hardin has reexamined the underlying model of the individual to develop a more general theory than originally developed by Olson.

Barry further addresses the question of whether there is a necessary ideological content to an assumption of methodological individualism. Methodological individualism does not require any particular assumptions about individual

motivation; rather, it simply insists that an adequate level of motivation be developed. Barry argues that there is nothing ideological about the methodological principle even though some applications are ideological. Sprague illustrates in his chapter a micro theory of motivation that is *not* an economic theory. Sprague demonstrates what Barry says can be done. Kiser and Ostrom also return to the same theme in the last chapter.

J. Donald Moon (Chapter Six) further explores the questions of how individuals make decisions and what types of theory enable scholars to advance understandings of political phenomena. Two fundamental strategies used by political scientists are identified by Moon: the interpretative approach, which he argues does not draw upon general laws and theories, and the theoretical approach, which does. Moon reexamines the situation facing European leaders in the 1930s and their decision concerning macroeconomic policies. In this case, macrostructural variables do not appear to account for the difference in policy decisions made by Sweden and Germany compared to Britain. Within systems that were relatively similar in structure, one set of leaders adopted a theoretical explanation for how the economic world operated that differed from the economic theory used by the other sets of actors.

Kiser and Ostrom (Chapter Seven) present a metatheoretical framework for the analysis and synthesis of a large body of political-economy literature. The microinstitutional political-economy literature they examine explains individual actions and aggregated results occurring in decision situations affected by institutional rules, the nature of goods, and the type of community. Further, they present the key aspects of this approach as a series of component working parts. In any particular theory, a scholar in this tradition will make implicit or explicit assumptions about the specific attributes of each of the working parts. What distinguishes this approach from that used by macropolitical economists is the prominence of a model of the individual at the central core of any particular model. Kiser and Ostrom return to the theme developed earlier by Barry and Sprague, that many different models of the individual are consistent with the principle of methodological individualism.

In their framework, any model of the individual will include assumptions about the individual’s level of information, the individual’s valuation, and the individual’s calculation process for selecting among alternative actions or strategies. Thus, the narrow rational choice model of the individual used in neoclassical economics and by some formal theorists of political behavior is characterized by assumptions that the individual possesses complete information, the individual values a single, externally measurable value (such as profits or the probability of being re-elected) and the individual selects that strategy which maximizes this value. A model of the individual drawing on the work of Herbert Simon would instead posit an individual with limited information and bounded capacities for processing information, with multiple goals, and with a calculation process involving limited search for satisfactory outcomes. The type of model of the individual that Sprague presents is characterized by an individual with incomplete information, multiple goals (political goals plus a desire to please those with whom he or she interacts

regularly) and a learning strategy based on rewards or punishments meted out for the political attitudes expressed.

The Kiser and Ostrom framework thus provides for comparing political and economic theories that use the individual as a basic unit of analysis. There is a whole family of such theories. At times the 'family' resemblance is difficult to discern. Some of the debates among proponents of one or another model of the individual have also tended to mask the fundamental similarity in the broad structure of these theories. The broad framework presented in the last chapter should enable scholars using different theories of individual behavior more closely to identify where their similarities and differences are. If we are ever to develop more general theories related to the political and economic world, we must be able to step back from the advocacy of any particular theory to examine how specific assumptions combined together make a difference and produce different explanations. It is hoped that the framework presented in this chapter will enable readers outside and within the tradition to identify some fundamental similarities in the work described.

This introduction began with the speculation than an era was ending in political science. Personally, I feel this may have been a necessary but unpleasant era in the discipline's growth, somewhat like adolescence. The tendency to reject the work of our predecessors and adopt new languages and new technologies in an effort to start over is an important phase of a discipline gaining maturity. But real maturity comes when the worth of past efforts is recognized and new languages and technologies are integrated with the best work of former times. In these chapters one finds many references to and uses of the work of political philosophers, a recognition of the importance of history, an awareness of diverse philosophies of science, a basic concern with the central place of theory in the development of the discipline, the use of formal models, and a recognition of the importance of rigorous methods in data analysis. While pessimism about the discipline has permeated the literature of political science during the past few decades, we may be moving into a more optimistic era. As theory precedes empirical work, and empirical studies help to refine our theoretical understanding of the world, the hoped-for cumulation in that understanding may finally occur. However, if we take the warnings of Boynton, Barry, and Benjamin seriously, the cumulation we do achieve will be limited in scope to specific types of theoretically defined situations rather than sweeping theories of society as a whole.

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