Reflections on a Half Century of Political Science: Lecture Given by the Winner of the Johan Skytte Prize in Political Science, Uppsala, 30 September 1995

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Robert A. Dahl, Department of Political Science, Yale University

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Let me begin by calling your attention to the fact that what had been a rather parochial field fifty years ago has now become virtually universal, in several senses. Although systematic inquiry into politics has its roots in classical Greece, before World War II the scholarly study and teaching of politics was pretty much confined to a limited array of Western writers, texts, subjects, and countries. Political science was taught in universities as a subject in its own right in only a few countries. With the exception of in the United States, departments, faculties, and even chairs in political science were a rarity. A notable exception, as I need not remind you here, was the Chair in Eloquence and Government founded here in 1622, which is not only one of the oldest professorships in the world but, I must assume, the oldest chair in what we today would call political science. With somewhat rare exceptions, however, politics, the state, and political philosophy were ordinarily taught as subjects in history, law, or philosophy – by historians, legal scholars, and philosophers.

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In Britain and the United States the study of democratic government was confined almost exclusively to the politics and constitutional systems of these two countries and France. Germany provided the key instance of a democratic breakdown succeeded by authoritarian or totalitarian rule. Although reliable information about the Soviet Union was not easy to obtain, interest in its political and economic system stimulated a great deal of writing and not a little misinformation. The other countries of Europe, including the dozen or so smaller democracies, among them the Scandinavian countries, were mainly or entirely ignored, as were the North American neighbours of the United States, Canada and Mexico, as well as Latin America, Japan, China, India, Southeast Asia, Australia, New Zealand, and Africa.

Half a century later, political science is a field of research, teaching, and robust scholarship at universities and research centres throughout the world. As scholars around the globe exchange views, engage in collaborative research, and interpret political systems from different vantage points, older and more parochial Anglo-American paradigms, theories, descriptions, and prescriptions have been replaced by interpretations more consistent with the extraordinary diversity of political experience the world provides.

The gains are not without cost. The sheer volume of information about widely differing systems, much of it qualitative or at best only weakly quantitative, outstrips the capacity of political scientists to interpret it. New classification schemes are an inventor's paradise. A theory sometimes becomes a sort of bed of Procrustes into which everything is forced to fit, no matter if the poor victim is fatally damaged in the process.

* * *

A second important change is a greatly heightened concern for verifiability, verification, testability, and falsifiability. I do not mean to imply that a majority of political scientists have become slavish – or even well-informed – followers of Karl Popper. I mean to say only that we are now much more likely than we once were to insist that the authors of a hypothesis, theory, argument, or conjecture provide satisfactory answers to such elementary questions as: How do we know it is true? What is the evidence for it? How reliable and valid is the evidence? Is the argument falsifiable in principle, or is there simply no way we could ever show it to be wrong, or, for that matter, correct?

I am aware that questions like these harbour deep philosophical issues. I am aware, too, that among some intellectuals – few of whom, I seem to observe, are physical or social scientists – it has become fashionable to deny the very possibility of finding satisfactory answers to questions that imply the existence of a reality that is something other than a construction of our own minds – an objective reality, if you will. While the social

construction of reality can be, when prudently used, a highly revealing way of interpreting the world, I would recommend that we reject the temptation to pursue that path as far as some seem to propose. It eventually leads, I think, to fruitless discourse about a world that is epistemologically indeterminate, ontologically chaotic, morally so relativistic as to deny all ethical criteria, and ultimately meaningless. Taken to its extreme, the view that all "reality" is socially constructed and nothing more is, I think, inherently self-contradictory: one cannot both claim that this particular view should be taken seriously and still adhere strictly to the premises on which it is based. I cannot help wondering whether it is a perspective that can be enjoyed only by intellectuals who are in the privileged position of being able to espouse it in words and deny it in their actions as they pursue their everyday lives.

* * *

The heightened concern for the possibility of verification has gone hand-in-hand with two other important changes: a greatly increased attention to questions of methodology and the employment of an extraordinary range and variety of methods of research and analysis. The growth in methodological sophistication among political scientists during the past fifty years is impressive. And because politics stretches across so many different aspects of human life, many different approaches have become necessary and useful. These include statistical and mathematical analysis, participant observation, in-depth interviews, sample surveys, rational choice models, studies of institutional permanence and change, historical, biographical, and psychological approaches, comparative and cross-national studies using one or more of the methods I have just listed – and not a few others. Although the study of politics is not and cannot be an experimental science – a point I want to return to in a moment – nicely designed experiments have occasionally produced some interesting, important, and I think valid findings.[1]

Like the other changes, these developments in methodological sophistication and range carry certain risks. It is tempting to define and choose research problems not because they are interesting, relevant, and important but rather because they are suitable for inquiry and analysis by means of one's favoured method. Yet political life is too varied to be described and explained with the aid of just one approach. Indeed, though a scholar strongly committed to a particular methodology may object to methodological eclecticism, or perhaps find it it too daunting to attempt, on some questions research may have to close in from many sides. I confess that in my own research I have been shamelessly eclectic in my methodological choices, often using a variety of approaches in a single work.

Despite these reservations, considered all in all, the change in our awareness of methodological issues and possibilities seems to me a clear gain.

Let me now mention a fifth and somewhat related development: Political science has also become considerably more theoretical, more concerned with theory and less sympathetic with a-theoretical descriptions. I do not seek to make too much of the point, but I am struck by the amount of attention that political scientists now give to providing a theoretical frame for their work. Although on the whole this heightened attention to theory seems to me worthwhile, I must confess to feeling at times that in place of endless description without much serious effort to make a description theoretically relevant, we now often substitute endless theorizing without much serious effort to make the theory empirically relevant. Elaborate, fine-spun theory is often poorly connected, if at all, with what I at least view as the world of politics.

One curious legacy of the older conception of political science still remains, at least in the United States. Among American political scientists the term "political theory" ordinarily refers to work by scholars who devote their primary attention to interpreting the rich trove of already existing works about politics composed by Western writers from Plato and Aristotle, through Machiavelli and Hobbes to Rousseau, Marx, Mill, and, more lately, John Rawls. It is important, as I see it, that political scientists acquire some familiarity with the ideas and arguments of the greatest past contributors to our understanding of the historical, moral, philosophical, conceptual, and even empirical dimensions of politics. However, at a time when theoretical approaches to political understanding exist across the entire range of political science I confess I find it odd that many political scientists and political science departments implicitly view "political theory" in a way that excludes a great deal of work in political science that I would regard as having great theoretical importance – and to include a great deal under that title that strikes me as hardly theoretical at all but, for example, almost entirely historical or exegetical in character. To define "political theory" as we still tend to do strikes me as a quaint survival from a time when political science was a narrower and more parochial field than it has become.

* * *

In yet another crucial development, important new fields of political science, or better, subfields, have also arisen. Let me offer a few examples.

The analysis of elections and voting behaviour was primitive until after World War II. Before then, although historians and political scientists were not at all timid about interpreting elections, their explanations were essentially anecdotal. Or at best (though even this was uncommon) they relied on aggregate data that permitted correlations to be made between election returns reported by territorial units – districts, cities, states,

provinces, and so on – and, for example, census data. Despite technical statistical excellence, making inferences from aggregate data to explain why voters made the choices they did, what their views were about candidates, and the like, was a crude and risky approach. The systematic sample survey literally transformed our capacity to study, describe, interpret, explain, and understand voting, elections, ideology, political attitudes, partisanship, and a host of other phenomena. And as surveys accumulated over time, they provided a much more powerful tool for describing and explaining changes in political phenomena that previously could be be accounted for only by armchair speculations or conjectures based on rather gross correlations.

The focus on the attitudes and actions of *individuals* engaged in political life, or, for that matter, unengaged, was sometimes referred to as the *behavioural approach* in political science, in contrast with the earlier focus on political institutions, governments, states, and other collective actors. Although the behavioural approach initially encountered the hostility of many traditional political scientists, it rapidly gained such momentum that by the end of the 1960s no American political science department, as I observed the academic scene, felt it could afford to be without at least one member engaged in the study and teaching of "political behaviour". In some departments, that approach became the dominant mode. And it was soon adopted by political scientists, particularly younger scholars, throughout the world.

Though my own graduate training had occurred before this important development took place, I was highly sympathetic toward it. With the aid of colleagues, research assistants, and graduate students whose training in behavioural methods of research was better than mine had been, I sometimes employed such methods in my own work – notably in *Who Governs*? Unlike a few of its most enthusiastic advocates, however, I was also convinced that the behavioural approach, which for some purposes was a huge improvement on the methods of earlier political science, had its own limitations. By 1959, the momentum acquired by the behavioural approach was strong enough, it seemed to me, to make it useful to emphasize not only the contributions of this approach but its limitations as well.[2]

By the 1980s, the elan and confidence that had marked the introduction of the behavioural approach into political science had shifted to theories of rational choice. By virtue of their assumptions about human behaviour, theorists were able to build mathematical-deductive systems in which conclusions about collective behaviour were rigorously demonstrated to follow from the premises. The novelty of this approach, its apparent power, its seeming success in having provided economics with considerably more precision and rigour than political science, and probably, too, the difficulty that political scientists without adequate mathematical training encountered in criticizing rational choice models despite their reservations, all

contributed to the prestige of rational choice approaches. As with the behavioural approach a generation earlier, major departments of political science soon saw themselves as incomplete without at least one, and sometimes more than one, member who employed rational choice models.

Just as had been true with the behavioural approach, so too with rational choice: it had distinct limits, and by the mid-1990s these were becoming evident. In a masterly critique in 1994, my colleagues Green and Shapiro argued – and to my satisfaction, demonstrated – that rational choice theories had so far added little significant empirical knowledge to political science. Like other methods, the rational choice approach has a definite place in political science, I think, but that place is considerably more modest than many of its more enthusiastic advocates seem to believe.[3]

A subfield to which much of my own work is particularly relevant is democratic theory. Since the time of Socrates, political philosophers had dealt directly or indirectly with democratic theory and practice, and most of the great works in Western political thought have a bearing on it to some degree. Yet in my earlier years as a political scientist, "democratic theory" was not a recognized focus in political science anywhere, so far as I know. Indeed, the expression "democratic theory" was, if I recall correctly, virtually non-existent. It may have appeared for the first time in the title of a book when my brief book, *A Preface to Democratic Theory*, was published in 1953. Giovanni Sartori, one of the major architects in building 20th century democratic theory, was, I believe, the first to use the term straight-out and unadorned as a title for his *Democratic Theory* in 1962.[4] Yet it was several decades before the term "democratic theory" came into fairly common usage among political scientists. Although what fell into that expansive subfield was still not very well defined, there was considerable agreement on certain questions, problems, works, and avenues of research that would surely have to be included within the purview of democratic theory.

In view of the extraordinary increase in the pace of democratization in the last decades of the century, it is not surprising that the number of papers, books, and conferences bearing on democracy and democratization also increased at an extraordinary rate. Going back to my earlier point about the internationalization of political science, the world-wide scope of these works, meetings, and other exchanges was striking. The content of papers and books, the scholars who wrote them, the location of conferences, now spanned the globe.

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During the latter half of the 20th century, then, the systematic study of politics – political science – has perhaps undergone more changes than during the preceding two millennia. As I have already said, although these

changes have brought problems and difficulties, the benefits seem to me to outweigh the disadvantages by a very considerable margin. Yet I feel obliged to qualify this judgement by calling attention to the limits that are imposed on us by the very nature of the phenomena with which we deal. Let me mention three.

1. The study of politics cannot be an experimental science

As I mentioned earlier, on some topics our understanding has definitely been enhanced by well-designed experiments. But the scope for experimentation is severely limited. Since that point is obvious and is often made, I need not pursue it. The upshot is that except in rare cases we cannot use experiments to confirm or falsify our hypotheses and theories. Lacking experimental proof or disproof, our general "laws" are in danger of being inadequately supported, or too broad to add significantly to our understanding, or, if precise and narrow, trivial or false.

Now it is true that what are sometimes referred to as quasi-experiments can be made if the measures and data available are valid, reliable, sufficient in quantity, and properly subject to appropriate statistical techniques.[5] Often, however, these conditions cannot be completely fulfilled. It is also true that several of the natural sciences – as examples, I have in mind geology, cosmology, and evolutionary biology – cannot subject some of their key hypotheses directly to experimental testing. However, they can test their hypotheses indirectly by employing findings and even techniques from experimental sciences such as physics, chemistry, and microbiology. We can rarely do so in political science.

2. Political science has an ethical component

The study of politics should do more, I think, than provide a better understanding of the political world. It should also help us to act more intelligently to achieve our goals, ends, hopes, desires. To act in politics is also, inevitably, a form of moral action. Consequently, I do not believe political science should aspire to be exclusively an *empirical* science, important as that is.

Over several decades a question of intense controversy among political scientists was "facts vs. values" – that is, whether empirical work based on "facts" inevitably asserts "values". I do not want to resurrect here a discussion that seemed to me rather unprofitable in the end. Much of what the proponents of "values" argued I thought then to be mistaken, and continue to think so now. Yet I have never had the slightest doubt that as a field of study, research, teaching, and writing, political science should include a serious concern with moral and ethical issues – in the jargon of American political science, with normative questions. I certainly do not want to argue that every political scientist has an obligation to write on problems in political ethics, much less that every work in political science should

explore ethical implications. But I do believe that such a concern has a vital place in the field taken as a whole. And it is because political philosophers since Socrates have grappled with ethics and politics that their work must remain, I think, an important subject in the field of political science.

3. Important political phenomena are not identical, they change over time, and for the foreseeable future at least they cannot be resolved into identical and unchanging units

The importance of this proposition may seem a little obscure at first glance. Perhaps an analogy will help. Let us imagine a world in which no two electrons are alike. Each electron has its own peculiar personality and behaviour, so to speak. Suppose that an electron in New Haven is significantly different from an electron in Uppsala; there is no reason at all for believing that an electron on earth is identical with an electron elsewhere; and what is more, there is no reason for thinking that an electron today is identical with an electron yesterday, let alone a billion years ago. Suppose that what is true of electrons is true of other elementary particles – neutrons, protons, mesons, and, worse still, their constituent particles, the quarks. As a consequence, if we move to a more complex level it turns out that the elements also have this same property: a hydrogen atom here is unlike a hydrogen atom there, an atom of oxygen yesterday is just not the same as an atom of oxygen today. The names – electron, quark, hydrogen, oxygen – are just labels for rather similar but definitely not identical things. What a mess physics would be! And chemistry. And microbiology. And geology. And the rest. (For that matter, what a mess the universe would be.)

Now I would be grossly exaggerating – fortunately – if I were to say that political life is as chaotic and variable as the hypothetical universe I have just portrayed. It definitely is not. Yet the phenomena of politics do change over time. Although both the American and Swedish political systems are, on the world stage, marvels of stability, neither system today is what it was a century ago, and not what it will be a century from now. A democratic political system today is not what it was in classical Athens; all democratic systems today are undergoing important changes that will make them different in the next century. Moreover, democratic systems all differ from one another to a greater or lesser extent and surely not in trivial ways. And so do all other political phenomena: parties, legislatures, executives, elections, states, revolutions, civil conflicts, social and political movements, beliefs, attitudes, ideologies.

I do not mean to suggest that political life is truly chaotic, though it may have some chaotic aspects. My point is only that we have not discovered, and I think we are unlikely to discover, unchanging, universal, and strictly comparable elements (or particles!) to which we may reduce political phenomena.

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Because of these three limits, and perhaps others, political scientists will continue to incorporate into their accounts of political phenomena certain explicit or implicit *judgements* – empirical, ethical, methodological, epistemological – that can reasonably be contested, and surely will be contested, by other political scientists, social scientists, or others. Consequently, in interpreting many important political phenomena – though I think not all political phenomena – political scientists are unlikely to converge on a standard, generally accepted, interpretation, even one that in its turn may be superseded by or integrated into a new standard theory. As has been true for more than two millennia, in the coming century competing and conflicting theories about politics will probably continue to dominate much of the important and relevant discourse among political scientists.

At this point you might well be troubled by an important question. Suppose we accept the conclusion that political science is not and probably cannot be a hard science like physics or chemistry. On few, if any, important questions, therefore, will political scientists arrive at answers so well confirmed by evidence and analysis that all or almost all knowledgeable persons will accept them as beyond serious challenge. If that is so, you might ask: Of what use is political science?

My answer is this: On few, if any, important matters can we hope to arrive at judgements like those of the natural sciences. Most of our judgements and most of our choices and decisions are fraught with uncertainty. It might even be the case that the more important the matter at hand, the more uncertainty we face. Yet we must and do make judgements, choices, decisions. And though our judgements may be informed by theoretical or scientific knowledge, they are practical, not strictly theoretical or scientific. Yet we make better or worse judgements, and whether they are better or worse depends on how well we understand the world. To put it in another way, even if we cannot gain perfect information we can make judgements that are badly informed or better informed.

Perhaps we are destined to navigate through an uncertain world for which no maps provide the precise, detailed, and certain knowledge we would like. Yet we do not have to navigate blindly. We can and do gain knowledge that helps, sometimes immensely, not only to avoid the reefs and shoals that threaten disaster but to identify and evaluate the alternative routes to our goal – indeed, even to appraise the worth of the goal itself.

The systematic study of politics can and does contribute to our ability to form intelligent judgements about navigating through this inordinately complex and uncertain world in which we must live. Looking back on the past half century of political science, I have not the slightest doubt that our understanding of political life and political systems – surely among the most

complex phenomena in our universe – has greatly improved. I have little doubt that it will continue to do so in the century to come.

We could, of course, escape the limits imposed on political science by the complex nature of political phenomena if we were to stop asking difficult and challenging questions. We could trade uncertainty about important questions for greater certainty about trivial ones. However, that solution would be a retreat from our responsibilities not only as scholars but also as citizens. For better or worse we live in a highly complex world, and troublesome as that complexity surely is, I think it is better to try to understand it than to flee from it.

NOTES

1. The finest example with which I am familiar is the study of the effects of television news on public opinion, by Shanto Iyengar and Donald R. Kinder (1987).
2. "The Behavioral Approach in Political Science: Epitaph for a Monument to a Successful Protest", *American Political Science Review* 55, (December, 1959), 763–772.
3. ". . . [T]o date, few theoretical insights derived from rational choice theory have been subjected to serious empirical scrutiny and survived. Many empirical tests turn out on inspection to have been so poorly conducted that they are useless in evaluating rational choice hypotheses. Tests that are properly conducted tend either to disprove these hypotheses or to lend support to propositions that are banal. Furthermore, rational choice hypotheses are too often formulated in ways that are inherently resistant to genuine empirical testing, raising serious questions about whether rational choice scholarship can properly be regarded as social science (Green & Shapiro 1994, 9)".
4. Based on his translation of *Democrazia e definizione* (1958), *Democratic Theory* was published by Wayne State University Press in 1962 and in paperback by Frederick A. Praeger in 1965. He returned to the general subject in *The Theory of Democracy Revisited* (Chatham, NJ: Chatham House Publishers, 1987).
5. An instance of a generalization that might well be described as a law because it is well supported by data and robustly survives rigorous tests is the proposition advanced by Bruce Russett and associates that countries with democratic governments never (or almost never) go to war with one another (Russet 1990, ch. 5, 119–145). A critique in *The Economist*, (1 April 1995) was rebutted by Russett and Andrew Moravcsik in *The Economist* (29 April 1995, 8).

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