Political Science as a Vocation

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About 90 years ago, at the end of World War I, Max Weber gave two now-famous lectures, published in English as "Science as a Vocation" and "Politics as a Vocation." They well repay reading and re-reading. Thinking of those lectures, it seemed appropriate, on this occasion, to reflect on "Political Science as a Vocation." As the title of my lecture indicates, I am directing my comments principally to the graduate students in attendance here, who are beginning careers in our field. After the lecture, I want to hear about your reasons for becoming political scientists, and your aspirations. In the lecture, I will reflect on our vocation from the vantage point of someone who has been a practicing political scientist—teaching, reflecting, and writing about politics—for 43 years.

I begin by pointing out that, viewed historically, you are in distinguished company. Aristotle was probably the first systematic Western political scientist, theorizing the relationship of politics to other spheres of life and creating a typology of regimes—what we would now call comparative politics. Machiavelli not only advised the prince but sought to analyze the nature of leadership, the characteristic hypocrisy of political speech, and the sources of republican greatness. Hobbes provided what is still one of the most compelling discussions of the causes of political violence and the sources of, and justification for, the state. Montesquieu and Madison developed a durable theory of constitutionalism, and Toqueville put forward insights into the nature of democracy that remain vibrant today—for example, in the work of Robert Putnam. I have already mentioned Max Weber. In the generation of political scientists born in the first three decades of this century I would list, somewhat arbitrarily, Gabriel Almond, Robert Dahl, Judith Shklar, and Kenneth Waltz—all of whom profoundly affected our knowledge of politics. Today, there are so many fine colleagues doing insightful work that to mention a few would be to risk slighting others whose work is equally important. The point is that you are joining a vibrant profession with a rich history. If I were conversant with classical Chinese and Indian sources, I could probably add to this list and extend this history even further into the past.

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Following Virginia Woolf, many of you probably noticed that except for Judith Shklar, this is a "procession" of men. Fortunately, however, this lamentable situation has changed. Had I listed contemporary political scientists of note I would have had to include Elinor Ostrom, Theda Skocpol, Margaret Levi, and Suzanne Rudolph, as well as many younger women who are now leaders in our profession. Although exclusion on gender and racial lines was long a reality, our profession is now increasingly open to talented people from a wide variety of backgrounds.

What, then, is "political science"? I have an economist colleague who likes to say that any discipline with "science" in its name is not really a science—that it protests too much. Were one to adopt a narrow view of science, as requiring mathematical formulations of its propositions, precise quantitative testing, or even experimental validation, *political science* would indeed be an oxymoron. But today I will defend our nomenclature by taking a broader view.

I define *politics* as involving attempts to organize human groups to determine internal rules and, externally, to compete and cooperate with other organized groups; and reactions to such attempts. This definition is meant to encompass a range of activities from the governance of a democracy such as Great Britain to warfare, from corporate takeovers to decisions made in the UN Security Council. It includes acts of leadership and resistance to leadership, behavior resulting from deference and from defiance. I define *science* as a *publicly known* set of procedures designed to make and evaluate *descriptive and causal inferences* on the basis of the self-conscious application of *methods* that are themselves subject to public evaluation. All science is carried out with the understanding that any conclusions are *uncertain* and subject to revision or refutation (King, Keohane, and Verba 1994, 7–9). Political science is the study of politics through the procedures of science.

TEACHING

Most of this lecture will be devoted to an explication of how, in my view, political science should be carried out: that is, the processes of thinking and research that yield insights about politics. But I want to begin by talking about *teaching*. Teaching is sometimes disparaged. Colleagues bargain to reduce their "teaching loads." The language is revealing, since we speak of "research opportunities" but of "teaching loads." National and global

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reputations are built principally on written work, not on teaching. But when we look around, we see that virtually all top-ranked political scientists in the world today are active teachers. Few of them have spent their careers at research institutes or think tanks. In my view, there is a reason for this. Teaching undergraduates compels one to put arguments into ordinary language, accessible to undergraduates—and therefore to people who have not absorbed the arcane language of social science. Teaching graduate students exposes one to new ideas from younger and more supple minds—as long as the students are sufficiently critical of the professor's views.

I want to emphasize this point about criticism, because in my experience, most students—but rarely the best—are too deferential. In 1927, so the story goes, the chief justice of the United States, former president William Howard Taft, came to Yale Law School, where his host was the chancellor of Yale Law School, Robert Maynard Hutchins, who was only 28 years old. Yale was then seen as a radical place; Taft was a conservative. So the chief justice said to Chancellor Hutchins, "Well, I understand that at Yale you teach your students that judges are fools." To which Robert Maynard Hutchins replied, "No, Mr. Chief Justice, at Yale we teach our students to find *that* out for themselves." Like the Yale students, you need to discover for yourself which senior political scientists are wise, and which are fools—by using your own critical faculties.

Teaching is rewarding in other ways. I have learned a lot from my colleagues indirectly through students, who come to me with insights, or works to read, suggested by other faculty members. And over the long run, one may see former undergraduate students become politicians or even rise to high positions. A former student of mine just entered the United States Senate, and another one is president of the World Bank. With former Ph.D. students ties are much stronger, since they remain in the profession. Two of my most valued colleagues and best friends at Princeton are former students, including the chair of politics, Helen Milner, and the eminent student of the European Union, Andrew Moravcsik. Former students of mine are scattered at colleges and universities around the United States, with some in Europe or Japan. In my office I keep a shelf of books that began as Ph.D. dissertations under my supervision. Paraphrasing Mark Twain: "It is good to do research. It is also good to advise others on how to do research—and a lot less trouble."

Never disparage teaching. It is an intrinsic part of political science as a vocation. Furthermore, it provides much more immediate gratification than research. When I am working on a major project, I never know whether the results will be worthwhile. I have left unpublished quite a bit of work, when I realized that the premises or methods that I used were flawed. Sometimes even one's published work will be ignored. Like politics for Weber, research can seem like the "long, hard boring of hard boards." The eventual rewards may be substantial or they may be meager; you never know until quite a bit later. If you give a good lecture or teach a lively, thoughtful seminar, however, the gratification is immediate: you know you accomplished something that day. During periods of self-doubt, teaching can keep you going.

THE SCIENCE IN POLITICAL SCIENCE

I now turn to research, asking: what do political *scientists* do? What are the processes they go through in the search for knowledge? I will focus on four activities: *puzzling, conceptualizing, describing, and making causal inferences.*

Puzzles

Interesting work begins not just with a problem—how democracy works in the United States, for instance—but with a puzzle. Puzzles are anomalies: what we observe does not fit with our preconceptions based on established theory. Hobbes sought to make sense of civil war and regicide. Toqueville wanted to understand how a decentralized, individualistic society as the United States in the 1830s could exhibit such overall cohesion, and even suffer from oppressive public opinion. Barrington Moore and a line of successors have sought to explain why some societies develop stable democracies while others do not; Theda Skocpol and others seek to account for great revolutions—and their absence. Great leaps forward in political science often take place when someone sees puzzles, where others have only seen facts. The great philosopher of science, Imre Lakatos, says that "science proceeds on a sea of anomalies," which certainly applies to political science.

There is an implication for graduate students, and teachers, of the importance of puzzles: never dismiss what appears to be a naïve question. This point was brought home to me when I was at Stanford in the 1970s, trying to understand economics better. I was not trained in economics but sought to pick it up on the fly, partly by attending economics seminars. I remember one such seminar, by an eminent student of multinational corporations who spoke very well, in a highly organized way. After about three minutes a young bearded man raised his hand to ask a question, which the speaker answered to my satisfaction. After three or four minutes the same hand went up, then again a few minutes later with a different question, each seeming rather obvious to me. After three or four questions, I was getting annoyed: can't you please let the speaker proceed? But after five or six questions it dawned on me that the questioner was the *only* person in the room who really understood the topic: his apparently naïve questions had dismantled the premises of the talk. I have forgotten everything else about the seminar, but I vividly remember the questions from the back of the room and the lesson: apparently naïve questions are often the most fundamental. So if you are puzzled, *ask*. In our field, there are no dumb questions. If you ask a naïve question, 90% of the time you may just have missed the point, and you will get the benefit of being corrected. But 10% of the time, you may be the only person in the room to see the anomaly—to sense, like a good detective, that there is something wrong about the story you are being told. The rewards of identifying major puzzles, for the profession and for yourself, are very large indeed.

Conceptualization

The next step is conceptualization: being clear about the meaning of concepts. As Giovanni Sartori pointed out long ago, concepts get "stretched" out of shape by political scientists seeking to do too much with too little (1970). And much often depends on definitions. How we think about the relationship between democracy and liberty, for example, depends on how we conceptualize both key terms. Likewise, whether democracies ever fight one another, or whether international institutions degrade or enhance democracy, depends heavily on how we define democracy. Whether civil wars are becoming more or less frequent may turn on how we conceptualize what is a civil "war" rather than a lesser form of civil conflict. And whether peace requires justice or is often in conflict with it depends on how we define both of those contested terms.

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There are, in my view, no right or wrong definitions. But there are explicit and implicit definitions, those consistent with ordinary usage and those that are not. And authors can be consistent or inconsistent in their use of terms. At the conceptualization stage, it is our obligation to put forward explicit definitions and to seek to operationalize them consistently. The more our definitions conform to ordinary usage, furthermore, the less confusion is likely to result.

Description and Interpretation

The core of what most political scientists do, most of the time, is *descriptive inference*. Inference means drawing more general conclusions from established premises plus a particular set of facts. For example, from known facts—such as that each of 150 countries has a particular form of government and particular economic characteristics—we may infer that there is a correlation between wealth and democracy. Properly speaking, such a conclusion rests on a chain of inferences—for instance, we may have inferred from a sample survey of tax returns what per capita GDP is for the country and from observation of three elections whether the country is democratically governed. These inferences are subject to error: people might systematically falsify their tax returns and the incumbent government might conceal decisive manipulations from election monitors. Other examples of descriptive inferences in political science are the generalization that democracies do not fight one another, the claim that international institutions typically provide information to governments about other governments' compliance with rules, and Moravcsik's claim that the European Union was formed by leaders concerned more about economic gains than security benefits (1998). As the examples indicate, descriptive inferences can be generalizations about a wide set of cases, or statements about events at a particular time and place. We can make inferences about individuals—for instance, the sincerity or hypocrisy of leaders or their perceptions of other leaders' behavior. We can also make inferences about the behavior of collective actors that may have subunits pulling in different directions: what "the United Kingdom" or "China" did in a particular situation, or whether "the United States," or unauthorized individuals, engaged in torture at Abu Graib or Guantanamo. And we can make inferences about relationships. Were there backchannel communications during the Cuban missile crisis between the Soviet ambassador and Robert M. Kennedy, and if so, what were they about? Did NGOs and state representatives collaborate in leaking information about the OECD negotiations 10 years ago about multilateral rules for investments?

Often political scientists seek to make their descriptive inferences more precise by attaching numbers to whatever process they are seeking to understand. Precision is certainly enhanced by numbers that are both reliable and valid, so such activities are to be encouraged. But it is important to understand the importance of both reliability and validity. Reliability essentially means that, using the criteria publicly employed, the number could be replicated by an independent observer. If, by criteria defining wars as organized violence involving 1,000 or more deaths, one team of observers finds 50 wars in a given period of time, another team, using the same criteria, should also find 50 wars. Validity is different: it refers to whether the measurement used—in this case, 1,000 battle casualties—fairly reflects the underlying phenomenon being discussed: that is, war. Are all conflicts involving 1,000 deaths wars, even if some take place in small societies, so that 1,000 deaths is a large proportion of the active population, while others are undertaken by very large societies that suffer many times more deaths in traffic accidents? Should all wars—from the skirmish that took 1,100 lives to World War II—be counted equally? These are questions of validity that cannot be solved by quantification, but for which one has to think hard about how one's conceptualization relates to the phenomena that can be measured. Before we accept a descriptive inference, we need to have asked questions about validity as well as reliability.

The issue of validity is highlighted by the famous philosophical distinction between a wink and a twitch. As Clifford Geertz writes, "the difference between a wink and a twitch is vast, as anyone unfortunate enough to have taken the first for the second knows" (1973). And, one might add, *vice versa*. If you see someone who is attractive to you moving her eyelids rapidly, you need to engage in interpretation before moving to a descriptive inference. If you interpret the eye movement as a wink, you may infer: "she loves me, too." But woe to you if you act on that interpretation and it was only a twitch!

Political scientists engage in interpretation all the time. When states "reject" a public offer, as China and India were reported as rejecting last summer's recent G8 proposal on climate change, are they really rejecting it, or simply establishing a bargaining position? When the International Criminal Court indicts the president of Sudan, is it seeking to bring him to justice or making a symbolic statement about Sudan's behavior toward its own people? When Bill Clinton pointed out during the U.S. primaries last spring that many white people were hesitant to vote for a black man, was he simply reporting an unpleasant reality or appealing to racism?

My point about descriptive inference is twofold. Descriptive inference is not the same as simple description: it involves an inference, from known to unknown, that can be incorrect or otherwise flawed. And both description and descriptive inference often rest on the interpretation of inherently—sometimes deliberately—ambiguous actions.

Causal Inference

Causality necessarily involves consideration of a counterfactual situation. If Charles II had not been executed in 1649, would Great Britain have a different political system now? If nuclear weapons had not been invented, would the United States and the Soviet Union have fought World War III? If Hillary Rodham Clinton had planned beyond Super Tuesday, February 5, would she have won the Democratic nomination for president? If the United States and Great Britain had occupied Iraq with twice as many troops, would the insurgency have been prevented? Since we cannot observe what actually happened and what didn't happen at the same time, making causal inferences is extremely difficult.

In experimental science the answer is to conduct experiments in which only one feature is different—for instance, adding or not adding a chemical to a solution—and observing how outcomes differ. Experiments are the best way to make valid causal inferences, and some of the most exciting work in political science now involves experimentation, sometimes in conjunction with surveys. I expect the domains in which experiments yield new causal knowledge to expand as imaginative political scientists explore the possibilities as well as the limitations of experimental methods.

Unfortunately for science but perhaps fortunately for the human race, political scientists cannot manipulate large-scale

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political phenomena, such as the outcomes of elections or the incidence of war, for their convenience—not that human subjects committees would let us even if we could do so! If we can find and measure many highly similar instances of the same action—for example, votes for Parliament or expressions of party preference in surveys—we may be able to make quite good causal inferences through the use of statistics. But even then, our procedures may contaminate our findings—for instance, people often make up answers to public opinion polls because they do not want to seem ignorant, and they deliberately recall having voted for winning candidates more than can actually have been the case.

More seriously, our inferences may be flawed because of omitted variables—something else changed that we failed to measure, and this change, rather than the one on which we focused, may explain what we want to understand. Or we can confront endogeneity: what we stipulate as the effect is actually, in whole or part, the cause rather than vice versa. A recent article in the *American Economic Review* disputes even the long-held view of political scientists and economists that increasing wealth promotes democracy. Daron Acemoglu and his colleagues believe that the correlation between wealth and democracy—which is very strong—does not suggest causality, because of omitted variables: other factors correlated with wealth that are also correlated with democracy (Acemoglu et al. 2008). None of our sacred cows is immune to criticism!

Furthermore, anticipation of consequences may create false impressions of causality. States may comply with international law not because they have incentives to follow it, or believe they are obliged to do so, but because they have carefully agreed only to rules with which they intended in any event to comply. Conversely, real causality may be obscured. Political scientists seeking to determine the effect of deterrence threats in international crises did not find any significant effects. Critics, however, pointed out that states that would submit to deterrent threats should have anticipated the threats, and their submission, and therefore not have stimulated crises in the first place. The difficulties of causal inference seem endless. As many of you know, social scientists have worked out ingenious responses to all of these problems, and continue to do so; but all of these responses are imperfect, relying on uncertain inferences. They are responses, not solutions.

Making causal inferences is the "Holy Grail" of political science, but with respect to large-scale events involving strategic interactions we are not very good at it—not because we are stupid, but because of inherent difficulties. Causal inferences are particularly difficult in international politics, where each major event seems to have multiple contributing causes and to be sufficiently different from other events of the same name that aggregation is problematic. There was only one French Revolution and only one World War I. However important it may have been, the Orange Revolution in the Ukraine was not very similar to the French Revolution, nor can the Iraq War be closely matched with World War I. Furthermore, events separated in historical time not only have different contexts—technological, political, social, economic, and ecological—they are affected by knowledge of earlier events. So any methods that require independence are jeopardized.

Aspirations to causal inference are often linked to hopes for prediction. Our causal knowledge of gravity helps us to predict the movement of planets and other celestial objects, and our causal knowledge of biology and, increasingly, genetics helps us to predict the incidence of disease. Sometimes we can make predictions—for instance, of election outcomes on the basis of economic conditions—but even our best predictions are imperfect. For my own subject of international politics the situation is even worse, because it revolves around conscious strategies of reflective actors. I act as I do because I anticipate what you will do, but you, knowing this, act differently than I expect, and I, in turn anticipating this, change my behavior. This is an infinite regress about which no prediction can be made: one would have to know how many cycles the players would go through, but if this could be ascertained, smart players would learn it and go one step further.

WHY CHOOSE POLITICAL SCIENCE AS A VOCATION?

If causal inferences in our field, and prediction, are so intractable, why choose political science as a vocation? My short answer is that we study politics not because it is beautiful or easy to understand, but because it is so important to all fields of human endeavor. I readily admit that I cannot prove that politics is important. Weber, in "Science as a Vocation," says that the presupposition that something is "worth knowing" "cannot be proved by scientific methods. It can only be *interpreted* with reference to its ultimate meaning" (2004, 18).

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Without governance, as Hobbes said, life is "poor, nasty, brutish and short." Democracy is in my view immensely better than autocracy, much less tyranny; but "making democracy work" is hard and imposing it from the outside seems close to impossible (Putnam 1993). Peace, economic development, health, and ecological sustainability all depend on political institutions and on political decisions, and often on leadership. If the state fails or gets involved in wars that involve high levels of violence on its own territory, creative activity in virtually every field except weapons development is likely to be stymied. Without a vibrant political science, leaders would be guided only by their limited personal experiences, historical analogies, and folk wisdom—all highly unreliable as a basis for inference.

We should therefore judge our work, in my view, not according to some idealization of science, or by the standards of the physical and biological sciences. Unlike Newtonian physics, we cannot properly aspire to knowledge of grand covering laws that explain a myriad of disparate events. Instead, we should ask whether knowing the political science literature on a given topic has prepared us better to anticipate what could happen and assign probabilities to these various scenarios. Are the results superior to historical analogies, extrapolation from the very recent past, and common sense? The answer is not always affirmative, but

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there are enough phenomena that we understand better because of the work of political scientists—from the operation of democracies to the operation of international institutions, from the exercise of various forms of power to the incidence of civil war—that we can be proud, within limits, of our profession.

But I said at the outset of this lecture that my audience is principally the graduate students assembled here, and that you should be critical of your elders. Although you are learning to build on previous work, I hope you are dissatisfied with the accomplishments of earlier generations, and skeptical about many of their inferences. I also hope that you see puzzling anomalies in some of the conventional wisdom—issues that need to be unpacked. And I hope that you have objections to express to what I have said here today.

Many of you will have noticed that my sources and examples come almost entirely from Europe and the United States, and from international politics, which has been dominated for five centuries by Europe and its offshoots. There is a sort of parochialism, therefore, about the way I have presented this subject. This parochialism is presumably due in some measure to my own limitations, but it also reflects the discipline, which is heavily American and to some extent European, with relatively few genuinely important independent contributions from scholars on other continents. As the economic and political centers of gravity shift away from Europe and the United States—as we move into the "post-American era" as Fareed Zakaria calls it—this is bound to change. Political science will become a global discipline. It will, however, only prosper if liberal democracy thrives. If we do our job, we political scientists will be irritating to political leaders, since we illuminate their deliberate obscurities and deceptions, we point to alternative policies that could be followed, we question their motivations and dissect the operations of organizations that support them and governments over which they preside. They will try to buy us off or, failing that, if not prevented from doing so, to shut us up. As a result, we have a symbiotic relationship with democracy. We can only thrive when democracy flourishes, and democracy—in a smaller way—needs us, if only as a small voice of dispassionate reason.

Our symbiotic relationship to democracy means that political science cannot be value-neutral. Nor can we be neutral with respect to order vs. chaos, war vs. peace. In our particular investigations we need to seek objectivity—a goal that is never realized but that we should strive for—because otherwise people with other preferences, or who do not know what our values are, will have no reason to take our findings seriously. In the absence of a serious culture of objectivity, no cumulative increases in knowledge can take place. But the overall enterprise should never be value-neutral. We should choose normatively important problems because we care about improving human behavior, we should explain these choices to our students and readers, and we should not apologize for making value-laden choices even as we seek to search unflinchingly for the truth, as unpleasant or unpopular as that may be. So I hope you will consult your values, as well as the literature, in deciding what to work on.

In conclusion, let me express the hope that you—the new generation of political scientists—will see openings where we see closure, and that you will have ideas about how to move through those openings to the insights that lie behind them. There are surely productive new interpretations to offer, and new descriptive and causal discoveries to be made. You may already be formulating some of these new views. The continuing vitality of our discipline depends, as it always has, on the critical imagination, conceptual boldness, and intellectual rigor of successive cohorts of newly trained scientists. The best of these political scientists will have learned theory, method, and much empirical knowledge from their predecessors—but will also have learned to question what they have learned. ■

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