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A QUANTITATIVE TOUR OF THE SOCIAL SCIENCES

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PART V. POLITICAL SCIENCE

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15. What Is Political Science?

Politics is part of virtually any social interaction involving cooperation or conflict, thus including interactions within private organizations ("office politics") along with larger political conflicts. Given the potentially huge domain of politics, it's perfectly possible to talk about "the politics of *X*," where *X* can be anything ranging from table manners to animal "societies." But although all of these are studied by political scientists to some extent, in the American academy "political science" generally means the study of a rather circumscribed range of social phenomena falling within four distinct and professionalized fields: American politics, comparative politics, international relations, and political theory (that is, political philosophy).

AMERICAN POLITICS

Broadly speaking, the academic field of American politics is exactly what one would expect: public opinion, voting, elections, legislatures, courts, and so on.

What's new and interesting here? Well, something very peculiar and quite disturbing has happened in American politics (the real thing). Every day in the halls of Congress and every night on political talk shows, partisans from the right and left fight a culture war with the intensity and viciousness of scorpions locked in a bottle. But for the most part, the public doesn't notice, isn't listening, and doesn't care. In short, political elites have polarized ideologically but, on average, ordinary citizens have not. Perhaps the most interesting recent work in American politics (the field) documents this strange development, explores why it has occurred, and investigates its consequences.

How do we know that the split between elites and masses has occurred? The evidence on the nonpolarized public comes from public opinion polls. Two excellent references are DiMaggio, Evans, and Bryson (1996) and Fiorina, Abrams, and Pope (2005). Basically, the evidence shows that most Americans tend to be centrists, and their opinions are no more extreme or polarized today than they were several decades ago (with one notable exception, opinions about abortion).

Figure 15.1 gives a sense of the general evidence on polarization. The data come from the National Election Study, a survey conducted every two or four

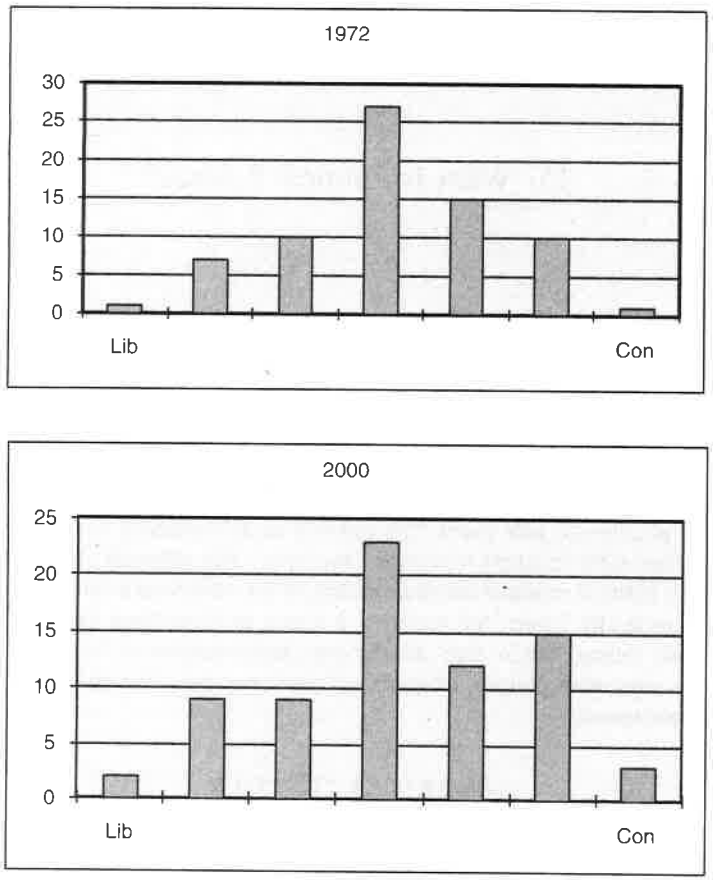

Figure 15.1. Americans' self-placement on the National Election Study's 7-point political ideology scale, from survey data in the presidential election years from 1972 to 2000.

years by political scientists in connection with national elections. The survey asks the respondents to place themselves on a 7-point scale, with "1" being extremely liberal and "7" being extremely conservative. Compare an early distribution of self-placements – say, from 1972 – with a later one – say, from 2000. If you didn't know which distribution corresponded to 1972 and which one to 2000, would you be able to guess? There is a little more polarization (more people place themselves at the extremes, fewer in the middle category) in more recent years, but not as much as you might expect from the discussion of "culture wars" in the media.

What about political elites? Measuring the ideology of elites is trickier, but one way to get at the question is to infer the ideology of congressmembers – critical elites, such as presidents, Supreme Court justices, governors, and state legislators – through their roll call voting. At one level, this exercise can be straightforward: Many interest groups give congressmembers a score based on

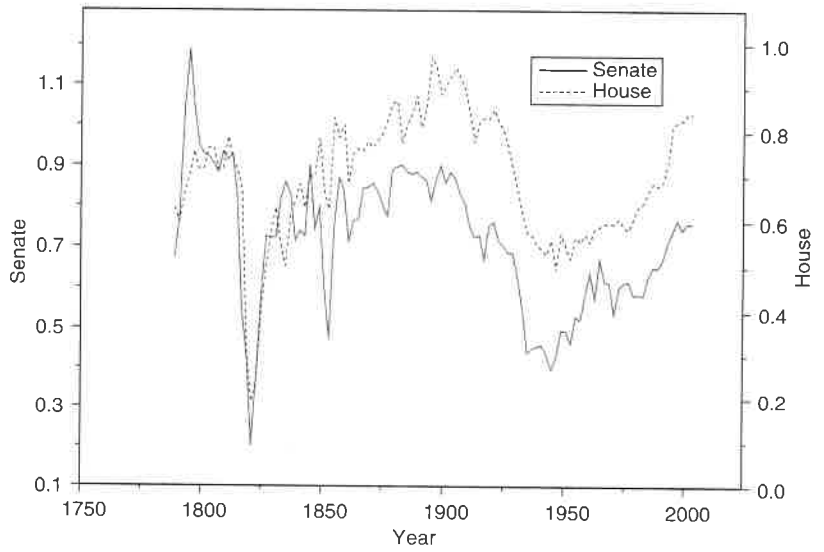

Figure 15.2. Ideological polarization in the House and Senate, 1790–2000. Adapted from Poole and Rosenthal (1997).

how frequently they vote the "right" way, according to the group. So, a senator who receives a score of 80 from the liberal Americans for Democratic Action is more liberal than one who receives a score of 20.

Political scientists have extended this simple idea by using multidimensional scaling techniques to study roll call votes in the House and Senate. The estimated ideology scales can be tied down across congresses and between the House and Senate by taking advantage of the fact that some congressmembers serve for multiple terms and some move from the House to the Senate. Then, given scores for individual congressmembers, one can calculate the score of the average Republican and the average Democrat (or whatever the major parties were at the time). The distance between the two average party members offers a simple and intuitive measure of polarization across the parties.

If one calculates this score, what does it show? To see, glance at Figure 15.2. It displays this measure of cross-party polarization, using the most widely employed ideology scores, Poole and Rosenthal's NOMINATE scores. One could use the figure to structure a political history of the United States. But focus on the second half of the series. You will see that polarization was relatively high at the time of the Civil War and subsequently peaked during the 1880 or 1890s, a period of huge social change and social strife in the United States. From about 1900 to about 1950, polarization steadily declined and remained low throughout the 1950s and 1960s. But then cross-party polarization began to tick up and continued to do so steadily for the next three decades. At present, polarization in Congress is at about the same levels as before the Civil War.

This is all quite interesting and extremely consequential for American politics. But rather than pursue the question, for the moment let me also point out the

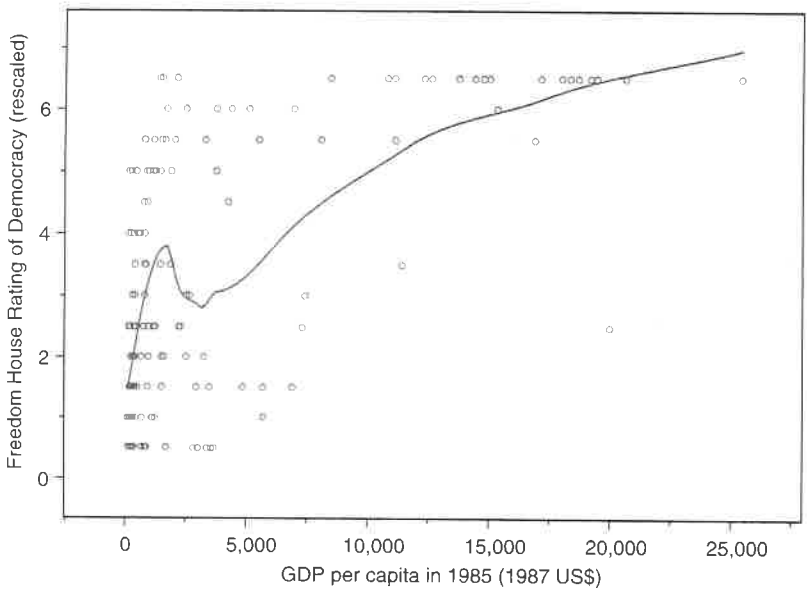

Figure 15.3. Democracy and per capita GDP for countries around the world. Line shows a fitted, locally weighted regression (lowess) curve.

staggering amount of effort behind Figures 15.1 and 15.2: Thousands of people were interviewed with an identical question, using statistically representative samples, over decades, and hundreds of thousands of roll call votes were tabulated and analyzed using sophisticated statistical techniques. Both of these graphs represent an enormous investment in social science by the American government and by many scholars.

COMPARATIVE POLITICS

Specialists in comparative politics in the United States study the politics of countries other than the United States. An example of a topic in this area is the study of democratic transitions: What causes countries to become, and stay, democracies? A long-standing answer has relied on the apparent link between wealth and democracy: If a country becomes rich enough, it becomes a democracy. Figure 15.3 shows the positive correlation between gross domestic product (GDP) per capita (adjusted for inflation, by pegging to the 1987 dollar) and democracy scores from Freedom House.[1]

A graph like this raises more questions than it answers. Does growth cause democracy or democracy growth? (Or, to put it more precisely, to what extent does growth affect democracy, and to what extent does democracy affect growth? There is no reason that causality has to go in one direction or the other.) If there is

[1] We flipped the Freedom House scores, so a high value means a robust democracy and a low value means a nondemocracy. This sort of simple transformation can be extremely helpful in data analysis.

some relationship between wealth and democracy, why should this be? And are the forces that encourage transitions to democracy the same ones that prevent transitions out of democracy?

At least on the last score there has been something of a debate. After examining democratic regimes in 135 countries from 1959 to 1990, Przeworski and Limongi (1997) insist that economic development does not necessarily trigger transitions to democracy. Rather, they suggest, affluence helps sustain democratic regimes once they come into existence. We thus see an apparent relationship like that in Figure 15.3, but it is simply because wealthy countries tend to remain democratic if they ever become democratic.

Boix and Stokes (2003) dispute this claim. Their empirical strategy is to extend the data to include observations from the mid-nineteenth century through World War II. These authors insist that economic growth does trigger democracy. They show that in more recent decades, some countries that developed but remained dictatorships would be expected to democratize very quickly.

The relationship between economic development and democracy remains a topic of active investigation. Clearly, the results have important implications for the future.

INTERNATIONAL RELATIONS

The field of international relations focuses on political relations between countries. The archetypal examples are war and trade. Recently, international human rights and international law have been actively studied. But surely the most interesting and potentially consequential development has been the idea of the so-called democratic peace.

The basic idea is that democracies are much less likely to fight wars against one another than against other types of states (autocracies, dictatorships, monarchies).[2] So, a world of democracies would be a world at peace – the fulfillment of one of humanity's eternal dreams. By implication, a prime goal for the foreign policy of democracies like the United States should (perhaps) be to support fragile democracies and encourage dictatorships to become democracies. Doing so makes the world safe for democracy.

The idea of a democratic peace is extremely contentious within the field of international relations. One reason relates to theory. Within international relations, one of the most influential theoretical frameworks has been "realism," the model in which the competitive pressure for survival is so strong at the international level that the domestic structures of countries don't matter – democracies, autocracies, and monarchies must all behave more or less the same way or they go under. The idea of the democratic peace thus flies in the face of a fundamental assumption held by many of the field's leading authorities.

Given the subject's intrinsic interest, relevance for foreign policy, and theoretical bite, you will hardly be surprised to learn that the literature on the democratic peace is huge. A handy review article is Ray (1998). At the heart of the

[2] There are a number of related empirical claims – for example, that democracies are more successful at warfare than nondemocracies, suffer fewer casualties, and fight shorter wars.

literature are two questions. The first is empirical: Is it true that democracies are more peaceful than nondemocracies, especially with respect each other? The second is theoretical: If it is true, why?

You might think it is easy to tell if democracies are less warlike than nondemocracies: Just tally the wars by regime type and perform a simple statistical test, say a *t*-test. But if you think about it for even a little while, you'll see that what seems so simple at first must be rather difficult in practice.

First, what do we mean by "war"? Should we include civil wars? How many battle deaths does it take before we call something a war (a frequently used definition requires 1,000 battle deaths)? What time period should we use (often 1816 on)? The simple logistics of tabulating the data will be formidable.

Second, what do we mean by "democracy"? How extensive must the franchise be before we call a country a democracy? Was nineteenth-century Britain a democracy? If a society allows slavery, is it a democracy? Was Periclean Athens a democracy? If we conceive of democracy as a continuum, at what level of democratic-ness do we score a country as a democracy for purpose of the democratic peace?

A third problem involves statistical methods and the idea of country-year "dyads." War involves a relationship between two states. In any given year there is a certain number of countries in the world, and each may interact (engage in war) with another. In particular, if there are n states, there are $\frac{n!}{2!(n-2)!} = \frac{n(n-1)}{2}$ possible dyads (pairs of countries). With a sizable number of countries, the number of dyads can be immense: Many of the empirical studies in this area examine something on the order of a quarter of a million dyads. Of course, most dyads are nonsensical as real war-pairs: Mongolia versus Uruguay, Tibet versus Zambia, and so on. What is a reasonable way to compare the war rate between democracy dyads versus nondemocracy dyads, taking into account the rarity of war and the absurd quality of most dyads?

By the time people argue themselves into exhaustion over these questions, there may not be anything left as an empirical finding. In fact, though, most analysts agree that the incidence of war among democracies is low, and it is much lower than that among nondemocracies and between democracies and nondemocracies. This is a striking empirical finding.

Of course, the finding could be spurious. For example, as argued by Farber and Gowa (1995), it may just be an artifact of the Cold War (others disagree). It might be that peace causes democracy rather than democracy causing peace. It might be that some other factor causes both peace and democracy (like economic prosperity). Getting serious about the endogeneity of regimes leads in the direction of instrumental variable studies, similar to Acemoglu, Johnson, and Robinson's (2002) study of democracy and economic growth.

Still, if the finding is real, the question remains: why? Are democracies just nicer than nondemocracies, or at least tend to share similar values (the "norm" argument)? Are the costs of warfare higher for democratically elected officials, making them more reluctant to engage in it (the "political survival" hypothesis)? Are democracies more economically interdependent and thus more reluctant to blow up profitable trading partners? Are democratic governments more transparent so that war (due to incomplete information) becomes less likely? All these ideas have a degree of plausibility and have been (and are) under investigation.

POLITICAL THEORY

The fourth subfield of political science, political theory, examines the normative dimensions of politics – for example, what is the best type of government (and what do we mean by "best" anyway?)? I won't spend much time on this normatively oriented subfield. However, a useful introduction to recent approaches is Shapiro (2003).

CONNECTIONS TO OTHER SOCIAL SCIENCES

Topics of interest to political scientists often overlap with other fields. An example is the study of racist attitudes in the United States. First, if you ask people on surveys questions about race, their answers sound a lot less racist than they once did (Page and Shapiro 1992). There are some long-running survey questions that make it appear, if you take the data at face value, that there has been a transformation in American racial attitudes. Second, if you look at voting scores in Congress during the twentieth century using dimensional analysis, for the most part you need two dimensions: the economic dimension and the racial dimension. Archetypically, you would find southern Democrats who were liberal on economic issues but very conservative on racial issues. Those two dimensions have more or less collapsed into one. One way to look at this finding is to say that race has become unimportant and it's all economics. Another way to look at it is to say that race and economics have become so intertwined as to become one. It will typically be the case that economic conservatives will oppose affirmative action while many liberals will support it, the result being a sort of ideological polarization of racial attitudes.

So, on the one hand, there is a group of political psychologists who say that Americans are still fundamentally racist but that they cloak their racism in economic doctrine, and that the conservatives are in fact racists but they try to avoid that label by recourse to economic arguments. When someone says, "I'm opposed to affirmative action because I'm in favor or equality of opportunity," that's not really a statement of a liberal attitude but just a cover for racism. From the other perspective, you could say, "This is nonsense. There's no clandestine racism here, and the arguments in favor of equal opportunity based on merit are sincere." This second school of thought sees a principled objection and argues that people have become less racist. Political psychologists become very heated about this. The difficulty here is, of course, that it would probably be impossible ever to prove the sincerity or disingenuousness of these arguments about affirmative action. For this reason, the argument usually degenerates into an ideological battle between true believers.

The question still remains: Are Americans really becoming less racist or are they just cloaking their racism under new guises? There are obviously normative considerations here, and it's tough to decide who has the better argument. By "normative considerations," I simply mean the values by which one might assess practice.

There has also been some interesting experimental work with media in this area using specially prepared videotapes of television news stories (Iyengar and Kinder 1987). The videos show the same stories but vary the race of the actors. The subjects of the experiment are asked questions before and after to get their response to the video. The finding is a sort of priming of race on certain issues. This experimental method appears promising and may be able to answer some of the questions about how Americans feel about race. There have also been some surveys that were cleverly designed to get people to tell the experimenter things they normally don't want to say. By designing the questionnaires properly, the experimenter can get estimates in the aggregate of the percentage of respondents who are not being candid. So, there have been innovations in survey research that have been quite interesting.

ROLE OF QUANTITATIVE METHODS

Quantitative data and methods have become central to the study of American politics, comparative politics, and international relations. My three previous examples, which all involve quantitative methods and data, are fairly representative of a great deal of work in political science. Quantitative methods are relevant even to political theory, because political philosophers are increasingly interested in evaluating real politics rather than debating about imaginary worlds that could never exist. (Sometimes this style of political philosophy is called "empirical democratic theory.") I don't want to exaggerate, though: In most political theory, the only numbers you'll find are on the corners of the pages. But today, one can't be a serious student of American politics, comparative politics, and international relations without a degree of facility in quantitative methods. The importance of quantitative methods often comes as a shock to first-year graduate students, who discover that their undergraduate courses did a poor job of conveying what the discipline is really like. On the other hand, for a student in a neighboring area such as psychology, sociology, or economics, the sort of political science that is necessary might use game theory (see Chapter 3), which is quantitative in its own way but different from statistical modeling and data analysis.

There is a good reason why quantitative data and methods are so important in professional political science: Evidence matters. When it comes to politics, you are entitled to your own opinion but not your own facts. And if you care about facts, you naturally gravitate to quantitative data. Given a number, you can argue whether it really measures what you're interested in. If not, you can work to improve the measure and show other people why the new measure is better. Given a group of numbers, you can deploy amazingly powerful methods for finding reliable patterns, methods developed over hundreds of years by some of the brightest people who've ever lived. It takes a Picasso to paint a Picasso and it took the Wright Brothers to build an airplane, but even an average person can, with sufficient training, fly a plane or, more to the point, use statistical tools – and use them pretty well. Then other people can check your findings for themselves and see whether you've made a mistake, exaggerated what you've discovered, or missed something important. Perhaps because politics is so contentious, methods of argumentation with high transparency and easy verifiability are extremely attractive.

I don't mean to suggest that quantitative evidence is the only kind of evidence about politics or even, in some circumstances, the best kind of evidence. A refreshing aspect of contemporary academic political science is its openness to many different kinds of evidence: qualitative, historical, ethnographic, and introspective as well as numeric. And this is surely a good thing. Still, political scientists have invested enormous effort in building public databases on many of the subjects central to the discipline, such as public opinion, elections and voting, legislative roll call votes, types of governments around the world and throughout history, the frequency and severity of wars, and many, many other topics. If you have a computer connected to the Web, you can access much of this data instantaneously at zero marginal cost. In addition, collecting new data has never been cheaper – although, in some cases, it is not so easy anymore, for example in sample surveys, where response rates have steadily declined over the decades as Americans have become saturated with surveys of all sorts.

In addition, political scientists are increasingly sensitive to the value of nontraditional data, for example from laboratory and field experiments. As the accessibility of old data has grown, the cost of collecting new data fallen, and the openness to nontraditional data increased, the reach of quantitative political science has expanded tremendously. In truth, we are living in the golden age of quantitative political science.

Is there anything distinctive about the quantitative tools political scientists use or the way they use them? Basically, no. Quantitative data and techniques are central to contemporary political science, but the methods we use are also applicable to – and are often derived from – other fields. Examples include regression analysis, propensity score matching (used by statisticians), instrumental variables and models for selection bias (used by economists), network models (used by sociologists), and measurement and scaling models (used by psychologists). The applications of these methods to political science problems are often innovative, but the basic structure of the models is similar across the social sciences.

On the other hand, I do think that political scientists deserve great credit for their openness to techniques invented elsewhere. In this sense, political scientists are the magpies of the social sciences: always on the lookout for methodological developments elsewhere and quick to steal them for their own purposes. This openness to new methods has a cost: a sometimes embarrassing amateurishness or naiveté about new methods. It also creates somewhat silly cults and fads in methods. But it does have the great virtue of injecting perpetual freshness, excitement, and growth into quantitative political science. The perpetually moving frontier means that whole new fields of inquiry suddenly become accessible. If you enjoy learning new skills and applying them in novel ways, you'll find quantitative political science tremendous fun.

ORGANIZATION OF CHAPTERS 15 TO 17

Before getting to the detailed examples, I will briefly discuss the relationship between theory and evidence in political science. I feel pedagogically obliged to present this material, but I'm not going to belabor it unduly.

Then, in the following two chapters, we will examine in detail an analysis of an empirical puzzle I pursued for several years, concerning the politics of Supreme Court nominations. Ultimately, the puzzle involves the strategic use of scandals and other bad news to sink nominees. I'll take you through this research roughly the way it actually unfolded. We use least squares regression, locally weighted regression, logit analysis, kernel density smoothing, Poisson regression, quasi–maximum likelihood estimation, and negative binomial regression, as well as some interesting techniques for analyzing data visually using conditional plots. The data used in these sections are publicly available, so you can download them and explore them more on your own if you want to.

HOW POLITICAL SCIENCE WORKS

Political science has been called "slow journalism." There is a way in which this quip captures something important about the field. But there is also a way in which it wildly misrepresents it. Let's start with the way in which political science really is slow journalism and then look at of the some ways it isn't.

Facts, Bounds, and Causal Mechanisms

A big part of political science is establishing facts by collecting systematic evidence and using the facts to put bounds on the size of social relations. Learning the facts is important because it rules out ridiculous claims based on "what everybody knows" but isn't actually true, freakish anecdotes, and other types of junk evidence. Making incorrect or exaggerated claims based on crappy evidence is a ubiquitous practice in political debate. (You may have noticed this.) So, simply discovering the facts, or putting some bounds on what you can claim with a straight face, is a very valuable endeavor. In this drive to get at the facts, political science (like all social science) does resemble journalism. But because we emphasize systematic evidence, which often takes time to collect, it is slow journalism.

But establishing facts and bounds is just the beginning. Once you have some grasp of what you're dealing with, there is a natural drive to go beyond mere facts to ask "why" questions. The emphasis shifts from "just the facts" to explaining the facts in terms of causal mechanisms.

I appreciate that "causal mechanism" has the clunky sound of social science jargon. But I have to say that this is a piece of jargon I like. The idea that the phrase tries to convey is: We are interested in something less than natural laws, because there aren't any natural laws in social science – just people making decisions and trying to live their lives. But even if there aren't any natural laws, things are not completely random. There is a logic to campaigning for office, voting in legislatures, directing bureaucracies, offering and accepting bribes, making revolutions and initiating wars, and so on. Nothing is deterministic but things often happen for a reason, at least on average. The causal mechanisms are the little engines driving the empirical regularities.

It's thinking in terms of causal mechanisms that distinguishes political scientists from political journalists, at least in my opinion. Thus, it's not the statistics

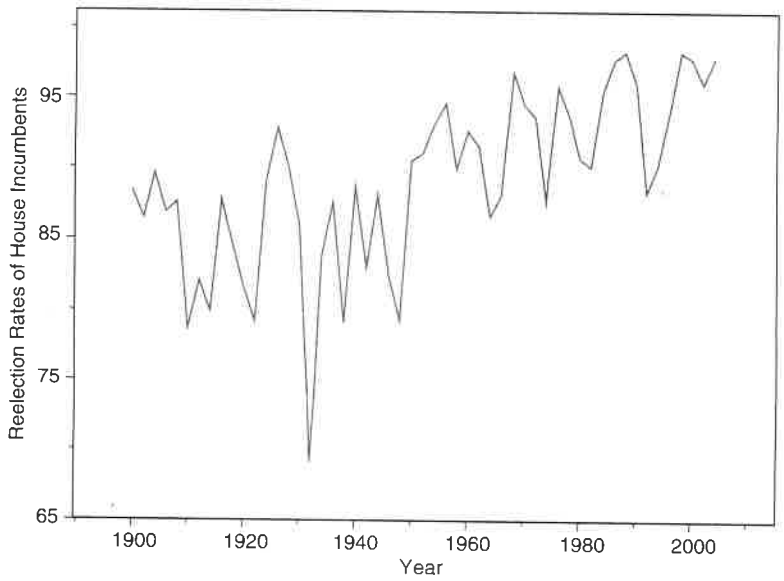

Figure 15.4. Reelection percentage (among those running for reelection in each year) in the U.S. House of Representatives, 1900–2000.

and game theory per se – these are just aids for finding systematic patterns, on the one hand, and thinking clearly about them, on the other. Rather, it's the habit of looking beneath the surface to try to understand the logic of what's happening that's key. On this account, De Tocqueville and Madison were great political scientists, which seems right to me.

I have been emphasizing the so-called positive side of the endeavor – facts, bounds, and causal mechanisms. But it's important to recognize the normative or evaluative side as well. Over time, one sees the community of political scientists obsessing over different questions. A question catches on and a seemingly endless stream of articles flows out, many dealing with apparently small, technical, or even trivial parts of the question. Is this just faddish behavior? Not really, because almost invariably the questions that really grab people have a big normative component. All the obsessive concern over measurement or just the right statistical technique, then, comes from a desire to get it right – because the answer matters for a burning normative question.

Let me illustrate the facts-bounds-mechanism trajectory. From 1980 to 2000, what percentage of incumbent congressmembers running for reelection won their races? Now, I could give you any number from, say, 25% to 98%, and you might believe me. The actual answer is 86% for senators and 95% for representatives. These numbers appear quite high. Certainly a world in which most congressmembers regularly win reelection is a different world from one in which most lose reelection.

Let's look more closely at the congressional reelection data. Figure 15.4 shows that reelection rates in the House of Representatives really are high and always

have been, at least for the past century. So, if we hear the claim that American democracy is falling apart because congressmembers don't have to worry about reelection, we know that we should be skeptical because, if this is true, American democracy has been falling apart for 100 years or more.

Let's think a little harder about that high rate. House members must get reelected every two years. For a congressmember to last for a decade – which doesn't seem to be an unreasonable ambition for someone who aspires to a career in politics – he or she has to win five elections in a row. Suppose the chance of winning in each case is 90%. Then the chance of making it to a decade is $0.9^5 = 0.59$ – in other words, better than 50–50 but not hugely so. From this perspective, a congressmember who wants a reasonable chance of holding on to his or her seat for an appreciable length of time absolutely needs what appears to be an astronomical reelection rate. This is an example of what I meant by bounding a social phenomenon. (This analysis is only an approximation, though, because actual elections are not independent events: Some congressmembers have essentially a 100% probability of reelection, while for others the probability is closer to 50% – and congressmembers in these districts typically do not have long careers.)

Now let's look at Figure 15.4 again. Note the big dip in about 1930. What went on there? The obvious explanation involves the stock market crash and the Great Depression. Could there be a systematic relationship between congressional reelection rates and economic performance? That would be interesting if true, because it suggests that congressmembers might have an incentive not to wreck the economy.

Second, note the way the time series appears to have two halves, one before about 1950 and one after. Reelection rates appear to jump from about 85% to about 95%. To confirm this intuition, let's do a little time series analysis.

Time series analysts have devised "structural break" tests exactly for situations like this (Hansen 2001). The basic idea is to estimate time series models before and after a hypothesized break and then compare the estimated parameters to see if the difference is more than could be explained by chance. Recent advances allow you to detect breaks without specifying their location beforehand. Performing such a test (the Zivot and Andrews [1992] structural break test, which is available in Stata), we find strong evidence of a permanent shift in the time series (statistically significant at the 1% level) in 1950. So, the pattern we see by eyeballing the data seems likely to exist.

Why should there be a structural break after the Second World War, with higher reelection rates after the war? Maybe part of the answer involves better management of the economy – perhaps the rates went up because the government understood better how to avoid economic busts. But that might not be the entire story.

Another obvious difference before and after World War II is the growth of government. Between the Great Depression and World War II, the federal government got much, much larger. This has led some scholars to suggest that congressmembers use the opportunities created by a burgeoning bureaucracy to perform ombudsmen services for their constituents – activities like finding lost Social Security checks, dealing with problems involving immigration, the Internal Revenue Service, and so on. In turn, such services create a personal connection between some voters and their congressmember, a so-called personal vote.

This story has a degree of plausibility if you have had any experience working for a congressmember or hanging out in his or her office. Moreover, it has a nicely ironic touch because it suggests that congressmembers had it coming and going – that is, congressmembers got reelected by giving voters the big government they wanted, then did even better by helping voters with the problems created by big government.

If you want to pursue these ideas, you might check out Jacobson (2003), Fiorina and Noll (1978), and Cain, Ferejohn, and Fiorina (1990). But my point here is not to analyze congressional reelection rates per se. Rather, it is to show the movement from (1) acquiring a factoid (quick journalism), to (2) reviewing systematic evidence and establishing bounds (slow journalism), to (3) asking "why" questions, trying to tease out what causes what, and thinking in terms of causal mechanisms (social science).

One can – and I would argue, should – take this example even further. Suppose we could get some good ideas about why congressional reelection rates look the way they do. We can now ask: Would making elections more competitive actually increase democratic accountability? If the reelection rates are so high because congressmembers work frantically to give people what they want, boosting competition might not increase accountability very much. But if legislators use high rates to pursue extremist policies or pander to interest groups, greater electoral competition might well boost accountability. And if so, how can we make elections more competitive? In other words, we can start to ask normatively oriented "so what" questions.

I find data like this endlessly fascinating, but some people of my acquaintance find them a bit boring, at least when the data come from the United States. For some reason, they insist on examples from countries across the big water. So, let me just refer to another example. Let's start with a factoid question: What percentage of votes cast in national elections in Germany's Weimar period were cast in favor of the Nazis or other antidemocratic parties? The answer is: 33% for the Nazi Party and 17% for the Communist Party of Germany in the eighth German Federal Election on November 21, 1932, and 44% for the Nazi Party and 12% for the Communist Party of Germany in the ninth German Federal Election on March 5, 1933. In other words, a large portion of the electorate did *not* support antidemocratic parties.

This factoid suggests a very different world from one in which an overwhelming majority of the German public voted for the Nazis. Knowing this factoid might lead us to ask a question requiring systematic data: When democracies perish, how much support for the antidemocratic forces is there in the public at large? This question has a real normative punch, because it is another way of asking whether mass electorates can be trusted with democracy. And then: If mass electorates can be trusted and the problem is antidemocratic elites, how can we protect democracy from its elite enemies? If you find these questions interesting, you can learn more in Bermeo (2003).

An interesting related question, perhaps falling closer to psychology or history than political science, is why it is such a common belief that the Nazis won in a democratic election. Perhaps this (false) belief is popular because it leads to an interesting story and a "paradox" of democracy – What should be done if an

antidemocratic party wins in a democratic election? – or perhaps it simply arose from a misunderstanding of historical writings.

Theory

Perhaps you've already discerned the structure I tried to set up in my description of research trajectories: on the one side the facts, on the other side normative evaluation and possibly prescriptive design, and in the middle causal mechanisms or theory. Thus: facts to theory to normative evaluation/prescription. In this framework, the careful elaboration of causal mechanisms – the theory – is the key component connecting data to practical recommendations. This emphasis on theory, and the willingness to concede it primacy, makes political science quite different from slow journalism.

Theory is not an end in itself, or at least no more than barefoot empiricism is. One wants theory because one wants to make sense of a mountain of brute facts (on the one hand) and to guide normative evaluations and policy or design prescriptions (on the other). From this perspective, nothing is more practical than a good theory.

But what's a good theory? I used that phrase as if it were obvious what makes a theory good. Unfortunately, it isn't. As silly as it may sound to non–social scientists, the proper standard for evaluating theories frays tempers in many departments. In fact, a few years ago, hundreds of political scientists attending the big national convention went around wearing little colored buttons to show their allegiance to one side or the other in the "theory wars," sort of like the Ghebbellines and Guelphs in the Middle Ages – or, perhaps I should say, like kids in the color wars at a summer camp. One could get a laugh out of grown people behaving this way if professional advancement and status weren't so tied up in it. Which, of course, is why things get so heated.

This is not the time or place to provide a detailed exegesis of political science's theory wars. Also, I am hardly a neutral party. But people love controversy, so let me just sketch the basic contours.

I have suggested that, in the realm of quantitative techniques, political scientists are the magpies of the social sciences. This is true for theory as well. If you look at classic works of political science from the 1950s and 1960s, you find that their authors borrowed heavily from sociology and social psychology. More recently, political scientists have borrowed heavily from the theories of economics and cognitive psychology. Some of this shift just involves keeping up with developments in other fields. But many political scientists see compelling reasons to move from a vague, ad hoc style that I associate with sociology to a more theory-based approach that I associate more with economics. Basically, these political scientists want their work to have strong microfoundations, they want the logic of their arguments to be as tight as possible, and they want the evidence they use to be closely tied to theory (which, in turn, demands clear theory).

This sounds fine in the abstract, but if you push really hard on strong microfoundations, tight logic, and clear theory, you can end up in some surprising places. One place is "rational choice theory," which isn't a theory at all but a set of theoretical impulses (or bets) plus a formidable toolbox. The theoretical impulse is

that many people approach politics instrumentally, that is, they have something they want from politics and they go about getting it in a fairly sensible way. The theory is silent about what people want from politics – you can fill in that blank any way you want (including "feeling good about yourself" or "meeting your civic obligations"). The toolbox includes decision theory, standard noncooperative game theory, evolutionary game theory, agent-based modeling, and so on: lots of math-based tools.

It is quite incorrect to see rational choice theory as hegemonic in political science, in the way it truly is in economics. But it is the single most cohesive theoretical movement and is well represented in all four fields, especially American politics. Not surprisingly, it has provoked a backlash (and did so from its first appearance in the early 1960s).

The backlash takes two forms. The first comes from people who are not opposed to theory in theory, just in practice. These critics are typically very fact-oriented, skeptical about the empirical reach of any theoretical framework, and hostile to pure theory as a waste of time. Sometimes they also dislike quantitative work, because they are suspicious of generalization in any form. (This tends to be the case in comparative politics and international relations.) Thus, these critics tend to favor work that is historical or semianthropological in style. But sometimes the "hostile in practice" critics are very quantitatively oriented, but in an exclusively inductive fashion. This tends to be the case in American politics, for example. Members of the latter group typically favor the older social psychological mode of work.

The second group of critics are not just opposed to rational choice theory in practice; they are opposed in theory. To see where they are coming from, recall that old quip about the difference between economics and sociology: "Economics is about how people make choices; sociology is about how they have no choices to make." If you think about this, you can see that both points of view have validity. On the one hand, people are constantly making choices, including ones they see as important. Understanding what they are doing, as they perceive it, seems worthwhile. On the other hand, our lives are severely constrained by our economic situation, our understanding of the world, how we see ourselves (our identity), and how other people perceive us. From this perspective, the room for individual choices is relatively uninteresting; the interesting part is where the constraints come from and how they change.

You can take any of these positions – rational choice, inductive/historical, or sociological/constructivist – and argue for or against it in the abstract and make a seemingly convincing case. Well, of course! Otherwise, bright people wouldn't hold to them. In this sense, the theory wars strongly resemble arguments in theology or literary criticism, a fact that has not gone unnoticed. But social science isn't theology or lit crit, or at least I don't think it should be. To me, the test of a methodological position is: Does it deliver the goods? In other words, can you use the ideas to produce really compelling social science, what they call "killer apps" in Silicon Valley? If you can, I'm willing to listen. If you can't, then the sound of angels dancing on pins becomes deafening.

In the examples we'll pursue, I will employ the rational choice approach because I will ask: Why are people doing this? And the answers I come up with

typically have the form: They had a reason, that is, they were trying to get something, and this appeared to be a good way to do it. You can decide for yourself whether this approach seems methodologically suspect in these cases.

Exercises

1. *American Politics.* "The Democrats will win elections if they stand for something. They should forget moderates and concentrate on getting out their base." "The Republicans have lost their way because they have abandoned their conservative principles." Perhaps you have heard one or both of these claims. Design a research strategy that would provide hard quantitative evidence on whether the claims are true or false.
2. *American Politics.* In his book *What's the Matter with Kansas?* (2004), journalist Thomas Frank claims that the Republican Party has forged a dominant political coalition by convincing lower-middle-class Americans to vote against their own economic interests in an effort to defend traditional cultural values against radical bicoastal elites. Have lower-middle-class whites abandoned the Democratic Party due to a conflict over values? Is this argument correct or incorrect? How could you tell? Describe the data and the statistical methods you would need to evaluate the argument. Hint: After you have thought about this for a while, look at Bartels (2006) and the accompanying discussion in the *Quarterly Journal of Political Science.*
3. *Comparative Politics.* Do good political institutions cause prosperity or does prosperity bring good political institutions? Consider this question from the perspective of instrumental variables. You may need to reread this section in your statistics textbook. After you have thought about it for a while, read Acemoglu, Johnson, and Robinson (2002). How does their article alter your thinking?
4. *International Relations.* Does peace cause democracy or does democracy cause peace? Again, discuss this question from an instrumental variable's perspective and in light of Acemoglu et al. (2002).