In this chapter, we look more closely at the nature of political theories and at the factors that influence the decision to do research on a particular theory. Along the way, I will discuss some standards to use in deciding whether a theory is weak or strong.

Although this chapter deals with political theories, you should not assume that it is important only for what I have called theory-oriented research. Indeed, as I pointed out in Chapter 1, the key to solving many engineering problems may be a political theory of some sort. To effect a change in some given phenomenon, you may need to develop a theory that accounts for several factors and allows you to manipulate them to produce the desired change. Much applied research on the problem of enriching the education of underprivileged children, for example, has had to concern itself with developing theories to explain why one child learns things more quickly than another. The Stouffer study, cited in Chapter 1, is another example of an engineering study in which it was necessary to develop a theory. In that case, Stouffer and his collaborators had to explain why MPs had higher morale than air corpsmen. This was necessary if they were to devise ways to raise the morale of Army personnel in general.

On the other hand, many engineering studies do not require that a theory be developed; they simply involve measuring things that need to be measured. The U.S. census is one example of such engineering research. Others include the Gallup Poll, studies measuring the malapportionment of state legislatures, and comparisons of the relative military strength of various countries.

In sum, engineering research may or may not involve the development of political theories; theory-oriented research always does. Theory is a tool in one type of research; it is an end in itself in the other. But no matter which type of research one is currently engaged in, it is worth taking a closer look at the nature of theory.
CAUSALITY AND POLITICAL THEORY

In the social sciences, theories are generally stated in a causal mode: “If X happens, then Y will follow as a result.” The examples we looked at in Chapter 1 were all of this form. In the Duverger example, if a certain configuration of political conflicts exists, and if the country adopts a certain electoral law, then the number of political parties in the country can be expected to grow or shrink to a certain number. In the Craw study, the author tested a theory that if a city has high exit costs, then it might be expected to offer greater social services than other cities.

A causal theory always includes some phenomenon that is to be explained or accounted for. This is the dependent variable. In Duverger’s theory, the dependent variable was the number of parties. A causal theory also includes one or more factors that are thought to affect the dependent variable. These are called the independent variables. Duverger used two independent variables in his theory: the nature of social conflicts in a country, and the country’s electoral system.

All of these factors are called variables because it is the variation of each that makes it of interest to us. If party systems had not varied—that is, if each country had had exactly the same number of parties—there would have been nothing for Duverger to explain. If one or the other of his independent variables had not varied, that factor would have been useless in explaining the dependent variable. For instance, if all countries had the same electoral system, the variations in party systems that puzzled him could not have been due to differences in the countries’ electoral systems, inasmuch as there were no differences.

The dependent variable is so named because in terms of the particular theory used it is thought to be the result of other factors (the independent variables). The shape it takes “depends” on the configuration of the other factors. Similarly, the independent variables are thus designated because in terms of the particular theory, they are not taken as determined by any other factor used in this particular theory.

A variable may be an independent variable in one theory and a dependent variable in another. For instance, one theory might use the social status of a person’s father (the independent variable) to explain the person’s social status (the dependent variable). Another theory might use the person’s social status as an independent variable to explain the way the person votes.

Thus, no variable is innately either independent or dependent. Independence and dependence are the two roles a variable may play in a causal theory, and it is not something about the variable itself. It all depends on the theory:

Theory 1: Democracies do not tend to initiate wars.
Theory 2: Countries with high per capita incomes are more likely to be democracies than poor countries are.

In Theory 1, democracy functions as an independent variable; the tendency to wage war depends on whether or not a country is a democracy. In Theory 2, democracy functions as a dependent variable; whether or not a country is likely to be a democracy depends on its per capita income.
Chapter 2 Political Theories and Research Topics

WHAT DOES GOOD THEORY LOOK LIKE?

Three things are important if we are to develop good, effective theories:

1. **Simplicity.** A theory should give us as simple a handle on the universe as possible. It should use no more than a few independent variables. It would not be very useful to develop a theory that used thirty variables, in intricate combinations, to explain why people vote the way they do. Such a theory would be about as chaotic and as difficult to absorb as the reality it sought to simplify.

2. **Predictive accuracy.** A theory should make accurate predictions. It does not help to have a simple, broad theory which gives predictions that are not much better than one could get by guessing.

3. **Importance.** A theory should be important. However, what makes a theory important is different in engineering research than in theory-oriented research, so we shall consider them separately.

In engineering research, a theory should address a problem that is currently pressing. This is a subjective judgment, of course, but before you begin your research, you should try to justify your choice of topic, not only to yourself but also to your audience. Your research report should include some discussion of the importance of the problem and of possible applications for your findings. It may seem unnecessary to point this out, but it is an important part of the engineering research project, one that is often carried out sloppily and in an incomplete way. A paper that concludes with the obvious applications of a study might often have been richer with a little additional work by the author. The obvious applications may be obvious, but an imaginative researcher who sits down and thinks about it for awhile may be able to point up additional, more varied ways in which the results can be used.

In theory-oriented research, the theory should give a handle on as big a portion of the universe as possible; that is, it should apply broadly and generally. It is easy to develop a trivial theory. A theory of the organization of borough presidencies in New York City, for example, might predict quite accurately for that specific situation. But inasmuch as the borough presidents have little power, it would not help us very much to reduce the chaos of New York City politics, let alone the chaos of politics in general.

When we say that a theory should apply “broadly” and “generally,” we are referring not only to how large a selection of items from reality the theory deals with but also to how many preexisting theories are affected by the new theory. A theory can attain great generality rather economically if it helps to recast older theories, each of which involves its own portion of reality. Thus, a theory of electoral change might take on importance partly from the phenomena it explained directly—changes in people’s votes; but it would be a more valuable tool if it could be shown to have significant implications for other areas of social theory—democratic theory, general theories of attitude change, or whatever. In effect, it would perform two simplifying functions: It would not only give us a handle on the rather limited portion of our environment that it sought to explain directly, but it would also shed light on the wider universe dealt with by the other theories.
In the example just cited, a theory to explain the organization of borough presidencies in New York City, the theory accrues so little importance directly as to look absurd. But it might be possible, if the borough presidencies were taken as examples of some broader concept in urban politics, then the study would borrow importance from this underlying phenomenon. The borough presidencies might, for example, serve as a useful microcosm for studying the workings of grassroots organizing.

If a theory can succeed reasonably well at meeting these three criteria—importance, simplicity, and predictive accuracy—it will be useful as a tool for simplifying reality. Such a theory is sometimes described as **elegant**. One difficulty in creating an elegant theory is that trying to meet any one of the three basic criteria tends to make it harder to meet the other two. In the example of Duverger’s theory, we saw that he might have improved the accuracy of his theory’s predictions by bringing in additional explanatory variables; but this would have reduced the simplicity of the theory. Similarly, an attempt to make a theory more general will often cost us something in either the simplicity of the theory or the accuracy of its predictions.

Aside from its utility and simplicity, there is also an element of “beautiful surprise” to elegant research. A piece of research that goes against our expectations, that makes us rethink our world, gives us a special kind of pleasure. Political scientists often jokingly refer to this element as the “interocular subjectivity test” of research—does it hit us between the eyes?

A good example of research with beautiful surprise is a study of the impact of “get-tough” policies against illegal immigration across the United States–Mexican border. In the 1990s, the U.S. Immigration and Naturalization Service added extra guards and imposed punishments on employers found to be hiring illegal immigrants. Subsequently, a thousand extra border patrol officers were added each year for several years. Douglas S. Massey (2005) found that since the border crossing had been made tougher, illegal immigrants who originally would have come to the United States for only a few months of seasonal labor now stayed permanently because they knew it would be hard to get back into the United States if they went home to Mexico. The end result was that the number of illegal immigrants in the United States was increased, not decreased, by the stepped-up enforcement.

It appears to be particularly hard to achieve elegant research in the social sciences, compared with other scientific areas. Human behavior is more complex than the behavior of physical objects—in fact, some think it may be largely beyond explanation. On the other hand, it may be that human behavior can be understood, but that we have not yet come up with a social theory that could show the true potential of our field. At any rate, it is rare for theory in the social sciences to achieve elegance. If a theory’s predictions are reasonably accurate, it is usually because the scope of the theory is restricted or because many of the exceptions to the theory have been absorbed into it as additional variables, making it very complex.¹

¹The choice of this word typifies the aesthetic pleasure—and sometimes, the vanity—with which researchers approach their work.

²Another reason for the difficulty of attaining elegance in social research is simply that most social science terms are ambiguous. This problem is addressed in Chapter 3.
The fact that most social science theory is not very elegant does not mean that it is not good. The real test of a theory’s value is whether its subject matter is important and how close it has come to elegance, given that subject matter. If it is important to understand humankind’s behavior, it is important to try to develop theories about it, even if things do not fall as neatly into place as we would like.

I am always amused when people say of a question that is being made to look more difficult than it really is: “This shouldn’t be that hard; what the heck, it’s not rocket science”—implying that rocket science is the essence of difficulty and complexity. Not to take away from the difficulty of rocket science, but plotting the trajectory of an object in a vacuum is far simpler than understanding the motivation of a human being. Perhaps one day the old saw will become, “This shouldn’t be that hard; what the heck, it’s not political science.”

**Example of Elegant Research: Philip Converse**

In his article “Of Time and Partisan Stability” (1969), Philip Converse came about as close to developing an “elegant” theory as one can commonly do in the social sciences. His study is worth looking at in some detail.

Converse took as his dependent variable the strength of the “party identification” of individuals—their sense that they are supporters of one or another of the political parties. In an earlier study, he had found that whereas about 75 percent of Americans who were polled identified with some political party, a similar poll conducted in France showed that fewer than 45 percent of the respondents did so (Converse and Dupeux, 1962). Other studies had shown high levels of party identification in Britain and Norway and lower levels of party identification in Germany and Italy. Because the overall extent to which citizens of a particular country felt bound to the existing parties seemed likely to have something to do with how stable politics in that country would be, Converse wanted to know why the level of party identification varied as it did from country to country.

At the time of their earlier study, he and Dupeux had found that the difference in percentage of party identifiers between France and the United States seemed to be explained almost wholly by the fact that more Americans than French had some idea of what party their fathers had identified with. As we can see in Table 2-1, within each row there was practically no difference between the French and American levels of party identification. In both countries, about 50 percent of those who did not know their father’s party expressed identification with some party themselves. About 80 percent of those who did know their father’s party expressed identification with some party themselves.

At the time, Converse and Dupeux accepted this as an interesting finding and did not elaborate on it. But in “Of Time and Partisan Stability,” Converse used the

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3Remember that this was an early study, done in 1962. It was not long before further work showed similar effects for mothers!
earlier finding to suggest a general theory of the process by which countries developed stable patterns of party preference.

In doing so, he brought two strands of theory together. First, he reasoned that the difference between France and the United States could be explained easily if the previous generation in France had indeed included very few voters who identified with a party. It could have been, of course, that the difference was due to the fact that the French did not talk to their children about politics as much as the Americans did. But for the purposes of argument, Converse chose to assume that this was not the case. He then showed that if his assumption about the previous generation’s low level of party identification were true, one could expect the next generation in France to be much more like the Americans. Also, if the assumption were true, France must be moving toward the level of party identification found in the United States, Britain, and Norway. (This development can be seen in the box, “Markov Chains,” on p. 20.)

Converse further reasoned that the 80 percent and 50 percent figures might be universally true. (He knew only that they held for France and the United States.) If this were so, then both France and the United States might simply be examples of a general process that all countries undergo when their citizens are first given the vote. In the first election, scarcely any voters in a given country would identify with a party, but 50 percent of second-generation voters would express identification. (This is so because of those second-generation voters whose fathers had not identified with any party, 50 percent developed an identification of their own.) Thus, gradually party adherence would reach a stable level. According to this scheme, the relatively low level of party identification in France must have resulted because the vote was extended later and less completely there than in America. (French women, for one thing, were first given the vote in 1945.) Thus, France must be at an earlier stage of the process than America.

The second strand of theory came into play when Converse tied his theory of national development to some older findings on individual voters in the American electorate. Voting studies commonly had shown that across an individual’s life span, the older he was the more likely he was to identify strongly with a party. Moreover, this had been shown to be a result of how long he had been exposed to the party by being able to vote for it, rather than of his age itself (see, e.g., Campbell et al., 1960, pp. 161–164).

Working from these two angles, Converse developed a simple theory that predicts the strength of a voter’s party identification from just two factors: (1) the number of years the person has been eligible to vote (which is a dual function of

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<th>Table 2-1 Percentage Having Same Sort of Party Identification</th>
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age and the length of time elections have been held in his/her country) and (2) the likelihood that the individual’s father had identified with a party (which in turn depends on what portion of the father’s adult life elections were held in which he was eligible to vote). The first of these factors derived from the earlier research on individual development and the second from the comparative study of France and the United States by Converse and Dupeux. Thus, essentially, party identification could be predicted from the individual’s age and the length of time that the country has been holding elections.

A few examples of predictions from his theory are as follows: (1) at the time elections are first held in a country, the pattern we typically observe in Europe and America (the young being weakly identified, the old strongly) should not hold; all should identify at the same low levels; (2) if elections were interrupted in a country

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**MARKOV CHAINS**

Converse’s reasoning is based on some cute, simple mathematics that you can play with for yourself. If the rates of transferring identifications are in fact the same in two countries, then even though the countries differ greatly in the level of identification at present, we would expect them to converge rapidly. For example, Converse and Dupeux estimated for France and the United States that about 80 percent of those whose fathers had identified with a party developed an identification of their own, and that, of those whose fathers had not identified with a party, about 50 percent developed an identification of their own. Given these figures, and assuming that party identifiers have the same number of children as nonidentifiers, if 30 percent of the population of country A presently identify with a party, and 90 percent of the population of country B presently identify with a party, in the next generation we would expect to see

$$(0.8 \times 30\%) + (0.5 \times 70\%) = 59\%$$

of country A having an identification, and

$$(0.8 \times 90\%) + (0.5 \times 10\%) = 77\%$$

of country B having an identification. In the next generation after that, we would expect to see

$$(0.8 \times 59\%) + (0.5 \times 41\%) = 67.7\%$$

of country A having an identification, and

$$(0.8 \times 77\%) + (0.5 \times 23\%) = 73.1\%$$

of country B having an identification. Thus, in two generations the two countries, which had started out being quite different, would have moved to similar levels of party identification. The process involved here, called a “Markov chain,” is described in J. Kemeny, J. Snell, and G. Thompson, *Introduction to Finite Mathematics* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1957), pp. 171–178.
What Does Good Theory Look Like?

(as in Germany from 1933 to 1945), levels of party identification should decline at a predictable rate; (3) if the transition rates for all countries were roughly the same as for France and the United States, then party identification levels in all electoral democracies should converge over the next few generations toward a single value of about 72 percent.

Thus, although Converse’s theory was quite simple, it was applicable to a wide variety of questions. It simultaneously explained individual behavior and characteristics of political systems. It implied a more or less universal form of political development at the mass level—with a prediction of initial, but rapidly decreasing, potential for electoral instability in a new electorate. And it included the startling suggestion of a convergence of “mature” electorates to a common level of party identification approximately equal to that of Britain, Norway, or the United States.

The theory was simple, and it was broadly applicable. What was more, it seemed to predict fairly accurately, thus fulfilling the third criterion for “elegance.” Using data from Britain, Germany, Italy, the United States, and Mexico to test the theory, Converse found that the theory predicted quite well for all five countries.

Over the years after it appeared, the Converse article stimulated a great deal of further research, which is what one would expect of elegant work. His findings served as assumptions for further theoretic work (Przeworski, 1975). They also stimulated researchers to investigate whether in fact the transition probabilities on which the Markov chain is based are the same in all industrialized countries (Butler and Stokes, 1969, p. 53), and to test whether new electorates actually behave as Converse’s theory predicts they would (Shively, 1972; Leitner, 1997; Tilley, 2003; Dalton and Weldon, 2007). It is in this way that a good piece of theoretical work feeds, and becomes enmeshed in, the whole body of theoretical exploration.

To Quantify or Not

A side issue in the question of how to develop elegant theory is the old chestnut: Should political science be “quantitative” or not? There has been much rhetoric spilled over this.

It is a bit hard to pin down what the term quantitative means, but, generally, research that pays a good deal of attention to numerical measures of things, and tends to make mathematical statements about them, is considered quantitative. Research that is less concerned with measuring things numerically, and tends to make verbal statements about them, is considered relatively less quantitative.

Anything in political science can be stated with varying degrees of quantification. To give a crude example: “The length of service of members of the U.S. House increased from 1880 to 2012 on the average by 0.78 years every decade; the rate of increase was 0.68 years per decade before 1922, and 0.86 years per decade after 1922,” says approximately the same thing as “From 1880 to 2012, representatives served a steadily increasing length of time in the House; the change proceeded a bit more rapidly in the latter half of that period.” The first form of the statement gives more precise information, but the sense of the two statements is the same.
Each approach involves costs and benefits for research. Most people would agree that precise information is more useful than imprecise information, all other things being equal. But it may be that the time and attention spent in gathering precise data make it difficult for the researcher to appreciate the larger aspects of a theory. Also, because some kinds of data by their very nature are more amenable to precise formulation, there is the danger that overconcern with precision may restrict our choice of research to those variables we can more easily quantify. It is striking, for instance, how little the U.S. presidency is studied by political scientists. This oversight may well be due to the difficulty of getting “hard” data on what goes on in that office.

I deal with the issue of proper levels of precision in Chapter 5. For our purposes here, the important thing is to see what relationship degrees of quantification bear to elegance in research.

First of all, the particular subject we are studying affects the extent to which it is possible for us to quantify. In election studies, there is considerable scope for quantification. Records from earlier elections are usually kept in fairly good order; the results of many attitude surveys are also available, and most voters do not regard their actions as something about which they need to maintain secrecy. Thus, the quantitative researcher is able to do a great deal. On the other hand, in Chinese studies, or in studies dealing with the U.S. presidency, sources of quantitative data are quite restricted, and most research must be relatively nonquantitative.

In virtually every field of political research, however, work can be conducted in either a primarily quantitative or a primarily nonquantitative mode. It is probably best that studies with varying degrees of quantification be carried on simultaneously in any given field of political research, for the different levels of quantification complement each other. Typically, less quantitative research provides greater breadth, greater openness to totally new theories, and a greater awareness of the complexity of social phenomena. On the other hand, studies employing more quantitative data are more likely to produce simple, usable theories; and they are certainly more likely to give us a clear idea of how accurate a theory’s predictions are. Thus, each approach has its own costs and benefits, and it is well to remember that no particular degree of quantification has a corner on elegance.

CHOICE OF A TOPIC

The choice of a research topic is intimately bound up with the elegance of what comes out of the research effort. In selecting a topic, of course, the first step is to choose a general area that is interesting and significant for you. By choosing to study political science, you have already begun to narrow the field, and you certainly will narrow things more before you are ready to begin. There is no difficulty in this; you simply follow your interests.

But once you have chosen a general area to work in, picking a particular topic to research is difficult. This is the critical decision in doing research. It is also the most difficult aspect of research to teach anyone. It is at this step—seeing that a problem
exists and that there is a good chance you can provide new insight into it—that originality and talent are most critical.

The important thing in choosing a topic is to pick one that shows promise of giving you new and elegant results. This implies two things: (1) You want to formulate your topic question so that your results will be likely to alter existing opinion on a subject, and (2) you want your results, as much as possible, to attain the three criteria for elegance: simplicity, predictive accuracy, and importance.

**Engineering Research**

Choosing a topic is somewhat simpler in engineering research than it is in theory-oriented research. Here, it is primarily a question of using your time and talents efficiently. To yield elegant results, the topic should be one that deals with a pressing problem and one on which you think you are likely to come up with findings that are both accurate and simple enough to be useful. At the same time, you will want to state your thesis so that your results will not duplicate an earlier study, or at least point up where that work produced mistaken results. There is no sense in wasting your time running over ground that has already been worked unless you think you are likely to discover discrepancies.

One difficulty in choosing the topic is that you probably will have to compromise among your goals. You may decide that for the problem nearest your heart, there simply is not enough material available to let you study it satisfactorily. Many topics relating to defense or to the executive are of this sort. Or, it may be that a topic interests you not because it deals with the most pressing problem you can think of, but because you have seen some research on it that you think would be rather easy to correct.

The main thing to do in looking for a topic is to read. You should read so that you are certain you are picking an important problem, and you should read to find out how likely it is that your topic will yield useful results. Finally, you should read to see what other work has been done on the problem, or on similar problems, so that you will see where you are most likely to produce results that are new.

**Theory-Oriented Research**

Choosing a topic that will produce important results for theory is more difficult than formulating a question that may yield important practical applications. You will recall that if theory-oriented research is to be important, it should have a broad and general effect on theory. This effect can be achieved either directly through the phenomena it explains or indirectly through the variety of other theories it affects. Similarly, to be “new,” the research results must either produce totally new theories or lead to some change in the status of older theories.

This means that in framing any topic for research, you are involved at once in the full body of political science theory, for a single piece of research may simultaneously affect many different theories. Research on how a congressional committee reaches its decisions, for example, can affect theories about power in Congress,
general theories about committees and organizations, theories about executive—
congressional relations, or theories about elite political behavior.

The researcher in this area must decide which research topic is going to produce
the greatest change in the status of existing theories. This task requires not only that
she be familiar with as broad a range of existing theories as possible, but that she also
have some idea of where an existing body of research is weakest and most needs to
be supported or changed.

Deciding where you are likely to produce theoretical results that are simple
and predict accurately requires the same sort of guessing as in engineering research,
but in theory-oriented research it is harder to decide how important the results of
a study are likely to be. You must juggle all of these decisions around so as to get the
best mix—a topic that will produce results that are as new and as elegant as possible.
This is not something for which rules can be laid down—it is an art.

DEVELOPMENT OF A RESEARCH DESIGN

It may be true, as I say, that choosing a topic is not something for which rules can
be laid down. But it is certainly something for which rules have been laid down.
Because of an exaggerated fear of ex post facto argument, some social scientists have
developed a very restrictive procedure to serve as a standard in carrying on research. 4
According to this procedure, the researcher should first frame a theory, stating it
in the form of a set of hypotheses to be tested. These hypotheses presumably are
based on work others have done in the past. The researcher should then gather fresh
data with which to test the theory. Finally, having tested the theory, the researcher
should either reject it or enshrine it solely on the basis of those new data. It is true that
this procedure erects formidable barriers to protect us from ex post facto argument,
but it has a number of serious drawbacks.

In the first place, it lends an exaggerated significance to the results of the new
study. Even in cases where a variety of previously existing evidence favors a particu-
lar theory, that evidence presumably is to be ignored if the new test gives contradic-
tory results. Second and more important, the usual procedure deters researchers from
casting about creatively for research topics and theories. Because it requires that
hypotheses be fixed firmly at the beginning of the research process, it effectively
reduces the research task to a selection of obvious hypotheses. It offers researchers no
encouragement to think about their theories once research has begun. Researchers
are not supposed to remold theory as they go along, learning more about the subject.
They are merely supposed to react to old theories and concepts rather than to think

4 Ex post facto argument results when an investigator forms a theory on the basis of certain evidence, and
then uses that evidence to affirm the theory. If a political scientist formed a theory of congressional commit-
tees on the basis of intimate experience with the House Appropriations Committee, for example, and then
carried out a study of the House Appropriations Committee to test the theory, this would be ex post facto
argument. The danger in this is that any given situation has certain unique aspects, and these are likely to be
included in any theory based on it. If the same situation is then used to test the theory, it will look as if the
unique aspects are indeed general, whereas if a different test situation had been used, those parts of the theory
might have been found wanting.
up entirely new problems for explanation. In short, this approach encourages the researcher to function as a clerk.

The epitome of this type of thinking is the research design—a common student exercise in which students are instructed to frame some hypotheses (presumably based on their reading) and show how they might gather data to test those hypotheses. A doctoral candidate whom I once talked with seemed to me the perfect example of repeated exposure to exercises such as these. He needed to find a topic for his dissertation and he thought that a good way to do this would be to look through a book laying out theories of elections, pick a few propositions about voting behavior, and test them with some data.

This is how we train people to do research, but most of us have better sense than to follow our own precepts. A search of articles in political science journals will turn up only a few that report research that follows the rules. One of the better-kept secrets in political science is that good political scientists generally do not draw up research designs before they start to work on a topic. Nor do they usually “frame hypotheses” in any formal sense before they start to work, although they usually have some operational hunches about what they expect to find. And they most certainly do not ignore older evidence, even the evidence that suggested a theory to them in the first place.

Their procedure is less formal than the rules prescribe. They play with data, immerse themselves in what other people have written, argue with colleagues, and think. In doing so, they grope for interesting theories, theories that are elegant and give a new slant to things.

Although I have condemned the formal procedure for designing research, I hasten to add that it should not be rejected completely. One of its advantages is safeguarding against ex post facto argument. Furthermore, even though the research design undoubtedly stifles initiative and creativity, it is more methodical and easier to apply for the beginning researcher. Because students usually operate under stricter deadlines than other researchers, it may make sense for them to work with specific goals in mind so that they can estimate accurately at the beginning of a project when it will be completed. Also, it is hard to teach someone to grope for interesting topics and theories. Perhaps a good way to learn is by starting with the more clear and obvious procedures, then gradually loosening up as experience is gained.

OBSERVATIONS, PUZZLES, AND THE CONSTRUCTION OF THEORIES

One way to look at choosing topics and developing theories is to realize that they are at heart very commonsensical processes, based on our daily experiences. This is often lost in the forest of scholarship, in which scholars frequently deal with abstractions (and with each other’s rival abstractions). But at heart all theory-oriented research in the social sciences is of the following sort:

1. Something in our lives puzzles us, and we try to think of an explanation to account for it.
2. To account for it, we put it in a broader, general category of causal relationships (i.e., a theory).

3. To test whether the broader theory is valid as an explanation, we draw other specific predictions from the theory and test these to see whether the theory’s predictions are generally true. If they are, the theory qualifies as a plausible explanation of the thing we are trying to explain.

As an example, consider the puzzle that the United States has always contributed proportionally more than almost all other members of the NATO military alliance, especially as compared with the smaller members of the alliance, as seen in Table 2-2.

The small allies all appear to varying degrees to ride on the coattails of the United States. One way to explain this would be to treat it as a specific instance of a more general relationship—that in any voluntary cooperative group the member with the greatest resources always tends to make disproportionate contributions. That is, a member who sees that the group would fail without her contribution will come through strongly; a relatively insignificant member will see that the group would do about equally well whether or not she contributes and will tend to sit back and be a free rider. In the NATO example, if the United States does not contribute vigorously, the alliance languishes, but little Estonia hardly makes a difference one way or another.

A lot of the political scientist’s creativity will then come into play in devising other, testable predictions from the theory to see whether it is generally valid. In this example, we might examine chambers of commerce to see whether the biggest merchants in town usually carry most of the freight. Or we might look at trade union–supported political parties such as the Labour Party of Great Britain to see whether the largest unions carry a disproportionate share of the burden. If the theory holds up well across a variety of such tests, it will be a plausible potential

### Table 2-2 U.S. Defense Spending, Compared with the Seven Smallest Members of NATO

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<thead>
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<th></th>
<th>Gross National Product (billions of $)</th>
<th>Defense Spending as Percentage of Gross National Product</th>
</tr>
</thead>
<tbody>
<tr>
<td>United States</td>
<td>14,700</td>
<td>4.9</td>
</tr>
<tr>
<td>Luxembourg</td>
<td>53</td>
<td>1.0</td>
</tr>
<tr>
<td>Slovenia</td>
<td>47</td>
<td>1.4</td>
</tr>
<tr>
<td>Lithuania</td>
<td>36</td>
<td>0.9</td>
</tr>
<tr>
<td>Latvia</td>
<td>24</td>
<td>1.1</td>
</tr>
<tr>
<td>Estonia</td>
<td>19</td>
<td>1.8</td>
</tr>
<tr>
<td>Iceland</td>
<td>12</td>
<td>0.3</td>
</tr>
<tr>
<td>Albania</td>
<td>12</td>
<td>1.7</td>
</tr>
</tbody>
</table>

explanation for the disparity in spending in NATO—and it will also have yielded us an interesting theory, with broad applications.3

Lest working out a theory or a puzzle should seem too easy or too pat from these examples, let me review for you a puzzle currently in real play, to show the uncertainty and the progression of steps by which scientific discussion of a puzzle usually proceeds in real life. A number of scholars have tried to understand the implications of the rise of economic inequality in the United States since the 1970s, despite the democratic structure of American politics. In theory, a democracy should lead to greater equality of incomes, since everyone has exactly one vote; the large number of citizens with low incomes should greatly outnumber the small number with high incomes and push for government policies that redistribute income in their direction. Larry M. Bartels (2008) and Martin Gilens (2005) have argued that unequal political power allows those with more money to dominate politics despite the theoretical equality of voters and that increasing political inequality since the 1970s has been the source of the increase in economic inequality. Both find that the rich and the poor disagree on public policies, but that policies tend to fit the preferences of the rich rather than the poor.

However, Stuart Soroka and Christopher Wlezien (2008) argue that American democracy is equally responsive to both those with high incomes and those with low incomes, because both groups tend to move in the same direction in their views about policy, and government policies respond equally to moves in the opinion of the two groups. In other words, American democracy responds equally to the two groups, because they do not really differ from each other in how they shift over time.

The two sets of findings present a contradiction. Nathan J. Kelly and Peter K. Enns (2010) attempt to resolve the contradiction by testing a theory that as inequality increases a greater number of people benefit from the inequality, and they have more to lose; therefore increased inequality leads to reduced overall support for redistributive government policies. The puzzle of increasing economic inequality in the United States’ democratic system could thus be accounted for by the fact that an increase in economic inequality leads to decreased overall support for governmental redistribution. Under this theory, increases in inequality that originate from something other than the government’s policies (such as a change in trade patterns, or changed technology like the development of information technology that can replace workers) will change the political dynamic so that the inequality is self-reinforcing until some other “outside” change comes; a rise in inequality makes the government less inclined to make incomes more equal, which in turn raises inequality further, which makes the government still less inclined, and so on.

In their test of this theory, however, they find yet another puzzle. As predicted by the theory, those with high incomes become less supportive of redistribution as economic inequality increases. But those with low incomes also become less

3The theory used in this example derives from the broader theoretical structure of Mancur Olson’s The Logic of Collective Action (1965), which was discussed in pp. 7–8. The structure of argument discussed in this section—see a puzzle, frame an explanation based on a more general principle, and devise other unrelated predictions from the general principle in order to test it—is presented skillfully by Charles A. Lave and James G. March in Introduction to Models in the Social Sciences (1975), especially in the first three chapters.
supportive of redistribution at the same time, which is not predicted by the theory and makes no sense.

At this point the overall puzzle remains wide open, posing several questions: Are the different results of Bartels and Gilens on the one hand, and Wlezien, Soroka, Kelly, and Enns on the other, due to differences in their research techniques? (Bartels and Gilens look at snapshots of the difference between the opinions of the rich and the poor at single points in time, whereas the others relate changes in policy over time to changes in the opinions of rich and poor.) And if Kelly and Enns have it right, then why is it that the poor move so unexpectedly to the right as inequality increases? As always in developing scientific puzzles, stay tuned.

Note that throughout this discussion of puzzles and explanations, I have said that what is produced is a plausible potential explanation. It typically is the case that more than one plausible potential explanation can be produced for anything needing explanation. It then becomes the task of the scholar to decide among them on the basis of how broadly they apply, how simple they are (remember, a theory that is as complicated as the reality it is meant to explain has not gotten you very far), how accurately they predict, and so on. But the basic building blocks of political explanation are plausible potential explanations, derived in just the way that I have outlined here.

MACHIAVELLIAN GUIDE TO DEVELOPING RESEARCH TOPICS

There are really no guidelines that I can give you for developing a research topic other than to remind you once again that you are working toward results that are both new and elegant. Perhaps if we view the development task from the perspective of political research in general, however, we will gain some clue as to its place in the entire scheme.

Implicit in this chapter is the view that scholarly research represents a loose cooperative effort among many people. I mentioned earlier the pleasure that researchers feel in creating something that no one has seen before. This is mixed, however, with a sense of pride in being part of an ongoing tradition. One’s work is something brand new, but it also draws on Karl Marx, or Emile Durkheim, or Robert Putnam, and modifies the meaning of their work. Scholars involved in developing theory form a kind of priesthood—admittedly sometimes run less on faith and more according to the laws of laissez-faire and caveat emptor—focused on the common goal of perfecting elegant theories. As we have seen, the celebrants carry on this process by developing new theories and adapting old ones, fitting these theories to the real world to see how accurately they predict things, and feeding the results of such research back into the body of theory.⁶

⁶Needless to say, it is not quite as neat as this. For one thing, a given person usually does not handle all these aspects of a particular problem. One person may work at clarifying theories, another may do a descriptive study of a particular case, and a third may relate the new evidence to the body of older theory.
From this description of the process of research, we can derive some principles to guide the individual researcher. I have titled this section a “Machiavellian” guide to signal that the ideas here are meant to show how a cunning person might operate in order to do research that both helped the researcher (think an A on a paper, or tenure for a professor), while also serving the greater good. In that sense, it references Niccolò Machiavelli, who in the Renaissance wrote *The Prince* to offer advice to rulers as to how they could maintain power and rule effectively.

If empirical research is motivated by a desire to affect the state of theories, either by confirming them or by working changes in them, you will be doing your best job when you maximize your effect on theory with a given investment of time and money. To do this, you should:

1. **Maximize the generality of the theory you intend to examine.** This is basically a restatement of the first criterion for elegant research. Note, though, that this rule is not something absolute, for any phenomenon can be examined at different levels of generality. One person may be hit on the head by an apple and form a theory of falling apples; another may have the same experience and form a theory of universal gravitation. The physical activity of the “study” is the same in both cases; the difference lies solely in the level at which the researcher works.

   As an example from political science research, consider the variety of studies done on the presidency. The narrowest range of theory is found in biographies of particular presidents. The researcher in such a biography generally is concerned only with explaining what happened during a particular president’s life, especially during his term in office. A broader range of theory is aimed at in studies of the U.S. presidency, which may analyze the nature of the office, the sources of executive power, the way in which presidents’ personalities can influence their behavior in office, and so on.  

2. **Pick a weak theory to work on.** The weaker the previous confirmations of a theory have been, the greater your contribution will be. Of course, you have a greater probability of refuting a weak theory. But also, if your research does confirm the theory, your work will again be more significant than if the theory already had a good deal of confirming evidence.

   Obviously the best way to use the strategy of picking a weak theory would be to state a new, original theory yourself. In this case, your hypotheses are necessarily in need of proof, and any evidence you can buttress them with will be important. Remember, though, that “new, original theories” that are also elegant are hard to come up with.

   Another way to follow this strategy is to pick an anomaly—that is, a question on which previous research has been contradictory. For instance, Jennifer L. Lawless and Kathryn Pearson (2008) noted the anomaly that many studies have shown that women win elections at the same rate as their male counterparts, but that nonetheless there are fewer women in elected office than we would expect from their proportion in the population. If there is no electoral handicap for

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7 Two good examples are James J. Best’s study of how Lyndon Johnson gathered information and opinions on policy (Best, 1988) and Fred Greenstein’s analysis of presidents’ leadership (Greenstein, 2000).
women, why then are so few elected? Lawless and Pearson looked further into elections and found that part of the explanation for the paradox is that though women do as well as equally qualified men in the general election (as the earlier research had shown), they face special problems in the primary election that precedes the general election. Women running in primary elections are challenged more often than men who run in primaries and therefore lose primaries at a greater rate than men. As all earlier studies had shown, the electoral process is gender-neutral at the general election level. But at the primary election level, women must be better than men if they are to prevail in the more frequent challenges they face.

A current anomaly, on which political scientists are now working, is the "paradox of participation." In general, the more highly educated people are, the more likely they are to vote. (In the 2008 presidential election, for instance, 79 percent of college graduates voted, 55 percent of high school graduates did so, and only 39 percent of those with less than a high school education went to the polls.) So one would expect that as the citizens of a country became better educated, voter turnout would rise. But while the American population became dramatically better educated after World War II, participation in elections stagnated. In 1940, for instance, 76 percent of Americans had less than a high school education, a number that had dropped to 14 percent by 2007; but in 1940, 59 percent of eligible adults voted in the presidential election, a figure only slightly below the 64 percent turnout in the 2008 election.

Another example of a nice anomaly just waiting to be analyzed is the fact that in the 2008 election, the lower voters' incomes were, the more likely they were to vote for Barack Obama. But if you look at the "blue states"—the states he carried—they are on the whole well-off states with high average incomes (California, Connecticut, Vermont, New York, etc.). What sort of theory might you devise to explain this?

Anomalies like these are hard to come by, because earlier investigators generally have noticed them already and have tried to resolve them. If you can find an anomaly having to do with a significant area of political theory, however, you can be certain that any plausible efforts at resolution will be interesting.

Besides anomalies, you might choose a problem you believe has just not been sufficiently researched, perhaps one in which all variables have not been covered. Thus, you might replicate a study in a different context from the original one. David Samuels (2003) tested a very basic theory of legislative behavior, that legislators' choices of what policies to pursue and how to shape their legislative careers are all caused by their desire to be reelected. The theory had first been laid out by David Mayhew (1974) in his book Congress: The Electoral Connection and had been applied in many studies since then; it was based on the U.S. Congress, and most testing of it had been done in the United States. Samuels tested the theory in Brazil and found that members of Brazil's

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8The paradox is reviewed in a study by Dalton and Klingemann (2007).
9U.S. Census Bureau, "Voting and registration in the election of November 2008."
Chamber of Deputies did not design their actions in order to help get reelected; in fact, relatively few deputies ran for reelection at all. Rather, they designed their actions in the chamber to further careers in local government, which are more prestigious and more lucrative in Brazil than in the United States. Because deputies were not motivated by reelection to the chamber, many aspects of their behavior were different than what standard legislative theory would have predicted. Among other things, the president had relatively little influence with them; rather, they were influenced greatly by their states’ governors, who could help determine their political futures at home. Samuels’ work did not negate the theoretical and empirical work that had been done in the United States, but it enriched it by testing it in a new context and showing how the theory operated when some of its basic assumptions were changed.

3. **Make the connection between the general theory and your specific operations as clear as possible.** This really just boils down to making sure you say what you think you are saying. It involves such things as the accuracy of your deductions from the theory to the specific situation, the accuracy with which you have measured things, and so on. Much of the rest of this book focuses on such problems.

   You may have noticed that these three rules resemble the criteria for elegance fairly closely. You may have also noticed that the basic philosophy behind them—“Do research that makes as big a splash as possible”—reads like a guide for ruthless and hungry assistant professors. But each of the rules, derived from the underlying Machiavellian outlook, also has a beneficial effect on the field as a whole. If individuals choose those problems of theory that have so far had the weakest verification, for example, the entire field will benefit from an examination of those theories most in need of investigation.

   Needless to say, these guidelines should remain flexible enough to allow different mixes of research strategy. There is no one “scientific method” involved here. One person may find a tool that measures a variable better than had been done before and then simply apply it to sharpen previously examined relationships. Another may note an anomaly in a theory and organize an experiment to resolve the problem. A third may look over previous research findings and place a new, broader, or simpler interpretation on them. All are following the rule of maximizing their impact on theory.

4. **Present your theory as clearly and vividly as possible.** A Machiavellian researcher wants to influence as many people as possible, so it makes sense to make your reader’s life easier and your message more compelling. This means, write well and present any graphic information well. People often think how you say something is separable from what you say, but that is simply not true. If the purpose of theory is to change people’s understanding of the world, then the way the theory is communicated to them is an integral part of the development of the theory.

   How to write well and design graphic displays well are beyond the scope of this book; each really requires a book in its own right. Fortunately, I can suggest three truly good books that will help you. For writing, I recommend

Key Terms

- anomaly 29
- dependent variable 15
- elegant research 17
- independent variable 15
- Markov chain 20
- quantitative research 21
- variable 15

Further Discussion

An excellent introduction to building elegant theories is found in the first three chapters of Lave and March (1975). Two books outside political science that are very good examples of seeing and solving puzzles (and great reads, as well) are Steven Levitt and Stephen J. Dubner, Freakonomics (2006), and Thomas Eisner, For Love of Insects (2005).

Some questions you might consider are as follows:

1. Presumably, work in normative theory or in positive theory could be evaluated in terms of elegance, just as empirical research is. What changes would this require in the definition of elegance?

2. This chapter has implied that the usual way to come up with a theory is to focus on a body of observations and look for regular patterns in them. Although this is the usual procedure, it is neither the only nor necessarily the best approach. What drawbacks might it involve? In what alternative ways might one develop a theory?

3. I stated in this chapter that most social science theories are causal. What would a noncausal theory look like? Under what circumstances would it likely be used? (Hint: Consider Einstein's famous theory, \( E = mc^2 \).)