How the Cases You Choose Affect the Answers You Get: Selection Bias in Comparative Politics
Author(s): Barbara Geddes
Source: *Political Analysis*, Vol. 2 (1990), pp. 131-150
Published by: Oxford University Press on behalf of the Society for Political Methodology
Stable URL: http://www.jstor.org/stable/23317768
Accessed: 05-02-2016 13:18 UTC

How the Cases You Choose Affect the Answers You Get: Selection Bias in Comparative Politics

Barbara Geddes

Abstract

This article demonstrates how the selection of cases for study on the basis of outcomes on the dependent variable biases conclusions. It first lays out the logic of explanation and shows how it is violated when only cases that have achieved the outcome of interest are studied. It then examines three well-known and highly regarded studies in the field of comparative politics, comparing the conclusions reached in the original work with a test of the arguments on cases selected without regard for their position on the dependent variable. In each instance, conclusions based on the uncorrelated sample differ from the original conclusions.

Comparative politics, like other subfields in political science, has norms and conventions about what constitutes an appropriate research strategy and what kind of evidence makes an argument persuasive. One of our most durable conventions is the selection of cases for study on the dependent variable.[1] That is, if we want to understand something, for example, revolution, we select one or more occurrences and subject them to scrutiny.

Most graduate students learn in the statistics courses forced upon them that selection on the dependent variable is forbidden, but few remember why, or what the implications of violating this taboo are for their own work. And so, comparativists often ignore or forget about it when carrying out or assessing nonquantitative comparative research.

I am grateful to Chris Achen, Jim Caporaso, David Collier, Jeff Frieden, Miriam Golden, Jonathan Hartlyn, Robert Jackman, and John Zaller for comments on earlier versions of this article.

1. The subfield of comparative politics is not the only one bedeviled by problems with selection bias (compare Achen and Snidal 1989).

This article will show the consequences of violating the taboo. It will do so by comparing the conclusions reached in several influential studies that selected cases on the dependent variable with conclusions reached in tests of the same arguments using samples not correlated with the outcome on the dependent variable.

All the studies discussed in this paper are intelligent, plausible, insightful, and possibly true. All have been advanced by highly respected social scientists. The effort here is not to discredit arguments or belittle authors—who are, after all, working within accepted conventions—but to demonstrate the deficiencies of the conventions.

The Nature of the Problem

The problem with selecting cases for study on the dependent variable stems from the logic of explanation. When one sets out to explain why countries *A* and *B* have, say, developed more rapidly than countries *C* through *G*, one is implicitly looking for some antecedent factors *X* through *Z* that countries *A* and *B* possess, but that countries *C* through *G* do not. The crux of the difficulty that arises when cases are selected on the dependent variable is that if one studies only countries *A* and *B*, one can collect only half the information needed, namely what *A* and *B* have in common. Unless one also studies countries *C* through *G* (or a sample of them) to make sure they lack factors *X* through *Z*, one cannot know whether or not the factors identified are crucial antecedents of the outcome under investigation. Countries *A* and *B* may be the only countries that have *X* through *Z*, in which case the hypothesis seems plausible. But many other countries may also have them, in which case the hypothesis would seem dubious.

This point can also be made graphically. Suppose a universe of developing countries *A* through *G*, where *A* and *B* are among the fastest growing. On the basis of an intensive study of *A* and *B*, one concludes that factor *X* is the cause of their success. In concluding this, one implicitly assumes that if countries *C* through *G* were examined they would turn out to have less of factor *X* than do *A* and *B*, and that one would observe the relationship shown in figure 1.

Yet, if one examines only countries *A* and *B*, it is possible that the full range of cases would look more like figure 2. That is, it is possible that there is no relationship between *X* and the rate of development. The only things that can actually be explained using a sample selected on the dependent variable are differences among the selected cases.

When one looks only at the cases above the dotted line in figures 1 and 2, two kinds of mistaken inferences can easily occur. The first, as shown in the figures, involves jumping to the conclusion that any characteristic that the

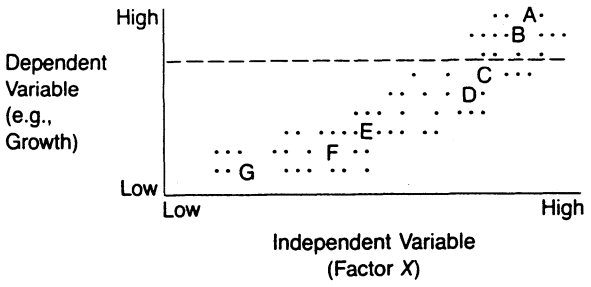

Fig. 1. Assumed relationship between factor *X* and the dependent variable

selected cases share is a cause. The other involves assuming that a relationship (or the absence of a relationship) between variables *within* the selected set of cases reflect relationships in the entire population of cases.

In the statistical literature, attention has focused on the second kind of faulty inference (Achen 1986; King 1989). If the true relationship between factor *X* and the dependent variable is that shown in figure 1, but one selects cases in a manner that results in the examination only of cases located above the broken line, statistical procedures carried out on the selected cases may indicate that no relationship exists. Thus, selection on the dependent variable often biases statistical results toward finding no relationship even when a relationship, in fact, exists.

In nonquantitative work, however, the first kind of faulty inference is at least as common as the second. This occurs when figure 2 shows the true relationship, but the analyst—based on bits and pieces of information—assumes that cases *C* through *G* are located in the lower left quadrant, and concludes that factor *X* causes the outcome of interest even though, in fact, no relationship exists. An example may make these points clearer.

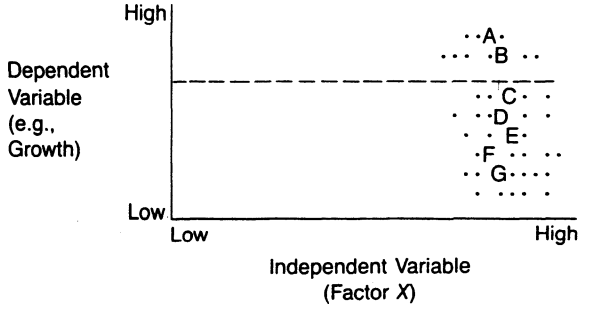

Fig. 2. An alternative possibility for the relationship between factor *X* and the dependent variable

A Straightforward Case of Selection on the Dependent Variable

Analysts trying to explain why some developing countries have grown so much more rapidly than others regularly select a few successful new industrializing countries (NICs) for study, most often Taiwan, South Korea, Singapore, Brazil, and Mexico. In all these countries, during the periods of most rapid growth, governments exerted extensive controls over labor and prevented most expressions of worker discontent. Having noted this similarity, analysts argue that the repression, cooptation, discipline, or quiescence of labor contributes to high growth (compare Haggard 1986, 354–56).

Chalmers Johnson (1987, 149), for example, asserts that weak unions and "federations of unions devoid of all but token political power are real comparative advantages in international economic competition." Different reasons for this advantage have been advanced, some more plausible than others. Guillermo O'Donnell (1973) argues that the transition from the easy stage of import substitution industrialization to a more capital-intensive stage creates a need for reduced consumption and, hence, a demand for the repression of labor. Both Fernando Henrique Cardoso (1973) and Hagen Koo (1987) assert that labor control is necessary in order to attract foreign investment. Frederic Deyo (1984 and 1987) maintains that an export-led growth strategy requires cheap skilled labor and, consequently, a disciplined and quiescent labor force.

Whatever the details of the argument, many scholars who have studied the NICs seem to agree that repression or cooptation of the labor force contributes to growth. Taiwan, South Korea (especially after 1961), Singapore (after 1968), Brazil (1964–81), and Mexico (before 1982) all had repressed and/or coopted labor forces and very high growth rates. In other words, all have the outcome of interest and all exhibit another common trait—labor repression—so analysts conclude that labor repression has caused the outcome.

But the conclusion does not follow. Perhaps there are other countries that suppress labor to the same or a greater degree and that have failed to prosper economically. In order to establish the plausibility of the claim that labor repression contributes to development, it is necessary to select a sample of cases without reference to their position on the dependent variable, rate each on its level of labor repression, and show that, on average, countries with higher levels of repression grow faster.

The two tasks crucial to testing any hypothesis are to identify the universe of cases to which the hypothesis should apply, and to find or develop measures of the variables. A sample of cases to examine then needs to be

selected from the universe in such a way as to insure that the criteria for selecting cases are uncorrelated with the placement of cases on the dependent variable.

If the whole universe is too large to study, examination of a random sample is usually recommended as a means of insuring that the criteria of selection do not correlate with the dependent variable. One can, however, make valid inferences from any sample selected by some rule not correlated with the dependent variable. Moreover, randomization does not guarantee the absence of correlation. If, at a particular time, the universe itself only contains cases that have passed a certain threshold of success because "nature" has in some fashion weeded out the others, then even random or total samples will, in effect, have been selected on the dependent variable. If, for example, potential nations that failed to adopt a given military innovation in the sixteenth century were defeated and incorporated into other states, one would not be able to find evidence of the importance of this innovation by examining a random sample of the states that existed in the eighteenth century. All surviving states would have the innovation.

For the hypothesis that labor repression contributes to growth, the universe is easy to identify: all developing countries. In the test to follow, I have included Taiwan and all developing countries for which the World Bank collects data except high income oil exporters, those with Communist governments, those embroiled in civil war for more than a third of the period covered, and those that are extremely small (less than a million inhabitants). Communists countries are excluded because the theory only applies to countries with capitalist or mixed economies. The other exclusions involve countries with characteristics not related to labor repression which could be expected to affect greatly their growth rates.

The dependent variable, growth rate, also presents no problems. Various measures are readily available. For the test, I used World Bank (1984, 218–19) calculations of GNP per capita between 1960 and 1982, since most of the studies of development strategies focus on the period before the debt crisis.

Labor repression/cooptation/quiescence is more difficult to measure. Standard indicators are not available, and labor repression can take different forms in different contexts, for example, state cooptation in one country and private violence against workers in another. To deal with this difficulty, I developed criteria for ranking each country on labor repression, using the *Country Reports on Human Rights Practices* prepared for the congressional committees on Foreign Relations and Foreign Affairs (U.S. Department of State 1981 and 1989).

Countries received a score of one if unions are free to organize and choose their own leaders; labor organizations are not controlled by the gov-

ernment or dominant party; strikes are legal, not constrained to any significant degree by government regulations, and occur reasonably often; and labor can participate in politics. India, Jamaica, and Venezuela are examples.

Countries received a score of two if unions are free to organize and choose their own leaders, labor organizations are not controlled by the government or dominant party, and strikes are legal but constrained by government regulations or simply occur infrequently in practice; or if unions and strikes are legal, etc., but violence against workers curtails the exercise of workers' rights, though without eliminating strikes and demonstrations. Colombia, Zimbabwe, and Malaysia are examples.

Countries received a score of three if union organizations are constrained by links to the government or dominant party; strikes are legal in some cases, but subject to considerable government regulation; and government or private violence against workers is no more than moderate. South Korea, Brazil, and Pakistan are examples.

Countries received a score of four if unions are illegal or completely controlled by the government or dominant party; the right to strike is severely constrained or strikes simply never occur; or if violence against workers is very severe. Chile (1973–79), Syria, and Benin are examples. Where radical changes in levels of repression had occurred between 1960 and 1982, I scored countries in accordance with the more recent regime unless it had been in power less than five years in 1982.

Although this is an imperfect measure of a complex set of phenomena, and experts might disagree about the placement of cases between adjacent categories, this measure is at least as precise as the verbal descriptions available in the literature. It seems, therefore, adequate to the present task of demonstrating a methodological point.

Tests of the hypothesis linking labor repression to growth using these data are shown in figures 3, 4, and 5. Figure 3 shows the relationship for the sample of NICs most frequently studied. This scatter plot reflects the most commonly chosen research strategy for studying the NICs. It shows that repression is moderately high in all five countries. Analysts assume, without checking rigorously, that most of the cases they have not examined lie in the lower left quadrant of the figure. From data like these—but in nonquantitative form—researchers have concluded that labor repression contributes to economic growth. Yet, from inspection of these data, that inference is obviously unfounded. There is no relationship at all between labor repression and growth among the cases shown in figure 3.

Note that the faulty inference here is the opposite of the one a thoughtless analyst using statistical methods would have drawn. Where a number cruncher would have jumped to the conclusion that repression made no contribution to growth because the variance in repression explained none of the

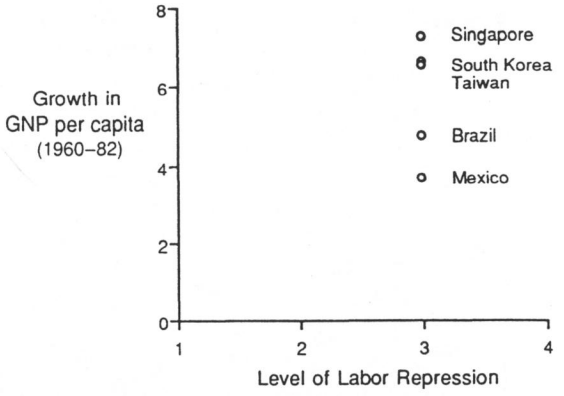

Fig. 3. Growth and labor repression in selected NICs. (Repression scores based on U.S. Department of State 1981 and 1989. Growth rates taken from World Bank 1984.)

variance in growth rate within this high-growth sample, the comparativist jumps to the conclusion that since all cases are high on both growth and repression, repression *must* be a cause of growth. But, in fact, no conclusion can be drawn from figure 3. It simply contains too little information.

Scholars working on East Asia, where the fastest growing NICs are located, have played an important role in developing the argument linking labor repression to growth. If, rather than selecting the five industrializing

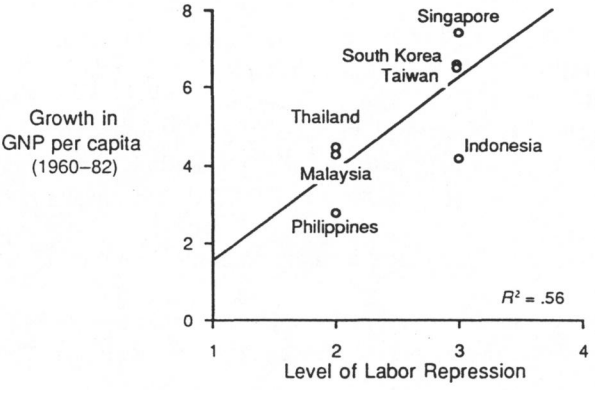

Fig. 4. Growth and labor repression in East Asia. (Repression scores based on U.S. Department of State 1981 and 1989. Growth rates taken from World Bank 1984.)

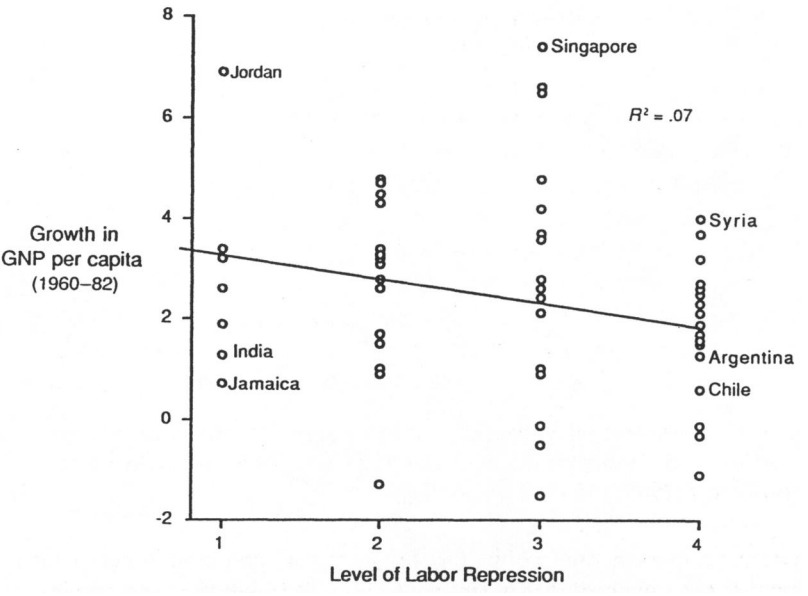

Fig. 5. Growth and labor repression in the Third World. (Repression scores based on U.S. Department of State 1981 and 1989. Growth rates taken from World Bank 1984.)

countries most frequently described in the literature, one examines the cases most familiar to East Asia specialists, it appears that repression does contribute to growth, as shown in figure 4.[2]

Based on findings of this sort, analysts make general arguments about the role of labor repression in growth which imply that the relationship will also characterize the entire Third World. Such an inference cannot be justified because the selection of cases by virtue of their location in East Asia biases the sample just as surely as selection explicitly based on growth rates. This is so because, on average, growth rates in East Asia are unusually high. (See table 1.) Geographical area is correlated with growth, and consequently the selection of cases by geographical location amounts, in effect, to selection on the dependent variable.

Up to this point, I have reminded the reader that one should not make inferences based on samples selected on the dependent variable. This is not to

2. A significance test for a difference of means between the two repression categories shows that the difference is significant at .02. The regression line is shown in fig. 4 only to make the relationship more immediately intelligible.

TABLE 1. Average Country Growth Rates

	1960–82	1965–86
East Asia	5.2	5.1
South Asia	1.4	1.5
Africa	1.0	0.5
Latin America	2.2	1.2
Middle East and North Africa	4.7	3.6

Source: Calculated from data in World Bank 1984 and 1988.

say that a relationship found within the sample will never characterize the population, but only that one cannot assume that it does. In the example used here, as it happens, it does not.

This point is made apparent in figure 5. When one looks at the relationship between labor repression and growth for a larger sample of cases that includes slow-growing countries as well as fast, the apparent relationship between labor repression and growth disappears. The slope coefficient is slightly negative, and the R^2 is .07. In other words, level of labor repression has no effect at all on growth, as shown in figure 5.

It might be objected that several of the arguments linking labor repression to growth were never intended to apply to the entire Third World. Rather, their logic depends on tensions that only develop after industrialization has progressed to a certain stage. Figure 6 shows the relationship between labor repression and growth in the subset of countries at least as advanced as South Korea. Since some confusion exists in the literature about exactly how advanced countries are when the hypothesized difficulty with labor participation begins, I have, in order to be conservative, used the country from among the most discussed cases that was least advanced in the 1970s as the cutoff point.

As figure 6 shows, there is no linear relationship between labor repression and growth, even in this subset of cases. Two possible interpretations of the scatterplot are possible. The first is that the relationship between repression and growth within the more advanced Third World countries is curvilinear. That is, moderate amounts of repression or cooptation contribute to growth, but extreme repression and violence against workers do not. A second possible interpretation is that there is something unusual about Singapore, Taiwan, and South Korea that causes very high growth rates, and that their presence in any small sample will give the appearance of a curvilinear relationship, whether one exists or not. Some support for the latter interpretation can be found in the fact that if the cutoff point for inclusion in the sample of

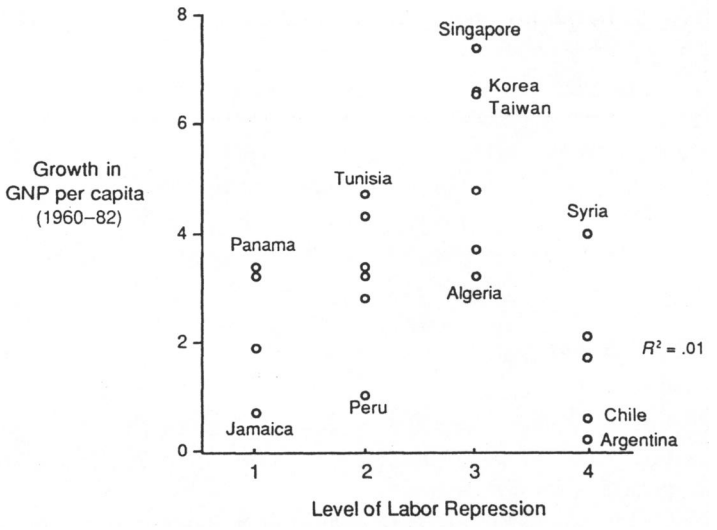

Fig. 6. Growth and Labor repression in the more advanced nations of the Third World. (Repression scores based on U.S. Department of State 1981 and 1989. Growth rates taken from World Bank 1984.)

more advanced countries is lowered to include a few additional cases, the curvilinearity declines markedly.

Whichever interpretation is correct, the point here is not to demonstrate that the hypothesis that labor repression contributes to growth is false. This simple bivariate test cannot disconfirm the hypothesis. It may be that the addition of appropriate control variables would make clear a relationship that does not show in the bivariate test. This test does show, however, that the simple relationship that seems to exist when the analyst examines only the most rapidly growing countries disappears when a more representative sample is examined. If analysts who try to explain the success of the NICs had examined a more representative sample, they would probably have reached different conclusions about the relationship between the repression of labor and growth. As figure 5 shows, labor is just as frequently repressed in slow-growing Third World countries as in fast.

The first example above (fig. 3) demonstrates selection bias in its simplest form: the cases are selected precisely because they share the trait one wants to explain. In the second example (fig. 4), cases are selected on a variable—geographical region—that is correlated with the dependent variable. In both cases, the hypothesized relationship was a simple, direct one: the higher the level of X (labor repression), the higher the expected level of Y (growth).

Not all causal arguments are so simple. Researchers sometimes posit arguments with complicated structures of prior and intervening variables that are more difficult to test rigorously. The consequences of selection on the dependent variable, however, are the same no matter what the form of argument. Subsequent sections will consider two frequently encountered variations on this theme: selection on the dependent variable in a complicated, contingent historical or path-dependent argument; and selection of the end points of a time-series or historical case study on the dependent variable.

Selection on the Dependent Variable in a Path-Dependent Argument

Theda Skocpol's stimulating and thoughtful book *States and Social Revolutions* (1979) combines selection on the dependent variable with a complex, path-dependent argument. She wants to explain why revolutions occur, so she picks the three most well-known instances—the French, Russian, and Chinese—to examine. She also examines a few cases in which revolution failed to occur as contrasting cases at strategic points in her chain of argument. The use of cases selected from both ends of the dependent variable makes this a more sophisticated design than the studies of the NICs.

The central argument in *States and Social Revolutions* is schematized in figure 7. Skocpol's argument is that external military threats will cause state officials to initiate reforms opposed by the dominant class. If the dominant class has an independent economic base and a share of political power, its opposition will be effective and will cause a split in the elite. If, in addition, peasant villages are solidary and autonomous from day-to-day landlord super-

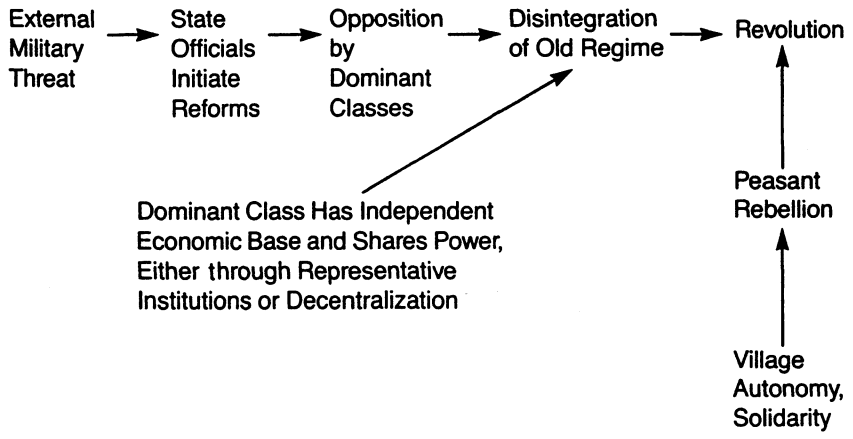

Fig. 7. Schematization of Skocpol's argument

	Elite Splits	Elite Cohesive
Dominant Class Economically Independent, Shares Power	France China, after Taiping Rebellion	
Dominant Class Dependent, Excluded from Power	Russia, World War I	Prussia Japan China, before Taiping Russia, before World War I

Fig. 8. Effect of external military threat

vision, peasants will take advantage of the elite split and rebel, which will lead to revolution. This explanation, according to Skocpol, mirrors the historical record in France and in the parts of China controlled by the Communists. The Russian case differs from the other two in that the upper class lacked the independent economic base necessary to impede state-sponsored reforms, and, consequently, the elite remained unified and revolution failed to occur after the Crimean War. Nevertheless, defeat in World War I caused elite disintegration that opened the way for revolution in 1917.

At two points in the chain of argument, Skocpol introduces contrasting cases to strengthen her contention that structural features identified as important in these three cases are generally important. In an examination of Prussia during the late eighteenth to early nineteenth century and Japan during the late nineteenth century, she finds that dominant classes lacked the independent economic base necessary to obstruct state reforms. Both faced military threats at least as severe as that facing France, but did not experience the disintegration of the elite or, in consequence, revolution. She also looks at Britain during the Civil War and Germany in 1848 and finds levels of village autonomy low. In both, elites fragmented, but peasants were not in a position to take advantage of the situation and, as a result, revolutions did not occur. The results of these comparisons are summarized in figures 8 and 9.

There is no question but that the examination of a few contrasting cases makes the argument more persuasive than it would otherwise be, though a test of the argument based on a few cases selected from the other end of the dependent variable carries less weight than would a test based on more cases selected without reference to the dependent variable. Nevertheless, it is a step in the right direction. A rigorous test would be nearly impossible since the

	Revolution	No Revolution
Village Autonomy	Russia France China, in area controlled by Communists	
Village Dependent		Britain, 1640–60 Germany, 1848 China, before Communists

Fig. 9. Effect of elite split

amount of research needed to assess the situation of the dominant class and the degree of autonomy in peasant villages precludes, for all practical purposes, the examination of many randomly selected cases.

Skocpol makes no effort, however, to test other links in the chain of argument. In particular, she offers no contrasting cases to strengthen her claim that

> . . . developments within the international states system as such—especially defeats in wars or threats of invasion and struggles over colonial controls—have directly contributed to virtually all outbreaks of revolutionary crises. (1979, 23)

This claim seems especially problematic if we accept her implicit definition of "threatened," that is, as threatened as late eighteenth-century France. France—arguably the most powerful country in the world at the time—was certainly less threatened than France's neighbors.

Many countries in the world have suffered foreign pressures as great as those suffered by France and yet revolutions occur infrequently. This raises the question: are revolutions infrequent because of the absence of appropriate structural conditions, as Skocpol's argument implies, or because foreign threats only occasionally set off the sequence of events leading to revolution?

The cases Skocpol selects for examination confirm her argument, but would a differently selected set of cases do so? Ideally, a test of this link in Skocpol's argument would examine all nations characterized by the structural features—that is, village autonomy and a dominant class with an independent economic base and access to political power—she identifies as necessary to complete the sequence from military threat to revolution. Then one could

determine whether revolutions occur more frequently in countries that have faced military threats or not.

In practice, identifying the universe of cases that meet the structural criteria is probably an impossible task. It would require extensive knowledge about every country in the world from the French Revolution to the present. Nonetheless, a serious test of her argument is possible. As it happens, several Spanish-American countries (Mexico, Guatemala, El Salvador, Honduras, Nicaragua, Ecuador, Peru, Bolivia, and Paraguay) have the structural characteristics she identifies and so can be used as a set of cases on which to test the hypothesis linking military threat to revolution. These cases are obviously not selected at random, but, since their geographical location is not correlated with revolution, geography does not serve as a proxy for the dependent variable (as occurred in the test of the relationship between labor repression and growth among the East Asian NICs).

In all these countries, dominant classes had an independent economic base in land and/or mines from the nineteenth century until well into the twentieth. They also shared political power. Thus, they had the economic and political resources that Skocpol argues are needed in order to successfully oppose state-sponsored reforms and thus pave the way for revolution.

These countries also all contained (and most still contain) large, severely exploited indigenous and mestizo populations, many of whom lived in autonomous, solidary villages. Spanish colonial policy reinforced, and in some areas imposed, corporate village structure. After independence, changes in property rights reduced the village control over land, but this reduction in the functions which had contributed to building village autonomy and solidarity was at least partially offset by the increase in absentee landlordism that accompanied increasing commercialization.

Most of the land in these countries was held in large tracts. Some peasants lived on the haciendas, but many lived in traditional villages, owned tiny parcels of land, and worked seasonally on the haciendas. These villages often had long histories of conflict with large landowners over land ownership, water rights, grazing, etc. Villages governed themselves in traditional ways. Landlords have rarely lived in villages in these countries. In short, the rural areas of these Latin American countries approximate Skocpol's description of the autonomous, solidary village structure that makes possible peasants' participation in revolution.[3] With these structural features on which the outcome is contingent held constant, it becomes possible to test the relationship between external threat and revolution.

3. Differences of opinion are, of course, possible about whether peasants in these countries were really autonomous enough from day-to-day landlord control to enable them to play the role Skocpol allots to them in bringing about social revolutions. Perhaps the best evidence that they were is that revolutions have, in fact, occurred in several of these countries and that widespread, sustained peasant revolutionary movements have occurred in most of them.

In the following test, I have used a higher level of threat than that experienced by France in the late eighteenth century. I wanted to choose a criterion for assessing threat that would eliminate arguments about whether a country was "really" threatened enough, and I found it hard to establish an unambiguous criterion that corresponded to the "France threshold." Consequently, the criterion used here is loss of a war, accompanied by invasion and/or loss of territory to the opponent. With such a high threat threshold, finding cases of revolution in the absence of threat will not disconfirm Skocpol's argument since the countries may have experienced external pressures sufficient to meet her criteria even though they have not lost wars. If several countries have lost wars (and the structural conditions identified as necessary by Skocpol are present) but have not had revolutions, however, this test will cast doubt on her argument.

Figure 10 shows the relationship between external threats and revolutions in these Latin American cases since independence. Skocpol's definition of revolution is used: rapid political and social structural change accompanied and, in part, caused by massive uprisings of the lower class (1979, 4–5). It is not clear how long the sequence from external threat to revolution should take, but I thought 20 years a reasonable amount of time for the effects of military threat to be felt.

Figure 10 shows seven instances of extreme military threat that failed to lead to revolution, two revolutions not preceded by any unusual degree of external competition or threat, and one revolution, the Bolivian, that fits Skocpol's argument. These findings suggest that if Skocpol had selected a broader range of cases to examine, rather than selecting three cases because of their placement on the dependent variable, she would have come to different conclusions.

This test does not constitute a definitive disconfirmation of Skocpol's argument. Competing interpretations of all the variables—village autonomy, dominant-class independence, military pressure, etc.—exist, and different operationalizations might lead to different results. In particular, my operationalization of threat fails to capture the complexity of Skocpol's argument, and a different operationalization might put Nicaragua and Mexico in the threat/revolution cell. I would argue, however, that any indicator of threat that identified Nicaragua in 1979 and Mexico in 1910 as threatened would add dozens of other country-years to the threat/no revolution cell. In short, despite some deficiencies in operationalization, this cursory examination of cases not selected on the dependent variable does cast doubt on the original argument.

Selection of the Endpoints of a Time-Series

The final example of how case selection can bias one's conclusions involves time-series analyses, that is, research designs based on the observation of coun-

	Revolution	No Revolution
Defeated and Invaded or Lost Territory	Bolivia, Defeated 1935, Revolution 1952	Peru, 1839 Bolivia, 1839 Mexico, 1848 Paraguay, 1869 Peru, 1883 Bolivia, 1883 Bolivia, 1903
Not Defeated within 20 Years	Mexico, 1910 Nicaragua, 1979	All Others
	Cuba, 1959* [El Salvador]** [Peru]** [Guatemala]**	

*The Cuban Revolution is an intermediate case in Skocpol's terms because it did not entail massive uprisings by the lower classes.

**These countries have experienced long, peasant-based insurgencies that have not so far resulted in revolution but may yet.

Fig. 10. Relationship between defeat in war and revolution in Latin America

tries (or other units of analysis) over a period of time. Here the problem facing the analyst centers on choosing the endpoints of the study in a neutral way.

Perhaps the most widely known demonstration of the effect of the selection of endpoints on conclusions is the work by Raúl Prébisch, which apparently demonstrates a secular decline in the terms of trade for primary products (1950). The Prébisch thesis influenced academics and policymakers all over the developing world, but subsequent work has demonstrated that his findings hold only for the particular period of time included in the original study. Studies using different endpoints have failed to replicate Prébisch's results (e.g., Haberler, 1961; Gonçalves and Barros 1982).

The selection problem in this instance is that the analyst chooses as the endpoint for the study a year in which the variable to be explained has attained some high or low point. The analyst may feel that he or she has no choice in selecting the endpoint; it may be the last year for which information is available. Nevertheless, if one selects a case because its value on some variable at

the end of a time series seems particularly in need of explanation, one, in effect, selects on the dependent variable. If the conclusions drawn depend heavily on the last few data points, they may be proven wrong within a short space of time as more information becomes available.

An easily examined—because quantitative—example of this comes from Albert Hirschman's *Journeys Toward Progress* (1973). In the essay on inflation, Hirschman advances two interlocked arguments. He suggests that observers should take a more benign view of inflation, first, because it may serve as a peaceful alternative to political violence or even civil war. Inflation may give all groups battling over shares of the national pie the illusion that they are gaining. Second, he contends that inflation will be brought under control in time as competing groups realize the futility of their competition and politicians come to understand the problem better. He concludes the essay:

> Inflation then offers an almost miraculous way of temporizing in a situation in which two or more parties who are psychologically not ready for peaceable compromise appear to be set on a collision course. It permits them . . . to maintain a militant and hostile stance while playing an elaborate, largely non-violent game in which everybody wins sham victories. . . . [A]fter having played the game a few times, the parties will realize its futility. . . . (Hirschman 1973, 223)

Hirschman bases his argument on a case study of inflation in Chile that ends in 1961 when the conservative administration of Jorge Alessandri seemed to have succeeded in bringing inflation under control. Chile's yearly inflation rates from 1930 to 1961 are shown in table 2.

Alessandri was elected in 1958 and began an all-out stabilization effort in mid-1959 that succeeded in lowering inflation. Does this show, as Hirschman suggests, that inflation was at long last being conquered after serving the purpose of averting violent confrontation between classes? Figure 11 shows Chile's inflation rate from 1930–72. Even with the hyperinflation of 1973–76 excluded, the figure shows no evidence that groups had learned the futility of pressing inflationary demands or that political leaders had learned to solve the problem. Rather, 1960 and 1961 appear to be unusual years, best explained by the orthodox stabilization policies of Chile's last conservative administration before the military seized power in 1973.

Hirschman would probably have reached different conclusions if he had waited a few years to write his analysis. Even writing at the time Hirschman did, a more cautious observer would have been hesitant to rest an argument on two data points markedly below the trend line. Those two points might turn out to be harbingers of a new phase of declining inflation and thus evidence of

TABLE 2. Chilean Inflation, 1930–1961

Year	Rate	Year	Rate	Year	Rate
1930	−5%	1940	10%	1950	17%
1931	−4	1941	23	1951	23
1932	26	1942	26	1952	12
1933	5	1943	8	1953	56
1934	9	1944	15	1954	71
1935	−1	1945	8	1955	84
1936	12	1946	30	1956	38
1937	10	1947	23	1957	17
1938	2	1948	17	1958	33
1939	7	1949	21	1959	33
				1960	5
				1961	10

Source: Hirschman 1973, 160.

"political learning" as Hirschman thought, but they might not. And an alternative explanation for them—the never to be repeated election by a slim plurality (31 percent of the vote) of a conservative president—was readily available.

Conclusion

The reexamination of these three well-known arguments has shown that choosing cases for study on the basis of their scores on the dependent variable

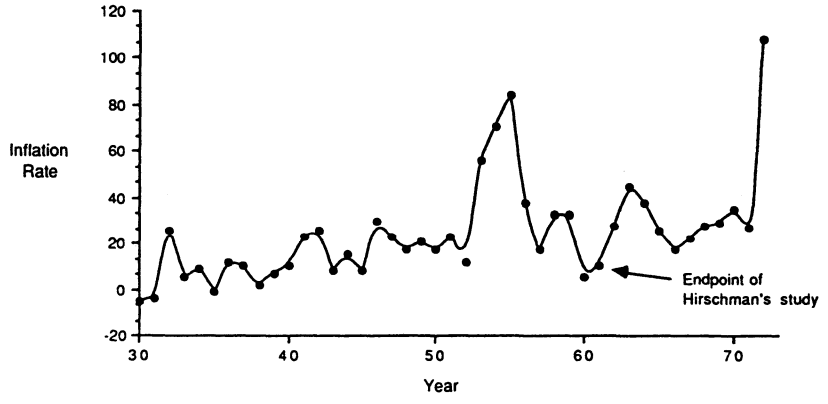

Fig. 11. Inflation in Chile, 1930–72. (Data for 1930–61 from Hirschman 1973, 160. Data for 1962–70 from Valenzuela 1978, 19. Data for 1971–72 from Ramos 1986, 14.)

may bias the conclusions one reaches. Apparent causes that all the selected cases have in common may turn out to be just as common among cases in which the effect they were supposed to have caused has not occurred. Relationships that seem to exist between causes and effects in a small sample selected on the dependent variable may disappear or be reversed in a sample uncorrelated with position on the dependent variable. Arguments that seem plausible if a historical study or time-series ends at a particular date may seem ludicrous if the years included in the study are changed. In short, selecting cases on the dependent variable entails a high probability of getting the wrong answer.

This is not to say that studies of cases selected on the dependent variable have no place in comparative politics. They are ideal for digging into the details of how phenomena come about and for developing insights. They identify plausible causal variables. They bring to light anomalies that current theories cannot accommodate. In so doing, they contribute to building and revising theories. By themselves, however, they cannot test the theories they propose and, hence, cannot contribute to the accumulation of theoretical knowledge (compare Achen and Snidal 1989). To develop and test theories, one must select cases in a way that does not undermine the logic of explanation.

If we want to begin accumulating a body of theoretical knowledge in comparative politics, we need to change the conventions governing the kinds of evidence we regard as theoretically relevant. Speculative arguments based on cases selected on the dependent variable have a long and distinguished history in the subfield, and they will continue to be important as generators of insights and hypotheses. For arguments with knowledge-building pretensions, however, more rigorous standards of evidence are essential.

REFERENCES

Achen, Christopher. 1986. *The Statistical Analysis of Quasi-Experiments*. Berkeley: University of California Press.

Achen, Christopher, and Duncan Snidal. 1989. "Rational Deterrence Theory and Comparative Case Studies." *World Politics* 41:143–69.

Cardoso, Fernando Henrique. 1973. "Associated Dependent Development: Theoretical and Practical Implications." In *Authoritarian Brazil: Origins, Policies, and Future*, ed. Alfred Stepan. New Haven: Yale University Press.

Deyo, Frederic. 1984. "Export Manufacturing and Labor: The Asian Case." In *Labor in the Capitalist Economy*, ed. Charles Bergquist. Beverly Hills: Sage.

Deyo, Frederic. 1987. "State and Labor: Modes of Political Exclusion in East Asian Development." In *The Political Economy of the New Asian Industrialism*, ed. Frederic Deyo. Ithaca, N.Y.: Cornell University Press.

Gonçalves, Reinaldo, and Amir Coelho Barros. 1982. "Tendências do termos de troca:

A tese de Prébisch e a economia brasileira—1850/1979" [Tendencies in the Terms of Trade: The Prébisch Thesis and the Brazilian Economy, 1850–1979]. *Pesquisa e Planejamento Econômico* 12(1): 109–31.

Haberler, Gottfried. 1961. "Terms of Trade and Economic Development." In *Economic Development for Latin America*, ed. Howard S. Ellis and Henry C. Wallich. London: Macmillan.

Haggard, Stephan. 1986. "The Newly Industrializing Countries in the International System." *World Politics* 38:343–70.

Hirschman, Albert O. 1973. *Journeys Toward Progress: Studies of Economic Policy-Making in Latin America*. New York: Norton.

Johnson, Chalmers. 1987. "Political Institutions and Economic Performance: The Government-Business Relationship in Japan, South Korea, and Taiwan." In *The Political Economy of the New Asian Industrialism*, ed. Frederic Deyo. Ithaca, N.Y.: Cornell University Press.

King, Gary. 1989. *Unifying Political Methodology: The Likelihood Theory of Statistical Inference*. New York: Cambridge University Press.

Koo, Hagen. 1987. "The Interplay of State, Social Class, and World System in East Asian Development: The Cases of South Korea and Taiwan." In *The Political Economy of the New Asian Industrialism*, ed. Frederic Deyo. Ithaca, N.Y.: Cornell University Press.

O'Donnell, Guillermo. 1973. *Modernization and Bureaucratic-Authoritarianism: Studies in South American Politics*. Berkeley: University of California Institute of International Studies, Politics of Modernization Series, no. 9.

Prébisch, Raúl. 1950. *The Economic Development of Latin America and Its Principal Problems*. New York: United Nations.

Ramos, Joseph. 1986. *Neo-Conservative Economics in the Southern Cone of Latin America, 1973–1983*. Baltimore: Johns Hopkins University Press.

Skocpol, Theda. 1979. *States and Social Revolutions: A Comparative Analysis of France, Russia, and China*. Cambridge: Cambridge University Press.

U.S. Department of State. 1981. *Country Reports on Human Rights Practices for 1980*. Washington: U.S. Government Printing Office.

U.S. Department of State. 1989. *Country Reports on Human Rights Practices for 1988*. Washington: U.S. Government Printing Office.

Valenzuela, Arturo. 1978. *The Breakdown of Democratic Regimes: Chile*. Baltimore: Johns Hopkins University Press.

World Bank. 1984. *World Development Report 1984*. New York: Oxford University Press.

World Bank. 1988. *World Development Report 1988*. New York: Oxford University Press.