CHAPTER 1

Reflections on the History of European State-making

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Political Development and History

The analysis of political development has had about the same relationship to historical experience as a dog on a long leash to the tree at the other end of the leash. The dog can roam in almost any direction. He can even get the illusion of rushing off on his own. But let him rush too far, too fast and his collar will jerk him back; it may even knock the wind out of him. Some political scientists want to break the leash or at least move the tree. The authors of this book want, instead, a leash which is very long but very sure. Our minimum position is this: major political transformations which occurred in the past may not repeat themselves in the present and future, and are very unlikely to repeat themselves in exactly the same way, but any theories which claim to encompass general processes of political transformation must be consistent with past experience, and ought to be checked carefully against that experience before gaining wide acceptance. We often move beyond that minimum position; we consider the historical experience to be more important than contemporary observation in the formulation or verification of some kinds of generalizations about large-scale political changes.

Political analysis as a whole has never strayed far from the tree. The French Revolution, the Athenian city-state, the Chinese bureaucracy have always stood as cases to be conjured with, examples to illustrate an argument, realities against which to dash an opponent's theory. Since World War II, nevertheless, a number of students of large-scale political process have sought to escape the tyranny of the past through the employment of techniques developed in fields oriented mainly to the present: survey research, international quantitative analyses of data from censuses or bureaucratic reporting, and

Warm thanks to Val Lorwin, who threshed several versions of different parts of this chapter, and found plenty of chaff; one of those parts circulated earlier as Tilly 1970 (citations in this form refer to the bibliography at the end of the book).

so on. The creation of scores of independent states made the swing toward the present possible, and even pressing. It had a psychedelic effect; it widened the vision of those whose textbooks had told them that "comparative government" consisted mainly in the systematic scrutiny of republican, fascist, communist, and monarchical constitutions. "Political development" called attention to change, process, emergence in the present and the future.

The move toward the dynamic analysis of the present entailed serious costs, some of which were unavoidable. Two especially concern us here. If the span of time observed is very short—either because the states under study are new or because data are only available for one or two points in time—it will be impossible to detect trends and to verify theories about what the trends should be. The attempt to extrapolate the experience of Ghana, Jordan, or Jamaica from a span of five or ten years is risk itself. And the alternative use of international comparisons at a single point in time (assuming that Ghana, Jordan, Jamaica, Belgium, and Israel, let us say, stand at different points on some continuum of political development all of them will eventually traverse) begs the very question which theorists of political development are struggling to answer.

The second real cost (this one avoidable in principle but nevertheless widespread) is the implicit introduction of misconceived models of Western experience as the criteria of political development. Misconceived, in three senses: (1) they often caricatured the Western experience by assuming a fairly continuous rationalization of government, broadening of political participation, pacification of the masses, and so on; (2) even where reasonably accurate, they skirted the possibility that the Western experience was a lucky shot, an aberration, a dead end, or simply one among many paths open to "modern" government, whatever that may be; (3) the criteria of modernity tended to twist the question of paths of development into another one: under what conditions and through what transformations might we expect the governments of today's new worlds to end up looking like old-world governments?

The most profound historical analysis in the world will not turn us into clairvoyants. Yet careful examination of the longer run of historical experience will at least defend those who ardently desire to make sense of the contemporary world and the world of the future from the two kinds of error I have described. Indeed, we find many political analysts who began with contemporary affairs moving back

to grapple with the long run: a Barrington Moore transferring his attention from recent Soviet politics to the origins of the world's great political alternatives, a G. W. Skinner gliding from the Chinese in modern Thailand to China over its vast imperial history, a Samuel Huntington bounding from contemporary soldiers to the political development of the major western powers, and numerous others in their company.

This volume, then, may be a leap into a bandwagon already in motion. We hope, at least, that the leap will be graceful and distinctive.

Origins of this Book

The Committee on Comparative Politics gave us a springboard and a starting shove. That committee of the Social Science Research Council had already helped shape the character of American studies of large-scale political change through its patronage of inquiries eventually published under such titles as *Bureaucracy and Political Development*, *Communications and Political Development*, and *The Civic Culture*. Early in 1969, the committee asked a group composed of five of its members plus two interested outsiders—Samuel Huntington, Robert Ward, Myron Weiner, Aristide Zolberg, Gabriel Almond, Rajni Kothari, and Charles Tilly—to formulate a plan of research on state- and nation-building in Western Europe which the committee might be able to support. The planning group recommended a "workshop" for the summer of 1970. The plan was accepted; Almond and Tilly took on the chief responsibility for organizing the 1970 meeting.

The general plan was straightforward: choose a diverse but interlocking set of activities in which all European states engaged to some degree and whose changes and variations were crucial to the subsequent development of the state as a whole; persuade one or two specialists in the study of each of those activities to prepare synthetic essays comparing the historical experience of several European states; bring together the authors of those essays plus a few other general analysts of political development for two months of discussion of the papers, mutual criticism, further research and rewriting; arrange a regular (but not overwhelming) flow of visiting experts and critics. And that is what we did. The authors of this volume were the core group. At the end of a summer's work at the Center for Advanced Study in the Behavioral Sciences, we were sufficiently convinced that the work we had done was interesting (if not neces-

sarily valid or definitive) that we decided to bring together the final versions of the papers we had been discussing, plus some general reflections on the papers, in a collective volume. Here it is.

We began our work intending to analyze state-making and the formation of nations interdependently. As our inquiry proceeded, we concentrated our attention increasingly on the development of states rather than the building of nations. There were several reasons for this drift. One was the greater ease with which we could arrive at some working agreement on the meaning of the word "state." "Nation" remains one of the most puzzling and tendentious items in the political lexicon. Another was our early fixation on the periods in which the primacy of states was still open to serious challenge; they were not generally periods of nationalism, of mass political identity or even of great cultural homogeneity within the boundaries of a state. The third was the bias in our original set of topics toward the extractive and repressive activities of states.

The bias was deliberate. The singling out of the organization of armed forces, taxation, policing, the control of food supply, and the formation of technical personnel stresses activities which were difficult, costly, and often unwanted by large parts of the population. All were essential to the creation of strong states; all are therefore likely to tell us something important about the conditions under which strong or weak, centralized or decentralized, stable or unstable, states come into being. Of course, other activities might have found their way onto the same list: control of manufacturing, enforcement of public morality, propaganda, colonization and imperialism, and so on. This particular list emphasizes activities which became important early in the state-making experience and remained so for a long time, and on which there already exists a substantial body of historical work. As our work proceeded, the chief omission which the group came to regret was the judicial system. Because courts, judges, and judicial proceedings antedate national states and appear in so many unstately guises, it is easy to forget how large a part certain kinds of courts played in the day-to-day construction of Western states. The issue comes up, of course, in papers dealing with taxation, food supply or policing, but the volume as a whole treats the judicial system much less adequately than we would have liked.

Another deliberate decision significantly affected the character of our work, and of this book. We might have asked our specialists to write their papers as commentaries on some particular set of theories

concerning political change. That would have focused our joint inquiry from the very beginning. It would, however, have risked fixing the inquiry on invalid schemes, on problems of terminology, on issues which missed the substance of the phenomena we were trying to understand. Instead, each participant received the invitation to write about his subject as best he knew how, so long as the treatment was synthetic and comparative. (Regardless of native language, incidentally, each participant prepared his contribution in English; where that produced obscurities, I tried to edit the language into comprehensible—but not always idiomatic—English.)

Even the time span was left open. As it worked out, the greatest part of our collective effort went into the seventeenth and eighteenth centuries. An earlier focus would have produced more discussions of the antecedents of full-fledged states than this book contains; a later one would have weighted the discussion toward the accumulation of functions and powers by established states, and the collaboration among existing states in drawing the rest of the world into a system of states. The seventeenth- and eighteenth-century focus has us dealing with periods in which, for most of Europe, both the primacy and the ultimate form of the state were much in doubt. Perhaps that is the most important historical insight the book has to offer: as seen from 1600 or so, the development of the state was very contingent; many aspiring states crumpled and fell along the way.

Of course, all of the participants were aware of open issues in theories of political change to which our conclusions might be relevant; our summer's discussion reinforced that awareness. But the freedom to set each problem in its own terms surely made the individual contributions more valuable, as well as more variable, than they would have been if they had concentrated on a few available general schemes.

The Nature of the Evidence

The materials with which we work in this book will have a spurious familiarity for many a political analyst. They will remind him of faded lecture notes on old constitutions and older states. Yet very few of our readers will currently be attempting to confront theories of political development with systematically assembled historical data. Even fewer will ever have worked firsthand with the actual documents left behind by state-making, as opposed to the standard accounts of state-making bequeathed to us by political historians. In dealing with the broad topics assigned to them, all of our authors have, as one would expect, relied heavily on syntheses prepared by

other scholars. In our own routine work, we vary greatly in our use of primary historical sources. Yet in one way or another all of us—readers and writers alike—are ultimately prisoners of the documents. And the chains are hard to see.

The character of the evidence now available concerning the state-building of two or three centuries ago sets important limits on our ability to frame or validate hypotheses about political development. We must rely mainly on the residues of the paperwork carried on as part of the very construction of the states we are studying. Because of the way such records are produced and preserved, the residues overrepresent the final, official, legal, organization, public sides of the affair; we shall always be hard-pressed to find adequate documentation on the preliminary, unofficial, illegal, informal, immoral, and private aspects of state-making. To the extent that our arguments require reliable evidence on the attitudes and intentions of state-makers or citizens, we are bound to be disappointed. What is more, before the nineteenth century the more or less continuous and comprehensive description of the base population now provided by censuses, surveys, and the like is only available for occasional small fragments of time and space; such simple operations as calculating per capita government revenues by region thereby become difficult or impossible.

As compared with historians of the family or of popular culture, nevertheless, we are in a privileged position. For state-makers are papermongers; their files become our archives. From 1500 onward the documentation becomes ever more abundant. By the nineteenth century it is almost inconceivable that a single person could master in his lifetime the whole range of documentary evidence available for more than ten or twenty years of any important European state's operation.

The documentary residues of state-making fall into several distinct categories. For convenience, we might distinguish among (1) direct inside reporting of the conduct of public affairs, (2) routine by-products of organizational work, and (3) records and reports of interactions between governments and members of the general population. Administrative correspondence bulks largest in the first category, both in volume and in value; few sources can give a more rapid and dramatic sense of the problems and preoccupations of state-makers than the exchanges between a Richelieu and his provincial agents. The memoirs, journals, and personal correspondence of

statesmen (which are rarer, less accessible and harder to interpret than administrative correspondence) also belong here.

The routine by-products of organizational work include some direct inside reporting, to be sure, but they also include a variety of materials which must be systematically collated and reinterpreted before they yield reliable information on how the government did its work. In Western Europe, the largest stores of this kind are records of taxation and registration, personnel files, budgets and financial materials, and the residues of policing and judicial proceedings.

The records and reports of interactions between governments and members of the general population overlap with the routine organizational by-products, since a major part of governmental activity consists of such things as collecting taxes from individuals, registering births, drafting young men, arresting them or verifying their claims for public assistance. Yet a wide range of relevant documents come into existence outside the bureaucracy: the routine papers of other organizations (firms, monasteries, municipalities, guilds, political associations) which have to deal with the state; the reports of travelers or other outsiders, like the British consuls whose detailed dispatches to London are prime sources for the study of eighteenth-century Italy; the memoirs, journals, and correspondence of individuals outside the government.

With sources as varied as these, an energetic investigator can find documentation of a great range of state-building activities. Yet the nature of the sources sets some interesting constraints on his work. I have already mentioned the difficulty of verifying arguments emphasizing attitudes and intentions with this sort of documentation. The analyst who wishes to get at organizational structure in any but the most formal sense finds himself in a rather different bind: the residues of routine organizational activity are abundant and informative, but their very interpretation requires us to adopt unverified assumptions about how the documents came into existence, which are in fact assumptions about how the organization operated. (I have repeatedly run across this problem in using nineteenth-century French police records to study collective violence and governmental repression—two phenomena which overlap with each other more than one might think.)

The great bulk of the documentary sources, furthermore, do not comprise descriptive testimony in the sense that an eye-witness report describes a crime; they are residues of organizational work re-

sembling the files of any of us who spends part of his time in bureaucracies. Expenditure records provide an excellent example: properly verified and compiled, they describe a major feature of an organization's behavior, but only rarely do they include anything like a narrative of the way the expenditure came about, and the verification and the compilation themselves require a theory of how the organization behaves, if only in the form of guesses about what was disguised or left out. Willy-nilly the historian constructs the history of his sources and of the organizations producing his sources as he reconstructs the phenomena he was pursuing in the first place.

Of course, something like this happens in many other branches of historical inquiry. The student of state-making faces the problem with particular acuteness because the organization he is tracing has itself been the principal accumulator of the sources available to him. The correctives are obvious but difficult: the cross-checking of sources drawn from different positions inside and outside the state, the deliberate bringing to light and testing of implicit hypotheses about the organization's behavior, the devising of internal consistency checks, and so on.

This heady difficulty concerns us here because it means that even in the best of circumstances the comparisons we make between two different states (or between the same state at different points in time) will never quite permit us to distinguish between variations in recordkeeping and variations in the phenomena the records are supposed to reveal. A reader who looks between the lines will notice the difficulty again and again as he goes through this book: in our attempts to judge the relative extent of market production in different periods and places, in our efforts to compare levels of government income and expenditure, in our encounters with the varying repressiveness of different regimes, in our estimates of the redistributive effects of different systems of taxation. We are inclined to think that similar difficulties beset attempts to compare the current experiences of twentieth-century states; they are not peculiar to historical analysis. We are also inclined to think that the advantage of having long spans of time to study outweighs the disadvantages of uncertain comparability.

In the last analysis, the suitability of large historical comparisons for the verification, falsification, and elaboration of theories of political development depends on the scopes of the theories in question. Very general theories about long-term development (like those of

Marx, Weber, or Schumpeter) are ripe for historical criticism; they are indeed difficult to test in any other way. Theories of short-run process in contemporary states, like the party-and-interest-group analyses of V. O. Key, Maurice Duverger or Alessandro Pizzorno, by contrast, do not lend themselves to the same sort of testing very well.

That much is obvious. The interesting case is the establishment of hypothetical relationships among major variables: public liberties as a function of bourgeois dominance, political instability as a function of autonomous military power, the relative bulk of the government's fiscal apparatus as an inverse function of the commercialization of the economy, and so on. Political scientists have customarily sought to establish such relationships through the study of variation across numerous areas at the same point of time. Yet at the same time the theories are commonly evolutionary, developmental, oriented to change over time. In such cases, the cross-sectional comparisons hardly bear on the hypotheses which are supposedly being tested. Data on long-run change are required.

Bruce Russett's *World Handbook of Political and Social Indicators* contains an excellent set of cases in point. Most of the handbook consists of straightforward presentations of quantitative data. But later sections of the book move from simple description of correlations to discussions of (1) "stages" of economic and political development, (2) changing relationships between variables, and (3) multifactor explanations of social change, all on the basis of cross-sectional comparisons of countries in the 1950s. A graph of the logarithm of "deaths from domestic group violence per 1,000,000, 1950–1962" (compiled from the *New York Times Index*, the *New International Yearbook*, *Facts on File*, and the *Britannica Book of the Year* and scattered other sources) against the logarithm of per capita Gross National Product in 1957 bears the caption "Economic Development is Associated with Political Violence, at Least in the Early Stages." And the commentary runs:

> This would suggest—to the extent that our cross-sectional model provides useful insights for change over time—that underdeveloped nations must expect a fairly high level of civil unrest for some time, and that very poor states should probably expect an increase, not a decrease in domestic violence over the next few decades. The reasons, of course, are not hard to suggest. In a traditional society knowledge is limited, aspirations are limited,

> and expectations as to the proper activities of government are limited. All this changes with development (Russett 1964: 307).

This passage would be worth squeezing slowly for its implicit view of the world. For present purposes the qualification "to the extent that our cross-sectional model provides useful insights for change over time" is all we need extract. Insight is the most that could come from such comparisons, for they have no logical bearing on propositions about the consequences of change. The cross-sectional differences in deaths from domestic group violence during the 1950s may represent permanent differences between Western and non-Western countries. They are compatible with a world in which collective violence is rising in every country, or falling in every country. And they could result from the ability of the wealthy twentieth-century states to quell dissidence within their own populations, and promote it in poor countries. If we are to formulate essentially historical hypotheses, we shall have to acquire essentially historical data.

The Historical Questions

In their most general terms, all our historical questions go back to one: what are the crucial problems and events in the emergence of the alternative forms of Western states? That immediately raises two issues: common properties and variations. Our attempt to discern the common properties of state-making experiences in different parts of Western Europe has the advantage of extensive documentation, and the logical difficulty of the missing comparison; to really detect the standard features of the West European development, one would want to compare Western Europe with the state-making experience of Eastern Europe, of Asia, of the contemporary world. That we have not done with any zeal.

Our attempt to account for variations *within* the European experience puts us in a somewhat stronger position, since some important differences among, say, Spain, Prussia and England do appear; the main risk we run is of attaching great general importance to what are in fact minor distinctions within the same family of states, or to divisions which are in fact quite idiosyncratic to West European development. The outcome of the Reformation in different European countries, for example, undoubtedly affected subsequent political forms rather seriously; historical peculiarity or general principle?

The problem of variation itself splits neatly into two parts. Part one: what determined the principal variations in the early forms of West European states? Part two: what difference did the character of early state-building make to the subsequent form and substance of political activity in one country or another?

In the first part, we are asking how it is that a centralized monarchy arose early in France, that the Hapsburg domains remained fragmented into ethnic-religious units weakly subordinated to the imperial center, that the regions from Rhine to Oder sustained such a large number of weak principalities, and so on. In the second part, we are asking instead in what respect the early prominence of great landlords and military institutions in Brandenburg-Prussia affected the chances for parliamentary democracy, what features of seventeenth-century Bavaria might have permitted a seer to foretell her failure to survive the nineteenth century as an independent state, whether the long subordination of most of Italy to outside powers somehow determined the special character of Italian unification. That these are enormous questions, and the standard questions of European political history to boot, we are perfectly aware. Our only hope of contributing to their resolution lies in the possibility that the particular variables we have chosen to scrutinize—the variations in the extractive, repressive, control activities of governments—operate in a regular way, and that we have caught some of the regularity in our formulations.

Let me return to the sorts of conclusions we have reached later. What if we *have* made some sense of the Western European experience; why should anyone who is mainly interested in the twentieth-century world outside of Europe pay any attention? For three simple reasons: (1) a large proportion of the reasoning still employed in the analysis of contemporary political change rests implicitly on a reading of European experience, and could therefore be proved wrong on the basis of European evidence; the continued salience of the English, French, and Russian revolutions in contemporary analyses of revolutionary processes illustrates the point perfectly; (2) the European historical experience, for all its special features, is long enough, well-enough documented, and a large enough influence on the rest of the world that any systematic conclusions which did hold up well in the light of that experience would almost automatically become plausible working hypotheses to be tried out else-

where; in contemporary countries the growing utility for demographic analysis of conclusions concerning the conditions for rising or falling fertility drawn from the close examination of European populations over long periods of time offers an attractive parallel; and (3) conversely, ostensibly general formulations which can already be proposed to account for the contemporary world deserve checking against the vast, well-documented European experience; the least that could come of it would be a delimitation of the applicability of such formulations.

But how we attack the European record will determine the utility of our conclusions. Three choices appear to be crucial: between prospective and retrospective forms of analysis, between probabilistic and deterministic formulations and among the searches for recurrent sequences and recurrent relationships.

Prospective vs. Retrospective Analysis

A retrospective analysis begins with some particular historical condition (within our territory the emergence of stable parliamentary democracy has been a favorite item) and searches back for its causes. Its ideal conclusion appears in something like the form "Y occurs if and only if A, B, C . . . X obtain." A prospective analysis begins with a particular historical condition and searches forward to the alternative outcomes of that condition, with a specification of the paths leading to each of the outcomes. Thus Aristotle tells us that given the existence of a democratic constitution, democracy is likely to persist if the wealthy and well-born are checked but not attacked; that oligarchy is likely to replace it if the demagogues attempt to dispossess the rich; that tyranny is likely to appear if the demagogues employ military might in overcoming their opponents; that kingship, polity, and aristocracy are unlikely to grow from democracy in any circumstances, and so on. The ideal formulation of a prospective argument runs "if A occurs,

W will develop if B, C, D . . . N;
X will develop if C, D, E . . . O;
Y will develop if D, E, F . . . P."

In the case of an ideally complete theory of a phenomenon, to be sure, the retrospective statement will be nothing but a special case of the prospective one made about all possible starting points; then we will know all possible paths to W, X, and Y as well as all possible

paths from A, B, and C. As a practical matter, however, retrospective investigation is unlikely to yield valid prospective conclusions, and vice versa. If, then, we are hoping to specify the conditions under which, say, a predominantly peasant population with weak institutions of central government produces (1) military dictatorship, (2) parliamentary democracy, or (3) agrarian socialism, we shall have to set our thinking into a prospective frame.

In beginning our work, we did not fully sense the tension between retrospective and prospective ways of posing questions. That initial uncertainty caused a good deal of trouble, some of which is still visible in the book. We began with a tendency to phrase our most general questions retrospectively, and our detailed historical questions prospectively.

The tension appears in the very selection of a small number of West European states still existing in the nineteenth and twentieth centuries for comparison. For England, France, and even Spain are *survivors* of a ruthless competition in which most contenders lost. The Europe of 1500 included some five hundred more or less independent political units, the Europe of 1900 about twenty-five. The German state did not exist in 1500, or even 1800. Comparing the histories of France, Germany, Spain, Belgium, and England (or, for that matter, any other set of West European countries) for illumination on the processes of state-making weights the whole inquiry toward a certain kind of outcome which was, in fact, quite rare. Having chosen to deal comparatively with those large historical experiences, we never quite escaped the difficulty. Nevertheless, with many fits, starts, stalls, and backtracks, our net movement went away from retrospective questions about political development in general toward prospective questions about the possibilities open at each stage in the formation of Western states.

Probabilistic vs. Deterministic Explanations

Again, any analyst of state-building must choose between probabilistic and deterministic modes of explanation. Having stated the whole range of outcomes theoretically open to the European political situation of the period before states became dominant, we ought to explain why most of those which were theoretically open did *not* occur. That explanation may be probabilistic, and therefore compatible with the conclusion that an unlikely set of outcomes actually occurred in Europe. Or it may be deterministic: in the circum-

stances, Spain had to develop the kind of political structure it acquired, Brandenburg had to develop its special type. In practice, the "explanation" is more likely to take the logically unsatisfying form of an argument that the most probable set of outcomes actually occurred—a form of argument which would have much more bite if one could run history over and over as a series of tries at the slot machine. In any of these cases, however, we shall be engaged in formulating or accepting general rules for change in political structure.

Yet we have a choice. The search for a deterministic explanation will require us to isolate the effects of a large number of variables, including such items as the genetic inheritance of royal families, the vagaries of war, and the mineral composition of a country's subsoil. It will be correspondingly difficult to extend our conclusions to new political systems—in principle, because the permutations and interactions of the variables outside the range we have already observed are certain to be complicated; in practice, because the more variables, the greater the difficulty of acquiring comparable evidence. The search for a probabilistic explanation will permit many of these variables to be treated as "random" and make it easier to identify and examine the appropriate cases among the new political systems, but it will produce explanations of the old systems which are less satisfactory from the point of view of conventional historical reconstruction, and will run the risk that one of the "random" variables will some day reveal itself as fundamental. Since we are self-consciously seeking to employ historical analysis for the purpose of editing ostensibly general theories of political transformation, we have little choice but to adopt a probabilistic approach.

Events, Sequences, and Relationships

Finally, we must choose among attempts to generalize about recurrent events, recurrent sequences, and recurrent relationships. At the level of events, others have often tried to frame general statements about the patterns of revolutions, of wars, of national crises, and so on. At the level of sequences, we have a number of trials at the delineation of standard stages of political development or the equivalent. At the level of relationships, political analysis abounds in arguments that parliamentary institutions thrive to the extent that a bourgeoisie acquires power, that states in which the army is a primary channel of social mobility are vulnerable to coups and the like —without any necessary implication that certain classes of events re-

cur in essentially the same pattern or that all governments of a certain general type pass through the same sequence of transformations. The choice among events, sequences, and relationships will rest in part on the kinds of theories we hope to confront, in part on our guesses as to whether true regularities at a world scale are likely to appear at the level of events, sequences, or relationships.

The authors of this volume differ more widely on this issue than on the questions of prospective-retrospective analysis or probabilism vs. determinism. In their usual professional work, for example, Fischer and Ardant tend to adopt economic reasoning which concentrates their attention on relationships (for example, between commercialization and fiscal efficiency, or employment opportunity and population growth) and they tend to be skeptical about standard-stage theories. Rokkan, by contrast, has in the past devoted large efforts to the detection of standard sequences of representation, mobilization or even state-building in general. Finer and Tilly, to take the third case, have at least occasionally tried to delineate recurrent features of coups, revolutions, and other transfers of power—of events.

Yet when it comes to the particular task of this book, we converge on the analysis of relationships rather than of events or sequences. There are several reasons for that. First, we find that the arguments about political development which take a propositional form (e.g., if X and Y, then Z) and therefore are susceptible of proof deal almost exclusively with relationships; hypotheses concerning standard sequences and recurrent events ordinarily turn out, on close examination, to be tautological or purely heuristic. Second, the greater logical strength of our position in identifying patterns of difference among European states than in detecting their common properties disposes us to emphasize possible relationships between geopolitical position and military strength, between homogeneity of population and legitimacy of political institutions, and so on. Third (and most important), none of us thinks the European experience will repeat itself as a set of events or sequences. Yet all of us are willing to entertain the hypothesis that some of the relationships which showed up there are quite general.

What the Europe of 1500 Had in Common

Precisely because our object is to look forward from 1600 or 1650, we should step back a bit in time to see what lies behind our vantage point. The processes we are trying to examine were already well in

motion by the seventeenth century. At the beginning of the sixteenth —1500—we can get some sense of the common conditions and raw materials on which the state-making processes were operating. By comparison with other eras and other parts of the world, Europeans of the centuries since then have experienced some common conditions which have given their state-making a measure of uniformity and have distinguished it from that now going on elsewhere. First, the Europe of 1500 had a kind of cultural homogeneity only rivaled, at such a geographic scale, by that of China. The earlier unification of the Roman Empire had produced some convergence of language, law, religion, administrative practice, agriculture, landholding, and perhaps kinship as well. In 1500 Celts and Basques held out in the North and West, Magyars and Mongols in the East, Turks and other Muslims in the South; but the populations settled between them shared a common culture and maintained extensive contacts via an active network of trade, a constant movement of persons, and a tremendous interlocking of ruling families. A single relatively centralized church dominated the continent's religious life, an enfeebled empire sprawled over the continent's central sections, clutching fragments of a common political tradition.

In a large part of the area a single family system predominated; bilateral descent leading to the diffuse kindred (rather than a corporate group like the lineage) as the chief larger kinship unit, tendency toward nuclear family residence, small households, relatively late marriage, frequent celibacy, and, consequently, moderate birth rates. These arrangements probably held back the rate of population growth when, in later centuries, mortality fell. But the major significance of this vital and prosperous cultural homogeneity for the emergence of states was the ease it gave to the diffusion of organizational models, to the expansion of states into new territories, to the transfer of populations from one state to another, and to the movement of administrative personnel from one government to another.

I do not mean that variation in position or previous historical experience made no difference in 1500. The *Reconquista* left the Spanish an abundant, often impoverished, nobility which was prepared to alternate between war and disputatious idleness, unlike the monied aristocrats of the Netherlands. The Austrian Hapsburgs were constantly distracted from the work of internal consolidation by the threat of the Turks, a fact which the "most Christian" king of

France did not scruple to use against the projects of the Hapsburg dynasty. Despite the great international web of trade, the economic and social lives of most Europeans were highly localized, market production was not very extensive, internal communications were slow. Well-defined vernaculars (a number of which would later become the identifying features of major states) already divided Europeans into linguistic groups incapable of mutual communication. Nevertheless, in world perspective the cultural homogeneity of the area in which the first powerful national states arose is a condition of prime importance.

The Peasant Base

A second condition is closely connected to it: the prevalence of peasantry. The great bulk of the population and the great bulk of the resources which eventually came under the command of the European state-makers were, in 1500, committed to a peasant way of life. Settled cultivation in which peasant households and communities exercised substantial claims to the land but, under a variety of incentives and pressures, turned over a significant part of their production to town-based consumers (not to mention the idlers of the countryside) in the form of rents, dues, taxes, and cash sales, absorbed most of the working energy expended in the Europe of that time. Nomads, slash-and-burn agriculturalists, fishermen, hunters, and even herders were rare in most of Europe. (Castile and much of northern and western England were heavily engaged in wool production, however, and after 1500 the demand for wool and for meat would hasten the growth of flocks and the dispossession of ploughmen in several parts of Europe.)

The complement, or parasite, of this enormous class of peasants was a small but widespread class of landlords. They were typically titled, which meant that some special relationship to a sovereign guaranteed another special relationship to the land as well as to political power. Their control over the land ranged from (1) symbolic dues paid by peasants who exercised practical freehold, to (2) ownership in the solid nineteenth-century sense of the word, to (3) discretion not only over the land but also over the labor of the men fixed upon it—serfdom. In 1500, however, serfdom of a medieval type was extremely rare, and the alliance between great nobles and statesmen which was to bring something like it into being in Eastern

Europe during the seventeenth and eighteenth centuries had not yet formed.

Within this peasant world, cities had been growing up as centers of trade, communication, administration and manufacturing for some five hundred years. The Europe of 1500 was, proportionately speaking, most likely more urban than China and nearly as urban as Japan. To be sure, cities spread themselves very unevenly across the European landscape, with the greatest concentration in a central band from south to north. The central band also contained a disproportionate share of European manufacturing. For a century or so before 1500, manufacturing for urban markets had been spreading into the countryside—most notably in the form of the cottage industries of Flanders, southern England, and northern Italy. So the city-based bourgeoisie already had a variety of holds on the countryside: as merchants handling peasant produce, as masters of cities exerting organized pressure on the countryside to assure their own provisioning, as entrepreneurs in rural industry, as lenders of money, and, increasingly, as landlords in the hinterlands of the larger cities.

Why should this set of social arrangements affect the emergence of states? First, it meant that Europe as a whole already had a great deal of wealth and productive capacity, but a wealth and productive capacity not only heavily tied to the land but strongly committed to a large number of individuals and local groups. Second, it shaped the paths by which the state-makers could gain access to those resources. Eric Wolf (1966: 90–91) has framed some powerful general hypotheses about the correlates of the presence or absence of large, kin-based coalitions linking peasants to powerholders outside their communities:

> Such coalitions occur in India, the Near East, and China. They do not occur in manorial Europe, post-Conquest Middle America and the Andean area, the Mediterranean, and neotechnic Europe. The distinction appears to divide societies based on centralized and despotic power, exercised largely through the delegation of prebendal domains, from those in which power is more decentralized. The decentralized systems, however, show two subpatterns. The first, characteristic of the Mediterranean, is built up largely in dyadic terms through patronage relationships. The second, found in medieval Europe and in Middle America and the Andes after the Spanish conquest, usually sub-

ordinated a corporate peasant community to a dominant domain owner in the vicinity. This figure then operated as a patron towards the community as a whole.

The second major distinction divides all the systems from neotechnic Europe, which in its emphasis on associational forms has been able to construct vertical relationships on a single-stranded rather than a many-stranded basis.

As compared with political entrepreneurs elsewhere in the world, therefore, European state-makers had to contend rather little with solidly organized lineages, tribes, and the like. But the landlord was crucial. An ambitious king could form coalitions with the landlords, could attempt to destroy, subvert, or bypass them or (more likely) could try a combination of all of these tactics. He could not ignore them, especially when the chief among them formed a self-conscious hereditary caste, a nobility.

Decentralized Political Structure

This set of power relations drawing its sustenance from a peasant base virtually implies the third general condition of the European state-making experience: the emergence of states from the midst of an extensive, decentralized but relatively uniform political structure. The debris of several unifying empires remained. According to Joseph Strayer, medieval theorists working with Roman tradition, Catholic doctrine, and the realities of feudal life had already fashioned a workable theory of the sovereign state—which is to say a set of coherent justifications which could be widely used in the consolidation of power. The agents of the relatively uniform political structure—princes, popes, *podeste*, parliamentarians—sometimes allied themselves with kings in the making. Indeed, they were themselves often kings in the making. But by the same token they supplied the most obvious rivals of those who succeeded, and the most threatened opponents of the process as a whole.

Two features of the background to European state-making deserve emphasis because our unilinear notions of "political development" tend to disguise them: (1) the early political importance of deliberative assemblies, which the rise of centralized states generally eclipsed; (2) the tenacious and widespread resistance to the expansion of state power.

Before the period of state preeminence, power-wielding deliberative assemblies were acting at all levels of political life from the village council to the Electors of the Empire. The institutions did not, of course, represent individuals in the radical democratic manner of the nineteenth and twentieth centuries. But the Parliament, the Cortes, and Estates did ordinarily incorporate the principal segments of the population which had acquired or maintained liberties, privileges sanctioned by law, in the face of the sovereign. Such assemblies normally had some control of taxation, which typically took the form of an extraordinary grant for a specific purpose. Building strong royal power meant co-opting, subordinating or destroying these institutions; that program absorbed a large part of the energy of seventeenth-century kings, and its outcome strongly affected the next phase of political history. Local assemblies everywhere lost power to expanding states, within a range from the retention of considerable autonomy by Swiss communes to the French absorption of existing communal assemblies into the state structure to the virtual destruction of such institutions in much of the Germanies.

The fate of provincial and national bodies was more various, and no doubt more important to the states' later political experience. In England the Parliament survived serious threats from the Tudors and the Stuarts, then went on to govern. In France and Prussia the kings were able to undermine the Estates after considerable effort, and at the expense of erecting large administrative structures to supplant them. And in Spain crown and Cortes lumbered to a standstill. In 1500 the governments of Europe bore considerably greater resemblance to one another than two or three centuries later. One of the chief reasons for that increasing divergence is the varying course of the contest between central power and power-wielding assemblies from the sixteenth century onward.

Nor were the existing assemblies the only groups which resisted state-making. Three broad classes of people resisted: (1) the ordinary people pressed to surrender men, crops, labor, goods, money, loyalty, and sometimes land to the emerging states; (2) the established authorities pressed to relinquish or share their power; and (3) the rival claimants to sovereignty.

For all their reputed docility, the ordinary people of Europe fought the claims of central states for centuries. In England, for example, the Tudors put down serious rebellions in 1489 (Yorkshire), 1497 (Cornwall), 1536 (the Pilgrimage of Grace), 1547 (the West),

1549 (Kett's Rebellion), 1553 (Wyatt's Rebellion), all responding in one way or another to the centralizing efforts of the crown. These massive provincial rebellions dwindled after 1600. Yet the revolutions of the seventeenth century—despite the modernizing consequences we are now inclined to identify with them—grew most directly from the Stuarts' effort to concentrate power in the crown. The result was an enormous amount of conflict and resistance. As a recent student of the period after the seventeenth-century English revolutions has written: "By 1688 conspiracy and rebellion, treason and plot, were a part of the history and experience of at least three generations of Englishmen. Indeed, for centuries the country had scarcely been free from turbulence for more than a decade at a time" (Plumb 1967: 15). The political transformations of the next three or four decades, according to Plumb, firmly seated an oligarchical state in England, although even that onset of "political stability" did not, by any means, eliminate violent conflict from English political life. Many of the same sequences of demand, resistance, repression and control were, for instance, yet to be repeated in Ireland.

Nor were things calmer across the channel. France experienced the bloody religious wars of the sixteenth century, which pivoted to some extent on questions of royal prerogative versus regional liberties. The clear, concerted, and violent resistance to the imposition of state control rose to its acme in the seventeenth century, as the crown demanded greater tax revenues from the French population, and acted to neutralize or destroy the authorities which stood in its way. While later on the pressing or conscription of men for military service and the forced delivery of grain to cities and armies incited widespread, if localized, rebellions throughout Europe, the earliest, most dramatic, and most influential forms of popular resistance centered on taxation. After all, taxation was the chief means by which the builders of states in the sixteenth century and later supported their expanding armies, which were in turn their principal instrument in establishing control of their frontiers, pushing them out, defending them against external incursions, and assuring their own priority in the use of force within those frontiers. Conversely, military needs were in those first centuries the main incentive for the imposition of new taxes and the regularization of old ones. The need fed itself, furthermore; the overcoming of resistance to taxation required the maintenance of a military force. So turned the tight cir-

cle connecting state-making, military institutions and the extraction of scarce resources from a reluctant population.

The state-makers only imposed their wills on the populace through centuries of ruthless effort. The effort took many forms: creating distinct staffs dependent on the crown and loyal to it; making those staffs (armies and bureaucrats alike) reliable, effective instruments of policy; blending coercion, co-optation and legitimation as means of guaranteeing the acquiescence of different segments of the population; acquiring sound information about the country, its people and its resources; promoting economic activities which would free or create resources for the use of the state. In all these efforts and more, the state-makers frequently found the traditional authorities allied with the people against them. Thus it became a game of shifting coalitions: kings rallying popular support by offering guarantees against cruel and arbitrary local magnates or by challenging their claims to goods, money or services, but not hesitating to crush rebellion when the people were divided or a sufficient military force was at hand; magnates parading as defenders of local liberties against royal oppression, but not hesitating to bargain with the crown when it appeared advantageous. Ultimately, the people paid.

There were also a multitude of unsuccessful rival claimants—the princes, bishops, dukes, and brigands who bid for sovereign power but failed. For the reduction of Europe from some five hundred more or less independent political units in 1500 to twenty-odd states in 1900 produced a large number of losers. Unlike the Chinese and Roman state-builders of earlier times, the Europeans of 1500 and later did not ordinarily expand from a highly organized center into a weakly organized periphery. (Which is not to say there are no analogies like the English conquest of Ireland and the expansion of Brandenburg-Prussia through Central Europe, or that regional variations in this regard made no difference.) Only in European colonial expansion did that become the dominant experience.

Nor did the European state-builders often have the chance to seize and strengthen a single preexisting political structure, as has been the frequent experience of anticolonial rebels since the Americans of 1775. Indeed, building substantial states in much of Europe meant absorbing numerous political units which already exercised significant claims to sovereignty—free cities, principalities, bishoprics, and a variety of other entities. The European state-makers en-

gaged in the work of combining, consolidating, neutralizing, manipulating a tough, complicated, and well-set web of political relations. They sought to fashion something larger and stronger than had existed before. In order to accomplish that, they had to tear or dissolve large parts of the web, and to face furious resistance as they did so.

Yet one feature of the earlier political structure greatly favored the enterprise. The Europeans of 1500 had a tradition of kingship which stretched back in diverse ways to Roman times. Just behind them lay seven hundred years of king-making experience which (for all its chaos) had resulted in almost every European's being at least nominally subject to one crown or another. The state-makers who followed were for the most part kings and agents of kings. They were very much members of the system they were gradually destroying. They commanded a measure of submission on quite traditional grounds. The genuine breaks with the tradition came with the formation of federated republics like the Dutch or the Swiss, and with the extension of European power into overseas colonies.

Conditions Favoring the National State

In order to isolate the conditions which favored the increasing dominance of national states over the people of Western Europe, we have to play an unseemly trick on history. We have to consider what could have happened, instead of what was. Without some such comparison—implicit or explicit—between what occurred and what could have occurred, we have no means of separating the trivial antecedents of state-making from the weighty ones. In order to find a time when the national state does not already seem to have preempted all the alternatives, furthermore, we probably have to go back as far as the thirteenth century.

Even then, according to Joseph Strayer, the form of the European state was already well set, the time of its dominance already begun.

> By 1300, it was evident that the dominant political form in Western Europe was going to be the sovereign state. The universal Empire had never been anything but a dream; the universal Church had to admit that defense of the individual state took precedence over the liberties of the Church or the claims of the Christian commonwealth. Loyalty to the state was strong-

> er than any other loyalty, and for a few individuals (largely governmental officials) loyalty to the state was taking on some of the overtones of patriotism (Strayer 1970: 57).

Strayer's test for the emergence of a state, however, is a relatively soft one: ". . . the appearance of political units persisting in time and fixed in space, the development of permanent, impersonal institutions, agreement on the need for an authority which can give final judgments, and acceptance of the idea that this authority should receive the basic loyalty of its subjects" (Strayer 1970: 10). The test excludes government by corporate groups or networks which are either geographically fragmented or constantly on the move, even if they exercise primary claims to the obedience of their members. It also excludes government by the coalition or federation (or, for that matter, competition) of multiple authorities exercising jurisdiction over the same populations. It excludes, finally, the incidental exercise of political control by a structure whose principal activity is trade, control of marriage, manipulation of sacred rites and symbols, or something else of the sort. The test does not, on the other hand, require any particular *scale* of government, any particular *degree* of integration among the "permanent, impersonal institutions," or any great concentration of power in the central authority. When it comes to defining the state-making processes of later centuries, those criteria take on the greatest importance.

As a matter of fact, Strayer explicitly brings empires and city-states under his heading; he presumably would not have great difficulty bringing in rule by bishops and popes as well, just so long as they controlled specialized institutions of government. So the claim for early predominance of the state in Europe does not have quite the sweep it first seems to have. In the thirteenth century, then, five outcomes may still have been open: (1) the form of national state which actually emerged; (2) a political federation or empire controlled, if only loosely, from a single center; (3) a theocratic federation—a commonwealth—held together by the structure of the Catholic Church; (4) an intensive trading network without large-scale, central political organization; (5) the persistence of the "feudal" structure which prevailed in the thirteenth century. The rational appraisal of any such list is a peculiar task, since it is so easy to give reasons why events which did not happen could not happen. (On seeing this list, Joseph Strayer has said to me that the political fed-

eration was no more than barely possible; the theocratic federation impossible, since the church had already conceded the temporal priority of the state; the persistence of the "feudal" structure equally impossible, because that structure was already gone by 1300.) Nevertheless, each of the unreal outcomes corresponds to developments which did at least begin in Western Europe.

Not only had the Roman Empire prevailed in Europe, not only had the Holy Roman Empire persisted as its shadow, but the Hapsburgs actually kept their own rickety federation operating into the nineteenth century. At times they even dominated the continent. A church-dominated federation seems implausible because we so regularly oppose church and state. Yet some churchmen held great political power well into the modern period. The papal states survived somehow into the nineteenth century. And there are examples elsewhere in the world (including the nearby Near East) of large-scale, yet decentralized, government by priests. The interlocked trading cities of Germany and northern Italy did, in fact, long resist consolidation into national states. They created commercial federations like the Hanse, but not common armies, common bureaucracies or common fiscal structures. Finally, the "feudal" organization of manors, towns, and overlords—for all the imprecision of the word *feudal*—has a special claim: it predominated in Western Europe for several centuries.

The structure which became dominant in Europe after 1500, the national state, differed from these alternative possibilities in several significant ways: (1) it controlled a well-defined, continuous territory; (2) it was relatively centralized; (3) it was differentiated from other organizations; (4) it reinforced its claims through a tendency to acquire a monopoly over the concentrated means of physical coercion within its territory. So, in a sense, explaining how the national state won out amounts to accounting for territorial consolidation, centralization, differentiation of the instruments of government from other sorts of organization, and monopolization (plus concentration) of the means of coercion.

The common conditions we have already noticed as prevailing around 1500 obviously affected all these processes. It is not so clear, however, that they *caused* the dominance of the state. The conditions prevailed earlier than 1500. In 1300 as well, Europe was relatively homogeneous culturally, drew the great bulk of its resources from a peasant base, and maintained a decentralized political struc-

ture, or, rather, a congeries of political structures. Homogeneity itself probably did not predispose Europe toward national states. Elsewhere in the world, large homogeneous populations have lent themselves well to the building of empires; although cultural homogeneity makes the policy of *divide et impera* less feasible, it decreases the difficulty of extending uniform administrative arrangements over large populations, and it makes common allegiance easier to promote. The principal way in which the relative homogeneity of the European population facilitated the emergence of the national state rather than some other political dominant, it seems to me, was in making it easy to divide the continent up into mutually exclusive territories which were at once rather arbitrary and subject to considerable change.

Nor does there seem to be any particular affinity between peasants and national states; not only have peasants borne the burden of empires and city-states, but also European feudal arrangements depended from the beginning on the presence of a subordinate peasantry. My earlier point was not that peasants are essential, or even favorable, to the creation of national states; it was rather that the predominance of peasants drew state-makers willy-nilly into struggles and coalitions with the men who controlled the land. The strongest argument one could make for the peasant base as a cause of the state's victory, I suppose, is that the presence of peasants gave power to major landlords, and the necessity of coalitions with regional groups of landlords (who had some choice with which authorities, or would-be authorities, to ally themselves) both limited the scale at which princes could operate and pushed them toward territorial agglomeration. Those circumstances, however, resulted more directly from the decentralized political structure than from the predominance of peasants.

It seems odd to consider that political fragmentation as one of the conditions contributing to the final dominance of national states. Did not the state-makers struggle continually against fragmentation? Yes, but the very presence of multiple contenders for power, mutually aware and relatively equal in strength, promoted a process of consolidation by means of shifting coalitions among geographically concentrated elites, made it likely that more than one such process would be going on at a time, and hampered any effort either to impose an authority without contiguous territory or to subordinate a large part of Europe to a single authority. We have the *reductio ad*

absurdum of the condition S. N. Eisenstadt analyzes in *Political Systems of Empires* (1963): a large, functioning empire goes through a continuous cycle of building up power in its peripheral units only to see those who have direct access to that power turn it to their own ends and against the central structure; when an empire disintegrates as the Roman Empire did, perhaps it follows that its fragments are that much harder to bring back into a common structure.

The weakness of corporate structures, especially those linked by kinship, in Europe probably abetted this process of growth through the manipulation of shifting coalitions. If lineages controlling land, labor and loyalty had sprawled across the European map, it would have been harder to break up the population into discrete territories, co-opt powerful members of local elites without extending privileges to their clienteles or reinforcing the lineages as such, differentiate government from kinship, and so on. The recurrent confrontations in our own time between corporate kin groups and non-Europeans who have sought to construct states on the European model reinforce this speculation. Perhaps the apparent difficulty of state-making in those portions of Europe which *did* harbor fairly powerful corporate kin groups (I have southeastern Europe especially in mind) points in the same direction.

All the "explanations" of the victory of the national state over its theoretically possible alternatives I have proposed so far fall into the category of preconditions. But some features of state-building processes, and of the circumstances which accompanied them in Europe, also deserve inquiry. First, specialized organization works. If the task at hand is well defined and consists of manipulating the outside world, the ruler who builds a special-purpose organization and keeps it supplied with resources tends to have the advantage over his rivals. Success in war is a notable example: the criteria of success are relatively clear, the main elements of the task (if not the secret of their accomplishment) rather clear, and the superiority of the well-supplied special organization quite likely to tell in the long run. For the purposes of routine regional administration, fiscal control or the acquisition of supplies for the needs of government, a record-keeping bureaucracy has a number of advantages over a kin group, a patron-client chain, a trading network or the other structures built into the theoretical alternatives to the national states.

That assertion is, to be sure, far from self-evident. One can challenge it either by recalling the importance of certain kinds of bu-

reaucracy in imperial China and ancient Babylon or by insisting on the costs and inefficiencies of contemporary bureaucracies. I do not want to argue out that whole problem here. I simply want to suggest that in medieval Europe princes who specialized their instruments of government by forming a separate exchequer, a group of legal advisors, and so on gained the organizational advantage—and that the very transformation of the instruments of government in that direction promoted the formation of the large, autonomous, differentiated, territorially distinct organization we call the national state.

Another circumstance which probably favored the fortunes of the national state over its possible alternatives in Europe was the openness of the European periphery. I have in mind both the lack of important concentrations of power around the immediate areas in which states were forming, and the availability of territories for expansion, conquest and extraction of new resources. The Byzantine Empire and the various Muslim empires which formed around the Mediterranean put the most immediate pressure on European political life. Europeans were relatively free from that sort of pressure around some two-thirds of their perimeter. As a consequence, relatively small political units could grow without being gobbled up (or at least dominated) by adjacent empires. Furthermore, the relatively small powers had room—eventually including land across the Atlantic—to expand, colonize, and establish their own imperial control. Without this combination of circumstances, I want to suggest, a much larger organization than the European national state might well have been the only one that could have survived.

We should not, finally, neglect the contributions of cities, trade, merchants, manufacturers, and early capitalism. No doubt if the merchants and burghers of the thirteenth or fourteenth century had laid out a political master plan for Europe, it would not have included national states. They were much more interested in maintaining the autonomy of cities and the links among them. Yet the intensity of mercantile activity in Europe before the national state probably played a major role in freeing resources, making taxation and related forms of governmental extraction feasible, and motivating old authorities to develop new forms of control over the population.

Later on a powerful reciprocal relationship between the expansion of capitalism and the growth of state power developed, despite the frequent efforts of capitalists themselves to fight off state power; Fernand Braudel has made the conjunction a major theme of his

analyses of the European sixteenth century (Braudel 1949, 1966). Well before then, however, the presence of a taxable wool trade was helping an English monarchy pay for its operations, and the merchants of Paris were discovering that they had to keep the crown supplied with money. The intermittent but substantial expansion of European trade and industry gave a competitive advantage to the political authorities who could devise means of diverting the resources to their own purposes.

In looking back over these supposed causes of the national state's success, we must again distinguish between the easy version of the argument and the hard one. Obviously, all of these conditions (to the extent that I have portrayed the conditions prevailing at the beginning of European state-making accurately) *affected* the way national states formed and grew. That point is trivial. The hard questions are: (1) what structural alternatives were possible; and (2) why this alternative rather than the others? I have identified the empire, the theocratic federation, the trading network and the feudal system as possible alternatives. I have then gone on to propose that the preexisting political fragmentation, the weakness of corporate structures, the effectiveness of specialized organization, the openness of the European periphery and the growth of cities, trade, merchants, manufacturers, and early capitalism weighted the outcome toward the national state. Such a statement implies that a number of other factors like the particular geography of Europe, its cultural homogeneity, and its largely peasant population did not affect the choices among outcomes so much. The way to check out such assertions is through a detailed comparative examination of the correlates of state-making and the alternative processes both in Europe and elsewhere. But this book can be no more than a preface to that noble enterprise.

Different Exits from the Common Conditions

The common conditions—the relatively standard culture, the peasant base, the preexisting, decentralized political structure—shaped the states which developed in Western Europe sufficiently that they have all remained recognizably of the same species up to our own time. Not that their trajectories have been identical; Greece, Switzerland, and Italy produced rather different kinds of states. Let us save the detailed comparison of individual histories for later. Right now the problem is to identify the largest features and grossest

variations in the European state-making experience from 1500 onward.

In its simplest version the problem has only three elements. First, there is the *population* which carries on some collective political life —if only by virtue of being nominally subject to the same central authority. Second, there is a *governmental organization* which exercises control over the principal concentrated means of coercion within the population. Third, there are *routinized relations* between the governmental organization and the population. Historians and political scientists have at one time or another called attention to a huge range of characteristics of such populations, governmental structures, and relations between them. In raising preliminary questions about the alternative patterns of state-making in Europe, I want to call attention to only one broad characteristic of each of the elements. First, the pattern of mobilization within the population subject to each state. I mean the identity, sequence, scale, and success of the various segments of the population which formed, acquired collective control over resources, and participated in national struggles for power. The second is the degree of "stateness" of the governmental structure. Borrowing from J. P. Nettl (1968), I mean the degree to which the instruments of government are differentiated from other organizations, centralized, autonomous, and formally coordinated with each other. Third, the forms of political rights exercised by the population with respect to the governmental structure. I have in mind the enforceable claims that members of the population can make on agents of the government—both compelling them to do something and preventing them from doing something else.

Mobilization

Although the cultural homogeneity of Europe restricted the kinds of groups available for mobilization anywhere and although the generality of urbanization, industrialization, and capitalization eventually guaranteed that every country would experience some degree of mobilization on the part of its urban and its rural working classes, the patterns of mobilization have varied strikingly over time and space. Nevertheless since 1500 Europeans have rarely mobilized on the basis of color, kinship, age, sex, or ecological position. The principal exceptions are, no doubt, the nineteenth-century drive for

women's rights and the recurrent movements of youth. Europeans have, on the other hand, frequently mobilized on the basis of religion, language, previous political status, community membership, class, and, more narrowly, occupation.

As mass politics grew up from the French Revolution onward, class, language, and religion became relatively more important as bases of mobilization. Stein Rokkan (1970a: 101) analyzes the shift in this way:

> Territorial oppositions set limits to the process of nation-building; pushed to their extreme they lead to war, secession, possibly even population transfers. Functional oppositions can only develop after some initial consolidation of the national territory. They emerge with increasing interaction and communication across the localities and the regions, and they spread through a process of "social mobilization." The growing nation-state developed a wide range of agencies of unification and standardization and gradually penetrated the bastions of "primordial" local culture. So did the organization of the Church, sometimes in close cooperation with the secular administrators, often in opposition to and competition with the officers of the state. And so did the many autonomous agencies of economic development and growth, the networks of traders and merchants, of bankers and financiers, of artisans and industrial entrepreneurs.
>
> The early growth of the national bureaucracy tended to produce essentially territorial oppositions, but the subsequent widening of the scope of governmental activities and the acceleration of cross-local interactions gradually made for much more complex systems of alignments, some of them *between* localities, and others *across* and *within* localities.

The order and relative weight of the diverse forms of mobilization depended, in Rokkan's view, on three main factors: (1) the state-church relationship established, first, at the Reformation and, second, at the advent of mass participation in national politics; (2) the relative timing of participation and of industrialization; and (3) the cultural—and especially religious—diversity of the population under the jurisdiction of the state in question. (Rokkan in Chapter 8

reformulates and extends the earlier argument, but on the question of mobilization its essence remains the same.)

The extent of mobilization on the basis of language and belief depended to an important degree on the form and policy of the state. Mobilization on the basis of class position within the industrial system occurred regardless of the character of the state, although twentieth-century states sometimes acted to promote working-class mobilization (as in socialist regimes) or to check it (as in fascist regimes). The state's own policy with regard to cultural homogenization, selection of governmental personnel, control of mobilizing groups and other interactions between government and people itself helped shape the pattern of mobilization.

Both peasants and workers were slow to mobilize at a national scale; before fairly late in the nineteenth century, their mobilization was ordinarily short-term and defensive. Before then there was greater variation in the mobilization of the landowning and mercantile classes; everywhere some kind of accommodation between landlords and the crown played an important part in the extension of state power, even though landlords also supplied the most serious resistance to state-building. That is hardly surprising, given the importance and the diversity of the people who qualify somehow as landlords in an agrarian society.

Stateness

During the last century or so, all West European governments arrived at a relatively high level of stateness, as measured by formal autonomy, differentiation from nongovernmental organizations, centralization, and internal coordination. They have collaborated with each other and with their overseas offshoots in promoting the division of the entire world into state-like units. It has not always been so. Although the drift after 1500 throughout Europe ran toward increasing stateness, different governments moved at very different rates. As a result, international disparities in stateness increased during the sixteenth and seventeenth centuries. By this criterion, the sixteenth century was a time of significantly rising stateness, the later seventeenth century a frenzy of state-making, the eighteenth century (outside) and the East a period of consolidation, the nineteenth century and early twentieth century an age of convergence among governments which were still significantly different from each other in 1800.

Such a summary exaggerates the regularity of the process, and obscures crucial leads and lags. With respect to autonomy, differentiation, centralization, and internal coordination, France led Europe through almost all the period after 1500, while England accumulated stateness at a slower pace and at a lower level. Yet in 1650 England lay under the control of a centralizing military dictatorship, while France underwent the fragmentation of the Fronde; for that brief moment the English government was very likely the most state-like in Europe. All things considered, however, England (which did so much of its governmental business through justices of the peace, private corporations, merchant fleets and similar groups only indirectly attached to the central structure) survived into the nineteenth century with a rather low level of stateness. Again, Spain produced a sixteenth-century spurt of state-making which, if continued, could easily have brought her to Europe's highest level of centralization, differentiation, autonomy and coordination; the process slowed, and sometimes reversed, in the seventeenth and eighteenth centuries. As a result, Spain entered the age of industry and empire with one of the least stately governmental structures of the continent.

Extreme stateness, of course, neither guarantees political stability nor assures power in the international arena. One might guess that an increase in stateness does ordinarily increase a government's command of the mobile resources within its subject population, does increase its capacity to free resources which embedded in traditional networks of obligation, and thereby augments the government's power to apply resources to objectives at a national or international scale. But if the European experience is a guide, the short-run cost is an increase in the likelihood of resistance and revolt. Hence, a close historical connection among increases in stateness, expansion of armed forces, rises in taxation, and popular rebellion.

Political Rights

Between the development of stateness, on the one hand, and the pattern of mobilization, on the other, comes the acquisition of political rights binding on agents of the government by the members of the mobilized groups within the subject population. Extensions of the suffrage, for example, do not follow from the pace of state-making alone, or from the pattern of mobilization alone, but from an interaction between the two processes. One could, it is true, make a

credible case for two subtly complementary propositions: (1) among European governments, those which were higher in stateness by the nineteenth century extended the suffrage farther and faster; (2) but extensions of the suffrage were more durable and supported by surer guarantees in the less state-like governments (cf. Rokkan 1970: 82–88). The propositions would, however, break down entirely if applied to changes over time, since the movement to a wider suffrage and the continued growth of stateness occurred together, yet in nothing like the same rhythm. To understand the early (if temporary) advent of manhood suffrage in France (1793) or Prussia (1849), we shall have to grasp the vast mobilization of peasants and workers which occurred just before those years, and the acute political pressure it placed on the managers of the states in question.

A number of other political rights ought (at least in principle) to yield to the same sort of comparative analysis which has aided theorizing about electoral systems: the state's assurance of the forms of justice, the right of assembly, freedom of the press, the right of petition, protection of religious minorities, defense of life and property, and other claims of individuals or groups on the state which are guaranteed in principle and in general rather than through personal influence or special connections. The development of such rights is much harder to define, detect and compare than the acquisition of voting rights. They fluctuate from regime to regime. They are vulnerable to invisible abridgements. Conversely, they often accrete slowly and inconspicuously before being sanctified in law. They always apply unequally in actual practice. And the managers of states always claim that they are more general than reality shows them to be. In compensation for these difficulties in the systematic study of political rights, they cluster to some degree, and may even form a kind of scale.

For present purposes, the significant thing about such rights is not that they should exist in the abstract, but that the state, rather than some other organization, should become the focus of their enforcement. Indeed, rather than thinking of abstract rights which are enforced (or violated) by states, it would be more illuminating to think of political rights as claims binding the agents of the state to specific groups of people: religious minorities can call on the state to defend them against persecution, adult citizens can insist the state give them their day in court, the indigent can claim sustenance from

the state. These are political rights in a large sense—political in that they constitute binding claims on the agents of government, rather than on some other group.

That specification clarifies a large historical transformation. The European national revolutions of the last few centuries did not so much expand political rights as concentrate them in the state and reduce their investment in other sorts of governments. A large part of the process consisted of the state's abridging, destroying or absorbing rights previously lodged in other political units: manors, communities, provinces, estates. In cases like the state's seizure of control over justice from manorial lords, churches and communities, the right itself continued in more or less the same form, but under new management. In other cases, the right disappeared entirely. The right of a household to pasture its flock on the village commons is a notorious example, the right of the household's head to punish its members is a less obvious one.

Nothing could be more detrimental to an understanding of this whole process than the old liberal conception of European history as the gradual creation and extension of political rights. Europeans created and used a wide variety of representative institutions (although not, of course, in the image of the nineteenth-century parliaments) before the heyday of the national state. Far from promoting such institutions, early state-makers struggled against them. Bernard Guénée gives us an idea of the fourteenth-century prelude:

> In the middle of the fourteenth century, most likely between 1345 and 1360, the chivalric orders were born, the progress of bureaucratization was checked for a long time to come, the first great revolutionary wave swept across Europe, representative assemblies had their first great successes, and the peoples of Europe seized their most handsome rights from their princes.
>
> Fifty years later, the weight of the English Parliament was so great that Stubbs could speak . . . of Lancastrian parliamentarism; a new revolutionary wave was crossing France and Catalonia, the Estates were achieving their greatest strength in Germany, the Council of Constance weakened the foundations of papal monarchy, the Hussite revolution shook the Empire and challenged the social order. After that came the reflux. Little by little, the Pope and the princes got a grip on themselves, demo-

> cratic convictions faded, the orders of chivalry died, and the bureaucratization began again (Guénée 1971: 405).

For a long time after then, the builders of states worked to stamp out or absorb existing rights, not to extend them.

Rights in the form of claims on the state emerged through an interaction between populations mobilizing at a national scale and governments acquiring greater and greater stateness. The mobilizing groups ordinarily made the claims long and insistently before statesmen, with good grace or bad, honored them. The right of assembly illustrates the point very well. Far from being a right well established in the abstract, it comes into being as a claim of specific segments of the population and specific types of groups—Jews, young people, vagabonds, revolutionaries, sportsmen, patriots—to the protection of the state in their collective occupation of public places. For this reason, changes in the law and in the policing of public assemblies are among our most valuable signs of the acquisition and loss of political power by different segments of the population.

The three sorts of political change I have singled out for examination—changes in stateness, patterns of mobilization, acquisitions and losses of political rights—do not fit together into any single pattern we could confidently call "political development." Each includes some of the transformations which analysts of the contemporary world have put under that heading. For our purposes, rather than patching the battered label, it seems more useful to portray the great political changes involved in the emergence of the European state system as an interaction between two partly independent processes, mobilization and state-making, with the acquisition and loss of rights by different segments of the population being a product of that interaction.

What Distinguishes the Survivors and Victors?

Most of the European efforts to build states failed. The enormous majority of the political units which were around to bid for autonomy and strength in 1500 disappeared in the next few centuries, smashed or absorbed by other states-in-the-making. The substantial majority of the units which got so far as to acquire a recognizable existence as states during those centuries still disappeared. And of the handful which survived or emerged into the nineteenth century

as autonomous states, only a few operated effectively—regardless of what criterion of effectiveness we employ. The disproportionate distribution of success and failure puts us in the unpleasant situation of dealing with an experience in which most of the cases are negative, while only the positive cases are well-documented.

Yet we ask what distinguishes the positive cases from the rest. We must at least raise the question; we mean to confront available general ideas about state-making, and those ideas have to do mostly with success and failure. Unfortunately, most of the available ideas straddle the level of generality at which we are attempting to work: either abstracting so greatly that the propositions come close to being matters of definition, or staying so close to one historical model that all other cases become, in their own ways, failures. Nevertheless, it is clear enough to what features of political experience most of the available theories call our attention. By and large we get the image of two rather autonomous clusters of processes: the first cluster variously called social change, modernization or social and economic development, the second cluster labeled political modernization, political development, or even nation-building. Obviously the old division between state and society has gone into motion. The two clusters of processes are supposed to interact, disrupting or facilitating each other. Thus the managers of the political structure face two sorts of "problems": (1) coping with the difficulties produced by social change; and (2) directing social change toward some desired set of outcomes. Performance in these two regards (which emphatically include the survival of the government itself) then provides the criteria of success or failure.

The disagreements within this predominant tradition of theorizing generally concern the direction of causal connections between the "social" and "political" processes (to what extent does change in the government simply reflect change in social structure?), the governmental arrangements most likely to produce the two kinds of success, the degree to which each of the two clusters of processes follows the same path in every country, and, alas, the proper terminology for discussing all these difficult questions. Beneath the surface a debate about the proper and possible roles of government also continues. To my mind, the most interesting thing about this way of setting up the inquiry is its perfect consistency with the world-view of the high administrative official. There it is: an outside world to be coped with or transformed by means of governmental instruments,

themselves subject to improvement or decay. No doubt that is the phenomenology most compatible with the aspiration to wring state-making instructions for the present from the experience of the past.

The main questions I am raising here, however, are somewhat less instrumental and somewhat more naturalistic. At each point in time from 1500 onward, what features of a political unit would have permitted us to anticipate whether it would (1) survive into the following period as a distinct unit; (2) undergo territorial consolidation, centralization, differentiation of the instruments of government from other sorts of organization and monopolization of the means of coercion; (3) become the nucleus of a national state? What features of a population would have permitted us to make the same kinds of predictions about the political units claiming jurisdiction over it? In general, it seems to me, the answers have to do with whether the managers of the political units undertook activities which were expensive in goods and manpower, and built an apparatus which effectively drew the necessary resources from the local population and checked the population's efforts to resist that extraction of resources. Such a statement, to be sure, simply shoves many of the more interesting questions back one step. (It sidesteps, for example, the problem of how England grew powerful and survived into the nineteenth century with a relatively puny central structure.) Nevertheless, to the degree that the statement is correct we should only consider the form of representation, the geopolitical position, the culture of the local population or any number of other factors in so far as they affect the propensity of political units to carry on expensive activities and the ability of those units to extract the essential resources from the population.

The individual papers in this volume contain a number of observations on these very questions. I shall itemize the salient arguments which emerge from the papers later in this chapter. Here I only want to mention the most general conditions which appear, in the European experience, to predict survival and state-making: (1) the availability of extractible resources; (2) a relatively protected position in time and space; (3) a continuous supply of political entrepreneurs; (4) success in war; (5) homogeneity (and homogenization) of the subject population; (6) strong coalitions of the central power with major segments of the landed elite. A high standing on one of the factors can make up for a low standing on another. Brandenburg-Prussia, for example, began with an impoverished territory,

but its succession of political entrepreneurs squeezed every available resource from the population. France, on the other hand, went through several shortages of entrepreneurial talent at the center, but was able to draw continuously on a rich and populous territory.

The availability of extractible resources is hard to disentangle from the actual fact of their extraction. The value of resources depends on their use, and their use depends on their availability. Nevertheless it is safe to say that the chance to tax goods passing through the Sound gave Danish state-making a great, if temporary, boost; that Spain built a substantial state apparatus with American silver before its seventeenth-century slowdown; and that the levying of customs duties on wool eased the English way to a durable national state.

The "protected position in time and space" generally distinguished the Scandinavian powers from the principalities of Germany—although any such generalization depends on where and when *other* concentrations of power are growing up; in the time of Swedish expansion, the other Scandinavian states had a precarious hold on their existence. For much of the time under consideration here the Poles and the Austrian Hapsburgs found themselves in exposed conditions, while Portugal had only her neighbor (and sometime spouse) Spain to worry about. In any case, the political units which were vulnerable to military attack and/or adjacent to expanding states had great difficulty in maintaining their autonomy and in keeping their dissident populations under control.

Where a single dynasty exercises strong claims on the crown, a continuous supply of political entrepreneurs may depend on fertility and genetic endowment. One can make a case that the mediocrity of the later dukes of Burgundy destroyed the real opportunity for a Burgundian kingdom to the northeast of France, or that Spain simply ran into an ungifted string of kings in the seventeenth century. If so, these are not "accidents," but more or less foreseeable consequences of relying exclusively on inheritance to fill the entrepreneurial positions. Royal inheritance was not, for that matter, an immutable fact of life. We ought to remember that the English contrived to change their ruling family three or four times in the seventeenth century; as late as the accession of George I (1714), there was still a real chance that the magnates would intervene to produce a more acceptable king. In any case, over the whole European experience the ability to recruit talented ministers like a Cromwell, a

Colbert, a Pombal, or a Cavour was probably more important than the gene pool of the royal house. And that was no accident.

Success in war will receive a good deal of attention later in this book. I call attention to it now for more than the trivial reason that most of the political units which disappeared perished in war. The building of an effective military machine imposed a heavy burden on the population involved: taxes, conscription, requisitions, and more. The very act of building it—when it worked—produced arrangements which could deliver resources to the government for other purposes. (Thus almost all the major European taxes began as "extraordinary levies" earmarked for particular wars, and became routine sources of governmental revenue.) It produced the means of enforcing the government's will over stiff resistance: the army. It tended, indeed, to promote territorial consolidation, centralization, differentiation of the instruments of government and monopolization of the means of coercion, all the fundamental state-making processes. War made the state, and the state made war.

The homogeneity of the subject population was, by contrast, no more than a contributing factor. The presence of a culturally homogeneous population no doubt lowered the cost of state-making by making uniform administrative arrangements feasible, by promoting loyalty and solidarity of the subject population (so long as the managers of the state belonged to the same culture), and by putting ready-made communication systems at the disposal of the rulers. Joseph Strayer makes the important distinction between the unitary state and the mosaic state: the first, like England, forming by a process which leaves no significant provincial liberties; the second, like France, reflecting the successive absorption of areas with distinct traditions and political institutions in the persistence of variable law and variable administration. (Great Britain, by drawing Scotland and Ireland under the control of a single crown, became more of a mosaic state, but only after its English core had produced quite a sturdy state apparatus.) Strayer gives even greater emphasis to the distinction between the state formed of a single *regnum* (a population which acknowledged its attachment to some particular royal family in the period after the collapse of the Roman Empire) and the state which was only a fragment of such a *regnum*. He argues:

> Both these differences are important in the next stage—changing the state into a nation. Where a whole *regnum* became a

state, nationalism developed early and naturally, with no great strain or exaggerated emotional appeals. In such a state, people were gradually brought into closer and closer association with each other. The ringwall of the state cut them off, to some extent, from the rest of the world; they were forced to work together and to adapt to each other. They had time to gain a clear sense of identity, to smooth out some of their regional differences, and to become attached to their ruler and the institutions through which he ruled. Where the framework of the state was strong enough and persistent enough, it even created a common nationalism out of very different linguistic and cultural groups. Languedoc was very like Catalonia and very unlike north France, yet it finally became thoroughly French.

It is also clear that the unitary state had an advantage over the "mosaic" state. The central government of a unitary state did not have to worry about provincial privileges, nor did it have to create a huge, and often unpopular, bureaucracy to coordinate and control diverse and quarrelsome local authorities. Local leaders did not have to be looked on with suspicion as men whose primary loyalty was to their province. Instead, they could be used to explain and adapt the government's program to their communities. They gradually began to think in terms of the national interest, because there were no provincial interests to distract their attention. Common laws and common institutions created a greater sense of identity than there was in countries where a man from one province could not understand the governmental procedures of a neighboring province (Strayer 1971: 346–347).

As compared with the accounts you will find later in this book, Strayer's analysis has rather a lot of consensus and rather little coercion in the process by which states formed. Once translated into statements about the costs of securing compliance under varying conditions of homogeneity, however, Strayer's summary fits neatly with the line of argument we pursue.

Over and above the early homogeneity of the population subject to a particular political unit, the unit's success or failure of homogenizing its population also affected the likelihood that it would become and remain an autonomous state. Almost all European governments eventually took steps which homogenized their populations:

the adoption of state religions, expulsion of minorities like the Moors and the Jews, institution of a national language, eventually the organization of mass public instruction. The tolerance of the states of Southeastern Europe for linguistic, cultural, and religious diversity stood in sharp contrast to the intolerance of their Northwestern brethren, and surely stood in the way of effective state-making. The failure to homogenize increased the likelihood that a state existing at a given point in time would fragment into its cultural subdivisions at some time in the future.

Finally, the favorable effects on state-making of strong coalitions between the central power and major segments of the landed elite resulted from a simple reality we discussed earlier: the great predominance of peasants in the European population. The predominance of peasantry meant (1) that the bulk of the resources which might be available for the building of states were committed to the land in one way or another; (2) that control over that land was widely dispersed; and (3) that landlords—especially landlords who wanted reinforcement in their efforts to coerce the local peasantry—became indispensable allies and formidable enemies in the effort to tax, conscript, and requisition.

This summary does not in the least contradict the fact that the Prussian electors, the French kings, and most other European sovereigns had to invest a great deal of their energy in struggles *against* great magnates and corporate bodies of the nobility. The electors, after all, helped create a division of labor between Junkers and bureaucrats (at the expense of peasants and townsmen alike) which carried on into the nineteenth century; according to Hans Rosenberg (1958) and Barrington Moore (1966), that division of labor became the key to the authoritarian Prussian state. Where no strong coalition formed and where the landlords completely outweighed the crown—Spain and Poland are two likely examples—the work of state-making tended to halt, or crumble.

At least one big thing is missing from this preliminary inventory of factors promoting the survival of some states and the disappearance of others. That is the international context of any particular state's emergence and growth. I see two sides to the problem: (1) the changing structure of the European economy as a whole; and (2) the emergence and evolution of an international system of states.

Immanuel Wallerstein has done a superb analysis of the way what he calls the "European world-economy" came into being from the

mid-fifteenth to the mid-seventeenth centuries (Wallerstein 1974). He points to the emergence in that period of a large-scale economic division of labor, extending outside of Europe to the Americas and elsewhere. Within that world economy, by his account, a small and changing set of core states dominated the commercial transactions, did the lion's share of the manufacturing, and pushed the peripheral areas toward large-scale monoculture. In the process, the once-thriving manufacturing and commerce of the periphery dwindled, sometimes to nothingness; even splendid Venice declined. Formerly tributary areas like England became central.

Wallerstein points out that the formation of the European world economy was exceptional, perhaps unique, in not leading to a single *imperium* of the sort which had come to govern the great economic spaces of Rome, Byzantium, and China. Instead, multiple states, at least partly independent of each other, sprang up. This fact was in itself very likely crucial for the development of capitalism, since it reduced the capacity of any particular government to capture (or smother) the arrangements of investment, lending, production, or distribution. Moreover, state-making efforts at the center of this emerging world-economy, fed by the flow of resources from the periphery and eased by the monetization of economic life around the center, had a far greater chance of success than those at the periphery. The correspondence is imperfect: Holland stood at the middle of the entire process, and yet did not mount a particularly strong state; the commercialized regions of western Germany resisted incorporation into substantial states; nor is it clear that the sixteenth-century eminence of Spain depended on its being a "core state" in the same sense that England was. Still the position of any population or any political unit within the world economy deeply affected its prospects for state-making.

The second international process which my analysis has so far neglected is the crystallization of a *system* of states acknowledging, and to some extent guaranteeing, each other's existence. Perhaps the Treaty of Westphalia (1648), at the close of the Thirty Years War, first made it clear that all of Europe was to be divided into distinct and sovereign states whose boundaries were defined by international agreement. Over the next three hundred years the Europeans and their descendants managed to impose that state system on the entire world. The recent wave of decolonization has almost completed the mapping of the globe into that system.

The filling-in of the state system significantly constrained the later participants in European state-making. Whereas the territory eventually to be controlled by the Prussia or the Spain of the fifteenth century was still quite open, the maximum space—at least within Europe—which a united Italy could claim in the nineteenth century was essentially set by the outcome of the Napoleonic Wars. Furthermore, the new states came increasingly to form as consequences of wars among established members of the state system and of the negotiations which ended those wars. The Treaty of Westphalia, the Congress of Vienna, and the Treaty of Versailles provide increasingly dramatic demonstrations of the point. As a consequence, the later the state-making experience under examination, the less likely the sorts of internal processes I emphasized earlier are to provide an adequate explanation of the formation, survival or growth of a state.

The Tasks of Our Papers

Still, we have to get those internal processes right. They absorb most of the attention of the essays in this book. Our authors, true to their commissions, have touched on a great variety of questions, each in his own way. Some, like Bayley, have strongly emphasized the systematic, structural questions in a manner clearly influenced by the style of comparative politics. Others, like Braun, have insisted on the byways, particularities, and continuities in a manner just as clearly instilled by historical training. As a result, any attempt to array the papers as if they were repeated answers to the same insistent question will be presumptuous and unfair. Presumptuous, because of the pretense that one knows better than the authors what they were finding out. Unfair, because of the sacrifice of each paper's separate richness and coherence.

Nevertheless, my attempts at synthesis follow just such an unfair, presumptuous course. Here I attempt to see what sorts of general conclusions concerning the ways national states actually formed in Western Europe emerge from our varied essays. In the final chapter, I try to treat the same material more analytically, asking what general statements about the formation of national states might follow if the European experience were the universal one, and how those statements jibe with existing theories of large-scale political change. Although the two essays overlap, the first proceeds mainly from the perspective of history, the second mainly from the perspective of political science. In either case, the best we can hope for is a set of pro-

visional conclusions: more solid than working hypotheses, perhaps, but less firm than theses one nails up, to challenge all the world, at the end of a long inquiry.

Our authors claim no more. They often have to take sides on questions which they know to be still undecided—as when Finer commits himself to the view that the Brandenburg Recess of 1653 in itself "was to have the most far-reaching consequences for the future polity and to set a distinctive stamp upon the military format until the defeat at Jena in 1806; and in many respects till well on into the third quarter of the nineteenth century." In so saying, Finer commits himself on an open question. He separates himself to some extent from an argument like Barrington Moore's, where 1653 appears as only one of many incidents in the jostling and balancing of the urban commercial classes, the great landlords and the crown in Brandenburg-Prussia.

None of us claims his arguments or evidence will themselves decide the issue. Each of us concedes that if he has adopted a mistaken position on one of the great questions, his more immediate arguments will be weakened as well. My own analysis of food supply, for example, adopts the standard arguments that influential English landlords of the seventeenth and especially the eighteenth centuries were actively involved in the export of grain and pushing the government toward a policy of favoring exports even to the short-run detriment of English consumers. That could be wrong. To the extent that it *is* wrong, my tracing of variations in national food policy to the major class alliances formed by the crown loses plausibility. So it is with a great many positions the other authors have adopted provisionally, and with trepidation.

If the papers in this book have value, it will not be due to the new general explanations they offer for the rise of capitalism or the decline of Spain. Instead, their originality lies in their emphasis on what Gabriel Ardant calls the "physiology" of state-making: how the builders of states actually performed, or tried to perform, the extractive, coercive, and coordinative side of their work. This emphasis on mechanisms draws attention away from the forms of states and the broadest ends of state-making toward the implications of alternative public policies. At many points, our authors are writing as if the years after 1750 or 1800 had not yet occurred, as if the choices were still open, as if they were calculating the probable consequences of one policy or another on the basis of a more powerful analytic sys-

tem than was actually available to anyone at the time. They generally opt, that is, for a probabilistic analysis over a deterministic one, for the study of relationships over the study of recurrent events or sequences, and for a prospective rather than a retrospective view of state-making processes.

Some Biases of the Papers

From the prospective point of view, however, the papers have some significant biases. They are heavily weighted toward whole states, and big states like France and Spain at that. They make it easier for us to place ourselves in the seats of power in Versailles or Madrid than to reconstruct the calculations of an Andalusian landlord or of a German princeling. They are heavily weighted toward the states which survived past the eighteenth century: England, Prussia, and Sweden rather than Bohemia, Scotland, Lorraine, or even Naples. And they are somewhat weighted toward *features* of states which were visible within the last century: bureaucracy rather than the sale of offices, mass professional armies rather than militias, specialized police forces rather than posses. If venality, militias, and posses were not simply the fading features of the old regime, but the intermediate institutions which were crucial to the emergence of the states we know, our analyses will tend to misrepresent the developmental processes which created and then destroyed them.

I do not mean these remarks as attacks on our authors. These biases result from the original charges we gave them: to compare the carrying out of some particular state-making activity in several major European countries over a substantial segment of the period since 1500. These biases toward what survived, furthermore, pervade the existing historical literature. To some extent, they even pervade the sources themselves. Whatever their origin, the biases mean that we have only been partly successful in our aim of creating a prospective analysis of state-making processes.

The original agenda also gave little weight to several institutions and processes which our papers disclose as crucial to the phenomena we self-consciously set out to study. Churches and religious organization should have received more direct attention from the start, for two reasons: (1) churches and churchmen were significant political actors at the national and international levels throughout most of the period we are examining; at times they comprised the most formidable rivals, allies, enemies, or instruments of the great state-makers;

and (2) for several centuries of our era, nationalism and antinationalism alike customarily wore the mantle of religious faith; great states like France and the Netherlands were rent by struggles which inextricably combined religion and politics. For those reasons, control of belief and devotion should probably have been on our initial agenda.

Likewise, linguistic and cultural policies deserve more direct attention than our general plan allotted them. If Stein Rokkan is right in asserting that early cultural homogeneity within a territory strongly facilitated the emergence of a durable state, that its absence significantly limited the capacity of any state to mobilize its resources, and that the successful European states generally engaged in a deliberate program of homogenization, we have slighted one of the major dimensions of our problem, and one of the most interesting points of contact between the early history of Europe and the current experience of the world's many multicultural states.

I mentioned earlier another omission which we often noticed in our discussions: the administration of law and justice. In passing, the papers on armies, taxation, police, and control of food supply inevitably make judgments on law and justice. But none of them deliberately treats in its own terms the use of courts, lawmakers, punitive power and legal jurisdictions as a means of extending the power of the state. The result, I fear, is to make less clear than need be the close relationship between policing and the other processes of control and extraction we analyze. Bayley's argument linking changes in the organization of policing to the emergence of new challengers to the existing distribution of power points in that direction. It does not quite bring out the extent to which European states used their legal apparatus not merely to hold off threats to public order, but to define "disorder," create "disorder," and press their right to suppress the same "disorder."

Finally, an omission which now seems odd indeed. None of our papers contains a sustained discussion of the general administrative structure of the various European states, or of their changes over time. Finer's paper naturally says a great deal about the organization of armies, Braun's plenty about the personnel of fiscal administration, and so on. Fischer and Lundgreen are specifically concerned with the ways Europeans set, judged and acquired the qualifications for office. Yet just as we tend to assume knowledge of the judicial system, we also tend to take the general administrative organization

for granted. This weakness, too, flows from our original setting of the problem; we emphasized function rather than form. Since one of our recurrent discoveries has been the constraint set on emerging functions by existing forms, we pay a substantial price for that emphasis.

All this means that our papers fall far short of presenting a comprehensive or balanced portrayal of the whole European state-building process; that was never our purpose. The countries, periods, institutions and processes under study are slanted toward those which yield the clearest pictures of effective extraction, coercion, and control on the large scale. The advantage of that selection is to reveal subtle relationships between different kinds of extraction, coercion, and control; Finer's neat formulation of "cycles" of extraction, economy-technology, etc. lays out the interdependencies more deliberately than the other arguments do, but every paper identifies relationships of this kind. Taxation, for example, ordinarily shows up in European histories (not to mention theories of political development) as an epiphenomenon—and a rather uninteresting one at that. Guided by Ardant and Braun, we begin to see how crucial the form and effectiveness of taxation were to the military strength of different European states, on the one hand, and to the likelihood of mass rebellion against those states, on the other.

Again, specialized policing often seems to be a more or less automatic response to individual or collective threats against public order—a response whose vigor and effectiveness vary from one setting to another, but whose character is pretty much set by the technical problem of controlling criminals and rioters. Bayley shows the differentiation of police forces in the contemporary sense from the much wider range of activities and personnel implied by the German *Polizei* or the Italian *polizia*. He shows that the degree of differentiation varied considerably from country to country, in response to the domestic political situation. Most importantly, he shows (or rather argues, since the hypothesis is fresh and the evidence far from tabulated) that changes in the format of policing depended closely on the national elite's responses to the perceived threats of new groups making bids for power from outside the political system. The analyses of taxation and of policing illustrate very well what it means to analyze state-making as a series of relationships, rather than as a set of recurrent events or of standard sequences.

A Review

Before examining the entire set of papers for major relationships, let me review them individually for the main themes the authors themselves have stressed. That will, perhaps, relieve the authors of the blame for later misapplications of the conclusions.

FINER ON MILITARY FORCES

S. E. Finer traces the relationship between the development of major features of the modern state (territorial consolidation, specialized personnel, integrity recognized by other states), its special case the modern nation-state (which adds self-consciousness of common identity, as well as some mutual distribution and sharing of duties and benefits) and the changing character of national armed forces (especially as summed up by "format," which includes basis of service, size composition and stratification). His principal method is to compare the transformations of France, Britain, and Prussia-Germany in both regards, arguing causal connections by comparison. And his chief conceptual device is the identification of clusters of variables, or "cycles": (1) economy-technology-format; (2) stratification-format; (3) beliefs-format; (4) format options; (5) extractin-coercion; and (6) state-building. Having identified the cycles in this way, he then forms his argument as a series of statements about links among the cycles.

Within this complicated chainwork, Finer lays particular emphasis on the first cycle, economy-technology-format. He shows how each major military invention over a thousand years (most of the earlier "inventions" being organizational and tactical rather than technical in any narrow sense) drove those rulers who wished to continue the pursuit of their objectives by force of arms to push their subject populations harder and more continuously for the necessary resources. Those who succeeded used their armed strength to consolidate their control of a territorial base as well as to assure external acceptance of that control; since they characteristically promoted the formation of specialized personnel for the performance of these tasks, the military enterprise played a major part in the creation of all three distinctive signs of the modern state: territorial consolidation, specialized personnel, recognized integrity. Many failed; their states or protostates fell into rebellion, disintegration, or absorption by others. For the survivors, instead of a continuous accretion of central power, we witness an alternation of long pauses with giant

steps closely following changes in military technology and the scale of war.

Finer is no narrow technological determinist. He sees the political effects of military reorganization as conditioned by the extent and accessibility of resources within the subject population, on the one hand, the degree of popular commitment to the national cause, on the other. This comes out clearly in his discussion of the mass mobilization called for by Napoleonic warfare, and of increasing demands of war since the time of Napoleon. It is also precisely at this point that Finer brings out most sharply another argument which lies hidden through most of the paper: that the state-makers were actually building an interlocking *system* of states: general wars became the principal means by which the realignments of the participants and their boundaries occurred, the principal moments at which multiple changes of membership and alliance occurred, as well as principal occasions on which the relations between rulers and ruled changed rapidly. The French Revolution, the Napoleonic Wars, and the Congress of Vienna bring out these interdependent effects most fully and dramatically. The virtue of Finer's analysis is to show that 1789–1815 was no odd exception to the long run of European experience. That conclusion brings a tragic tone to his final passages. For it comes close to saying that war is the characteristic condition, and armed force the characteristic instrument, of the state system.

ARDANT ON FINANCIAL POLICY AND ECONOMIC INFRASTRUCTURE

Gabriel Ardant is far from an incurable optimist, but he is more intent than Finer on drawing practical lessons from the past. He asks himself which financial policies will work under what social conditions, with what social costs. In his analysis, financial policy centers on taxation, but includes monetary regulation, public credit, and the whole range of procedures by which governments control their revenues. Obviously these procedures intertwine with those by which governments attempt to intervene in the economies on which they are based—income policies, incentives for investment, and so on. Ardant is interested in them mainly in so far as they contribute to the viability of the state itself, and to its ability to pursue the goals its rulers have set for it. The social conditions to which he attaches greatest importance are the forms of production and marketing. And

the social costs he considers most seriously are political ones: the impositions of arbitrary rule, on the one hand, the outbreak of bloody rebellion, on the other.

At its simplest moments, Ardant's model of the state contains only a handful of variables: (1) a set of expensive goals articulated and pursued by the rulers of a state; (2) a bundle of financial policies designed to draw the resources necessary for the pursuit of those goals from the population under the state's control; (3) an economic infrastructure producing and containing such resources as the population has at its disposal; (4) financial consequences of applying a particular set of policies to a particular sort of infrastructure; and (5) social and political consequences of variables 2 (policies) and 4 (financial consequences), including the extent to which the rulers' goals were actually accomplished.

Schematically, the model is seen in Figure 1–1.

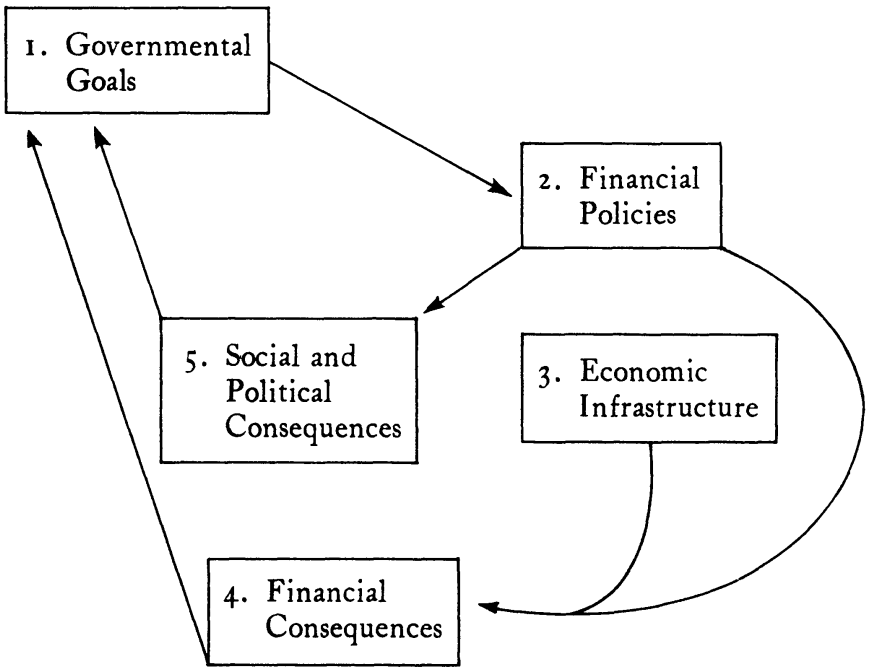

Figure 1–1. Ardant's Model of a State

In his other works, Ardant has paid considerable attention to the impact of financial policy on economic infrastructure, for example, through the tendency of taxation in money to drive self-sufficient peasants into the market. Here, that connection remains almost untouched. Again he is aware of the way that the existing economic infrastructure affects governmental goals; the essay in this volume does not deal systematically with that relationship. The diagram rep-

resents the causal sequence he stresses in his argument: from goals to policies to consequences, and then back again to goals.

For purposes of analysis, Ardant generally takes the goals and the economic infrastructure as given. What interests him is the effect of matching or mismatching financial policy with infrastructure. Ordinarily he considers a match to occur when the policy brings the state a high return over the medium and long runs without deterring production. To be sure, he insists that fiscal systems defined and limited by public law work better than capricious force does. With Balzac (in *Cousine Bette*) we hear him saying, "L'arbitraire, c'est la démence du pouvoir" ("arbitrary rule is power gone mad"). He also thinks that in the long run an egalitarian and redistributive policy is not only more just but more effective. However, neither belief appears to be essential to his main argument.

The argument centers on the assertion that a policy apportioning fiscal obligations according to access to wealth tends to produce a high return at low political, social and economic cost to the extent that (1) marketing is extensive and intensive; (2) the fiscal rules are known, binding on the state and widely considered to be equitable; and (3) the economy is productive. Much of Ardant's historical analysis consists of comparing England, Prussia, the Hapsburg possessions and France in these regards: comparing them with the ideal, with themselves at other points in time, and with each other. France is the prime case under discussion, although at many points England provides the standard against which the others are measured. Only at the end does it become clear that the real point of the comparative analysis is to produce lessons for the guidance of contemporary states.

Nevertheless, Ardant makes several fundamental historical observations on his way to the lessons. The first, which only comes *en passant*, echoes Finer: the largest and most persistent stimulus to increases or changes in national fiscal burdens over the great period of European state-making was the effort to build armed forces and wage war. The second observation goes beyond Finer: over that same period, the most serious and persistent precipitant of violent conflicts between European state-makers and the populations they attempted to rule were attempts to collect taxes. Third, in the early period of state-making the most important single spur to the expansion and reorganization of officialdom was very likely the effort to

collect taxes from a recalcitrant population; the less extensive the market, the more heterogeneous the population and the greater the demand for revenues relative to total production, in Ardant's analysis, the greater the bulk of the governmental apparatus created in the process. Finally, and most controversially, the pressure to extend the suffrage, increase national consciousness, give representation to the working classes, and generally draw the bulk of the national population into political life, which so marked the nineteenth century in Europe, came to an important degree from the fiscal demands of the great military and administrative machines brought into being by the Napoleonic Wars. We shall return to some of these conclusions later on.

BRAUN ON TAXATION AND SOCIO-POLITICAL STRUCTURE

Rudolf Braun adopts a rather different approach to taxation from that of Gabriel Ardant. He devotes himself more assiduously to the construction of a continuous account of fiscal policies in England and Brandenburg-Prussia from the sixteenth century onward. He is much more concerned to pin down the differences between the two countries, and to explain them in historical terms. Whereas Ardant emphasizes the obstacles to the success of different kinds of fiscal policies and the respects in which the economic infrastructure constrained the system of taxation, Braun usually begins with the political situation which led the British or German rulers to adopt a given set of policies at a given time, then takes the existence of that set of policies as a fact whose long-run consequences become the focus of historical investigation.

The trouble with conducting such an analysis through the comparison of two countries is that such a large number of other differences between them are plausible candidates for the explanation of the differences which interest the analyst. In the contrast between the economic histories of England and Brandenburg-Prussia, we have a larger number of interesting candidates than we can possibly handle: the power of the Junkers in Prussia, the greater urbanity of England, the nature of war on the Continent and in England, the variety of agricultural regions within England, the wool trade versus the grain trade, the enclosures versus the "new serfdom," and so on. Given this complexity, one can try to eliminate potentially crucial variables one at a time by intensifying the comparisons and increas-

ing their range in time and space; or one can try to detect differences in the configurations of the variables. Braun chooses the configurations.

He pays particular attention to three classes of long-run consequences of fiscal policy: (1) the kind of national administrative structure it created; (2) its influence on patterns of political power and participation; (3) its impact on economic growth. In this analysis, he deals with the third variable as an outcome of the first two; the direct influence of alternative systems of taxation on investment, saving, factor mobility, etc., hardly figures in his analysis. (Elsewhere, I hasten to add, Braun has shown that he is splendidly capable of dealing with the impact of economic growth on administrative structure, political power and participation.) We get a picture of a Prussian state smothering the economy with its bulk and strangling the economy with its extractiveness. The English state, to oversimplify Braun's subtle account, escaped its evolution in the same direction through a civil war which destroyed the prospering royal bureaucracy.

Here we come upon an historical point which recurs elsewhere in our papers. With the later Tudors and the Stuarts, the English crown was acquiring strength and bulk in ways which resembled the state-building of the continental countries. Although J. P. Cooper complains about the tendency to assimilate English kingship of the early seventeenth century to the absolutism of Spain or Brandenburg-Prussia, he eventually suggests

> that England was well aware of continental political patterns and there were at least the possibilities of similar developments under the early Stuarts; even the parliamentary sphere, if the attempts of Salisbury and Cranfield to solve the crown's problems had been successful, the similarities with other assemblies might have been greater. But in fact the Civil War, though in some ways a reflection of forces generally at work in Europe, finally consolidated a pattern very different from that of most European states (Cooper 1960: 90).

The Civil War consolidated a pattern of government by Parliament in alliance with the notables of city and country. But in the previous century, the ability of the Tudors to draw substantial returns from their own domains had reduced their dependence on Parliament,

just as the Hohenzollern monarchs were later to benefit politically from their ability to draw 40 or 50 percent of the government's revenues directly from the royal domain. This partial independence became crucial to royal power; Estates and Parliaments everywhere in Europe used their approval or disapproval of taxes (uncollectable without their collaboration until and unless the crown disposed independently of strong military forces) to secure ratification of their own privileges and to resist the penetration of the royal bureaucracy into their own spheres of influence. As Braun shows, that process went on both in Britain and in Brandenburg-Prussia. Even in autocratic Prussia, the eminent domain of the crown tended to stop at the gates of the Junker estate; the institution of the rural commissioner (*Landrat*) only nominally incorporated the nobility into the vast centralized structure of government.

This way of putting it, however, slightly miscasts Braun's analysis. It suggests a royal authority alone against the world, when in both Britain and Brandenburg-Prussia the class alliances of the crown (and against the crown) were crucial. The tracing of fiscal policy in Brandenburg-Prussia permits us to see the great landlords allying with electors and kings to milk the weakened cities in one way, exploit the helpless peasantry in another way, reinforce their privileged position in a whole variety of ways. For all the wider base of taxation in England, there we find the great English landlords using their influence in the counties to tilt assessments of the land tax away from themselves, and their influence in Parliament to assure such measures as the bounty on exports of grain.

Braun barely mentions another feature of the early modern states that resulted from the search for expedients and alliances which all monarchs had to pursue: what appears (in our virtuous retrospect) to be their extensive corruption and waste. H. R. Trevor-Roper offers a collective sketch:

> So "the Renaissance State" consisted, at bottom, of an ever-expanding bureaucracy which, though at first a working bureaucracy, had by the end of the sixteenth century become a parasitic bureaucracy; and this ever-expanding bureaucracy was sustained on a equally expanding margin of "waste": waste which lay between the taxes imposed on the subject and the revenue collected by the Crown. Since the Crown could not afford an absolute loss of revenue, it is clear that this expansion of the

> waste had to be at the expense of society. It is equally clear that it could be borne only if society itself were expanding in wealth and numbers (Trevor-Roper 1967: 68).

The waste included the open magnificence of the Renaissance court, the conduct of costlier wars, and the ever-present payoffs, bribes, sinecures, or outright thefts which drained away the payments theoretically exacted for public ends.

Trevor-Roper concludes that the Renaissance state destroyed itself in the seventeenth century by pushing its exactions beyond any capacity of the population to support them. That argument dovetails nicely with Ardant's insistence on the centrality of fiscal questions in the frequent rebellions of the seventeenth century. Then Braun and Ardant alike part company with Trevor-Roper. They consider the farming of taxes, the sale of offices, the private handling of royal loans, the enrichment of favorites and financiers to have been almost inescapable consequences of the search for expedients to extend the monarch's power. They attach a relatively small quantitative role to conspicuous consumption as such, and trace the greatest demand for new funds back to the royal efforts to wage war and strengthen their armed forces. The war-making efforts of the Hohenzollerns, the weaker economic base of eastern Germany, the established power of the Junkers combined to produce a far greater extractive apparatus in Brandenburg-Prussia—the coupling of the War State with the Police State.

BAYLEY ON POLICE

That is the point of contact between the studies of taxation and David Bayley's analysis of policing. This historical junction is not overly clear, because Bayley concentrates on the period during which differentiated, professional police forces were forming in European states: the nineteenth and twentieth centuries. Our analysts of taxation have much more to say about the seventeenth and eighteenth centuries. What is more, Bayley takes a deliberatively retrospective view of his problem; he asks himself what accounts for national differences in police forces which are visible in our own times. None of our other authors concentrates so much on present-day outcomes. Finally, Bayley lays out his analysis in a timeless propositional style, relating specific dependent variables—the scope of police tasks, the structure of police forces, the nature of accountability, the

within their territories and on the general approaches to state-making they had developed; in both regards, the forms of alliance among state officials, landlords and merchants were crucial. In their turn, the choices of food policies cemented the class coalitions with which the state entered the modern era, affected the success of its fiscal policy by accelerating or impeding the commercialization of agriculture, augmented or diminished the bulk of the national bureaucracy, promoted or hindered economic growth, and governed the form and severity of conflicts over control of the food supply.

The cumulative weight of all these choices produced striking national differences in patterns of regulation. Toward the end of the eighteenth century we can see an England far on the road to a national market and to agrarian capitalism, entrusting such regulation of the domestic market as still went on to the justices of the peace, and giving subsidies to exporters of grain. By contrast, we see a Spain enmeshed in a great web of protective regulation, a France striving energetically but inconsistently to "free the grain trade" and assure deliveries to its great cities, a Brandenburg-Prussia with an enormous regulative machinery geared to the supply of the armed forces. Over the very long run, we can also see what appears to be a general trend in the European state-building experience: a movement away from relatively similar forms of organization in the fourteenth or fifteenth century to a great diversity of solutions in the seventeenth and eighteenth centuries, followed by a certain amount of convergence on the same standard forms of mechanisms during the nineteenth and twentieth centuries.

FISCHER AND LUNDGREEN ON ADMINISTRATIVE AND TECHNICAL PERSONNEL

That double movement of early divergence and late convergence also appears in the recruitment and training of technical personnel, as portrayed by Fischer and Lundgreen. In comparing the experiences of Britain, France, and Prussia over the entire period since the Middle Ages, they discover an evolution from relatively similar arrangements of personnel among the protostates, to increasing national differences in solutions to broadly similar problems during the epic phases of state-making, to extensive borrowing and standardization of organization in the bureaucratic expansion of the last century or so. Fischer and Lundgreen interpret the term "technical personnel" broadly. So they must, to deal with the changing salience of dif-

ferent sorts of attributes and skills over the centuries they survey. At one time or another, all European state-makers had to create, co-opt or subjugate judges, soldiers, local administrators, tax collectors, engineers, accountants, spies, demographers, scribes, sea captains, bankers, policemen, and propagandists. Which mattered most changed from one decade to the next.

Fischer and Lundgreen insist on the dilemma of loyalty versus effectiveness. Many governments have had servants who were neither effective nor loyal. A few have had servants who were both effective and loyal. Yet the two virtues do not necessarily coincide, especially in the short run. And the government which does not assure itself a minimum of both does not survive.

In the earlier phases of European state-making, loyalty (at least in the sense of some guarantee that officials would not appropriate all the resources entrusted to them, and would not become the ruler's rivals) posed the more pressing problems. That was true for two main reasons: (1) every aspirant builder of a strong and autonomous state in Europe found himself surrounded by serious rivals, both inside and outside the territory currently under his control; his dissatisfied underlings could often find better allies elsewhere; (2) the early rulers had little autonomous coercive power at their disposal; they could not hope to raise armies, collect taxes, or even maintain their dynasties without the assent of powerful organizations which even in principle were largely independent of the ruler. These conditions affect every government to some degree; they constituted the central facts of political life for the early European state-makers.

This situation, as Fischer and Lundgreen say, helps explain the prominence of churchmen as political aides to the early state-makers. It is not just that the church monopolized literacy, legal acumen and formal education. Clerics were useful partners because they brought some of the resources of Europe's largest and wealthiest organization to bear on their work; they found it more difficult than did their brothers the soldiers and landlords to convert the state resources under their control into transmissible private property. They were also less apt to build their own rival dynasties. (These are relative matters; the Borgias did, of course, supply more than their share of bishops and popes; Cardinal Mazarin did, indeed, die wealthy, with his kinsmen well-placed.) However, alliances with churchmen and the church eventually placed serious limits on kingly autonomy. With

the obvious exception of the Papal States, all the European states shifted decisively toward lay administrators as their autonomous coercive power increased.

Yet the dilemma of loyalty and effectiveness persisted. Loyalty took a long time to become commonplace. Fischer and Lundgreen suggest that most of the salient features of official recruitment during the great periods of state-making in Britain, France and Prussia resulted from more or less deliberate compromises between the two principles. The sale of offices occurred widely in all three countries, especially with regard to military commissions; the distinction of France was to have used that sale as an important source of government revenue, instead of simply tolerating traffic in offices among private individuals. Although the venality of offices diminished the ability of the state to assign positions according to technical competence, it also expanded the number of people who had a lively interest in the state's survival. Kings followed one of C. Northcote Parkinson's prime principles; they multiplied subordinates, not rivals.

The co-optative procedure operated quite generally. Fischer and Lundgreen point out the extent to which the apparently rationalized and centralized system of Prussian territorial administration grew from the bottom up, with local notables assuming office before the middle levels of administration took shape, and ratified the existing distribution of power, both by putting selection of the Landrat in the hands of a region's landlords and by giving the positions allotted to the landed elite extraordinary precedence within the bureaucratic rank order. Thus they identify some interesting parallels between Britain and Prussia: in both countries the central power carried out a great deal of its work through the partial integration of existing local elites into the state structure. But the manner of integration—for example, through hierarchically arranged offices—had enormous political impact at a later historical stage. Although the French employed similar procedures in the early phases of subordinating the whole country to royal control, once the kings had substantial military forces at their disposal they turned increasingly to the imposition of orders by outsiders directed from the center.

On the other side, Fischer and Lundgreen observe that a number of recruitment systems which appear to reward merit, skill and knowledge actually confirm the hold of a particular class on a set of public offices. The most striking example is Prussia, where the famed

eighteenth-century "tightening of standards" strengthened the nobility's claim to high office. Through the expansion of military service and the increasing recruitment of civil servants from the army, the reinforcement of the Junker position within the army may well have increased the aggregate influence of their class. But it did so at the cost of their autonomy as a group.

Likewise, when Fischer and Lundgreen reconsider the famous supplanting of officers (independent representatives of traditional and particularist forces) by commissars (new agents dependent on the king's centralizing will) in France, they have to observe that the commissars came very largely from the same social classes—indeed the same families and career lines—as the officers. Once again what changes is not the class base of a government but the political autonomy of the classes in question. Only with the emergence of the technically trained functionary after 1750 or so, they tell us, do we find government service opening up as a major channel of social mobility. Even there we find the training and the recruitment procedures perpetuating a style and spirit established by the old elite.

Perhaps without intending it, Fischer and Lundgreen raise doubts about the administrative superiority of disciplined, autonomous bureaucracies, as such, over other means of government. But they confirm the significance of the political choices European states made in creating different sorts of staffs to carry on their work. They note, for example, that governmental control over access to technical knowledge was extreme in Prussia, moderately great in France, slight in Britain—a difference which probably influenced the timing and character of industrialization in the three countries, and certainly affected their relative reliance on private corporations, citizen commissions and the like for the solution of public problems. They fill out our picture of an army pervading a population in Prussia, a coalition of landlords and merchants checking royal power in England, and a civilian central authority achieving remarkable autonomy (at the cost of vulnerability to revolution) in France. Despite the convergence in administrative structures due to common problems and mutual modeling since 1800, the choices made in the sixteenth and seventeenth centuries still mark these countries.

ROKKAN ON DIMENSIONS OF STATE FORMATION AND NATION-BUILDING

With Stein Rokkan, we turn to an extraordinary generalization of the variables underlying such international difference. Doubly ex-

traordinary because efforts of this kind are rare, because this effort is rich and stimulating. Rokkan offers a model of the full-fledged nation-state (central control, standard culture, mass political participation, extensive redistributive activity). He converts the model into a set of phases resembling those a number of other scholars have proposed: penetration, standardization, participation, and redistribution. He asks himself what accounts for variations in the loci, timing, and political outcome of these phases within Europe. That leads him to devise several conceptual matrices. The fundamental matrices are schematic maps of Europe; in them large geographic variations in strategic position, culture, and social structure replace strict linear distance.

As Rokkan says himself, the very nature of the exercise fills the paper with contestable generalizations. One can easily argue, for example, with Rokkan's treatment of England and France as broadly similar cases of close interaction between urban and rural economic elites, or with his classification of Savoy with Prussia among the powers making major conquests "inward towards the center of Western Europe." Ultimately such judgments will have to stand up to critical scrutiny. For the scheme to be worth using in the interim, however, it is sufficient that there be an approximate fit among the reality, the specific judgments, and the general scheme. It is rare enough to find a general developmental scheme containing verifiable propositions rather than recurrent tautologies. It is rarer still to find one which simultaneously yields statements about the system as a whole, deals with the full range of variation, and provides unexpected hypotheses concerning the individual instances. Rokkan's scheme does. The present form of the scheme is complicated. The numerate reader, however, will not have much difficulty imagining it as a simple set of simultaneous equations.

Rokkan provides some constants for the equations in the form of common background characteristics which constrained the emergences of states everywhere in Europe: the residual influence of the Roman Empire, the presence of the Catholic Church, the precedent of the autonomous Germanic kingdoms, the continental web of cities, the general growth of the feudal and manorial structure, the early emergence of vernacular literatures. True, internal variation with respect to these features of the European experience made some difference to the development of states in different parts of Eu-

rope. But within a world perspective these are special properties of European state-making as a whole.

Within the European area, Rokkan brings out several dimensions of variation which generally lie hidden in our other papers: (1) geopolitical position; (2) urbanity, (3) concentration of landholdings; and (4) homogeneity with respect to language and religion. Most of the time he is simply showing that the diverse European experiences do fit without great distortion into the four-dimensional space defined by these variables, and that in combination these four variables yield a plausible account of the geographic pattern of state-making. The implicit argument, however, goes something like this: on functional grounds only a limited number of the large set of possible combinations of these variables could yield viable states; where rulers attempt to build states in the absence of one of these viable combinations, their efforts will fail in characteristic ways; each viable combination will produce a somewhat different but predictable historical sequence and outcome; nevertheless, these controlling conditions do change slowly with time, and are manipulable to some extent, so the geographic pattern shifts in the long run.

This line of reasoning appears in Rokkan's interesting analysis of the slowness of the urbanized region running between Amsterdam, Copenhagen, and Palermo to produce substantial, centralized areas. He considers that the multiplicity of trade-linked cities made it easy for the elites of that region to pool resources in temporary coalitions and federations, and just as easy to band together against the achievement of preponderance by any one of their number. Although a few federations and a few mini-states in the region survived the crystallization of a state system over Europe as a whole, the unifying conquests came from outside. The expansion of Prussia from an eastern base played an important part. The conquests of Napoleon and the Congress of Vienna which settled their outcome gave the last major shoves to the formation of a complete European state system. To an important degree, they cleared the way to a unified Germany and a unified Italy.

The instances of Germany and Italy bring out one of the more intriguing complexities in Rokkan's scheme. For purposes of comparative analysis, how should we set the phases and timing of the state-building? Relatively old states lay at the core of each: Brandenburg-Prussia in Germany, Savoy-Piedmont in Italy. Yet Rokkan's

treatment of Germany and Italy as nineteenth-century creations leads him to conclude that they faced a "cumulation of crises" of penetration, standardization, and participation. The plausibility of the argument depends heavily on what we regard as the unit of analysis: Savoy or Italy?

Indeed, the case of Italy identifies the two most embarrassing difficulties in applying a developmental-phase model like Rokkan's to the building of states: (1) If the "phases" can appear simultaneously, repeatedly or in different orders, do they actually constitute more than the elements of a *definition* of the modern state? (2) What is the *unit* which passes through these phases? The difficulties become even more acute when one notices (as does Rokkan) that a number of changes tended to occur at the same time over the whole range of European states, old and new; military innovations are the archetype of these roughly simultaneous changes.

In fact, Rokkan calls attention more directly than any of our other authors to the interaction and interdependence of the changing European states, to the sense in which they formed an operating system. Economically, the industrial North and West fed on the agrarian South and East. Politically, territorial consolidation proceeded at the peripheries of the system rather than at its center. By the end of the eighteenth century the major states were implicitly collaborating in dividing all of Europe into a limited number of sovereign states. What is more, their colonial expansion was spreading the territorial pattern (minus sovereignty) over the rest of the world. By the twentieth century almost the entire world was mapped into well-defined territories which were nominally sovereign, or destined to be so. Struggles over the territory which was to belong to one state or another became rare. (Yes, territorial disputes like those separating India and Pakistan, India and China, Israel and Egypt, or China and the Soviet Union in the 1960s and 1970s still occur, but they have become rare as compared to their prevalence in the nineteenth century.) The main options open to the new state-makers were (1) to occupy the whole territory allotted to them or (2) to break it—or let it break—into smaller states.

Not that this implicit international agreement makes everything easy for contemporary state-makers. It simply changes the nature of their problems. Rokkan's concluding balance sheet offers a bleak outlook for the states acquiring nominal sovereignty after World

War II. It also specifies how the contemporary acquisition of influence over remote parts of the world by Washington, Moscow, or Peking differs from the earlier territorial expansion of a France or a Prussia. Empire there is, but in the form of power relations among formally independent states whose territorial integrity almost all other states collaborate to maintain. The struggle shifts to control over the country's economic resources, its public ideology, its foreign relations, and thence to the personnel of government.

These changes enter Rokkan's argument as a set of models, contacts and pressures from outside the new state. The pressures are more powerful by far than those operating during the great periods of European state-building. The European state-makers constructed, then imposed, strong national governments before mass politics began. In new states, the two processes tend to occur together. That is the "cumulation of crises" already anticipated by the experiences of Germany and Italy.

The reasons that mass politics now arrives so early in a state's life, according to Rokkan's paradigm, almost all have to do with changes in the international system. They are the "high and diversified" pressures from such major centers as London, Paris, Washington, Moscow, and Peking, the "increasing exposure of masses to outside communication," the readily available models of universal suffrage politics, and so on. A skeptic might grumble that the court of Frederick the Great, after all, spoke French, or that the fine hands of the Hapsburgs were to be seen everywhere in sixteenth-century Europe. The fundamental fact—a decided drift toward dependency and interdependence in the twentieth century—would nevertheless remain. Ultimately Rokkan faces us with a paradox: the very international system which eases the creation of new states within the remaining niches in the world map reduces the likelihood that the new state-makers will either retain their personal power or create the sorts of acquiescent, nationalistic populations their European predecessors fashioned for themselves.

Recurrent Themes

Rokkan's survey touches on almost all of the themes which recur throughout the other papers. Their recurrence does not, of course, prove that they are intrinsically important—only that they are important to us. We hardly claim to have arrived at our estimates of

importance independently. Nonetheless the common themes deserve mention as pointers toward the general conclusions concerning state-making which one can properly draw from the whole body of papers.

WHAT ARE STATES?

Our seminar did not spend much time discussing definitions. We did not try to legislate a common definition of the state. Sometimes we came to sharp disagreement about the appropriate criteria. (Bayley, in particular, objects to the idea of "stateness" broached earlier in this chapter.) Nevertheless, in their work our authors lean toward a narrow definition of the state. Within the whole historic range of political institutions, they concentrate on a smaller set than Joseph Strayer (1970) sweeps together with his criteria of durability, spatial fixity, permanent and impersonal institutions, final authority, and loyalty. Within the whole range of social relations, they single out what Ralph Miliband (1969) calls the government rather than that entire power structure he calls the "state system."

Finer is more explicit. In his view, a "modern state" is an organization employing specialized personnel which controls a consolidated territory and is recognized as autonomous and integral by the agents of other states. For the most part, Finer and the rest of our authors converge implicitly on the notion of stateness: an organization which controls the population occupying a defined territory is a state *in so far as* (1) it is differentiated from other organizations operating in the same territory; (2) it is autonomous; (3) it is centralized; and (4) its divisions are formally coordinated with one another.

Today's governments differ considerably on each of these dimensions. But over the last five centuries the world as a whole has moved decisively toward "stateness." Thus the processes bringing states into being in Western Europe were: consolidation of territorial control, differentiation of governments from other organizations, acquisition of autonomy (and mutual recognition thereof) by some governments, centralization and coordination. Our authors find the development of national consciousness, participation and commitment—"nation-building"—interesting and important. They exclude it, however, from the definition of the state. They argue that in Europe it generally occurred after the formation of strong states, and by no means as a direct or automatic consequence of state-building alone. In short, they insist on the analytic separation of state-building from

nation-building, and consider the nation-state only one of several possible outcomes of state-building.

THE HIGH COST OF STATE-BUILDING

Explicitly, our authors agree that the building of states in Western Europe cost tremendously in death, suffering, loss of rights and unwilling surrender of land, goods, or labor. Implicitly, they agree that the process could not have occurred without great costs. (However, we do not agree so completely on the *minimum* costs it would have taken, on how much the actual costs exceeded the minimum, and for what reasons, or on the extent to which the benefits outweighed the costs; all these judgments contain large moral and larger speculative components.) The fundamental reason for the high cost of European state-building was its beginning in the midst of a decentralized, largely peasant social structure. Building differentiated, autonomous, centralized organizations with effective control of territories entailed eliminating or subordinating thousands of semiautonomous authorities. If our analysis of armed forces is correct, most of the enormous cost of military activity—by far the largest single cost of state-making—sprang from the effort to reduce rivals inside and outside the territory. Building states also entailed extracting the resources for their operation from several million rural communities. If our analyses of taxation and food supply are correct, European states could not have acquired much more power than they had at the beginning of the seventeenth century without collaborating in the destruction of the landed peasantry. In any case, they did collaborate.

Most of the European population resisted each phase of the creation of strong states. Our analyses of taxation, of food supply, and (less directly) of policing show that the resistance was often concerted, determined, violent, and threatening to the holders of power. The prevalence of tax rebellions, food riots, movements against conscription, and related forms of protest during the great periods of state-making help gauge the amount of coercion it took to bring people under the state's effective control. Even if we consider that the arrival of effective policing greatly increased the day-to-day security of the average individual, we shall have to weigh that against the coercion the average individual endured along the way, and the long-run increase in his exposure to death and destruction through war.

The incompatibility of the old European peasant social organization with extensive state-building comes out most clearly in the interdependence of financial resources without extensive production for the market. It is true that Postan (1954) has long since warned us against treating "the rise of a money economy" as a general and continuous feature of European experience. It is also true, as Rokkan points out, that the larger European states first grew up outside the most intensely commercialized zone of the continent, the urban band from Flanders and the Baltic ports down into Italy. Intensive marketing did not cause states; it may even have inhibited their formation. If Ardant is right, furthermore, the less commercialized the economy, the more exhaustive the extractive apparatus the state has to mount to get the same return, hence the bulkier the state as such. Yet within such existing states, commercialization facilitated the flow of revenues to the governments. Regions (or periods) of minimal trade blocked governmental efforts to extract resources and carry on expensive tasks. That is one of the main connections Fernand Braudel (1949, 1966) has in mind when he traces the rise and fall of states around the Mediterranean to large swings in the European economy.

In Europe, commercialization and the destruction of the peasantry occurred largely as a consequence of the spread of capitalist property relations and production. Our papers bring out the considerable historical connection between the rise of national states and the expansion of capitalism, especially its agrarian varieties. They do not give us grounds for concluding that the connection was intimate or ineluctable, since early capitalist ventures like the Hanse were quite foreign to state-making, and since early strong states like Spain and France formed outside the principal centers of capitalism. The historical connection had two sides: (1) the expansion of capitalism freed the resources which state-makers captured for national ends; the consolidation of previously fragmented property rights in land, for example, facilitated its taxation; and (2) the growth of cities and of industrial production (often outside the cities) in southern England, Flanders, the Rhineland, northern Italy, and elsewhere in Western Europe produced profitable markets for big agricultural producers elsewhere in Europe, incentives for landlords to hasten the creation of a docile, land-poor labor force, and conditions for political alliance between great landlords and aspirant state-makers. With variations running from the extensive proletarianization of

Midlands England to the "new serfdom" of Poland and Prussia, that alliance became one of the staples of European state-making.

C. B. Macpherson (1962) has proposed a third relationship between expanding capitalism and the European brand of state-making: the elaboration of a political theory of "possessive individualism" modeled on the property relations of capitalism. Likewise Karl Polanyi (1957) has linked the emergence of the new political and moral doctrines more explicitly to the increasing dominance of the market. Since our papers contain rather little discussion of doctrine, they bear only indirectly on the validity of these arguments. Nevertheless, they suggest that the drift toward governmental adoption of liberal doctrines—which was, in fact, widespread in Europe after 1750—grew not only from the accession of capitalists to political power and the penetration of market relations into everyday life, but also from the experience of statesmen seeking to put larger and larger resources at the disposition of the state.

The relationship was reciprocal and complicated. The growth of governmental staffs, the inflation of armies, the expansion of seats of government, the incessant search for new revenues all contributed in various ways to the drawing (or driving) of peripheral areas into national markets, to urbanization, and to the extension of capitalist property-relations. It remains debatable whether the extent of economic regulation and the sheer weight of government slowed economic growth in countries like Spain and Brandenburg-Prussia. (Among our authors, Ardant and Braun tend to disagree on that point.) The massive expenditure of resources on the military establishment may have drawn capital away from industry. The effect of state-making on the later course of economic change was therefore contingent. But it was powerful.

ARMIES, WARS, AND STATES

The formation of standing armies provided the largest single incentive to extraction and the largest single means of state coercion over the long run of European state-making. Recurrently we find a chain of causation running from (1) change or expansion in land armies to (2) new efforts to extract resources from the subject population to (3) the development of new bureaucracies and administrative innovations to (4) resistance from the subject population to (5) renewed coercion to (6) durable increases in the bulk or extractiveness of the state. The chain stretched more tightly in some states

than in others; the classic comparison sets military Prussia against civilian England. Even in England, however, the building of a New Model Army entailed the same series of effects.

These connections among state-making, the building of armed forces, and the maintenance of internal control help account for the tendency of revolutions to occur in conjunction with the preparation and the termination of war. As Walter Laqueur says, "War appears to have been the decisive factor in the emergence of revolutionary situations in modern times; most modern revolutions, both successful and abortive, have followed in the wake of war . . ." (Laqueur 1968: 501). Our papers identify two main paths to the revolutionary outcome: (1) the exaction of men, supplies and—especially—taxes for the conduct of war incites resistance from crucial elites or important masses; the European revolutions of the 1640s exemplify this pattern; and (2) the absorption or weakening of a government's repressive capacity by war, coupled with a decline in the government's ability to meet its domestic commitments, encourages its enemies to rebel; the Russian Revolution provides the type case. These are not necessary, or even probable, effects of war-making. They are only likely if the government seriously depletes its coercive reserves. The paradox is that the building up of the government's coercive capacity for war sometimes has that very consequence, because it leads to diversion, dilution, disloyalty or defeat of the forces destined for domestic control.

Where the populations remained docile, wars still weighed heavily. Joseph Strayer (1971: 339) tells us that the first powerful precedent for general taxation by the crown came from the pope's promotion of forced contributions to finance the Third Crusade. Kings were not slow in adapting that newly legitimized procedure to their own secular military needs. Up to our own time dramatic increases in national budgets, national debts, numbers of governmental employees or any other indicator of governmental scale in European countries have occurred almost exclusively as a consequence of preparations for war. The general rule, furthermore, has been for some contraction in governmental scale to occur after a war—but almost never a return to the prewar scale. Preparation for war has been the great state-building activity. The process has been going on more or less continuously for at least five hundred years.

At an international level wars and war settlements have been the great shapers of the European state system as a whole. The Peace of

Westphalia (1648), the Congress of Vienna (1815), the Treaty of Versailles (1919) and the provisional settlements ending World War II produced incomparably greater realignments of the identities, relations, and relative strengths of European states than any long periods of incremental change between them.

To be sure, some of these effects came after substantial delays; to an important degree, Italy and Germany owed their existence as unified, independent states to the course and the settlement of the Napoleonic wars, but those two national states did not actually take shape until decades later. The immediate state-making effects of the Congress of Vienna were nonetheless profound: not only was France shrunken to her nearly definitive borders, but the shrinking process left behind a consolidated Prussia, a consolidated Austro-Hungarian Empire, a Netherlands soon to split definitively into Belgium and Holland, and a two-paneled European map, with the northwestern panel headed for consolidation into a smaller and smaller number of political units without much redrawing of the main national boundaries confirmed in 1815, and the southeastern panel headed for proliferation through the breakup of the Ottoman and Austro-Hungarian Empires. Thus war shaped and reshaped the European state system.

The consolidation of the state system also constricted the opportunities for the building of new states by war. That shift lies behind a paradox which many observers of contemporary state-making have noticed: armies of new states which are unlikely ever to fight an international war adopt the latest armaments, absorb the largest part of the public revenues, employ their might in putting down dissidents and guerrillas, play the parts of arbiters, king-makers and, on occasion, kings, yet fail repeatedly in their efforts to transform the social structure. That they have a "vested interest in the status quo" is of course true . . . but so did the state-making monarchs who nonetheless transformed Europe.

The conventional explanations of militarism in contemporary countries run to the ease with which military models (as opposed to models of industrial production or of family structure) can be imported by new states, the advantage of any army in a power vacuum, and its special significance as the most "modern" institution in a poor country, a major arena for education and communications, a likely instrument for collective tasks running from canal-building to traffic control. Our authors do not deny these effects. Instead, they call

attention to their placement within a distinctive twentieth-century international structure: tremendous inequalities of military and economic power, deliberate exportation of military models and assistance by a handful of great powers, likely involvement of several of the same great powers in any war anywhere, consequent irrelevance of the small state's military establishment to its pursuit of international objectives by means of war. As a result of this international situation, military forces become favored links with (and instruments of) the great powers, and become much freer than their predecessors to intervene in domestic politics. The building of substantial armed forces, on the other hand, becomes much less likely to produce the gradual subordination of the subject population, the transformation of the fiscal structure, the freeing and absorption of resources that it did in the European seventeenth and eighteenth centuries. Its state-making impact appears to have diminished and changed.

THE SIGNIFICANCE OF MULTIPLICITY

A state system embracing a relatively small number of participants emerged from centuries of contestation among what had once been quite a large number of virtually independent political units. The thousand state-like units spattering the political map of fourteenth-century Europe dwindled to fewer than thirty by World War I. *E pluribus pauci* might serve as the European motto. Furthermore, throughout the entire reduction process the European system has included not one dominant power but a number of rivals. Unlike much of the Roman or Chinese experience, the multiplicity of medieval and postmedieval Europe almost always made it possible for a coalition to form against the greater power which could overcome that power alone. At an international level, the European political process therefore entailed the constant formation and reformation of coalitions.

Multiplicity marked the European state-making process at a smaller scale as well. In the earlier phases of state-making, every aspiring ruler found himself surrounded by rivals—even within the territories he nominally controlled. Landlords who exercised nearly autonomous control of their own estates (and thereby of most of the resources necessary for the waging of war, the maintenance of courts, and other stately tasks) rivaled and resisted the princes at the local level. Great magnates, royal cousins, and neighboring princes eased or elbowed their way into the prince's own jurisdic-

tion. In his essay on armed forces, Finer shows us how mortal a threat the enemies of every prince posed. Successful state-makers had to absorb, check, or destroy most of their immediate rivals.

The variant strategies they employed comprise much of the news of this book. Yet some general features stand out: As a rule the European monarchs of the great state-building period allied themselves with the landlords of their territories, who received a certain license to exploit their own shares of the land and the peasantry. Landowners generally comprised the nerves of the armed forces, the core of the coercion employed in crushing the early forms of resistance to state-making, the bulk of local administration *in nomine principis*. That princes should have formed such coalitions within the matrix of feudal society does not surprise us much in retrospect. That it did happen, nevertheless, hastened the subjugation of the vast mass of peasant population to a state-landlord combine, left power in the Estates even through the so-called age of absolutism, and facilitated the eventual creation of a landless population through such devices as enclosure.

One feature of this coalition process to which several of our papers (notably that of Fischer and Lundgreen) call attention is the co-optation of potential opponents of the state through apparently antistate institutions like the sale of public office. Although France provides the textbook case, most European monarchies at least tolerated the sales of some substantial set of offices during some military predominance and revolutions from above, while the opening of the English alliance to the commercial classes cleared the way to liberal democracy. Here the implicit arguments of our papers join the explicit arguments of Barrington Moore's *Social Origins of Dictatorship and Democracy* (1966). The alternatives of autocracy, democracy, federalism, or oligarchy which clearly differentiated European political systems in the nineteenth century were forming in the class alliances made by the state-makers of the previous three centuries.

HOMOGENEITY AND HETEROGENEITY

Largely as a result of the previous unification under the Roman Empire, European state-making began in a setting of considerable cultural homogeneity: in a world perspective, relatively little disparity in language, kinship system, cosmology, religion, aesthetic form, or even political tradition. This relative homogeneity no doubt

facilitated the movement of state-making models, ideologies, techniques, and personnel from one area to another. As compared, for example, with the cultural diversity of the Americas or Africa of the time, fourteenth-century Europe provided a favorable setting for the construction of substantial unified states.

Yet European state-making involved a further move toward homogeneity within states, along two criss-crossing paths: (1) via the deliberate attempts of state-makers to homogenize the culture of their subject populations through linguistic, religious, and, eventually, educational standardization; and

substantial phase of their histories. In England sales of major offices ended before 1700. Yet even then

> in the Exchequer, at least, sinecures abounded. Sir Robert Howard, as bad an administrator as he was poet, could not be removed from his post as Auditor of the Exchequer, which he had bought and so held for life. His security permitted the luxury of political opposition to the King. The Exchequer also provided a notable army of offices that were discharged by deputy, and so provided outdoor relief for the lucky families that held them. Few however, were as fortunate as the family of Walker, who held the office of Usher of the Exchequer. It had been granted this office in perpetuity by Henry II, and members of the family were still drawing their stipends in William III's reign (Plumb 1967: 114).

For all their dead weight, offices of this kind generally committed their holders to the continued existence of the government. Similarly, the ceding of control over Prussian local government to the regional landlords compromised the power of the state in principle, but helped assure the collaboration of the Junkers in state's exploitation of the peasantry and the urban population.

The form of class alliances thus worked out by the earlier state-makers significantly affected the later political pattern of the state. The coalition of crown, Junkers, and bureaucrats prepared Brandenburg-Prussia for (2) via the tendency of those states enclosing relatively homogeneous populations to survive and prosper, while those containing wide cultural disparities tended to stagnate or to explode. We need not exaggerate the resultant homogeneity; diversity is the stock in trade of local historians in France, Germany, Italy, or England: historical traditions, dialects, field systems, ethnic origins do

vary from region to region. In a large perspective, nevertheless, the European state-making process minimized the cultural variation *within* states and maximized the variation *among* states. Hence the plausibility of doctrines of national self-determination to nineteenth-century Europeans—just so long as they were not dealing with their own ethnic/religious minorities.

Why should homogeneity make any difference? Only the Fischer-Lundgreen and Rokkan papers take up the issue. They suggest two complementary reasons. First, a homogeneous population was more likely to remain loyal to a regime of its own kind, just as it was more likely to mount a successful rebellion against foreign domination. Second, centralized policies of extraction and control were more likely to yield a high return to the government (in terms of resources returned by the subject population per unit of pressure exerted by the government) where the population's routine life was organized in relatively uniform ways. There a single successful policy could easily be generalized to all parts of a state. The more heterogeneous the population, the more often a policy notably successful in one place would fail utterly in another, and the more effort and personnel absorbed in the elaboration of alternative plans, and the greater the relative payoff from policies which put a considerable share of the available resources into the hands of local magnates and traditional authorities. Hence the incentive of state-makers to homogenize.

Gabriel Ardant's controversial argument about the fiscal incentives for the extension of political participation takes the analysis one phase further: into what has loosely been called "nation-building." Gabriel Almond and Bingham Powell distinguish between state-building and nation-building in the following way:

> We need some way of talking about these challenges which may lead to political development, these changes in the magnitude and content of the flow of inputs which put the existing culture and structure under strain. As a beginning we may suggest four types of problems for challenges to a political system. The first of these is the problem of penetration and integration; we refer to this as the problem of *state-building*. The second type of system-development problem is that of loyalty and commitment, which we refer to as *nation-building*. The third problem is that of *participation*, the pressure from groups in the society for having a part in the decision making of the system. And the fourth

> is the problem of *distribution*, or welfare, the pressure from the domestic society to employ the coercive power of the political system to redistribute income, wealth, opportunity, and honor (Almond and Powell 1966: 35).

Ardant does not go as far into definitions as do Almond and Powell, but he appears to have similar distinctions in mind. In these terms, Ardant is asserting that beyond a certain level of state-building, the builders found that they had to greatly increase the loyalty, commitment, and acquiescence of the subject population (a nation-building task) in order to expand state power and that only substantial increases in participation would accomplish that objective; thus they became willing to accord much wider involvement in governmental affairs to the general population; partly as a result of the expansion of participation, according to Ardant, they found themselves increasingly involved in distribution and redistribution. Of course, Ardant concentrates on the fiscal aspect of these processes. By the very centrality of fiscal activity in the operation of states, however, Ardant's argument has implications for the whole range of state work.

Our papers—on purpose—do not deal with nation-building nearly so fully as with state-building. Therefore, they do not provide the means for testing this portion of Ardant's argument in detail. They provide just enough mixed evidence and contrary argument to make it clear that Ardant's position is a controversial one. He attaches great importance to the needs and plans of state-makers in accounting for the nineteenth-century broadening of participation in national politics. Other papers (notably Bayley's, Rokkan's, and my own) work with a model of strong pressure from below and great resistance from above, leading to a reluctant concession of political rights and guarantees of different mobilized segments of the general population. The controversy is familiar. It is the theme of such distinguished books as Reinhard Bendix's *Nation-Building and Citizenship* (1964), T. H. Marshall's *Citizenship and Social Class* (1950), and E. E. Schattschneider's *The Semi-Sovereign People* (1960). It recalls the very old argument among political historians about the relative weight of popular demands and of statesmen's concessions in the great nineteenth-century transformations of England or Italy. Our papers lead up to that famous old question and connect it with the logic of state-making. They do not resolve it.

Why Europe Will Not Occur Again

The European state-building experiences will not repeat themselves in new states. The connections of the new states to the rest of the world have changed too much. The statesmen of the contemporary world find themselves faced with alternative models of state-building, not to mention eager promoters of those models. The manager of a contemporary state may well be ineffective and/or wrong, but he is likely to assume the necessity of promoting an efficient and submissive civil service, a general and uniform system of taxation, a well-trained native military force, and a high level of industrial production. In Europe of the fifteenth or sixteenth century the available models were fewer, different, less well-defined and less obviously appropriate for the objectives of the powerful.

Moreover, the European state-makers and a few non-European collaborators, through war, conquest and alliance, eventually fashioned a worldwide system of states. As the nineteenth and twentieth centuries have worn on, the newcomers to the system have had less choice of the positions they would occupy in it, even down to the exact territories they would control. Among other things, that prior existence of a state system has fundamentally altered the role of the military forces in the smaller states, since their strength or weakness no longer makes the major difference in the territory controlled by the state or in its relations with other states.

Again the resources on which today's state-makers draw and the forces against which they struggle are deeply different from those of the early European experience. All the builders of European states occupied themselves, one way or another, in wresting their wherewithal from largely self-sustaining agrarian populations. They could not borrow military might, technical expertise, or development funds from neighboring states. They could not assume the existence of a world market for any of their products, or the readiness of their producers to respond to a world market if it existed. They could not dispossess foreign capitalists (unless one wants to press the analogy of the Catholic Church with Kennecott Copper). They could not even nationalize the land. On the other hand they could use their personal positions as suzerains and landlords to bring men, food, and rents to the service of the crown; could forge alliances with fellow landlords to assure the acquiescence of the rural population in their grouping; and could drum up public funds

by such devices as selling offices. Most of these conditions are entirely gone and unlikely to return.

Finally, the managers of contemporary states have undertaken different tasks from their predecessors: building a certain kind of economic system, creating specific facilities like research institutes, steel mills, airports, or holiday resorts; maintaining some minimum of public welfare; promoting one variety of patriotism or another; increasing the supply of scientists; and others. The new tasks flow in part from the available models of state-building, in part from the logic of the international system, in part from pressures within each individual country. In this regard as well, our ability to infer the probable events and sequences in contemporary states from an informed reading of European history is close to nil.

The profundity of all these changes might make worthless any inference whatsoever from European experience to today's world. The authors of this volume take a slightly more sanguine view of the matter. We think there is a reasonable chance that some general relationships among the ways of building state power, the forms of relationship between men and government, and the character of the political institutions which emerge from the process of state-building which held within the European world still hold today. There appears, for example, to be a strong and general connection between the ultimate bulk of national governments and the extent of their reliance on land armies in their formative periods. We propose a number of hypotheses along these lines, and attempt to knit them together: cautiously within our particular areas of competence in the substantive essays on armed forces, police, taxation, and so on, a bit more boldly in the synthetic essays.

The Plan of the Book

At its strongest points, our analysis will present well-founded hypotheses, not conclusions for the ages. The papers which follow are serious; the authors have set themselves demanding questions; they have tried hard to find the answers. Yet everyone of us feels the incompleteness and imprecision of what he has to report. If level of confidence is the measure, in fact, this book takes a step backward from previous statements about "political development," even to the extent of putting the phrase itself into quotation marks. "Not so easy as all that," we frequently conclude. If we have made it clear why

it is not so easy and what the possible alternatives to established doctrine are, that will have to do.

The papers themselves appear in a roughly descending order of extractiveness: the most obviously extractive activities first. Samuel Finer, a political scientist-historian, begins with a sweeping comparative analysis of the most expensive governmental activity of all: the building and maintenance of armed forces. Gabriel Ardant, an economist and economic historian, treats financial policy and economic infrastructure, with a strong concentration on the means by which states extracted resources from subject populations. Rudolph Braun, a historian with strong sociological leanings, takes up many of the same themes in a study focused on taxation in Britain and Prussia. David Bayley, a political scientist previously known for his work on new states, carries out a systematic comparison of police systems in West European countries. Charles Tilly, a sociologist who often works with historical materials, follows with a somewhat less systematic survey of problems of food supply and the kinds of conflicts they involved. Wolfram Fischer and Peter Lundgreen, social and economic historians, close the specialized essays with a comparative analysis of state involvement in the recruitment and training of various kinds of officials and technicians. Then we turn to two general essays. Stein Rokkan, a political scientist with fingers in most of the other social sciences, proposes a set of variables with which to analyze the European experience of state-making and nation-building. In the final essay, Tilly considers the implications of the West European experience with states for existing theories of political development and political decay.