STATES AND SOCIAL REVOLUTIONS

States and Social Revolutions

A COMPARATIVE ANALYSIS OF FRANCE, RUSSIA, AND CHINA

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for Bill

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Preface

SOME BOOKS PRESENT fresh evidence; others make arguments that urge the reader to see old problems in a new light. This work is decidedly of the latter sort. It offers a frame of reference for analyzing social-revolutionary transformations in modern world history. And it uses comparative history to work out an explanation of the causes and outcomes of the French Revolution of 1787–1800, the Russian Revolution of 1917–1921, and the Chinese Revolution of 1911–1949. Developed through critical reflection on assumptions and types of explanation common to most received theories of revolution, the principles of analysis sketched in the first chapter of the book are meant to reorient our sense of what is characteristic of – and problematic about – revolutions as they actually have occurred historically. Then the remainder of the book attempts to make the program of Chapter 1, calling for new kinds of explanatory arguments, come alive in application. In Part I, the roots of revolutionary crises and conflicts in France, Russia, and China are traced through analyses of the state and class structures and the international situations of the Bourbon, Tsarist, and Imperial Old Regimes. Particular emphasis is placed upon the ways in which the old-regime states came into crisis, and upon the emergence of peasant insurrections during the revolutionary interregnums. Then, in Part II, the Revolutions themselves are traced from the original outbreaks through to the consolidation of relatively stable and distinctively structured New Regimes: the Napoleonic in France, the Stalinist in Russia, and the characteristically Sino-Communist (after the mid-1950s) in China. Here special attention is paid to the state-building efforts of revolutionary leaderships, and to the structures and activities of new state organizations within the revolutionized societies. In their broad sweep from Old to New Regimes, the French, Russian, and Chinese Revolutions are treated as three comparable instances of a single, coherent social-revolutionary pattern. As a result, both the similarities and the indi-

vidual features of these Revolutions are highlighted and explained in ways somewhat different from previous theoretical or historical discussions.

Books grow in unique ways out of the experiences of their authors, and this one is no exception. The ideas for it germinated during my time as a graduate student at Harvard University in the early 1970s. This was—however faint the echoes now—a vivid period of political engagement for many students, myself included. The United States was brutally at war against the Vietnamese Revolution, while at home movements calling for racial justice and for an immediate end to the foreign military involvement challenged the capacities for good and evil of our national political system. The times certainly stimulated my interest in understanding revolutionary change. And it was during these years that my commitment to democratic-socialist ideals matured. Yet it would be a mistake to imply that *States and Social Revolutions* sprang immediately from day-to-day political preoccupations. It didn't. Instead it developed in the relative "ivory tower" quiet of the library and the study. As a graduate student, I pursued studies in macrosociological theory and in comparative social and political history. Puzzles kept emerging at the interface of these sets of studies. My attempts to formulate answers to problematic issues, and then to follow answers through to their conclusions, led me, through many stages of formulation, to the arguments and analyses now embodied here.

There was, for one thing, my early intellectual confrontation with the case of South Africa. The history of that unhappy land struck me as an obvious refutation of Parsonian structure–functionalist explanations of societal order and change, and as an insuperable challenge to commonplace and comforting predictions that mass discontent would lead to revolution against the blatantly oppressive *apartheid* regime. Liberal justice, it seemed, did not inevitably triumph. Marxist class analysis impressed me as much more useful than structure–functionalism or relative deprivation theory for understanding the situation of the nonwhites in South Africa and deciphering the long-term tendencies of socioeconomic change. But, working strictly in terms of class analysis, it was difficult to conceptualize, let alone adequately explain, the structure of the South African state and the political role of the Afrikaners. Yet these seemed to be the keys to why no social revolution had occurred—or likely soon would—in South Africa.

Another formative experience was a lengthy, in-depth exploration of the historical origins of the Chinese Revolution. To structure my program of study, I compared and sought to explain the relative successes and failures of the Taiping Rebellion, the Kuomintang Nationalist movement, and the Chinese Communist Party, looking at all three movements in the historically changing overall context of Chinese society. Deeply fascinated by late

Imperial and modern China, I came away from this research profoundly skeptical about the applicability (to China, and perhaps to other agrarian states as well) of received social-scientific categorizations such as "traditional" or "feudal." I also became convinced that the causes of revolutions could only be understood by looking at the specific interrelations of class and state structures and the complex interplay over time of domestic and international developments.

If most other students of comparative revolutions have moved, so to speak, from the West to the East – interpreting the Russian Revolution in terms of the French, or the Chinese in terms of the Russian – my intellectual journey has been the other way around the globe. After first investigating China, I next learned about France as part of a general program of studies on the comparative political development of Western Europe. Although I realized that France was "supposed" to be like England, her absolutist Old Regime seemed in many ways similar to Imperial China. I also deciphered basic similarities in the French and Chinese revolutionary processes, both of which were launched by landed upper class revolts against absolutist monarchs, and both of which involved peasant revolts and culminated in more centralized and bureaucratic New Regimes. Finally, I came to interpret old-regime and revolutionary Russia in the same analytic terms that I had worked out for China and France. And the emphases on agrarian structures and state building seemed a fruitful way to understand the fate of this "proletarian" revolution after 1917, through 1921 and the early 1930s.

There was yet another peculiarity worth noting about my induction into systematic research on revolutions. Unlike most sociologists who work in this area, I learned a good deal about the histories of actual revolutions *before* I read very extensively in the social-scientific literature that purports to explain revolutions theoretically. When I did survey this literature, I quickly became frustrated with it. The revolutionary process itself was envisaged in ways that corresponded very poorly to the histories I knew. And the causal explanations offered seemed either irrelevant or just plain wrong, given what I had learned about the similarities and differences of countries that had, versus those that had not, experienced revolutions. Before long, I decided (to my own satisfaction, at least) what the fundamental trouble was: Social-scientific theories derived their explanations of revolution from models of how political protest and change were ideally supposed to occur in liberal-democratic or capitalist societies. Thus non-Marxist theories tended to envisage revolutions as particularly radical and ideological variants of the typical social reform movement, and Marxists saw them as class actions spearheaded by the bourgeoisie or the proletariat. No wonder, I said to myself, that these theories offer so little insight

into the causes and accomplishments of revolutions in predominantly agrarian countries with absolutist–monarchical states and peasant-based social orders.

From this mélange of intellectual experiences, a possible project, destined to culminate in this book, presented itself to me: Use comparisons among the French, Russian, and Chinese Revolutions, and some contrasts of these cases to other countries, to clarify my critique of the inadequacies of existing theories of revolution, and to develop an alternative theoretical approach and explanatory hypotheses. Although I rejected the assumptions and substantive arguments of the theories of revolution I knew, I still had the urge to clarify the general logic that I sensed was at work across the diversely situated major revolutions I had studied. Comparative historical analysis seemed an ideal way to proceed.

To my good fortune, the three Revolutions that I wanted to include in my comparative analysis had been extensively researched by historians and area specialists. A large existing literature may be a bane for the specialist who hopes to make a new contribution based upon previously undiscovered or underexploited primary evidence. But for the comparative sociologist this is the ideal situation. Inevitably, broadly conceived comparative historical projects draw their evidence almost entirely from "secondary sources"– that is, from research monographs and syntheses already published in book or journal-article form by the relevant historical or culture-area specialists. The comparative historian's task– and potential distinctive scholarly contribution– lies not in revealing new data about particular aspects of the large time periods and diverse places surveyed in the comparative study, but rather in establishing the interest and prima facie validity of an overall argument about causal regularities across the various historical cases. The comparativist has neither the time nor (all of) the appropriate skills to do the primary research that necessarily constitutes, in large amounts, the foundation upon which comparative studies are built. Instead, the comparativist must concentrate upon searching out and systematically surveying specialists' publications that deal with the issues defined as important by theoretical considerations and by the logic of comparative analysis. If, as is often the case, the points debated by specialists about a particular historical epoch or event are not exactly the ones that seem most important from a comparative perspective, then the comparative analyst must be prepared to adapt the evidence presented in the works of the specialists to analytic purposes somewhat tangential to those they originally envisaged. And the comparativist must be as systematic as possible in searching out information on the same topics from case to case, even though the specialists are likely to emphasize varying topics in their research and polemics from one country to the next. Plainly, the work of the comparativist only becomes possible *after* a large primary literature has been built up by specialists. Only then can the compa-

rativist hope to find at least some material relevant to each topic that must be investigated according to the dictates of the comparative, explanatory argument that he or she is attempting to develop.

As the Bibliography for this book is meant to indicate, I have been able to draw extensively upon rich literatures about France, Russia, and China. Each literature has great depth and scope, and each includes many books and articles originally published in (or translated into) English and French, the two languages that I read most easily. With occasional exceptions attributable to the thinness of interest about particular topics in one historical literature or another, the challenges I have faced have not been due to difficulties of finding basic information. Rather they have been challenges of surveying huge historical literatures and appropriately weighing and using the contributions of specialists, in order to develop a coherent comparative historical argument. How well I have met these challenges is for readers (including historians and area specialists) to judge for themselves. For myself, I shall be satisfied if this book serves in some small measure to provoke debate and inspire further investigations, both among people interested in one particular revolution or another and among people concerned to understand modern revolutions in general, their past causes and accomplishments and their future prospects. Comparative history grows out of the interplay of theory and history, and it should in turn contribute to the further enrichment of each.

Working and reworking the argument of this book over the last few years has often felt like an unending lonely struggle with a giant jig-saw puzzle. But, in actuality, many people have lent a hand, helping me to see better the overall design and pointing out where particular pieces fit, or do not.

My most fundamental scholarly debt is to Barrington Moore, Jr. It was my reading of his *Social Origins of Dictatorship and Democracy* while I was still an undergraduate at Michigan State University that introduced me to the magnificent scope of comparative history and taught me that agrarian structures and conflicts offer important keys to the patterns of modern politics. Moreover, the graduate seminars I took from Moore at Harvard were the crucibles within which my capacities to do comparative analysis were forged, even as I was allowed the space to develop my own interpretations. Moore set rigorous tasks and reacted with telling criticisms. And student fellowship in the seminars provided a supportive and intellectually lively atmosphere. In fact, two friends among fellow students in Moore's seminars, Mounira Charrad and John Mollenkopf, have given me encouragement and advice through all stages of this project on comparative revolutions.

Another crucial, longstanding influence has been Ellen Kay Trimberger. I first became aware of her kindred work on "revolutions from above" in

Japan and Turkey in 1970. And, ever since, Kay's ideas, comments, and friendship have helped me enormously to develop my analysis of France, Russia and China.

Like many first books, this one had an earlier incarnation as a doctoral dissertation. That phase of the project was certainly the most painful, because I undertook too much in too short a time. Nevertheless, in retrospect it was worth it, for a "big" thesis, however imperfect, offers more potential for the subsequent development of a publishable book than a more polished narrow dissertation. For encouraging me to undertake the nearly impossible, I owe thanks to Daniel Bell, who also made detailed and provocative comments on the thesis draft. The dissertation was formally advised by the good and admirable George Caspar Homans, who gave careful feedback and exerted unremitting pressure for me to finish quickly. The remaining member of my thesis committee, Seymour Martin Lipset, made astute suggestions from beginning to end and was kind enough not to hold it against me when the thesis took longer to complete than I had originally planned. Financial support during my final years of Ph.D. work came from a Danforth Graduate Fellowship, which leaves its holders free to pursue research topics of their own choosing.

After the dissertation was completed, Charles Tilly generously offered encouragement and recommendations for the major revisions that lay ahead. Colleagues and students at Harvard, where I teach, helped in innumerable ways to facilitate and stimulate my progress on the book. And once the revisions were partially done, many others helped speed the book to completion. Walter Lippincott, Jr., of Cambridge University Press, arranged for early reviews of the manuscript; these resulted not only in a contract for publication but also in very useful advice on the introduction from John Dunn and Eric Wolf. Peter Evans also made suggestions that helped with the revisions of the first chapter. Mary Fulbrook provided research assistance for revisions of Chapter 3, and her work was paid for by a small grant from the Harvard Graduate Society. I likewise benefited from the Sociology Department's Fund for Junior Faculty Research.

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Mrs. Nellie Miller, Louisa Amos, and Lynn McKay did wonderfully speedy and accurate work in typing the final manuscript. Mrs. Miller deserves thanks above all, for she did most of the typing in every stage of revision. I was fortunate indeed to be able to rely upon her perfectionism and intelligence.

Finally, of course, I lovingly acknowledge the help of my husband, Bill Skocpol, to whom this book is dedicated. His comments on all parts of the text through many revisions, his willingness to help with practical chores such as the typing of early thesis versions and the checking of quotes at the end, and his patience in the face of my emotional ups-and-downs throughout the entire process—all of these contributions are embodied in every part of *States and Social Revolutions*. Bill is an experimental physicist, but without his willing aid this work of comparative historical sociology could not have developed to completion.