The Political Economy of the American Frontier

This book offers an analytical explanation for the origins of and change in property institutions on the American frontier during the nineteenth century. Its scope is interdisciplinary, integrating insights from political science, economics, law, and history. This book shows how claim clubs – informal governments established by squatters in each of the major frontier sectors of agriculture, mining, logging, and ranching – substituted for the state as a source of private property institutions and how they changed the course of who received a legal title, and for what price, throughout the nineteenth century. Unlike existing analytical studies of the frontier that emphasize one or two sectors, this book considers all major sectors, as well as the relationship between informal and formal property institutions, while also proposing a novel theory of emergence and change in property institutions that provides a framework to interpret the complicated history of land laws in the United States.

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Political Economy of Institutions and Decisions

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Preface

When I first began studying property institutions on the American frontier, I expected to find that squatters were rugged individualists who sowed the seeds of capitalist development. My thoughts were certainly influenced by neoclassical microeconomics, which generally views entrepreneurs as a source of innovation in the economy. Squatters were archetypical entrepreneurs, occupying and improving land in the major frontier sectors of agriculture, mining, logging, and ranching throughout the nineteenth century even though they lacked formal (legal) authority to do so. My perspective was also shaped by popular accounts of the Wild West, which tend to portray the frontier in an individualistic fashion. Each tradition – neoclassical microeconomics and Hollywood accounts of the Wild West – shares in common emphasis on individuals over groups or governments.

As my search continued, I found that the story of the origins and change of property institutions was much more complicated than I had initially thought. To be sure, my findings revealed that squatters were powerful and profound engines of capitalist development. Indeed, they established informal private property institutions that competed with, and in many ways substituted for, state-backed private property rights. However, they were not successful because they did things on their own. Rather, they were good at working together. And when their interests congealed, it was not always a source of social benefits.

In addition, the means by which squatters managed their affairs did not fit with fables typically told by economists interested in informal order. Indeed, one of the defining features of economic studies of social order is explaining how people succeed without relying on governments or even hierarchy. Although there is some truth to accounts offered by economists, my findings suggest that the truly remarkable feature of frontier development was the ability of squatters to devise their own governments. Their informal associations, which were called claim clubs, typically had all the features of the state – executives,
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deliberative bodies, a system of administration, judges, and juries – despite operating in the state’s often pale shadow.

Despite sharing features in common with the state, claim clubs swept formal governments aside in their effort to provide their own property institutions. Over time, they ensured their members got legal title for free. This is a book about claim clubs and their consequences.

Although my stories emphasize claim clubs, this book is organized around two of the fundamental questions of political economy: where do property institutions come from and why do they change? Despite their importance in the political and economic history of the United States, generalized theories of the origins of property institutions have a limited role for organizations such as claim clubs. Economic studies of the origins of property institutions emphasize highly decentralized processes of emergence of property institutions, such as ownership norms arising through spontaneous order or reputational contracting without considering institutions specifying power relations. In contrast, political theories of property institutions alert us to the role of the state as a source of private property institutions. Despite providing many insights into the origins of property institutions, it is my contention that neither of these disciplinary perspectives provides an analytical framework capable of explaining how governments arise within the state to specify and enforce property institutions. Since claim clubs challenged Leviathan while moving substantially beyond the highly decentralized arrangements of economics, a new analytical framework is necessary to understand them; this book is the first attempt at such a framework.

Once we understand where property institutions come from, the next logical question is why they change. As far as institutional change is concerned, my interest is in the evolution of land laws governing the agricultural, mineral, ranching, and logging sectors throughout the nineteenth century. My initial belief was that squatters were a source of efficiency, or Pareto improvement, in the process of institutional change. This assumption also turned out to be incorrect. Claim clubs, rather than benevolently responding to an environment of bureaucratic and legislative incompetence, were in many ways a source of rather than a solution to transaction costs in the land-allocation process.

My initial mistake was to overlook the state’s interest in land. I was so concerned with emergence of private property institutions that I did not anticipate the fiscal consequences of land laws for development of the American state. Later, it became clear to me that the fundamental conflict during this period was not so much about private property rights as much as it was about the price of land. My preoccupation with the origins of private property institutions led me to overlook the fundamental conflict during the nineteenth century, which was a bitter and often bloody struggle over the price of land.

In the simplest sense, conflict over the price of land emerged because the state owned a lot of land that squatters wanted. Competitive auctions of state-owned land would have satisfied the state’s interest in revenue to provide
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public goods, including security, as well as a growing demand for private property. Yet the state’s interest in competitive markets for land clashed with the aspirations of distributive-minded claim clubs – and as I found out, clubs were astonishingly successful in getting what they wanted, which was free land. Throughout the nineteenth century, claim clubs harnessed their organizational fury to undermine competitive markets for land at every turn and in the process undercut the state’s revenue aspirations.

Despite the importance of price in economics and political economy, existing studies of change in property institutions have not considered explicitly conflict over the price of land during the privatization process. Once we recognize the centrality of conflict over the price of land, a different picture of squatters emerges. Far from an efficient response to poorly designed laws, claim clubs were perhaps the fundamental rent-seeking coalitions of the nineteenth century. Squatters were remarkable in their ability to assign and enforce private property institutions informally as well as a fundamental source of distributive politics; my mistake was to assume that the former implied efficiency in the process of institutional change. As far as institutional change is concerned, my contribution is to explain how claim clubs could be simultaneously a remarkable source of self-governance as well as a source of social costs in the process of formal institutional change.

Today’s choices are also informed by history (or at least we hope that they are). My findings also led me to reconsider the conventional wisdom regarding the lessons of the American frontier for today’s conflicts over land. Existing studies of the frontier generally infer from experience during the nineteenth century the virtue of formalization of title, praising squatters. My lessons differ in two respects.

First, my account suggests that formalization of informal claims to land is less important than balancing the state’s interest in land ownership with the demands of individuals for private property rights. Land reforms often have important fiscal consequences, including consequences for the state’s ability to provide public goods such as security. Public policies that fail to consider the fiscal consequences of land laws are incomplete, to say the least; yet few studies of emergence and change in property institutions consider carefully the fiscal consequences of land laws.

Second, my findings suggest that the fundamental political problem during decentralization of ownership is committing credibly to competitive markets for state-owned land. One of the primary mistakes the federal government (repeatedly) made was to believe that it could provide one-time or temporary concessions to squatters. These concessions, by undermining credibility of commitment to state ownership, led to new waves of squatters and ultimately ensured that the state was unable to profit from its vast landholding. Although a logic of credibility of commitment provides a lens to understand key developments on the American frontier, the implications in general are far reaching: a government should be concerned not only with its commitment to protecting...
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private property but also to state ownership if it hopes to ensure an orderly process of decentralization of ownership.

Providing a new analytical framework to interpret the political economy of the American frontier, one that integrates insights from political science, economics, history, and law, is a challenging task. To the extent this book does what it sets out to do, many people deserve credit. My greatest debt is to David Weimer, who read the manuscript in its entirety many times, each time offering detailed comments and suggestions that dramatically improved the clarity of my argument. He has been a wonderful mentor and friend, and a continual source of encouragement and inspiration. I also owe a special debt of gratitude to Daniel Bromley for teaching me institutional economics and for an extremely useful set of comments on an early version of the manuscript that led me to rewrite it in its entirety. Special thanks also go to Scott Gehlbach for dozens of discussions about my theoretical argument and political economy more generally. My intellectual debt to them should be clear in the pages that follow.

Several others offered comments and suggestions that influenced my argument in important ways. Stanley Engerman provided extremely useful suggestions early in the project that set it on a productive path. David Canon and Gerald Gamm, two scholars whose knowledge of Congress far surpasses that of anyone I have ever met, helped me improve my analysis of legislative decision making during the nineteenth century. Pete Karsten offered extremely useful suggestions regarding key works from the discipline of history, as well as shared insight from his own work on the study of institutional change; my only regret is that I did not have the benefit of his advice earlier in the project. My analysis of judicial institutions and legal history benefited greatly from discussions with Donald Downs and Howard Schweber, two people who shaped my understanding of public law. John Coleman helped me better understand how my study relates to the American Political Development (APD) perspective. It is my hope that this book illustrates how we can bridge what is often seen as a divide between APD and new institutional economics. Peter Toumanoff, who introduced me to the property rights approach, transaction costs economics, and new institutional economics as an undergraduate also offered valuable insights when I was writing this book on these same subjects.

Several scholars offered useful comments as well as encouraged me to think about the comparative implications of my study, including Melanie Manion, Aidan Vining, Catherine Boone, Ed Friedman, Aili Tripp, Yoshiko Herrera, Lou Picard, J. David Stanfield, and M. Yasin Safar. Although this book is at its core a study in the American frontier, they ensured that I was continually thinking about current conflicts over land in the developing world. The final section of this book is entitled “Land and the State”; it is meant to be a transition to my next book, which will consider in earnest the political economy of land in the developing world, with emphasis on how land reform can make or break the state. To the extent the transition from American economic history...
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to the current developing world is seamless, they deserve credit for ensuring a comparative book was always on my mind.

The anonymous reviewers for Cambridge University Press provided exceptionally detailed, thoughtful, and precise comments on the entire manuscript at a late stage in its development. They suggested many changes yet also offered insight into how to implement them. Indeed, their comments led to the final comprehensive rewrite of the manuscript. In the end, I adopted nearly all the changes suggested, and this book is much better for it.

The editorial and production team at the Press has been remarkably helpful. Lewis Bateman deserves special thanks for seeing promise in this manuscript and for kind words of encouragement. Anne Lovering Rounds and Shaun Vigil were extremely helpful in preparing the manuscript for production. From the moment I began working on the dissertation that set the foundation for this book, I envisioned it as fitting into a long line of books that I have learned a great deal from in the Political Economy of Institutions and Decisions series, and I was extremely happy when the editors of the series, Stephen Ansolabehere and Jeffry Frieden, chose this book to be a part of it.

My wife, Jennifer, has been an unflinching source of support and encouragement from the moment I began writing about the American frontier. Although she studies a very different part of the world, I have learned more from her about informal governance than anyone. This book would not have been possible without her love, patience, and understanding. This book is dedicated to her, and to our beautiful children, Zoe and Leo. It is amazing how much insight a one- and three-year old provide into the emergence of informal private property rights (as well as the challenge of sustaining a sharing rule).