Institutions and Economic Performance

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Preface

The impact of economic and political institutions on economic outcomes has interested economists and political scientists for many years. Nevertheless, the subject did not make it into the economic mainstream for a long time. Recently, however, a surge of research has provided new theoretical and empirical insights, turning this topic into a vibrant area of enquiry. This book collects original studies on institutions and economic performance, written by economists, political scientists, and economic historians, who are members of the program on Institutions, Organizations and Growth (IOG) of the Canadian Institute for Advanced Research (CIFAR). All the chapters of the book were written by members of this program, some in collaboration with other scholars. Members of the IOG program met regularly over a period of three years to discuss their work. The amount of research they produced is much too large to be collected in a single volume; it has been published in scientific journals, edited volumes and books. What we offer instead is a glimpse into this research, which is representative of the group's larger effort and, arguably, of the research agenda of this entire field.

I thank Jane Trahan for supervising the production of this book from beginning to end.

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Contents

Part II. Theory

Part III. Contemporary Evidence

Introduction

ELHANAN HELPMAN

Nations grow at different rates, and income per capita differs greatly across countries. Economists have studied the extent to which these differences can be explained by differences in resources, including physical and human capital. They find that, after accounting for the cumulative effect of capital inputs, large differences in income per capita remain across countries. So the natural questions are, why do total factor productivity (TFP) levels differ so much, and why do they grow at different rates in different countries?

One line of research provided important insights into the role of technological progress in explaining differences in TFP. It showed, for example, that investment in research and development (R&D) explains a substantial part of this variation, particularly in the industrial countries. However, substantial differences remain after accounting for both the accumulation of physical and human capital and R&D investment. The question is, why?

A new effort directed toward answering this question focuses on the impact of institutions on economic outcomes. Its main premise is that institutions have a direct effect on productivity, as well as an indirect effect, because they influence the form and rate of technological progress and they shape the incentives of economic and political agents. Institutions are more fundamental determinants of economic prosperity than capital accumulation or R&D investment, because they frame the environment in which these activities take place, and because they are difficult to change; one can think of institutions as a state variable that changes very slowly over time. However, not all institutions are equally difficult to change. One view holds that political institutions are more difficult to change than economic institutions, and that for this reason political institutions have a substantial impact on the evolution of economic institutions. Be this as it may, the study of institutions carries the potential for a better understanding of the determinants of human well-being. Naturally, these issues make up a rich research agenda, which is only partly covered by the thirteen chapters of this book. I believe, however, that these chapters are representative of the larger effort in this field.

The first part of the book deals with the history of institutions, the way they evolved over time, and the way they influenced economies. In Chapter 1, Avner Greif hypothesizes that political, economic, and legal institutions are inadequate for understanding economic success. He argues that administrative structures have an important influence on both institutional and economic developments. His motivating observation is that policymakers—be they monarchs or presidents—rely on an administration to execute their choices, implying that administrators are in a particularly advantageous position to challenge them. It is therefore reasonable to conjecture that constitutional institutions—the rule of law, the formulation of constitutions, and political representation—first emerge as equilibrium outcomes in the relations between the policymakers and powerful administrators. As in much of his work on the institutional foundations of economic history, Greif studies equilibrium administrative structures as self-sustaining entities, examining their stability, change, and significance. He explores why some structures and not others lead to administrative power and constitutional institutions that are self-enforcing. Among other insights, his analysis highlights the roles played by legal and political institutions in fostering intra-elite cooperation, limiting the effectiveness of the rule of law, and the distributional impact of political institutions. Greif then extends the analysis to examine the implications of different initial administrative structures on subsequent economic and institutional change. His key conjecture is that some structures have a comparative disadvantage in controlling non-elites and thus provide stronger incentives for them to develop new sources of wealth. In particular, the same decentralized structures that support constitutionalism also motivate wealth creation, and the two processes complement and reinforce each other. The analysis thereby highlights conditions that are conducive to a "virtuous" cycle of economic growth and constitutional development. His essay is rich in historical examples. They include Frederick Barbarossa's relations with the German dukes in the twelfth century; King John's conflicts with the English barons in the thirteenth century, which led to the adoption of the Magna Carta; the Polish–Lithuanian Commonwealth, a constitutional monarchy from the sixteenth to the eighteenth centuries; and the relations the Ottoman sultans had with their janissaries from the fifteenth to the nineteenth centuries. He concludes by using this analysis to conjecture about the administrative foundations of the rise of the West.

In Chapter 2, Joel Mokyr takes up the institutional foundations of the Industrial Revolution in Britain. He does not challenge the now mainstream view that political developments—such as the Glorious Revolution of 1688 and other seventeenth-century developments around it, which protected property rights and placed constraints on the executive (i.e., the Crown)—played an important role in British economic success. Yet he raises a challenging question: what role have institutions played in the country's process of technological innovation and the adoption of new technologies? After all, technological change was at the heart of the Industrial Revolution, and it is not clear how the formal political institutions that emanated from the Glorious Revolution contributed to technological change. Pan-European developments, such as the Enlightenment and the growth of open science, helped Britain during the Industrial Revolution. But they cannot explain British leadership and its place at the head of the pack. Mokyr argues that formal institutions, important as they were, tell only a partial story, because informal institutions played a central role not only in fostering technological progress but also in sustaining economic growth until the mid-nineteenth century. In addition to property rights, law and order, constraints on the executive, intellectual property rights (such as patents), and financial institutions—all embodied in formal codes—there was open science, gentlemanly codes of behavior, and a high degree of trust in society, which supported the ever more complex arm's-length transactions among economic agents. Although it is hard to establish the precise mechanism that sustained this cooperative behavior, which was a predominantly middle-class phenomenon, the arguments for its existence and its contribution to British economic development are strong. Moreover, the sheer size of the British middle class on the eve of the Industrial Revolution was a contributing factor to the success of these social norms in shaping the process of change, including the acquisition of skills that set Britain ahead of other countries. All these helped transform the British economy from one of privilege and exclusion to one of open access and economic freedom.

Chapter 3, by Drelichman and Voth, discusses the impact of silver on Castilian institutions and the consequent decline of Spain. Their starting point is the resource curse, also known as the "Dutch Disease." First, the discovery of natural resources has an impact on a country's terms of trade.

Second, it leads to rent seeking and redistribution. The degree to which the struggle for rents has negative economic consequences depends on the quality of a country's institutions. The authors' main thesis is that the flow of silver influenced Castilian institutions in a way that hindered Spanish long-term economic development. Castile was a major power in western Europe after its emergence at the end of the fifteenth century as the sole victor of the Reconquista wars; and the effective merging of Castile and Aragon, under Ferdinand and Isabella in 1474, gave Castile control of the Iberian Peninsula. In subsequent years, the Crown strove to build a centralized administrative apparatus. The king presided over the Royal Council, which made the major decisions, and the royal judiciary had supremacy over local courts. In this structure, the Crown had supremacy over the nobility and the clergy. Ferdinand also obtained the right to name bishops and to control church activities in newly conquered lands. Under these circumstances the Cortes, a parliamentary body that originated in medieval times, remained the main institutional counterweight to the monarchy. Importantly, the Cortes had to approve new taxes and the renewal of extraordinary ones, but it had no jurisdiction over the use of mineral resources. Thus the silver inflow from across the Atlantic provided the Crown with revenue that was not controlled by domestic institutions, and silver became a large component of its income. This newly gained financial independence allowed Philip II, who was crowned in 1556, to restructure public finances in a way that reduced his reliance on the Cortes' consent. With the new system in place, the king could pursue expensive projects, such as the construction of monasteries, expansionist wars, and the building of the Invincible Armada. He could also leverage his financial needs with unprecedented amounts of debt. Since expenses exceeded income from silver by a wide margin, additional financing was needed from domestic sources. The king therefore used his financial commitments, and especially the need to finance troops in foreign lands, to request new taxes from the Cortes. In the evolving struggle between the Crown and the Cortes, the king managed to weaken the domestic institutions in a way that permanently damaged economic development.

Engerman and Sokoloff have argued in a series of influential papers that factor endowments had a major impact on economic development, and that they explain the divergent paths of North and South America. Their argument has several parts. First, factor endowments determined the use of slave labor, especially on plantations. Second, the use of slave labor resulted in extreme economic and political inequality, which prevented the formation of institutions that support economic development. In other words, regions with many slaves formed worse institutions than regions with fewer slaves. Third, economic growth was positively correlated with the quality of institutions; it was slower in regions with bad institutions. In Chapter 4, Nathan Nunn examines the Engerman–Sokoloff hypothesis econometrically. Although he does not study each one of its components, he paints a picture of mixed results. By estimating the impact of the fraction of slaves in a country's population circa 1750 on its per capita gross domestic product (GDP) in the year 2000, he finds that within the group of New World countries, those that had larger fractions of slaves in the mid-eighteenth century were poorer at the end of the twentieth century. This correlation is robust to various controls, such as initial well-being, colonial relations, and other variables. A similar relationship holds for a smaller sample of countries from the British West Indies, for which Nunn uses slavery data from 1830. For these countries, estimates exist for plantation and urban slaves, and both categories are found to have a negative impact on income per capita in 2000. Finally, he finds a negative impact of slavery on economic development across U.S. states. Evidently, these findings support the relationship between slavery and economic growth that is at the heart of the Engerman–Sokoloff hypothesis. Yet Nunn fails to find an influence through the *inequality channel* proposed by Engerman and Sokoloff. On the one hand, he finds a positive correlation between land inequality in U.S. states or counties in 1860 and the fraction of slaves in their population. But he finds no impact of land inequality in 1860 on income per capita in 2000. He therefore concludes that although slavery affected economic development, at least across the U.S. regions, its impact did not operate through land inequality.

Acemoglu, Bautista, Querubin, and Robinson also study the impact of inequality on economic outcomes, using the Engerman–Sokoloff arguments as a starting point. Yet they take a very different path. In Chapter 5, Acemoglu et al. draw a distinction between economic and political inequality. The Engerman–Sokoloff argument starts from the effects of economic inequality, and factor endowments that support this inequality, on economic allocations and politics. Moreover, it presumes that economic and political inequality will be mutually self-reinforcing. While there are various reasons to expect economic and political inequality to go hand in hand, in many societies there are marked differences between these two aspects of inequality. These authors point out that, while in the data for U.S. states in the early part of the twentieth century, land inequality is negatively correlated with school enrollment, it is positively correlated across areas in the state of Cundinamarca in Colombia. In the United States, land inequality in 1860 had a negative effect on school enrollment in both 1870 and 1950. In Cundinamarca, the average land inequality in 1879 and 1890 had a positive effect on school enrollment in 1993. (In both cases, data availability dictates the choice of periods.) What the authors of this chapter ask, however, is whether political inequality could have played a role in driving inequality in schooling. The answer is that it did. In Cundinamarca, political inequality, as measured by the political concentration of town mayors at the end of the nineteenth century, was negatively correlated with school enrollment rates in 1993. Towns that were dominated by the same mayors or their relatives had particularly bad outcomes. Importantly, however, the rich landowners were not necessarily the dominant local politicians, because entry into politics was relatively free and open to people from many backgrounds. In fact, unlike the stylized pattern in Latin America, in Cundinamarca, land inequality was negatively correlated with political concentration (this is not to say that political power was not handsomely rewarded with private gains, because it was). The landed elite served as a counterweight to the political elite, and this provided restraint on the local executives who invested in turn in publicly beneficial projects. Acemoglu et al. propose an explanation of why land inequality may affect outcomes differently, arguing that the relationship depends on whether the polities are weakly or strongly institutionalized. In weakly institutionalized polities, in which formal political institutions do not adequately constrain the executive, economic inequality may generate an effective counterweight to the executive. In strongly institutionalized polities, economic inequality may enable the few rich to capture politics, to the detriment of the general public. The picture that emerges from this chapter about the political economy of inequality and institutions in Latin America is quite different from the conventional wisdom that has emerged from the work of Engerman and Sokoloff.

The second part of the book contains theoretical chapters, although all chapters in the last part—which deal with evidence—use theory as well. The interactions between bicameralism and staggered-term legislatures are studied in Chapter 6 by Muthoo and Shepsle. They use a standard cake-division game to examine the outcomes in different institutional structures when each chamber is in charge of half the cake. Their institutional structures include systems in which elections take place in both chambers simultaneously with no staggering, both chambers have similar staggered elections, and elections are simultaneous in one chamber and staggered in the other. In all cases the task of a legislature is to distribute the surplus (cake) across two districts. A legislator does not benefit directly from this allocation; he just enjoys rents from being in office. As a result of retrospective voting, the probability that a legislator will be reelected increases with past allocations of the cake to his district. Moreover, voters have finite memory as to past allocations. At the constitutional stage, the founding fathers choose the chamber structure, the term structure of legislators, and the allocation of proposal power. For each such structure the pure-strategy equilibrium is a Markov equilibrium. In this equilibrium the proposer takes the entire cake. This provides the benchmark result. Muthoo and Shepsle then proceed to argue that if restricted to a unicameral legislature at the constitutional stage, there are circumstances in which the founding fathers would prefer a staggered-term legislature to a simultaneous-term one. Moreover, in the baseline model, the founding fathers choose a bicameral legislature over a unicameral one. Finally, if the agenda-setting power of legislators from each district in a staggered-term chamber is the same on average as in a simultaneous-term chamber, then the founding fathers are indifferent between all possible mixes of simultaneous- and staggered-term structures of the two chambers. The baseline model is then extended in order to address various concerns. These extensions imply that two chambers lead to more bias toward the status quo than a single chamber, and that tax rates are higher in a bicameral system than in a unicameral one.

Civil wars are prevalent and they cause major economic damage. In Chapter 7, James Fearon provides a theoretical analysis of such wars. He starts the discussion with two stylized facts. First, most civil wars take the form of guerrilla conflicts. Second, poor countries are more likely to have civil wars. In fact, per capita income is the single best predictor of the probability of a civil war. To explain these regularities, Fearon constructs a game-theoretic model in which a government and a rebel group choose how many armed people to enlist. This then determines how many rebels are captured or die, while the surviving rebels tax peasants or businesses for their own benefit. Contrary to standard models of conflict, in which resources are wasted but no actors risk death, in this model, violence is an equilibrium outcome. Moreover, the model provides insight into the contest-success function, which is typically treated as a black box. In particular, the author argues that network connections among rebels can make expansion of the rebel force increasingly risky for an individual rebel. As a result, small forces have an advantage. The negative correlation between income per capita and civil wars cannot be explained in simple terms, however. In poor countries the opportunity cost of becoming a rebel is small, and there is little to be gained from fighting; in rich countries the opportunity cost is large, but there is also more to be gained. Declining marginal utility of income or risk aversion might suggest that wealthier individuals would be more reluctant to risk capture or death, yet this is insufficient to generate the empirical correlation. What is required in addition is for wealthier people to be relatively more risk averse than poorer people. In the model, the extent of rebellion is driven in large measure by the inefficiency of government counterinsurgency and the share of income that can be extracted by the guerrillas' taxation "technology" of house-to-house visits. Fearon argues that empirically both are likely to be greater in poor countries.

In Chapter 8, Grossman and Helpman study the impact of party discipline on the supply of political patronage (pork) to electoral districts. By "party discipline" they mean the degree of enforcement of electoral promises in legislative decisions. To address this issue they develop a three-stage model in which parties make electoral promises in the first stage, elections take place in the second, and legislative policymaking takes place in the third. A key feature of the model is that a party can impose costs on its legislators if they implement policies that deviate from the party's electoral promises. When these costs are zero, there is no party discipline whatsoever, and when they are infinite, a party representative implements the party's electoral promises for certain. In between, the degree of discipline varies widely. A conflict between the party and its legislators arises from the fact that the party is interested in maximizing the probability of controlling the legislature, while ex post, after being elected, a legislator is interested in supplying pork in the form of a local public good to his or her district (the electoral system is assumed to be majoritarian). In this type of polity, neither the electoral rhetoric nor the reality of pork-barrel spending bears a monotonic relationship to the degree of party discipline. In particular, promised spending per district is very high when party discipline is very low, it falls with rising discipline initially, but rises with party discipline eventually. Actual spending levels also follow a nonmonotonic pattern. And importantly, the expected welfare of voters is nonmonotonic: it is high and rising with party discipline when party discipline is high, but it can rise and eventually fall with party discipline (then rise again) when party discipline is low. One implication of this analysis is that high party discipline is desirable on welfare grounds, yet small improvements in discipline are not necessarily beneficial when discipline is low to begin with. Another implication is that the cross-sectional effects of political institutions cannot be adequately described by simple correlations between actual spending levels and measures of party discipline.

Chapter 9 closes the theory part of the book with a study by Diermeier and Fong of the "ratchet effect" of a dynamic fiscal policy. Static models of political economy have become rich in institutional detail, while dynamic models have so far suffered from highly simplified institutional structures. This state of affairs resulted from the technical difficulty of incorporating desirable institutional characteristics into dynamic political economy models. Recent advances have made this possible, however. The authors of this chapter employ a new dynamic model of legislative decision making to study the ratchet effect of government spending, which refers to the finding that government spending—as a fraction of GDP—rises during recessions but does not decline during expansions. Importantly, this is not true in all political systems. Only in one class of systems, that is, parliamentary democracies with proportional representation, is this effect present. This group includes European countries, such as Germany, Italy, the Netherlands, and Spain, but not the United Kingdom or the United States. Diermeier and Fong develop a dynamic model of parliamentary democracy with proportional representation that generates the ratchet effect. Their model has several important features. First, once a policy is enacted, it remains in force until a new law is passed. Second, the policy can be repeatedly amended during the electoral cycle by legislators with agenda-setting power. Third, due to the fact that in proportional representation systems no single party typically controls the legislature, policy formation involves bargaining among multiple parties. Fourth, as is the case in parliamentary systems, the executive has the agenda-setting power during the legislative period. The ratchet effect emerges as a result of the dynamic evolution of the status quo, and especially of entitlement programs, and asymmetries in the constraints; while it is always possible to raise spending on desired items, it is not possible to reduce spending below zero on undesirable items.

The last part of the book focuses on contemporary evidence. In Chapter 10, Anderson and Francois study the degree of formalism adopted by