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A Review of Avner Greif's *Institutions and the Path to the Modern Economy: Lessons from Medieval Trade*

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Avner Greif's Institutions and the Path to the Modern Economy: Lessons from Medieval Trade *(Cambridge University Press, 2006) is a major work in the ongoing project of many economists and economic historians to show that institutions are the fundamental driver of all economic history, and of all contemporary differences in economic performance. This review outlines the contribution of this book to the project and the general status of this long standing ambition.*

1. *Introduction*

Avner Greif's eagerly awaited book, *Institutions and the Path to the Modern Economy: Lessons from Medieval Trade*, is ambitious, complex, long, and difficult. It will cause much work and trouble to reviewers. It will vex students for generations to come. This is in part because the volume actually contains two very different books that have been forcibly married and that cohabit in domestic discord. The first book is a revision of that minor classic in the field of institutional economic history, Douglass North and Robert Paul Thomas's *Rise of the Western World* (1973). Here Greif attempts to locate the eventual rise of Western Europe to world dominance in its unique development of institutions that fostered economic growth, starting in the early middle ages. The second book is a long, deep, thoughtful, indeed brooding, meditation on the nature of social institutions in general, their stability, and their dynamics: *A Prolegomena to any Future Institutional Theory*. In this second work, the specific institutions of medieval trade serve only as illustrations of proposed general principles.

Both of these are bold undertakings, but their combination in one volume creates unique difficulties. For those interested in the rise of Europe and the eventual Industrial Revolution, the long sections of abstract rumination over the nature and underpinnings of institutions, such as chapter 2—a twenty-four page discussion of how we should define the term *institution*—will make the book at times an exquisite torture. Also, among the general principles Greif adduces in the theoretical sections is that there is no simple mapping between explicit institutional rules and the actual operation of

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institutions. Institutions are subtle forms whose real functioning cannot be discerned without a deep knowledge of their context and history. This theoretical conclusion cuts against the parts of the book that attempt a quick and superficial link between European trade institutions and European economic success.

For those interested instead in the origin, stability, and evolution of institutions, the book may serve better. But for them the specific trade institutions cited as examples will not be the best material, since the details of the operations of these institutions and of their origins in tenth century Europe are sketchy, so that the empirical tests of any of the propositions advanced in the case studies are extremely limited. This is not the fault of the author, who has impressive command of the historical sources, but of the extant materials from this distant era. Such sources do not allow systematic empirical testing of the hypotheses advanced. As terrain to test institutional theories, medieval Europe has the great advantage of offering interesting and exotic animals. But it has also the disadvantage that we glimpse these beasts only imperfectly, flittingly, far off in the mist.

In this review, the analysis is thus in two distinct parts. The first deals with the factual issue of the role of institutions in the rise of the West. The second with the general theory of institutions. The general conclusion will be that the book succeeds in neither of its aims. But this failure implies no dishonor to the author. Greif is a scholar in the best sense of the word: someone who has pursued his own independent vision of institutional analysis. Both questions he addresses are excruciatingly difficult ones that have defied resolution for generations. Far better to try for the mountaintop and fail than to stay rooted resignedly in the turgid swamplands below.

2. *Institutions and the Rise of the West*

Why did Western Europe, which was still in 900 A.D. a backwater compared to the glories of the Byzantine, Muslim, Chinese, and Indian civilizations, eventually overtake all these potential competitors, conquer much of the world, and launch the Industrial Revolution? In the short (171 pages) but elegant and spirited book, *The Rise of the Western World* (1973), North and Thomas state, with an economy few other institutional economists have matched, "Efficient economic organization is the key to growth; the development of an efficient economic organization in Western Europe accounts for the rise of the West. Efficient organization entails the establishment of institutional arrangements and property rights that create an incentive to channel individual economic effort into activities that bring the private rate of return close to the social rate of return . . . if a society does not grow it is because no incentives are provided for economic initiative" (North and Thomas 1973, pp. 2–3).

Growth is thus just a matter of establishing the right rules of the economic game. Why then did earlier societies not have efficient institutions and why didn't all societies develop such institutions? North and Thomas answer that the rulers of preindustrial economies were motivated in setting up institutions by their private costs and benefits: "new institutional arrangements will not be set up unless the private benefits of their creation promise to exceed the costs" (p. 6). This has an air of certainty that perhaps only truism can deliver. But they flesh out this claim by arguing that efficiency institutions developed in parts of Western Europe because such factors as population growth changed the costs and benefits of institutional forms for rulers between 1000 and 1700 in the favored parts of Western Europe.

Medieval Europe in 900 AD on the vision of North and Thomas was a world of constricted factor and product markets. Most agricultural labor was supplied through forced labor with serfdom, capital usage was distorted by usury laws, urban labor markets were distorted by guilds, and land usage was limited by common property rights. Output markets were local and limited by guild restrictions and "just price" laws. Somehow, and exactly how is left endearingly vague, through a favorable conjunction of cost parameters influenced by population, environment, and military technologies, parts of Western Europe escaped this environment and thus achieved economic growth.

North and Thomas were wrong on most specifics in their interpretation of preindustrial institutions and their effects. They relied mainly on the untested prejudices of earlier generations of historians. Thus, for example, they repeat the conventional wisdom that common property in land in preindustrial northern Europe was an institutional pathology whose removal was a vital component of the agricultural revolution of the eighteenth century. Quantitative research in recent years suggests that common rights, at least by the seventeenth century, had negligible impacts on agricultural performance.[1] But the élan and vigor of this vision of the institutional roots of economic success attracted many converts to "institutionalism" and helped North to the prominence that brought him the Nobel Prize in Economics in 1993. North and his followers have never lost the faith that "Institutions form the incentive structure of a society, and the political and economic institutions in consequence, are the underlying determinants of economic performance" (North 1994, p. 359).

For the "institutionalist" program, the aim is to show how economic outcomes depend on the institutional structures of societies and then explain why efficient institutions were so rare in the millennia before 1800. Greif is one of the most highly regarded of this academic movement, and right from the beginning of this book stakes similarly strong claims for institutions. "Studying institutions sheds light on why some countries are rich and others poor. . . . The quality of these institutional foundations of the economy and the polity is paramount in determining a society's welfare" (pp. 3–4).

Later in the introduction, Greif notes that the empirical studies of the book concern the period when "European economy, polity and society were embarking on the road that led to the Rise of the West, a process that began in the late medieval period with the growth of European commerce" (pp. 23–24). To this end, he approvingly quotes Robert Lopez, who coined the term *the commercial revolution* for the years 950–1350, that long distance trade "became the driving force of economic progress, and in the end affected every aspect of human activity almost as decisively as the Industrial Revolution changed the modern world" (Lopez 1967, p. 126). Greif sees the trade expansion of the late medieval period as a fundamental transformation in the possibilities of the medieval economy, sparked specifically by institutional innovations and not by demography, technological innovation, or any other noninstitutional forces. Greif indeed, like Lopez, regards this medieval epoch as a true revolution, an equal of the Industrial Revolution. One aim of the book is to describe and understand the institutional roots of the rise of the West.

The *Rise of the West* is, for Greif, the rise of market institutions and, in particular, market institutions that permit long distance trade. Many economists, starting with Adam Smith, assume that markets are an easy and natural thing, found wherever there are people, and dating from a time beyond any written record. Thus as Smith in a lecture in 1755 stated, "Little else is requisite to carry a state to the highest degree of opulence from the lowest barbarism but peace, easy taxes, and a tolerable administration of justice: all the rest being brought about by the natural course of things" (Dugald Stewart 1858, p. -).

Greif argues instead that the institutions that allow long distance trade are complex and difficult to create, and will not emerge

[1] See Robert C. Allen 1982; Gregory Clark 1998; and Philip T. Hoffman 1989.

naturally from a minimum of social order, as Smith seems to assume. Long distance trade, he assumes, had collapsed in the Dark Ages from 500 AD to 900 AD. The path to modern growth, a tortuous and difficult one, was through the creation of institutions that supported trade.

The book's assumption that long distance trade was a fragile thing, whose existence depended on the creation of elaborate institutions that emerged only in very specific contexts, is hard to square with the archeological record that shows long distance trade even before the Neolithic Revolution created settled agriculture. In Europe by the era of modern man, 45,000 years ago, there is evidence of continent wide movement of Baltic Amber and Mediterranean mollusk shells. The arrival of settled agriculture in Europe in the Neolithic coincided with abundant finds in tombs of luxury objects traded over long distances (George Grantham 2007). Long distance trade was present from the dawn of agriculture, and perhaps even before.

Given the assumptions of the author, the book focuses on describing and analyzing the institutions associated with long distance medieval trade. What made agency possible in trades over long distances where the average speed of travel of information was one mile per hour? How did merchants achieve security for their persons and their wares operating in distant cities where they were aliens? How did medieval trading communities achieve stable self-government without a seizure of power by one or another faction? Greif is expert on the surviving details of these institutions and he adroitly deploys the somewhat sketchy details that survive on their operation.

Thus, in chapter 4, Greif argues that even cities that profit from trade will have an incentive to expropriate or cheat isolated merchants who come to trade. The formal argument, intuitively, is that the marginal gain to the city from the last merchant who comes to trade is 0, so that expropriating some subset of merchants is always profitable even if the expropriated retaliate by never trading again (pp. 112–13). With atomistic merchants trade will thus occur at less than socially efficient levels if reputation is the only mechanism available to restrain predation. Merchant guilds by collecting merchants into groups that can retaliate for expropriation against their members allow trade to rise to optimal levels.

Similarly, chapter 10 (pp. 309–50) describes and analyzes the community responsibility systems in Western Europe between 950 and 1350. In this institution, if a merchant from town *A* was found liable for damages in a commercial dispute in town *B*, and town *B* refused to settle the damages, then the appropriate compensation was seized from any merchant in town *A* who happened to come from town *B*. This unlucky victim would then have to appeal for compensation for his injury in the courts of town *B*. Greif again argues that this was an efficient response to the absence of overarching legal systems. Town *A* would have an incentive only to pursue merchants from *B* who caused damages, and town *B* would have an incentive to lay those damages at the door of the offender. Through this mechanism, trades became possible that were otherwise not supportable.

The modeling and the reasoning here are ingenious. But the reader expecting from this introduction, and from the title of the book, *Institutions and the Path to the Modern Economy,* a book showing how and why the institutions that underpin modern market economies first emerged in medieval Europe will be disappointed. For it is unclear if any of the institutions studied made any difference to the course of European growth. There is a crucial difference between a good being supplied at socially inefficient levels and this deficiency in supply being quantitatively significant. From the window of his office at Stanford, Greif may be able to see the wealth of Silicon Valley all around him, but also the social inefficiencies represented by California's traffic-clogged roads, its arcane land use and taxation policies, and by its inefficient water-use laws. All societies have many institutions that involve some inefficiencies, but some are much richer and more dynamic than others. It accomplishes nothing in terms of economic growth to show that an institution is inefficient. The next crucial step is to show the quantitative impact of those inefficiencies.

But this the book never does, except in an illustrative way. Its proofs about inefficiency speak not at all about *quantitative* significance. Any tax on trade, for example, will reduce its level below socially efficient levels. Trade throughout history has been taxed. But this does not imply that taxation thwarted all trade possibilities and prevented economic growth before the Industrial Revolution. Magnitudes matter here, and the proofs wielded by Greif are not geared to magnitudes.

Greif does offer some empirical support to the idea that the absence of security for merchants was a quantitatively important barrier to trade. He instances cases where unprotected merchants were attacked or abused in foreign cities and where trade expanded immediately after merchants were granted privileges in a foreign city (pp. 95–105). Trade between Genoa and North Africa doubled, for example, after an agreement for protection was reached of Genoese merchants was reached in 1161 (p. 101). Similarly trade between Catalonia and Sicily expanded within months of a 1286 protection agreement (p. 100). But this does not address the question of the importance of trade volumes to the overall efficiency of preindustrial societies. For Europe as a whole before 1800, international trade was a very modest economic activity, whose volume even by 1800 averaged less than 4 percent of GDP (Patrick O'Brien 1982).

Those who study history have learned to be wary of such arguments by example. It is too easy to choose examples, however unwittingly, to support the favored hypothesis and ignore those that might contradict it. A more disciplined empirical test would systematically survey trade volumes between city pairs and correlate these with the types of negotiated trade protection. The nature of Greif's sources probably preclude such a systematic empirical enquiry. We shall see also below that Greif's theory of institutions suggests that such systematic empirical investigations would be of limited value.

Another issue that Greif's empirical confirmation does not address is whether the growth of protections for merchants, or the community responsibility system, were themselves induced by an increased demand for trade. As North and Thomas argued in *The Rise of the Western World*, institutions all have costs for their creation and enforcement. They only emerge when their benefits exceed these costs. In a world where trade volumes were limited by small population sizes, low incomes and high transport costs trade will be anarchic and unstructured. But when trade volumes rise there is more incentive to create institutions which facilitate it. Did the institutions create the trade in medieval Europe or did trade possibilities create their own institutions?

Some institutional economists have been alert to these problems and have done interesting work in trying to uncover the direction of causation by looking for exogenous sources of institutional variation, as with Daron Acemoglu, Simon Johnson, and James A. Robinson (2001, 2005) or Dan Bogart (2005). But, for the examples Greif examines, all of 0the crucial variables seem inherently endogenous, denying these opportunities.

A further problem with the institutions dissected in the book is that they are mostly not those that can explain the eventual European takeoff. For example, there is an extended discussion of Greif's earlier work on "private-order contract enforcement institutions." This concerns how merchants monitor and reward agents operating at distant locations, a significant problem in long distance trade all the way up to the operations of the East India company in the nineteenth century. Greif showed, again with ingenuity and modeling skill, that the trading practices of the eleventh century Jewish Maghribi community, where any agent accused of dishonesty was shunned by the entire community, can allow agents to be hired for lower rewards (pp. 58–90). Such an institution could even allow for the possibility of agency when individualistic incentives would not support this relationship. But did this matter in practice? It is not evident that this was other than a curiosity.

In fact, the long account of these private enforcement mechanisms lies at cross purposes with the main thrust of the book, insofar as it concerns the rise of the West. This is one of the cases where the desire to theorize about the nature of institutions in general obstructs the desire to tell a story about the rise of Europe. The Maghribi equilibrium, not found in the West, allowed more efficient trade in an eleventh century world of distance and uncertainty. Yet, Greif notes later, in comparing the individualist Genoese with the collectivist Maghribis, "the relative efficiency of individualistic and collectivist systems depends on the magnitude of the relative parameters" (p. 301). The Maghribi system turned out to be a dead end and the individualist system of the Genoese that relied on such third parties as courts for contract enforcement the efficient outcome (because it came to dominate). In that case, the long discussion of the Maghribi's is probably not germain to the rise of the West.

Greif speculates that the early adoption by groups such as the Maghribi's of collective solutions to the agency problem may have, through creating "different cultural beliefs" (p. 300), locked these communities into a cultural system that was inimical to modern growth. Yet here, as he readily recognizes, he is guessing at interesting possibilities, unsupported by modeling or empirical evidence (p. 301). So the connection of the collective enforcement system of the Maghribi traders, interesting though that institution was, to the eventual rise of the west and the relative decline of the Islamic world is unknown. It is certainly clear that the Muslim world by the time of the Ottoman empire had an extensive court system that arbitrated on all kinds of formal contracts that look very similar to those of the West (Sevket Pamuk 2006).

His account of the community responsibility system is similarly a tale of an interesting institution, but one whose importance to the eventual domination of the west is unexplored. By the thirteenth century, after all, when the Islamic world was still a vigorous competitor of Christian Europe, the community responsibility system was disappearing. Greif argues that the community responsibility system decayed in the later thirteenth century because people were less easily identified by communities because of greater geographic mobility. In support he cites the fact that English merchants between 1257 and 1271 greatly increased registration of debts in the chancery rolls, placing the transactions under the jurisdiction of common law (p. 340).[2] Whatever the specific reasons, the role of this system in the rise of the West is thus again rather tangential.

There is also reason to doubt the crucial factual assumptions that underlie the Greif analysis of European experience 950–1350. This is that it was the development of more efficient trade institutions that led to a growth both in the size and in the productivity of the European economy in these years. There is no doubt that between 950 and 1350 western Europe witnessed major growth in population, in urbanization (particularly in northern Italy and Flanders), and

[2] Between 1250 and 1270 there was an explosion the production of written records of all kinds in England—accounts, court rolls, inquisitions post mortem. And documents such as charters showing land ownership began for the first time to actually show specific dates for transactions. Thus the greater numbers of chancery records may not reflect a migration of merchants to other legal mechanisms as much as an economywide adoption of more durable methods of recording facts and agreements.

in the volume of long distance trade. But the causes of these changes are difficult to disentangle: was it demographic change, improvements in agricultural productivity, improvements in industrial productivity? Greif, while he is an innovator in his economic models, is very old fashioned in his history. Greif's views on the importance of trade to the medieval revival are those that originated with Henri Pirenne in the 1920s. This earlier view has been displaced in England since the 1960s by the arguments of M. M. Postan emphasizing demography and agrarian technology as the key drivers of the medieval expansion.

Long distance trade in goods such as pepper from India, for example, existed already in northern Europe by at least 950 AD.[3] The volume of this trade increased greatly between 950 and 1350, but then the population of Europe also greatly increased over these years by an unknown magnitude. By the thirteenth century, Flanders had become the center of an industry producing fine wool cloth, which was exported to the Mediterranean in exchange for such goods as spices and silks. England mainly supplied the fine wool used in these cloths. Was the great expansion of the Flanders cloth manufacturing industry from the tenth century on the result of improved trade opportunities? Or did technological and organizational advances in Flanders, and/or more efficient production of raw wool in England, create the possibilities for trade that Italian merchants merely responded to. So it is hard to know whether trade was, as Greif assumes, the driving force in the growth of European economies from 950 to 1350 or just a response to technological and organizational changes in some of the constituent territories of Europe.

However, since the cost of goods such as pepper in England were almost entirely the transport and transactions costs in getting it from southern India to the end consumers we can get some idea of the efficiency with which this long distance trade was carried out by comparing prices of goods such as pepper with general prices in England. The data for this calculation exists only for the years after 1220, but that covers a lot of the period of the medieval commercial revolution that Lopez identifies. Thus by 1300, the population in England was probably twice as great as in 1220, cities like London had expanded even more than this and the volume of international trade had also swelled. Figure 1 shows this normalized pepper price, as well as the estimated linear trend in these real pepper prices from 1220 to 1350. There is no sign in these years of any improvement in the efficiency of the trading system that brought pepper to England. Thus we do not even know if trade institutions played any significant role in improving trade in medieval Europe.

At the global level the transition between the preindustrial world of slow growth and stagnation and the modern world of rapid growth shows up in a change in the measured rate of total factor productivity (TFP) advance from around 0 percent per year in all societies before 1800 to closer to 1 percent per year in modern successful economies. We also know this change, at least proximately, was caused largely by innovation in production techniques, as has been emphasized by Joel Mokyr and others.[4] Again the link between trade institutions and these fundamental innovations in production technique is unknown, but unexplored and unquantified in this work. Certainly, in the one economy where we can measure this, England, there is no sign that the medieval expansion of population, towns and trade in 1209–1315 was associated with any great increase in the measured TFP of the economy. Figure 2

[3] Indeed long before this. Islamic silver coins were found in numbers in coin hordes in the Baltic region even at the beginning of the ninth century (Ron Findlay and Kevin O'Rourke forthcoming). Pamela Nightingale notes the use of pepper in England even in the eighth century (Nightingale 1995).

[4] See Mokyr 1990. See also Clark forthcoming.

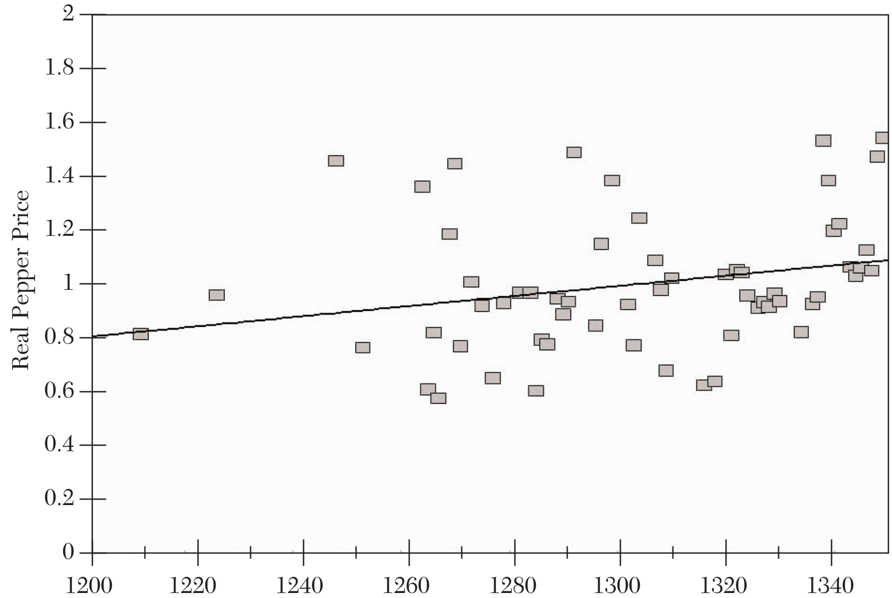

Figure 1. The Price of Pepper in England Relative to All Other Goods, 1221–1349[1]

[1] Author's calculation.

shows the measured TFP of England from 1220 to 2000. There are oscillations in the preindustrial era, but in terms of measured TFP growth it is not until around 1800 that we see a distinct break from the preindustrial era. The period 1260–1315, *the Commercial Revolution*, is actually associated with a decline in measured TFP in England. This is because as population expanded real wages fell without land rents rising to counterbalance this.

These TFP measures may well understate the degree of innovation in medieval society, but they certainly show that any assumption that an event occurred in Europe in 950–1350 that rivaled the Industrial Revolution in importance is wildly optimistic. But at the very least figures 1 and 2 show it is premature to hypothesize about the institutional roots of a revolutionary change in medieval Europe, when we are not sure that anything of great import happened.

Finally, the dual duty of the book as both European history and general theory leaves no space for discussion of the whole "Europe versus China" issue that Ken Pomeranz's 2000 book has brought to such prominence. Pomeranz argues that, in 1800, China had an economic system that was as developed, market driven, and individually rational as Europe's. Europe's advantage that led to its industrialization lay instead, believes Pomeranz, in the ecological factors of coal and colonies. Pomeranz's argument has its own problems but, if Greif wants to convince that European institutions really were the key to its success, some attention to the nature of markets and trade in the East would be potentially illuminating.

3. *The General Theory of Institutions*

Despite the title of the work, the material directed to laying the groundwork for a general theory of how to do institutional analysis, as opposed to discussing the specific role of institutions in Europe's development, predominates. Greif's general theory of institutions starts from a rejection of the notion that institutions are just the politically determined

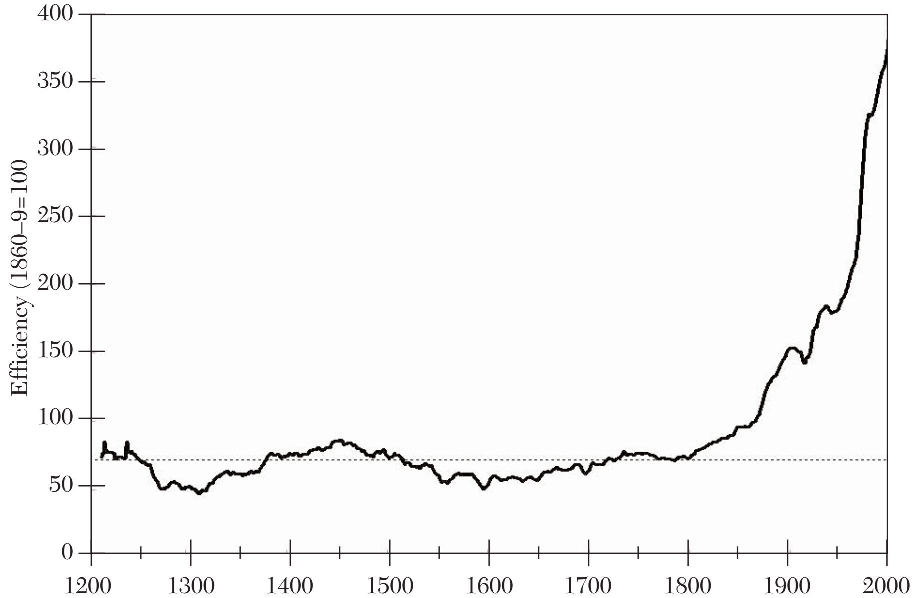

Figure 2. Measured TFP, England, 1220–2000[1]

[1] See Clark, forthcoming.

rules of the economic game in any society: the legal rules that define who owns what and how ownership changes. This *institutions-as-rules* view, Greif argues, fails to take into account that whatever the formal institution may be, the actual constraints that operate on people can be very different. The real institutions are the combination of the formal rules and actual social practices. He also argues that this earlier framework is ill suited to analyzing the formation, persistence, and evolution of institutions. To understand why growth promoting institutions took so long to develop we need a different framework that analyzes institutions as stable equilibria, where each participant is maximizes their benefit by adhering to the rules of the institution.

There is certainly profound truth in Greif's insistence that institutions are generally much more complex than the formal rules and that this creates major difficulties for institutional analysis. Any historian, for example, who used the formal rules of the institution of serfdom to analyze medieval English agrarian society would end up with an absurd description of rural life. Though the formal rules made the serfs the mere instruments of their lords, equivalent to the farm animals, with no legal status to appeal outside the court of the lord their master, the serfs ended up expropriating their masters and acquiring for their own use the land on which they were settled at very easy terms. They had to make payments to the court of the lord for such behaviors as marrying their daughter outside the manor or for extramarital sex. They had to labor in the lord's fields (or at least send a substitute). Yet, by 1300 or earlier, the masters would have gladly replaced their serfs with free tenants, had they been able, so generous were the customary rents to the tenants. The serfs ended up enslaving the lords. Serfdom disappeared in England without ever being formally abolished. Governments can make any rules they want, but what institution actually results will be determined by much more complex social interactions.

Formal usury restrictions can similarly have very different practical implications than any reading of the formal rule would imply. In early Christianity, and in Islam still, interest was regarded as usury, an immoral activity. But banning all lending at interest frustrates many possible mutually beneficial bargains in any economy. Thus, in both Christianity and Islam, religious scholars soon sought ways of reconciling the pure principles of faith, banning lending at interest, with the profit opportunities of the market.

While the Catholic church formally adhered to the doctrine against usury throughout the middle ages, ingenious theologians showed that most types of interest payment were actually nonusurous. By the late middle ages, the following exceptions on collecting interest on loans were all well accepted: *profits of partnership* (as long as each partner took the risks, returns were allowed on capital directly invested in an enterprise), *rent charges* (perpetual loans secured by real estate were allowed), *life annuities* (permissible since the amount of the payment was uncertain), *foregone profits* (compensation was allowed for profits foregone in making a loan), *exchange risk premium* (a premium on a loan was permissible if it was made in one currency and repaid in another, to cover the exchange rate risk). The prohibition on usury was thus extremely limited. Since there was still a demand for such loans this was met in two ways. The first was by allowing Jews, as non-Christians, to engage in such lending. The second was by simply ignoring the church rules when it proved convenient to the rich and powerful.

Islamic societies similarly found ingenious ways to circumvent the usury ban. The primary one was the *double sale.* In this transaction, the borrower would get, for example, both 100 dinars cash and a small piece of cloth valued at the absurdly high price of 15 dinars. In a year he would have to pay back 100 dinars for the loan of the cash and 15 for the cloth. These debts were upheld by Sharia courts. A study of Islamic court records in the Ottoman Empire in the sixteenth century found, even more blatantly, literally thousands of debt contracts being enforced by the courts. Similarly, the foundations set up by pious Muslims to maintain mosques, pay imams, support the poor, or provide public goods, the *waqfs*, frequently held cash assets that they lent at interest (Pamuk 2006). Even modern Muslim states that ban usury have banking arrangements where depositors still collect interest on their money, though in a "partnership" instead of explicitly as "interest." Such banks currently operate in Egypt, Kuwait, the Gulf Emirates, and Malaysia. So an institution is a complex of legal rules and people's responses to those rules.

As noted above, there is an implicit rejection here, but one not developed explicitly in the book, of almost all that currently passes for institutional analysis in economic journals. North and Barry Weingast, for example, have promoted the Glorious Revolution of 1688–89, where parliament effectively became the supreme legislative and executive body in England, replacing rule by Kings who were constrained only by the need to have Parliaments authorize taxes (North and Weingast 1989). But Greif would argue that the true nature of the change of institution cannot be deduced simply from the changes in formal rules. How then can we ever test the effects of institutions when we cannot identify their characteristics through such observable features as the explicit rules? Greif addresses this problem through the technique of the analytic narrative discussed below.

In chapter 2, Greif lays out a formal definition of an institution. This is, "An institution is a system of rules, beliefs, norms and organizations that together generate a regularity of (social) behavior" (p. 30). This reviewer was puzzled by what was gained in the subsequent analysis from the length and formalism of this section. Each of the component terms in the definition—"rules," "beliefs," "norms," "organizations," "regularity," "behavior"—is itself a loosely defined ordinary language term. We do not get clarity by defining one ambiguous concept in terms of six others equally ambiguous. At one level, this definition is so general that almost any behavior might constitute an institution. At another, the one specific element it contains, the insistence on regularity, seems to rule out some social arrangements that most would want to call institutions. Greif thus states "The object of study is restricted to regularity of behavior, meaning behavior that is followed and is expected to be followed in a given social situation by (most) individuals."

Consider, for example, Western European and East Asian marriage patterns before 1800. These were crucial determinants of living standards in the Malthusian era. In East Asia, marriage was early and universal for women, occurring typically within a tight band around age 19. Many people would call this marriage behavior an institution and, under the Greif definition, it would clearly qualify. In Western Europe, in contrast, first marriage occurred at all ages from 16 to 40, with a mean age of about 26. Typically ten percent of women never married. The absence of regularity in individual behavior seemingly implies that Greif would say that, in contrast to Asia, Western Europe lacked a marriage institution. Yet, though individual behavior was highly variable in Europe, there were high degrees of regularity from year to year, and from village to village, in the proportion of women who married and in the average age of marriage. Explaining that regularity thus becomes an interesting issue for historical demographers and there have been a number of explicitly institutional explanations for these regularities. But also, more widely, institutions that allow individual choice can be just as much institutions as those that constrain all persons to a narrow field of action. "Do what you want" can be as much an institution as "Do X."

There are other instances of regularities in behavior that we would not want to call institutions. Thus, in the preindustrial era, Europeans typically bathed little, while the Japanese bathed frequently. Was that a difference in social institutions? Most people would describe this as a difference in tastes. But undoubtedly it is a regularity of behavior induced by *a system of rules, beliefs, norms, and organizations.*

The purpose of the above is not to engage in a debate about how exactly institutions should be defined, but to suggest that such definitional exercises are premature. It would seem better just to work up from examples of institutions toward a more general conception, rather than begin with long encompassing abstract analysis that can only really be tested by considering specific examples.

As noted, Greif defines an institution as a self-reinforcing set of behaviors. Greif pioneered in applying game theory to historical institutional analysis and his 1993 study of the Maghribi traders remains a classic of this still modest genre. This was certainly an exciting development for economists. For the first time, seemingly grounded the explanation of informal institutions in optimizing individual rational behavior. Behaviors that would seem to the layman to be based on blind irrational custom could be shown to be consistent with individual optimization. Given the incredible intellectual elaboration of game theory, and its meager harvest in terms of actual economic applications, the finding was welcome to both game theorists and to economic historians. The Maghribi study also allowed for the possibilities of institutional change resulting just from changes in parameters. Since the equilibrium depended on certain parameter values, changes in transportation costs or observability could terminate the old equilibrium and lead to a new one. The 1993 article seemed to point to new micro foundations for institutions that would ground them in individual maximizing behavior.

But this book is almost certainly not what many economists who welcomed the 1993 article expected as the generalization of its ideas. Some indeed will be shocked by, and perhaps hostile to, the path Greif has taken. Were economists of a more literary bent, the word *apostasy* would be on their lips. In a search for generality, Greif concludes that such a set of limited rational actor assumptions is not constraining enough to describe real-world institutions. For a start, "multiple equilibria usually exist in the repeated situations central to institutional analysis" (p. 125). There have to be more constraints on the structure of the interaction to explain the equilibrium. These constraints include "cognitive norms" (p. 128) as well as "the social and normative foundation of behavior" (p. 143). Issues such as "losses of esteem," "norms," "fairness," or "social exchange" have to be introduced. Also such social and normative behavior is "situationally contingent" (p. 144).

Greif posits this as just an extension and elaboration of the original individualistic rational-actor game theoretic ideas. Once we are compelled to admit, however, into the explanatory apparatus almost the entire sociological zoo of ill defined and unmeasurable constructs, we lose all explanatory power. Explanatory power requires few objects and small degrees of freedom. Greif notes that "a useful feature of game theory is that it allows us to study all intertransactional linkages—economic, coercive, social and normative—simultaneously" (p. 147). But he does not seem to appreciate the price of this generality in terms of testability. All we are left with is the idea that people operating within institutions act as they do because, given the cognitive, intellectual, cultural, and normative constraints they face, their actions seem to them as being the best available. But, in an informal sense, we knew that already. Without any consideration of the ins and outs of game theory, we can appreciate that any lasting institution likely constitutes some set of self-reinforcing behaviors. Yanomamo males, for example, engaged in recurrent raids against other bands aimed at capturing women and revenging previous raids (Napoleon A. Chagnon 1983). This was clearly an institution in the sense of Greif and must be maintained by some kind of self-reinforcing set of behaviors. But we knew that, even if we had never studied game theory. So what insights have we gained from page after page of elaboration on the idea of equilibria and the elements that enter into them (pp. 124–53)? If we were able to reduce all such social equilibria to a game theory equilibrium of purely self interested rational individuals interacting with common knowledge that would be a radical, novel, and testable theory. This book denies that possibility, but without providing any alternative that has empirical content.

So far we have just considered the explanation of stable social institutions. But institutions can and do change over time. In search of a general theory of institutions, Greif has to also give an account of their dynamics, which he does at length on pages 158–216. It is clear that, in some cases, just a change in some other parameter can make the institutional equilibrium no longer self-reinforcing, and lead to another equilibrium. But, given the pervasiveness of multiple equilibria noted above, we cannot get even any potentially deterministic dynamics without resorting to all the elaborations discussed above. How then do past institutions influence the present? Greif posits that past institutional elements "reside in individuals' memories, constitute their cognitive models, are embodied in their preferences, and manifest themselves in organizations" (p. 188). Thus the institutional histories of societies matter as they settle on which of a range of technologically feasible institutional forms to use now. Here again the multiplication of terms and concepts continues. We learn about the "fundamental asymmetry," "institutional refinement," "the inclusion effect," "institutional complexes," "institutional trajectories," "contextual refinement," and more. The problem here is the multiplication of theoretical entities well ahead of even the handful of cases of institutional change that are analyzed in the book.

Chapter 11, titled *Interactive, Context-Specific Analysis* (pp. 350–76), is essentially a primer on how to apply the approach outlined in the rest of the book to the analysis of institutions. Greif here starts from the basis that we will never be able to predict institutional structure from exogenous features of the situation—including institutional history. He thus must reject the general feasibility of projects such as that of Acemoglu, Johnson, and Robinson (2001, 2005), or Stanley L. Engerman and Kenneth L. Sokoloff (2002), which seek exogenous roots for current institutional differences. Given the many potential stable equilibria in each institutional context, the outcomes are inherently unknowable. After the attention given to elaborating the theory of institutional stability and dynamics in the preceding 350 pages, this conclusion comes as something of a surprise. The structure and tone of the previous discussion is that of laying the groundwork for a theory of institutions. The reader now learns that the extended theory encompasses a perhaps uncountable number of possible institutional equilibria, so that there can be no advance prediction.

Just as deductive methods cannot succeed, Greif asserts also that inductive generalization about institutional forms will also fail to reveal any patterns. This is because unobservable elements of the situation—beliefs and norms—are crucial to the determination of the outcome. The same observable elements will be associated with radically different institutional equilibria. Further, as discussed above, the observable elements of institutions—rules, organization structures—often provide little insight into the actual functioning of the institution. Greif's work thus implicitly rejects most of the work currently classified under the *institutionalist* banner, which all too happily takes variations in formal political and legal frameworks as definitive measures of institutional variation, and then correlates that with economic performance.[5]

On pages 357–76, Greif summarizes his positive strategy for institutional analysis. First, learn the history and context of the institution. Next, the investigator should formulate the simplest conjectural model that might explain the equilibrium using only observable elements. The model should then be tested, and perhaps altered, based on empirical evidence, but "what is to be avoided in this interactive analysis is the tautology in which the model is adjusted to fit the evidence" (p. 366). This empirical evidence will include qualitative as well as quantitative material.

As conducted in the book, this is essentially the method of "analytical narratives" popularized by Greif and Robert Bates, Margaret Levi, Jean-Laurent Rosenthal, and Weingast. An analytical narrative consists of matching institutional detail to a formal, or more often informal, interpretation of the situation as some kind of rational choice equilibrium, interpreted in the broad sense above (Bates et al. 1998). It is not clear how this is distinguished from such things as Harvard Business School case studies. As applied by Greif and his colleagues, an "analytical narrative" seems to be just an interpretation of an institution in terms of a loosely defined equilibrium. This is fine as an approach to generating hypotheses, but as an endpoint of analysis, as it generally is in the book, it offers little conviction.

Consider, for example, Greif's application of the technique to the institution of the *Podesteria* introduced in Genoa in 1194, following factional fighting 1164–69 and 1189–94. The *Podestà* was a ruler hired for a limited term from outside the city, who was permitted by his contract to bring with him some magistrates and military retainers. These officials were initially imposed by

[5] Such works would include North and Weingast 1989; Acemoglu, Johnson, and Robinson 2001; and Edward L. Glaeser and Andrei Shleifer 2002.

Emperor Barbarossa as a way of controlling the Italian cities in his domains, but quickly became instead the servants of the cities. Greif interprets the key function of the *Podestà* in Genoa as being to serve as a balance of power between warring clans, strong enough to ensure that no clan would be powerful enough to attempt to take power on its own, but weak enough to be unable to take power himself. But though he begins by interpreting the role of the *Podestà* in the language of speculation fitting to the limited sources at his disposal—"theoretically" "arguably" "might" (pp. 241–42)—as the chapter develops the tone becomes one of proof and certainty. Two pages later,

> The Podesteria . . . was a self-enforcing institution: the belief that any attempt by a clan to gain political dominance by using force was futile deterred clans from doing so . . . . The Podestà himself was motivated not to allow one clan to become weak either, as his compensation was conditional on no clan dominating Genoa at the end of his term. As we have seen, the system was set to ensure that no clan would be able to commit to pay a Podestà his promised remuneration if that clan gained control over Genoa. (p. 244).

In fact, we have seen none of these things. They are just the consequences of a certain game-theoretic interpretation of the role of the *Podestà*. There is presumably not enough evidence in the sources to test what the relative military strength of the various clans versus the *podestà* was. This is not to say that Greif's interpretation is incorrect. Just that the loss of the clear distinction between evidence and hypothesis is an undesirable aspect of the analytical narrative technique as applied here.

An institution very similar to the *Podestà*, for example, was independently created in the black townships of South Africa under apartheid. Weekly competitions were held between male Zulu song and dance groups performing a style of music called *Isicathamiya*. These contests were by tradition judged solely by a lone white man, found by the participants by scouring the streets immediately before the contest for someone who would accept the offer of beer and cigarettes as payment (Veit Erlmann 1992). This judge would thus generally have no insight into the conventions and aesthetic of the performers, he could only serve once, but under apartheid his impartiality was assured. Hiring such a judge was costly to the performers: he had unknown tastes, it cost time to locate him, he had to be supplied beer and cigarettes. These white judges, often homeless or teenagers, did not command armed contingents in a complex game where they supplied a balance of power, as Greif supposes for Genoa. Their sole function was to award prizes in an assuredly faction free way. Given the limited sources for medieval Italy, how do we know that the *Podesteria* of the Italian city states was not more like the apartheid era *Isicathamiya* judges—disinterested distributors of prizes in faction-riven societies—than the powerful game-playing protagonists Greif supposes?

The insistence on the uniqueness of each institution makes the possibility of moving to certainty in interpreting institutions such as the *Podesteria* remote. The *Podesteria* was found in most Italian city states by the early thirteenth century. That would suggest empirical tests of the Greifian interpretation of the institution in Genoa. For example, in Florence a *Podesteria* on the Genoese model was firmly established by 1207, and the *Podestà* continued as the sole executive until the democratic revolution of 1250. In Florence, however, the various *Podestà* stood by largely ineffective as armed conflict recurred between 1215 and 1250 between the factions that became the Guelphs and the Ghibellines.[6] In Pisa, in contrast, "he [the *Podestà*] represented the domination of one group over another" (p. 306). Why, on Greif's theory of the Genoese *Podesteria*

[6] Ferdinand Schevill 1961; Francis A. Hyett 1903; W. F. Butler 1906.

would the Florentines want to hire a high priced supernumerary unable to quell factional violence? Why did Pisa, dominated by one faction, need such a useless ornament? And why would the Pisan *Podestà* ever get paid at the end of his term based on the analysis reported above?

Greif would answer, I believe, that alone and uniquely in Genoa the *Podesteria* succeeded in its inventors' design. But the persistence of the form in other city states in very different conditions suggests that the office may have fulfilled functions very different to the one Greif posits for Genoa. Greif can reject this criticism by analogy by insisting on the uniqueness of each social configuration. But this creates seemingly insurmountable barriers to ever getting to the truth about the functioning of particular institutions.

Does the Genoese *Podesteria*, in place for 150 years until 1339, explain the economic success of Genoa in these years? Again the focus on Genoa makes an answer impossible. Other cities with the same formal institutions and reasonable internal order, such as Pisa, did not do so well. Yet others, such as Florence, with a *Podesteria* ineffective in quelling internal violence, flourished in the early thirteenth century though the streets often ran with the blood of the contending factions. So, in sum, the analytical narrative provides no secure answer as to how the Genoese *Podesteria* functioned and certainly no hint of its role in explaining Genoa's economic success.

The language of fact continues in the section of the chapter describing the decline of the Genoese *Podesteria* in the fourteenth century. This is ascribed to changes in certain parameter values that eliminated the old self-sustaining equilibrium, such as the rise in wealth accumulation by nonclan members (pp. 224–26). Yet there is no evidence offered as to the values of these parameters. All that Greif knows, as fact, is that the *Podestà* was unable to prevent armed factional feuding within the city by the early fourteenth century. Even that, as we saw for Florence, is not a clear cause of its demise as an institution. The best the analytical narrative can offer here is a conjecture about the functioning of the institution: and even that conjecture is applicable only to Genoa, not Florence, Milan, Pisa or any other northern Italian city.

The problem here, in part, is that actual situations quickly become too complicated to model successfully using game theory. Genoa not only had feuding clans, but a variety of outsiders such as the Pope, the Holy Roman Emperor, and other city states, who could potentially ally themselves with individual clans, and internally a group of nonnobles, the *popolo,* with rising political influence. This was a cast of actors that only Shakespeare in his pomp could do justice to. The analytic narrative becomes likewise much more of a traditional political history case study, with a more elaborate vocabulary incorporating game theory intuitions.

In settling on analytical narratives as the way of resolving the radical indeterminacy embodied in game theory, Greif also closes institutions off from some promising other paths that might help narrow the range of possible equilibria and their dynamics. One of these is just the constraint of economic efficiency. Institutions and societies are often in competition and in such competition the societies that can generate more economic output have generally been favored. Radically inefficient equilibria tend thus not to survive. A second constraint, explored by evolutionary anthropologists, is that of reproductive competition in the hunter gatherer era that dominated human history, which may have hard wired some dispositions towards trust and cooperation. Chagnon thus tries to explain Yanomamo violence as a self-reinforcing strategy that rewards with reproductive success males who participate.[7]

[7] Chagnon 1988. Alexander Field (forthcoming), explores this issue of potential biological substrates to behavior.

In summation, Greif intends in his book to develop at least the outline of a new, micro grounded theory of institutions. Stating, explaining, and elaborating this theory takes 503 densely written pages, including a primer on game theory. By the end, however, this reviewer, to the contrary, read it mostly as a demonstration of the impossibility of a systematic account of institutions along the lines he proposes. The efflorescence of concepts, combined with the constriction of possible empirical tests, makes the hallmark of any scientific account, prediction and testing, impossible. And this shows in the case studies conducted in the book. Each institution in his formulation has to be analyzed in its full idiosyncracy, aided by the expert judgment of the investigator as to the social and epistemological context. But, as we saw in the case of the *Podesteria*, that kind of analysis, even in the hands of a careful enquirer like Greif, is fraught with the danger of conflating conjecture and fact. Kant's *Prolegomena to any Future Metaphysics as a Science* never led to his proposed science of metaphysics. Unfortunately Greif's *Prolegomena* to a future institutional theory similarly serves mainly to indicate the barriers to a science of institutions.

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