book proceeds to make a compelling case for public policy intervention. Chapter 3 poses the question, "Why should policymakers care?" The answer is reached through meticulous scrutiny of econometric evidence and case studies linking entrepreneurship to innovation and growth.

However, even in the face of this remarkable policy success, the more general implications for public policy in other contexts are not so obvious, as made clear by the title of the fourth chapter, "Things Get More Complicated." What is more complicated, as Lerner makes clear, is that not only does it matter which policies are pursued but also how they are implemented, that is with which instruments. As in other aspects of economic life, the devil lies in the details.

Lerner sifts through a complex set of policies, programs, and instruments and finds that there are four types of entrepreneur-enabling efforts classified into four categories—getting the laws right, ensuring access to cutting-edge technologies, creating tax incentive or removing barriers, and training potential entrepreneurs. In his chapters titled, "How Governments Go Wrong: Bad Designs," and its ensuing companion, "Bad Implementation," he makes the unusual choice of presenting not only best practices but rather worst, or at least bad, practices.

The good, the bad, and the ugly policies to promote entrepreneurship are assessed in a simple and compelling assessment in the final chapter, "Lessons and Pitfalls." These insights are no-nonsense guidelines and rules-of-thumb for entrepreneurship policy.

The gulf between academic scholarship and real world public policy often seems wide and insurmountable. In this book, Josh Lerner spans the gap between theory and practice, making it clear how important scholarly knowledge can be as a guide for framing public policy. While the title suggests a pessimistic view toward entrepreneurship policy, policymakers making use of the rich and nuanced insights and experiences garnered by Lerner in this book in devising and implementing entrepreneurship and venture capital policy would actually go a long way toward ensuring a more positive and optimistic outcome.

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N Economic History

Natural Experiments of History. Edited by Jared Diamond and James A. Robinson. Cambridge and London: Belknap Press of Harvard University Press, 2010. Pp. 278. $29.95. ISBN 978–0–674–03557–7. *JEL 2010–0642*

Natural Experiments of History offers a defense of quasi-experimental methods (and a partial how-to manual) aimed at historians and other "receptive" social scientists. The empirical strategies outlined in the volume will already be familiar to and well-accepted by most applied microeconomists. The volume's editors, scientist Jared Diamond (lately of a geography department) and economist James A. Robinson (lately of a government department), define a natural experiment as a "controlled comparison" between "systems that are similar in many respects but that differ with respect to the factors whose influence one wishes to study" (p. 2). This statement is broad enough to encompass a range of methods, from multivariate regression analysis to instrumental variables techniques. What the editors have in mind most closely resembles a difference-in-differences research design that compares two areas as one is subjected to some form of treatment, whether a policy change, political event, or an inflow of people or goods.

The editors' methodological views are most clearly laid out in the volume's afterword. In it, they define two types of natural experiments: those that compare areas with *similar* initial conditions that are differently "perturbed" (or, in economist parlance, differently "treated") and those that contrast areas with *different* initial conditions that go on to face a common set of events. The first case is more common in economics and the book contains a few prime examples, including a version of Nathan Nunn's work assessing the role of the slave trade on the subsequent economic performance of African countries and a chapter by Daron Acemoglu and coauthors on the effect of Napoleonic conquest and the spread of liberal institutions on German regions. The second type of natural experiment is somewhat rarer; the main example provided in the volume is the colonization of Polynesian islands that varied in size, soil quality, and so forth by peoples of a common origin around the year 800 CE.

The most important lesson that the editors provide to potential new users of these methods is that, before studying any apparently "natural" experiment, researchers must first ask themselves why some units of analysis experienced the perturbations of history while others did not. Unlike in a laboratory, where the researcher can ensure that the initial conditions faced by experimental cases are truly identical, in the realm of history, one must wonder why human actors chose to act upon one stage and not on others. For example, why did Napoleon decide to invade the Rhineland but not Baden? If the path of Napoleon's armies were determined solely by military or political expedience with no heed for factors that may predict future economic success, such as population density or the availability of provisions, then the Napoleonic invasion can be considered a truly natural experiment and we can evaluate its effects on later economic activity accordingly.

None of these tips about effective research design will come as a surprise to economists who have already embraced the logic of natural experiments. The book is pitched to "receptive" historians and other social scientists and, indeed, I could imagine assigning the afterword in a graduate methods class in historical sociology, political science or, possibly, in some broad-minded history departments. However, the volume contains few methodological propositions that are not already featured as part of the standard curriculum in applied field courses in economics departments, including in courses on economic history. Various handbook chapters, including the modern classic by Joshua D. Angrist and Alan B. Krueger (*Handbook of Labor Economics* 1999), offer explicit introductions to experimental thinking, while many journal articles, including the work by Nunn, Acemoglu, and others that is synthesized in this volume, provide useful templates for teaching graduate students about how these methods can be successfully applied.

Given that natural experiments are, at this point, quite accepted within economics, the volume must be judged for how successfully it engages historians and other social scientists who might benefit from this approach. Unfortunately, the volume does little to reach across the often vast divide between the historical social sciences and the field of history as it is commonly practiced. None of the eleven contributors are members of a history department nor are there any economic historians (cliometricians), who may have formed a natural bridge between these two groups. As a result, it may be all too easy for historians who are already skeptical about the concept of natural experiments in particular and quantitative work in general to dismiss the book and the methods that it represents.

Anecdotally, I found this indifference borne out when I attempted to engage friends from various disciplines in short conversations about the concept of natural experiments. I emailed twenty colleagues and solicited their opinions about the usefulness of Diamond and Robinson's concept of a natural experiment ("comparing . . . different systems that are similar in many respects") without revealing the source.[1] A short note saying only "it sounds like a lot of malarkey to me" represents the standard response from historians. Indeed, natural experiments are not currently part of historians' regular vocabulary and, if we are to engage historians in a methodological conversation with an aim toward converting some and at least being understood by others, perhaps we need to think more carefully about how to translate these concepts most effectively.

Of course, some historians already use "controlled comparisons" (a phrase that the editors use synonymously with natural experiments) even if their sample sizes are small and their identifying assumptions may not be stated explicitly. Stephen Haber's essay in the volume comparing the development of the banking system in three New World economies is one example; others include Kenneth Pomeranz's comparative history of industrial development in Europe and China, *The Great Divergence*, and Rebecca J. Scott's *Degrees of Freedom: Louisiana and Cuba after Slavery*. For these "receptive" historians, the volume may be a helpful guide to the formal logic and terminology of natural experiments, while also providing gentle encouragement to clarify

[1] Because the volume is intended for a wide audience throughout the social sciences, I found it particularly useful to solicit opinions in person and by email and Facebook from colleagues in many disciplines. My thanks to all who participated in these conversations with me. All opinions expressed in this review and any errors of fact are, of course, still my own.

the often implicit assumptions behind their selection of comparative cases.

However, the volume has little, if anything, to say to historians who see their intellectual mission not as isolating causal factors to explain a single phenomenon but rather as providing a detailed reading of social phenomena as they unfold within a specific time and a place. These historians tend to view causal relationships as intricate, context-specific, and difficult to pry apart into separate strands and, therefore, rarely aim to *prove* explicitly how *A* causes *B*. Perhaps the proper message to this group is not an exhortation to master "our" methods but instead an invitation to engage with us, at times, in a division of academic labor. That is, rather than attempting to convert historians into second-rate economists, we, as economists, may be able to learn more from historians doing what they do best: uncovering new facts about the past and interpreting historical evidence with attention to the context in which the evidence was produced.

In addition to being an end in itself, historical work of this kind can be a useful input into the type of causal analysis conducted in the social sciences in a variety of ways. First, and most simply, historical work can help establish the plausibility of the identifying assumption that units are selected for treatment on a quasi-random basis necessary for the conduct of convincing natural experiments in various contexts. Consider the Acemoglu et al. essay in this volume evaluating the effect of Napoleonic occupation on Germany economies. The force of the argument depends on proving that the path taken by Napoleon's armies was determined by military, rather than economic, considerations. For this task, the authors draw upon the work of many "traditional" historians of nineteenth century France and Germany.

Furthermore, historians' rich institutional knowledge may help establish the proper extent of external validity for causal relationships determined in experimental settings. All of the essays in the book address broad questions—such as, what is the role of institutions in economic development?—by analyzing a specific historical context. But can we transplant lessons learned about the role of institutions from nineteenth-century Germany to the nineteenth-century United States? How about to developing countries today? Historians can help us think about how these cases may be either similar enough to learn from or too different to compare. (Perhaps historians would too often reject the possibility of learning about *A* from *B*, claiming that *A* and *B* are irreducibly different. Yet even this note of skepticism can be helpful to historical social scientists that otherwise may have tendencies to overgeneralize from specific findings).

My sense is that historians would be much more receptive to the idea that natural experiments can be the right tool for *some* historical questions if their proponents did not insist that they are the right methods for *any* historical questions worth asking. It is not coincidental that, despite differences in region and time period, all of the studies in the volume address the process of colonization, or at least globalization and trade, in some way. Three of the essays cover postindependence New World societies, while the remainder address colonial policy in India, the peopling of Polynesian islands, the occupation of German regions in the Napoleonic period, and the global slave trade. Colonialism is rife with opportunities for natural experiments to arise as, for example, the winds blow would-be colonists this way instead of that or as weather shocks dictate the path of invading armies.

In general, any moment of historical rupture, whether due to colonialism, military activity, or scientific invention, is more conducive to the development of natural experiments than are moments of historical continuity and secular change. As a result, adopting the methods of natural experiments will inevitably lead scholars to pursue one set of questions and not others. Consider, for example, historical explanations for the rise of female labor force participation in the twentieth century. The role of the birth control pill on women's employment has arguably received more attention among economic historians than other, equally plausible factors, such as changes in attitudes and social norms, because of the exogeneity of the discovery and diffusion of the pill (see, for instance, work by Claudia Goldin and Lawrence F. Katz and by Martha J. Bailey). In raising this example, my intention is not to argue that practitioners of natural experiments, a club to which I very much belong, should put aside their techniques in despair that they will never be able to address all types of historical causes. Rather, my view is that, because any method will be appropriate for some questions but not others, our collective understanding of the past will be improved by cultivating a variety of approaches across many disciplines, instead of insisting on methodological uniformity.

Finally returning to the group of "receptive" historians to whom this volume is targeted, I wondered what their reaction would be to the parenthetical suggestion that natural experiments "preferably [be] quantitative and aided by statistical analysis" (p. 2). In an effort to be welcoming, the editors describe large sample sizes as "preferable." But could it be that they are, in fact, necessary? After all, even if two subjects start with identical initial conditions, as is the case, for example, in studies of monozygotic twin pairs, researchers still compile large samples in order to extract causal signals from noise. Just as economists would not trust the results from a twin study conducted on a single set of siblings, should we as social scientists reject historical case studies that compare, for example, one city to another? If so, how can we ever hope to share methods with even the most receptive of historians?

One answer to this question is that historical case studies can generate hypotheses that can be further tested by gathering a large sample that can be subjected to statistical scrutiny.[2] Diamond's essay in the volume offers an useful template of this approach. In the first half of the piece, he notes fundamental differences in the economic development of Haiti and the Dominican Republic; despite being located on the same island, GDP per capita in the Dominican Republic is six times higher than in Haiti. Haiti, he observes, is also substantially more deforested than its eastern neighbor. But, deforestation is only a proximate cause for underdevelopment. Diamond digs deeper to search for underlying causes of this environmental outcome, suggesting that Haiti may suffer from a less suitable micro-climate or from a destructive colonial past. In order to determine the importance of these various factors, Diamond moves beyond the two-part comparison to a dataset of sixty-nine Polynesian islands, some of which also suffered from devastating episodes of deforestation. Haber's essay on banking systems in the United States, Mexico, and Brazil provides another example of how this shared scholarly process could operate. Haber proposes various causal factors that can explain the emergence of a democratic banking system, including broad-based suffrage and political competition. Further tests of the "Haber hypothesis" would require collecting a larger sample in another setting, for example comparing across U.S. states.

Few of us, as individual scholars, have the time, resources, or aptitude to both perform in-depth case studies and collect large datasets to test hypotheses using statistical methods. As a result, conducting natural experiments in history will require an academic division of labor that includes historians and historically inclined social scientists. *Natural Experiments of History* offers a first step in this interdisciplinary conversation, providing a valuable primer in experimental logic for scholars amenable to the idea of controlled comparisons. However, I think that the conversation should go substantially further than it does in this volume and believe (hopefully not too naively) that historians of many persuasions can be persuaded that our methods are complements and that there can be substantial gains from trade across the disciplines.

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[2] Some economists may reject the idea of generating hypotheses from historical case studies, arguing that hypotheses should arise from models rather than from observation. While addressing this philosophical debate is beyond the scope of this review, I will simply say here that, to my mind, it is a mistake to view historical observation as "a-theoretical." Rather, I believe that theory of some kind—whether explicit or implicit—will always determine the selection of historical cases, the variables to be compared, and the interpretation of evidentiary patterns.

O Economic Development, Technological Change, and Growth

Information Technology and Productivity Growth: German Trends and OECD Comparisons. By Theo S. Eicher and Thomas Strobel. Ifo Economic Policy series. Cheltenham, U.K. and Northampton, Mass.: Elgar, 2009. Pp. viii, 102. $90.00. ISBN 978–1–84844–091–3.

JEL 2010–0676