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Summaries of Doctoral Dissertations

Essays in International Political Economy

My dissertation studies three important questions in international political economy: The long-run consequences of social divisions, the endogenous evolution of institutions, and how coercive labor market institutions determine the distribution of the gains from trade.

I study these three questions in two very distinct populations: Native American reservations and the British Caribbean.

The first paper asks whether the large differences in economic development across Native American reservations today can be explained by social divisions that were created more than 150 years ago when the U.S. government forcibly integrated distinct Native American bands into shared reservations, condemning them to a system of shared governance that was not consistent with their political traditions and tribal identities.

The second and third papers study the politics and economics of the nineteenth-century British Caribbean, a "natural laboratory" of 17 highly comparable but in precise ways distinct islands.

The first Caribbean paper provides insights into the working of colonial institutions by showing how planter elites manipulated representative institutions to maintain their control over policymaking after Abolition had freed over 95 percent of the population from slavery.

The second Caribbean paper—coauthored with Dan Trefler and Avner Greif—studies the nexus of trade, institutions, and economic development. It embeds coercion into a standard principal-agent framework to decompose the effect of output prices on wages into a neoclassical and a coercive/institutional effect and to show how the division of the gains from trade depends on institutions and how institutions themselves are shaped by trade rents.

In the following, I sketch the main points of each of the three papers.

FORCED COEXISTENCE AND ECONOMIC DEVELOPMENT: EVIDENCE FROM NATIVE AMERICAN RESERVATIONS

The motivating fact for the first paper is that there are very substantial differences in income levels across Native American reservations today, despite the fact that reservations share very similar histories and operate within the same framework of exogenously imposed local institutions and federal Indian policy.

The paper argues that these differences can in large part be explained by something that happened on average more than 140 years ago, i.e., the historical process of reservation formation. It uses a combination of historical data on reservation formation and anthropological data on tribal structures from Murdoch's Ethnographic Atlas to

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I omit references from this summary to conserve on space. This should not take anything away from the important papers which provide the context to my dissertation.

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show that many reservations were formed out of subtribal bands that had historically been separate autonomous political units, something I call Forced Integration, and that forcibly integrated reservations are today about 30 percent poorer than otherwise identical reservations. The paper uses an IV strategy based on historical mining rushes which exogenously changed the government's incentive to overcome resistance by local bands against being integrated onto shared reservations. The resulting IV estimates confirm that the 30 percent reduction in incomes seems to constitute a causal effect of forced integration on outcomes today.

The paper then moves from the cross-section into panel data. It shows that the effect of forced integration was constant and relatively small from 1970 to 1990 and then magnified in the 1990s. Two-thirds of the effect in 2000 is explained by differential growth in the 1990s. Strikingly, this implies that something that happened 140 years ago determines incomes today to a larger degree than 30 years ago.

Rather than a persistent poverty trap, this pattern points to an explanation where small differences in social organization survived unnoticed in the background as a latent variable until they interacted with big changes in the regulatory environment which put decision-making power with reservation governments in the late 1980s.

Turning to mechanisms, I argue that the most important link between forced integration and economic outcomes is likely the political channel. This is because federal regulation in 1934, which specified the institutional rules imposed on reservations, put almost no checks and balances on the power of local politicians so that politicians who cater only to their groups' interests have particularly destructive effects. While I cannot pin this mechanism down, I provide evidence from newspaper counts of reported reservation internal strife to support the view that forced integration is indeed strongly associated with political conflict.

The paper adds to an already large body of evidence that historical events can have large effects on economic outcomes today. In contrast to much of the literature on the effect of social divisions, this paper arguably estimates a causal effect rather than partial correlations. It is also to my knowledge the first paper that studies group divisions below the ethnic level, in this case between bands within the same Native American tribe.

The evidence on timing in this paper is important for our thinking of economic growth because it suggests that small differences in social organization can survive latently in the background for long times until they begin to really matter when there are big changes in the environment.

On a positive note, there is anecdotal evidence to suggest that this divergence within Indian county is self-limiting in the sense that poorer reservations are now actively looking to implement institutional innovations that help them emulate more successful reservations.

INSTITUTIONAL CHANGE AND ELITE PERSISTENCE: THE NINETEENTH-CENTURY CARIBBEAN

The second paper, the first of two Caribbean papers, starts with the motivation that when it comes to historical colonialism, there is a view in the empirical institutions literature that at the time colonies were settled, initial conditions determined whether Europeans instituted inclusive or extractive institutions and that these initial choices continue to impact long-run development to the present day through institutional path dependence. A salient example is the contrast between the so-called "Neo-Europes" —places like Canada, the United States, Australia, and New Zealand—which had high

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settler mortality and low indigenous population densities and which enjoy good inclusive institutions and high income levels today, and places like the Caribbean in which the opposite is true.

Despite this apparent contrast, it turns out that in terms of de jure inclusiveness, i.e., local representative institutions, the British Caribbean colonies were set up with the same institutions as the Neo-Europes. Furthermore, the Caribbean colonies retained their inclusive representative institutions well into the nineteenth century. It was only in the middle of the nineteenth century, that these democratic institutions contracted and there was an institutional divergence between the inclusiveness of Caribbean and Neo-European representative institutions.

Rather than quantitatively comparing the Caribbean and Neo-European colonies, this paper then focuses on only the Caribbean—getting empirical identification only off a panel of institutional changes in the Caribbean.

The paper's main contribution is to explain this late nineteenth-century contraction of representative institutions in the Caribbean. When slavery was abolished in 1838, the Caribbean was dominated by small planter elites, which Abolition pitted against a vast majority of freed slaves. As freed slaves were becoming smallholders, the franchise expanded because the right to vote was tied to landownership. Lacking the coercive capacity to effectively prevent this, planter elites were faced with an increasing likelihood of having the median voter become a freed slave. Running out of options, white elites abolished the formal powers they had exercised through their locally elected parliaments and ceded control over government to the British colonial administration. In a regression setting, the gradual exogenous expansion of the franchise, driven by a slow process of freed slaves obtaining legal title to their smallholdings and therefore the right to vote, explains the timing of the described constitutional changes. Data on the identity of elected politicians shows that the expanding franchise led to constitutional changes because it increased political competition for planter elites in the local parliaments.

The paper then investigates the effect that ceding local representation had on local policy choices. The existing literature suggests two priors: One, ceding local representation should have relaxed the executive's spending and taxation as the taxed gave up formal control over the budget. Two, spending should have shifted in favor of the poor because colonial administrators were ostensibly more likely to act in their favor than local elites. The data rejects both hypotheses. When local elites ceded their de jure elective powers, taxation and spending did not increase and if anything, pro-poor spending declined further from an already low base.

The paper's first main point is simply that there was in fact relatively little difference in the formal institutions of the two sets of colonies that we typically think of respectively as inclusive and extractive.

The paper's second main point is that initial conditions' primary effect on long-run outcomes did not necessarily work through the shaping of initial institutions but rather through shaping an elite-dominated political economy which only translated into a divergence in formal institutions when the big exogenous shock of the Abolition of slavery pitted planter elites against their former slaves.

This matters for how we think of the role of initial conditions in long-run institutional and economic development.

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The paper's third main point is that the effect of endogenously changing institutions is likely to be very different from the effects of exogenously imposed ones, and that the explanation likely has to do with the fact that elites were willing to give up their de jure powers precisely because they anticipated that they could offset them with increases in de facto collective action.

TRADE RENTS AND COERCIVE LABOR MARKET INSTITUTIONS: THE NINETEENTH-CENTURY CARIBBEAN

The third paper studies the relationship between exogenous output prices and wages when employers can invest into coercive labor market institutions. The motivation for this paper comes from the fact that we think of the "first" globalization in the nineteenth century mostly in terms of the neoclassical Heckscher Ohlin framework, which says that labor gets paid its marginal product.

This is at odds with two other literatures in economics: First, a literature on coercive labor markets suggests that most labor markets throughout history were coercive in the sense that employers could invest in technologies that would make workers accept labor contracts they otherwise would not. Second, a literature at the intersection of trade and institutions suggests first that institutions determine how the gains from trade are divided and second that trade rents themselves influence institutional development.

While the existing empirical evidence on the first globalization is broadly consistent with the neoclassical view, most data comes from countries that even back then had relatively well-functioning factor market institutions. Looking beyond this rich country club in the nineteenth century invariably means looking at colonies, and the Caribbean offers the particular advantage that it consisted of many separate but comparable labor markets.

The paper attempts to unpack the effect of output prices on wages into a standard neoclassical and a nonstandard institutional channel. In a principal agent framework, it allows the principals (plantation owners) to invest into a coercive technology that reduces workers' outside option (of being smallholding farmers) and makes them accept a lower wage by relaxing their participation constraint. In practice, that meant planters would manipulate local courts and legislatures to press hard on smallholders through measures like eviction and high parochial taxes.

The theory unpacks the relationship between the price of plantation goods and wages: Lower prices reduce wages through a neoclassical channel but also shrink the economic and therefore political power of planters, which raises wages through lowered coercion.

These patterns are strongly confirmed in the data: In a panel of Caribbean colonies from 1838 to 1914, there is absolutely no correlation between the price of sugar, the main plantation crop, and wages. However, once we unpack this reduced form relationship into the two channels, we find that both channels are highly significant but exactly offset each other. Additional support for an institutional story comes from regressions of direct measures of coercion, rates of imprisonment, and court convictions, which indeed strongly correlate with the dominance of the plantation economy.

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Innovation and Industry Development: Lessons from the British Cotton Textile Industry During the U.S. Civil War

This dissertation uses the large shock to the British cotton textile industry in the nineteenth century, caused by the U.S. Civil War (1861–1865), in order to address long-running questions about technological progress and industry development. The cotton textile industry was a large and important sector in the British economy during the nineteenth century. The industry was entirely dependent on imported raw cotton, most of which came from the U.S. South prior to the Civil War. The onset of the war sharply reduced the supply of Southern cotton to the British market, causing a severe downturn in the industry. In response to the shock, cotton textile producers turned to other sources of supply, chiefly India, but also Egypt, Brazil, and others, to help meet their raw cotton needs. But cotton from these alternative suppliers, and India in particular, was very different from the U.S. cotton that British producers were used to spinning.

The first two chapters of this dissertation describe how the cotton textile industry developed new technology in order to deal with the challenges posed by the shortage of U.S. cotton and what this response can tell us about the process of innovation. Chapter 1 focuses on directed technical change, the idea that changes to the availability of inputs to the production process can affect the type of new technologies that are developed. Chapter 2 looks at whether an increase in the stock of a particular type of technology generates incentives for further innovation in that technology type. Chapter 3 looks at how the depression in the cotton textile industry affected other industries through localized interindustry connections, providing causal evidence that temporary shocks to one industry can have a long-term impact on the location of related industries.

This historical setting has a number of important features. First, the shock was both large and exogenous. The size ensures that the response will be large enough to clearly observe, while the fact that it is exogenous means that it can be used to uncover causal relationships. Second, the impact was largely industry-specific; while the impact of the war on the cotton textile industry was severe, most other sectors of the British economy, including textile industries based on wool, linen, and silk, were not directly affected. Thus, I can control for other time-varying factors by comparing the cotton textile industry to these other similar industries. This also means that I can uncover evidence of interindustry connections, since other industries will be affected through their relationship with the cotton textile industry.

Chapter 1 considers the idea that a change in the availability of inputs to the production process can influence the direction of technological progress. The main contribution of the chapter is to provide the first well-identified empirical test of the leading theory of directed technical change (Acemoglu 2002). Both of the main predictions of the theory are confirmed by my results.

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The idea of directed technical change, first suggested by John Richard Hicks (1932), has been used to explain a diverse set of economic phenomena. For example, economic historians have suggested that a shortage of labor drove the development of labor-saving innovations which played an important role in industrialization in Britain and the United States (Habakkuk 1962; Allen 2009). Despite wide application, the predictions of leading directed technical change theories remain largely unexplored empirically.

The model introduced in Daron Acemoglu (2002) provides two new predictions about directed technical change. The first is that the direction of technical change depends on the elasticity of substitution between inputs. When this elasticity is low, technical change will be directed towards augmenting the relatively scarce input. In contrast, when the elasticity of substitution between inputs is high, technical change will be directed towards augmenting the relatively more abundant input. The second prediction is that when the elasticity of substitution between inputs is sufficiently high the relative price of the more abundant input can increase, the strong induced-bias hypothesis. The first step in the empirical analysis is to estimate the elasticity of substitution between inputs so that I know the predictions of the theory. Using the shock as an instrument, I find that the elasticity of substitution between U.S. cotton and Indian cotton is well above one and likely above two. Given these, the predictions of the model are, (1), that we should observe technical change that augments Indian cotton, and, (2), that the long-run relative price of Indian to U.S. cotton should increase.

To track innovation patterns, I collected detailed new British patent data from over 1,500 pages of printed documents. These data reveal a sharp increase in cotton-related technology patents during the 1861–1865 period, on the order of 52–67 percent. In contrast, patents related to wool, linen, or silk show no similar increase. The increase in cotton textile innovation was driven by an increase in patents for machines used to prepare the raw cotton for spinning—gins, openers and scutchers—which were needed in order to take advantage of the lower quality Indian cotton. Thus, I show that technical change was directed towards technologies that augmented Indian cotton, confirming the first major prediction of the theory.

Next, I consider the impact on input prices. While the price increased substantially for every cotton variety during the Civil War, my focus will be on what happened to the price of Indian cotton relative to U.S. cotton. In the absence of directed technical change, the model predicts that the relative price of Indian cotton should have fallen as it became relatively more abundant. However, the new technologies developed for using Indian cotton may offset this, by increasing demand for that variety. Graphing the relative price of Indian to U.S. cotton shows a decrease in the first two years of the Civil War, followed by a rapid rebound starting in 1863, around the time that the new technologies were becoming available. To control for other time-varying factors, I compare this pattern to those observed for Brazilian and Egyptian cotton, two smaller varieties for which I found no evidence of directed technical change. My results are consistent with the existence of strong induced bias acting on the relative price of Indian cotton, confirming the second major prediction of the theory.

Chapter 2 considers the possibility that the stock of existing knowledge related to a particular technology type may influence the level of innovation in that technology, which I call path dependence in innovation. Whether or not path dependence in innovation exists is central to our understanding of the process of innovation and economic growth, yet currently empirical evidence on this topic is sparse.

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Existing studies of path dependence in innovation, such as David Popp (2002), construct measures of the rate of innovation and the stock of knowledge for different types of technologies using patent data. They then run regressions comparing the current level of innovation on the lagged knowledge stock. While these studies make an important contribution, their approach is susceptible to a number of potential sources of bias. For example, a persistent positive shock to innovation rates in one technology will increase both the stock of knowledge and the level of innovation, generating spurious evidence of path dependence in innovation. To address these sources of bias, I offer an alternative approach that takes advantage of an exogenous temporary shock that induces innovation related to a particular set of technologies. If there is path dependence in innovation, then the resulting increase in the knowledge stock should lead to a higher rate of innovation in these technology types in the post-shock period. I implement this approach using my empirical setting. As discussed, the cotton shortage led to a surge in cotton textile innovation during the 1861–1865 period and this increase was concentrated in two particular types of cotton textile machinery.

I first look for path dependence in innovation by comparing patenting patterns in the cotton textile industry to the wool, linen, and silk industries. I find that despite the sharp increase in cotton textile patents during the 1861–1865 period, there is no evidence of a continuing higher level of innovation after 1865. This result is strengthened because I expect that any changes caused by the Civil War which persisted after 1865 should cause an increase in innovation in the post-shock period. Thus, this paper provides clear evidence against the existence of path dependence in innovation at the industry level. Next, I look for path dependence within particular types of cotton textile technologies. Two of these eight technology types—gins and openers/scutchers—experienced a large increase in patents during the 1861–1865 period. The analysis focuses on whether higher levels were sustained in the years after 1865, while using the remaining six technologies as controls. I find weak evidence of path dependence in innovation at the level of specific types of textile machinery. At both the industry level and the level of particular machine types, my results differ significantly from what I would have found using the previous methodology, highlighting the importance of my new approach in addressing potential sources of bias.

Chapter 3 considers the idea, suggested by Alfred Marshall (1890), that firms in different industries may benefit from locating near to one another, through localized interindustry spillovers. Specifically, I ask, can temporary changes in the local availability of interindustry spillovers, such as those created by economic shocks or policy interventions, have long-term impacts on the geographic location of economic activity? This question is particularly relevant for policies, such as local tax incentives or special export zones, yet it has not been addressed by the existing literature.

This chapter exploits the fact that the shortage of U.S. cotton during the Civil War severely impacted the textile industry in Lancashire County, the heart of Britain's cotton textile industry, while economically similar towns in Yorkshire, which specialized in wool textiles, were not negatively affected. The basic hypothesis that I test is that the shock negatively affected employment and employment growth in industries more closely related to the cotton textile industry, in locations more severely impacted by the shock, by reducing localized benefits available to these industries. Importantly, I focus on long-run impacts, those occurring in the years and decades after the Civil War had ended and the cotton textile industry had rebounded.

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This intuition can be illustrated using an example from the empirical setting, provided by the Engine & Machinery industry (E&M), which was an important industry in Britain at this time. E&M firms in Lancashire and Yorkshire competed with each other and shared interindustry connections to their local textile industries. Also, my data show that the E&M industry had a similar growth path in the two locations prior to the shock. Did the loss of localized spillovers in this industry in Lancashire towns affect growth in those towns relative to E&M growth in unaffected Yorkshire towns? In fact, I find evidence that E&M firms in Yorkshire towns gained an advantage relative to Lancashire producers in the decades following the shock, allowing them to expand employment more rapidly.

I look for similar patterns more generally by using panel data to compare across five decades (pre- vs. post-shock periods), eleven locations (towns with higher vs. lower shock intensity), and 171 industries (more vs. less related to cotton textiles). The primary data are drawn from the British Census of Production, which were gathered from original sources. I measure the intensity of the shock in each town using data from local Poor Law Boards. The relatedness of each industry to cotton textiles is calculated using the coagglomeration measure proposed by Glenn Ellison and Edward L. Glaeser (1997) and Ellison, Glaeser, and William R. Kerr (2010).

My results show that industries more closely related to the cotton textile industry suffered lower employment growth in more severely affected towns in the post-shock period. This implies that interindustry connections can play an important role in affecting the geographic location of economic activity across locations. Moreover, the impacts continue to appear through the 1881–1891 period, suggesting that temporary changes in the availability of these connections have the potential to generate long-lasting effects.

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Human Capital, Technology Diffusion, and Economic Growth: Evidence from Prussian Census Data

Prussia was a latecomer to the Industrial Revolution. Many researchers argue that she lacked the institutional preconditions for sustained economic growth. Throughout the eighteenth century, Prussia had been under strong conservative feudal rule. Tumescent calls for reforms remained disregarded until the lack of reforms was blamed for Prussia's defeat in the Napoleonic Wars in 1806. During the Napoleonic occupation period, until the Congress of Vienna (1815) reorganized the national boundaries of Europe, the "Stein-Hardenberg Reforms" changed the basic institutional setting, affecting all members of society in Prussia. These reforms were the foundations that allowed the country to become one of the economic leaders, by the end of the nineteenth century (Pierenkemper and Tilly 2004, pp. 23–41).

While the Prussian reforms as well as the Zollverein (customs union) might have set the preconditions that helped Prussia to become one of the great economic powers, they cannot explain why some regions within the country became more successful in seizing the emerging opportunities than others did. In order to understand in detail the developments that took place during the nineteenth century, one needs to study the sub-national level and to explain the location of industrial growth. Friedrich List, one of the most influential economists of nineteenth-century Germany, emphasized that the immigration of skilled workers contributed to early growth in Prussia, whereas education encouraged industrial development after the Napoleonic War (List 1856, pp. 152–53). He was also one of the great minds who advocated German railroads, the technology that soon became extremely important for economic growth during the nineteenth century. He thus emphasized two aspects of economic growth that exhibit regional variation—human capital and technology.

This thesis studies four different aspect of how human capital and technology diffusion shaped the growth and development of historical Prussia, using unique eighteenth- and nineteenth-century data. Within this period, Prussia managed the transition from a backward agrarian economy, marked by feudal conservatism, to become one of the great industrial powers of Europe. How did she do this? This thesis argues that regions within Prussia that possessed high levels of human capital and adopted advanced technologies were particularly successful during the process of industrialization and were able to catch up faster to the economic leaders of the world than others.

This thesis analyzes completely new data sets, originally produced by the Prussian Statistical Office. These sources, all of which digitized from official censuses for the purpose of this work, provide high-quality data allowing to draw a comprehensive picture of the developments during the period under consideration. Such data are very unique for the eighteenth and nineteenth century and Prussia is practically the only country to provide comprehensive data sets in such detail. Each chapter studies a different relationship at the micro-regional level in historical Prussia. Two main

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advantages over cross-country comparisons arise from such an approach. First, the data stem from a single standardized source, they are consistently defined and thus systematically comparable, which often is not the case in cross-country studies. Second, by comparing regions within one country, the geographical, institutional, and legal framework becomes much more comparable.

The structure of the thesis follows a chain of causal effects from human capital, to technological diffusion, to economic growth in Prussia. After a short introduction, chapter 2 studies how the implementation of agrarian reforms paved the way for educational expansion. Chapter 3 studies the effect of education on technological catch-up. Chapter 4 studies the effect of skilled immigration on technological diffusion, and chapter 5 studies the effect of the diffusion of a particular technology on growth.

Chapter 2 provides evidence that regions with a high concentration of large landholdings delayed the expansion of education until the end of the nineteenth century.[1] The institutional reforms were not fully implemented during the first half of the nineteenth century. The freedom of a large fraction of the rural labor force was still limited by manorial farming and feudal rule. Consequently, we find that the regional concentration of large landholdings—which serves as a proxy for the institution of serf labor—has a negative effect on school enrollment rates. Using county-level landownership and school enrollment data from 1816, 1849, 1864, 1886, and 1896, we are able to study how the effect changes over time. Comparing the five periods, we find that the effect diminishes substantially in the second half of the century, after the complete abolition of serfdom and the transfer of property rights to the peasantry. We argue that serfdom hampered the demand for education, whereas the successive emancipation of the peasantry triggered a demand thereof. Our estimation strategy rests on an instrumental-variable approach that establishes causality of the relationship by introducing soil characteristics to obtain exogenous variation in farm size. The instrument is based on an empirical regularity according to which there is a systematic negative correlation between soil quality and farm size. Further, we estimate panel models with county fixed effects to rule out unobserved heterogeneity.

Chapter 3 provides evidence that the initially better-educated regions within Prussia responded more successfully to the opportunities created by the technological changes brought from Britain.[2] The existing historical evidence refuted the idea that formal education was important for the Industrial Revolution (Mitch 1999). However, this evidence was mostly from British textile industries, which we argue might have evolved differently to other industries. We use a unique school enrollment and factory employment database and we link census information from the 334 Prussian counties from preindustrial 1816 to two industrial phases in 1849 and 1882. We use the education level observed before the onset of industrialization as an instrument for education levels during industrialization. We find that basic school education is significantly related to industrial employment in both phases of the Industrial Revolution. This database allows to distinguish between industrialization in three industries: metals, textiles, and all other industries (outside metals and textiles). It turns out that in the textile industry, where innovation was less disruptive and child labor was more prevalent, formal education apparently played a minor role during both phases of industrialization. However, formal education appears to have already

[1] A working paper version of this chapter is available as Cinnirella and Hornung (2011).
[2] This chapter was published as Becker, Hornung, and Woessmann (2011).

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played an important role in the metals and all other industries in the first phase of the Industrial Revolution, and its importance increased further during the second phase. In addition, we estimate panel models with county fixed effects in order to rule out unobserved heterogeneity and to confirm the cross-sectional results.

Chapter 4 provides evidence that firm-level productivity in textile manufacturing was higher in cities that had skilled immigration.[3] The previous finding that formal education only influenced the non-textile sector brought about the question of how technological progress in textiles took place. Eighteenth-century Prussia was underdeveloped in terms of technology when compared to the leaders of Belgium, Britain, and the Netherlands. During this time, technology could almost exclusively be transferred through the migration of skilled craftsmen who traveled as journeymen until the guild allowed them to become master craftsmen (Cipolla 1972). The transfer of skills remained restricted to a small circle of apprentices and laborers, as this required face-to-face contact, and advances in productivity remained limited. In this chapter, I study an early natural experiment. In 1685 religious persecution drove the Huguenots out of France into the economically backward electorate of Brandenburg-Prussia. This is a famous example of the immigration of an entire population that was, on average, more skilled than the native population. The Huguenots introduced to Prussia more advanced skills and also new technologies, mostly in the textile industries. I combine Huguenot immigration lists from 1700 with Prussian firm-level data on the value of inputs and outputs in 1802 to create a unique database. The results indicate a long-term effect of skilled-worker immigration on productivity in textile manufacturing. By the order of a centralized ruling from the king and his agents, Huguenots were channeled into Prussian towns to compensate for severe population losses during the Thirty Years' War (1618-48). I exploit this settlement pattern of the Huguenots in an instrumental-variable approach. Using population losses as an instrument for Huguenot settlement, I find causal effects on the productivity of textile manufactories remaining even a hundred years after their immigration. Furthermore, I show that the Huguenot immigration exclusively affected textile manufacturing but none of the other industries.

Chapter 5 turns to analyze the effect of technological diffusion on regional growth, after chapters 3 and 4 have established the relationship between human capital and technological diffusion.[4] I study the specific case of the effect of railroad diffusion on population growth at the city level in nineteenth-century Prussia. Urban population growth has proven to be an adequate proxy for economic growth in the absence of more direct measures (Acemoglu et al. 2002). Using geographic information software (GIS), I match triennial population data for roughly a thousand cities to geo-referenced maps of the Prussian railroad network. My identification strategy exploits the fact that until the 1860s, railroad lines were exclusively built between major cities. Cities that were located on a straight line between these major cities were thus, by chance, able to gain access to new railroad technology. Using both cross-sectional as well as panel estimations, I find that these cities grew substantially stronger than cities without access. Causality of this (potentially endogenous) relationship is established using an instrument that identifies exogenous variation in railroad adoption by constructing corridors around straight-line connections between terminal and junction stations. Analyzing different time periods, I consistently find positive short- and long-term effects of railroad

[3] A working paper version of this chapter is available as Hornung (2011).
[4] A working paper version of this chapter is available as Hornung (2012).

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access on urban population growth. Furthermore, I do not find evidence for the hypothesis that railroads appeared as a consequence of previous growth.

The four main chapters of this thesis show how human capital and technology shaped the economic development of Prussia during the eighteenth and nineteenth centuries. These developments had long-term consequences and might have become the foundation for Germany's economic environment.

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Quests for Community: The United States, Community Development, and the World, 1935–1965

Since World War II, the richer nations of the world have given aid to the poorer ones as a way of encouraging development. And yet, after seventy-odd years, how effective that aid has been remains an open question. That problem, which has long interested economists, has emerged as a major topic of interest for historians as well. And two decades or so of fruitful research have yielded a powerful way of understanding international development projects. As recent historians have

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encouraged us to see it, postwar development aid has been principally an exercise in "modernization"—a top-down attempt on the part of donor nations to replace traditional societies in the global South with modern ones, with "modernity" understood as a condition marked by industrial society, a robust state ruled by scientifically minded experts, the transformation of political problems into technical ones, and the achievement of abundance. Modernization projects, however, have been notably heavy-handed, achieving mixed economic results while legitimizing—and even encouraging—cultural disruption, ecological damage, and military violence in target societies. Not surprisingly, development experts in the past two decades have largely rejected modernization as a development agenda and have retreated to a supposedly more enlightened and community-based approach that focuses on small-scale interventions and defers to local preferences and conditions.

That community-based approach, however, is not as new as it seems. My dissertation argues that, from the very beginning of international development programs after World War II, many of the largest and most influential government aid programs emanating from the United States were grassroots, localist, and anti-technocratic in their stated orientation. The aid officials presiding over such projects, often experts on agriculture or rural society, entered the field of foreign relations with a set of preoccupations that differed from those of modernization theory. As a rule, they privileged small-scale works, local knowledge, democratic participation, and communal solidarity at the level of the village. In collaboration with Third World policymakers, they designed a political project—community development—that came to hold sway throughout the global South in the 1950s. Community development programs covered hundreds of million people in dozens of nations (the U.S. posted advisers to 47 countries by 1956) and regularly commanded heavy investments from the United States, host-country governments, the United Nations, and philanthropies such as the Ford Foundation and CARE.

This episode in the history of international development is not entirely a cheery one. Community development was always intriguingly Janus-faced, capable of embodying the highest hopes of some of the most admirable figures in modern history while at the same time contributing to vicious forms of oppression. My dissertation thus explains how strange bedfellows—the Peace Corps and the CIA, Mohandas Gandhi and Ferdinand Marcos, the agrarian radicals of New Deal and leaders of the Japanese internment camps, the proponents of the War on Poverty and those of the Vietnam War—were yoked together by the conviction that development should not be about scientists and engineers building dams but about villages and neighborhoods coming together to shape their own fates.

The story of community development begins in the New Deal, not with the Keynesian economists who sought to transform the economy by channeling investment into industry, but with the rural experts concerned with the devastation that such an investment would wreak on small farming communities. In the Tennessee Valley Authority and the USDA, these experts used their positions to launch experiments in participatory planning. Such efforts received a great deal of public approval. Although we often think of the United States as pursuing centralization with great enthusiasm during the Roosevelt years, there were, in fact, many who reacted to the Depression and to the growth of fascism abroad by concluding that humans had somehow lost control of their own creations and that society must be rebuilt, as urban theorist Lewis Mumford put it, "on a human scale." Small towns, villages, neighborhoods, and small social groups became constant points of reference from the mid-1930s through at least the 1950s—one need only think of Thornton Wilder's

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massively successful play, *Our Town* (1937), Norman Rockwell's sentimental paintings of small-town life, or the mainstream success that anthropologists such as Margaret Mead enjoyed for their studies of "primitive" cultures. My dissertation draws together this diffuse but still neglected episode in thought and culture and suggests it's important for policymaking.

The advocates of small-town life—both the theorists such as Lewis Mumford and the men working on rural problems for the New Deal—have received admiration from both intellectual historians and historians of agriculture. They are usually regarded, however, as prophets in the wilderness, celebrated perhaps for the clarity of their vision but not for their ability to effect change. The sense that such activists were ineffective, however, is merely a consequence of the fact that we have been looking for their influence in the wrong place. The small-town vision was indeed crowded out by wartime industrialization and state-building in the United States, but it held sway abroad. That is because after the United States ascended to its position of global hegemony after World War II, it began to invest heavily—and for the first time—in development aid to the global south. Having only a relatively small empire and no cadre of colonial officials or area-studies experts (those would come significantly later), the U.S. government fastened on rural experts from the New Deal programs to staff its agencies. Men who had been working on Southern agriculture joined with anthropologists who had been studying remote tribes to oversee development in the Third World. And as they joined the ranks of the civil service, they brought with them a fascination with rural cultures and traditions.

Small-town advocates from the United States were not the only ones enchanted by villages. Nationalist leaders in the decolonizing world also had reason to be skeptical about modernity's benefits, and they, too, often looked to the village as the locus of culture. It is no accident that the first community development program, designed by a U.S. aid worker (and a close colleague of Lewis Mumford's), was established in India, where it was strongly supported by and closely allied with the Gandhian movement. The Indian program grew so steadily that by the 1960s it covered, village by village, 10 percent of the world's population. Such growth was impressive on paper—and other countries quickly took note—but for Indians living in poverty it meant frustratingly little. Time and again, attempts to devolve power to the villages did little more than to bolster existing rural social hierarchies. Development projects designed by "the people" had a way of benefiting the village headman, the caste leader, and the landlord, and in the end India's massive community development experiment did conspicuously little to lift the new nation from poverty. The failure of such a large and, at the time, prominent application of Gandhian ideals in the realm of development is a tantalizingly rich story, and yet, for all that has been written about India's manifold development campaigns, there is no full historical account of it. Instead, almost all writing about development in postcolonial India stresses large-scale engineering projects and similar top-down schemes.

If the community development program in India was defined by the ideals of Mohandas Gandhi, the program in the Philippines, which was nearly as influential, was defined by those of the CIA. There, the landlord-dominated government faced a very threat of being toppled by a peasant rebellion in the early 1950s. Working with nationalist leaders, the United States imported community development from India to the Philippines, this time as a counterinsurgency campaign under the direction of the CIA legend Edward Lansdale, the Cold War hero who would later be instrumental in shaping the U.S. intervention in Vietnam. Lansdale's quick suppression of the rebellion encouraged Philippine leaders to invest heavily in community development

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for their very survival and forged a relationship between community development and counterinsurgency that remains to this day. The CIA carried community development from the Philippines to Latin America and to South Vietnam, where it dovetailed with the strategic hamlet program. At the same time, the dictator Ferdinand Marcos used the community development program to consolidate his power over the country. India had shown that community development could not do much to alleviate poverty, but the Philippines showed that it was a strikingly effective tool for political control of the villages of the Third World.

Despite community development's demonstrable failure to bring prosperity, it returned to the United States in the 1960s, like a boomerang. It was in the 1960s that policymakers there realized that postwar affluence had not reached all Americans, and that the tide that had lifted so many boats had caught others in its undertow. As liberals cast about for solutions to the increasingly evident crisis of the inner cities, they called community developers working abroad back from Third World villages to U.S. ghettos. My dissertation establishes the surprising fact that the men who emerged as the commanders of the War on Poverty were often veterans of overseas community development projects, many of them through the Peace Corps. Hoping that what they had tried in rural Asia and Latin America might be translated back to the "urban villages" of the United States, as they had come to know them, they placed at the center of the War on Poverty a controversial initiative called the Community Action Program, which they openly admitted was based on overseas community development. But inner-city neighborhoods, it turned out, were not the same as Third World villages. Whereas in the rural parts of the global South community development shunted power to the landlords and local potentates, strong urban segregation in the United States meant that there were few powerful local elites around to control the program. Instead, community action became the vehicle for a sudden rebellion, strongly linked to the increasingly militant civil rights movement. (The Black Panther Party, tellingly, was founded at a community action agency.) Politicians rushed to shut the program down and put in its place more politically palatable versions of "community-building." Fire, not ice, had destroyed this iteration of community development, but the result was largely the same.

The story of community development is, in the end, the story of how admirable ideals—ideals that are still very close to our hearts—went awry. It is the story of how the United States, acting on those ideals, sought to fight poverty at home and abroad. But in doing so it had to grapple with the fact that "community," just like "scientific expertise," is a treacherous receptacle for our uncritical faith. That is an important message today because, since the 1990s, the World Bank and other leading development agencies have turned heavily toward community-based development strategies. Whether in the eyes of the Nobel-winning economist Amartya Sen or the Nobel Peace Price winner Muhammad Yunus of the Grameen Bank, development is no longer seen as an attempt to make poor nations resemble rich ones. It is about poor people acting locally to take charge of their own destinies. But that ideal is not new. And it is only by grappling with its disappointing past that we can formulate a mature development policy in the present.

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Immigration and Cities in the Twentieth Century

For more than 150 years, the United States has grappled with the opportunities and problems associated with immigration. Nearly 100 million immigrants arrived between 1840 and 2000, with most newcomers since 1880 settling in urban areas. My dissertation explores the often interrelated phenomena of immigration and the evolution of cities in twentieth-century America. The first two chapters address the Age of Mass Migration (1850–1914) and the millions of European immigrants who came to the United States during this period. This historic movement of people altered both the American electorate and the policy preferences of the native population. I begin by investigating how these immigrants became integrated into the American political system. I then turn my attention to the native political response to this immigration and assess the impact of the Americanization Movement, a series of state laws intended to force immigrant children to learn English and assimilate. I close by considering the role of immigration along with other demographic forces in shaping the population distribution across cities and suburbs in the second half of the twentieth century.

The first chapter of my dissertation asks how early twentieth-century immigrants, largely from undemocratic European states, first became politically mobilized in the United States.[1] This period serves as a particularly useful laboratory for studying immigrant political mobilization. The United States maintained a nearly open border to European immigrants until 1921, freeing the analysis from interference from legal obstacles to becoming an American citizen that exist today. Cities in the early twentieth century invested substantial resources in infrastructure improvements and public health, and immigrant groups could compete for consideration in the allocation of funds if they became citizens, registered to vote, and translated their numbers into credible voting blocs.

I investigate how immigrants responded to the incentive to become politically mobilized by focusing on the choice of foreign-born individuals living in city wards to become naturalized American citizens. This empirical approach relies on the observation that citizenship was both optional and obtainable for European immigrants until 1921.[2] Furthermore, becoming a citizen was necessary only to obtain the right to vote, not the ability to work or receive public benefits. The census recorded the naturalization status of all foreign-born individuals in 1900 and 1910, so using citizenship choice as an indicator of political engagement allows me to use rich microdata in the analysis, thereby avoiding aggregate regressions of voter turnout or microdata with very limited geographic identifiers such as the National Election Survey.

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[1] A revised version of this chapter is Shertzer (2013).

[2] Unlike in the present day, there was no notion of illegal immigrants who were barred from political participation because they could not become citizens. Asian immigrants were allowed to work in the United States but not to obtain citizenship, and consequently I focus on Europeans in my study.

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I focus on the political mobilization of cohesive ethnic groups within city wards, constructing a panel data set of 106 wards in four cities (Boston, Chicago, the Manhattan borough of New York City, and Philadelphia) that were comparable over 1900 and 1910. Each of these wards was represented by a locally elected alderman who served on city council and was thus able to provide favors and consideration from the city government. In order to accurately measure the size of ethnic groups at the ward level, I collected 100 percent samples of the population of each city from a genealogy website. There is substantial variation in the distribution of immigrants across wards with groups of Czechs, Greeks, Italians, Poles, and Russians each comprising as much as a third of the ward electorate.

I show that immigrants were the most likely to initiate citizenship proceedings when their group comprised about 20 percent of the ward electorate, a relative size that was likely to be decisive in local elections. Graphically, the relationship between political mobilization and relative size is an inverted U-shape with the peak located around one-fifth electorate share. The presence of established social networks also mattered for mobilization; there is little evidence of coalition-driven group behavior for immigrant enclaves composed almost entirely of very recent arrivals. I also show that the main nonlinearity in relative group size is driven by the political mobilization of immigrants in wards where the Democratic Party likely needed their vote to win elections.

Americans with nativist sentiments were alarmed by the presence of the new immigrant masses. Legislators perceived unassimilated newcomers to be a threat: their Catholic and Jewish faiths, low literacy rates, inability to speak English, and general unfamiliarity with American customs and democratic government all appeared a danger to society. The second chapter of my dissertation (joint with Adriana Lleras-Muney) addresses the impact of the Americanization Movement, the diverse attempts of states to force immigrant children to learn English, attend school, and assimilate into American society.[3]

In contrast to the largely literate non-South native white population, in 1910 about 23 percent of the foreign born over 10 years of age were illiterate (Edwards 1923). Poorly educated immigrants were seen as vulnerable to exploitation by political parties since they "lacked any conception of democracy and willingly would submit to the authoritarian yoke that had bound them in Europe" (Ross 1994, p. 12). Furthermore, nativists could not rely on the existence of public schools to Americanize immigrant children. Poor parents often placed their children in the labor force or enrolled them in parochial schools where classes were taught in their native language.

We collected data on the passage of English-only laws between 1910 and 1930 and make use of previously collected laws on compulsory schooling and child labor during the same period. Previous work has examined the effects of compulsory schooling and child labor laws on the educational attainment of natives and found that this legislation had positive but modest effects on their education (Goldin and Katz 2011; Lleras-Muney 2002). However, no existing work documents whether these laws affected immigrants as legislators intended. We ask if the set of education laws affected immigrants and natives differentially and whether English laws affected immigrants at all.

Using the 1910, 1920, and 1930 censuses we assess how these laws affected the enrollment, literacy, employment, and English fluency of children. We find that that English-only laws did in fact increase the literacy of immigrants during this period, particularly for recently arrived children from non-English speaking countries and

[3] A revised version of this chapter is Lleras-Muney and Shertzer (2012).

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those of illiterate parents, but these laws did not by themselves or in combination with compulsory schooling laws affect the school enrollment or employment of immigrants or natives. Their effects were also modest in magnitude. We also find that compulsory schooling laws and continuation school laws had a large impact on the enrollment of immigrants. The effects we estimate are about twice the size of the effect these laws had on natives.

Although cities were growing throughout the first half of the twentieth century, central urban areas lost population to the suburbs after 1950. The third chapter of my dissertation (joint with Leah Boustan) asks how the changing demographics of the American population may have buttressed city populations against further decline.[4] The share of the metropolitan population in the United States living in a central city fell from 58 percent in 1950 to 36 percent in 2000, with the balance residing in the suburban ring. Suburbanization intensified residential segregation by race and income, hastened the contraction of the urban tax base, and augmented disparities in access to education and other locally provided public services (Baumol 1967; Benabou 1996; Fischer 2004).

Alongside these forces favoring suburbanization, a series of countervailing changes in the demographic composition of the metropolitan population bolstered the size of central cities. This paper identifies four such shifts: the growing share of the metropolitan population living in a household with a foreign-born or African American household head; the declining share in households headed by a veteran of the Armed Forces; and the declining share of households containing a child under the age of 18. We also consider the life-cycle mobility of the large Baby Boom cohort from city to suburb (and back again) but find that it did not have a quantitatively meaningful effect on residential patterns.

Central to our argument is the claim that demographic characteristics help to determine residential location. However, the causal relationship could go in the other direct as well, with residential location influencing mutable characteristics like family size and veteran status. We therefore employ instrumental variables to identify the causal effect of having an additional child or serving in the military on place of residence. In particular, we instrument for household size with the occurrence of twins on either the first or the second birth (Angrist and Evans 1998). We identify the effect of military service by comparing cohorts who came of age during and just after the mass mobilization for World War II (Bound and Turner 2002; Fetter 2012; and Page 2008).

In the final section, we use the estimated determinants of living in the central city to consider a series of demographic counterfactuals. Overall, we find that, absent these changes in demographic composition, the share of the metropolitan population living in the central city would have declined by an additional 10 to 32 percent from 1960 to 2000. However, these demographic changes were only strong enough to partially reverse but not to overcome the strong economic and social forces in favor of suburbanization.

[4] A revised version of this chapter is Boustan and Shertzer (2011).

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Essays on the Great Depression from a Micro Perspective

The Great Depression stands as one of the greatest puzzles for economists. Even after these many years, there still remains three major distinct questions. First, what was the initial cause of the Great Depression? Second, what made it so bad? Was it the type of initial shock that started the whole collapse, or were there other amplifying mechanisms to the "first mover?" Finally, what made it go on for so long? The goal of this dissertation is not to break new theoretical ground on the Depression, but instead to bring new data to bear on these perennial debates and uncover new facts worth exploring.

The problem for all of these questions like so many in economics generally stems from limits on the data available. There is only so much differentiation between competing theories that can be achieved using aggregate time series on output, inflation, and interest rates. This is especially the case in the Depression when everything seems to happen at the same time. Rolling bank failures were coincident with the collapse in the broader economy, but that was also coincident with declines in money supply. The recovery began to occur when banks stopped failing, but at the same time, there was strong productivity growth and policy experimentation. Being able to get any estimate of the causal effects of these various potential driving variables represents a major challenge. This dissertation takes a distinctly micro approach instead building up to the aggregate level from micro-level data. In particular, I use plant-level data collected from the Census of Manufactures taken in 1929, 1931, 1933, and 1935.

So what is the value of micro-level data for macroeconomics? The usefulness of disaggregated data goes beyond simply providing empirical support for theoretical assumptions. This type of data also generates new puzzles for macroeconomists. For example, the modern flow approach to labor markets was founded on the empirical work of S. Davis and J. Haltiwanger (1990), who used the Census of Manufactures to study job creation and destruction rates. This view of the labor market based on micro-level evidence has required economists to reconsider parts of the neoclassical view of labor demand. In addition, almost any model that has micro-foundations makes predictions about a broader range of variables than simply first moments. To take another example, menu cost models of price stickiness make sharp predictions on not just the inflation rate but also the cross-sectional variance of prices over the business cycle. Yet these latter implications have only recently been tested with data on individual prices (Berger and Vavra 2011).

Chapter 2 takes a step in the direction of showing how micro-level differences matter at the macro-level. The dynamics of aggregate productivity track changes in output remarkably closely and in an accounting sense, explain the vast majority of changes in output. This large role for productivity is not simply a phenomenon of the Great Depression but depressions at large (Kehoe and Prescott 2002). The decline is also much larger than one would expect given a demand-driven view of productivity

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(Ohanian 2001) and clearly inconsistent with the direct evidence on rapid technological advances (Field 2011). At the same time, there is striking evidence of differences during the Depression across industries in terms of productivity growth (Field 2011) and employment growth (Bernstein 1987). Furthermore, regions of the country perform quite differently during the Depression (Rosenbloom and Sundstrom 1999). The goal of this chapter is to show how these changes in second moments across different units of production affect aggregate measures of productivity.

The chapter attempts to account for part of the decline through an increase in resource misallocation at the plant-level. This idea of misallocation has been used in the development accounting literature (Hsieh and Klenow 2009). It seems quite natural to apply similar ideas to a business-cycle setting where a 20 percent decline in productivity is experienced. The basic idea of the framework is that while physical productivity may be different across plants due to product differentiation, productivity adjusted for output price should be constant. The chapter studies two particular industries that are particularly convenient for the accounting framework, the cement and manufactured ice industry. Both of these industries have essentially homogeneous products making it easy to compare productivity across plants. I find that in both industries, misallocation plays an important role in understanding productivity dynamics over the Depression.

The chapter goes on to argue that the increase in misallocation is driven by the breakdown in financial intermediation. The puzzle for supporters of the nonmonetary view of the banking crisis is that the behavior in investment during the Depression is not that puzzling given the decline in productivity (Chari, Kehoe, and McGrattan 2002). Furthermore, the vast majority of models of financial crises offer effects solely on investment and nothing more (Chari, Kehoe, and McGrattan 2002). Instead the establishment-level evidence presented in this chapter implicates the banking crisis in the decline of a broader set of macro variables. In the end, the point of the chapter is that one cannot condition on the change in productivity when using changes in investment as an indicator for the effects of a banking crisis. The banking crisis during the Depression drove changes in investment indirectly through changes in aggregate productivity.

The next two chapters show the additional roles micro-level empirical work can offer for macroeconomics. Chapters 3 highlights the fact that aggregate data are not the only way to formulate or test macro models, and in many cases, that source may not even be the best way. Disaggregated data allows for the possibility of exploiting unique sources of exogenous variation to identify relationships crucial at the macro-level. One such example is the link between financial distress and outcomes for other parts of the economy. This chapter exploits a natural experiment in the state of Mississippi discussed by G. Richardson and W. Troost (2009). While the nonmonetary view of the banking crisis stretching back to Bernanke (1983) has been much emphasized, the empirical support for it is not overwhelming. Cole and Ohanian (2000) point to the fact that in the cross-section there is no correlation between bank failure rates and income growth at the state level. The issue in identifying the link is one of endogeneity. Do bank failures drive bad economic conditions, or is it the revers? The paper by C. Calomiris and J. R. Mason (2003) attempts to address this issue through the use of instrumental variables finding a large negative effect of loan supply contractions. I too find very large negative effects of bank failures on a whole variety of outcomes from output to employment. What is most striking about my results is the lack of persistence in the effects, which dissipate two years after

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the shock occurs. This casts doubt on another popular view of the slow recovery of the Depression, which is that it was the hangover from the banking crisis (Reinhart and Rogoff 2009).

Chapter 4 offers evidence on micro-level effects of a policy taken as crucial for macro theories of the recovery from the Depression. Theorizing by both G. Eggertsson (2012) and Cole and Ohanian (2004) has taken it as given that the Codes of Fair Conduct drawn up under the National Industrial Recovery Act actually fostered collusion. The debate in those papers was not over the effects of the code at the industry-level but what that collusion meant in the aggregate. Like the link between banks and plants, the natural scale to look for collusion is at the plant-level. Strikingly, there is no evidence that I am aware of at the micro-level on the effects of these codes. So chapter 4 attempts to fill in this gap by providing the first plant-level study on the micro-level effect of these codes. In this chapter coauthored with Chris Vickers, we study an industry, macaroni manufactures, that was previously held-up as a paragon of why the codes failed (Alexander 1997). In contrast, we do find evidence that collusion was fostered by the NRA.[1] These results provide a crucial input into the models studying the aggregate effects of this policy though they do not rule out any explanation at the macro-level.

Recently, there has been a flourishing of micro-level studies to understand questions regarding the business cycle. A. Mian and A. Sufi (2012); and A. Mian, K. Rao, and A. Sufi (2012) have found at a county-level a tight link between increases in housing leverage before the Great Recession and declines in consumption and employment during the Recession. Numerous authors have extracted intriguing facts regarding differences across plants over the business cycle, e.g. M. Kehrig (2011). It is time for economic historians to address the macro questions of Depression with renewed interested starting from a micro-level perspective. This dissertation has attempted to take one step down that road.

[1] In a separate paper not included in this dissertation coauthored with Mark Chicu and Chris Vickers (Chicu, Vickers, and Ziebarth 2012), we examine another industry: the cement industry. We find strong evidence for the collusive effects of these codes in this industry as well.

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Comments on Dippel, Hanlon, and Hornung

Rituals often bring together the widely dispersed members of a tribe; they define social groups. Of the many rituals that create the economic history community, there is probably none more exhilarating than the selection of best dissertation winners at the annual Economic History Association meetings. That is why it is a great honor and privilege to present the final candidates for the Gerschenkron Prize in

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international economic history. The set of submissions received was extremely strong. There were twelve in total—eight for Europe, and four for the rest of the world. Of these, almost half were concerned with macroeconomics and issues of long-run growth. The remainder was evenly split amongst banking, the history of technology, and labor. More than half of the dissertations involved some degree of archival data gathering, while five used published data. A third of submissions had a "theoretical angle"; two-thirds used econometrics to a significant extent.

Making a choice of finalists is never easy, and I struggled to draw up this year's list. The finalists all blend innovative data work, a theory-inspired choice of research topic, and rigorous statistical analysis. Every one of the finalists' dissertation was humbling to read—more than once, I found myself wishing I had thought of such a clever idea myself. Next, I will discuss dissertations in alphabetical order.

CHRISTIAN DIPPEL

Christian Dippel's dissertation spans a broad range of topics. It contains chapters on Indian reservations and their economic performance today, on Caribbean island institutions, and on international trade. Of these chapters, arguably the most innovative is his job market paper on the causes of poverty on Indian reservations.

The first thing I learned from this chapter is the amazing range of per capita output. Some Native Americans living in the United States are indeed very poor; others remarkably rich. Equally surprising is the fact that the divergence in living standards is probably a recent phenomenon. What accounts for these differences? Dippel argues that it is the forced integration of tribal bands on the same reservation that causes politics to turn poisonous. Put another way, institutions don't work where tribal groups have no history of sharing an "executive."

Dippel presents an instrument that is both plausible and powerful—the presence of mining operations in the area. Where the white men had strong economic reasons to herd the natives onto the smallest bit of land available, forced integration was more likely. In terms of timing, Dippel finds that governance only starts to matter once the federal government gets out of the business of running the reservations directly. It is only after "devolution" in the 1980s that the big income differences between the different areas start to open up. There is also no reason to think that casinos are responsible for the growing divergence in the cross-section.

There is much to admire in this research exercise. First, it establishes new facts that are important, such as the enormous variation in per capita incomes on reservations, and the timing of the gap. Second, the paper is serious about establishing causality in a way that few papers are. Dippel reconstructs the entire causal chain—from the history of shared governance in the pre-reservation era and reservation formation to political conflict today, to the loss of entrepreneurial talent that is a key driver of the poverty of forcibly integrated Indian reservations.

The real importance of the paper lies in showing how much culture and the long arm of history can matter—even if institutions are identical. The legal framework that governs how reservations are run is the same everywhere in the United States, with minor exceptions. And yet, as Dippel demonstrates, the variation in per capita incomes between reservations is several times greater than that between U.S. states (and closer, but not quite on par, with international productivity differences).

If I had to come up with criticisms, I would urge the author to think more about exactly *why* a history of shared governance is apparently so useful economically today—over a hundred years after reservations were formed. We already know that

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culture can last for a very long time (Guiso, Sapienza, and Zingales 2008; Nunn and Wantchekon 2009; Voigtländer and Voth 2012). At the same time, I wonder if there are perhaps examples of tribes that were forcibly integrated where things have not "gone to hell." If so, these counterexamples could shed light on the underlying mechanisms. Is it that ancient hatreds simply refuse to die? Is it that large cultural and ethnic differences make it harder to communicate and resolve differences? Is politics more "tribal" for rent-seeking reasons? Is there less punishment of individuals that fail to co-operate for the greater good—a prime determinant of how much sharing and investment in public goods there is, if recent research in evolutionary anthropology and experimental economics is to be believed (Henrich et al. 2001; Fehr and Gächter 1999; Gächter, Renner, and Sefton, 2008)?

In another chapter, Dippel examines the decline of representative institutions in the Caribbean. He argues that it was mainly driven by the elite's concern about the assemblies being "highjacked" eventually by smallholders if property requirements were ever relaxed. They therefore lobbied for the islands being turned into directly ruled colonies, without formal representation of locals. This is an important case study. For most of the grand sweep of history, institutions and representation have improved in tandem. Just like output, literacy, and trade, the long-run trajectory is strongly upwards. What is good news for the welfare of mankind is hard luck for economists trying to analyze causes and effects—hence the fascination with Europe's interwar period, when things did go into reverse. To this case, Dippel adds an important and neglected case that makes fascinating reading. It strongly provides important detail for why ethnically fragmented and highly unequal societies work so poorly. Crucially, he shows that an important element of the institutional setup can actually be endogenous to the level of economic and racial cleavages in a society—an aspect that qualifies some of the grand narrative in the institutionalist literature (Acemoglu and Robinson 2012).

A similar counterpoint to "Whiggish" interpretations comes in Dippel's chapter on trade and labor market institutions. Most of the research on the effects of trade has analyzed settings when labor was free. For example, Jeff Williamson and Tim Hatton showed how migration across the Atlantic did much to improve wages in Europe. Dippel analyzes the eighteenth-century trade boom that turned the Caribbean islands into some of the richest places on earth. Here, labor was not free. Higher export prices did not translate into higher compensation. Instead, by swelling the coffers of the landowning elite, it spelled more coercion. The fewer the number of plantations, the greater the squeeze of wages. Dippel's work on the Caribbean in the eighteenth century, on the decline of representative assemblies and the growing exploitation of labor as trade boomed provides a healthy antidote to "Washington consensus" thinking that sees more trade as an automatic solution to many ills.

W. WALKER HANLON

Technological change used to be considered as exogenous—something that falls from the sky like summer rain. Recent work on directed technological change has changed this. Following on from the hallmark research by Daron Acemoglu, there is now a whole wave of empirical and theoretical work that argues that incentives shape the direction that technological development takes (Acemoglu 1998; Atkinson and Stiglitz 1969; Levy and Murnane 2003). Amongst economic historians, Robert C. Allen has powerfully argued that Britain led the Industrial Revolution because high wages created an incentive to economize on labor, leading to more energy- and

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capital-using (but labor-saving) inventions (Allen 2009). The prediction is that the factor of production that is relatively abundant is used more intensively as a result of complementary technological innovation: The canonical example is the use of college graduates in many sectors of production. While the supply of college graduates has surged, their wages have grown even more (Goldin and Katz 2009).

Testing the directed technological change hypothesis is not trivial. Ideally, to avoid the spaghetti-like tangle of causality issues with time-series evidence, we want cross-sectional evidence, combined with a clearly identifiable, exogenous price shocks to one factor of production. Ingeniously, W. Walker Hanlon identifies such a shock, drawing on a particularly striking historical experiment—the "cotton famine" in England during the American Civil War. While the Union and the Confederacy fought, little cotton reached the Old World. Hanlon uses this large, exogenous shock to the availability of one factor of production to examine how technological change reacted. He collects detailed data on patenting frequency, by sector, as well as on the cost of shipping cotton from the U.S. South to the United Kingdom (from newspapers). Here, Hanlon follows in the footsteps of a recent wave of publications using detailed information on patenting (Griliches 1998; Meisenzahl and Mokyr 2011; Moser 2005).

Hanlon finds big effects. As American cotton disappeared, the need to use cotton that had been largely ignored previously—from Egypt, India, and Brazil—increased. Hanlon finds a quickening in the pace of innovation in the cotton sector. Compared to innovation in other textiles—woolen goods, say—cotton saw a rapid spike in patenting in the United Kingdom during the U.S. civil war. Not only this, but exactly the type of innovation aimed at making the less useful cotton types—with different staples, and more adulteration—easier to process became much more common after 1860. Put another way, Hanlon demonstrates that patenting now focused on making the—relatively more abundant—Egyptian, Indian, and Brazilian cotton more useful. Not only that, but to take the exercise one step further, he estimates elasticities of substitution between different types of cotton—and then shows that changes in patenting rates stack up in the way predicted by the theory. The chapter also contains another nice innovation—a theoretical extension of the Acemoglu model that endogenizes input supply.

The main chapter of Hanlon's dissertation "has it all"—it contains innovative theoretical work; it thinks clearly about data and handles it cleanly. The question is big, and the results are impressive—one of the first direct confirmations of the directed technological change hypothesis. If one had to name a possible fly in the ointment, it is the fact that the response of inventors to the cotton famine seems almost too fast. The first shots of the Civil War had barely been fired when inventors must have already been busy thinking about how to use other sources of cotton. Given that not all contemporaries expected the conflict to a) last a long time b) lead to an almost complete stop of exports, this is at least in need of further explanation. Some additional discussion of assessments by contemporary newspapers, etc., would help to make an impressive research exercise even stronger.

Hanlon uses the same "natural experiment" when he examines path dependence of technological inventions. There is substantial interest in whether the future pace of technological change. Robert Gordon's recent working paper on "Is U.S. Economic Growth Over?" attracted massive media attention at the end of 2012. Going back to the early work by Chad Jones, it is well-known that long-run growth dynamics depend crucially on whether innovation becomes easier or harder ("fishing out") as the stock of knowledge accumulates (Gordon 2012; Popp 2002; Jones 1999).

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Hanlon's first contribution is to question the existing evidence. The most important problem is that we cannot normally tell why there is "momentum"—with innovation in sector i at time t predicting more innovation at t +1. It could be that there is temporal dependence because the accumulation of ideas makes new research easier—or because the shock that caused the initial rise persists. That is why it helps to have a well-identified shock, such as the one to particular types of cotton machinery after the outbreak of the Civil War.

In other words, Hanlon asks if innovation continued at a high rate in areas of major advances during 1861–1865. The answer is emphatically "no"—if anything, there is a *negative* effect of earlier innovation. As the shock that created additional research in cotton wore off, patenting declined—in both absolute terms and relative to patenting in other textiles. This is indeed powerful evidence, cleanly identified, that one-off shocks to innovation do not last due to hysteresis. Modern-day studies that claim the opposite probably suffer from confusing shocks that last with innovation that becomes easier.

ERIK HORNUNG

What is the effect of importing a culturally distinct minority—and one that has a particular set of skills? That is the question asked by Erik Hornung. He examines an important historical episode—the exodus of Huguenots after the revocation of the Edict of Nantes in 1685. The edict had guaranteed religious tolerance in France. When it was revoked by Louis XIV, Huguenots fled in large numbers. While the majority went to Holland and England, many found their way to Prussia. There, the Prussian rulers received them with open arms. The country had just suffered massive population losses because of the plague. In addition, the Huguenots were known for their skills in textile production. This ensured that they could add to the productive base of the country—a particularly desirable aspect of the new Prussian subjects in an age of mercantilism.

Hornung finds much higher labor productivity in textiles by 1800 in those areas of Prussia where the Huguenots had settled a century. Importantly, he argues that the allocation of Huguenots to cities in Prussia was largely exogenous. Where the late seventeenth-century outbreaks of the plague were worst, the likelihood of receiving an infusion of Huguenots was greatest. This allows for a highly plausible instrumental-variable strategy. When using the instrumentation strategy, the results actually become stronger.

These results matter not least because of the problems faced by modern-day economists in identifying the effects of migratory flows. While migrations of highly skilled (and discriminated) minorities are not rare, they are much harder to analyze today. Why? Because migrants typically go where their prospects are brightest. Their specific knowledge will also spread quickly, and in ways that are hard to track. Here, Hornung uses the primitive communication structure of the past to great effect—local knowledge spillovers are plausible if mobility is low and there is little long-distance communication. Combined with the allocation of migrants by government fiat, there is a good chance of cleanly identifying the effects of migrant inflows.

Three issues deserve further consideration. First, why focus on output per capita? Should total output of the textile sector not matter at least as much? Surely, we do not want to count a former Huguenot settlement as a success if it has little textile production, but even fewer workers than average. Second, the total share of variation explained by a history of Huguenot settlement is small—on the order of 2 percent.

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This begs the question what else is driving differences in the cross-section—and if these factors are not ultimately more important determinants of textile production performance. Finally, there is an issue with the instrument. As the maps clearly demonstrate, the overwhelming share of towns settled by Huguenot migrants was in the West of Prussia. While the plague that determined the settlement pattern was probably exogenous, the areas in which the Huguenots came to settle were, on average, much more commercialized, educated, and connected with markedly better transport networks. To hope that the instrument will solve all of these potentially confounding issues is probably too optimistic.

In another chapter, Hornung analyzes the effect of education on industrialization (Becker, Hornung, and Woessmann 2011). Cross-country studies typically find that there is no effect—some countries had near-universal literacy very early (Sweden, Prussia), but did not industrialize. Others—like England—had relatively limited educational attainment on average, and yet got to industrialize first (Mitch 1999; Sandberg 1979). Instead, Hornung (together with coauthors Becker and Woessmann) argues that education was important for industrialization in nineteenth-century Prussia. Using the detailed census information used to good effect in other papers, they show that areas with higher school enrollment rates also had markedly higher rates of employment in the manufacturing sector. This was especially true outside the textile and metal-working sectors.

Unfortunately, the same issue vexes the analysis of the data that was already present in the Huguenot paper. In a country famous for its high school enrollment rates early on, there is an obvious question why there should be any variation in attendance. The historical case studies make this clear—some territories (like Liebenwerda and Hoyerswerda), acquired by Prussia during its long history of successful territorial expansion, were not effectively integrated into the way the state was run. A large arc of ignorance also stretches from Silesia in the South-East to the borders of East Prussia, in the North—heavily Catholic areas recently acquired from what had formerly been Poland before the final partition. This means that variation in educational outcomes are heavily correlated with state capacity—also well-known to have a first-order effect on output (Besley and Persson 2009).

Throughout, the paper displays an imaginative use of instrumentation strategies (not all of them specific to this paper, such as the distance to Wittenberg, which influenced the share of Protestants in the population) (Becker and Woessmann 2009). At the same time, there could be more geographical analysis, especially of spatial variation over short distances, to take care of the issue that so much of the variation exploited by Hornung reflects an East-West gradient of output per capita, transport infrastructure, educational attainment, and the share of the population that is Catholic.

Finally, Hornung analyzes the effects of railroad expansion on city growth. Specifically, he examines how much faster cities grow that are connected by railroad than those that are not, using Prussian data from the second half of the nineteenth century. Of course, research on the effects of railroads is almost as old as the New Economic history. It has recently been revived by the innovative work of David Donaldson (Donaldson 2010; Fogel 1964). Hornung's work is in the spirit of Donaldson. In particular, he exploits finely grained geographical data on where the lines passed—and he thinks about causality in a serious way.

The instrument that Hornung proposes uses the straight line between major cities. Railways in the nineteenth century were mostly built "as the crow flies." Hence, whether a line would pass through a minor town or city or not was predetermined by

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geography. In earlier times, in contrast, features such as river crossings, Roman roads, etc. determined where roads ran. Thus, Hornung can present a plausible measure of how big the "railroad windfall" for cities really was. He finds big effects, with city growth accelerating by 1–2 percentage points per year. This is especially true of the period 1831–1837. The acceleration occurred close in time to the start of construction, with a sharp change being visible already somewhat before the opening of the line—which presumably reflects the effects of construction. While these results do not fundamentally question earlier work on the impact of railways on the aggregate economy, they add a useful and important aspect to how they shaped regional patterns of economic growth. In particular, they convincingly demonstrate that small and medium-size cities profited significantly if they were touched by the railway network.

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Comments on Immerwahr, Shertzer, and Ziebarth

It has been a great pleasure to read and comment on the dissertations of the three finalists for the 2012 Allan Nevins Prize in United States or Canadian Economic History. The finalists were chosen from a pool of eleven submissions. As always, it was difficult to figure out how to reduce the list to just three—there were one or two others that could easily have been included. There is no question, however, that the three finalists—Daniel Immerwahr, Allison Shertzer, and Nicolas Ziebarth—wrote outstanding dissertations. They all met the normal criteria we expect of contenders for the Nevins Prize. They did prodigious work in primary sources and analyzed those sources using state-of-the art methods for their part of the discipline. But there is something else that sets these dissertations apart from the pack: they all have that something extra that I will call spark. In the remarks that follow, I offer some suggestions about places where the finalists could have pushed harder or approached their topic a bit differently, but I want to focus mainly on the special qualities of originality and creativity that I think give these dissertations their spark.

DANIEL IMMERWAHR

Scholars today hotly debate the most effective way to promote economic development. Should governments undertake massive, national-level interventions? Or is it better to support small-scale projects at the local level? Reading the current

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contributions on one side or the other (I am thinking, for example, of Jeffrey Sachs's *The End of Poverty*, William Easterly's *The Elusive Quest for Growth*, Abhijit Banderjee and Esther Duflo's *Poor Economics*, and Mark Rosenzweig's review of the latter book in the *Journal of Economic Literature*), it is easy to get the impression that this discussion has taken shape only very recently. In fact, however, scholars have gone over this ground (or at least much of it) before, as Daniel Immerwahr shows in his dissertation, "Quests for Community: The United States, Community Development, and the World, 1935–1965." Immerwahr's review of the earlier debate between proponents of large, capital-intensive infrastructure investments and advocates of local programs that work from the bottom-up is a contribution in and of itself. But Immerwahr does a lot more in his dissertation than just summarize the controversy. His dissertation recovers the largely forgotten history of the scholars and policymakers who pushed the bottom-up alternative and analyzes their successes and failures in settings as different as India, the Philippines, and the United States.

Three aspects of Immerwahr's study strike me as particularly interesting. First, Immerwahr shows that many of the analytical tools we use today to study networks came out of this movement; proponents of small-scale projects believed that the effects of their interventions would be propagated through networks, so they developed methods to enable them to discover the types of connections most likely to be important. Second, Immerwahr traces the careers of the officials involved in these development projects and finds, surprisingly, that many of them had backgrounds in New Deal projects like the Tennessee Valley Association (TVA). Although one might expect TVA to have spawned devotees of large-scale interventions, the project had an important community-building side that incubated groups interested in bottom-up development. Immerwahr then goes on to show that after the proponents of small-scale development worked out their ideas on the ground in India, the Philippines, and other settings, they moved back into the U.S. domestic arena to push for local empowerment under the War on Poverty. As is well known, this aspect of the War on Poverty provoked a backlash from the politicians whose power local organizations challenged, which brings me to the third contribution of Immerwahr's dissertation: a convincing analysis of why the earlier experiments with small-scale economic development projects failed. As Immerwahr points out, the communities in which these interventions occurred were, for the most part, hierarchically structured. To the extent that the projects were successful in improving the lives of the poor, they posed challenges to those at the top of the hierarchy and hence provoked resistance. It would be good for the proponents of small-scale development today to ponder this history of failure.

Immerwahr was trained as intellectual historian, and I wish he had more of the social scientist in him. Throughout his dissertation he makes strong claims about the role that a relatively small number of people played in the formulation of policy as they moved from the domestic to the international arena and back again, but the evidence for these claims is largely impressionistic. I would have liked him to see him do something more systematic—to apply the tools of network analysis whose origins he uncovers to his case studies so that one could assess quantitatively the importance of the personal and intellectual channels he emphasizes. Nonetheless, it is Immerwahr's skills as an intellectual historian that make this dissertation so valuable. His work reminds us how important it is to track the history of our thinking about the economy, as well as the history of the economy itself.

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ALLISON SHERTZER

Allison Shertzer's dissertation consists of three papers related to the topic of "Immigration and Cities in the Twentieth Century." The most important of the three, "'Satisfied with the Crumbs' No Longer: The Role of Group Size in Political Mobilization," explores the question of how the millions of immigrants who came to the United States in the late nineteenth and early twentieth centuries came to participate in the political process. Shertzer reminds us that few of the newcomers had any experience with democratic institutions in their home countries. What motivated them to take the steps necessary to be able to vote? Shertzer hypothesizes that immigrants mobilized politically when the members of their group were numerous enough to have an influence public policy and thus secure public goods and access to jobs. Her most creative insight was to realize that she could use data on immigrants' naturalization status, available in the manuscript population censuses, as a proxy for political mobilization. In the early twentieth century, unlike today, there was no reason to become a citizen except to be able to vote. Taking out citizenship papers entailed costs, so immigrants did not usually bother to do it, she hypothesizes, until their group was large enough to make political mobilization worthwhile. Shertzer tests this hypothesis at the ward level for half a dozen cities, accomplishing the daunting task of assigning each resident in each ward of these cities to an ethnic group and then analyzing changes in their naturalization status over time. The results confirm her hypothesis. Shertzer found that naturalization rates increased with the size of the ethnic group and then flattened out after the group got large.

My main criticism of this very interesting exercise is that Shertzer takes the definition of an ethnic group to be exogenous. She defines the groups mainly in terms of twentieth-century national categories—for example, Germans, Russians, Hungarians, Poles, Italians, Czechs, and Slovaks—except for a few more aggregated categories such as Scandinavians. However, it is questionable that immigrants came to the United States with such clearly formed identities. Rather than thinking of themselves as members of national group, it is likely that they defined their identity more narrowly or in a different way—by region, by dialect, or by religious beliefs. I would very much like to see Shertzer develop a model that would allow her to explore the interaction between political mobilization and the process by which immigrants developed group identities in their new country. If groups formed endogenously, one might expect their boundaries to vary across cities, depending on the particular stream of immigrants that moved to each locality and the alliances they formed among themselves to increase their political clout.

Before moving on, I would like to call the reader's attention to Shertzer's second paper, "Did the Americanization Movement Succeed? An Evaluation of the Effect of English-Only and Compulsory Schooling Laws on the Education of Immigrants," coauthored with Adriana Lleres-Muney. Previous studies of the effects of compulsory schooling on children have concluded that, even when the laws were seriously enforced, they had only modest impacts on enrollment (See, for example, Goldin and Katz 2011; Lleras-Muney 2002). The problem with this literature, however, is that it is unable to isolate the effect of the laws on the targeted population: immigrants. Shertzer and Lleres-Muney found a way to measure the effect of these laws on immigrants, and also the effect of laws mandating instruction in English (which they collected for the first time for this paper). They conclude that both sets of statutes worked as intended. In particular, they find that compulsory education laws had an

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impact on the schooling of immigrant children that was about twice as large as the effect that other scholars have measured for the population as a whole. This result is a valuable addition to the literature.

NICOLAS ZIEBARTH

The microeconomic perspective that Nicolas Ziebarth brings to the study of the Great Depression is what gives his dissertation its spark. Ziebarth bases the three papers that constitute his "Essays on the Great Depression from a Micro Perspective" on new data sets that he collected from the manuscript returns of the U.S. Census of Manufactures from 1929 to 1935. Some years ago, Daniel Raff discovered the manuscripts in the National Archives, and he used them to write a couple of illuminating articles about the dynamics of heterogeneous industries and the response to the Depression (Bresnahan and Raff 1991; and Berlin, Bresnahan, and Raff 1996). However, this important data source had not been exploited since the 1990s, when Raff's articles were published.

Ziebarth uses the census data in a number of different ways—some simple, some flashy. His first chapter, "Misallocation and Productivity During the Great Depression," falls into the latter category, even though the industries it studies—cement and paper—are anything but. The chapter shows off Ziebarth's modeling skills and estimation techniques, but in the end the connection it makes between the Great Depression and misallocation of resources in these two industries depends on what are really just correlations with trends in the banking sector. In my opinion, the simpler methods that Ziebarth uses in his second and third chapters are more illuminating. The second chapter, "The Real Effects of Bank Failures: Evidence from Mississippi During the Great Depression," picks up where Gary Richardson and William Troost left off in their important article on Federal Reserve policy during the early years of the Depression (Richardson and Troost 2009) Part of Mississippi is in the territory of Atlanta's Federal Reserve Bank and part in St. Louis's. Richardson and Troost used this "natural experiment" to assess the effects on the banking sector of the different monetary policies that the two Federal Reserve banks followed in response to the first banking crisis. At the end of their article, they explore the real effects of the difference in policy using data on retail sales, but Ziebarth is able to push their exercise further and show that there were significant effects on the manufacturing sector as well. The chapter does more than this, however. It also tracks the ways in which manufacturers adjusted to the lower level of demand. Intriguingly, Ziebarth finds that, at the firm level, the adjustment mainly took the form of a reduction in the number of hours worked rather than in the total number of workers employed. At the county level, however, he observes declines in employment. The difference in the two perspectives is a consequence of firms entering and exiting. Some of the rise in unemployment at the county level resulted, as one might expect, from firms disappearing, but Ziebarth shows that most came from a lack of entry. Of course, these results are for Mississippi, a primarily agricultural state. One can only hope that Ziebarth will pursue a similar analysis for states in the heart of the manufacturing belt.

Ziebarth's third chapter, "Did the National Recovery Administration Foster Collusion? Evidence from the Macaroni Industry," coauthored with Chris Vickers, examines the effectiveness of the cartelization promoted during the early New Deal under the National Industrial Recovery Act. The case at the heart of this chapter (macaroni) has been the subject of a previous study arguing that the industry was

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too heterogeneous for collusion to be successful—that large firms and small firms had interests that were too fundamentally different for them to be able to agree on a cartel strategy. (See, for example, Alexander 1997) Ziebarth and Vickers counter this conclusion by posing a clear, simple intuitive test. They hypothesize that output prices should be much more responsive to changes in input costs in the absence of collusion than if the cartel was effective. Using census data on firms' costs and prices before and after the National Recovery Administration helped the industry organize a "code of fair competition," they find that prices of macaroni were much less response to costs under the code than they had been before.

Ziebarth has already added significantly to our understanding of the early years of the Great Depression. His dissertation is a powerful demonstration of the value of careful data work in combination with clearly posed quantitative tests.

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