Summaries of Doctoral Dissertations

Northern Spain Between the Iberian and the Atlantic Worlds: Trade and Regional Specialisation, 1550–1650

That Spain's economic performance in the early modern period was worse than that of northwestern Europe is well known.[1] Previous studies have concentrated on the roles of transatlantic expansion and regional divergences in economic development, but have treated these two factors as independent of each other. My thesis draws on recent trade theory and institutional economics and uses new archival material to show that growing transatlantic trade caused internal structural improvements that account for at least part of the regional differences; and that only by considering these two factors jointly can we understand Spain's success and failure in this period. The dissertation focuses on the integration of the northern Spanish region comprising the Cantabrian coast and its hinterland into the wider Atlantic commercial world.[2] It begins by developing a framework, which serves as a basis for analysing the relationship between foreign trade and the regional economy. This is followed by a two-part analysis of the commercial sector in the Cantabrian region and its interaction with the Atlantic and Iberian markets. Part one examines the structure of the sector in the late sixteenth century and the first two decades of the seventeenth century. Part two investigates and interprets the changes to this structure in the period 1620s to 1650s in response to the opening up of a new trans-Atlantic trade. This is followed by the overall conclusions of the dissertation.

THE HYPOTHESIS

The thesis begins with a discussion of the potential consequences on the distribution of gains from trade of an opening up of new trades. Recent trade theory has stressed that gains from trade come in the form of two distinct processes: gains from exchange and gains from specialization.[3] Studies of commercial expansion have focused predominantly on *gains from exchange* in the form of additional factors of production and "colonial" goods. The debate was on whether or not the increased amounts of land, (forced) labor, sugar, tobacco, bullion, and such like could have had any lasting repercussions for the participating economies.[4] This thesis argues instead that *gains from specialization* were more important in changing the economic structure of Spanish regions during the commercial expansion of the early modern period.

In the pre-industrial world, specialization was the most potent agent of economic growth. Gains from specialization are distinct from gains from exchange in that the former is a dynamic concept whereas the latter is static. Specialization gives rise to a number of separate dynamic welfare gains, which can cause important structural changes. Increased competition reduces monopolistic rents and can result in improved organizational structures. Early modern markets were subject to extremely high information costs. Under these

[1] The author would like to thank her supervisor S.R. Epstein and examiners B. Yun Casalilla and D. Ormrod for invaluable criticism and support. Financial assistance from a European Union TMR Marie Curie Fellowship, the Cusanuswerk Begabtenförderung and the London School of Economics is gratefully acknowledged.

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[2] The present day provinces of Guipúzcoa, Vizcaya, Cantabria, and Asturias are understood as one economic region. While its political organization was fragmented, its economy depended on a more-or-less uniform structure based on fishing, shipbuilding, iron production, and maritime trade combined with a relatively poor agricultural base.

[3] For the basic concept see Markusen et al., *International Trade*.

[4] See, for example, O'Brien, "European Industrialization"; O'Rourke and Williamson, " Heckscher-Ohlin Model"; and Stein and Stein, *Silver*.

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circumstances external economies of scale account for the large degree of specialization of regions or single ports on only a few products. Internal economies of scale lower costs and increase competition forcing high-cost firms to exit, albeit increasing the risk of one firm or organization becoming a monopolist. These dynamic factors do not give an unambiguous prediction as to the distribution of gains from trade.[5] If an organization benefits hugely from internal economies of scale the increase in competitive pressure and the resulting decrease in monopolistic position might well be offset or even overcompensated. Similarly, if the pattern of specialization is determined by scale economies rather than by comparative advantage, it is arbitrary which region's respective sector establishes itself as a prime-mover in the market. It could thus reap initial benefits, which make it impossible for another region's economy to move into the same market.

As a consequence of imperfect markets the gains from trade would not have been shared equally between regions, between sectors within regions or, for that matter, groups within society. Therefore, the dissertation argues that an institutional analysis of the forms of organization and strategies employed to overcome market imperfections such as information asymmetries shed further light on the repercussions of trade expansion on the regional economy. Commercial organizations and individual merchants were the agents that had to respond to new opportunities from new trades opening up.[6] Merchant guilds' maximization strategies varied from defending regulated monopoly trade that tried to capitalize on internal economies of scale to endorsing unregulated "free-trade."[7] Equally, merchants could change their attitudes towards collective action in guilds or similar organizations.[8] Thus, a complete picture of the impact of commercial expansion has to combine a study of the traded quantities with one of structural change comprising (inter-) regional specialization, degree of monopolization (market structure) and organizational adaptation.

The dissertation is based on previously unpublished data collected from various Spanish and English archives.[9] Three sets of documents deserve special mention. About 270 criminal and mercantile court cases provided valuable insights into the trading networks in northern Spain and shed light on the relations among merchants and between the merchants and the local merchant guilds. The second large group of sources comprised notarial documents mainly contracts between merchants and producers, sales, tax bills, bills of lading and wills. Among these were 1200 tax bills, which were used to construct a new set of data for wool exports from Bilbao in the first half of the seventeenth century. Finally, I constructed the main trade series from the Bilbao port books for the first half of the seventeenth century.

CANTABRIA, CASTILE AND THE SPANISH ATLANTIC IN THE SIXTEENTH CENTURY

In the sixteenth century, the Cantabrian region's trade was integrated with that of the bigger northern-Castilian commercial networks. Centered on the Castilian fairs and the commercial town of Burgos, this commercial system maintained many characteristics of its late-medieval structure. Commerce was organized through privileges and monopoly rights granted to certain towns and their merchant communities; this caused specific activi-

[5] Krugman and Venables, "Globalisation"; and Krugman, "Increasing Returns."

[6] Landa, *Trust*; North, *Structure*; Williamson, *Economic Institutions*.

[7] Some issues related to the strategies of such companies can be found in Jones and Ville, "Efficient Transactors"; and Carlos and Nicholas, "Theory."

[8] There is a large literature on different strategies of overcoming principal-agent problems in high-information-cost situations inspired more recently by Greif, "Reputation."

[9] Archivo Foral de Bizkaia (Bilbao), Archivo Histórico Provincial de Vizcaya (Bilbao), Archivo Histórico Provincial (Santander), *Archivo Histórico Nacional* (Madrid), *Biblioteca Nacional* (Madrid), the Public Records Office (London) and the British Library (London).

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ties and trades to concentrate in one town, while reducing specialization between firms. In the second half of the sixteenth century, the inflow of Spanish American silver reinforced the predominance of the fair system and the existing trading monopolies. The Crown used the fairs to clear its payments with American remittances, and the additional cash inserted into the system lubricated both the money market and the goods trade. The injections of cash created profit opportunities for northern Spanish merchants, but they also raised the potential risks considerably.

The two big mercantile organizations, the *Consulados* of Burgos and Bilbao, within this trading sector opted for strategies that captured internal economies of scale. Both followed policies similar to that of regulated companies and competed for monopoly rents in the most important export trade, wool for Flanders and northern France. The structure of trade was also reflected in the pattern of economic specialization. Burgos enjoyed a stronger position due to its links with the fairs and a royal monopoly; and it deliberately slowed a process of commercial agglomeration that was under way in the Cantabrian region, by diverting the wool trade from Bilbao to smaller ports. Although Bilbao was the main commercial center on the coast it was confined to shipping services, smaller trades and fishing, notably the Newfoundland cod fisheries.

The *Consulados* provided vital services for the commercial community by reducing transaction costs and risks that resulted from imperfect information. Within this organizational framework merchants chose a strategy of integration. Local merchants formed part of their *Consulado*, whereas foreigners tried to become part of the local community through marriage, business alliances and other economic and social strategies. In addition, non-citizens sought collective protection through consuls or formal commercial treaties. Available data on taxes demonstrate how this commercial structure declined after the 1580s. The financial crisis of the Habsburg Crown unsettled the fairs when American silver ceased to flow in. The simultaneous conflicts with the Netherlands, England, and France dislocated the northern European wool trade. The interdependence of financial and commercial functions concentrated in a small number of places and firms accelerated the decline further into a prolonged recession at the end of the sixteenth century.

NEW OPPORTUNITIES OF THE SEVENTEENTH CENTURY: CANTABRIA BETWEEN THE NORTH ATLANTIC AND CASTILE

Bilbao port book data show that the integration of the northern coast into the British North Atlantic trading networks allowed the area to escape recession in the late 1620s and early 1630s, earlier than hitherto assumed. The same source shows that the revival was driven, on the one hand, by an upturn in wool exports and, on the other hand, by a large increase in fish imports. Neither wool nor fish were new in Northern Spanish trade; but a closer examination showed that although the products traded changed little, the structure of this trade had changed substantially. Firstly, a new series of wool tax data for Bilbao in the 1630s to 1640s confirms an impressive rise in wool exports. But wool was now exported to the English West Country rather than to the Netherlands or France as previously. Secondly, I am able to use a seasonality analysis and the reconstruction of the journeys of individual ships by matching English and Spanish sources to show that the largest part of the increasing cod imports came from the young North American mainland colonies as early as the 1630s. The supply of one of Spain's staple food stuffs, dried and salted codfish or *bacalao*, had been transformed from a traditional regional fishing sector into a mercantile enterprise organized by English and New England merchants within less then two decades. This is a clear example of how trade dynamics altered the structure of particular sectors in the Cantabrian economy.

A triangular trade came into existence based on wool exports from Northern Spain to the English West Country, manufactures from there to the mainland British colonies and

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imports of *bacalao* to Northern Spain. The trade permitted the North American colonies to improve their balance of payments. Furthermore, my conservative estimates suggest gross total profits of up to 30 percent on cod exports from North America. The English West Country benefited from a supply of high-quality wool equivalent to about 8–10 percent of total English production, which was crucial for the development of the "new drapery" industry in that region. For Cantabria, integration into the new triangular trade was the way out of commercial depression. The triangular trade was mainly in the hands of English merchants from a large number of provincial ports with correspondents in North America and English agents resident in Bilbao. The exclusion of outsiders from all three legs to secure internal economies of scale would have been very costly. Instead the trade fostered a very different type of economic specialization based on external economies of scale and geographic concentration of commerce: the new trade data show that the process of economic agglomeration in the Bilbao area accelerated and secondary ports declined. This was not a sign of commercial crisis but of external scale economies that the rise of a single commercial center offered. The thesis also shows how the changing structure of foreign trade re-distributed the gains from trade among Cantabrian sectors, benefiting consumers, merchants, and iron producers but harming shipbuilding and shipping.

The structural changes in external trade also transformed the economic relationship between Cantabria and Castile. A large number of sales contracts for wool and fish shows that trade between the ports and the wool producing-regions and fish-consuming areas in Castile became more direct. Cantabrian merchants bought wool directly from the producers in order to supply English merchants in Bilbao. Small-scale muleteer–traders rather than the large Castilian merchant houses organized the distribution of fish to Madrid and elsewhere. The disappearance of the northern Castilian intermediaries reduced transactions costs. The thesis shows that inter-regional trade changed in style but it did not disintegrate as some historians have argued.

The *Consulados* reacted in different ways to altered trading conditions. Burgos was losing trade and market power and attempted to reinforce the sixteenth-century-style monopoly trade patterns. Bilbao's *Consulado*, which saw its trade increasing after 30 years of crisis, embraced the new opportunities. It transformed itself from a "regulated company" into a body defending local and foreign merchants' access to trade vis-à-vis any attempts to monopolize commercial activity. The new English community in Bilbao and the ways in which individual merchants adapted are a strong indicator that northern Spanish commerce had grown out of its late-medieval mold. The successful co-existence of a predominant English merchant community with the local Cantabrian one challenges traditional accounts of obstacles to economic activity on the Iberian Peninsula. The dissertation argues that the Cantabrian trade was characterized by unusually low barriers to entry for foreigners and Spaniards alike, and that the regional legal framework effectively guaranteed the enforcement of contractual agreements.

CONCLUSIONS

This dissertation concludes that gains from specialization produced dynamic welfare gains at the level of international trade that translated into incentives for structural improvements within the northern Spanish economy. These in turn led to further welfare gains in the Cantabrian region. In the sixteenth century, the integration of the northern Spanish system into the silver flows from Spanish America served to re-enforce the existing structure. The high degree of vertical integration provided benefits for members of the merchant guilds but guild strategies resulted in lower agglomeration economies in the coastal region and increased transaction costs for other economic actors. Gains from specialization transformed the northern Spanish commercial sector in the first half of the seventeenth century and ultimately account for the relative economic success of the coastal region compared to its Castilian

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hinterland. The new trade was based on traditional products, not typical "colonial" goods. Structural alterations rather than increased availability of goods or factors of production formed the most decisive link between the external-trade sector and the regional economy.

Relations between the coast and Castile became less subject to the control of monopoly-trade organizations. Within the coastal region a market-led process of agglomeration occurred providing incentives for more competitive trade. The *Consulados* adopted a maximization strategy according to the conditions they found in the market. Burgos' merchants acted perfectly rationally—if ultimately unsuccessfully—when they defended their old monopoly trade far into the seventeenth century; the formerly monopolist Bilbao *Consulado* modified its strategy when new commercial opportunities arose. The thesis also demonstrates how merchants adapted to the changing organizational structure of trade in an economically rationally—one might even say, "progressive"— way. New patterns of specialization pushed northern Spanish trade out off the Middle Ages and into the (early) modern period.

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Peasants, Productivity, and Profit in the Open Fields of England: A Study of Economic and Social Development

This thesis provides the first direct measurement of agrarian labor-productivity rates for medieval western Europe and pre-industrial England.[1] Economic historians believe that the output of food per worker directed the course of development before industrialization. The general wisdom is that agrarian labor productivity was low for much of the middle ages. It began falling over the course of the thirteenth century or at the turn of the fourteenth century. By 1300 population outstripped resources, and the amount of food produced per worker was at the lowest level that it would reach in the pre-industrial period. A Malthusian crisis was either imminent or in progress, depending on the historian, until the Black Death stripped Europe of its "surplus" population.[2] There is also a general consensus that agrarian labor productivity rose sometime in the seventeenth or eighteenth century in England. Debate centers on why the rise occurred. Explanations usually center on shifts in institutions (for example, efficiency gains garnered by a change to private-property rights or economies of scale provided by enclosures) or in the quality of labor inputs.

Because of the scarcity of medieval aggregate data, agrarian labor productivity can only be measured accurately at the micro level. In this thesis, nine open field English villages, 1279–1415, were reconstructed at the local level and studied comparatively in their socio-economic and environmental contexts. The focus was largely upon the arable sector because it is here that researchers predicted a dramatic surge upward in output per worker during the early modern era. Medieval manorial farm accounts supply information on all factor inputs and outputs on the lord's share of the open fields, the demesne. It was known that these records could supply accurate direct estimates of labor productivity but their complexity served as a deterrent. It took three years to construct a methodology to measure labor-productivity rates. My efforts were facilitated by the exceptional detail concerning labor practices in Ramsey Estates' records. These villages, scattered throughout four counties, were all located within Ramsey Estates. Considerable evidence indicates that labor-productivity rates on the demesne are representative of those in the vast peasant sector.[3]

Local records also allowed the reconstruction of adult workforce numbers, which are crucial in establishing the impact of labor-productivity rates on the medieval economy. Population levels are unknown for most of this period. Estimates of population numbers in

[1] I would like to thank, with much affection and gratitude, my thesis supervisor, J. Ambrose Raftis. Many thanks are also due to committee member, R. Albert Berry. Sincere thanks are owed to Kathleen A. Biddick and Peter H. Lindert. All errors in the dissertation are, of course, my own. Funding for this research was partially supplied by the Social Sciences and Humanities Research Council of Canada. This dissertation was completed in 2001 at the University of Toronto.

[2] The best known proponent of this perspective as applied to English development is Postan. See, as an example of his work, "England" or *Essays on Medieval Agriculture.* A recent critique of Postan's model is provided by Hatcher and Bailey in *Modelling the Middle Ages*. See also Biddick, "Malthus." For an alternative perspective on medieval development, see, for example, the work of Britnell. He argues that strong growth in English markets over the course of the thirteenth century led to a rise in productivity in all sectors until 1300. Markets promoted productivity increases by encouraging innovations and specialization. See "Commercialization." For arguments that technological progress allowed agrarian labor productivity to rise until the Black Death, see, Hallam, "Population"; and for a rise until 1300, see Persson, *Pre-Industrial Economic Growth*. Again, for a critique of these views, see, Hatcher and Bailey, ibid.

[3] For an outline of the methodology employed in the thesis, see, Karakacili, "Peasants," pp. 15–104, 178–91.

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England for *c.*1300 range from about three and one-half to six million or more.[4] The agricultural sector dominated the English medieval economy and employed most people, directly or indirectly. The real value of arable output per agricultural worker, then, provides insights into GNP per worker and thus GNP of the economy as a whole.

The average output of food per worker in the pre-1350 period in these villages is almost equal to or slightly higher than the literature's best estimates for early-nineteenth-century workers. In mixed bushels, average output in the villages ranged from 222 to 371 bushels of grains and legumes per worker, or an average of 224 wheat equivalent bushels (gross). Robert Allen's work implies an output of around 195 wheat equivalent bushels (gross) for workers on arable oriented farms by 1800. Gregory Clark estimated 246 wheat equivalent bushels per worker by 1851 (net and including both arable and pastoral sectors).[5]

The net surplus of food (net of seed and legumes) per worker before 1350, even in relatively poor harvest years, sufficed to feed him or herself, some nonworkers, and at least four adults, who could therefore hypothetically be employed in nonagricultural activities. This surpasses the levels predicted by E. A. Wrigley for English agricultural workers in 1801 who could feed 1.76 adults with their food surplus.[6] Evidence from after the Black Death indicates that the average output of food could have increased even further before 1350, with additional labor inputs. The point of diminishing returns to the average productivity of labor was not reached before the Black Death.

The surplus of food produced by each worker, in the period before the Black Death, during which time it was allegedly at its lowest level, was very high. There could not have been any danger of a Malthusian crisis, because this scenario would necessitate that the output be insufficient to feed the worker. And if Malthus does not apply in this period, it is doubtful whether he is applicable to later centuries with lower population levels.

However, these estimates only tell us about the potential of medieval agrarian workers to support nonagrarian workers. In other words, they indicate the considerable amount of surplus food available. It would be the number of adults available to work, coupled with productivity rates, which would be a crucial element determining how many people could be occupied with nonagricultural tasks. In considering arable and pastoral labor inputs in conjunction with actual village workforce numbers, it appears that a maximum of 36 percent of the adults were needed full-time in agriculture before the Black Death. Wrigley, Phyllis Deane, and W. A. Cole, estimated that about 36 percent of the English workforce was employed in agriculture around 1800.[7] Peter Lindert places 22.5 percent of men or less in agriculture *c.*1801.[8]

Medieval evidence sheds some light on the major influences upon agrarian labor-productivity rates. Output per worker varied from village to village. Labor inputs from village to

[4] For a discussion of various population estimates, see, Smith, "Demographic Developments." See also, Campbell, *English Seigniorial Agriculture*, especially pp. 402–04.

[5] Allen argued that a 100 percent increase in the output per worker on crop- oriented farms took place between 1600 to1800; at about 77 wheat equivalent bushels, pre-1350 rates would be below those found in 1600. Allen's implied estimates were constructed using data found in his article "Growth," table 11, p. 131, and table 13, p. 140. Because his workers on crop-oriented farms would no doubt spend some of their days working in the pastoral sector, these calculations are lower-bound estimates. Clark argued that the rise in agrarian labor productivity was in the 300 percent range, *c.*1300 to *c.*1850, changing from 56 to 246 wheat equivalent bushels. Clark's most recent estimates are found in "Labor Productivity." My thanks to Allen and Clark for their help.

[6] Implied in Wrigley, "Urban Growth," table 4, p. 700. If Wrigley is correct and England was a net importer of food by the end of the eighteenth century, this number is a high estimate. See on this point, ibid., p. 695.

[7] Wrigley, "Urban Growth," table 4, p. 700. Dean and Cole's estimate refers to Great Britain. The estimated numbers employed in agriculture would be slightly below 36 percent of the workforce because the percentage given by Deane and Cole includes those working in forestry and fishing, *British Economic Growth*, table 30, p. 142.

[8] Lindert, "English Occupations," table 3, pp. 702–03.

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village, and thus output, were shaped by farmers' diverse orientations to arable or pastoral production, or commons enterprises as well as their divergent environmental contexts. Shifts in output of food in the same village from year to year were determined by changes in yields per acre. There is no indication that there were any significant changes in labor inputs in these villages from year to year as the result of growing population pressures before the Black Death. Instead, during this time, as today, farmers' yields per acre were influenced mostly by climate.[9] Crop yields could vary up to 49 percent from year to year. Allen and Clark suggest that an increase in agrarian labor productivity during the early modern period was partly achieved by a rise in the yields per acre; an overall increase in labor productivity for all farm workers might occur as workers in the harvesting sector produced more food per day without a large increase in labor inputs.[10] The manorial account rolls supply empirical evidence to buttress this speculation that a rise in the average output per farm worker could be achieved as a result of an increase in the productivity of labor in harvesting.

Serfdom clearly did not lower labor-productivity rates, as some historians argue, because rates on the lord's share of the open fields with much of the work performed by tenants, "serfs," equaled estimates for free-wage labor in the early nineteenth century. Analysis of the labor-rent system on Ramsey Estates, one typical of medieval estates, seems to indicate that the manor was not a labor-based system. The lord, as many estate historians have noted, was not usually a commercial farmer attempting to make as much money as possible but one whose orientation was to capitalize at a low level, one sufficient to fulfill his consumption needs.[11] The manor was set up to minimize and set capital inputs to a fixed level, and the operation of labor rents should be understood within this institutional structure. The labor required to harvest and process the lord's crops was extensive, could fluctuate from year to year, and in the case of the former, was the most expensive. Tenants performing labor on the demesne for all or part of their rent were simply a part-time agricultural workforce whose inputs were expended largely upon harvesting and processing the demesne crops. From the tenants' perspective, such work occupied a minute percentage of household time, perhaps 6 percent. Given the medieval labor-productivity rates, performance of labor rents would not strain tenants' labor resources for their own holdings.

The open fields did not have a negative impact on labor productivity; this is demonstrated by the fact that these high pre-1350 rates were achieved in open fields. Current views on the open fields have shifted away from the very pessimistic views of the past. In particular, the research of Carl Dahlman, David Hall, and D. McCloskey have pointed out the sound economic and environmental rationale of the system.[12] Still, many economic historians thought that small holdings and crop rotations lowered returns; enclosures allowed farmers to reap benefits from economies of scale and specialization, increasing output per worker and maybe even per acre. Instead, when the daily operations of the open fields are examined in detail, it emerges as a sophisticated system set up to facilitate small-scale investment and maximize the profits of its practitioners, the peasants. In particular, the furlong system of the open fields, which distinguished land by soil type and typography, helped farmers to determine the optimum amounts of seed, manure, labor, crop type, and cropping rates. The open fields allowed farmers to minimize their inputs and maximize their returns. Its very efficiency might help to explain how these medieval labor rates could equal or surpass the literature's estimates for early-nineteenth-century farm workers

[9] For an overview of climate's effect upon English yields per acre over the course of the fourteenth century, see Farmer, "Grain" and "Grain Prices."

[10] Allen argues for a 50 percent increase in agrarian labor productivity over the course of the seventeenth century due to a rise in yields per acre, *Enclosure*. Clark thinks that output per worker rose about 25–45 percent from 1600 to 1850 as yields per acre rose without a corresponding rise in labor inputs. See Clark, Labour Productivity, pp. 221–31.

[11] For an analysis of the landlord's managerial policy on Ramsey Estates' manors, see Raftis, *Estates*.

[12] Dahlman, *Open Fields*; Hall, *Open Fields*; and McCloskey, "Prudent Peasant."

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on enclosed farms, who had such technological advantages as improved seed yields and partial mechanization.[13]

Before the Black Death, the real value of the output per worker was high, comparable to the literature's estimates for early-nineteenth-century levels. Most economists would expect that agrarian labor productivity would have increased to even higher levels after the Black Death. However, after 1350, the real value per worker in most locales eventually declined from 17 to 33 percent. In part, the fall in productivity is attributable to changes in the crop mix, seeding rates, and fallow acreage. Real value per acre was probably far more important for small-holders and family farms. The decline here was even more severe, up to 43 percent.

A comparative analysis of the real value of output per worker and per acre, pre- and post-1350, suggests a very different picture of medieval economy and society. Based on the belief that agrarian labor-productivity rates were very low before the Black Death, most historians thought that poverty was widespread and the economy was stagnant. This perspective shaped the understanding of the post-1350 period where, based in part on the belief that agrarian labor productivity would rise, it was thought that living standards would improve for most people. The evidence suggests that the converse was the case.

Peasants were not subsistence farmers driven by hunger but commercial farmers driven by a desire to maximize their profits.[14] After the Black Death, labor inputs did not fall in proportion to the huge population loss but generally rose or remained constant with earlier levels. Changes in labor inputs and crop mixes after the Black Death were responses to relative price changes.

Economic trends after the Black Death seem to have been shaped by a growing concentration of wealth in the vast peasant sector. J. Ambrose Raftis has tracked the emergence of open-field farms totaling 100 acres or more in most of these villages. The open fields came to be dominated by relatively few farmers.[15] This event shifted the pattern of investment and the level of profits in agriculture. These farmers' decisions appear to have had a detrimental impact on growth and living standards after the Black Death. In the first half of the fourteenth century, a more equitable division of wealth in the peasant sector seems to have fostered growth and promoted a higher level of living standards for many more people.[16]

More studies that measure agrarian labor productivity directly are necessary to gauge the degree to which these villages are representative of England as a whole and of the continent. Still, these doctoral findings cast considerable doubt on the traditional perspectives concerning England's development path to economic greatness. An agricultural revolution, currently defined as a sharp rise in agrarian labor productivity, was not a necessary prelude to industrialization, at least in these villages. Even before 1350, at least 64 percent of the workforce in

[13] Positive reassessments of medieval agricultural technology and medieval English land productivity have been offered by the pioneering research of Campbell, for the middle ages, and Overton, for the early modern period. They found that yields per acre in English open fields before the Black Death were not surpassed there until the seventeenth and eighteenth centuries. See, for example, their "New Perspective," and most recently, Campbell, *English Seigniorial Agriculture*. Their research and that of other historians has underscored the constancy of technology widely employed by farmers in pre-industrial agriculture. The amount of labor input per acre was the key determinant of the level of yields per acre. This point led Grantham to argue that French agrarian labor productivity should be pretty constant between 1200 and 1800 until harvesting was mechanized in the nineteenth century. He also thought that the output of food per worker was relatively high before the nineteenth century. See, Grantham, "Divisions."

[14] For a recent attempt to assess the scale and the effect of English peasants' market activities on a national level, see, Masschaele, *Peasants*.

[15] Raftis, *Peasant Economic Progress*.

[16] Jan De Vries' work has highlighted the degree to which decisions made by peasant households in the huge agricultural sector shaped the direction of economic development. See, for example, " Industrial Revolution."

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these villages were free to engage in nonagricultural pursuits. Agrarian labor-productivity rates could change, as a comparative analysis of the pre- and post-1350 periods indicates. However, there was no barrier, institutional, tenurial, or technological in nature that held agrarian labor-productivity rates in check until the seventeenth or eighteenth century.

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New World Order: The Emergence of an International Monetary System, 1850 to 1913

By 1910 most nations on the planet had come to adopt the gold standard.[1] The global adoption of a common monetary regime was a significant change from 1870 when a variety of monetary regimes were used and only a few countries used a gold-based currency. This worldwide transformation reveals how diverse nations make the trade-offs associated with monetary regime choice, that *network externalities* contribute to the formation of monetary regimes because these institutions can decrease the transaction costs of trade and that these arrangements may significantly promote the evolution of a global economy. Moreover, the emergence of the classical gold standard appears to have been the result of path dependence, and it was only one of several economically viable regimes that might have emerged after 1870.

These conclusions become evident after the German Empire adopted the gold standard in 1872. This move set in motion the process of building an international gold standard. Falling like dominoes, Scandinavia, Holland, Belgium, Switzerland, France, Finland, and the United States achieved gold convertibility by 1879. These countries abandoned silver or bimetallic systems that had been in use for at least a generation.

Gold-based regimes arrived later in peripheral countries such as Argentina, Chile, Greece, and Italy in the 1880s. These countries' experiments did not last, and they soon reverted to inconvertible fiat regimes. However, most re-adopted gold by the first decade of the twentieth century. In the 20 years after 1890, Japan, The Straits Settlements, India, and The Philippines opted for some form of the gold standard. Circa 1910 the diffusion had become global.

[1] This dissertation was completed in May 2001 in the Economics Department of the University of California, Berkeley under the supervision of Barry Eichengreen. The Institute for Business and Economic Research (IBER) and the John L. Simpson Fellowship from the Institute for International Studies at Berkeley graciously provided financial assistance.

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I begin my study of these institutional changes by re-analyzing the end of bimetallism in France. Revisionist arguments in the 1990s came to the conclusion that the German switch to gold was not large enough to make France fall victim to Gresham's Law.[2] Rather than focusing on one country's actions, I ask if the regime choices of *other* nations including Germany affected bimetallism's viability. Between 1873 and 1877 the French very much hoped to resurrect bimetallism after having foiled Bismarck's attempt to dump silver on France. However, many other significant countries were selling silver and joining the gold standard. These sales made bimetallism impossible to resuscitate by 1875 at the mint ratio of 15.5 to one. World silver would have been dumped on France while its gold would have been completely drained. Moreover, a much-talked-about international agreement between the United States and France to revive bimetallism was apparently not feasible. Although this removed one regime option for countries, it remains to explore why gold triumphed.

Chapter 3, co-authored with J. Ernesto López Córdova, begins this task by exploring the benefits from choosing a particular monetary regime. We provide evidence consistent with the idea that a commodity money regime and a currency union could significantly increase trade among those countries choosing to adopt. To do so we use a gravity model of trade on a new, broad-based macro-historical dataset. After controlling for economic, geographic, and political variables, we find that countries sharing a gold standard traded up to 60 percent more with each other. Countries in a currency union traded two times more. The latter results compare with research on contemporary data.[3] We also find that our qualitative results are robust to regime endogeneity, sample selection, estimation technique, and the inclusion of variables that might have biased our baseline estimate. By reducing transaction costs to trade, international monetary institutions appear to have had a significant impact on nineteenth-century globalization.

In chapter 4 I examine the reasons why countries chose gold. The late nineteenth century provides a unique "laboratory"' for empirical research on monetary-regime choice as most nations ended up adopting the gold standard but the diffusion was spread out over nearly 40 years. My arguments rest on the history and experience of over 40 independent countries between 1870 to 1913, and the econometric analysis uses data from 18 leading nations over 43 years.

In this chapter I develop a theoretical model of the timing of monetary-regime change, generate testable hypotheses and check these against the data. The micro-founded theoretical model describes the interactions between trade, development, and monetary regime choice. It suggests that countries would adopt the gold standard sooner after 1870 the more they traded with gold-standard countries and the less they traded with countries that shared their current regime. These variables naturally increase as more countries adhere to a particular regime, and so network externalities are pervasive in the international monetary system. The model also argues that more-developed nations and countries with sound financial systems would adopt the gold standard earlier.

Using duration analysis, the data confirm the model's key hypotheses. I find strong evidence for network effects. A one-standard-deviation increase in the ratio of trade with gold standard countries to GDP decreased the time until adoption after 1870 by nearly 42 percent. This dropped the median expected time to adoption from 1879 to about mid-1875. On the other hand, adding one standard deviation to the ratio of trade with countries on the same standard to GDP increased the median time until adoption by about eight and a half years.

Development and financial systems mattered, too. Higher levels of real per capita output strongly decreased expected adoption time. As alternative measures of financial-system strength, I use gold reserves relative to financial liabilities outstanding or an indicator of

[2] See Flandreau, "French Crime"; and Oppers, "Was the Worldwide Shift."
[3] See Rose, "One Money."

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whether a country had a fiat currency. Countries with high ratios adopted the gold standard earlier; fiat countries adopted much later. Explanations for why countries with a paper currency delayed include lengthy political debates over the par value of the currency, the benefits of a floating exchange rate or the wish to avoid a future financial crisis.

My baseline empirical results are also robust. To circumvent a possible endogeneity bias, the average distance to gold standard countries, whether a country shared a border with a gold standard country, and population size serve as instrumental variables for trade patterns. Instrumental-variables regressions show the magnitude of the network externalities coefficient is larger while it is just significant at the 90 percent level of confidence.

I also find that many other factors cannot explain the timing of the move to gold convertibility. For example the data reveal the gold standard was not a defense against exchange rate volatility nor did creditors foist the gold standard upon debtors. Also, the search for a "good-housekeeping seal of approval" cannot explain the timing of the implementation of the gold standard.

My dissertation suggests that there are important linkages between the global economy, foreign policies, and domestic regime choice. Moreover, the rise of the gold standard was likely part of an important virtuous cycle. Inter-connection inspired institutional homogenization. In turn, regime coordination significantly contributed to late-nineteenth-century globalization.

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The Economics of Cotton Harvest Mechanization in the United States, 1920–1970

The hallmark of economic development the world over is the long run transfer of labor from agriculture to the industrial and service sectors.[1] This study investigates the mechanization of the cotton harvest, one of the most dramatic episodes of technological and demographic change in United States history. Following World War II millions of workers from California to the Carolinas handpicked all American-grown cotton, but by the 1970s farmers gathered the entire crop with cotton-harvesting machines. This transformation increased the productive efficiency of inputs to agriculture, restructured the American economy, and forever changed the economic, social, and political life of cotton communities and states and the nation at large. The mass exodus of African-Americans from the cotton fields transformed race from a local matter to a national concern with profound implications for racial economic equality, the decline of America's cities, and the welfare state.

Two puzzles motivated this study of technological change. First, why did farmers adopt cotton-harvesting machines in a distinctive west-to-east pattern? To help explain this pattern

[1] This dissertation was completed in 2000 in the Department of Economics at the University of Illinois, Urbana-Champaign, under the supervision of Lee Alston, Larry Neal, and Werner Baer.

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of diffusion, I estimate a time series that measures growers' incentives to switch from the traditional hand harvest method to cotton-harvesting machines. Part of the explanation for the pattern of technological diffusion relates to a second puzzle: why did cotton producers in the western and southern parts of the cotton belt differ so markedly in the organization (and ultimately mechanization) of production and in the types of harvest labor they employed?

Prior to harvest mechanization, two zones of labor-market arrangements and production practices existed across the pan-continental Cotton Belt: the west, central Texas to California, and the south, east Texas to the Carolinas. While sharecroppers and tenants picked the bulk of southern cotton, day-haul laborers, migrants, and foreign workers gathered the crop from Texas to California. By employing workers in spot labor markets for each phase of production, westerners could adopt profitable labor-saving devices in a piece-meal fashion for each task in order to lower costs on every margin. In contrast, southern reliance on annual labor contracts discouraged adoption of labor-saving devices or techniques for individual tasks until all operations could be mechanized. Although many commentators have blamed tenancy and sharecropping arrangements for the poor record of southern economic development, Warren Whatley argues that annual labor contracts allowed southern planters to efficiently meet their most important constraint: roughly equal dual peak labor demands.[2] Thus, annual labor contracts prevented adoption of labor-saving technology until each period of peak labor demand could be mechanized. If that is true, though, why did not growers in the Mississippi Delta adopt western labor market practices (i.e., hire day-haul and migrant workers) to achieve greater flexibility in substituting capital for labor in order to minimize costs?

I show that agroclimatic differences prevented southern growers from adopting labor-market arrangements suited to the West. Cotton producers in the hot, humid South faced a more complicated cotton-harvest problem than did those in the semi-arid West. The harvest could not occur when the fields were too muddy or the cotton fibers were too moist (which deteriorated crop value); both factors meant that southerners had many fewer hours suitable for gathering the crop than the number of calendar days and daylight hours. Further, high heat and humidity accelerated the growth of microorganisms which discolored the white cotton fibers, diminishing their value. To avoid such deterioration of cotton quality, southern planters established a resident labor force, housed adjacent to the fields, that could be rapidly mobilized with bell signals during the limited available harvest hours in order to conduct a timely harvest.[3] Thus, I find that the tenant plantation constituted a cotton quality-preserving production organization, regionally-adapted to agroclimatic constraints.

Growers in the semi-arid west faced a much less complicated cotton-harvest problem (i.e., less threat of deterioration and more potential harvest hours), permitting them to depend upon a less reliable, transitory workforce. In the humid South weeds grew abundantly creating two peak periods of labor demand (weeding in the spring through summer and picking in the fall) which encouraged the use of annual labor contracts to lower labor market transactions costs.[4] Because weeding required much less labor in the semi-arid West, weather conditions, in another way, help account for the variance in labor-market arrangements. Thus, I argue that differences in climatic conditions prevented southerners from adopting western labor-market practices. I use this differential agroclimatic thesis to help explain the spatial distribution of plantations within the south, namely its concentration in the Yazoo-Mississippi delta, an area in Mississippi between the Yazoo and Mississippi rivers. Historically, cotton planters in the Yazoo-Mississippi delta produced particularly long and valuable cotton fibers. Because such price premiums depreciated sharply if the

[2] Whatley, "Southern Agrarian Labor Contracts."
[3] On the bell system, see Brannen, "Relation"; and Woofter, "Landlord."
[4] Whatley, "Southern Agrarian Labor Contracts."

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cotton was discolored, the quality-preservation benefits of the tenant plantation (as well as economies of scale in production and marketing) were the greatest in the Yazoo-Mississippi delta. Understanding how and why labor-market arrangements varied so distinctly helps to explain differential long-run patterns of technological change, in this case of the cotton-picking machine across the pan-continental Cotton Belt.

Historians and economists have devoted considerable effort to understand the process and implications of the mechanization of the cotton harvest.[5] Absent estimates of total hand-harvest costs, though, this debate has occurred in somewhat of an empirical void without correctly measuring farmers' incentives to adopt the new technology. To address this lack of data, first, I estimate the share of the cotton-harvest labor market composed of local workers, day-haul labor, migrants, and foreign workers. While tenants and sharecroppers harvested their own fields, the rest of the southern cotton crop was either gathered by the owners or by employing local workers (typically, tenants or croppers who supplemented their incomes with some seasonal cash employment) and some day-haul labor (especially near cities, such as Memphis). In contrast to the claims that western growers only employed migrants,[6] I find that westerners relied upon spot labor markets but employed various combinations of local day-haul, migrant, and Mexican contract workers (known as *braceros*). For example, *braceros* gathered over 75 percent of the cotton in New Mexico but only 11 percent in California where local workers provided two-thirds of the harvest labor force.

Using these labor-market shares by type of harvest workers, I construct a time series of labor costs per pound of cotton lint for the12 major cotton *picking* states from 1949 to 1964.[7] To date hand-harvest expenses have been computed using only cash costs, a poor measure of total hand-harvest costs. Estimating growers' costs per pound of lint requires: converting the nominal wages paid by growers for seed cotton (which contained cotton seeds and assorted field debris) to the total real cost per pound lint; adding in-kind benefits (mainly, meals and housing); and overhead expenses.[8] Total hand-harvest costs differed significantly from wage rates over time and space. For example, nonwage labor costs averaged as much as one-quarter of total hand-harvest costs during the 15-year period studied, not zero as suggested by Frank Meier and Whatley.[9] Contrary to common perceptions, at the beginning of the period of analysis, from 1949–1951, the South did not constitute a low-labor-cost region: growers in the Mississippi Delta states paid no less to have their cotton crop harvested by hand than did those out West, although southeastern planters did pay 25 percent less. By 1962–1964, the South was a cheap-labor region where hand picking cost growers 14 percent less than in the West.

Whereas my labor-cost calculations for each type of worker are novel, this is especially the case of my estimates for *braceros*, Mexicans employed temporarily in the United States under the U.S. Department of Labor's Mexican Farm Labor Program, authorized by P.L. 78. I determine that *braceros* composed 17 percent of the national cotton-harvest labor force during the 1950s. Unlike domestic farm labor, Mexican contract labor received an enviable package of living and working conditions and compensation guarantees mandated by federal law and a diplomatic accord (compliance is another matter which I discuss at

[5] Alston and Ferrie, *Southern Paternalism*; Fite, *Cotton Fields*; Kirby, *Rural Worlds*; Peterson and Kislev, "Cotton Harvester"; Street, *New Revolution*; Whatley, "New Estimates"; and Wright, *New South*.

[6] Whatley, "New Estimates."

[7] The 12 major cotton-producing states that used mechanical pickers are Alabama, Arizona, Arkansas, California, Georgia, Louisiana, Mississippi, Missouri, New Mexico, North Carolina, South Carolina, and Tennessee. Growers in parts of Texas and Oklahoma used mechanical strippers for which no cost data exist.

[8] Labor-specific overhead expenses included labor-recruitment and harvest-organization costs. The latter entailed the costs associated with employing weighers, inspectors, water boys, paymasters, and field bosses.

[9] Meier, "Analysis"; and Warren Whatley, "New Estimates."

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length). My work documents the fact that nonwage expenses of employing *braceros* exceeded those for migrants three-fold during the 1950s and in the early 1960s by a factor of five. Complaining of increased absenteeism and turnover rates among day-haul and migrant workers, growers paid such premiums for more dependable and hard-working Mexican guestworkers. Farmers certified by the Department of Labor could request and personally select hundreds of *braceros* on a few days notice. Mexican contract laborers harvested 25–40 percent more per week than other males, I find in the first such estimates, by working longer days and more days per week.

To explain the diffusion pattern of harvest mechanization I constructed a measure of relative cotton-harvest costs for the 12 major cotton-picking states from 1949 to 1964 by combining my hand-harvest-cost time series data with a corrected version of an existing machine-cost series. Relative harvest costs correlate well with the west-to-east pattern of mechanical-harvester adoption (as well as the patterns by state): throughout the 1949–1964 period growers in the West maintained a substantial cost advantage of mechanical harvesting over the Delta and those in the Delta over the Southeast.

These relative harvest-cost data also allow me to evaluate Meier's claim that mechanical-harvester diffusion resulted from falling machine costs rather than rising hand costs.[10] This characterization depicts events in the Mississippi Delta region I find, but in the southeast rising labor costs played as large a role in spurring technological change as did cheaper machine-picking rates. In the West, hand-harvest expenses rose one-third as much as machine costs fell. As I show, labor costs figure centrally in this episode of technological change with very different regional effects.

These time-series estimates of relative cotton-harvest costs will prove useful for those interested in the labor or technology aspects of cotton harvest mechanization, the post–World War II southern out-migration, and the cotton-belt rural economy. These data have broad implications for understanding the nature of postwar labor markets, changes in racial economic equality, the demise of American cities, the welfare state, and the social consequences of government funding (given that the federal government subsidized the technology but did not assist displaced cotton workers).

According to the most recent survey data available, farmers mechanically harvested only 20 percent of the world's cotton, mainly the U.S. and Australian crops. Most of the world's major cotton-producing countries, namely China, India, Pakistan, and Uzbekistan, pick their cotton entirely by hand.[11] This study of the patterns and consequences of cotton-harvest mechanization provides insights for policymakers in countries where this type of technological change may someday lead to enormous rural out-migration and to the substitution of capital for labor. Recently, for example, Brazil has experienced just such an episode of rural-to-urban migration due to the mechanization of the sugar cane harvest.[12]

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[10] Meier, "Analysis."

[11] Following the break-up of the former Soviet Union, the cotton harvest was de-mechanized in Uzbekistan, the fifth largest producing country in 1999 with 7 percent of world output.

[12] Romero, "Spoonfuls."

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Shattered Rails, Ruined Credit: Financial Fragility and Railroad Operations in the Great Depression

Modern macroeconomics developed as a response to the cataclysmic events of the Great Depression.[1] Although six decades have passed since its conclusion, many basic questions about the Depression remain unresolved. There is no single explanation that can fully account for its onset, depth, and persistence. Therefore, empirical researchers have tended to follow an eclectic approach, combining elements from several theories. This dissertation emphasizes one leading theory: Bernanke's "credit" view, which is based on Fisher's debt– deflation hypothesis.[2] Essentially, the idea is that financial-state variables, such as a firm's balance-sheet position, exert significant influences on decisions with respect to real variables. When incomes decline, financially fragile agents are expected to make deeper cuts in employment, purchases, investments, etc. than are their healthier counterparts. This notion is supported by tests using recent data, but there are few studies using data from the Depression era itself. Those that do exist lend support to the theory, but exclude 1929– 1933 due to lack of data.

The railroads are an ideal laboratory for tests of the financial fragility view. They composed 6.1 percent of GDP in 1929; they suffered severe financial distress, leading to the bankruptcy of one-third of the system by 1938; many railroad analysts believed that financial weakness caused cuts in real variables that would have otherwise been avoided; and detailed firm-level data are available.

The dissertation is structured as follows: Chapter 1 asks how the railroads came to be financially fragile on the eve of the Depression. I use insights from corporate-finance theory to explain the historical evolution of debts and fixed charges since the advent of railways circa 1830.

Chapter 2 is the empirical core of the thesis. Using a flexible accelerator model, it derives a benchmark for maintenance behavior in the absence of finance constraints. Do financially

[1] This dissertation was completed in 2000 at Columbia University under the supervision of Charles Calomiris.

[2] Bernanke, "Non-Monetary Effects"; and Fisher, "Debt-Deflation Theory."

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constrained firms exhibit significant deviations from benchmark values? What are the effects of bankruptcy and government loans? I address these questions using a new panel dataset.

Chapter 3 develops a complete, forward-looking model of the railroad firm, which improves on the flexible accelerator framework in certain respects. A preliminary empirical test is conducted.

CHAPTER ONE

Did railroads borrow excessively prior to the Depression? If so, why? An exploration of these questions is valuable for three reasons: it sheds light on an important aspect of the historical development of the railroads; it yields new insights into corporate governance and financing opportunities and the interplay between them; and it clarifies whether the level of debt in the 1930s is predetermined or endogenous. The latter point is crucial for the empirical analysis in subsequent chapters.

Was 1929 the first instance of railroad financial crisis? Certainly not. Following the years 1857, 1873, and 1893, many railroads failed. The main cause was overexpansion of physical facilities, financed mostly by debt. The combination of highly leveraged capital structures and highly procyclical revenues made the railroads especially vulnerable to downturns. If so, why did railroads emphasize debt finance over equity finance? Why was this policy continued into the twentieth century, despite repeated demonstrations of its disastrous consequences?

In the early stages of their financial history (1830–circa 1885), railroads became established as reliable bond issuers, thanks to the fixed and tangible nature of their properties. They were free of the "asset substitution problem"—the potential for managers to shift funds towards new, risky ventures unrelated to the core business. This characteristic also helped to attract foreign investors, who preferred to avoid involvement in matters of management. Furthermore, railroad promoters sought to raise cash while keeping control of the firm within a small circle of associates. They did not wish to share windfalls with outside shareholders. Investors had other concerns: Many managers transferred profits or assets out of the firm for their own benefit, a practice known as "tunneling." To protect themselves, potential investors began to insist on fixed-income securities. Another factor was the small size of most U.S. banks, which prevented them from serving large firms effectively.

Around 1885 new factors emerged that made stock financing difficult. The reorganizations of the 1890s, led by private bankers, involved greater restrictions on managers along with new financial instruments. These had the unintended effect of discouraging stock issues by reorganized firms. Institutional investors emerged as major players in the railroad field. In the early 1900s a number of states (including New York) imposed legal restrictions on their holdings; railroad stocks were left off the "legal list." Finally, railroads were subjected to punitive rate regulation. Beginning in 1907 they were denied rate increases in the face of spiraling wages and prices. Eventually, this led to low profits and dividends, poor credit, underinvestment and bankruptcy. These events unfolded over the period from 1910 to 1917, as industrial stocks and war finance emerged as formidable competitors for investors' funds.

CHAPTER TWO

Railroads lost just over half of their revenues over the 1929–1933 period (–50.7 percent). Over the same period, they reduced employment less than proportionately (–41.5 percent), maintenance of equipment proportionately (–50.2 percent), and maintenance of way (track) and structures more than proportionately (–62.3 percent). These cuts were strongly interrelated and had significant macroeconomic effects. Did financial fragility magnify these

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reductions? Contemporary analysts pointed to the widespread practice of cutting maintenance "excessively" whenever revenues declined. It is difficult to explain such behavior without appealing to credit-market imperfections. We expect, therefore, that the greater a firm's burden of debt, the greater its difficulties in obtaining credit and the greater its inclination to cut maintenance and employment.

The basic methodology is as follows: First, I model maintenance and employment decisions in an environment without financial frictions. Then, using the model as a benchmark, I ask how a firm's degree of leverage, bankruptcy status, and size condition the responses of employment and maintenance expenditures to changes in operating revenues. These responses may be asymmetric, varying across states of the business cycle: We expect credit constraints to bind more during the worst years of the depression. The handout includes a definition of the "effects" to be computed from the estimated coefficients.

What are the results? As expected, leverage has a negative effect on *Maintenance of Way*, which is stronger during the worst years of the depression. It is significant in small firms only. Leverage affects *Maintenance of Equipment* in a negative and asymmetric way, again for small firms only. Weaker results obtain for the leverage-on-maintenance effect in large firms; we cannot reject symmetry across states of the business cycle. For the leverage-on-employment effect, the reverse is true—large firms, but not small firms, display a significant asymmetry. Small firms show a weaker, barely significant effect during the worst depression years.

The pattern of leverage effects is best understood by noting that in general, small firms tend to face tighter credit constraints than do large firms. On the other hand, managers of small firms have closer relationships with employees; this motivates a greater degree of labor hoarding.

Turning to bankruptcy effects, we see that they are strikingly large in magnitude, and significant in large firms only. For two of the three dependent variables, we can reject the null of symmetry. We see that large firms, placed in bankruptcy, displayed much faster growth in maintenance and employment than their nonbankrupt counterparts. This is consistent with economies of scale in bankruptcy costs. Furthermore, I find that government loans had no effect on maintenance and employment. They only prolonged states of financial distress, while delaying inevitable bankruptcies.

CHAPTER THREE

Chapter 2 assumes that leverage exerts a negative effect on maintenance expenditures. Here, I derive this relationship formally within a forward-looking model of the railroad maintenance decision. The main features of the model are:

(i) Shareholders derive utility from dividends. They seek to maximize the present discounted value of a stream of expected utilities.
(ii) Railroad service is produced using two types of capital, way/structures and equipment, plus freely variable factors.
(iii) Capital depreciates endogenously, according to the degree of utilization. Maintenance activities build up the effective capital stock, thus counteracting the effects of depreciation.
(iv) Maintenance is subject to a convex adjustment cost.
(v) The firm may borrow and lend freely, subject to an external finance premium that is increasing in leverage.
(vi) All after-interest profits are distributed as dividends;
(vii) Demand, productivity, and the economy-wide (zero leverage) interest rate are subject to exogenous shocks.

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With some assumptions, the model yields linear solutions for the log-differences of maintenance and utilization. Some of the results are (ceteris paribus):

(i) Higher leverage has the same effect as an increase in the economy-wide interest rate: Both imply that the firm discounts the future more, and will therefore shift profits to the present via cuts in maintenance and increases in utilization;
(ii) Utilization varies directly with current revenues, but maintenance does not;
(iii) If revenues are expected to be higher next period, the firm increases current maintenance and reduces current utilization.

Because the capital-stock variables are not observable, I devise a way to replace them with observables.[3] I then estimate a simplified version of the model, using maintenance-of-way data from the railroad sample. The results are not definitive. This suggests that there is room to refine to the model, which I hope to do in future work.

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[3] The method is described in a handout used at the presentation. Please see the complete dissertation or contact the author for details.

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Real Estate Values and the Location of Economic Activity: A Case Study of the Urban Form of Cleveland, 1915–1980

Urban decay is a term that evokes images of abandoned buildings, boarded-up storefronts, and trash-filled vacant lots.[1] Although urban decay has affected cities throughout the United States, over the past three decades many Americans have come to associate the term with cities of the upper Midwest. Despite the severity of the decay experienced by the "Rust Belt" cities, economists have not studied the changes in these cities' urban form. Specifically, there have been no attempts to quantify the impact of the decay. In my dissertation I take a step towards closing this gap in the economics literature by studying urban decay in the quintessential Rust Belt city—Cleveland, Ohio.

My analysis of Cleveland's urban form targets two questions. First, when did urban decay begin to afflict Cleveland? Second, why did urban decay take root in the city? Although the first question may be answered empirically without any knowledge of Cleveland's rich history, the second question must be addressed through the lens of economic history. Therefore, I begin my dissertation with a brief history of Cleveland. Having provided the appropriate historical background in chapter 2, I present the core of my empirical analysis in chapters 3 and 4.

Chapter 2, entitled "Nearly Great: A Brief History of Cleveland, Ohio," contains a whirlwind tour of Cleveland's history. Using the city's founding by Moses Cleaveland in

[1] I completed this dissertation in 2001 at Vanderbilt University under the direction of Jeremy Atack. Robert Margo and William Collins also provided me with invaluable support and guidance.

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1796 as a point of departure, I start the chapter by tracing Cleveland's 100-year transformation from a small commercial town to an industrial metropolis. The city's transformation in the nineteenth century is noteworthy, however my empirical analysis focuses on Cleveland in the twentieth century. Therefore, I devote the majority of chapter 2 to a discussion of Cleveland's twentieth-century history. My discussion focuses on three themes in Cleveland's history: changes to the city's infrastructure, changes to the city's demographic profile, and changes to the city's economy.

I document Cleveland's decline in chapter 2, but a written history cannot provide a quantitative measure of urban decay. Although an exact measure of urban decay would be desirable, there is no way to perform such a measurement. Therefore, I use land values as a tool to measure urban decay. Economists have modeled urban form using land values for over 150 years, but a formal mathematical model was developed only recently. In chapter 3, entitled "From the Sixth City to the Mistake by the Lake: Land Values in Cleveland, 1915–1980," I present this model, the monocentric city model.[2] One of the relationships developed in the monocentric model is the bid–rent function. The bid rent function posits a negative relationship between the price of a parcel of land and that parcel's distance from the center of the city.

In order to be able to estimate a series of bid–rent functions for Cleveland, I needed a data set containing land values from the city of Cleveland.[3] Therefore, I collected assessed land value data from the Cuyahoga County Archives for the years 1915, 1922, 1930, 1940, 1950, 1960, 1970, and 1980.[4] I used the data to estimate several different specifications of the bid–rent function. The results from all of the specifications point to the same conclusions: The value of land at the center of Cleveland fell dramatically between 1915 and 1980, and the impact of the automobile on transportation costs led to substantial suburbanization in Cleveland by 1930.

Ultimately, the specifications of the monocentric model I estimated for chapter 3 share a common characteristic—each specification only accounts for a parcel of land's distance from the city center. Direction is not taken into consideration. Thus, every parcel of land located "x" miles from the city center is predicted to have the same value. Although this is a necessary assumption to permit the derivation of the monocentric model, it is also very restrictive. Direction matters.

Being able to account for directional factors is particularly important for the questions I address in my dissertation, for in order to examine the timing and the causes of urban decay in Cleveland I needed to know where urban decay had taken root in the city. Urban economists have yet to develop a comprehensive theoretical model that incorporates direction-specific factors, but they have made important modifications to the monocentric model. In chapter 4, "Decaying at the Core: Urban Decay in Cleveland," I employ a modified version of the monocentric model created by Dan McMillen in 1996.[5] McMillen's model, the spatial-expansion model, allows a parcel of land to be precisely located in an urban area. Specifically, it accounts for how far north or south and east or west a parcel is from the city center.

The results I obtained from estimating the bid–rent functions from the spatial expansion model confirm my results from the estimation of the monocentric model's standard bid–rent functions. The value of land in downtown Cleveland fell precipitously between 1915 and

[2] Alonso, Mills, and Muth developed the monocentric model in the 1960s.

[3] Urban economists and urban economic historians have, traditionally, focused their empirical explorations of urban form on the city of Chicago. The reason for this is simple. Chicago is the only city for which twentieth-century urban land-value data are readily available. It is my hope that the land-value data I collected from the Cuyahoga County Archives will be of use to other economists, thus I will gladly share the data upon request.

[4] For a complete description of the data-collection procedures I employed, please consult my dissertation.

[5] McMillen, Daniel, "One Hundred and Fifty Years."

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1980, and substantial suburbanization had taken place by 1930. However, the results from the estimation of the spatial-expansion model also allowed me to examine urban decay on a neighborhood-by-neighborhood basis. Two important findings emerge from my results.

First, my results challenge the conventional wisdom in Cleveland regarding the relationship between demographic change and land values. Robert Whipple Green, Cleveland's preeminent expert on land values once asserted:[6]

> An enormous increase in the number of Negroes in a particular section of the city with their influence on property values is always reflected sooner or later in mortgage finance.

My results suggest that Mr. Green's statement is incorrect. Three of Cleveland's East Side neighborhoods experienced dramatic demographic change in the period from 1915 to 1980. The Cedar-Central neighborhood was predominantly white in 1920, but by 1930 a majority of its residents were African-American. Similarly, in the 1950s, the neighborhoods of Hough and Glenville went from being predominantly white to nearly entirely black. The core census tracts in Hough were less than 10 percent black in 1950, but by 1960 over 85 percent of the residents living in Hough were African-American.[7] Despite the tremendous demographic changes experienced by these neighborhoods, there is very little evidence to suggest that the presence of African-Americans led to a reduction in land values. The aggregate value of land in Cedar-Central fell by a third in the 1930s, but this was a much smaller percentage decrease than the decrease experienced by white neighborhoods elsewhere in the city. In Hough and Glenville the aggregate assessed value of land declined by nearly 25 and 45 percent (respectively) between 1950 and 1960. However, the neighborhood of Shaker Square, which remained a white neighborhood during the 1950s, experienced an even more dramatic decline in aggregate assessed land value. Furthermore, the Glenville and Hough neighborhoods experienced a reversal of fortune in the 1960s: Each neighborhood experienced a 25 to 40 percent increase in aggregate land value. Ultimately, a causal link between demographic change and urban decay does not appear to exist prior to 1970.

My second finding from the regression results in chapter 4 pertains to urban decay in Cleveland after 1970. Between 1970 and 1980 the aggregate assessed value of land fell dramatically in the African-American neighborhoods on the city's East Side. A comparable reduction in land values in the white neighborhoods on the West Side did not take place. Hough, for example, experienced a 66 percent reduction in aggregate land value between 1970 and 1980. During the same decade Glenville's aggregate land value fell by 71 percent. One of the white, West Side neighborhoods I studied—Rocky River/Lorain—actually experienced a 2 percent increase in aggregate assessed land value during the 1970s. While I may reject the idea that black families somehow "caused" urban decay in Cleveland by looking at my results for the period from 1915 to 1970, it is, nonetheless, very clear that black neighborhoods disproportionately suffered the effects of urban decay in the 1970s. This is a phenomenon that I am unable to explain, but my results are similar to those obtained by other authors. In particular, Collins and Margo have found that, relative to the value of homes owned by whites, the value of homes owned by blacks in Midwestern cities fell between 1970 and 1980.[8]

In my dissertation I use land values to quantify the urban decay that has afflicted Cleveland in the twentieth century. I confirm that the timing of the decay of the central city matched closely the rise of the automobile and changes in industrial production technologies. I also tie suburbanization to the reduction in transportation costs afforded by the car.

[6] Miller and Wheeler, "Concise History," p.135.
[7] U.S. Census, various volumes.
[8] Collins and Margo, "Race."

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More importantly, I challenge the conventional wisdom that attributes urban decay in Cleveland's East Side neighborhoods to the demographic transition the neighborhoods experienced earlier in the century. Simply stated, the arrival of African-Americans in Cleveland's East Side does not appear to have been the causal force in the decay of its neighborhoods. Although the East Side of Cleveland did experience severe urban decay during the 1970s, the reasons behind the timing of this decay are unclear. Ultimately, this is a puzzle that will require future research before it will be solved.

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Comments on Grafe, Karakacili, and Meissner

About a month and a half ago, I was delivering a lecture first thing in the morning. Someone walked into the classroom and gave me the news of the collapse of Tower 2 of the World Trade Center. One of the great architectural achievements, it housed the workings of centuries of commercial progress and innovation, and it was a symbol of the financial heart of the world's most global city. As we all know, the next few hours—days— were chaotic, and it was only after the white dust had settled and the fires were smoldering underground that we began trying to make sense of what had happened.

Two themes emerged—the mixed blessings of globalization, which produces both material advances and geopolitical tensions; and the perils of ignorance about cultural diversity, which can cause (and has caused) those tensions to escalate. Now the world over is confused about what steps to take next, and we desperately need a better understanding of the tensions that arise as the unifying forces of the global marketplace merge the spaces occupied by diverse cultures.

As I began to compose a few remarks about the namesake of our prize, it occurred to me that Alexander Gerschenkron has handed us the clearest vision we have of this merger and its outcomes, at least in Europe, which he described as exhibiting "a unified yet graduated pattern." The themes of his life's work revolved around how technological abilities born in one place, transferred either wholly or partially into other settings, created discontinuities and tensions that, when resolved, produced outcomes that were of different character depending on their settings. He said, it is the economic historian's task to identify and comprehend the uniformities without overlooking or destroying the significance of diversity. Simple extrapolations from the past to the future just will not do. Professor Gerschenkron also inveighs against insularity in thinking. "Insularity in thinking," he says, "is not specific to any geographic area," . . . "[but it] is a limitation on comprehension." . . . "The quality of our under-

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standing of current problems depends . . . on the broadness of our frame of reference."[1]

The dissertations of our finalists contain the elements of what he is describing. Each contains a global force that produces tensions, and in each the tensions have profound long-lasting consequences. Since Professor Gerschenkron's time it has become more customary to try to capture divergent behaviors using the concept of institutions. In fact, all of the dissertations submitted for this prize had this quality. The periods of study, the questions, and the methods of the three finalists could not be more different, but they are unified by attributes that we cannot deny are rooted in Professor Gerschenkron's influences.

Regina Grafe, to set her work apart, begins it with a criticism—that much of the literature on early modern Europe is shaped too much by the rise and fall of the nation-state at a time when nations were only loosely integrated geographically and when experiences between regions differed significantly. Her insistence that economic historians do more careful analysis at the regional level echoes a more general movement toward greater emphasis in regional divergence and interaction as central to the processes of modernization, such as in recent work on proto-industrialization.

Dr. Grafe focuses on the Cantabrian coast of northern Spain. The economic history of Cantabria follows a path divergent from the nation seated in Castile. As the Spanish empire fell into decline in the late sixteenth century, Cantabria freed itself from the institutional ties that bound it to the Meseta Central, and formed independent trading ties with the British economy.

The dissertation is divided conceptually into two main parts—the period before the sixteenth-century Spanish crisis, and the period after 1630. In the first, the Cantabrian coast played a central, but not autonomous, role in the rise of Spain to wealth and power after the American conquest. Spanish wealth and military might was fed by remittances of American silver. One of the many avenues by which those riches flowed through the Spanish economy into the rest of Europe was through the Spanish wool trade to the north.

The merchant guilds of two rival cities sought to gain control of this lucrative trade— the Castilian city of Burgos, in the interior, and the Basque coastal city of Bilbao, across the rugged Cantabrian mountain range. Control of the trade depended on formal rights granted by the Spanish Crown. In the sixteenth century preferential treatment went to the Merchant Guild of the City of Burgos, which received a royal charter granting it a monopoly on the wool trade with northern European ports. The Merchant Guild of Bilbao fought hard to obtain a comparable legal foothold in the trade, but without success. The coastal region was thus largely limited to providing shipping services to Burgalese merchants. There were attempts by the Cantabrian merchants to organize and appropriate some of the rents earned in Burgos. But the northern coast was rich with suitable ports, and a prisoners' dilemma prevented any effective cooperation.

These trade patterns and arrangements persisted until about 1570, when diminished silver remittances produced a fiscal crisis. As it did, the northern wool trade collapsed and did not recover until about 1630. Burgos was no longer in control, and the City of Bilbao came into prominence as a regional commercial center.

Dr. Grafe has gleaned precious information from the *libros de averías* (the port books) of Bilbao to show a significant change in the composition of traded goods after 1630. Exports remained concentrated in wool and iron, but imports were now dominated by the fish trade. Further, the principal trading partner was now England instead of Flanders, and the sight of English merchants residing in Bilbao became commonplace.

The importance of this trade is surprising, given that scholars looking at English trade records had found little evidence of the volume of trade implied in the Bilbao port books. To investigate the discrepancy, Dr. Grafe links customs records of entry and exit of vessels in English and Spanish ports and finds a regular pattern by which English ships arrived in Bilbao laden with North American cod or haddock, which afterward departed for the Eng-

[1] Gerschenkron, *Economic Backwardness*, p. 6.

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lish West Country loaded with Spanish iron or wool.

One reason English records have not uncovered this trade is that, on the outbound journey from England, ships typically did not register, being either empty or loaded with duty-free salt. When Bilbao records document an English ship's arrival from a North American port followed by its departure to England, the English records often failed to report the stopover in Spain. Ship captains evidently misrepresented their voyages in England to avoid paying duties, but not in Spain where only a nominal customs fee was due. What emerges from Dr. Grafe's detective work is a specific example of the North Atlantic triangular trade —one which until now has gone unnoticed.

The story she tells is fascinating and highly readable. My main objection to the work as it stands is that it depends crucially on institutional arguments that are not thoroughly developed. For that analysis, she has put considerable effort into collecting and examining commercial contracts and legal disputes, but her analysis of them is not as systematic, or as formal, or as statistical, as it could have been. I would encourage her to strengthen that element of the work.

Eona Karakacili, in her dissertation, aims to bring new light to an old question: How did labor productivity and material well-being change before and after the Black Death?

To do this, she has compiled important new data using the manorial account rolls from the Ramsey Estate records. As one might imagine, these texts are not easy to use—before the days of ledgers and balance sheets, they were written in terse Latin prose using Roman, rather than Arabic, numerals. One can only imagine the tedium and care required to extract the numbers embedded in these dusty manuscripts. Therefore, to keep the work manageable, she selected nine manors which seemed likely to present the least problems of interpretation.

She used these accounts to compile information about labor inputs and outputs in the demesne in order to assess labor productivity in arable production in England from 1280 to 1450. And despite scholars' awareness of their existence and potential, this is apparently the first attempt to use manorial account rolls to estimate labor productivity.

The account rolls give detailed records of labor rents, wage labor, and the output produced in demesne fields, which were administered under the open-field system. Her aim is to estimate productivity in food production, in which she includes grains and legumes. Animal production was left out because the records are less complete, which she argues is not too damaging because pastoral products were a small portion of the diet. The base estimates are calculated as arable output per worker per fulltime day.

Most of the problems that had to be resolved involved difficulties of measuring the labor input. The accounts recorded the number of "works" owed by each tenant each year and the expenditure on each type of work (or task) performed each year. But how much time the tenant had to spend to complete a "work," or task, had to be reconstructed. Describing how she solved this problem took up a good portion of the thesis and made for some very intricate reading. It also required her to reconstruct the customary practices of the labor-rent system, as practiced on the Ramsey Estate, which she admits was not a trivial endeavor.

Her findings are sharply revisionist. They radically challenge two generally held views about the path of economic growth in the western world over the last millennium.

The main challenge is to the conventional Malthusian story of the Black Death. Before the epidemic, Europe was in a crisis of overpopulation. Population estimates are uncertain, but the story relies on a sudden drop in population such that, as the story goes, life after the Black Death was better than life before, if one had survived.

Dr. Karakacili's findings contradict the conventional story, which tell of a rise in output per person after the epidemic. In her estimates, average labor productivity did not rise after the Black Death, it fell. Furthermore, looking at the small cross-section of the manors she has, labor productivity fell most in those manors that experienced the greater population losses. So the Ricardian relationship between land and labor productivity, for some reason, did not hold.

The second challenge regards the common view that living standards during the late

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Middle Ages were meager, and that Europe experienced a marked increase in labor productivity sometime in the early modern period. But in her estimates, average consumption levels do not suggest substandard nutritional levels. In fact, quite astoundingly, she finds pre–Black Death labor productivity in agriculture to be comparable to estimates found by Greg Clark for early-nineteenth-century England, considerably higher than his estimates for the fourteenth century.

The source of this discrepancy could turn on a number of factors. One is the assumed number of days per year that a fourteenth-century peasant worked. Most other estimates are calculated on a basis of output per worker-year whereas Dr. Karakacili's base calculations are output per fulltime worker-day. Comparisons obviously will be very sensitive to the conversion factor used to make the time periods consistent. From my reading of the literature, one problem seems to be that we do not know much about how intensively or regularly peasants worked. Dr. Karakacili takes a different approach, but if we were to suppose that both sets of estimates—per worker-day and per worker-year—are reasonably accurate, then comparing the two tells us something about customary work *intensity* at that time. This has been a question of enormous interpretive significance in the literature but, until now, largely a matter of conjecture.

As a matter of suggestion, much attention had to be given to detail to make the account rolls "speak" to us quantitatively. Dr. Karakacili is careful to describe each problem and how she addressed it, often introducing downward biases to avoid overstating labor productivity. Her empirical method and expository style can be described as "examining the forest tree-by-tree," making very limited use of statistics to summarize findings. I would suggest that a greater use of basic statistical methods would benefit the exposition and act as a check on the manor-by-manor analysis.

Because Dr. Karakacili has aimed her sights at views deeply embedded with tradition, she will expect, of course, that her analysis will be subjected to some intense scrutiny. Yet I sense that Dr. Karakacili has a flare for controversy, as does some of the other scholars working in this area; and that should make for some highly entertaining reading in next several years. I believe, given the richness and provocativeness of the material she has brought forth, it will also be very illuminating.

Chris Michael Meissner's dissertation is organized into three self-contained essays, all motivated by a common purpose to demonstrate the influence of network effects and path dependence in the institutional evolution of the gold standard.

The overall vision of the work shows strong influence from Douglass North and Barry Eichengreen. Each essay is motivated, first, by an important question, followed by a formal model and a carefully formulated empirical test of the model using a panel of 19 or more countries, depending on the exercise. The driving assumption in each case is that countries reap transaction-cost advantages by choosing currency regimes similar to those countries with which they trade most frequently. Transaction costs introduce network effects into the choice of currency regimes related to the rising globalization of trade and the rising centrality of London's financial center.

Network effects are operationalized by looking at changes in the relative importance of trade inside and outside each country's currency bloc. In each essay, he conducts tests to identify those network effects and exhaustively subjects those tests to variations in parameterization and estimation procedure to ensure robustness. The evidence he finds for the presence of network effects in the choice of currency regimes is both strong and robust.

He sheds light on two main questions: Why was bimetallism as a monetary institution abandoned? And what explains the world movement from an array of heterogeneous currency regimes to a near uniform set of international rules under the gold standard?

First, why did France never resume bimetallism, even though French monetary authorities vowed they would? Previous studies have assessed the effects on France of Germany's defection, to which French officials were reacting, and found that Germany's defection cannot not explain France's failure to resume silver convertibility. Dr. Meissner emphasizes

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that it was the cumulative effect of a series of departures to the gold standard—Germany, first, followed by the Netherlands, and the Scandinavian countries—that explains France's inability to resume silver convertibility.

Ironically, France's defensive action against the German departure may have triggered the defection of other countries. If so, this may be an example in which an action based on an incorrect subjective model, à la North, caused a monetary institution that had existed for centuries to disappear.

He comes at the second question with two angles. First, he asks: How did the coordination of monetary institutions affect the globalization of trade? Using a gravity approach, he examines pairwise trade between the countries in his sample and finds that membership in like currency regimes produced a significant increase in the bilateral trade of the countries sharing the regime. Second, he asks: Why did some countries adopt the gold standard early and others late? Using a hazard model, he examines the average behavior of his set of countries and finds a very significant effect, both statistically and economically. A one-standard-deviation change in the size of a currency network had an effect of several years on the expected date of transition to the gold standard.

He concludes that the high transaction costs of exchange risk erected barriers to trade between countries that were lowered whenever countries adopted similar currency regimes. The benefits of taking such an action were related to the size of the trading network within a currency bloc. His empirical findings show that the benefits of institutional coordination were not only large, they were compounded by the number of countries that coordinated.

These are not questions that have not been looked at before. Dr. Meissner's contribution to this well-trodden area of research is his able cross-country empirical analysis that incorporates the influences of institutional path-dependence and network effects into the formal logic of his models. The sophistication of technique, which he brings to bear in the analysis, produces strong results that both support his conclusions and clarify some nagging questions that have been lying around in the literature.

My main criticism is that he tends to understate the value of previous authors' findings when they are in contradiction with his own. Without going into too much detail about the reasons I say this, let me just say that I would have preferred to see him attempt to reconcile his findings with, and interpret his findings in the context of, supporting and competing evidence in the existing literature. In particular, the value of other evidence should be assessed in conjunction with his own.

Finally, I would suggest that a cross-country analysis of this nature is useful but probably not sufficient for understanding the process. If institutional change was path-dependent, countries could feasibly have diverged along different institutional paths. But also originally dissimilar countries might have converged onto like institutional forms yet have followed different paths to get there. The outlying countries in his sample—Argentina, Indonesia, or the Philippines, for example—may have had different reasons than the core countries for adopting the gold standard. This should imply that more analysis of outlier behavior is also warranted. His approach contains an implicit presumption of a uniform decision criterion. The assumption of uniformity adds tractability and power to the analysis, but the weaknesses of that assumption should also be kept in mind.

To conclude, I want to extend my personal congratulations to all the finalists for their important contributions to our diverse profession.

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Tractors, Trains, and Trolleys: Comments on Grove, Schiffman, and Smith

Before discussing the three finalists for the Allan Nevins Prize, I want to acknowledge the other submissions. All were exceptional, all deserve recognition, and the selection was difficult. The range of topics was impressive, if the geographic coverage was not. All the submissions dealt with the United States even though North America, for purposes of this competition, includes Canada, Mexico and Central America. I was briefly excited when I received a package from Canada, but even this thesis used the U.S. manuscript censuses. Despite the lack of geographic coverage, the range of topics was broad: financial-market integration in the late nineteenth century, the corporate form and the laying of water pipe in Philadelphia in the early nineteenth century, economic influences on U.S. foreign policy in the early twentieth century, federal policy and the gender gap after 1945, immigrant assimilation in the mid-nineteenth century, retirement among Civil War veterans, and banking in the Midwest. If the health of a field is judged by the breadth of issues addressed, economic history is hearty indeed.

I did my best to be eclectic in my tastes and I believe I succeeded. Before I began working through the stack of paper, I considered the titles and held out little hope for some of them, but two of these emerged as finalists. Thus I am satisfied that my topic priors did not unfairly influence the final selections. Other priors were less easily set aside. To my way of thinking, good economic history is built on three pillars. First, it should be economic. This may sound redundant, but a predecessor in this task remarked that she could not abide story telling because story tellers tend to obfuscate or lose track of their own hypotheses in the prose.[1] If the writer cannot keep track of her hypothesis, how can the reader? Good economics poses a refutable hypothesis and marshals evidence bearing directly on it and does so in as concise a manner as possible. A couple theses failed on this metric. Second, it should be historical. This, too, sounds redundant, but one difference between economics and economic history is an appreciation of the past, an explicit recognition of the rich institutional and social milieu in which events take place. Choices are not made in spatial and temporal vacuums. A couple dissertations were technical marvels, but they paid lip service to the literature and showed little concern for institutional details. Third, the data—preferably archival, but of whatever sort—must be treated respectfully. While original data sets require excavation, not every one needs to be pounded into econometric dust.

The finalists shared these three traits. They all gathered data and information from a host of sources, everything from published U.S. Department of Agriculture (USDA) field studies to local land plat books and tax records. They demonstrate great care and respect for the data and how it is manipulated. They all make use of explicit economic theory. They all show that the twentieth century is history in that all three stories effectively begin in the 1920s or 1930s. And, finally, they all respect economic history as a distinct subdiscipline in that the authors are conversant in the literature and accept that there are many useful methodologies and perspectives.

Wayne Grove's dissertation, "The Economics of Cotton Harvest Mechanization in the United States, 1920–1970," focuses on the adoption and diffusion of the mechanical cotton harvester in the American South. In the 1920s the cotton belt employed two distinct regional labor arrangements. Producers in the Southeast relied on tenant farmers and sharecroppers; producers in the Southwest relied on day laborers. Grove notes that this difference set the stage for the different regional development trajectories. Indeed, his thesis has broad implications for the interpretation of post–Second World War history. It sheds light on the tumultuous events in southern labor markets, especially on the "push-or-pull" debate over black out-migration in the 1950s and 1960s, on differential regional

[1] Romer, "Discussion," p. 507.

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patterns of technological adoption, and the social consequences of government agricultural support programs.

According to Grove, previous studies of the diffusion of cotton mechanization have relied on USDA statistics on daily or annual wages of cotton pickers, which are incomplete and misrepresent actual harvesting costs. Grove takes us through two exercises: the first develops new estimates of regional hand-picking costs; the second, new estimates of machine-picking costs. Although we may quibble with an assumption or two, he shows that in 1950 total labor costs in the Southeast were about 20 percent lower than in the Southwest or Mississippi Delta. By the mid-1960s, southeastern and delta wage rates converged, but producers in the Southwest still paid about 13 percent more. After accounting for utilization rates, risk costs, field losses and grade reductions due to machine harvesting, weed infiltration, and rigidities in ginning procedures, Grove finds that machine picking became the low-cost method earlier than previously believed. Machine costs declined in all regions between 1949 and 1965, but fell unevenly and did not converge. The Southwest retained a large and increasing machine-cost advantage. In the Southeast, rising hand-picking costs were as important an impetus for mechanization as declining machine costs.

The fourth, and final substantive, chapter of Grove's thesis is the most provocative. Delayed southern mechanization was not the result of an entrenched system of racially motivated share tenancy. Alternative regional techniques were rational responses to herbaceous and climatological conditions. Humid conditions in the Southeast encouraged weed growth, which required considerable pre-harvest labor until the development of effective herbicides. In the Southeast, weeds competed with cotton plants for nutrients, moisture, and sunlight. Dry, loose soils in the Southwest were less prone to weed infiltration and better suited to tractor usage. Throughout the early twentieth century, best-practice in the Southeast involved sending workers into the fields with hoes to chop weeds and thin the cotton plants. Up to the early 1940s, pre-harvest labor demands exceeded harvest labor requirements by 20 to 60 percent. Yields increased in the 1950s and farmers responded by adopting mechanical harvesters, yet labor demands remained relatively unchanged because of the weeding problem.

At this point, Grove would have done well to consider two contributory causes of the slow diffusion of the mechanical cotton harvester: the slow diffusion of the tractor and lagging educational attainment in the South. He mentions that mechanical cotton harvesting was, in some measure, dependent on previous adoption of the tractor in that pickers and strippers were often attached to tractors. Alan Olmstead and Paul Rhode find that tractors diffusion in the South was relatively slow.[2] In 1950 only 23 percent of farms in the South Atlantic region and 19 percent of farms in the East South Central region had tractors compared to 39 percent of farms in the West South Central. Factors affecting tractor usage undoubtedly paralleled mechanical harvester adoption. Southern educational attainment was below the national average, and education is a widely cited prerequisite for technological adoption.[3] More educated farmers adopted new technologies sooner, and the southern education gap was not closed until the 1950s. Although it is unfair to expect Grove to discuss everything, this is an opportunity missed to place his story within a broader context.

Professor Grove also would do well to place his findings within the literature on learning and diffusion curves.[4] The adoption of new technologies is notoriously slow. If the new technology represents a radical departure from existing technologies, the potential users' knowledge about it may be limited. But as the technology is adopted gradually, more is learned and learned quickly. One study examines 21 innovations and find that it takes about 15 years for an innovation to reach a 10 percent diffusion rate.[5] In the next 15 years it

[2] Olmstead and Rhode, "Reshaping."
[3] Margo, *Race and Schooling*; Goldin, "Human-Capital Century."
[4] David, "Horndal Effect," Greenwood, "Third Industrial Revolution."
[5] Greenwood, "Third Industrial Revolution."

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reaches 90 percent diffusion. Grove's findings for the mechanical cotton harvester in the Southeast are fully consistent with this type of diffusion process. A further exploration of learning and diffusion effects in cotton harvesters seems warranted because there is a growing sense that the productivity-enhancing effects of new technologies are typically overstated.[6]

Whereas Grove considers the adoption of a new technology, in "Shattered Rails, Ruined Credit: Financial Fragility and Railroad Operations in the Great Depression," Daniel Schiffman considers the maintenance of railroads, an existing, diffused technology. Schiffman's thesis builds on the credit-channel theory of financial influences on cyclical downturns. With imperfect capital markets and asymmetric information, an unexpectedly large macroeconomic shock negatively affects firm balance sheets. Lenders respond by raising the costs of external finance and, in the limiting case, banks ration some borrowers (typically small, informationally opaque firms) out of credit markets altogether. The result is an economic deceleration in which an exogenous reduction in spending further deteriorates firm balance sheets, which further reduces credit availability, which reduces spending and around again in a vicious cycle. Schiffman believes that the history of railroad investment and maintenance expenditures present good examples of this cycle.

The first chapter outlines the economics of railroad finance in the century prior to the Great Depression and builds a solid foundation for the credit-channel hypothesis pursued in Chapters 2 and 3. For a number of legal, regulatory, and institutional reasons, railroads were highly leveraged firms. Heavily burdened with long-term debt on the liability side of the balance sheet, railroads were among a small group of industries (including real estate development and utilities) whose balance sheets were especially vulnerable to a debt-deflation cycle. Indeed, by 1938, about one-third of all railroad mileage was in bankruptcy.

As influential as the credit-channel hypothesis has become in the past decade, its detractors note that most tests use data and techniques unsuited to fundamentally microeconomic hypotheses. One of Schiffman's notable contributions is that he constructs a firm-level data set of railroads between 1929 and 1940 capable of providing a good test of the hypothesis. The data include firm-level balance-sheet information, records of track and equipment-maintenance expenditures, and employment. With these data, Schiffman can perform relatively straight-forward tests of the basic hypotheses: that financially sound railroads were more likely to continue maintenance projects during the Depression than firms with weaker initial balance sheets; and that larger railroads had greater access to outside finance, allowing them to continue scheduled maintenance projects. It also predicts that railroads in reorganization spent more on maintenance projects than troubled firms not in bankruptcy because bankruptcy freed them from some fixed charges. The empirical results show that maintenance of way was highly responsive to leverage and even more so during macroeconomic downturns than otherwise. They also show that the effects are more pronounced among small firms than large.

In the third chapter, Schiffman constructs a formal dynamic-optimization model, which predicts that firms with heavier debt burdens will reduce maintenance more than average firms, all else equal. As with most dynamic-optimization models, an array of simplifying assumptions are made, some eminently reasonable, others less so. Schiffman, for example, assumes a rational-expectations equilibrium in the 1930s, as well as zero long-run inflation. He also assumes away adjustment costs and solves the model by assuming a long-run steady state. I readily admit that I do not know nearly as much about the Great Depression as I might, but I have sat through enough seminars to know that, although the Great Depression was characterized by many things, one of them is *not* that it was a steady state. Nevertheless, the predictions arising from the model are interesting and useful, if not compelling, and seem to be worth the trip.

[6] Gordon, "Does the 'New Economy.'"

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Overall, I found little to criticize in Schiffman's dissertation. The testable hypotheses are expressed clearly, the econometric techniques are appropriate to the data, and the conclusions seem warranted. Rather than nit-pick a tightly constructed piece, I will focus on a couple of issues and potential extensions. First, Schiffman has reaffirmed our collective belief that a carefully constructed data set is an invaluable tool in capable hands. As noted previously, most tests of the credit-channel hypothesis are indirect and ambiguous. Schiffman's success in testing the implications of the hypothesis should prompt others to invest their time and energies in constructing similar firm-level data sets for other industries and for other periods to further test the generality of the theory. Second, it will also be interesting to see how the recent high-tech bust affects the maintenance of the information infrastructure. Will the widespread bankruptcy of small firms and the serious financial troubles faced by giants such as AT&T, Lucent, and Cisco, among others, negatively affect future service as credit-constrained firms allow infrastructure to deteriorate? Third, and more importantly, Schiffman notes that the Reconstruction Finance Corporation (RFC) was counterproductive. An issue worthy of scholarly endeavor is whether a different type of bureaucratic organization with a different mission could have been effective. While the business cycle is down, it is not out and it may be good to know what type of intervention, if any, might be effective in countering a serious deterioration in the balance sheets of a new breed of high-tech firms whose principal tangible assets are infrastructure requiring constant maintenance and regular upgrades.

Despite the influence of scholars such as Alan Pred and Jane Jacobs, few economic historians have placed the city at the center of the study of economic development. Frederic Smith corrects that oversight. In "Real Estate Values and the Location of Economic Activity: A Case Study of the Urban Form in Cleveland, 1915-1980," Smith traces out the rise and decline of Cleveland as a major American urban center.

Cleveland is the quintessential rust-belt city. Its history is a tale of the rise and fall of heavy industry; the rise and fall of a vibrant center city; the rise of the automobile and the fall of public transportation; the rise of an increasingly ghetto-ized, impoverished African-American population; the rise of white flight to the suburbs; the rise of public housing; the fall of a viable tax base; and the rise of urban blight. The city has been long disparaged as the "mistake on the lake," and remembered as the place where a river caught fire. Its ups and downs provide the researcher with much fertile ground and Smith cultivates his garden with care.

Following introductory chapters about Cleveland and urban economics, the third chapter provides a formal model of spatial land pricing, does an exceptional job of deriving the economic ramifications of the technical derivations, and provides some preliminary empirical results. Before discussing those results, however, I should mention that Smith painstakingly collected his data from city plat books and land tax records. He should be applauded for his patience in collecting these data. It was no easy task to wrestle with volumes that measure up to 48 inches across and probably weigh 50 pounds or more.

The information gathered from these sources, unequivocally demonstrates Cleveland's decline as an urban center. Land values in center-city Cleveland fell markedly in the twentieth century. Between 1915 and 1980, the real value of a square foot of land at Public Square fell by 84 to 91 percent. Yes, that is correct. Land surrounding Public Square circa 1980 was worth just 9 percent of what it had been worth 65 years earlier. Smith attributes declining center-city land values to transportation costs. The earliest suburbs, built in the 1910s, were specifically designed to have convenient access, by way of trolley cars, to center city. It was the automobile that fundamentally altered the nature of urban land use and, therefore, land values. Smith argues that emergent pricing differences in the 1930s and again in the 1980s were not coincidental. Pricing changes observed in 1930 are attributed to the spread of the automobile in the 1920s. Changes observed in 1980 followed the completion of Cleveland's freeway network in the 1970s. A complementary factor was the influx of

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African Americans in the 1920s, which reduced the desirability of land close to center city for white homeowners and businessmen. White out-migration also explains the deepening trough in land prices in the 1970s and 1980s.

The fourth chapter is engrossing. Smith notes that, to date, no comprehensive theoretical models have been built to account for directional factors in land pricing, but empirical models have been developed and Smith makes good use of them. He accounts for directional pricing and generates a number of three-dimensional topographic price maps with Public Square at the center. These maps reveal donut-shaped price contours. Center-city is encircled by a band of low-value land, values then rise to about four or five miles from the center, and then fall toward the city's periphery. Using these estimates, Smith constructs predicted land values in several neighborhoods and landmarks, which have a number of interesting features. First, land values at the eastern edge of the city were very high in 1915, which corresponds to the development of the Shaker Heights suburbs just outside Cleveland's city limits. Second, by 1930 the western edge of the city is no longer evident. Third, the 1930 gradient in land prices is steepest to the southeast, which is the direction of the first principally African-American neighborhood. Fourth, it is not until the 1980s that we see an unequivocal tilting of land values from east (mostly black neighborhoods) to west (mostly white). Fifth, and most important, between 1915 and 1970 land values rose in the western, mostly white districts and remained stable in the eastern, black districts. Between 1970 and 1980, however, land values fell everywhere, but declined by 33 percent in the western districts and by an astounding 67 percent in eastern districts.

It is at this point that Smith's dissertation disappoints. He has led us on an interesting journey through Cleveland's history, but offers only a single conjecture for its 1970s experience. He contends that the differential decline in land prices between predominantly white and predominantly black neighborhoods was likely due to differences in the provision of public services. He offers no evidence to support this other than to note that Cleveland's school board was sued in 1976 for not better integrating its schools. After taking us through such a well-constructed exercise, I was disappointed that a better answer was not provided to the 1970s puzzle.

Taken as a whole, however, the dissertation is a success. It makes clear that the cause of the decline in center-city Cleveland was the automobile. The emergence of the automobile as the principal means of transportation reduced the importance of being downtown; the city lost its agglomeration economies and it paid a heavy price. By the 1970s downtown department stores, restaurants, and theaters were abandoned and boarded up despite several public-renewal efforts.

I conclude by offering my thoughts on how this work might be usefully extended. First, I recommend a more detailed investigation of the 1970s. Did the race riots of the late-1960s convince even the least-prejudiced whites that the time to leave had come? Did the riots destroy businesses that were abandoned, which accelerated blight and reduced the city's tax base? A shrinking tax base would then force the city to reduce basic services, which would further erode land values. Thus the provision of city services and land values were jointly determined, depending on each other as well as other social and economic forces. Pursuing this line of inquiry, however, requires that we first establish a marked difference in the provision of city services between eastern and western districts. There must be information on schools (class size, teacher salaries, maintenance), the location and staffing of police precincts and fire stations, the existence of and expenditures at publicly subsidized hospitals, and so forth. If the relative deterioration of land values in the eastern districts was as marked as Smith contends and city services were to blame, we should be able to find evidence in these or other measures.

Second, and more ambitiously, while declining land prices document Cleveland's downward trajectory, they do not capture the human costs of urban blight. Smith notes that urban decline brought a murder rate that tripled between 1965 and 1974. Assault, rape, and rob-

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bery increased markedly, as well. In such environments, life is cheapened and people lose hope. Manifestations of rising human costs include diminished scholarly achievement, rising infant mortality, declining life expectancy, a high incidence of single-parent households, and excessive poverty rates. Perhaps it is time to construct human development indices for America's cities; better yet, construct them on a neighborhood-by-neighborhood basis. Doing so would provide a solid foundation for public intervention. Which brings me to my third and final recommendation, which is to encourage similar studies for other U.S. cities. Detroit, Newark, and Birmingham might teach us as much as Cleveland. Ultimately, Smith's dissertation succeeds not just on its own terms, but in the kinds of issues it raises.

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