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major trunk lines. One important strategy in the competition for these routes was vertical integration. Because of the ineffectiveness of laws requiring interconnection, a company could, by expanding vertically, deny its competitors connections to important markets, thus undercutting its competitiveness. The legal environment thus made a competitive market structure unstable, and under these circumstances the industry moved rapidly down the path to consolidation. It is at least conceivable, however, that an alternative legal environment could have produced a very different market structure. These objections notwithstanding, Nonnenmacher tells a fascinating story with considerable skill and much insight.

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### *Comments on Cowen, Hanley, and Voth*

Cowen, Hanley, and Voth have so ably summarized their work that I can avoid the exercise. Instead I will attempt what my high school teachers dubbed a *commentaire composé*, a reasoned discussion of the implications of their work guarded by a few remarks about the potential weaknesses of their analyses. I should note that I never succeeded in garnering a passing grade in the *commentaire composé* despite having to offer one every fourth week for two years. Still I forge ahead, reviewing the three dissertations in alphabetical order. The topics and periods span some 200 years, a pair of hemisphere, and more countries than I can count on my fingers. Each illustrates a major interest among the members of the EHA: macroeconomics, institutions, and the microeconomics of factor markets. In fact, this batch of dissertations demonstrates that our discipline continues to investigate a broad range of phenomena, with an equally broad range of topics.

David Cowen's dissertation focuses on the choice of exchange rate during the 1920s and early 1930s. My ignorance of macroeconomics has led me to extreme caution in these comments—yet the historian and the microeconomist in me can venture a few thoughts. Given recent efforts at stabilizing currency fluctuations in the European Union as well as between Mexico, Japan and the United States, the topic is of considerable current interest. The first section of the dissertation is devoted to monetary regimes and interest rate differentials in Europe after World War I. It plows terrain that has been subject to nearly as much attention from monetary historians as Verdun received from the Great War's gunners. The second half of the work focuses on Latin American governments' attempt to adjust their exchange rates to aggregate shocks. The analysis there builds on the first part of the thesis by using the United States, France, Britain, and Germany as controls. By and large Cowen concludes that for the well-integrated Western economies changes in monetary regimes had little effects on real variables. For the smaller and more peripheral countries of Latin America, however, floating rates were associated with changes in real variables.

To come to this conclusion Cowen has assembled a remarkably consistent data set over 18 countries for 12 years; anyone trying to collect similar information for even a pair of countries knows what effort that entails. Further, Cowen has delved deeply into the chronology of monetary regimes. Yet he has kept the narrative and the quantitative analysis separate and the concluding sections are extremely terse. One gets the feeling that the linkages between the statistical and chronological sections were left as an exercise to the reader.

One remarkable example of the disconnection between history and data is the omission of the German hyperinflation from the quantitative analysis, yet it would be hard to argue

that the experience of Germany from 1918 to 1923 did not condition the rest of the interwar period. Cowen's implicit assumption is that World War I and the Treaty of Versailles affected interest rate differentials largely as one-time shocks (occurring before the sample period) but that events in the later 1920s and 1930s had few persistent effects. I would have liked to know what that says about the period under consideration. That October 1929 did not create a switch in regime is somewhat surprising, until we consider the fact that the interest rates used by Cowen are 90 day rates. Perhaps using rates on long-term government bonds would have helped—indeed October 29 may have caused short-term disruptions on the price side of commercial paper but afterwards much of the movement must have been on the quantity side. Such an adjustment was possible precisely because of the limited duration of short-term debt: much of what came due after 1929 was simply not renewed. Cowen, however, does not investigate the quantity side of the short-term market. For long-term government debt, the World War I insures that it was present in abundance in all Western countries. The very persistence of that abundance implies that all changes in that market had to be reflected in prices. The mass of long-term debt would thus have magnified price movements allowing history—and exchange-rate regimes—to have detectable effects.

The second half of the dissertation addresses the debate over optimal currency areas through an investigation of the conditions leading countries to adopt or abandon fixed exchange rates with the United States. We learn a good deal about the macroeconomic correlates to fixed or flexible exchange-rate regimes. For instance, aggregate shocks caused monetary regime switches in Latin America. Yet it is not clear how such findings relate to the optimal currency issue. No effort is made to decipher whether or not the regimes chosen actually maximized growth. The fact that countries selected different monetary policies in different circumstances cannot address whether they chose the right policies. To deal with such questions one would have to ascertain what their growth experience would have been had they chosen an alternative regime.

To perhaps belabor the point, monetary regimes would likely differ depending on macroeconomic circumstances whether or not regime choice was the result of a benevolent dictator, or of some political coalition with narrower goals. Given the vast literature on dependency in Latin America, Cowen could have used, and should in the future use, the data to address the question of whether monetary regimes maximized growth in the aggregate or merely increased the wealth of a few individuals. Should it happen that Latin American countries manipulated their currencies in a way unfavorable to foreign interests that would be another important nail in the coffin of dependency theory.

Turning from Cowen to Ann Hanley's research is moving from the impact of monetary policy on growth to the impact of money on growth. Hanley zeroes in on the São Paulo province of Brazil to determine the effects of changes in financial institutions on industrial growth. Careful archival work coupled with the patient combing of newspapers allows Hanley to reconstruct the activities of São Paulo banks in great detail. To her surprise, despite close personal connections between firms and banks, firms could not rely on banks for long-term finance. Rather the banking sector was narrowly focused on short-term credit. Rates of return indicate that banks that were most focused on short-term credit earned the highest profits. The rise of a formal stock market, which could have provided an alternative source of finance, proceeded haltingly at best. In the end, the stock market seemed to be used primarily for local government bonds. Long-term industrial finance then came largely through informal institutions that effectively competed with modern forms of finance. The market therefore rather than the government dictated the expansion of Paulista financial institutions.

Further research is unlikely to reverse these broad findings, yet one would accept them with greater confidence if a few points received more elaboration. Hanley argues that long-

term finance was unprofitable because of lack of information. It is crucial to determine where the lack of information lay. If it was aggregate uncertainty then the market should have been able to price it—provided there were no restrictions on interest rates. If it was uncertainty in titling and other property rights then the state's failure to properly survey the land—as governments did in continental Europe and the United States—was of nontrivial consequence. If credit was restricted because borrowers knew much more about themselves than banks, then it we need to know what it did not pay for the banks to invest in long-term information gathering. I suspect that informal credit intermediaries—which the dissertation alludes to but does not investigate—had much better information than banks. It was these informal intermediaries who blocked the rise of a modern financial sector because they used their informational advantages to out compete modern institutions. To the extent that Hanley wants to exonerate government policy she must document that there were nonbank sources of long-term finance. Where might one find evidence on traditional channels of long-term finance? One should begin with notaries who wrote up the bulk of long-term agreements—including joint stock company charters, loan agreements, and probates. In Roman Law countries economic history begins with notaries not newspapers.

The second broad point that needs to be firmed up is the direct role of government policy. Here Cowen and Hanley could fruitfully exploit some comparative advantages. After 1880 Brazil underwent a set of nonneutral financial experiments that alternatively lifted the market up and dragged it down. This kind of monetary policy created uncertainty. As a result it is likely that some people refused to hold nominal contracts to avoid the uncertainty and preferred keeping their wealth in land. A second important effect is that these experiments created redistribution. Brazilian scholars frequently point to the Encilhamento period as an epiphany for industrial finance because the rapid expansion of the money supply coincided with a stock market boom. In other countries—for example, France—stock market booms based of monetary expansion were followed by crashes during which most of the redistribution occurred. Was this not the case in Brazil? If so then the efficiency of the firm creations of the Encilhamento period should be netted out of those firms that failed to survive the return to orthodoxy. In short—even neglecting tariffs—government policy was unlikely to be neutral either towards finance or towards industry. The burden of proof is not on those who seek to show that institutions matter but rather on those who want to argue that they do not.

These quibbles aside, Hanley's research fits neatly into a vast body of literature on the rise of capital markets started some 40 odd years ago by a scholar at Harvard, whose name not incidentally graces the prize that these dissertations seek to earn. Gershenkron set out a theory of economic development in which the mobilization of capital played a crucial role. He further argued that the mobilization of capital would require new financial enterprise—in his view large and implicitly impersonal banks. His view that banks and impersonal finance go hand in hand has become the backbone of many broad texts of economic history. Yet that view is increasingly coming under attack. Naomi Lamoureaux has found banks that only dealt in personal finance. Meanwhile, the largely bankless Parisian capital markets of the eighteenth century featured bilateral credit agreements between parties who had no prior contact. In both cases the distribution of income was relatively even. In São Paulo the distribution of wealth was quite skewed and Hanley found personal banks that did little long-term lending. The plutocracy that ran São Paulo clearly created banks to alleviate liquidity problems, but it resorted to informal channels for long-term finance. There was no advantage for a coffee baron to place his funds into a bank to make long-term loans. Indeed he might as well make them himself. To the extent that firms remained small enough that they could be founded during cocktail parties, the need for either a stock market or banks remained scant. Railroads did change that but only up to a point. Hanley's

work is further evidence against the argument that banks should perform the same function at all points of the time-space continuum.

From Hanley to Voth we move a century back in time and from the question of what did banks contribute to industrialization to what caused industrialization. In the absence of reliable firm-level data, a generation of nimble experts willing to make mind boggling assumptions has convinced us that Britain's industrialization was unconventional. Its cause was not technical change, nor was it capital accumulation, nor could have been the sudden discovery of the vast endowments of a western frontier because Britain lost the American colonies precisely at the time that the steam engine was perfected. Thus, if we do not want to deny the whole event in the first place we must call on labor to perform the growth miracle. The population of England did grow, but so did per capita output. So growth must have rested on labor intensification. How then might we document that people worked more in 1800 than they had 50 years before? Voth offers a solution to this problem—activity statements from court records—that is imaginative and contains just the right numbers of leaps of faiths to fit nicely with the rest of the literature on the British Industrial Revolution. Further, Voth finds just the right amount of increased labor input to get us a slow but steady growth rate of output.

The use of court records to resolve the Industrial Revolution is sure to raise of few eyebrows. Potential problems of sample selection abound, and given the small sample size one might worry about how representative the data really are. Voth subjects his data to trial by statistical fire and finds little internal evidence of selection bias. Yet this conclusion is in a large part driven by the small size of the sample and the large variances that characterize the timing of many activities. In such a setting statistical tests have little power; in other words, they are unlikely to reject the notion that there are no problems of selection. Yet statistical significance is not always intellectual significance. Take for instance the problem of rising and going to bed. Since witnesses must be awake to witness a crime, the sample is likely to favor early risers and those who burn the midnight oil. Victims of crime on the other hand have no requirement to be awake prior to the critical event (although we do not know how many were awakened by the crime). Statistically it is not possible to distinguish between the rising time of victims and witnesses. The total population is less than 100, but victims do get up later and got to bed earlier than witnesses. The small sample leads to the tentative conclusion that there is sample selection, which is a relief since Londoners seem to have slept some three hours less than I do.

For those who would suggest that these problems might be neatly resolved by increasing the sample size, one should be aware that the examination of 11,000 court cases yielded 153 rising times (93 witnesses and 60 victims). These come from an extensive survey of 18 years of data from the second half of the eighteenth century. Should the rest of the years be collected it would only add another 30,000 or so cases or maybe 400 rising times. Thus more data is available but at what cost?

If court records appear to be an ambiguous means to measure the activities of Londoners during the day, they seem to be much more robust for measuring variation in work over the week and year. And that variation is the most economically significant. Voth documents a dramatic rise in work on Monday—which was the second day of the weekend in Georgian Britain. Little is likely to change his finding on the erosion of the restful-slack Monday. However, if one is going to attribute the Industrial Revolution to a Monday-effect then it would be best to find it outside of London; preferably in the industrial North. After all since London was a declining industrial city in the eighteenth century, intensification could have occurred there as an attempt to offset high wages—an issue that Voth mentions but does not confront.

If we assume that the decline of Saint Monday was a Britain-wide phenomenon then we

must rethink our notions of labor supply both in history and the world today. The liberation or subjugation of idle hours has the potential to rapidly increase output. Yet what are the mechanisms behind this liberation. Current evidence is mixed. Market reforms in Third World countries are associated with rapid increases in labor-intensive activities where the wages of those who participate are often higher than in other sectors of the economy. In Europe and the United States some at least have argued that the last few decades have seen increases in family labor participation without increases in hourly wages. The aggregate evidence for England is scant but point to stagnant or falling real wages. With more detailed evidence it might be possible to pin down whether the sectors experiencing intensification were high- or low-wage sectors. We could then begin to understand the mechanism whereby output rose during the Industrial Revolution.

In closing I would like to thank the three presenters for the opportunity to read their work. Given how much I learned from their research, I expect that we will all be enriched by this generation of economic historians. Its a privilege to welcome new colleagues into the Association.

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