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Economic History and Economic Development: New Economic History in Retrospect and Prospect

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ABSTRACT

I argue in this paper for more interaction between economic history and economic development. Both subfields study economic development; the difference is that economic history focuses on high-wage countries while economic development focuses on low-wage economies. My argument is based on recent research by Robert Allen, Joachim Voth and their colleagues. Voth demonstrated that Western Europe became a high-wage economy in the 14th century, using the European Marriage Pattern stimulated by the effects of the Black Death. This created economic conditions that led eventually to the Industrial Revolution in the 18th century. Allen found that the Industrial Revolution resulted from high wages and low power costs. He showed that the technology of industrialization was adapted to these factor prices and is not profitable in low-wage economies. The cross-over to economic development suggests that demography affects destiny now as in the past, and that lessons from economic history can inform current policy decisions. This argument is framed by a description of the origins of the New Economic History, also known as Cliometrics, and a non-random survey of recent research emphasizing the emerging methodology of the New Economic History.

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New Economic History in Retrospect and Prospect

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The New Economic History was born about 50 years ago. As economics changed after the Second World War, economic history changed as well. The New Economic History started in the 1960s as a part of economic history and has grown to become the dominant strain in economic history today. I survey this progress and think about the future of economic history in three stages. The first stage recalls some of the early days of the New Economic History, its origins and early development. The second stage reflects on the achievements of the New Economic History as shown in recent publications by Robert Allen and Joachim Voth. Taken together, these contributions build on a half-century of research and suggest promising areas for the future. The third stage surveys some other contributions to the New Economic history in a partial and idiosyncratic way and distils implications for the future.

Paul Samuelson arrived at MIT in 1940. Receiving his PhD from Harvard in that year, he was snatched up by MIT when Harvard failed to make him a faculty appointment (Keller and Keller, 2001, pp. 81-82). From this event came both the birth of the MIT economics department, and a revolution of economics itself. Samuelson's PhD thesis, published as *The Foundations of Economic Analysis* (1947), championed the use of mathematics in economics. He was not the first economist to use math, but he showed how math could be systematically employed to reformulate familiar and unfamiliar economic arguments. He was like Adam Smith, organizing various strands of existing economics into a new coherent synthesis.

The MIT economics department started its graduate program after the war. It was constructed like a three-legged stool, resting on required courses in economic theory, econometrics, and economic history. But while the legs of a stable stool are equal, these required courses were not. Economic theory and measurement were in their ascendancy, and economic history needed to find a way to coexist with the new theories and econometrics to survive. As in older economics departments, economic history had been taught before the Samuelsonian revolution, but it had been more like history than what we now think of as economics.

One effect of the change in the focus of economics was to change the main mode of reasoning from inductive to deductive. This meant that papers in economics changed from being primarily narrative to starting with a model. New economics papers progressed from a model to data and then hypothesis tests. Economic historians responded to this change in economics by embracing the new tools of economic theory and measurement in what became known as the New Economic History.

This movement was led by the two recipients of the 1993 Nobel Prize in Economics, Douglass North and Robert Fogel. North was editor of *The Journal of Economic History* with William Parker in the 1960s with the conscious aim of attracting papers using formal economics in their analysis. He gained most fame by stimulating the growth of the New Institutional Economics through his many publications. Fogel burst into this scene with publications first on the social savings of American railroads and then, with Stanley Engerman, on American slavery. These contributions were showcased first at annual meetings of what would come to be called cliometricians held in in the 1960s at Purdue University in the dead of winter.

The New Economic Historians threw their lot in with the econometricians. They turned to the collection of historical data and their use in testing hypotheses about economic activity. In this way, the New Economic History brought itself into the mainstream of economics as it was developing, but it caused a growing problem for economic history as economics departments turned their face toward the new theories championed by Samuelson and Solow.

The economic history paper was central to one of the legs of the three-legged stool supporting the MIT economics department. The paper requirement began soon after the war when most field courses had term papers. It was only a remnant of this pedagogical approach to graduate studies by the beginning of the twenty-first century. The two surviving papers, the remnants of the omnipresent term papers in most courses in the 1950s and 1960s, shared several characteristics. Students had to select a question to answer or a hypothesis to test, drawing on their course work or their general knowledge. They had to answer their question or test their hypothesis with using evidence from empirical data. And they had to write this up in the form of an article for an economics journal. They were, in short, two variants of an assignment in applied economics. In fact they were hard to distinguish at the margin and sometimes overlapped.

The two papers also differed in important respects. The history paper drew from economic history—defined loosely to follow the economics convention of focusing on events a quarter-century or more past—for its questions and hypotheses. The aim was for the students to analyze events in a different institutional setting or with unfamiliar relative prices. Given the scarcity of historical data for many interesting historical questions, particularly those about foreign countries, many different quantitative techniques were used. The econometrics paper by

contrast was focused on the econometric methodology being used and less on the context in which it was used. And the history paper came in the first year of graduate work, while the econometrics paper was a feature of the second year.

I began to teach economic history at MIT in 1965, and I attended the cliometrics conferences at that time. The dominant memory I have of the conferences was the attention to data. An econometrics professor at MIT had remarked to me that when he could not find data for 1800 that he needed for a regression, he used data from 1900 instead. This was not the culture at the cliometrics conferences. Great attention was taken to the collection and interpretation of data, and disagreements were as often concerned with these issues as with the arguments and hypotheses built on the data.

I presented a paper at one of my first cliometrics conferences on the American iron industry, the topic of my thesis. As I recall, I found the ante-bellum iron data hard to reconcile with my hypotheses, and I proposed what I thought was a reasonable revision of the data for future use. The conferees thought this was a terrible idea, and there was a lot of critical commentary demonstrating to me that the worlds of economic historians and econometricians had drifted apart. Bob Fogel came up to me after the session, asking how I could remain so calm under the fire I had just sustained. I remarked that the criticism was directed at my paper, not at me. Bob shook his head and rejected that distinction. We went on from there to become friends who often disagreed with each other.

There was palpable excitement among New Economic Historians during the next two decades. Two well-known and controversial books from that time can help us remember this excitement. *A Monetary History of the United States, 1867-1960* by Milton Friedman and Anna

J. Schwartz appeared in 1963. They offered a new interpretation of fluctuations in the United States for the previous century and promoted the view that changes in the stock of money were the prime determinants of economic activity. Their claims and Friedman's awesome debating skills made this book a *cause célèbre* among economists and economic historians alike. Their data continue to be used, and their point of view is relevant to current debates. Ben Bernanke, Chairman of the Federal Reserve Board, once said to Friedman that he would not repeat the mistakes Friedman claimed the Fed made in the 1930s.

Time on the Cross by Robert W. Fogel and Stanley L. Engerman (1974) appeared a decade later. It too became famous and controversial, albeit more among historians and economic historians than among economists. They offered a new interpretation of American slavery as a more benign institution than previous authors and in which the rate of exploitation of slaves was markedly lower than previously thought. It is interesting that they derived this latter result by assuming that slaves had to pay for their own upbringing. This approach has returned today as college students increasingly have to pay for their own education as public support for state universities has declined. The growth of student debt is analogous to the debts Fogel and Engerman asserted slaves owed to their owners.

A measure of this intellectual enterprise was taken at the annual meeting of the American Economic Association in 1984. The papers presented in this session were published in the annual *Papers and Proceedings of the American Economic Association*, and the whole session was published in *Economic History and the Modern Economist* (1986), edited by William N. Parker. The session consisted of two papers by economic historians and two by Nobel-laureate economists. The economists took it upon themselves to discuss the place of the New Economics in economics as a whole.

Kenneth Arrow concluded his essay by saying, "In an ideal theory, perhaps, the whole influence of the past would be summed up in observations on the present. But such a theory cannot be stated in any complex uncontrolled system, not even for the Earth, as we have seen. It will always be true that practical understanding of the present will require knowledge of the past (Parker, 1986, pp. 19-20).

Robert Solow made essentially the same argument in different words:

> The economist is concerned with making and testing models of the economic world as it now is, or as we think it is. The economic historian can ask whether this or that story rings true when applied in earlier times or other places, and, if not, why not. So the economic historian can use the tools provided by the economist but will need, in addition, the ability to imagine how things might have been before they became as they now are. … It was once suggested—by my kind of economist—that the division of labor is limited by the extent of the market. Perhaps what I have just been doing can be thought of as suggesting that economists extend their market and accept the specialized services that, in a more capacious market, the historians as well as other scholars, can provide. (Parker, 1986, pp. 28-29)

These eminent economists gave good advice. The New Economic History has endeavored to follow it by examining questions drawn from a wide range of places and times, ranging from prehistory to recent events and all around the world. Anywhere there are data or information that can be construed to test hypotheses is fair game.

Three techniques have emerged as particularly useful in these wide-ranging explorations. The first is modern econometrics. New Economic Historians of the first generation used simple econometrics, which were a new way to learn from data in the historical literature. In its new approach to economic history as economics, however, simple econometrics looked like undergraduate econometrics. The use of econometrics was enough to get the first generation employed at good universities, but it was not sufficient for the next generation.

Fortunately, these students had been educated in modern econometrics, and they began to use it in their research. Younger scholars interested in economic history consequently have been able to get jobs at good universities and their articles published in top economics journals. For example, compare my experience at MIT with my younger colleague, Dora Costa. I published largely in economic-history journals and used simple regressions in my work. (I cannot resist noting that my use of even a simple regression about trade in ancient Rome sent ancient historians into a tizzy.) Costa by contrast used cutting-edge econometrics in her work, published regularly in major economic journals and taught econometrics at MIT.

The second technique utilizes the ideas behind event studies to examine the effects of turning points and decisions in economic history. Discontinuities provide information on the structure of economic systems that may not be apparent from their smooth operation in normal times. Legal boundaries provide discontinuities over space, and events ranging from crises to discoveries provide discontinuities over time. These important historical events clarify the structure of economic activity and provide evidence to test preconceived ideas about economic history.

The third useful technique is to examine events over several generations, an opportunity given to economic historians and students of economic development that distinguishes them from some other fields of economics. We can study the effects of demography and education that often are simply held constant in current economic analyses. These two approaches run into each other as we go further back in the past, as we sometimes find the effects of dramatic events in the fortunes of people over several generations. As usual among economists, we distinguish ideal types to think about processes that can be seen as a continuum from another point of view.

The big events of economic history are the Black Death of the fourteenth century, the European discovery of America in the sixteenth century and the Industrial Revolution in the eighteenth century. We keep going back over these dramatic and far-reaching events to learn more about the path from the slow-moving economies before them to the fast-moving ones today. We know more about the most recent of these events, and it has overshadowed studies of the earlier ones. I want to return to the first of them to illustrate how the New Economic History is reshaping our conception of this transition and to illustrate how much we have gained from this collective activity we call the New Economic History.

When I started teaching these events, we saw the Black Death in very simple ways. It was a demographic shock that sharply reduced the supply of labor while leaving the supply of land intact. The result was a dramatic rise in the real wage, chronicled for England by Phelps Brown and Hopkins (1962) and revised and explored further by Clark (2005, 2007). The English data were extended to continental Europe by two less well-known contributions. The first one was the discovery of what Hajnal (1965) called the European Marriage Pattern. This pattern, as I recall teaching it long ago, had three components. The age of female marriage was high, in their twenties; many women did not marry at all; and married women did not automatically join the household of their husbands. According to Hajnal, this contrasted with an Asian marriage pattern where almost all women married at menarche and moved into extended households of their husbands' families. Hajnal observed this pattern in the early modern period, but he offered no clues where it came from.

The second contribution came from Brenner (1976), who argued that the effects of the demographic changes generated by the Black Death were modified by social and political structures. In the West, that is, England, the monarchy was strong and the aristocracy weak.

This left room for workers to take advantage of their relative scarcity and bid up their wages. In the East, vaguely identified as continental Europe, the aristocracy was strong, and it prevented workers from moving to better jobs. This reduced the bargaining power of labor, and wages in the East did not rise after the Black Death. Serfdom decreased in Western Europe and increased in Eastern Europe. Brenner's argument was more controversial than Hajnal's views, and it gave rise to extensive debate—although not to explicit hypotheses testing.

The Brenner debate took place largely outside economics, but it can be seen as an application of North's emphasis on the role of institutions (North, 1990). This view gave rise to the New Institutional Economics, a group of economists and economic historians who emphasize the role of institutions in shaping economic affairs. Brenner's ideas can be rephrased as a hypothesis about the role of institutions in shaping responses to the Black Death. The difference between strong monarchies in the west and strong aristocracies in the east was the key to the treatment of labor in this view.

The New Institutional Economics has spread beyond the bounds of standard economic history. It motivates a new view of the economic history of the Greco-Roman world (Scheidel, Morris and Saller, 1997). The editors of this volume tried to move away from the traditional opposition of primitivists and modernists in the study of ancient history into what they considered a more fruitful approach. They found inspiration in North's work and employed the New Institutional Economics to explain differences among provinces of the Roman Empire, providing insights which other ancient and economic historians have expanded (Temin, 2013).

This welter of seemingly unrelated contributions has now been clarified and reformulated by the New Economic History. Voitländer and Voth (2013) argue that the Black Death gave rise

to the European Marriage Pattern and set in motion a process that led to the Industrial Revolution. This is a large claim, and it leads to a sharp revision of Western economic history. It needs some explanation to be understood.

Voitländer and Voth argue that the scarcity of labor after the Black Death led to a change in agricultural technology. Moving along the wage-rental isoproductivity line, farmers changed from growing crops to tending animals, from arable farming to husbandry. In other words, movement along a smooth production-possibility curve was a sharp change in the underlying technology. Sir Thomas More expressed it most colorfully over a century after the Black Death in his *Utopia* (2012 [1516]): "Your sheep that were wont to be so meek and tame, and so small eaters, now, as I hear say, have become so great devourers and so wild, that they eat up and swallow down the very men themselves. They consume, destroy, and devour whole fields, houses and cities."

The result of this adaptation of agricultural technology changed the role of women in Medieval society. Switching from crops to husbandry reduced the demand for strength to push plows and expanded the scope of work that women could do. The result was a change in the status of women in society that Alesina, Giulano and Nunn (2013) observed at other times and places as well. The reduction in plowing reduced the demand for men's labor and increased it for women's labor. Women's wages rose and their opportunity for work expanded. They delayed marriage, entered service and became more independent. This in turn led to the European Marriage Pattern and the family pattern described by Laslett (1965). It was a massive change in the structure of society, but at the household level analyzed by Hajnal rather than the societal level described by Brenner.

The opportunities open to women delayed their marriage and reduced the rate of population growth. The result was the birth of the high-wage economies of England and a few neighboring countries. Voitländer and Voth test this theory in two ways. They use unpublished data from Broadberry, Campbell and van Leeuwen (2011) to estimate that the share of pastoral production in English agricultural output rose dramatically from 47 to 70 percent between 1270 and 1450. And they show by regressions that the age of first marriage after 1600—when data become available—was dependent on both the share of pastoral production and its increase since the Black Death in English counties. They conclude that the extensive use of pastoral production increased the age of female marriage by more than four years.

The rise in wages as a result of the Black Death was sustained by a shift in marriage patterns that increased the age of women's marriage and reduced the rate of population increase. The adaptation to the initial shock led to a durable rise in people's income. This in turn led to a demand for more meat in their diet, which of course was accommodated by more husbandry. The whole pattern fit together with the Black Death as a shock that shifted households and the economy from one equilibrium to another.

This all fits in with Allen's view of the Industrial Revolution being the result of a high-wage economy. In fact, Voitländer and Voth probably were inspired at least in part by Allen's work. Allen (2009a) argued that the initial innovations of the Industrial Revolution emerged from tinkering by producers to reduce the costs of expensive labor and reap the benefits of cheap power. In response to the awareness from other work by Allen *et al* (2005) that wages were high generally in Western Europe, Allen went to some lengths to show that the marginal gains from these initial innovations were not large enough to be profitable in either France or the Netherlands (Allen, 2009a, 2009b).

Allen (2013) argues in more recent work that wages and energy prices in North America were close enough to the British pattern for policy initiatives like tariffs, education and infrastructure investments to create conditions hospitable to industrialization. This clearly was true of countries in Western Europe that also followed the British pattern once industrial productivity advanced from its initial level. These countries did not have the factor prices to make the initial innovations of the Industrial Revolution profitable, but further development of these innovations rendered them profitable at factor prices close to those in Britain. And, as Allen noted, policy changes helped industrialization along as it spread.

But this was all within the high-wage area described by Voitländer and Voth. They noted that the European Marriage Pattern extended only from the Atlantic to a line from St. Petersburg to Trieste. Other countries in Asia or Africa were low-wage economies subject to Malthusian pressure on wages, and their factor prices were not close to English prices. Small changes in economic policies were not sufficient to make industrialization profitable in India or Egypt. The story that links the Black Death to the Industrial Revolution therefore is also a story why Europe has industrialized most easily in the past two centuries.

This synthesis reveals that these specific papers extend and unify a generation of contributions to the New Economic History. One strand has been to look at real wages in many times and places, finding evidence where none was suspected before. Another strand has extended financial history back to agrarian economies to reveal a very different index of how economies operated. And a third strand has been insights about odd and interesting facets of economic history that seem at first glance to be only isolated curiosities, but which later turn up as parts of arguments about how all of these strands can be woven together.

Three implications emerge from these recent contributions by the New Economic History. First, they rewrite Western history from soon after the end of the Roman Empire to today. Second, they provide a guide to the role of economic history in economics departments. And third, they call out for a change in publication strategy. I consider these implications in turn.

David Landes (1998) began his magisterial economic history of the West from the discovery of America. The expansion of Europe was an important event, but we now know it was hardly the beginning of the high-wage story. High wages in Western Europe could have resulted from the rise in the ratio of land to labor by the opening up of American land. But we now realize that the start of the high-wage economy came from the rise in the ratio of land to labor that resulted centuries earlier from the Black Death. The growth of commerce to the New World was helped by British and Dutch shipping and services, and the resulting prosperity kept wages in London and Amsterdam particularly high. The expansion of Europe is an important part of the story, but not the beginning.

Another part of the development of Western Europe was the invention of the printing press in the interval between the Black Death and the expansion of Europe. Printing clearly was a labor-saving innovation, and it is tempting to see it as the result of high wages. Dittmar (2011) however, argued that the spread of printing was related more to the distance from Mainz, where it was introduced, than to factor prices. In terms of this discussion, Dittmar argued that printing was not a marginal innovation like the spinning jenny, but rather a discontinuous change in costs that spread with knowledge. This can only be true in part, as printing spread for the first century or so only within the areas of the European Marriage Pattern.

This single example reveals a more complex story beneath the outline given here. We have to fill in the blanks to provide a new history that reveals the combination of shocks that produced Western history. And while this story is based on simple economics, it requires some modification of the simple Malthusian story. For the high-wage economies of Western Europe were not simply fluctuations around a pre-existing norm; they were a new equilibrium around which population fluctuated. The Malthusian model needs to be expanded to encompass important changes in production and distribution like those that followed the Black Death. The Industrial Revolution was not the first escape from the dismal conclusion that real wages could not long stay above subsistence.

This is an important story; how does it fit in modern economics departments? I propose that economic history and economic development should both be considered relevant to modern economic growth. The difference is that economic history traditionally directs its attention to the high-wage economies just discussed, while economic development focuses on the low-wage economies outside Europe. These two inquiries are closely related. They both analyze the growth of economies with new technologies, and they both are concerned with the incentives people have to adopt new innovations.

There is now a large gap between the technologies being used in high-wage and low-wage economies which mirror the large gap between real wages in these two types of economies. If we want to bring the low-wage economies to the level of high-wage economies, we have to modify either the technology being used in the high-wage economies or change the factor prices in the low-wage economies. These are two different directions of research and policy, and they are complementary to each other. If the education and employment of women lead to population control, this will lead to higher wages in poor countries that will make modern technology more

appropriate. And if technological innovations like cell phones broaden the factor prices at which they are useful, this too will promote economic development.

Once economic history and economic development are seen as two sides of the same coin, there should be interesting cross fertilization between economic historians and development economists. One interesting factor is the time involved in economic change. The world appears to be moving rapidly today, but the story of Europe now stretches from the fourteenth to the eighteenth. It is an interesting question how an interaction between these two fields might suggest ways to make a faster transition.

This brings us to the third implication of the New Economic History of Europe. We have to change our publication strategy. Voitländer and Voth published their contribution to European history in the *American Economic Review*, while Allen published his views on economic development in the *Journal of Economic History*. The papers are written for their respective journals, and there would be little point in simply reversing their position—should that even be possible. Instead, we need to think how to get the message across to the relevant audiences. How can we get historians to understand that they must start the story of modern Europe from the Black Death? And how can we get economists to understand that they must start analyzing policy interventions with a consideration of factor prices?

I hesitate to suggest how to do this to these established and prolific economic historians, but I do so to illustrate the paradoxical position of the New Economic History. And just as these contributions build on the work of many New Economic Historians, the job of communicating these results to the appropriate audiences probably would be most effective as a group effort.

Voitländer and Voth need to change from presenting a hypothesis test—the hallmark of the New Economic History—to presenting a narrative that historians will appreciate. They need to place their test in a narrative of Western European history that distinguishes the areas that adopted the European Marriage Pattern from those areas that did not. I have suggested some of the writings that should be included in the intellectual background, but the narrative should focus on telling a persasive story of a critical time in European history.

Allen needs to move in the opposite direction, to extract hypothesis tests from his impressive manuscript that can appear in a good economics journal. He might anchor his tests in a theory like that in Acemoglu and Zilibotti (2001) to provide a bridge between economic growth and economic history. He might incorporate his test of the suitability of the spinning jenny (2009b) or the graphs in his recent survey, but the paper must stand as a test of the overall proposition he made in his presidential address (2013). And it needs to have the bells and whistles that current economic articles now sport.

These suggestions of course can be safely ignored. They do however illustrate the paradox of the New Economic History. New economic historians have turned their back on traditional historians and sought their place among economists. This has provided good jobs for many scholars, but the acceptance by economists is still incomplete. We therefore have two challenges ahead of ourselves. The first is to argue that economic development can only be fully understood if we understand the divergent histories of high-wage and low-wage economies. And the other big challenge is to translate our economic findings into historical lessons that historians will want to read. These challenges come from our place between economics and history, and both are important for the future of the New Economic History.

These papers signal the achievements of the New Economic History, but not its breadth. I therefore conclude this paper with a very partial and highly idiosyncratic review of varied contributions to the New Economic History. It should become clear that the list deals mostly with people in and around Cambridge, MA, or that I know personally in other locations.

The first papers deal with the expansion of Europe, but from a different point of view. The Black Death changed Europe, but not at the expense of other people. The expansion of Europe a few centuries later was not as big an event in the economic history of Europe—if you believe the story I have just recounted—but it had repercussions outside Europe that have had lasting effects.

Melissa Dell (2010) investigated the effects of the Spanish silver mines in South America that led to the great European inflation of the sixteenth century. The Potosi and Huancavelica mines that yielded silver and the mercury to refine it were operated by indigenous labor under a *mita* system. Between 1573 and 1812, villages located near the mines in the Andes Mountains were required to provide one-seventh of their adult males as rotating laborers. Dell revealed the effects of this labor system by comparing current conditions in villages under the *mita* with adjacent villages.

Using all three of the techniques listed above, Dell found that the effects of the mita were apparent today, five centuries after the expansion of Europe. She used a "regression discontinuity approach," examining conditions at the edges of the *mita* area. Given the length of time involved and the complex geography, this was not an easy task. Dell exploited both the Spanish preference for workers close to the mines and from the Andean highlands and modern mapping techniques showing altitude for any location. She found that the long-run effect of the

mita reduced household consumption by one-quarter, resulting at these low income levels in significant stunting of children.

This dramatic finding raised an obvious question: how could it be that the costs of the Spanish exploitation could last over several centuries? Dell organized her explanation around haciendas, rural estates with attached labor force reminiscent of medieval manors. The Spaniards discouraged the growth of haciendas in the *mita* area to preserve their unimpeded access to their labor force. Here we see an inversion of the Brenner thesis that local aristocrats oppressed workers after the Black Death by limiting the extent of the labor market; haciendas would have limited the exploitation of workers by the central, Spanish government by limiting its access to the labor market.

The haciendas were a mixed bag. On the one hand, they expanded after the end of the *mita* by coercive activity ranging from using legal rules to physical violence. On the other hand, they built roads connecting the highlands to lowland urban markets. Access to markets was a critical factor in the history of the high-wage economies of early-modern Europe and North America; it appears to have had similar effects in the low-wage economy of South America. It is worth noting that haciendas cannot be the source of future progress. They were abolished in 1969.

Nathan Nunn (2008) looked at more labor-market effects of the expansion of Europe, this time in Africa. The effects of American slavery in the New World have been the subject of myriad research projects. Nunn inverted this question to ask about the effects of the slave trade on Africa. In other words, Nunn did not look at slaves and their descendants, but at the people who escaped this fate. Like Dell, he found persistent deleterious effects.

The Atlantic slave trade ended two centuries ago, but Nunn found that African countries that had more slaves per square mile taken from them have lower per capita GDP today. Like the *mita*, the slave trade is gone, but its effects linger on. Nunn made sure that the direction of causation was from trade to economic development, rather than vice versa, or that some other cause was to blame. One demonstration was that slaves were not taken from previously well-organized areas, but rather the reverse. It was the most organized areas that exported the most slaves.

The explanation for this reversal of fortune is that slaves were obtained for export by villages or states raiding each other. The lure of the profits to be gained from slave exports discouraged the expansion of village federations and the growth of ethnic identities. Suspicion and distrust impeded state formation. It is a truism of current development research that the multitudinous ethnic divisions in Africa impede economic growth. Nunn provides at least a partial explanation why there are so many ethnicities in Africa.

This idea can be generalized. Dasgupta (2007) argued that trust is the basis of economic prosperity. He devoted a short summary of economics to this single proposition. Revealing for this discussion, Dasgupta started his discussion by contrasting the conditions of a young girl in the United States with one in Ethiopia, one of Nunn's observations. This rather esoteric exploration into the durable effects of a defunct activity has led directly into the center of economics.

I stated earlier that the New Economic History focused on high-wage economies, yet this survey started with two important papers about low-wage economies. They illustrate how economic history and economic development work together to construct full pictures of poor

economies in the world that can lead to productive economic policies. These papers are significant contributions to both economic history and economic development.

Turning now to contributions to American economic history, I start with *Time on the Cross*, mentioned earlier. This innovational study combined the use of massive new data and explicit economic reasoning to reach surprising conclusions. It was not only controversial; it became emblematic of both the advantages and some possible drawbacks of the New Economic History. Its conclusions were contested by both other economic historians and more widely (David, et al., 1976).

One aspect of that discussion is unexpectedly relevant today. Fogel and Engerman measured what they called the exploitation of slaves by assuming that slaves were responsible for the costs of their own upbringing. In contrast to the more usual family pattern where parents support children in an intergenerational transfer, they assumed that slaves were isolated individuals who needed to "borrow" from slave owners to eat before they could work. The low earnings of adult slaves then was interpreted more as repayment of these loans than exploitation.

This argument appeared strange to their critics as a description of the nineteenth century, but it seems accurate for the twenty-first century. Slavery is long gone, of course, but its influence remains strong. Margo (1990) described the poor educational opportunities open to free slaves in the late nineteenth century, and education in urban areas today exhibits a similar pattern of purposeful neglect. Childhood and education have become longer as time has gone on, and a decent education today includes college.

Poor students in the second half of the twentieth century could get low-cost education in state universities where the costs were subsidized by their parents' generation in taxes in an

extension of public schools. But as states were strapped for funds at the end of the century and more recent years, it has been the path of least resistance for states to reduce their spending for state universities. State universities are largely private now with state funds accounting for only a minor part of their costs. The universities have raised tuition in an effort to offset this loss of revenue, returning young Americans to the position Fogel and Engerman assumed for slaves.

Wise men and politicians are telling us that the federal debt will burden our children and must be reduced. But the real burden on young people is educational debt caused by state educational policies. Our children have been made responsible for their own college education, which has become an important part of their preparation for work. They are graduating college with overwhelming debts of $100,000 or more, and even those who fail to graduate still leave college with ample college debts. College debt has surpassed credit-card debt, and the President and Congress have wrangled about the interest rate to charge.

This is a historical parallel of some interest and another reason to integrate the New Economic History with current economics. The discussion could even extend to macroeconomics, as the high debt of many young people will depress their consumption in coming years. The analog of slaves presumed repayments to their owners is the low consumption of debt-ridden young people today. The large amount of student debt outstanding suggests that this low consumption may be a drag on the American recovery from the global financial crisis.

Costa and Kahn (2008) examined social debts in a study of Civil War soldiers. They looked at the interactions of soldiers in war and captivity to see the effects of friends and comrades. They found that some soldiers were willing to risk their lives for others (heroes)

while others were more like the *homo economicus* of elementary economics (cowards). They reach out to other social sciences for other concerns about the effects of community ties and suggest a variety of hypotheses to be considered. Their research also recalls Adam Smith, using tools derived from *The Wealth of Nations* to raise questions about the topics of *The Theory of Moral Sentiments*.

Hornbeck (2012) extended our understanding of long-run effects of economic changes to natural disasters. The Great Depression is thought of as a macroeconomic event, but the dust bowl of the 1930s was an important part of the national experience. Hornbeck used the same kind of regression discontinuity as Dell to separate the effects of soil depletion and other factors. Land values fell thirty percent in high-erosion counties.

Hornbeck looked for the kind of substitution in production that Voitländer and Voth found after the Black Death, but found little movement along relevant cost curves. Instead, he found that people migrated out of the dust-bowl area rather than adjust their agricultural practice to the new conditions. The Okies, as the migrants to California were called, revealed another path of adjustment to change. As Hornbeck noted, this geographical adjustment is typical of recent American labor-force adjustments to other changes in employment opportunities (Blanchard and Katz, 1992).

The fall in land prices in the dust bowl is similar to the fall in house prices at the end of the recent housing boom. Many mortgage holders have found themselves "under water" with the value of their loans exceeding the value of the houses. Various forms of relief have been tried, but the banks have resisted writing down their loans. The result is that many people are unable to spend as they would like or move because of their outstanding mortgages. This then has

macroeconomic effects as noted already for educational loans. Consumption is down and geographical mobility as an adjustment to labor-market difficulties is not available. The New Economic History of the United States reveals that some of the factors that enabled us to recover from natural and man-made disasters are not available to us now.

Finally, the New Economic History has informed us of recent demographic events other than the Black Death. The "baby boom" in the United States was created by the return of soldiers from the Second World War after a long depression that depressed birth rates. Easterlin (1987) studied how the baby boomers fared in subsequent years. He found crowded schools and increased labor-force competition. The important new observation was the persistence of the effects of the demographic shock. As baby boomers aged, their problems aged with them in age-appropriate ways. For example, as the baby boomers have reached retirement age, politicians are worrying how the Social Security System will be able to handle them. A presidential commission increased the normal retirement from 65 to 67 over many years to prepare for this shock. More changes are under discussion. Urban economists are now even asking if the postwar American growth of suburbs that accommodated all those children is now outmoded. Cause and effect are unclear at this point, but a lower birth rate and shifting technology have begun to have their effects of living patterns. The New Economic History does not have much to say about historical processes just beginning, but the history Easterlin studied is relevant to the work of economists who analyze these movements.

Let us now turn our attention to good fortunes that have been illuminated by the New Economic History. Even if economics is the dismal science, economic history need not be. The largest favorable shock that has been illuminated by the New Economic History has already been mentioned. The Industrial Revolution was a major change whose effects are all around us still.

Allen (2009a) used the tools of the New Economic History to show that the Industrial Revolution emerged from the combination of high wages and low energy prices. As already noted, this was such a large historical event that the literature about is immense and ongoing. I can only allude to it here.

Instead, I focus on the good analog of the persistent damage done to people damaged in economic transitions. Clark (2014) has used the extensive data characteristic of the New Economic History to show that half of the variation in overall status of individuals is determined by their lineage. Clark and his colleagues showed that this is true from the United States to China and Japan, and from Sweden to India. Regression to the mean is apparent in their data, but the process takes hundreds of years.

The methodology was to use surnames to identify descendants. Instead of relying on scarce censuses and family records, Clark and his colleagues identified unusual names characteristic of prosperity at some historical time. They then looked at more recent data on prosperity and social standing to see if these names were over represented. Surprisingly, they were, in many countries and over long periods of time.

This view of durable status has been reinforced recently by Ferrie, long a student of population mobility in the United States (Ferrie, 1999). Using the more familiar approach of identifying families in censuses, Long and Ferrie (2014) extended the normal two-generation study of social mobility to three generations in a recent paper. They found more persistence over three generations than over two generations. Clearly, there is a great deal of noise in the mobility of individuals and in any single generation. But extending the length of study provides evidence of greater stability.

Goldin and Katz (2008) used a different approach to analyze the relative fortunes of different groups in America during the twentieth century. Their focus was on education and the difference between educated—and therefore skilled—workers and uneducated and unskilled workers. The progress of technology sets the demand for labor, and the interaction of supply and demand was characterized as a race between education and technology. This colorful metaphor drastically simplifies the many determinants of both education and technology. Their book goes into these complications in great detail.

This contribution is particularly relevant today. Economists examining the distribution of jobs have found that the progress of computers has hollowed out the demand for labor. There are demands for low-wage jobs and quite high-paying jobs, but the demand for factory jobs that were the mainstay of growing employment after the Second World War is down. This has created a need to rethink the simple macroeconomics of labor, since different aspects of technology have effects on different segments of the labor supply. The New Economic History provides a historical background that suggests several important lessons. The nature of technology has been exerting pressure on the wages structure for many generations before this one. Both progress in education (supply) and technology (demand) must be considered when trying to discover effective policies in this area. And, as Goldin (1990) observed in the history of women's work, participants in these sorts of changes cannot predict where they will end up.

Let me abandon this romp through economic history now and try to think more broadly about the future of the New Economic History. I do not like cherry picking in the work of others; I cannot imagine it is informative in much beyond methodology here. The brief sampling of work here does not lead directly to substantive conclusions; it rather suggests the scope of the New Economic History. The subject matter ranges over time from early history to recent events,

and over space across continents. If there is one safe prediction, it is that the discovery of new data and of new ways to use existing data will encourage this wide geographical and temporal spread.

As suggested earlier, two aspects of the New Economic History are keys to the growth of scholarship in this area. One is the focus on institutions as carriers of economic structures across generations and sometimes centuries. The other is the focus on causality through imaginative use of identification strategies.

The importance of institutions is undeniable, but its role in research is problematical. The tradition takes its cues from North (1990) and the support of the New Institutional Economics that carries on this tradition. As I have described, the New Economic History often appeals to the role of institutions in the long-term effects of various short-run changes. But while the econometrics is fine in these studies, the accounts of institutional change often are less fully analyzed. Greif (2006) tried to clarify the issues involved, but his concern with theory of institutions may have made the empirical task of finding changes in institutions harder. One issue is that the evidence on institutions frequently is qualitative instead of quantitative. Ways need to be found to quantify what before was not considered quantifiable. In addition, institutions often change only infrequently or very slowly. Finally, it is not always clear how to define the institutions in question. Have morals declined in the United States? Are morals even considered an institutional framework? These are the kind of questions that need more research.

The other characteristic of the New Economic History is the attention given to causality. This typically involves a strong identification strategy to disentangle the motives of different parties to a decision. As shown in the brief selection of work above, the New Economic

Historians are aware of this issue and devote a lot of thought to the process of identifying supply or demand influences. Voitländer and Voth went to great lengths to show that the Black Death was in fact the cause of the demographic transition in Western Europe, and Allen has supported his explanation of the Industrial Revolution by comparing factor prices in many other countries. Dell and Hornbeck used geographic boundaries to identify causal elements in their stories.

Let me illustrate these claims with two final examples, one from a young New Economic Historian and one from an old member of our tribe, one from far away and one from long ago. They both involve the consequences of plagues.

The first example is by Dan Li, a Chinese economic historian (Li and Li, 2014). She and her coauthor are part of a geographical expansion of the New Economic History to Asia. A recent paper summarized the literature on the history of Chinese economic institutions and macroeconomics for a millennium (Brandt, Ma and Rawski, 2014). The paper argues that economic history illuminates choices today—as I have stressed for issues in more familiar venues. Li examined migration from China to Manchuria in the early twentieth century, shortly after a plague that hit the destination of the migrants. The plague reduced population more strongly in some areas than in others. Migrants to areas where the plague hit hard fared better in future years than those to other areas. The question is why did migrants settle there. In other words, was this good fortune determined by design or by luck?

There are no records of individual choices being made, no questionnaires about why a specific destination was chosen. Instead Li and Li (2014) use their data to distinguish migrants to different areas in the data we have. They found that migrants with higher socioeconomic

status avoided plague-hit villages. Migrants to these areas were the least likely to do well in Manchuria.

The second example originated in a conference on quantification in the ancient world. My first reaction was that ancient data was an oxymoron. But my second reaction was that qualitative data—even if only the opinions of modern ancient historians—could be quantified. The process was made manageable by choosing only to quantify the data only in the binary way so typical of our modern electronic devices. By this metric, inflation was either present or not, and political instability was either present or not. American economic historians will recognize this approach as the technique used by Romer (1986) to compare the severity of business cycles throughout the twentieth century. She had to degrade the recent data to make it comparable to the older data. I had to simplify the desired information to quantify at all.

The quantification allowed a decision on timing. The empirical result was that both switches turned on at the same time. This suggested joint causation, and a third possible cause was likely. I looked for a plausible exogenous variable that could have set an interactive process of inflation and instability off together and argued that the preceding Antonine Plague was the cause of both inflation and instability. I commend you to my book for details of the change from the Early Roman Empire to the Late Roman Empire, an important institutional change in world history (Temin, 2013).

These two final examples are presented only to highlight the extension of familiar techniques to new fields of inquiry and the opportunities open to the New Economic History. If there is a theme that runs through this survey of where we were, where we are, and where we

might go, it is that the fields of economic history and economic development have much to learn from their interaction.

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