The state and scope of the economic history of developing regions

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Abstract
In a recent rallying call, Hopkins (2009) petitions historians to ‘re-engage ... in the study of Africa’s economic past not least because it is relevant to Africa’s future’. It is our contention that this statement is not only true for Africa but for all developing regions of the world. Standard growth theorists are often incapable of explaining the inability of poor regions to catch-up, or worse, stand indifferent to the complexities of these societies. Yet understanding the process of economic change is necessarily linked to the past. This paper examines the state and scope of the economic history of developing regions, underlining the importance of history for economic development. While the process of economic change in the industrialised North informs our understanding of its successes, exploring the economic history of the developing world may shed light on the causes of stagnation, and speed along the process of development.

Keywords: economic history, developing countries, South, Latin America, Africa, India, China, Middle East, Cliometrics

JEL: N01
1. INTRODUCTION

In the half century or so since the evolution of Economic History into a well respected economic discipline concerned with the causes of economic growth, important contributions have been made toward our understanding of the role of institutions, path dependency, technological innovation and evolution in determining growth. The largest portion of the evidence has focused on the economic histories of the currently developed nations and only a small fraction of our knowledge comes from currently developing countries. However, the limited evidence that we have has already played a major role in furthering our understanding of the longevity of institutions, the importance of trade and education for growth, and the economic and social consequences of colonialism.

Yet, despite this contribution, the proportion of articles focused on developing countries remains low, on average, less than 20% of all submissions between 2004 and 2008 have been on topics outside Western Europe, the United States and Australia/New Zealand.¹

Table 1: Submissions by region to the Journal of Economic History

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Source: Hoffman and Fishback (2009)

Major articles and books on Latin America, Asia and Africa have appeared (Pomeranz 2000; Sokoloff and Engerman 2000; Acemoglu, Johnson et al. 2001; Acemoglu, Johnson et al. 2002) but these are written by leading American scholars and focus on explaining the differences between Europe and America on the one hand and specific developing regions on the other. These are, it must be said, extremely valuable contributions. It is, furthermore, equally encouraging that there is an increasing flow of work coming out of the leading economic history journals, here reflected in the number of publications. Of the 29 papers published in 2009 by Elsevier’s Explorations in Economic History, only five dealt on topics outside Europe, the U.S. and Japan, four of them in a special edition on heights and human welfare. The proportions are similar for the Journal of Economic History, published by Cambridge University, with only six of the 32 papers published in 2009 covering topics on developing regions, and for the Economic History Review, published by Wiley-Blackwell, seven papers of the 37 published.

¹ This is also true for other leading economic history journals, here reflected in the number of publications. Of the 29 papers published in 2009 by Elsevier’s Explorations in Economic History, only five dealt on topics outside Europe, the U.S. and Japan, four of them in a special edition on heights and human welfare. The proportions are similar for the Journal of Economic History, published by Cambridge University, with only six of the 32 papers published in 2009 covering topics on developing regions, and for the Economic History Review, published by Wiley-Blackwell, seven papers of the 37 published.
universities that use history to enhance our understanding of the development process (La Porta, Lopez-de-Silanes et al. 2008; Nunn 2008; Dell 2010). The emergence of rich data sets and the digitalization of these, the pervasive presence of English as academic lingua franca, combined with more research graduates from developing countries at the top Western institutions specializing in economic history have brought into light a vast new research field formerly restricted to isolated departments of history, development studies or worse, hidden in archives. The search for natural experiments in history – the economist’s laboratory – has also redirected the attention of established scholars to such episodes in the developing world (Diamond and Robinson 2010).

The journal is also aware, and aims to encourage, the shifts that are taking place in the methods used to analyze the economic past. In a useful summary Nunn (2009) points out that rather than simply relying on (possibly spurious) correlations between historical events and present-day outcomes (with no clear indication of causality), better estimation techniques and richer data sets have allowed a shift towards better identification of the mechanisms by which the historical event shape future outcomes. Nunn (2009) predicts that future work in economic history will become more confined and specific in scope, using micro-data to identify ‘finer causal factors and more precise mechanism’. This implies less generalization of results and a reemergence of historical enquiry into each growth episode.

The increasing number of scholars working on these regions is likely to result in an increased demand for publication space. However, without an increase in the supply of forums for this type of research we may not be able to provide access to research on the economic history of developing countries. It is our hope that this journal can contribute to the publication space awarded research on the economic history of these regions, that it will be a forum for high quality economic research and ultimately a leading journal in the field of economic history.

2. THE ECONOMIC HISTORY OF DEVELOPING REGIONS

In a recent rallying call, Hopkins (2009) petitions historians to ‘re-engage ... in the study of Africa’s economic past not least because it is relevant to Africa’s future’. This statement is not only true for Africa but for all developing regions of the world. Understanding the process of economic change is necessarily linked to the past. Thus, exploring the economic history of the developing world must shed light on the causes of stagnation, and speed along the process of development.

As noted above, the existing research on developing countries’ economic histories has already been informative. This section highlights some of those contributions.

2.1 Latin America

The lion’s share of recent economic historical research on Latin America revolves around two closely interconnected questions. First, what explains Latin America’s growth retardation as compared to the West and, to a lesser degree, East Asia? Second, why is national income so unequally distributed in the majority of Latin American and Caribbean countries? With the exception of Haiti and Nicaragua all LACs are nowadays classified as middle-income countries, which basically means that they have sufficient economic power.

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2 Interestingly, nearly all the papers cited by Nunn (2009) dealing with the economic history of developing regions have been submitted or published in the economics journals, not economic history journals.
to eradicate poverty. Still, about one quarter of the region’s population lives at or under the World Bank defined poverty line (Frankema 2009). Indeed, many of the scholarly debates that have emerged in recent years are in search of explanations of this peculiar feature of Latin American development (Bulmer-Thomas, Coatsworth et al. 2006).

Adopting a very long term perspective, the conventional view is that colonial policies of social repression, resource extraction and trade monopolization have generated various forms of social, economic and political inequality that have hampered the development of markets and political order far into the post-independence era. Catholicism, Caudillismo and the authoritarian nature of Iberian colonial rule have often been contrasted with the principles of cooperative government, economic liberalism and Protestantism to explain the increasing income gap with the former British possessions in North America (Landes 1998; North, Summerhill et al. 2000).

Engerman and Sokoloff have argued, against this conventional view, that the origins of institutional divergence are tied to exogenous conditions such as Latin America’s population heterogeneity and natural resource abundance, rather than Iberian cultural values per se (Sokoloff and Engerman 2000). Yet, an even newer strand of literature goes further, by arguing that Spanish institutions may have been different, but not necessarily less efficient or ‘bad’ for long term economic development (Elliot 2006). The presumed ‘absolutism’ of the Spanish Crown and the ‘myth of relentless extraction’ is contested on the basis of new empirical evidence revealing extensive fiscal bargaining procedures and a sophisticated system of imperial revenue transfers, which allocated collective goods across the Spanish American empire, while outright confiscation was limited. Effective fiscal institutions, so it is argued, do a much better job in explaining why the Spanish American empire ultimately proved to be more viable than the British American empire, (Marichal 2007; Irigoin and Grafe 2008).

This debate intertwines with recent studies questioning the widely-held belief that Latin American inequality has been persistent from colonial times onwards. A number of recent studies have shown impressive changes in wage differentials, wage-rental ratio’s, labour income shares as well as Gini and Theil-coefficients of income distribution over the past two centuries (Williamson 1998; Arroyo Abad 2008; Bértola, Castelnovo et al. 2008). In view of this new evidence several scholars have argued that fluctuations in Latin American inequality have been driven by a combination of path dependent conditions and new, time-specific determinants, which are not necessarily rooted in colonial history, nor ossified in the region’s future (Prados de la Escosura 2005; Frankema 2009). Moreover, some recent backward extensions of real wage and income distribution studies into the colonial era do not produce immediate evidence for extraordinary low living standards, nor for exceptionally high levels of inequality (Milanovic, Lindert et al. 2008; Dobado and García 2009). Because most of the empirical research still has to be carried out, this line of research will continue for many years to come (Coatsworth 2008; Edwards 2009; Williamson 2009).

A third debate even more directly connects the past with the present by addressing the effects of globalization. Although the study of globalization - more narrowly defined as global or Atlantic market integration (and disintegration)- has a strong tradition in the famous Dependencia school, these recent studies are embedded in modern trade theory and largely neglect the once so influential Prebisch-Singer hypothesis (Prebisch 1962). How do the causes, characteristics and consequences of the first wave of globalization during the late
nineteenth and early twentieth century compare to those of the current wave of globalization that has emerged after the breakdown of import-substitution policies? (Taylor 2006; Arroyo Abad and Santos-Paulino 2009). The first wave seems to have spurred growth and inequality, but what about the second wave? What are the chances of a repeated resource curse? And does it matter that the former Atlantic market has now become a global market, including the new Asian giants?

In the meantime, current rates of catch-up growth in Brazil and the impressive pace of democratization in Chile, indicate that the socio-economic and political outlook of Latin America can potentially change very rapidly. If this pace of change continuous and spills-over to an increasing number of LACs, there is a good chance that these questions and interpretations soon have to be reformulated in order to keep up with new developments.

2.2 Africa

In a recent review of the state of economic history in Africa, Anthony Hopkins (2009) juxtaposes the dearth in economic history research by African historians against the evolution of the new-institutional economists investigations into the existence and performance of institutions in Africa and elsewhere. Although the 1990s economic growth literature ventured to explain Africa's underperformance, Acemoglu, Johnson and Robinson’s (AJR) seminal contribution at the start of the new millennium set a new research agenda that, according to Hopkins (2009), historians of Africa have failed to embrace. Although earlier authors had noted the impediments of colonial rule on long-run development, AJR found a deft empirical solution to explain Africa’s poor growth performance through the institutions that were implemented during colonial rule (Acemoglu, Johnson et al. 2001; Acemoglu, Johnson et al. 2002). They argue that colonies with a less deadly disease environment attracted greater European settlement which facilitated growth promoting institutions that protected property rights. Where European mortality was high (and settlement low), colonisers established extractive rent-seeking institutions that were detrimental to development. The empirical instrumental variables (IV) technique the authors use first estimates a strong negative relationship between initial settler mortality and institutional quality today and in the second stage, finds that domestic institutions exert a strong positive effect on per capita income.

The AJR contribution ignited interest in explaining the impact of African colonial history on current performance exploiting newly available data. Nathan Nunn's (2008; 2010) first contribution to the new African economic history combines data from historic shipping records and constructs estimates of the number of slaves shipped during four African slave trades: the trans-Atlantic, Indian Ocean, Red Sea and trans-Saharan. He finds that those areas from which the largest numbers of slaves were taken are today the poorest regions in Africa. Nunn then tests whether the regions supplying slaves were historically also the poorest; instead he finds that the more developed and more densely populated societies supplied the most slaves, which suggests that selection is not driving his results. Nunn and Wantchekon (2009) extends this to a theory of mistrust, postulating that the impact of the slave trade works through factors that are internal to the individual, such as cultural norms, beliefs, and values. The slave trade also resulted in Africans moving into areas that were more rugged, resulting in lower subsequent growth potential for these areas (Nunn and Puga 2009). More recently, Nunn (2009) uses information on the locations of Catholic and Protestant missions during the colonial period to test its impact on religious conversion, education, civic participation and attitudes toward democracy. He finds that Protestant missions had strong effects on conversion and increased educational attainment while
Catholic missions “appear to have had little effect on conversion and no effect on education” (Nunn 2009).

Bolt and Bezemer (2009) use data on colonial human capital and find a strong link with long-run growth. They argue that education explains growth better and shows greater stability over time than do the measures of extractive institutions posited by AJR, and that the impact of settler mortality is through education rather than institutions. Using household surveys from the 1990s, Huillery (2009) finds a positive relationship between early colonial investments in education, health and infrastructure on current levels of schooling, health outcomes, and access to electricity, water, and fuel at the district level. Her detailed microdata also allows for advanced estimation techniques to determine the differences in outcome of neighbouring regions only, thus keeping all other variables constant. More recently, Fenske (2010) use cross-sectional data on pre-colonial African societies to demonstrate that the existence of land rights, slavery and polygyny occurred in those parts of Africa that were most suitable for agriculture and in which population density was greatest. There are many more examples (Buelens and Marysse 2009; Fafchamps and Moradi 2009; Moradi 2009).

Other economic historians have criticised these new approaches Austin (2008), for example, has been critical of the ‘reversal of fortune’ thesis because of methodological insensitivity to diversity and context. Although Hopkins acknowledges that African economic history is in need of a serious reawakening – “...for reopening lines of enquiry that are important for understanding both precolonial and colonial history” (Hopkins 2009:177), he shares Austin’s concerns. Hopkins is correct in questioning the ‘sweeping’ or ‘broad brush’ collating effect of the early econometricians’ methodology in researching the economic history of Africa. Hopkins suggests that research needs to “…proceed cautiously on a case-by-case basis and abandon the attempt to formulate one prescription for a large and diverse continent.” The use of both quantitative and qualitative microdata in region-specific settings and more recent techniques of identification and falsification are increasingly used by both economists and historians (Fedderke and Schirmer 2006; Green 2009; Mariotti 2009; Boshoff and Fourie 2010; Fourie and von Fintel 2010). Such region-specific case studies pave the way for further interdisciplinary collaboration which is essential in broadening our understanding of Africa’s economic past, present and future.

2.3 India

Over the last decade, the Indian colonial experience has entered broader conversations within the economics literature on the “Great Divergence”, the relationship between colonialism and institutional development, and the persistence of institutions. Furthermore, Indian economic history has embraced cliometrics. Researchers have constructed new district-level datasets on railroad penetration (Donaldson 2008), educational spending (Chaudhary 2010) and communal violence (Jha 2008) to name a few examples. Economic theorists have used the East India Company operations to better understand the nature of contract enforcement (Hejeebu 2005) and the microeconomics of exports (Kranton and Swamy 2008). By adopting the cliometrics approach, Indian economy history has a bright future to answer specific questions about the Indian context and general questions of interest to other economists. We focus here on a few recent studies and their implications for colonial rule in India. This is far from a comprehensive overview, but rather a description of recent advances. For a detailed overview, we recommend the reader to Cambridge Economic History of India (Kumar 1983) and Roy (2000; 2002; 2004).
While older studies suggest the divergence in economic development between Europe and Asia began only after the 19th century (Parthasarathi 1998; Pomeranz 2000), recent studies drawing on Indian wages, incomes and market integration find evidence of diverging standards of living well before 1800 (Broadberry and Gupta 2006; Studer 2008; Roy 2010). A stronger understanding of when India fell behind has important implications for how we view the colonial experience. If India was diverging from Europe in the early modern period, colonialism alone cannot be held accountable for the slow pace of Indian development in the 19th and 20th century.

Several recent micro-studies of individual sectors of the Indian economy also support a nuanced reading of colonial policies and their effects on the economy. For example, education spending under the Raj was low relative to other countries at comparable levels of development and the Indian Princely States. But, local factors such as a high degree of social heterogeneity and a strong preference for secondary education among Indian elites were important barriers to the spread of mass primary education (Chaudhary 2009). A study of late 18th century Bengal finds remarkable stability in income per-capita in spite of the transition to colonial rule (Roy 2010). However, another novel study finds large and persistent effects of colonial land tenure systems on post-independence agricultural productivity (Banerjee and Iyer 2005).

Within infrastructure, the study of railways has enjoyed a recent resurgence. According to Andrabi and Kuehlwein (2010) (2010) railways had limited effects on price convergence between districts, but Donaldson (2008) finds large and positive effects of railways on price convergence and agricultural incomes using a sophisticated model and an original dataset from 1861 to 1930. Moreover, railways also appear to have reduced the severity of famines in colonial India (Burgess and Donaldson 2010). On the industrial organization side, government ownership of Indian railways lead to significant productivity gains unlike other countries where efficiency declined following nationalization (Bogart and Chaudhary 2010).

The consequences of colonial policies, thus, range from no effects as in the case of 18th century Bengal to positive effects in the case of railroads. Given the diverse findings, we need more research studying the effects of colonial rule disaggregated by region, sector and time period. How did specific policies interact with local conditions? Why did colonial policies succeed in some places and in some time periods? Why were some policies a complete failure? Can we attribute the negative effects to an extractive colonial state? Or, was colonial rule constrained by local factors? Detailed micro-studies are essential to answering such questions and assessing the net macro effect of colonial rule in India.

2.4 China

In recent years there has been great interest among economists and historians in the long-run development of China. Much of this interest can be traced back to a very famous question raised by a renowned British sinologist Joseph Needham, known as “The Needham Question”: why China had been overtaken by the West in science and technology, despite its earlier successes? Economic historians have extended this question: why did the industrial revolution take place in Britain instead of China? Elvin (1973) and a few prominent scholars propose a demand side explanation to this question, arguing that one major factor preventing China from advancing as an industrialized economy was a high labour-to-land ratio limiting the incentive to invent new technologies in ancient China. In comparison, Lin (1995) attributes ancient China’s technological stagnation to the supply-side. He argues that the long-standing Imperial Civil Service Examination system in ancient China was the main
channel through which bureaucratic officials were selected in a fair and impartial way. However, because the civil service examination system focused only on Confucianism and literary skills, most talented Chinese were fully devoted to either this examination or research of the humanities and lacked the incentives to accumulate knowledge in science. As a result, a scientific revolution was unlikely to spontaneously take place in China, even though China had satisfied many of the accepted crucial conditions for industrialisation as early as the twelfth century.

Although the explanations of China’s failure to industrialize are completely different, these studies share the same view: they attribute this divergence to some unique features in ancient Chinese society that made China intrinsically different from Europe. Philip Huang and quite a few traditional Chinese historians and demographers further advance this "pessimistic" view and argue that the Chinese economy was weighed down by overpopulation, and that a process Huang terms “involution”, resembling what others might characterize as a Malthusian trap, prevented China from realizing any progress in economic growth well into the 20th century. Since the end of the last century, this assessment has been challenged by Ken Pomeranz, R. Bin Wong, James Lee, and others, who have argued that China was doing much better than has been appreciated, especially in coastal areas, rivaling Europe in per capita income as late as the end of the 18th century. This more sanguine outlook, based on detailed (if also controversial) comparisons of China and Europe, suggests that conditions in pre-modern China were much more favorable, both as regards living standards and prospects for sustained growth, than scholars had previously thought. This group of economic historians, many of whom are affiliated with the University of California (and thus usually categorized as the California school), believes that there were essentially no such China-specific factors that made ancient China “inferior” to Britain or Europe. For example, Pomeranz (2000) attributes the divergence between Europe and China to the role of “geographic accidents” such as the proximity of coal deposits to early British centers of industrial production and the easily exploitable natural resources of the Americas. Their studies overthrow the ingrained Eurocentric growth model and have also been espoused by prominent European economic historians such as Jared Diamond, Greg Clark and Robert Allen (Diamond 1997; Clark 2008; Allen 2009; Clark and Cummins 2009; Allen, Bassino et al. 2010).

The California school has inspired more scholars to increasingly adopt the methodology of “horizontal” research, which frames the experience in China from the perspective of world economic history. They construct gauges of economic performance, such as output, real income and productivity, for regions of China and base comparisons between regions, and especially between them and regions or countries of Europe. Moreover, the traditional view that China was stagnating has not been subjected to much in the way of systematic empirical tests, either for the pre-modern or modern periods. A scarcity of data has long plagued scholars of China, and prevented them from constructing a reliable record of Chinese economic development over the long run. With the recent movement toward the opening of archives in China, and the greater ease of collecting information made possible by advances in the power and portability of computers and scanners, the opportunities for scholars have expanded enormously. Carol H. Shiue, Debin Ma and Se Yan are three scholars who have conducted excellent research by combining economic theory and econometric methods with original data sets collected from Chinese historical archives.

Trade expansion and market development have long been considered preconditions for industrial revolutions and sustainable economic growth. Therefore, examining the market
development in pre-modern China would shed light on the causes of China’s failure to industrialize. Shiue (2002) utilizes regional grain price data collected by the Qing government, combined with historical weather data, to study the inter-regional correlations of grain prices, which is used as an indicator of market integration. She finds that the overall level of market integration in China was higher than previously thought and that those inter-temporal effects are important substitutes for trade. Both factors reduce the importance of trade as a unique explanation for subsequent growth. More recently, Shiue and Keller (2007) compare market integration in Europe and China on the eve of the Industrial Revolution, finding little difference, although somewhat better performance in England than in the Yangzi Delta.

Debin Ma has assembled wage data of various types of laborers in different regions of China and, with historical price data, estimates the real income of these people from the eighteenth to the twentieth century (Ma 2008). The data are used to compare the standard of living in major Chinese cities to their counterparts in Europe, India, and Japan. Ma and his co-authors (Allen, Bassino et al. 2010) find that in the eighteenth century, the real income of building workers in Asia was similar to that of workers in the backward parts of Europe and far behind that of workers in the leading economies in northwestern Europe. Industrialization led to rising real wages in Europe and Japan. Real wages declined in China in the eighteenth and early nineteenth centuries and rose slowly in the late nineteenth and early twentieth. The income disparities of the early twentieth century were due to long-run stagnation in China combined with economic development in Japan and Europe. The painstaking efforts made by Shiue, Ma and other economic historians to collect data for pre-modern China are paving the way for a deeper understanding of China’s economic performances in that era.

Studying pre-modern China is crucial for a better understanding of the Great Divergence. However, study of the economic developments of modern China (1842-1949) is equally interesting. Until the 1840s, China was largely a closed, agrarian economy; however, pressure from Great Britain and other foreign powers led China to open its economy to international trade and later to foreign direct investment. Although the scarcity of data makes it virtually impossible to construct annual time series of GDP or other major economic indicators, scholars, such as John Chang, Ta-chung Liu, and Thomas Rawski, have compiled various estimates of the speed and magnitude of industrial expansion and economic growth (Brandt and Rawski 2008).

While this view of China’s accelerating economic change is shared by many historians and economists, its impact on people’s real income and standard of living has been poorly measured. In his doctoral dissertation, Se Yan (2008) compiles the first systematic evidence of patterns in real wages and living costs for China from 1858 to 1936. He constructs nominal wage series from the records of employees in the China Maritime Customs (CMC) service for nearly fifty Chinese cities. He also constructs group-specific cost of living indices from price data and household budget information contained in CMC trade statistics and surveys. With these new nominal wage series and cost of living indices, Yan estimates the long-run trends in real wages and in the ratios of wages for the skilled to unskilled workers and for highly skilled to unskilled workers. He finds that the skill premium rose rapidly during the first three decades of industrialization, but began to level off and decline from the mid 1910s. Yan (2008) and Mitchener and Yan (2010) further find evidence suggesting that the reversal of the skill premium is possibly driven by two factors. First, the trade boom in China during the early twentieth century benefited unskilled workers relative to skilled.
Second, educational progress increased the supply of skilled workers, thereby reducing the skilled wage.

Of course, this cannot be a complete introduction of recent academic studies in this field. Many outstanding researchers have contributed to the progress in Chinese economic history. A more recent example is Zelin’s book, *the Merchants of Zigong* (2005), which has received much scholarly attention. All these concerted efforts have made Chinese economic history one of the most vibrant fields in economic history.

**2.5 The Middle East**

Economic history of the Middle East has recently experienced a significant growth in both the volume and scope of scholarship. Until the late twentieth century, research on this region had been hampered by numerous obstacles, including linguistic barriers, government censorship, restrictions on access to archival resources, and lack of external demand and institutional support. Undeterred by these obstacles, prominent historians such as Gabriel Baer, Ömer Lütfi Barkan, Charles Issawi, Halil İnalcık, and André Raymond pioneered pathbreaking research programs, but progress in the field was slow and lagging behind that of other parts of the world. As these obstacles gradually waned and some of the significant historical questions of the Middle East and the Islamic world gained widespread attention, scholarship on the region has improved tremendously. The first decade of the twenty first century has witnessed the rise of economic history of the Middle East to a mature subfield, research being marked by the creative utilization of primary sources, innovative application of sophisticated tools of quantitative analysis, and skilful employment of the recent methodological and theoretical developments in modern economics.

As archival material has become more available and researchers have mastered innovative ways of using the available data, a proliferation of quantitative studies has taken place. Continuing a long established line of research, some historians have focused on specific regions and assembled information from various sources to identify how the resource profile, production patterns, size and composition of the population, and general economic outlook of the region has changed over time. These studies have typically used Ottoman tax registers as primary sources (Coşgel 2004). Other researchers have completed large gaps in our knowledge of how the Middle Eastern economies have performed in comparison with other parts of the world, providing reliable estimates of such macroeconomics indicators (measured in standard units to facilitate comparisons) as money, prices, incomes, agricultural productivity, and anthropometric measures (Pamuk 2000; Özmucur and Pamuk 2002; Coşgel 2007 ; Stegl and Baten 2009). Another recent line of research has been to use data for not just estimating regional variables but for quantitative analysis of larger economic and historical questions, such as how risks and transaction-costs shaped public finance and how military activities of the Ottomans affected intra-European feuds (Coşgel and Miceli. 2005; İyigün 2008).

Borrowing insights from new theoretical developments in modern economics, researchers have also brought new light to some of the longstanding puzzles of the region’s history and introduced new questions invoked by these developments. For example, adopting a New Institutional approach and comparing Western and Middle Eastern institutions, they have identified the reasons for why the Middle East has adopted distinct institutional arrangements from the West and how the institutional rigidities of the Islamic Middle East
have caused the economic underdevelopment of the region (Kuran 2004; Balla and Johnson 2009; Rubin 2010). Similarly applying developments in the political economy literature, they have studied where dictatorial rulers have obtained political power and how their search for legitimacy through agents has affected their choice of technology (Coşgel, Miceli et al. 2009; Coşgel, Miceli et al. 2009). Judging by recent trends in this field, future contributions to the economic history of the Middle East will most likely continue as increasingly more creative and sophisticated utilization of primary sources, economic theory, and quantitative analysis.

3. CONCLUSIONS

The quality of the existing research on the economic history of developing countries is all the more impressive given the low proportion of published research focusing on these areas. Furthermore, it suggests we need a forum for future research on these areas that contributes to our understanding of the roles of institutions, path dependency, technological change and evolution on economic growth.

Many valuable data sets relating to developing regions remain unexplored, and many interesting questions unanswered. This is exciting. Economists, historians and other academics interested in the changing economic past have an opportunity to work, separately and in collaboration, to begin to unlock the complex reasons for developmental differences, the factors behind economic disasters and the dynamics driving emerging success stories.

We hope that Economic History of Developing Regions will help nurture a new generation of economic historians to show how the economic history of the developing countries can add to our understanding of economic theories, and, by learning from the lessons of the past, contribute to improving the state of many of the world’s poorest economies.
4. REFERENCES


