

How and Why Does History Matter for Development Policy?

Michael Woolcock

Simon Szreter

Vijayendra Rao

The World Bank
Development Research Group
Poverty and Inequality Team
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Abstract

The consensus among scholars and policymakers that “institutions matter” for development has led inexorably to a conclusion that “history matters,” since institutions clearly form and evolve over time. Unfortunately, however, the next logical step has not yet been taken, which is to recognize that historians (and not only economic historians) might also have useful and distinctive insights to offer. This paper endeavors to open and sustain a constructive dialogue between history—understood as both “the past” and “the discipline”—and development policy by (a) clarifying what the craft of

historical scholarship entails, especially as it pertains to understanding causal mechanisms, contexts, and complex processes of institutional change; (b) providing examples of historical research that support, qualify, or challenge the most influential research (by economists and economic historians) in contemporary development policy; and (c) offering some general principles and specific implications that historians, on the basis of the distinctive content and method of their research, bring to development policy debates.

This paper—a product of the Poverty and Inequality Team, Development Research Group—is part of a larger effort in the department to understand the origins and consequences of inequitable institutions, policies and practices. Policy Research Working Papers are also posted on the Web at <http://econ.worldbank.org>. The authors may be contacted at mwoolcock@worldbank.org, srss@cam.ac.uk, and vrao@worldbank.org.

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How and Why Does History Matter for Development Policy?¹

Michael Woolcock, World Bank

Simon Szreter, University of Cambridge

Vijayendra Rao, World Bank

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Study history, study history. In history lies all the secrets of statecraft.

Winston Churchill

Getting history wrong is an essential part of being a nation.

Ernest Renan

[M]odern social science, policy-making and planning have pursued a model of scientism and technical manipulation which systematically, and deliberately, neglects human, and above all, historical, experience. The fashionable model of analysis and prediction is to feed all available current data into some notional or real supercomputer and let it come out with the answers... [S]uch a-historical or even anti-historical calculation is often unaware of being blind, and inferior to even the unsystematic vision of those who can use their eyes.

*Eric Hobsbawm*²

I. Introduction

There is now a broad consensus across the social sciences and among development policymakers that ‘institutions’ matter, indeed that they “are a key determinant of the wealth and poverty of nations” (Hoff 2003: 205). Logically, considering where much of the empirical support for this consensus comes from, the next step in this inferential chain has been to conclude that ‘history’ matters (Nunn 2009). Any attempt to understand contemporary institutional performance is bound to identify when, where and why given institutions came to take their particular form, and how these have changed (or not) over time (North 1990, 2005). These debates now play out at the highest levels. For example, two recent flagship World Development Reports from the World Bank—on markets (World Bank 2001) and equity (World Bank 2005)—have explicitly sought to incorporate a historical sensibility into their discussions of the origins, structure and persistence of institutions, the better to help understand how they help or hinder broader development trajectories and outcomes. This is to be welcomed and encouraged.

In arguing that institutions and history matter, however, the development policy community has largely failed to take the third (seemingly logical) step, which is to recognize that historians—and the discipline they represent—might matter. Selected economic historians working within the confines of economics departments (e.g., Stan Engerman, Kenneth Sokoloff, Peter Lindert, Ronald Findlay, Kevin O’Rourke, Jeffrey Williamson) have certainly been influential in these discussions³, as have some innovative economists, most notably Daron Acemoglu, Simon Johnson, James Robinson (for present purposes we shall call them historical economists), who have turned their theories, methods and quest for data to the past. Certain

² Hobsbawm (1998: 29)

³ See, among many others by these scholars, Sokoloff and Engerman (2000), O’Rourke and Williamson (2001), Lindert (2004) and Findlay and O’Rourke (2007). While it is true that economic historians often find themselves caught “between two cultures” (Cipolla 1992) and indeed are something of an endangered species in even (or especially) the most prestigious economics departments, the primary training of economic historians is in the prevailing theories, assumptions and methods of economics, and it is these tools (and only secondarily those of historians trained in history departments) that they deploy to make sense of the past. Important recent work by economic historians includes, among many others, Pomeranz (2000), Mokyr (2002, 2010), Fogel (2004), Allen (2009), Frieden (2007), and many others. At a conceptual level, North, Wallis and Weingast (2009) is perhaps the most ambitious ‘big picture’ contribution (though it is a decidedly Euro-centric and ‘supply-side’ account).

authors (e.g., Diamond 1997, de Soto 2000) of influential ‘big picture’ development narratives have also invoked a reading of the past to make their case.⁴ However, a great many professional historians of particular countries, regions, periods or thematic issues have been conspicuous by their absence from these deliberations, especially in policy circles.⁵

While historians hardly speak with a single voice or from a unified perspective, we believe it is unfortunate that most historians and their discipline are absent from development policy debates, despite everyone putatively agreeing that ‘history matters’: at best it leads to lost opportunities to enrich the quality of scholarship and policy responses; at worst it results in all manner of instances in which partisans erroneously or selectively invoke ‘history’ in support of their cause (see MacMillan 2009). Needless to say, it is almost impossible to imagine the reverse situation, namely a prominent policy issue in which there was a consensus that economics matters but that economists were somehow not consulted.⁶ This paper seeks to establish a more constructive space in which historians, social scientists (especially economists) and policymakers can more fruitfully engage one another around core development issues, in the first instance by identifying where historical scholarship supports, qualifies or (in some instances) challenges the recent contributions by the economic historians and ‘historical economists’ to understanding the dynamics of comparative economic development. Though most of this paper focuses on the failure of development economists and policymakers to take adequate account of scholarly research by historians (though also, we hope, the desirability and possibility of sustained dialogue), an equally strident critique could be made of work by many non-economists (e.g., those engaged in ‘participatory’ research), which is in many respects even more a-historical. Due to their discipline’s currently-accepted claims to greater practical policy relevance, economists are far more numerous and influential in development policy debates, so their oversight is more consequential. Moreover, historians themselves, we shall argue, need to engage more confidently and frequently with development policy debates.

We do not claim to be the first to attempt such an exercise; rather, building on Neustadt and May (1986) and the especially insightful collection published in Cooper and Packard (1997)—works largely preceding the contemporary policy ‘consensus’ regarding the importance

⁴ A problem with certain (by no means all) ‘big picture’ histories—space precludes a more detailed review of this particular genre—is that they are each merely using history illustratively and rhetorically to demonstrate the validity of the grand thesis being presented, which claims to be a profound and general truth about economic development throughout world history during the modern period. Such grandiose interpretations violate the fundamental, historicist insight encapsulated with irrefutable logic in Gerschenkron’s (1962) classic essay, where he pointed out that no national economy’s pathway of ‘development’ could possibly ever be essentially the same as any other’s. Once there had been a first mover (Britain’s industrialisation) this altered the conditions for all subsequent cases, who both had to compete with and could learn from the earlier economic development that had occurred; and with each further case this was a fortiori true. (See also Swain 2005 and Tilly 2006.)

⁵ Two exceptions, at least on the issue of globalization, could be Aghion and Williamson (1998: chapter 3) and Frieden (2007). Tosh (2008) issues a more general call to his fellow historians to engage in policy debates. Taking more pragmatic steps, the History and Policy initiative (www.historyandpolicy.org) has, since 2002, organised a number of seminar events and published over one hundred ‘policy papers’ by historians exemplifying ways in which historical research and historical perspectives on contemporary policy issues can produce constructive and practical new ideas in the policy field or can offer equally constructive admonitions. Such efforts to link historical scholarship and policy concerns, however, remain the exception.

⁶ Though a parallel case does seem to exist in geography (where there is a corresponding consensus that “space matters”); for example, a recent World Development Report on economic geography (World Bank 2008) contained not a single advisor or contributing author who was a geographer (see Rigg et al 2009).

of institutions and history—we seek to extend and fortify the bridge connecting historians and development policymakers.⁷

The paper proceeds as follows. Section two clarifies key terms and concepts, and explores the basis on which historical scholarship—or “thinking in time” (Neustadt and May 1986⁸)—can potentially help to enrich the quality of contemporary development policy. It also provides a brief overview of the arguments and evidence that underpin the prevailing consensus among development economists and policymakers that ‘institutions’ and ‘history’ matter. Section three focuses on the different theoretical and methodological underpinnings of contemporary historical scholarship as it pertains to comparative economic development, arguing that in order for non-historians to engage more substantively and faithfully with the discipline of history, they must make a sustained effort to both understand historiography and appreciate anew the limits of their own discipline’s methodological assumptions. Being a historian is not just a matter of “knowing more” about a particular time, place or issue than others, but acquiring an entire sensibility about how to compile, assess and interpret evidence, substantiate causal claims, and understand complex (often interdependent) processes. Section four outlines some of the distinctive types of general principles and specific implications that can be drawn from historical scholarship, and considers their relevance for contemporary development policy. Section five concludes with suggestions for how the evolving dialogue between historians and development policy can be enhanced and sustained.

II. Thinking in Time Revisited: Can the Past Guide the Present?

This paper considers how and why history matters for contemporary development policy. For present purposes, we deploy the term ‘history’ to refer to both ‘the past’ and to the academic discipline of history. As such, we are concerned with drawing upon the deep reservoir of historical scholarship about the past (events and their interpretation) to help provide a more comprehensive body of theory and evidence for wrestling with contemporary development policy concerns. Our goal is not to articulate yet another popular list of “lessons from history” for development, but rather to offer some general principles and specific implications drawn from historical scholarship for more rigorously incorporating time, contexts, and complex processes of institutional change into development policy deliberations.

We acknowledge from the outset that many reasonable people contend that it is naïve, foolish or even positively dangerous to expect history (either ‘the past’ or ‘the discipline’) to speak to contemporary policy problems, especially those pertaining to highly controversial concerns such as ‘development’. The basis for such a stance includes beliefs that (a) ‘history’

⁷ Elman and Elman (2001) is a similar exercise seeking to connect historians and political scientists studying international relations, but with less emphasis on the implications for policy; see also McDonald (1996) and Sewell (2005) on links between historians and sociology. Most recently, see Lewis (2009), who correctly argues that “[t]he lack of historical perspective with development agencies stems partly from the pressures of development work in which activities remain powerfully (and understandably) focused on the promise of generating future change, but it is also part of a broader problem of ideologically controlled managerialism” (p. 42).

⁸ See also the related terminology deployed by Pierson (2004) for political scientists. Pierson (2005) provides a useful discussion on the history of ‘policy development’. Other political scientists writing within the field of ‘historical institutionalism’ (Thelan 1999, Mahoney and Thelan (2010) have also been influential, though less so in development policy debates.

does not and cannot provide such ‘lessons’ (i.e., it contains no teleological or Hegelian imperative), (b) that each time and place is unique (i.e., there are inherently qualitative differences between ‘then’ and ‘now’, and/or ‘here’ and ‘there’⁹), (c) that only those acting with great hubris imagine that ‘the future’ can be effectively guided by the deployment of human reason, or (d) that any such actions inevitably unleash—no matter how seemingly noble the initial intention or diligent the implementation—potentially harmful and irreversible unintended consequences. In this regard, historians are also conscious that (e) many of the twentieth (and previous) century’s most infamous tyrants (Stalin, Hitler, Pol Pot) justified their actions on the basis that they were acting in accordance with, or to actively fulfill, a destiny or mandate borne of historical necessity¹⁰. Similarly, (f) historians may distance themselves from policy discussions because of a concern that their hard-won research findings—sobering, nuanced and finely crafted as they are likely to be—are either ‘unactionable’ through prevailing policy instruments or may be used for purposes (whether by dictators or by well-meaning bureaucracies wielding only the crudest of de-contextualized policy tools¹¹) that they find distasteful and/or for which they wish to bear no responsibility. Finally, (g) large international development agencies, formed as they were during the height of modernization theory’s influence, contain an inherent imperative to embrace, implicitly if not explicitly, presumptions that there is a ‘single’ and/or ‘best’ path to modernity (embodied in the ubiquitous language of “best practices”), a notion most contemporary historians reject.¹² These are all legitimate concerns and we do not wish to make light of them. (We provide a more detailed response to these issues below.)

A more strident (but to our mind, unpersuasive) critique of our project would dismiss the very possibility that historical scholarship can be, even if it so desired, a basis for informing contemporary policy choices. For many students of postmodernism and cultural studies (see Jenkins 1991), for example, both the content and the epistemological underpinnings of orthodox ‘history’ are suspect at best, since (for these scholars) such history is merely a series of hegemonic, ex post rationalizations propagated by powerful elites, the accounts of the past re-imagined by ‘winners’ in the present to ensure their status remains unchallenged (and, in its most complete form, unchallengeable) by the ‘losers’ (Trouillot 1995). According to this view, ‘development’ is among the most egregious of subjects for historical inquiry (see Rist 2009), since its very logic perfectly embodies, enables and justifies attempts by powerful countries, companies and social groups to provide narratives about the virtuous factors (thrift, diligence, intelligence, innovation, courage) that underpinned their economic success while simultaneously obscuring the less savory aspects of that process (slavery, colonialism, exploitation, suppression, theft).¹³ Moreover, they argue, as part of this obfuscation, the mantra of ‘development’ enables the rich to lecture the poor about their putative political, cultural and moral failings, doing so as a pretext to encouraging (if not forcing) them to buy goods and resources (by going deeply into debt) and/or to adopt policy measures, institutional reforms and behavioral traits that they are told will surely correct these failings (but in fact will most likely serve only to further advance

⁹ This historicist position (i.e., that every time and place is unique and thus should be understood on its own terms) was the view of, among others, A.J.P. Taylor. On historicism, see Tosh (2002: 6-13, 182-5).

¹⁰ We are grateful to Dietrich Rueschemeyer (personal communication) for stressing this point. See also MacMillan (2009).

¹¹ On this issue see Scott (1998).

¹² On the history of modernization theory, see Gilman (2003). A fascinating historical inquiry into the notion of “multiple modernities” is provided by the contributors to *Daedalus* (1998, 2000).

¹³ For a related argument, see Goody (2006).

and consolidate the interests of the wealthy). These are not idle matters; several high-profile graduate programs in development studies (most notably in Europe¹⁴) are informed, implicitly if not explicitly, by these notions; not surprisingly, engagement by the leaders (and graduates) of such programs with policymakers, practitioners and staff of international development agencies—to the extent it occurs at all—is often characterized by deep suspicion.

In distancing ourselves from this view, we are nonetheless mindful that some postmodernist historians, such as those from Subaltern Studies, have taught us a great deal about issues that are at the heart of contemporary development concerns. Extending the longer tradition of “history from below” exemplified by the work of E.P. Thomson, Subaltern scholars have demonstrated, among other things, that colonial subjects developed intellectual traditions and movements that often ran counter to the dominant colonial discourse (Sarkar 1983), and that this laid the foundations of movements for social change.¹⁵ They have also shown, in kinship with Scott (1985), that persistent inequalities cannot be understood without acknowledging that they are often accompanied by modes of resistance that demonstrate the agency of the oppressed (Guha 1983). As we argue below, ideas such as these lie at the heart of what we believe historians can contribute to development policy.

Nevertheless, it is clear that the relationship between history and development policy is often a contentious one. If nothing else, this should perhaps be a preliminary non-trivial (albeit contrarian) conclusion of this paper, one set against the dominant prevailing view in policy circles that, as we shall see, offers a remarkably ‘clean’ story in which the desire to incorporate history into development policy is largely preoccupied with the search for the key structural ‘variables’ and/or ‘factor endowments’—property rights, disease vectors, press freedom, population density, types of natural resources, labor scarcity—that were associated with the origins and consolidation of institutions that promoted (or precluded) productivity growth and expanded (or restricted) economic opportunities and political liberties in the pasts of today’s developed economies and societies.

We believe the rich historical scholarship on comparative economic development has much to offer contemporary development policy, indeed that the quality and usefulness of such policy deliberations is much the poorer for its failure to be informed by a sustained engagement with historians. We recognize the concerns raised above regarding the potential dangers this engagement entails, are conscious that how we make sense of the past is itself an evolving exercise¹⁶, and concede that some of the historians whose work we discuss below may recoil at their inclusion in this project. Nevertheless, we argue that judicious efforts to “think in time”—i.e., to take seriously the scholarly research that specializes in disentangling complex interdependent processes as they have played themselves out in particular contexts across decades and even centuries—are a desirable and potentially fruitful basis on which to try to

¹⁴ Indeed, though it is rarely acknowledged as such, ‘development studies’ as an academic field emerged directly out of the managerial and administrative aspects of the colonial and post-colonial experience (see Kothari 2006 and Duffield and Hewitt 2009).

¹⁵ See also Sangari and Vaid (1990) and Rai (2002) on the interactions between colonialism and gender relations.

¹⁶ That is, that what constitutes ‘history’ and how it is invoked to make sense of the present is itself a subject of ongoing historical enquiry (‘meta-history’); on this see the extraordinary work of Burrow (2007). See also Sewell (2005) for highly stimulating, honest and vigorous discussions of what is involved in achieving dialogue between history and social science.

enhance the quality of the responses to some of the contemporary world's most urgent policy problems. Much of this work is entirely complementary to (but non-redundant with) the work of economic historians and 'historical economists', but much of it is also significantly different, not least with respect to the types of evidence and arguments it brings to bear. More immediately, historical sensibilities can also help to 'deconstruct' popular (and often very powerful) myths pertaining to a development organization's origins, mandate and approach, showing how, at key junctures, particular options among several came to prevail.¹⁷

The strongest argument for the importance of bringing history into dialogue with policy and policymaking, however, is that history is already there, all the time: the only question is what *kind* of history is going to be used. Without the explicit input of critical and reflexive professional historians, the 'history' which policymakers use is likely to be naïve, simplistic, and implicit, often derived from unconscious assumptions or vague memories; as such it is likely to be highly selective, used to suit predetermined purposes, and to be largely unverified.¹⁸ The (ab)use of history in this form not only represents a problem of commission but also of omission, in that it both invokes a defective and distorted rendering of history but also denies the policy process the vast reservoir of imaginative resources available from more formal historical research. We continually deploy historical memory in all forms of activity, often as unarticulated, framing premises. It is the role of the discipline of history to attempt to keep that memory sharp and rich, vital and challenging, not complacent and forgetful of the more awkward aspects of the past.

"Institutions Matter": A Brief Intellectual and Policy History

If historians and their discipline can help provide useful and distinctive insights into contemporary development policy, it is instructive to examine the arguments and evidence put forward by those who have done the most to establish the contemporary 'consensus' regarding the importance of institutions and history for development policy—i.e., the "new" institutional economists. If historians are to demonstrate their value to this discussion, we need to be clear about the current state and terms of that discussion.

In many respects, the study of institutions and their contribution to development is as old as economics itself (see Bardhan 1993). Adam Smith, in both *The Wealth of Nations* and *The Theory of Moral Sentiments*, repeatedly stressed the importance of what we would now call political, legal and social institutions for making possible spectacular gains in productivity and exchange, and institutions of various kinds featured prominently in the accounts of

¹⁷ In a more compressed time frame, this is the task undertaken by Porter, Allen and Thompson (1991) and Mosse (2005) in their insightful analyses of development projects.

¹⁸ The broader point here, as Charles Tilly frequently points out (e.g., Tilly 2002), is that all of us are 'proto historians' in that we are inveterate storytellers: every individual, group, organization and nation must compile a coherent biographical narrative to make sense of itself to itself and to others (and itself *in relation to* others). These narratives are also called upon to inform, explain or justify particular 'policy' decisions going forward. Thus one of the useful (if sometimes controversial) contributions that historians can make to development policy is to help render such narratives explicit and, where necessary, identify both alternative narratives and the reasons why particular narratives prevail (and others do not). These are far from trivial issues; as current events in Afghanistan, Kosovo, Sudan and Kenya attest, they can be the basis of especially pressing (even deadly) political dynamics. (For numerous other examples, see Black 2008.)

‘development’ offered by Marx and Weber in the nineteenth century. From the late nineteenth century until the late twentieth century, however, the mathematical turn in economics saw institutions recede from center stage in that discipline. In the late twentieth century it was primarily the pioneering work of Douglass North (1982, 1990)¹⁹—and the subsequent availability of vastly greater computing power and more comprehensive datasets on institutional quality and economic performance—that enabled first the idea of ‘institutions’ and then the measurement of them to re-enter economic theory and the practice of development (see Lin and Nugent 1995). The initial empirical studies by, among others, Shleifer and Vishney (1993), Putnam (1993), Mauro (1995) and Knack and Keefer (1995) inspired literally hundreds of follow-up efforts to expand and refine economists’ understanding of how institutions shape (and in turn are shaped by) economic growth, poverty reduction and all manner of other development outcomes (e.g., conflict)²⁰. Institutions, and their operational counterpart of ‘governance’, are now (back) at the center of the development enterprise. This is as it should be.

In its simplest terms, these studies have yielded empirical support for the importance of institutions of various kinds, but most especially ‘property rights’, characteristically defined as the exclusive capacity of those (individuals or collectives) possessing intellectual, physical and natural assets to use, transfer and realize the economic value of those assets, usually through access to clear and legally enforceable titles. The presence in a given country of independent judiciaries, mechanisms for constraining corruption and abuse of executive authority (‘the rule of law’), ensuring the non-repudiation (or at least predictability) of contracts, and procedures for ensuring the non-violent transfer of political power are all now standard referents for what is meant by ‘institutions’ (Clague 1997). In development policy circles, these items are usually grouped together as part of a broader discourse on the importance of ‘good governance’. In conjunction with, indeed fuelled by, the comprehensive expansion and refinement of efforts to formally measure institutions,²¹ these renderings (or variations thereof) are now thoroughly embedded into everyday development research and policy debates; it is in this sense that we now have a ‘consensus’ on their importance. Again, to be clear: for the purposes of this paper we are not disputing these definitions per se or challenging their salience for development; for the most part, our individual research efforts only confirm their significance. Our concern, rather, is with better understanding the processes and mechanisms by which any of these ‘institutions’ in the abstract came to take specific concrete forms in particular times and places, how political and social processes of institutional change were encouraged and/or thwarted, and what such understandings might tell us about contemporary policy efforts to ‘improve’ institutions in settings often far removed (geographically, culturally, politically) from quantitative data, on which those understandings were generated.

¹⁹ Many would also want to credit the pioneering work of Olson (1965) with inspiring the revival of interest in institutions, especially as they pertain to the management of ubiquitous ‘collective action’ problems and the provision of public goods.

²⁰ To cite only a few among hundreds of contributions, see Clague (1997), Rodrik (2003) and Easterly (2001).

²¹ The most visible empirical manifestation of these general features of ‘good governance’ are the six widely-cited measures of institutional quality developed by Daniel Kaufmann and Aart Kraay (and their collaborators) at the World Bank (see, only most recently, Kaufmann, Kraay and Mastruzzi 2009). Andrews (2008) provides a powerful critique of this approach.

For all the attention garnered by the impressive quantitative studies documenting the importance of institutions for understanding contemporary economic performance,²² by the early 2000s the prevailing policy discourse was recognizing that institutions themselves clearly had not arrived overnight; they must have ‘evolved’ over time, whether they were now consolidating unhappy outcomes (e.g., high inequality, slow growth, civil war) or encouraging more virtuous ones (poverty reduction, service delivery, participatory democracy). The key empirical and policy questions then became: Under what conditions do ‘good’ and ‘bad’ institutions emerge?²³ If countries find themselves with ‘bad’ institutions, what can be done by whom to move things in a more constructive direction?

Into this conceptual and policy space stepped an array of impressive studies by economic historians and historical economists that were both intuitively appealing and empirically novel. Engerman and Sokoloff (2002, 2008; see also Sokoloff and Engerman 2000), addressing this phenomena in a comparative analysis of the divergent fortunes of North and South America, argued that the key lay in the types of natural resource endowments, since in both regions land was abundant and labor was scarce. Where the climate was conducive to the cultivation of crops that required large amounts of labor in order to be profitable, such as sugar, colonists resorted to the subjection of local populations (in South America) and/or the importation of slaves (the southern states of North America), in the process institutionalizing laws and social relations consolidating high inequality and elite dominance; where profits could be optimally gained by other means—that is, where the climate supported different kinds of crops or industries requiring different kinds of skilled labour (in the northern states of North America)—then colonists sought instead to attract immigrants and put in place more equitable legal, political and socio-economic arrangements. In these general terms, divergent “paths of development” were thereby set in motion, which, over several centuries, culminated in two qualitatively different development experiences in the Americas.²⁴

These debates accelerated considerably in terms of both scale and impact with the arrival of a series of seminal papers by Acemoglu, Johnson and Robinson (2001; 2002; 2005), who provided both a seemingly neat empirical solution to the enduring problem of establishing a causal link (courtesy of a new dataset on settler mortality) between institutional quality and development performance, and an explanation for what they termed the ‘great reversal’—the fact that countries that were relatively rich in 1500 were now amongst the poorest today, and vice

²² Most cross-national time-series datasets on institutions and economic performance begin around 1960, an artefact of when selected UN agencies began to collect (and coordinate the content of) the relevant figures from national governments. This was also, not coincidentally, a time when economists began to supplant lawyers as the dominant figures in public policy (on this see Markoff and Montecinos 1993).

²³ Most historians, of course, would refrain from a deploying a normative discourse of ‘good’ and ‘bad’ institutions; we use these terms at this point simply because it reflects how the debate is largely framed in policy discussions.

²⁴ An equally important paper by Sokoloff (1987), though one less influential in policy circles, sought to explain the divergent paths by which patent laws had evolved in the US and UK. Though the law in the former was ostensibly modelled on the latter, markets and social norms in the US proved to be much more open to participation by the lower classes than those in the UK (and within the US, more open in the north than the south), over time generating both different laws and different groups of patentees. Subsequent analysis showed this to be true of laws pertaining to land, suffrage, education, credit and local government (see discussion in Hoff 2003).

versa.²⁵ The explanation was that the colonizing powers encountered vastly different environmental settings, which shaped the length and terms of their engagement with local populations and natural resources; this in turn gave rise to very different incentives to erect particular forms of institutions. Colonial settlers established a legacy of ‘good’ (inclusive, prosperity-enhancing) institutions in places where they committed to settling in large numbers for long periods, in the process enacting and upholding private property rights; they did this in places where they were engaged in tasks where land was abundant and that required relatively little labor, and (most importantly) where disease burdens were low (Australia, New Zealand, Canada). ‘Bad’ (exclusionary, prosperity-stifling) institutions, on the other hand, were established in less hospitable environments, where the goal became one of enabling a small population of foreign transients to extract natural resource wealth as quickly and cheaply as possible; in such places, institutions—especially those pertaining to land ownership, civil liberties and conflict management—emerged that greatly concentrated political power in the hands of a powerful, dynastic elite, a process that, over time, led initial inequalities to accumulate and perpetuate, and broad growth processes to be thwarted (Latin America, Africa). In subsequent work, this general story has been refined and updated to account for (among other things) the persistence of sub-optimal institutions in the face of considerable gains to implementing ‘better’ ones; for example, institutional changes that could potentially generate economic expansion for large sectors of the population get blocked in those countries where political elites fear such change will lead to their replacement²⁶.

We stress again our respect for this work and our appreciation of the important contribution it has made (and doubtless will continue to make) in encouraging economists and development policymakers to recognize the important ways in which the past shapes the present²⁷. Absent such research, it is unlikely that development agencies would have begun to engage with these issues as seriously as they have. The acceptance and impact of this type of research in development policy circles, however, is in large part a function of the fact that it strongly comports with (even as it imaginatively expands) the canonical theories, assumptions and methods of mainstream economics research. This is, of course, absolutely fine if one is working within that epistemological space and gives greatest credence to research findings emanating from it, but it is *not* fine if one believes that, by absorbing such material, one has learned most of what is important from history and historians about development. The getting of historical wisdom is a qualitatively different task, yielding insights that are in large part a product of different methods, emphases and theories about issues ranging from processes of social change and the salient characteristics of context to how one substantiates causal claims and

²⁵ See Austin (2008) and Bayly (2008) for a more extended substantive engagement with these papers; for a methodological critique and alternative empirical strategy for explaining the Latin American case (using comparative case study analysis), see Katz, vom Hau and Mahoney (2005).

²⁶ See Acemoglu and Robinson (2006a), formalizing Gerschenkron (1962), and Acemoglu and Robinson (2006b), formalizing Moore (1966).

²⁷ A third strand of work pursued by a different group of ‘historical economists’ have argued that the differences in development trajectories between post-colonial countries were a function of whether they were bequeathed common law (British) or civil law (French) legal systems—the former seemingly generating more positive development outcomes than the latter (see La Porta, Lopez-de-Silanes and Shleifer 2007; La Porta et al 2008; Glaeser and Shleifer 2002)—but this view seems to have gained little policy traction. Even if this result is empirically correct, it’s not at all clear what the plausible and supportable policy implications are. Nunn (2009) provides an interesting (if rather too deferential) review of this strand of the literature.

works with evidence that may number only a single episode.²⁸ To do justice to this range of material entails a different set of commitments and sensibilities to those generating the recent influential work from economics; without them, we argue, development policy is the poorer.

III. The Craft of Historical Scholarship: Historiography, Context, Processes

If much of the recent work by economic historians, ‘historical economists’ and popular historians inadequately reflects the diversity and distinctive content and sensibility of scholarship by historians, then what are the defining features of this scholarship? What implications does it have for development policy? We turn next to seek some answers to these questions.

Arguably the primary feature of historical scholarship is its method, or its historiography (see Breisach 2006). ‘The past’ as measured by ‘time’ is not just another ‘variable’ to be included in a regression to thereby discern its ‘significance’ (though of course certain variables can certainly be assessed in this manner); nor is it a matter of searching for this or that large, measurable variable (or variables) from the past that can be used to plausibly explain the present. Rather, historical scholarship is primarily about locating, drawing upon and integrating different types and sources of material—much of it fragmentary (in quality and scope), textual and scattered across different domains—in order to discern coherently the specific processes and mechanisms by which one historical moment influences another. Even as most historians share with social scientists a commitment to generating and testing hypotheses (i.e., to inductive and deductive reasoning), and recognize that the veracity of a given explanation is stronger the larger the number of cases it can explain²⁹, the canonical skill of the historian is being able to immerse oneself sufficiently in the full context of a period or a juncture faced by those in the past that they can recreate the openness to the alternatives that were available at that time, in the way that our own future is currently indeterminate to us today. As such, their task is to explore what other outcomes were plausible, and how particular combinations of actors, structures and events coalesced or not (for whatever reason or reasons) at a particular moment to give rise to the outcome that did occur rather than another³⁰.

Getting oneself in a position to be able to make and defend such declarations requires not only ‘deep’ immersion in and familiarity with the time, place, and circumstances in question, but a capacity to distil from the array of available (usually highly imperfect) source material the components of a coherent and empirically based argument. It is in this manner that historians make—and assess one another’s—causal claims³¹. For many of the episodes under

²⁸ Diamond and Robinson (2010) present a range of interesting historical studies of development processes, seeking to exploit ‘natural experiments’ to more accurately identify causal mechanisms. In principle this is a clever and welcome innovation, though it is unfortunate that the volume is premised (in the Introduction) on a pejorative claim that historians are weak at mathematics and thus suspect at making causal inferences.

²⁹ In this sense, historians have much in common with social theorists (see Tosh 2002, Chapter 8); it also explains why the work of Polanyi (1944), Moore (1966), Bendix (1977) and Skocpol (1979) has been so enduringly influential in sociology and political science.

³⁰ This task is what political scientists call ‘process tracing’ (see George and Bennett 2005). One could also argue that such a task is, in effect, a search for plausible counterfactuals—that is, what could or might have happened but for the presence of a particular factor (or combination of factors) at a particular moment. On case study research methods in particular, see also Gerring (2006).

³¹ The changing basis of causal claims is itself, of course, a fascinating subject of historical enquiry (see Kern 2006).

consideration, the number of available cases may be very few—e.g., there was only one French Revolution—but this does not mean that historians are unable to identify (or at least make reasonable assertions about) what ‘caused’ what.³² Needless to say, this modality of causal reasoning is considerably different from that in econometrics (and policy deliberations more generally), where statistical power and (relatively) clear procedural techniques for discerning the effects of an independent variable, controlling for other variables, on a given dependent variable constitute the prevailing frame of reference. It is this frame of reference that, faced with an imperative to recognize that ‘history matters’, finds itself strongly predisposed to buy into the findings and arguments of the ‘historical economists’ over those of most other historians. Such frames also preclude taking seriously the power of ideas, rituals, ideologies and symbols in affecting outcomes (because they cannot adequately be ‘measured’), and for similar reasons strongly favors an empirical focus on factors of production (such as ‘property rights’) rather than changes in preferences.³³

For development policy purposes, historiography—or, by implication, the recognition that there is more than one way to make and substantiate a causal empirical claim, especially as it pertains to time—is only the first of three significant analytical contributions that history can contribute. The second is appreciating the importance of context. This is another idea on which there is increasingly broad agreement—i.e., few would dispute in the abstract that “context matters” for effective development policy—yet in practice it is largely honored in the breach. As Scott (1998) has argued, throughout the twentieth century the kinds of social and economic knowledge found to be most useful to the imperatives of state-sponsored planning (or what he terms ‘bureaucratic high modernism’) for ‘development’ purposes has been knowledge that takes a de-contextualized form. Indeed, there has been a sense in which only forms of knowledge (including theories) which appear to be able to predict outcomes, regardless of local contexts, can be considered sufficiently ‘scientific’ and powerful as to be relied upon for guidance by decision-making funders, officials and ministers. Yet this creates the self-defeating problem that such forms of context-free policy science are therefore severely handicapped as detailed guides to practical action in any particular context—with its specific local conditions and history—since these have been excluded by design from the policy model (Szreter et al 2004: 12-13). It is thus a form of knowledge strongly predisposed to favoring either technocrats (i.e., a few smart people, usually called ‘experts’³⁴) or standardized, uniform procedures; by contrast those decisions in development policy—and they are legion—that require instead both large amounts of highly localized expertise (discretion) *and* numerous people-based transactions (i.e., those that require a careful response to the idiosyncrasies of local contexts) are inherently more complicated and are (*ipso facto*) simply seen as a root cause of ‘project failure’ (Evans 2004, Pritchett and Woolcock 2004, Rao and Walton 2004). Giving more than lip service to the importance of ‘context’ requires not just an anthropological focus in the present but a historical sensibility regarding how the present came to be what it is, and how in turn policy actions in the present might shape future trajectories. Similarly, given that implicit and/or explicit historical claims are routinely invoked to explain contemporary development problems (and justify corresponding policy solutions), the

³² On this point see Goldstone (1998), Mahoney (2000), and Trachtenberg (2006). Mahoney, Kimball and Koivu (2009) provide the most comprehensive overview.

³³ See Rao and Walton (2004) for more on this point.

³⁴ On the role of agricultural ‘experts’ in shaping “agrarian doctrines of development” during British colonialism, see the masterful analysis of Hodge (2007).

incorporation of serious historical scholarship can help to sort out the sense and nonsense in such claims.

The third significant analytical contribution that history can make to development research and policy is helping to better understand process concerns. As with context, scholars and policymakers appear to be giving increasing importance to this issue—e.g., by stressing the importance of “getting inside the black box” to address the mechanisms by which cause gives rise to effect, and by slowly giving space to “process evaluations” in considerations of project effectiveness—but actually doing so is largely precluded by the dominant methodological practices in econometrics. For historians, taking process issues seriously is not a matter of compiling time series or panel data sets (though these may be useful in their own right) to track changes over time, but rather exploring in detail the specific contingencies by which the dynamics of an evolving set of actors, events and institutions come to coalesce (or not) at a particular time and place, and thereby shape future action³⁵ (indeed, how such actions can shape the salience of actions and events, actual or imagined, in the *past*). The most consistent ‘lesson’ from historical research on the study of process concerns is not just methodological (i.e., how to do it carefully and defensibly) but substantive—that is, that certain policy intentions usually give rise to a host of different outcomes, some intended and some unintended, and, conversely, that observed outcomes can themselves often be a product of multiple factors (intended and unintended, observable and unobservable, known and unknown).³⁶

Finally, historical research can also help alert development practitioners to the fact that the shape of the ‘impact trajectory’ that policy interventions take over time, much of which—especially in matters pertaining to social and political reform—is often likely to be anything but monotonically increasing and linear³⁷ (which is the default assumption in contemporary development policy debates, especially those pertaining to impact assessment)³⁸. Discerning

³⁵ These considerations go far beyond the now ubiquitous concept of ‘path dependence’, a term originally coined by economic historian Brian Arthur to refer to the manner in which certain technological choices (the most famous being the QWERTY typewriter) persisted long after their initial efficiency superiority had been surpassed, because of the manner in which they had become engrained in education systems and everyday practices (see David (1985) and the references to Arthur therein). Putnam (1993) and others popularized the extension of this idea into the institutional and political analysis of development trajectories, a step too far for many historians and social scientists (see, for example, Tarrow 1996).

³⁶ The tendency of economists to search for mono-causal explanations is more a consequence of their quest for perfect econometric identification than a result of actually denying the possibility of multiple causes. However, there is far too little public acknowledgement that virtually all ‘findings’ in econometric studies are subject to this.

³⁷ Most economists, importantly, don’t rely on linear explanations because they think they are inherently right; it’s just that non-linear econometrics is much harder and requires vastly more data than is usually available (especially in development research). The work by Acemoglu, Johnson and Robinson (2001, 2002) is not a linear explanation per se as much as what is known as a “discontinuity”—i.e., identification of a substantial break with the past that is used to explain the emergence of a shifting trajectory.

³⁸ Consider, for example, research by Brown (2006) and Hochschild (2006) on efforts by social reformers to end slavery in the British empire in the early nineteenth century, which shows how persistent and innovative campaigning (using techniques that endure to this day) eventually—despite decades of failure, rejection and hostility—eventually gave way to relatively rapid global reform. What if certain development efforts today (e.g., post conflict reconstruction) are on a ‘J-curve’ path like this? How would we know? It’s hard to name a single development intervention for which there is clear empirical evidence of its known impact trajectory over time, which is to say, the development fraternity is conspicuously ignorant of the processes underlying even its most

empirically the likely non-linear trajectory of women's empowerment initiatives, for example, or political and legal reform, and the manner in which they are influenced by scale and context, may not be tasks for which one would immediately hire an academic historian, but it is the absence of a serious historical *sensibility* among development policy administrators that contributes to normative expectations strongly favoring development projects whose impacts are large, immediate, knowable, predictable and positive, and (preferably) independent of scale, duration and context (i.e., they should be social technologies).³⁹

If methods, context and processes are the key analytical contributions that historians can make to development research and policy, then it is instructive to consider concrete examples of research by historians that exemplify these characteristics and that can, in conjunction with the deeper wellspring of research by historians over the centuries, be the basis for a more specific articulation of principles that historians can contribute to development policy deliberations. A central challenge of social science and development policy—measurement—provides one such instructive example.

Lessons from a Brief History of Measurement

History repeatedly reminds us that policy interventions can lead to significant change in ways intended by the policymaker, but can sometimes have thoroughly unexpected consequences. Take the imperative to count and measure human beings. In 16th century Britain, Thomas Cromwell, Henry VIII's vicar-general, introduced a system of identity registration that required all births, deaths and marriages to be recorded in parish registers. He said that this was "for the avoiding of sundry strifes and processes and contentions arising from age, lineal descent, title of inheritance, legitimization of bastardy, and for knowledge, whether any person is our subject or no" (cited in Elton 1972: 259-60). As Szreter (2007) shows, this resulted, for the first time in Britain's history, in citizens having an enforceable right over their identity. It was used by individual citizens to verify their property and inheritance rights and by local communities to verify social security claims. This facilitated the effective functioning of a nationwide social security system and a mobile market in both labor and capital, contributing to Britain's pioneering process of economic development. Seemingly small-scale institutional change, in short, can have significant unintended consequences that can contribute to shifting a country's destiny.

Another example of this is a recent paper by Vincent (2009), which shows how measurement mattered in generating improvements in literacy in Victorian England.⁴⁰ The idea that literacy could be measured came from the realization by a man of letters (who had been appointed Registrar-General) that simply counting the number of people in the population who could sign their name was an effective measure of the ability to write. Since all marriage registers required signatures, this data was immediately available and utilized to measure spatial

celebrated interventions, and has little knowledge of how these impacts are influenced by scale and (different types of) context (on this point see Woolcock 2009).

³⁹ These pressures, solidly reinforced by campaigns such as the Millennium Development Goals, manifest themselves in calls to "scale up" and "replicate" putatively successful interventions.

⁴⁰ See also Clemens (2004) for a historical perspective on the feasibility of attaining the education Millennium Development Goal.

and class disparities in literacy. While the limitations of the measure were also recognized, the low levels of literacy led to demands from the bottom, via the radical press, to equalize the supply of education. The data were analyzed to comment upon its link with violence, “moral health” and the “rational enjoyment of blessing.” This led to improvements in both the demand and supply of education, to the extent that in the 19th century one generation was on average 20 percent more literate than its predecessor, a fact that is currently evident in many developing countries. It was, at the same time, recognized that having one literate family member was often sufficient to confer a high level of benefits. The rise in literacy was thus not just the result of efforts in public education but the widespread market for private schools with untrained, unofficial instructors. It was not until 1880 that all parents were required to send their children to “inspected” classrooms with compulsory attendance, with a simultaneous increase in public funding for education. The experience of the UK, where the concept of public education was largely developed, shows, therefore, that the measurement and identification of literacy was important in establishing a social compact to ensure that basic education was made universally available.

On the other hand, Cohn (1984) and Dirks (2001) have argued that when caste identification was introduced into the Indian census for the first time in 1871 by British administrators, the process of translating the fluid local dynamics of caste into a finite number of standardized quantitative census categories hardened the caste system and “created” a new form of caste, one that was amenable to quantification, less fluid, and easier for policy makers to “manage”. It changed, in other words, the very nature of caste. This had the unintended consequence of sparking lower caste social movements because low-caste social reformers were made aware of their large proportions in the population and they used the new categories to mobilize disadvantaged groups against discriminatory practices and towards greater rights. The policy imperative to ‘measure’ thus can lead to powerful social and economic changes, sometimes intended and sometimes unintended. As Susan Bayly (1999) has shown, this process of categorization and recategorization of caste has been part of the political economy of India, dating back to at least the 16th century, when local caste structures were modified every time a new ruler arrived and imposed different systems of tenure and revenue generation. This process continues today (see Rao and Ban 2007), with the caste structures influenced by processes as diverse as affirmative action, social movements and local politics.

IV. How and Why History Matters for Development Policy

There are broadly three ways in which history matters for development policy. The first is through its insistence on the methodological principles of respect for context, process and difference when addressing the study of societies and policy efforts to bring about change in them. History views change as a complex causal process requiring a diversity of forms of knowledge, and a corresponding variety of methods for acquiring and interpreting that knowledge. Second, history is a resource of critical and reflective self-awareness about the nature of the discipline of development itself, its current preoccupations, why those preoccupations (and not others) have come to take their present form, and how they differ from past motives and aims, along with the crucial issue of how particular sources and forms of evidence are rendered salient. Third, history brings a particular kind of perspective to

development problems; it is a vantage point for framing and viewing the nature of development which is relatively long-term and comparative.

Beyond these three broad categories, one can identify eight more specific ways in which engagement with history—both the past and the discipline—matters for contemporary development policy.

First, recent work by social historians has centered on understanding the ways in which institutions come to take their prevailing form. Their analyses of these processes have stressed the significance of ‘hybridity’, which refers to the variety of sources of ideas, borne of intense two-way interactions between colonies and rulers (and others), that coalesce to inform the distinctive content of institutions (Bayly 2008; see also Benton 2002). These interactions characteristically emerge through a political process of contestation, and thereby have a content and legitimacy they would (and could) not have had if they had been singularly ‘imported’ from elsewhere. In this sense, even if the end-state form of the institution in question happens to be similar to that of one elsewhere, it will nonetheless be qualitatively different for having been forged through a domestic political process. Such a process certainly does not ensure that prevailing institutions are equitable or optimal—clearly many such institutions are the antithesis of this—but it does require researchers and policymakers alike to take seriously the recognition that the details of institutional design matter, and that these details are a product of idiosyncratic exchange processes. Key mediators of the indigenization of ideas have been called “peer educators” (Rao and Walton, 2004; see also Harper 2009); these are people who transform the meaning of the idea—often in the past this has been via a nationalist imperative—to make the idea their own and then transmit it within a country. “Investing” in peer educators should thus be a central component of institution building and reform efforts.

A prime example of hybridity is the Muhammadiyah, a modernist Islamic movement that arose in Indonesia as a response to Dutch rule and which was at the forefront of the democratization process. It clearly had an important influence on institutions (in North’s sense of institutions as comprising ‘the rules of the game’) that came to influence the concepts of private property, normative beliefs in commercial and market relations, and ‘the rule of law’. These each took on the variant and hybrid forms they did in Indonesia partly as a result of the influence of the Muhammadiyah movement (Heffner 2000); similarly, the Dalit movement in India, which led to the rise of lower castes competing effectively within democratic structures, is also an important hybrid institution (Omvedt 1994). Hybrid institutions, which have been central to development processes in Indonesia and India (and elsewhere), also served as indigenous mechanisms of accountability, and a key part of developing indigenous capacity is to look outside western frames. In many societies, for instance, religious organizations are a central part of civil society, i.e., as both service providers (schools, hospitals) and potentially part of the social accountability process. Such a rendering may complicate our very understanding of what “institutions” are, but too often in contemporary development policy circles our deployment of this term—as in discussions about the centrality of “property rights”—believes the historical reality of the many and varied ways in which prevailing institutions came to be.

Second, in order to be cognizant of hybrid processes and to build indigenous capacity, it is important to understand how, why and through whom such processes come about (or not)

(Bayly 2004). The role of elites is central here because they lead the process of hybridity and indigenization. Peer educators are drawn from their ranks, and good development strategies cannot be implemented without the support of commercial (entrepreneurial) groups. Elites also play an important role in forming a free press, civil society organizations, and other important elements of an indigenous public sphere that form a “critical public” that in turn constitute indigenous mechanisms of accountability. As Bayly points out, development is partly a “morale-raising” process, and “people need to believe that they can succeed and that their own societies are essentially benign” (2008, p. 17). However, history also suggests that broad-based and enduring improvements in living standards are facilitated when greater equality and empowerment is wrested from elites (cf. World Bank 2005); this process can be gradual and peaceful and/or mired in war and revolution. This reiterates the importance of forging accessible feedback mechanisms and legitimate political channels through which dissent can be aired before getting out of control.

Third, development demands a constant exchange between the center and the periphery—that is, between the capital city and provinces, between central and local governments, even at times in the past between colonizers and colonies (Wong 2008). This is a key system of accountability, particularly in non-democratic contexts; because of demands for spatial equity, it is key to learning via transfers of information and experimentation, and (of course) to the processes of hybridity and peer education. This teaches us that multilateral and bilateral donors should rely more on learning from innovations in their client countries rather than focus on a uni-directional transmission of “knowledge” (see also Rodrik 2007).

Fourth, going along with the temporal realism lesson, history teaches the non-linearity and conflictual nature of economic development.⁴¹ History clearly shows that nothing is so disruptive and dangerous to the health of the mass of the population *in the short run* as economic growth, particularly the transformative kind associated with initial stages of what (later) proves to be national economic development (Szreter 1997, Easterlin 2004). Similarly, wresting power from entrenched elites is highly likely to result, in the short run, in periods of conflict and stagnation before sustainable and equitable growth can be achieved.

Fifth, the historical perspective and associated research can frame our understanding of development problems in ways that would not be obvious in the absence of such an understanding. For instance, historical research can reveal long-term shifts in tastes, ideology and beliefs, issues which economists, despite recent advances, abstract away from (see de Vries 2008). Shifts in “preferences” are embedded within economic, political and social transitions, and understanding what drives them could help give us a more complete understanding of development processes. More significantly in this respect, history can uncover important aspects of the past development history of today’s developed economies that have been overlooked or are being unjustifiably ignored by development theorists or practitioners today. An example here would be Ha-Joon Chang’s (2002) insistence on recalling that virtually all of today’s leading economies—including the United Kingdom and USA as well as more well-known cases, such as Germany, France and Japan—all operated protectionist regimes to protect or promote infant

⁴¹ This is a central thesis of Skocpol (1979) and Moore (1966); see also Joseph Schumpeter’s (1975 [1942]: 82) oft-cited description of economic growth as a process of ‘creative destruction’ and Walter Lippmann’s (1982 [1929]: 51) arguments regarding the ‘acids of modernity’.

industries when they were in their infancy in terms of national economic development, as did Korea, China, Japan and other high-growth countries. Of course, the Gershenkronian principle suggests that this does not necessarily mean that this is a valid policy for all late-industrializers today, but a more complete historical awareness would make it incumbent on development policymakers today to articulate the positive case for making such infant economies open to free trade today when they were not in the past, rather than simply assuming that free trade is always and everywhere the right policy to foster their economic development. More generally, following Gershenkron (1962), there were a range of economic, geographical and politico-cultural reasons why some parts of the world industrialized first or earlier than others; and that in itself also ensured that those coming later to the feast of economic growth had to do it differently. As such, a primary lesson of history about development is that there are necessarily as many different national pathways to development as there are national economies.

Sixth, if all development policy makers and practitioners had to read serious scholarly accounts by historians of successful national economic development in the past they would come to a sobering realization of the kind of time-scale they should be envisaging for their policies and plans to come to fruition. They would realize that units of time of approximately a half-century and certainly at the very least a quarter-century are required. Policy horizons of five years and even of ten years are, frankly, painfully and unrealistically short to anyone acquainted with economic history (see Blanning 2007).

Seventh, history can provide development studies with an historical understanding of its own history as a discipline and of the field of development which it has been operating in. History can provide the discipline and practitioners with an important memory function of its own rich store of past successes and failures, of productive and unproductive ideas. What history can offer is an independent, professional, critical and thoroughly-researched record of the discipline's past, not a set of anecdotes or the preferred history of a few powerful figures in the field, which is the kind of amateur understanding of a discipline which circulates without proper historical enquiry and which almost always simply conveniently reinforces the prejudices and perpetuates the blind spots of the present generation. Development is certainly not a science, so yesterday's now-discarded or forgotten ideas are not necessarily entirely obsolete. An example here would be Lewis's seminal paper (Lewis 1954) on the problems of attempting to promote economic development with unlimited supplies of labor. Indeed, this paper is doubly significant for the historical memory of the discipline of development studies since it, arguably, founded the sub-discipline of development economics. It is intriguing, therefore, to note that Lewis's profound sense of the importance of history was foundational for the discipline, since Lewis's 1954 thesis was formulated as a resolution to an economic conundrum concerning the industrial revolution in England. Given the conditions prevailing in most of the world's poor countries and in many mega-cities today, an historical sensitivity would identify an analogy (certainly by no means a perfect one) between these present circumstances and those prevailing in the agrarian economies of poor countries when Lewis was thinking of these problems in the late 1940s and early 1950s. Historians are perfectly willing to enter into active and profitable debate with the great thinkers of the past; indeed, the whole, highly respected sub-field of the history of political thought or 'intellectual history' does this all the time.

As we demonstrate in the examples above, part of the self-critical historical awareness which history can offer to development policy is to be aware of the ways in which the data it frequently uses has been constructed through historical processes and negotiation in the recent or even quite distant past of several decades ago. All empirical data is classified, categorized and constructed; sometimes it can be very important to know how and why it was rendered into the form we now use. All the quantitative data used in economic and other social science models has to be constructed, often by government agencies. There is, in all cases of data construction, a complex history behind exactly how that data is produced in the way that it is. This inevitably feeds various biases, and filters into the way in which economists and those working on development problems can see the problems they are working on. Data collection is not a neutral activity; it can mobilize social movements and spark important social changes.⁴²

Finally, eighth, in relation to the pressing problems of the environment—problems integral to world economic development for which neither economics nor policy makers themselves have any simple solutions—history should also be investigated as one among many resources with which to think imaginatively about this challenge. History has many episodes of environmental degradation to investigate, some of them very carefully documented, such as the evil of ‘mining’ estates in the early modern period, deforestation throughout western Europe, and desertification (see McNeil 2000, Kula 2001, Burke and Pomeranz 2009).⁴³

V. Conclusion: The Past as a Foreign Country

In matters pertaining to development and national economic growth patterns, ‘path dependency’ has been the concept which has won an enduring acknowledgement among economists and other social scientists signifying that ‘history matters’. However, ‘path dependency’ can be a profoundly misleading way to understand the role of history. To the extent that the notion of path dependency can be invoked to mean that a set of historical events and institutions in a country’s or region’s past have exerted a deterministic influence upon its subsequent history, then this is a a-historicist viewpoint which no professional historians would wish to endorse. Paradoxically, to invoke path dependency in this manner merely commits the mirror image fallacy of ignoring history entirely, by suggesting that certain selected aspects of the historical past are inevitable destiny.

Historians see history as constitutive of the present, not determinative of it. If for no other, this is for one very important and powerful historiographical reason, namely that historians believe that it is through the study of the past that we continually modify our understanding of it and so shift our relationship with it. That is, after all, the fundamental rationale for the discipline; the past is never finished and complete. While the discipline of history lives as a practice, it is

⁴² It should go without saying that this point, and the arguments raised in the paper as a whole, should not be interpreted as hostility on our part to quantitative analysis; far from it. Our claim, rather, is that quantitative analysis (a) too often assumes an air of sophistication purely because it is quantitative, and not because it is a product of serious engagement with contextual realities and idiosyncrasies, and (b) is not inherently a more ‘objective’ mode of inquiry—as we have shown above, it can and does have unintended political consequences.

⁴³ Also see, for instance, Roodhouse (2007) on the WWII national emergency responses to severe shortages, especially lessons on how politically and culturally to ‘sell’ rationing to a democratic populace and how to get compliance and enforcement.

always subject to alteration and revision; in this sense, the ‘path’ itself is re-made anew by each generation of historians. To give one extremely important but simple example of this, we can point to the revolution in our understanding of the nature of the first ever case of modern economic development on a national scale, the transformation of the British economy into the world’s first commercial, industrial and imperial power. As recently as the early 1970s it was still an unchallenged orthodoxy that this was essentially a highly compressed episode of explosive activity taking place between 1780 and 1850, driven by science, technology, rapid capital accumulation and soaring population growth due to falling mortality. This led to the fashion of the time for focusing national economic growth plans on increasing the capital-output ratio. Due to a veritable historiographical revolution, however, by the end of the 1980s an entirely different view has emerged, which continues to be the orthodoxy driving further historical research today. This sees British economic transformation as a process which was occurring across a quarter of a millennium, c.1600-1850, with a wide range of institutions increasingly seen as each playing a crucial role, such as the character of the fiscal state, its protectionism, the universal social security system that was created, and the unusual laws of property and marriage.⁴⁴ Some of these historical insights entered into the development literature during the 1990s and 2000s with the growing interest in the importance of ‘getting institutions right’, though it is notable, for instance, that England’s precocious national social security system has not yet generated much serious discussion as a possible development policy strategy (Szreter 2007).

Rather than a firm path, which only has to be ‘found’ and its course and contours ‘mapped’, historians view history—the past—more as a flowing river of fluid and swirling potential, with many eddies and back currents in it. Only partially knowable at best, it is something moving at deceptively different speeds in various courses of its travel, with many undercurrents which can be hard to see and to estimate their power. History as flow is never finished and the present is not a fixed point at the end of history with everything in the future in a different space or dimension. Of course the future is even more unknowable and indeterminate but it is not disconnected from history. A policy intervention, therefore, is like pouring a chemical or a dye into this flowing stream. It joins, diffuses, gets diluted and may or may not change the color of the water in the intended fashion. In this sense, policymakers need to be more realistic about the way in which their policies will mix into the flow of a society’s history and not simply imagine they will achieve the ‘laboratory’ results they wish for them. This also means, in extreme circumstances, that some policy interventions should be abandoned and not applied if, despite their good intentions, a proper historical and sociological or anthropological appraisal suggests that the way in which they will be adapted will be counterproductive⁴⁵. At the moment the flow of history in a developing society is too often regarded as ‘the problem’, the embodiment of the inertia, the traditional ways, as something which needs to be changed or transformed by the application of development policies. More intelligent and realistic policies would start from the premise that the receiving society and its historical momentum are much

⁴⁴ On this see Erickson (2005a, 2005b).

⁴⁵ In a sense this is like the ‘corruption’ critique of aid, except that that critique is culturally ethnocentric and not very discriminating, but it does at least recognize, albeit crudely and negatively, that the historically-formed characteristics of the aid-receiving nation or community need to be respected because they will appropriate the resources brought to them in the ways they see fit and for their own purposes, relatively independently of the goals of the external agency bringing-in the resources.

more powerful and important than the applied policies, and the latter only really have a chance to succeed if they can work with the flow and the momentum of the society's history to encourage the desired kinds of selective adaptations. Such adaptations will take place; the only question is what forms they will take and whether these will correspond with the intentions of those attempting to promote development.

The English novelist L. P. Hartley opened his book *The Go-Between* with the famous lines: "The past is a foreign country; they do things differently there". In this paper, we have shown that students of the past also do things rather "differently" as well: those we have called historical economists are primarily concerned with resolving identification issues, seeking to build clean mono-causal explanations, while others—historians and some economic historians—strive instead to understand complex processes, contexts and contests, and the manner in which selective remembrances of this "foreign country" are invoked to justify actions in the present. In essence these approaches should be seen as complements, but too often they are regarded as substitutes, with informed dialogue occurring only rarely. The residents of and visitors to this foreign country speak different languages, hold different beliefs, and aspire to different goals; as with other such manifestations of this problem, the appropriate solution is effective diplomacy and respectful engagement, not willful ignorance or hubris.

If institutions and history matter, then historians and their discipline surely matter also. More and better dialogue between historians and those who oversee development policy is likely to yield both higher quality responses to some of the world's most urgent (if vexing) problems, and more informed critiques of those who purport to invoke 'history' in support of their cause but in fact are more likely to be speaking on the basis of a partial or flawed understanding of the past's continuing influence on the present. As MacMillan (2009: 169-170) wisely concludes

If the study of history does nothing more than teach us humility, skepticism, and awareness of ourselves, then it has done something useful. We must continue to examine our own assumptions and those of others and ask, where's the evidence? Or, is there another explanation? We should be wary of grand claims in history's name or those who claim to have uncovered the truth once and for all... [U]se it, enjoy it, but always handle history with care.

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