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## *How and Why Does History Matter for Development Policy?*

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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 recognise that historians (and not only economic historians) might also have useful and distinctive insights to offer. This paper endeavours to open and sustain a constructive dialogue between history—understood as both ‘the past’ and ‘the discipline’—and development policy by (a) providing a critique of recent ‘big picture’ accounts of comparative economic development (by economists, historians and others), (b) clarifying what the craft of historical scholarship entails, especially as it pertains to understanding causal mechanisms, contexts and complex processes of institutional change, (c) providing examples of historical research that support, qualify or challenge the most influential research (by economists and economic historians) in contemporary development policy, and (d) 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.

**Keywords:** History, Development policy, Institutions, Time, Contexts

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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*<sup>1</sup>

## **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. 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 fraternity has largely failed to take the third (seemingly logical) step, which is to recognise that historians—and the discipline they represent—might matter. Selected economic historians working within the confines of economics departments (e.g., Jeffrey Williamson, Kenneth Sokoloff, Peter Lindert) have certainly been influential in these

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<sup>1</sup> Hobsbawm (1998: aa)

discussions,<sup>2</sup> as have some innovative economists (most notably Daron Acemoglu, Simon Johnson, James Robinson (2001); for present purposes we shall call them historical economists) who have turned their theories, methods and quest for data to the past. Certain authors (e.g., Diamond, 1997; de Soto, 2000) and historians (e.g., Landes, 1998; Pomeranz, 2000) of influential 'big picture' development narratives have also invoked a reading of the past to make their case.<sup>3</sup> 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.<sup>4</sup>

While they 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 the development discourse, where everyone putatively agrees 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. 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. 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/or 'historical economists' to understanding the dynamics of

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<sup>2</sup> 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.

<sup>3</sup> See also Mokyr (2002), Fogel (2004), Clark (2007), Frieden (2006), O'Rourke and Findlay (2007) and many others. 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 Tilly, 2006.).

<sup>4</sup> Two exceptions, at least on the issue of globalisation, could be Aghion and Williamson (1998: chapter 3) and Frieden (2006). 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](http://www.historyandpolicy.org)) has, since 2002, organised a number of seminar events and published over 70 '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.

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. Economists just happen to be far more numerous and influential in development policy debates, so their oversight is more consequential.

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 of institutions and history—we seek to extend and fortify the bridge connecting historians and development policymakers.<sup>5</sup>

The paper proceeds as follows. Section 2 clarifies key terms and concepts, and explores the basis on which historical scholarship—or 'thinking in time' (Neustadt and May, 1986<sup>6</sup>)—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 3 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 of acquiring an entire sensibility about how to compile, assess and interpret evidence, substantiate causal claims, and understand complex (often interdependent) processes.. Section 4 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 5 concludes with suggestions for how the evolving dialogue between historians and development policy can be enhanced and sustained.

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<sup>5</sup> 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 Sowell (2005) on links between historians and sociology.

<sup>6</sup> See also the related terminology deployed by Pierson (2004) for political scientists. Pierson (2005) provides a useful discussion on the history of 'policy development'.

## 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' 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'<sup>7</sup>); (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 20th (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 fulfil, a destiny or mandate borne of historical necessity<sup>8</sup>. 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 well-meaning bureaucracies capable of wielding only the crudest of de-contextualised policy tools<sup>9</sup>) 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 modernisation theory's influence, contain an inherent imperative to embrace, implicitly if not explicitly, presumptions that there is a 'single' and/or 'best' path to

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<sup>7</sup> 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.

<sup>8</sup> We are grateful to Dietrich Rueschemeyer for stressing this point.

<sup>9</sup> On this issue see Scott (1998).

modernity (embodied in the ubiquitous language of 'best practices'), a notion most contemporary historians reject.<sup>10</sup> 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 rationalisations 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, 1997), 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 savoury aspects of that process (slavery, colonialism, exploitation, suppression, theft).<sup>11</sup> Moreover, they argue, as part of this obfuscation, the mantra of 'development' enables the rich to lecture the poor about their 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 behavioural traits that they are told will surely correct these failings (but in fact will most likely serve only to further advance and consolidate the interests of the wealthy). These are not idle matters; several high-profile graduate programmes in development studies (most notably in Europe<sup>12</sup>) are informed, implicitly if not explicitly, by these notions; not surprisingly, engagement by the leaders (and graduates) of such programmes with policymakers, practitioners and staff of international development agencies—to the extent it occurs at all—is often characterised by deep suspicion (if not outright hostility).

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

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<sup>10</sup> A fascinating historical inquiry into the notion of 'multiple modernities' is provided in *Daedalus* (1998, 2000).

<sup>11</sup> For a related argument, see Goody (2006).

<sup>12</sup> 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).

dominant colonial discourse (Sarkar, 1983), and that this laid the foundations of movements for social change. 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, press freedom, population density, types of natural resources, labour scarcity—in the past 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.

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 recognise 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 exercise<sup>13</sup>, 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 specialises 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 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 organisation's origins, mandate and approach, showing how, at key junctures, particular options among several came to prevail.<sup>14</sup>

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<sup>13</sup> 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).

<sup>14</sup> In a more compressed time frame, this is the task undertaken by Porter et al (1991) and Mosse (2005) in their insightful analyses of development projects.

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 primary school memories; as such it is likely to be highly selective, used to suit predetermined purposes, and to be largely unverified.<sup>15</sup> The (ab)use of history in this form not only represents a problem of commission but also of omission, in that it both invokes ‘bad’ history and 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 added 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 ‘development’ offered by Marx and Weber in the middle of the 19th century. From the late 19th century until the late 20th century, however, the mathematical turn in economics saw institutions recede from centre stage in that discipline. In the late 20th century it was primarily the pioneering work of Douglass North

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<sup>15</sup> 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, organisation 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 dangerous) 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.)

(1982, 1990)<sup>16</sup>—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 economists’ theory and 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)<sup>17</sup>. Institutions, and their operational counterpart of ‘governance’, are now (back) at the centre 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 realise 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,<sup>18</sup> 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.

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<sup>16</sup> Many would also want to credit Mancur Olson 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.

<sup>17</sup> To cite only a few among hundreds of contributions, see Clague (1997), Rodrik (2003) and Easterly (2001).

<sup>18</sup> The most visible empirical manifestation of these general features of ‘good governance’ are the six widely-cited measures of institutional quality developed by Daniel Kaufman and Aart Kraay (and their collaborators) at the World Bank (see, only most recently, Kaufman, Kray and Mastruzzi, 2007).

For all the attention garnered by the impressive quantitative studies documenting the importance of institutions for understanding contemporary economic performance,<sup>19</sup> by the early 2000s the prevailing policy discourse was recognising 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) or encouraging more virtuous ones (poverty reduction). The key empirical and policy questions then became: Under what conditions do ‘good’ and ‘bad’ institutions emerge?<sup>20</sup> 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 phenomenon in a comparative analysis of the divergent fortunes of North and South America, argued that the key lay in understanding the types of factor endowments in each region: where labour was scarce and land abundant, colonial rulers faced strong incentives to establish relatively benign institutions; conversely, where labour was abundant (and/or local populations readily able to be subdued) and land/natural resources scarce, more ‘predatory’ institutions were established.<sup>21</sup> 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.

These debates accelerated considerably in terms of both scale and impact with the arrival of two seminal papers by Acemoglu, Johnson and Robinson (2001; 2002), 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 colonised countries that were relatively rich in

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<sup>19</sup> 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).

<sup>20</sup> 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.

<sup>21</sup> Some studies (e.g., Glaeser and Shleifer, 2002) 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, but this view seems to have gained little empirical traction.

1500 were now amongst the poorest today, and vice versa.<sup>22</sup> The explanation was that the colonising powers had faced very different institutional settings upon their arrival, and very different incentives to erect particular forms of new institutions as they pondered the terms of their engagement with local populations and natural environments. Where colonial settlers had to live under the same rules as those people they controlled, were there for the long haul, were engaged in agricultural tasks where land was abundant and that required relatively little labour, and where there was a low disease burden (Australia, New Zealand, Canada), they established 'good' institutions; bad institutions, on the other hand, were established where the goal was to extract and plunder agricultural produce (and/or natural resources), where local diseases took a major toll on the settlers, and where a different set of laws (especially pertaining to land ownership, civil liberties and conflict management) applied to the rulers and the ruled, thereby enabling initial inequalities to consolidate, perpetuate and accumulate (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 replacement<sup>23</sup>.

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 recognise the important ways in which the past shapes the present. 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 works with evidence that may number only a single episode. To do justice to this range of material entails a different set of commitments and sensibilities to those

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<sup>22</sup> See Bayly (2008) for a more extended substantive engagement with these papers; for a methodological critique and alternative empirical strategy (using comparative case study analysis), see Katz, vom Hau and Mahoney (2005).

<sup>23</sup> See Acemoglu and Robinson (2006a), formalising Gerschenkron (1962), and Acemoglu and Robinson (2006b), formalising Moore (1966).

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 recognise that the veracity of a given explanation is stronger the larger the number of cases it can explain,<sup>24</sup> the canonical skill of the historian is being able to immerse themselves 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 another<sup>25</sup>.

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<sup>24</sup> 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.

<sup>25</sup> 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).

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 claims.<sup>26</sup> For many of the episodes under 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.<sup>27</sup> 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 recognise that 'history matters', finds itself strongly predisposed to buy into the findings and claims of the 'historical economists' over those of any and all other historians. Such frames also preclude taking seriously the power of ideas, rituals, ideologies and symbols in effecting outcomes (because they cannot adequately be 'measured'), and for similar reasons strongly favours an empirical focus on factors of production, rather than changes in preferences.<sup>28</sup>

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 honoured in the breach. As Scott (1998) has argued, throughout the 20th 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-contextualised 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

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<sup>26</sup> The changing basis of causal claims is itself, of course, a fascinating subject of historical enquiry (see Kern, 2006).

<sup>27</sup> On this point see Goldstone (1998), Mahoney (2000), and Trachtenberg (2006).

<sup>28</sup> See Rao and Walton (2004) for more on this point.

predisposed to favouring either technocrats (i.e., a few smart people, usually called ‘experts’<sup>29</sup>) or standardised, uniform procedures; those decisions in development policy—and they are legion—that require instead both large amounts of 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*) 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 an 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 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 action<sup>30</sup> (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).<sup>31</sup>

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<sup>29</sup> On the role of agricultural ‘experts’ in shaping ‘agrarian doctrines of development’ during British colonialism, see the masterful analysis of Hodge (2007).

<sup>30</sup> 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. Putnam (1993) and others popularised 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).

<sup>31</sup> 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

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 linear<sup>32</sup> (which is the default assumption in contemporary development policy debates, especially those pertaining to impact assessment)<sup>33</sup>. Discerning empirically the likely non-linear trajectory of women’s empowerment initiatives, for example, or governance 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 favouring 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).<sup>34</sup>

### **Brief examples on the history of measurement**

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.

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multiple causes. However, there is far too little public acknowledgement that virtually all ‘findings’ in econometric studies are subject to this.

<sup>32</sup> Most economists, importantly, do not 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.

<sup>33</sup> 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 19th 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 is 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 celebrated interventions, and has little knowledge of how these impacts are influenced by scale and (different types of) context (on this point see Woolcock, forthcoming).

<sup>34</sup> These pressures, solidly reinforced by campaigns such as the Millennium Development Goals, manifest themselves in calls to ‘scale up’ and ‘replicate’ putatively successful interventions.

History teaches us that policy interventions can lead to significant change in ways intended by the policy maker, 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 that all births, deaths and marriages 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, legitimation of bastardy, and for knowledge, whether any person is our subject or no' (cited in Elton, 1972: 259-260). 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 labour 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 shift a country's destiny.

Another example of this is a recent paper by Vincent (2008), which shows how measurement mattered in generating improvements in literacy in Victorian England. The idea that literacy could be measured came from the realisation by a novelist (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 utilised to measure spatial and class disparities in literacy. While the limitations of the measure were also recognised, the low levels of literacy led to demands from the bottom, via the radical press, to equalise the supply of education. The data were analysed 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, recognised that having one literate family member was often sufficient to confer a high level of benefits. The rise in literacy was 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 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 standardised 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 policymakers 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 mobilise disadvantaged groups against discriminatory practices and towards greater rights. The policy imperative to 'measure' can thus lead to powerful social and economic changes, sometimes intended and sometimes unintended. As Susan Bayly (1999) has shown, this process of categorisation and recategorisation 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**

The arguments and evidence presented above, together with our collective reading of some of the broader corpus of historical research on comparative development, now put us in a position to answer more directly the question posed by the title of this paper.

First, North's work has recast all economists' understanding of the nature of economic growth itself by making it clear that institutions matter crucially, both for markets to exist and for them to grow. However, an important extension of this theme, and one largely ignored by economists, is that effective institutions characteristically emerge through a political process of contestation, and thereby have a form, content and legitimacy they would (and could) not have had if they been 'imported' from elsewhere (Bayly, 2008). 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. The key idea here is the indigenisation of an idea by 'peer educators' (Rao and Walton, 2004), 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. Bayly (2008) argues that there is a free trade in ideas between colonies and their rulers, rather than a one-way interaction (as institutional economists have argued); as such, development is therefore much more of a 'hybrid' process.

We would posit that understanding this hybridity, and developing indigenous capacity via peer educators, is key to understanding why some countries are emerging from poverty

faster than others. One might surmise that, in matters pertaining to institutional, legal and political development, there may in fact be great value in 're-inventing the wheel'. This should compel us to reconsider what we mean by 'institutions'. For example, the Muhammadiyah—a modernist Islamic movement that arose in Indonesia as a response to Dutch rule and which was at the forefront of the democratisation process—is clearly an important institution (Heffner, 2000); the Dalit movement in India, which led to the rise of lower castes competing effectively within democratic structures is also an important institution (Omvedt, 1994). These hybrid institutions, which have been central to development processes in Indonesia and India, do not fit easily within orthodox Northian categories. These 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 organisations are a central part of civil society, i.e., as both service providers (schools, hospitals) and potentially part of the social accountability process.

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). The role of elites is a central here, because they lead the process of hybridity and indigenisation. Peer educators are drawn from their ranks, and good development strategies cannot be implemented without the support of commercial (entrepreneurial) groups. Also, elites play an important role in forming a free press, civil society groups, and other important elements of an indigenous public sphere that form a 'critical public' that constitute indigenous mechanisms of accountability. As Bayly (2008) 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' (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 political channels, through which dissent can be aired before getting out of control.

Third, development demands a constant exchange between the centre and the periphery—that is, between the capital city and provinces, between central and local governments, between colonisers 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, historical research can reveal long-term shifts in tastes, ideology and beliefs, issues which economists, despite recent advances, abstract away from. 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.

Fifth, 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 even the UK and USA, as well as more well-known cases, such as Germany, France and Japan—operated protectionist regimes to protect or promote infant 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-industrialisers 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 industrialised 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, as we demonstrate in the examples above, in addition to questioning the way problems of development are framed, history can question the way in which data is classified, categorised and constructed. 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 mobilise social movements and spark important social changes.

Seventh, 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 realisation of the kind of time-scale they should be envisaging for their policies and plans to come to fruition. They would realise 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.

Eighth, going along with the temporal realism lesson, history teaches the non-linearity and conflictual nature of economic development (Skocpol, Moore, and Schumpeter's famous description of economic growth as a process of 'creative destruction' in *Capitalism, Socialism and Democracy* 1942). 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 results, in the short run, in periods of conflict and stagnation before sustainable and equitable growth is achieved.

Ninth, in relation to the pressing problems of the environment—problems integral to world economic development for which neither economics nor policymakers 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).<sup>35</sup>

Finally, the history of the field of development itself 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. Development is certainly not a science, so yesterday's now-discarded or forgotten ideas are not necessarily entirely obsolete. An example here would be W.A. Lewis's seminal paper (Lewis, 1954) on the problems of attempting to promote economic development with unlimited supplies of labour. 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.

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<sup>35</sup> Also see, for instance, Mark Roodhouse, 'Rationing returns: a solution to global warming?' on the [historyandpolicy.org](http://historyandpolicy.org) website re 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.

## VI: Conclusion

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 has exerted a deterministic influence upon its subsequent history, then this is an 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 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 had 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.<sup>36</sup> 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.

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<sup>36</sup> On this see Erickson (2005a, 2005b).

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 colour 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 counterproductive.<sup>37</sup> 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 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.

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.

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<sup>37</sup> 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 recognise, 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.

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