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INSTITUTIONS, DEVELOPMENT AND GROWTH: WHERE DOES EVIDENCE STAND?

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Abstract

This paper explores the empirical evidence on institutions and growth. The empirical evidence on institutions ranges from historical studies to econometric analyses, with different studies making very different theoretical commitments as to what institutions means and how they affect economic development. This paper argues that this breadth of evidentiary forms has given credibility to empirical institutional economics, allowing for the emergence of robust evidence of the importance of institutions and robust recommendations for policy.

Institutions matter for growth and inclusive development. But despite increasing awareness of the importance of institutions on economic outcomes, there is little evidence on how positive institutional change can be achieved. The Economic Development and Institutions – EDI – research programme aims to fill this knowledge gap by working with some of the finest economic thinkers and social scientists across the globe.

The programme was launched in 2015 and will run for five years. It is made up of four parallel research activities: path-finding papers, institutional diagnostic, coordinated randomised control trials, and case studies. The programme is funded by the UK Department for International Development. For more information see <http://edi.opml.co.uk>.

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1 Introduction

Arguments on the relationship between institutions and growth have appeared throughout the history of economics. Nevertheless, the last two decades have seen institutions emerge as the touchstone of thinking on growth and economic development. The contemporary prominence of institutional explanations of economic success and failure has three interrelated sources.

1. Economic historians have provided detailed studies of the interactions of institutions and economic development in a wide range of historical episodes. These episodes range widely in time and in scale, with studies ranging from details of microeconomic relations to claims about the long term effects of particular institutions on macroeconomic development (see Nunn (2009) for an overview).
2. Development economists have linked relative successes and failures in environments where the institutions of advanced economies are either attenuated or absent. The types of institutions under study are very wide ranging. One side of the literature studies politics and governance in developing societies while another focuses on the ways that informal institutions such as social networks substitute for weaknesses in financial markets or legal systems. Surveys by Baland, Moene and Robinson (2008) and Cox and Fafchamps (2007) provide a useful counterpoint.
3. Growth economists have applied formal econometric and statistical models to cross-country data sets and identified general patterns linking empirical proxies for institutions types and quality with economic outcomes. Within this enterprise, a number institutional measures have been argued to have marginal explanatory power in predicting cross country growth differences. Institutions have been further argued to represent the most robust fundamental growth determinant in empirical exercises that center on such determinants (see for example Rodrik, Subramanian and Trebbi (2004)).

While distinct sources of evidence, these different dimensions of empirical research have built upon one another and have undergirded modern growth theory in a synergistic fashion. Historical episodes and evidence from development studies are ubiquitous in growth economics just as general claims in growth economics have informed economic history and development economics. One impressive feature of the institutions and growth literature is the way that these different evidentiary bases have been integrated in making the empirical case for the importance of institutions (see for example of the argumentation that underlies Acemoglu and Robinson (2005)).

This essay is designed to present a concise overview of the state of evidence on institutions and growth. There is no serious disagreement with the proposition that “institutions matter.” Disagreements have to do with specifics, how and to what extent particular institutions matter in particular contexts. Suppose one argues that the failures of centrally planned economies or the travails of post-Soviet Russia are evidence that “institutions matter.” While I think this is indisputably the case that this abstract proposition is true, this fact does not by itself directly entail much in terms of understanding the importance of institutions in other episodes nor say much about the specifics of policy reform of the type under contemporary discussion. My focus is on this more precise type of knowledge about the effects of

institutions on growth and development. This essay does not pretend to be exhaustive; a comprehensive overview would require a book length treatment. My goal here is to communicate a sense of the state of the literature, both successes and limitations.

Section 2 discusses definitions and measurement. I address conceptual issues in defining institutions as well as the transition from concepts to quantitative measures. Section 3 examines statistical evidence on growth and institutions. My conclusions for this class of studies will be largely negative, for reasons that involve the general limits in the ability of the main types of statistical models that are employed, cross-country growth regressions, to credibly identify particular growth mechanisms. Section 4 discusses structural analyses. These analyses, I argue, are useful in illustrating the potential extent to which a particular type of institution may be able to explain some aggregate outcomes. However, these types of studies provide, at best, very limited evidence on institutions per se. Section 5 considers historical studies. I argue that these studies are the strongest source of evidence on institutions. This strength, however, derives from a specificity of contextual knowledge that leads to questions about general conclusions. Section 6 gives an interpretation of evidence from these three sources. I argue that the state of knowledge in the institutions and growth literature is a successful example of abductive reasoning, also known as inference to the best explanation in the philosophy of science literature. Abduction/inference to the best explanation refers to the process by which evidence is integrated across available sources and conclusions drawn on which explanation of a given phenomenon best fits the evidence. I claim that abduction provides the appropriate conceptual framework for understanding how evidence has led to a consensus emerging for the salience of institutions in explaining cross-country heterogeneity and intra-country dynamics. Section 6 concludes with some thoughts on potential new directions for research on the institutions, growth, and development relationship.

I note one omission from the survey: laboratory studies. Interesting evidence has emerged, in a range of controlled experiments, for how different rules affect behaviors. Well known examples include Frey and Bohnet (1995) on institutions and preferences for fairness and Dal Bo, Foster, and Putterman (2010) on democratic choices and cooperation. I omit this work because its implications for the institution/economics relationship are so remote from the large scale contexts of the latter. As such, the experimental literature helps buttress the other sources of evidence, but unlike those sources, cannot stand on its own with reference to the sorts of questions the institutional literature addresses.

2 Definitions and measurement

Empirical analyses of the institutions/economic outcomes relationship naturally require specification of institutions. A first question concerns the meaning of the rubric “institutions”.¹ North (1990, p. 3) provides one classic definition of institutions as

“the rules of the game in a society or, more formally, humanly devised constraints that shape interaction. In consequence, they structure incentives in human exchange, whether, political, social, or economic.”

By way of comparison, Greif (2006, p. 39) states that institutions constitute

“a system of rules, beliefs, and norms, and organizations that together generate a regularity of (social) behavior” (italics removed).”

Baland, Bourguignon, Platteau and Verdier (this volume) describe institutions as

“rules, procedures, or other human devices that constrain individual behavior, with a view to making individual expectations about others’ behavior converge and to allowing individual actions to become more coordinated.”

There are dimensions along which these definitions differ. For example, while Greif treats beliefs as an institution, Baland et al argue that institutions matter *because* they shape beliefs. And neither conception requires that beliefs be thought of as constraints in sense of North. That said, there is clearly much overlap, most obviously in terms of the emphasis on “rules”.

Broad definitions, such as those proposed by North, Greif, and Baland et al are an important background for empirical work because they emphasize the varied and rich range of social relationships and interactions that should be considered in studying. The range of the institutions and economics literature is well reflected in them. However, definitions of this type do not provide much guidance for empirical work since they can include so many distinct forms of political and social structures. The breadth of these definitions also raises questions of whether other general categories, such as culture can be meaningfully distinguished from institutions; I return to this in the conclusion of this essay.

Such a broad conceptualization of institutions also runs the danger of defining institutions functionally. One example is social capital, which as discussed, in Knowles (2007), is closely linked to North’s conceptualization. Some treatments of social capital equate it with solidarity, others with trust. In my view, functional definitions should be avoided in empirical research because they undermine a clear strategy for meaningful empirical evaluation. Solidarity and trust are endogenous outcomes, and so to equate them with social capital is to miss the factors that create and shape social capital. Portes (1998) and Durlauf (2002)

¹While other general categories such as markets also require specificity for empirical work, institutions constitute an especially elastic category as discussed below.

discuss how functional social capital definitions end up ascribing successful social outcomes to its presence without a distinct social capital measure.²

To elaborate a simple example of the limits of overly broad definitions, consider a society in which individuals are all devout Kantians. Rational agents would exhibit high degrees of trust because of the trustworthiness of the individuals in the society. Hence the social capital as trust definition would be uninformative since trust is derivative from ethical behaviors. Further, if the process of transmission of ethics occurs at the family level, then there is no dimension along which the common norm should be thought of as collectively devised (North) or as a system (Greif). This example indicates how a functional definition, trust constitutes an institution, may be misleading. Further, even if institutions shape levels of trust, treating trust as an institution begs the question of how it is produced.

With reference to empirical work, the upshot of this discussion is that general definitions of institutions underdetermine what does or does not appropriately constitute an institution. Put differently, these types of broad definitions may embrace a superset of the union of different conceptions of institutions, but are not sufficiently precise to provide clear guides to empirical research.

While conceptual definitions may be elusive for institutions, this does not necessarily entail a fundamental impediment to empirical work. Both the New Institutional Economics and the growth and development literatures have addressed the definitional challenge by focusing on subsets of institutions. These subsets can involve extremely specific institutions, or larger classes. The first column of Table 1 provides a representative sample of the literature. This strategy is perfectly consistent with the absence of definitional precision for institutions; the boundaries of a category, set, or concept may be ill-defined while some members are unambiguously members of it. By analogy, ambiguity about the definition of money does not preclude analysis of the Federal Reserve interest rate or money supply rules. Some institutions are very precisely defined, such as a specific forced labor system in the Andes studied by Dell (2010). In other cases, empirical work is predicated on indices that aggregate disparate features of complex institutions such as democracy (Tavares and Warczarg (2001) among others).

In the empirical institutions literature, one basic empirical strategy has emerged around the analysis of classes of institutions characterized by certain general characteristics. A classic distinction divides institutions between formal and informal ones. Legal systems are very different institutions from social norms. Platteau (1994a, pg. 535) takes the approach of defining institutions as

“conscious coordination efforts; or by resorting to external sanction systems (which presuppose the existence of an authority structure).”

This allows for both formal and informal institutions, an example of the latter being a community in which ostracism punishes wrongdoers. Platteau (1994a,b) argues that his definition appropriately distinguishes institutions from cases in which rules of conduct can be “established and sustained in a completely spontaneous way” (p. 535). This distinction

² Some social capital definitions invoke specific phenomena, e.g. networks. But these definitions beg the question of what distinguishes social capital as a natural kind from the phenomena, which can themselves be defined as institutions.

means that moral norms interact with institutions to, for example, facilitate the solution of coordination problems. This is a useful approach from the measurement perspective because it equates formal institutions to collective action processes which are presumably identifiable given observability of the process itself or its direct consequences, what Platteau calls an authority structure. Platteau emphasizes how informal institutions interact with formal ones in the development process. A complementary division is proposed by Voigt (2010) between *de jure* and *de facto* institutions, a division that both focuses on the interplay of formal and informal institutions as well as focusing on the roles of different types of institutions in determining equilibrium outcomes. Hence, whether or not the Stalin Constitution of 1936 was or was not, in its written provisions, the most free in the world, is irrelevant, since the existence of the institution called the security organs determined actual levels of liberty.

A distinct strategy to defining sets of institutions is via their common effects. The most prominent example of this strategy is the research program defined by Acemoglu, Johnson, and Robinson (2001, 2002), Acemoglu and Johnson (2005), and Acemoglu and Robinson (2005, 2008), which studies institutions and their relationship to broadly defined property rights. Following Acemoglu, Johnson, and Robinson (2002, p. 1262)

“...we take a good organization of society to correspond to a cluster of (political, economic, and social) institutions ensuring that a broad cross section of society has effective property rights. We refer to this cluster as institutions of private property, and contrast them with extractive institutions, where the majority of the population faces a high risk of expropriation and holdup by the government, the ruling elite, or other agents. Two requirements are implicit in this definition of institutions of private property. First, institutions should provide secure property rights, so that those with productive opportunities expect to receive returns from their investments, and are encouraged to undertake such investments. The second requirement is embedded in the emphasis on “a broad cross section of the society.”

This approach defines institutions by the way they influence individual decision making, i.e. how they shape incentives, Acemoglu, Johnson, and Robinson go on to say (pp. 1262-1263)

“A society in which a very small fraction of the population, for example, a class of landowners, holds all the wealth and political power may not be the ideal environment for investment, even if the property rights of this elite are secure. In such a society, many of the agents with the entrepreneurial human capital and investment opportunities may be those without effective property rights protection. In particular, the concentration of political and social power in the hands of a small elite implies that the majority of the population risks being held up by the powerful elite after they undertake investments.”

The discussion makes it clear why the Acemoglu, Johnson, and Robinson approach is not a functional definition. Institutions are not directly defined by their effects on growth and development, but on how they alter individual decisions.

Other approaches complement the focus on property rights by considering the broad range of ways that institutions interact with economic life. Rodrik (2005) divides institutions into those that are 1) market creating (eg property rights), 2) market regulating (rules for

addressing market failures), 3) market-stabilizing (macro-level monetary fiscal, and financial policies), 4) market-legitimizing (political inclusiveness via democratic rules and social insurance). This approach, which focuses on institutions and governance, is not designed to be exhaustive, but to be policy relevant. The approach is also focused on complementing property rights with other dimensions along which institutions matter. Bardhan (2005, 2016) makes parallel arguments of this type, with an emphasis on the effects of institutions on resolving coordination problems and creating democratic resolutions of disagreements.

The plethora of possible ways to characterize classes of institutions, in my judgment, enriches empirical work. Choices on how to organize institutions into distinct categories need not be invariant across studies and in fact should be tailored to the questions that need to be addressed. Typologies of institutions matter for a given study, of course, because a typology defines the institutional features of interest. But the multiplicity of typologies does not necessarily entail an identification problem since conclusions about institutions organized in one fashion are substantively consistent with conclusions derived from a different organization. Put differently, institutional definitions do not “carve nature at its joints”. Each division provides a perspective on how institutions matter and these perspectives are not incompatible with one another. This is not a claim that “anything goes.” A study of the treatment effect of a given institution is well defined to the extent that the institution under study is well defined in the context of the study, not whether the definition is portable across studies. The same argument would apply to more structural analyses.

While I conclude that conceptual difficulties in institutional definition have been successfully addressed, in contrast, there are measurement issues involved in the transition from conceptually precise set of institutions to associated empirical measures for them. Here I highlight two major impediments to empirical work.

A first measurement problem is the relationship between the concepts by which institutions are understood and the statistical measures that have been constructed for empirical work. As discussed in Shirley (2005) and Voigt (2013), for example, variables measuring institutional quality are indices which aggregate very different dimensions of a political system. Consider the institution “democracy”. The democracy indices which are employed in empirical work create a scalar measure of the level of democracy based on elections, civil liberties and other conceptually distinct aspects of a polity³. Similar concerns may be raised for corruption indices and the like. Concepts such as democracy and corruption constitute umbrellas for many different facets of socioeconomic and political structures. These different facets do not naturally allow for a meaningful scalar measure of the underlying concept. This limit can matter. For example, as shown in Tavares and Warciarg (2001), greater democracy increases growth via its effects on income inequality and educational investment but also inhibits growth because of the greater government consumption and lower physical capital accumulation, differences are explainable from theory.

Difficulties in measurement differ across institutional types. While measuring legal systems is complicated because of the plethora of facets of a system, the challenges involve judgments based on dimension reduction, which in turn is based on the observables of the system. In

³ Other limits to the indices involve the distinctions between formal and informal or de jure and de facto institutions. Democracy indices are constructed using procedural measures which may not characterize either the operational aspects of a system (e.g. voting restrictions, corruption) or the extent to which polarization in a country means that minority preferences are ever reflected in policy choices.

contrast, measurement of social norms is necessarily indirect, requiring either survey data on beliefs and attitudes or the embedding of norms in a structural model of outcomes, which involve quite different judgments. This needs to be remembered when “horse races” are run to compare the explanatory power of alternative institutions.

A second measurement issue concerns the distinction between institutions and equilibrium outcomes associated with institutions, a distinction that is the basis of a famous critique of institutional measurement by Glaeser et. al. (2004). These authors argue that three standard measures of institutional quality, expropriation probability, government effectiveness, and limits to executive power, do not measure institutions per se. Rather, these are equilibrium outcomes, reflecting the interplay of institutions with the state of the economy and society. As such, they are not measures of “durable rules, procedures or norms that the word “institutions” refers to” (p. 274). Glaeser et. al. conclude that cross-country growth heterogeneity is better explained by human capital accumulation, and that economic development leads to better institutions. The distinction between equilibrium outcomes and underlying institutions is not simply an issue of endogeneity of institutions but rather is a question of time scale. This means that one cannot instrument the institutional measures with any set of “predetermined” variables. Valid instruments need to exhibit similar durability as the underlying institutions one wishes to capture.⁴ This concern has led these authors to focus on legal origins, itself a type of institution, as a source of long run differences between countries.

The importance of these measurement limitations differs across the two sources of empirical evidence on institutional effects: statistical models based on cross-country regressions, and historical studies.

⁴ I am not aware of any analyses of time scale and instrumental variable validity in the institutions literature.

3 Cross-country regressions

Much empirical growth evidence, in particular in the 1990 to 2010 period, relied on various formulation of cross-country growth regressions⁵. Table 1 provides a survey of representative studies. Given concerns over the endogeneity of institutions, there is a cognate literature based on various instrumental variables. These are summarized in Table 2.

The strength of the evidence from cross-country studies of course depends on the credibility of inferences using this methodology. As a source of substantive information on the mechanisms underlying growth and development, cross-country regressions have been subjected to very severe criticism (see Durlauf, Johnson, and Temple (2005) for a delineation of a host of problems). For purposes of this discussion, I would emphasize that cross-country regression evidence has often proven to be fragile because of the sensitivity of findings to the choice of control variables. Different papers come to different conclusions on institutions because of the choice of control variables. And this set of control variables is massive. Durlauf and Quah (1998) argue that there are virtually as many growth variables that have been proposed as countries while Durlauf, Johnson and Temple (2005) further argue that over forty distinct growth theories are proxied for by these variables. Further, growth theory is open-ended in the sense discussed in Brock and Durlauf (2001): one growth theory typically does not logically entail that another theory is either false or empirically irrelevant. In other words, specific growth theories are specific mechanisms that do not rule out other mechanisms in the overall growth process. Hence, the alternative specifications of growth regression controls are limited only by the number of combinations of growth determinants and associated measures that are available. Conceptually, each growth regression is a model and the cross-country growth literature faces massive model uncertainty, by which I mean each choice of control variables determines a distinct growth model.

In order to constructively address the sensitivity of growth regression evidence to model choice, Fernandez, Ley and Steel (2001) and Sala-i-Martin, Doppelhofer and Miller (2004) proposed the use of model averaging techniques to be applied to growth regressions. Model averaging, in essence, aggregates model-specific evidence across specifications, using weights that reflect relative goodness of fit of the individual models as well as any prior plausibility differences an analyst wishes to assign to the models. A relatively recent example is Durlauf, Kourtellos, and Tan (2008). From the perspective of understanding the effects of institutions on growth, this approach allows one to measure the effects accounting for the fact that the “true” growth regression specification is not known to the analyst. As such, the methodology is appropriate for identifying robust evidence concerning a given growth determinant. When done in a formal Bayesian fashion, one can compare the posterior probability that a given growth regressor appears in the true model, assuming the true model is an element of the space of models studied.

In the context of cross-country growth differences, model averaging exercises have not produced much evidence that institutions matter. For example, Fernandez, Ley and Steel (2001) assign a posterior probability of .516 to the presence of a rule of law measure (their empirical proxy for legal institutions) in the true growth model; the prior probability in the

⁵ Some studies look at intra-country regional heterogeneity in institutions.

analysis was .5. Among variables with posterior probability of .9, the fraction of the population that is Confucian is the only non-Solow growth determinant which is plausibly a proxy for institutions, that survives this standard. By this, I mean that these averaging exercises assign sufficiently large standard errors to growth variable coefficients that one cannot conclude anything about their relationship to growth. Sala-i-Martin, Doppelhofer, and Miller (2004) find that the posterior inclusion probabilities for various institutional variables are very small, using somewhat different institutional measures. Durlauf, Kourtellos, and Tan (2008) search for robust determinants of the aggregate growth as well as decomposition of the aggregate into components driven by human capital growth, physical capital growth, and total factor productivity growth. This study also fails to find much evidence of an institution/growth link, except for some effect of one institutional measure, constraints on the executive and on physical capital accumulation. This paper draws distinctions between deep growth determinants, and proximate growth determinants, in particular macroeconomic policy and concludes that proximate growth determinants are more robust predictors of cross country growth differences than fundamental ones, with the exception of regional fixed effects.

What should one make of the failure of growth regressions literature to produce robust evidence for the role of institutions? In my judgment, the reasons why the evidence has proven to be weak are more likely due to limitations of the methodology as opposed to the lack of importance of institutions. Recall the discussion of Glaeser et. al. (2004) on institutions as durable objects and the suggestion made earlier that institutions function on a slower time scale than the time intervals over which growth regressions are estimated. From this perspective, institutions represent a background context against which proximate growth determinants, e.g. savings or macroeconomic policy, function. Relative to this formulation, aggregate growth is not well approximated by a linear model. A better formulation is one in which the coefficients that link proximate determinants to growth are themselves functions of longer time scale variables such as institutions. Such a model is nonlinear as institutional determinants and proximate determinants interact. This can matter empirically, i.e. linear models can fail to reveal underlying nonlinear relationships in the growth process (Bernard and Durlauf (1996)) such as those that produce poverty traps.

A different approach to evaluating the empirical importance of institutions was initiated by Rodrik, Subramanian, and Trebbi (2004), who compare three “deep” or “fundamental” growth determinants: institutions, integration in the world economy, and geography. Using instruments for geography due to Frankel and Romer (1999) and instruments for institutions from Acemoglu, Johnson, and Robinson (2001). They find little evidence that either integration or geography matter for growth, once institutions are controlled for. Geography does affect institutions and so does have indirect consequences.

Additional evidence on the salience of institutions among fundamental variables is due to Tan (2010) who considers interactions between institutions and other variables. In this analysis, measures of institutions, geography and fractionalization are used to identify countries which obey a common (linear growth model). Tan finds that there are multiple growth regimes indexed by institutional quality. While finding little evidence of a distinct role for geography, he does find distinct role for ethnic fractionalization, so that if institutional quality is below a certain threshold, greater ethnic fractionalization can produce a low growth regime.

3.1 Instrumental variables

Much of the empirical growth/institutions literature has properly been concerned with the identification of causal effects of institutions in the light of endogeneity. For regression analysis, this has involved a search for instrumental variables. Table 2a describes some of the instruments that have appeared in the institutions/growth regressions. Of course, some form of an instrument is required for any study when institutions are codetermined with the outcomes of interest. Table 2b presents instruments that have appeared in various case studies.

Arguments on behalf of instrumental variables have, to a large extent, focused on whether they are predetermined relative to the time period over which institutions and growth are being studied. Thus, there is either an implicit or an explicit set of assumptions being made about the time scale at which variables are determined. One example of implicit assumptions involves the use of ethnolinguistic fractionalization as an instrument for ethnic conflict or social cohesion, e.g. Easterly and Levine (1997) and Easterly, Ritzen, and Woolcock (2006), or corruption, e.g. Aidt (2009) and Mauro (1995). The logic of these exercises is to treat exogenous border determination (typically by a combination of a stable post WWII world order in the West and borders determined by the vagaries of decolonization by various European powers). Use of an ethnolinguistic heterogeneity measure as an instrument for ethnic violence thus presupposes that migration patterns are slow relative to the time period under study. A famous example of an explicit assumption on time scales is the use, in Acemoglu, Johnson, and Robinson (2005), of settler mortality as an instrument for migration to colonies. In this analysis, the migration of institutions from the colonial power, i.e. institutions that support private property rather than extraction of rents, was exogenously influenced by whether settlers could endure in a given colony. While there has been some controversy over the measurement of the instrument (see Albouy (2012) and the response of Acemoglu, Johnson, and Robinson (2012)) the logic of the instrument is clear.

The search of instruments that evolve at lower time scales than the phenomena under study has naturally led to much interest in geographic instruments, for which it is trivial to argue that the instrument is, in any relevant sense, predetermined. As illustrated in Tables 2a and 2b, examples include distance from the equator, latitude, and distance from an ocean or navigable river.

In my judgment, the focus on predetermined instruments has often been misguided because it has directed attention away from the requirement for instrument validity that instruments are orthogonal to model errors. These errors include all growth determinants that are omitted in a given econometric model. This is a very difficult assumption to defend because of theory's open-endedness. Focusing on geography, there are a plethora of channels by which geography affects growth, ranging from effects on the distribution of disease to the effects on political institutions. Relative to institutions, this general criticism also has force. Any cross-country regression is a parsimonious low dimension approximation of the true growth process, one which omits theoretically valid and known growth determinants.⁶ A valid

⁶ The difficulty of identifying valid instruments for institutions in the context of growth regressions may be contrasted with the way instruments are identified in a simultaneous equations model (SEM), where exclusion restrictions generate instruments. A given SEM is a closed system, that allows a researcher, via exclusion restrictions or restrictions on the relationships between parameters, to use theory to generate instruments in a way precluded by theory open-endedness in a growth regression.

instrument has to be uncorrelated with the entire set of omitted growth determinants. This is the necessary standard for instruments for institutions to pass, and argumentation in the empirical literature all too often falls short. The complexities and richness of the growth process make the search for instrumental variables problematic.

The difficulties with drawing conclusions about mechanisms from instruments are illustrated by the ways in which ethnolinguistic heterogeneity and geography have been used. As noted, ethnolinguistic heterogeneity has been used as an instrument for both ethnic conflict and social cohesion as well as for corruption. Conflict and cohesion are distinct mechanisms determining social and political equilibria, and both differ from corruption. The fact that one instrument is plausibly linked to three separate mechanisms implies that any study that does not simultaneously include all three factors suffers from the critique I have described, i.e. the instrument will be correlated with omitted growth determinants. Similarly, geographic instruments are linked to multiple (more proximate) factors. If one considers an instrument for malaria prevalence, this can be associated with settler mortality or labor productivity in a given location.

While my overall assessment is that the cross-country regression literature has not established a decisive case for the empirical importance of institutions in general, let alone for the roles of particular institutions, that does not mean that they are without value. First, these studies establish data patterns and thus stylized facts about the interplay of growth/development and institutional measures. The analyses therefore matter in the sense of providing ways to allow interaction with data to update prior beliefs.

4 Structural analyses

A distinct, albeit small, set of empirical studies has constructed formal structural models to assess how particular institutions can affect aggregate outcomes. Table 3 provides examples of studies of this type. The studies are typically based on model calibration rather than estimation. Thus, these types of studies represent explicit examinations of the quantitative role of an institution in a fully delineated economic environment in contrast to cross-country growth regressions which are essentially reduced form exercises, even if theory is used *ex post* to interpret coefficients. In these papers, institutions are given mathematically precise formulations.

The state of financial development has been a primary focus of structural analyses. The reason for this is that the modelling of the level of financial development has drawn on ideas in the macroeconomics and finance literatures on market frictions. For example, Azariadis and de la Croix (2006) focus on financial market liberalization, which is modelled as the lifting of credit constraints for individual consumers from an initial condition in which borrowing is not allowed. This paper focuses on how both human and physical capital accumulation are affected and shows that deregulation can exacerbate inequality, when the relatively poor are not affected by the introduction of a market to allow for human capital loans. An important feature of this study is that it distinguishes between transition effects, where lower physical capital accumulation (in response to human capital opportunities) can hurt the disadvantaged versus steady state effects. These theoretical possibilities are shown to be quantitatively important. Greenwood, Sanchez, and Wang (2013) interpret financial development as improvements in the ability of lender to monitor borrowers and link this capacity to reduction in interest rate spreads between types of assets. A calibration based on the US and Taiwanese financial and output data is used to demonstrate the quantitative importance of these improvements and to, in turn, argue that a country such as Uganda could more than double per capita output if financial “best practices” were implemented.

Structural analyses have also focused on political and social institutions. Seim and Parente (2013) develop a positive analysis of the coevolution of industrialization and democracy, in which an elite chooses to shift a society from autocracy to democracy, essentially because of the levels of taxes and expropriation that exist under different political regimes. De la Croix and Delavallade (2015) argue that differences in southeast Asian economic development can be understood by different religious beliefs, as they affect attitudes towards children. Here religious heterogeneity is interpreted as preference heterogeneity.

These papers are useful in corroborating that various theoretical relationships between institutions and aggregate output can be quantitatively important. In this sense, these studies demonstrate that empirical evidence on the role of certain institutions *may* conclude that their effects are first order. The limitation of these studies is the standard limitation of calibration exercises. While the extent to which the model matches data moments can give an indication of whether the model can approximate the phenomenon under study, it does not address the question of whether it does. This does not mean that these exercises do not have empirical value. It is important to understand whether a particular low dimensional approximation is or is not close to the complicated reality that is approximated. Again, the value of these exercises is that they can update prior theoretical beliefs by interaction with data.

5 Historical studies

The most compelling evidence on institutions, in my judgment, comes from the study of institutions in historical context. This approach has combined rigorous economic theory with deep understanding of context. There are both microeconomic and macroeconomic versions of such work.

On the microeconomics side, a pioneering analysis is due to Greif (1989, 1993, 1994) who in a series of papers argued that reputation mechanisms emerged among Maghribi traders to enforce contracts. This work was based on detailed archival analyses and remains an exemplar. The claims have not gone unchallenged. Edwards and Ogilvie (2012) argue that formal legal rules were essential; see Greif (2008) for an unpublished response to the working paper version of Edwards and Ogilvie. This dispute does not question the role of institutions in facilitating Maghribi trading relations, but rather as to whether informal or formal institutions are key.

Other uses of history have involved the identification of long run effects of particular institutions. Rather than focus on institutions in a particular time period, these studies focus on how particular institutions have created path dependence in socioeconomic outcomes. Table 4 gives examples of these types of studies and indicates the breadth of this literature.

An impressive example of using a given historical context to identify long run institutional effects is Dell (2010) who explores the effects of a particular forced labor mining system, called mita, that operated in parts of Bolivia and Peru between 1573 and 1812. The system applied to a particular region, which allows Dell to compare developmental differences between adjacent mita and non-mita locations in an Andean area of Peru where population observables such as ethnicity are identical. This allows for application of regression discontinuity methods. Dell identified a substantial negative effect of the mita system on contemporary household consumption and child health. The success of this empirical strategy derives from the use of a sufficiently narrow context where one can plausibly argue that the plethora of “other” reasons for economic development heterogeneity are not present. This justifies the use of the regression discontinuity approach. Put differently, Dell identifies a context where the open-endedness of growth theory does not impede credible inference.

This analysis is also powerful because Dell is able to move beyond a credible identification argument to uncover the effects of the mita system and explore the channels that led to the consumption and health effects described above. This second stage exercise identifies lower levels of market participation, i.e. prevalence of subsistence farming as a proximate cause, and adverse effects of the mita system on provision of public goods such as roads and education as well as the adverse effects of mita on large landholding formation. The paper is an exemplar of integrating different sources of information into a comprehensive analysis.

The strengths of microeconomic studies, which derive from the specificity of the environments studied, naturally delimit the extent to which the empirical work can be extrapolated to other contexts or to general policy conclusions. Other studies focus on more macroeconomic contexts and have, in turn, led to more general claims about the institutions/growth nexus. To be clear, there is no clean dichotomy between microeconomic and macroeconomic studies. But, in my view, the distinction is important because it is suggestive of the identification problems involved in a given exercise.

The most visible example of macroeconomic studies are the pioneering studies by Acemoglu and Robinson, who have developed a broad theory of institutional differences to explain cross-country per capita income differences; Acemoglu and Robinson (2012) is a popular summary. This work has important predecessors in the economic history literature, notably North (1990) as well as relatively contemporary work such as Engerman and Sokoloff (2008). That said, the Acemoglu and Robinson program has been transformative by the integration of theoretical and historical analysis; more on this below.

Another dimension of this work has been driven to an important extent by the development of new data sets. As such, the success of these studies is an exemplar for what Caplin (2016) has called economic data engineering. Following Caplin, data sets have been, in the institutional literature, constructed in order to answer particular questions. Notice that this empirical strategy overcomes identification problems and complements the use of economic theory. The strategy is distinct from the search for natural experiments. While the choice of contexts for which to construct data is analogous to the search for a natural experiment, in the case of historical contexts, choosing one is only the start of a data construction process.

One important and well known example of this type of research is Nunn (2008) which studies the long term effects of the African slave trade on economic development. Nunn finds that the intensity of the slave trade that occurred in a contemporary nation (measured by the ratio of slaves exported to area of the country) is negatively associated with per capita GDP, a finding that is robust when one uses geographic instruments relevant to slave trade profitability to account for endogeneity of slave trade intensity.

A general issue in historical studies is the uniqueness of the institution studied relative to the overall developmental history of a country. To see why this needs to be done, consider the question of identifying how serfdom affected Russian economic development, which has been studied recently by, among others⁷, using cross-regional variation in the number of serfs and other measures of the extent of that institution. In order for long run data to identify a causal effect of spatial variations in serfdom intensity, it is necessary that the various momentous events in Russia, be it the famine of 1893, the civil or world wars, did not generate spatial effects that are correlated with serfdom intensity. The general problem is that if one relies on variations in a particular “large” event that occurs at one point in time, its influence will be preserved only if subsequent events are uncorrelated with it. Even if the individual events are, in isolation, *ex ante* independent of the one under study, a researcher has to be able to argue that none of them is *ex post* correlated with the initial event. Returning to the serfdom example, if spatial variation from famines and wars occurred independently from serfdom variability, that does not mean that none of them are *ex post* correlated. And once one considers how geographic or other factors operating at a lower time scale interact with the locations of events such as prevalence of serfdom or intensity of famine or locations of battles, it becomes evident that the independence assumption is itself extremely difficult to defend as plausible.

Returning to Nunn, one might argue that in order to demonstrate contemporary effects in Africa from the slave trade one needs to account for any correlations between the patterns of enslavement and the effects of ethnic conflict induced by arbitrary borders, which has been argued by Easterly and Levine (1997) among others, as an explanation. Nunn does not

⁷ To be clear, I discuss identification issues for these papers because they are significant research contributions.

directly address this issue, but argues (to be fair, tentatively) that higher levels of slave trade damaged the capacity for identities to emerge at higher levels of aggregation than ethnicity and so are causal with respect to the Easterly and Levine findings. This may well be the case, but in my judgment this indicates the importance of bringing more theoretical structure to such econometric exercises if one is interested in coevolution of factors at different time scales.

While obvious in the abstract, it is important to recognize that successful use of historical episodes requires deep immersion in the details of the relevant history. Ogilvie and Carus (2015) argue that a number of the standard lessons of the historical literature for the role of institutions in growth are overstated. At one level, these authors challenge the conclusions of particular episodes, I have discussed the example of the Maghribi traders above. At another level, they challenge monocausal approaches to institutions. For example, Ogilvie and Carus argue that contracting institutions, which involve private relationships, should be understood as functioning on the same plane as property rights and that the two interact, rather than being separate spheres. As such, Ogilvie and Carus make a persuasive argument that one should focus on portfolios of institutions as levers of development and growth, both because of interaction effects and because different configurations of institutions can produce similar effects. These are issues to which I will return below.

6 Abduction and the state of knowledge on institutions and growth

Where does the empirical literature on institutions and growth stand? There is much empirical evidence, regression, structural, historical, that supports a role for institutions in growth and development. There is no general disagreement on this proposition from any field of economics, be it economic history, development, or growth economics. But does the now banal claim that institutions matter translate into a first order conception of the determinants of economic prosperity or failure? Put differently, while a study such as Rodrik, Subramanian, and Trebbi (2004) evaluates whether institutions belong in the “true” model of fundamental determinants to growth, it is not designed to measure empirical salience.

Acemoglu and Robinson (2005, 2012), and their background research that underlies these, is arguably the most prominent exception to this claim as they integrate evidence from many contexts to draw very broad conclusions that institutions are fundamental to understanding historical patterns of development and growth.⁸ Combining evidence from stark comparisons such as North versus South Korea, historical examples such as the Mayan and Roman Empires, as well as evidence from formal econometric methods, these authors have developed general propositions that the institutions that protect private property while simultaneously ensuring that the benefits of economic development and growth are inclusive, constitute a powerful way to understand why nations prosper or stagnate. Further, they argue that institutional differences with respect to these features provides insights for contexts ranging from capitalist to socialist/centrally planned economies, advanced industrialized nations versus sub Saharan Africa, and within-region variation in development.

While their work is not presented using this language, the Acemoglu and Robinson research program should be understood as an example of abduction (Heckman and Singer (2017)), also known as inference to the best explanation. Abduction is the stuff of science, i.e. the drawing of conclusions from a host of evidentiary sources. Such inferences are neither deductive nor inductive. They involve complex judgments in integrating evidence from different sources. Abduction underlies much of the best empirical social science. Why? Because each type of evidence that can bring to bear on a topic such as the role of institutions in development and growth has important limitations. Formal econometric exercises identify general patterns, but they either are correlative in nature or contingent on assumptions of questionable validity. Historical examples may resemble experiments, but the heterogeneity of contexts makes it difficult to extrapolate. Structural modelling approaches are similarly contingent on assumptions that are questionable. For each approach, theory open-endedness delimits the evidentiary value of models. The point is not that each source of evidence is worthless, but that each is limited. Acemoglu and Robinson develop their generalizations from a variety of empirical approaches.

The abductive nature of the Acemoglu and Robinson research program also involves the rich interplay of theory and empirics. Again, this process underpins abduction. For example, consider how one accounts for endogeneity of institutions in assessing the effects of institutions. I have criticized instrumental variables approaches to endogeneity. The constructive approach is to explicitly examine the determinants of institutional quality, and to

⁸ The work of the school of economic history associated Douglass North is a fundamental predecessor.

use theory to structure how one draws inferences about institutions as mechanisms, for example from historical data.

Abduction is useful in interpreting critiques that have been made of the work, notably the Acemoglu and Robinson (2012) book *Why Nations Fail*. MacLeod (2013) argues that the absence of counterfactuals in historical examples, means that the role of institutions is not identified from them. This view equates the substantive question of the role of institutions in development and growth with the statistical question of whether historical evidence maps into a particular model of causality. They are not equivalent. In contrast, Sachs (2012), who argues against the conceptual framework on the ground it oversimplifies the process of development and prosperity at the expense of other factors, notably geography, can be placed in a comparison with Acemoglu and Robinson using abductive arguments.

Subramanian (2012) focuses on the utility of their general framework for understanding India and China. Here I only note that adjudication of these critiques itself requires abduction, which involves specifying explicit alternative theories to Acemoglu and Robinson (a theory of growth based on geography, a theory of growth based on interplay of geography and growth, etc.) and evaluating a range of qualitatively different types of evidence. One can do this as well in terms of evaluating Chinese exceptionalism as an alternative to the general Acemoglu and Robinson theory. Here abduction will involve assessment of the extent to which the broad theory addresses evidence from specific cases in sufficient depth and to what extent generalization across cases is warranted.

To be clear, the use of qualitative and quantitative evidence is a hallmark of the new institutional economics. Timur (2018) is a recent deep example of abduction with reference to understanding the effects of Islam.

7 Conclusions

The summary of my arguments is simple: institutions have been established to matter both in general and in a host of interesting specific contexts. This is true whether one considers a particular institution as broad as democracy or as pathological as slavery. It is also true when the objective is to explain why different regions of the world, or countries within regions exhibit substantial heterogeneity.

As rich, and in my judgment persuasive, as the current literature is, there are areas where I would recommend as fruitful research directions.

First, I believe that the emerging literature on the interplay of culture and institutions is an especially important development. Alesina and Giuliano (2015) surveys the state of this literature while Bisin and Verdier (2017) is an important recent example of the formal modelling of culture/institutions coevolution. By culture, I follow a commonly used definition from Guiso, Sapienza and Zingales (2006, p.23) who interpret culture as

“...those customary beliefs and values that ethnic, religious, and social groups transmit fairly unchanged from generation to generation.”

In this respect, I disagree with definitions of institutions that include values and beliefs. This is because institutions are intrinsically involved with interactions between individuals and rules that direct these interactions, and so it is appropriate to define them at a level of populations beyond an individual. The cultural economics literature, as surveyed by Guiso, Sapienza, and Zingales or Alesina and Giuliano also matters for understanding the endogeneity of institutions.

In this spirit, there is much value in expanding the understanding the interplay of institutions with the emergence of ethics. As formalized by Roemer (2010) among others, ethical conduct represents a path to the solution of collective action problems that is conceptually distinct from institutions per se. As demonstrated by Alger and Weibull (2013), for example, “homo moralis” can emerge via repeated interactions. It has long been understood that market activity presupposes behaviors that move beyond self-interest (Arrow (1974)). In my judgment, the usual study of norms as outcomes of repeated games with sanctions is usefully complemented by a recognition that there are normative influences on choice that are better understood as constraints as opposed to arguments of the utility function per se; this view is found in Baland and Platteau (2000), who (chapter 6) refer to “norms and constraints on self interest”. The importance of ethics in development is elaborated in Platteau (1994a,b, 2000) who draws an important distinction between social and moral norms, where the latter involve internalized senses of guilt and shame. Bardhan (2000) is one empirical example of how ethics affect collective actions problems in irrigation. Further, a number of authors have shown how institutions can affect ethics. One example is Bardhan’s (2005) emphasis on democratic participation, which has much salience, if one wishes to understand how the ethical underpinnings needed for a Constitution to function can be created. In contrast, Falk and Szech (2013) have shown how markets, whose behavior is one key objective of institutional design, can diminish the impact of certain ethical values on choices. Systematic development of the ethics/institutions relationship seems very promising.

Second, I believe the successes of the institutions and growth literature in terms of producing *de facto* abductive analyses would be enhanced by the development and use of explicit tools for abduction. This recommendation, of course, does not just apply to the subject of this article, but to any social science context in which multiple sources of evidence are available. Heckman and Singer (2017) provide a general conceptualization of abduction in economic analysis. In the institutions context, one area where there is a need for formal methods is suggested by my discussion of the identification problem for the persistent effects of large historical episodes. I argued that there are two problems with work of this type: *ex post* correlation of historical events that are *ex ante* independent as well as correlation in the events. These problems are not, I believe, amenable to standard econometric methods. I do not pretend to have suggestions on how to resolve these problems for historical studies, but their resolution is critical for credible identification. More generally, the explanation of major historical events is a key objective of the institutions literature, and so naturally requires abduction. Beyond formal tools, there are gains from trade in considering successful *de facto* examples of abduction outside of institutions framework, e.g. Donohue and Heckman (1991) to see how the weighing of evidence compares to examples in the institutions literature,

Formal tools for abduction are still in their infancy. Such tools amount to the creation of rules by which different sorts of evidence, in the current context, historical studies, regression analysis, random controlled trials, and the like, are quantitatively integrated to evaluate propositions. Katz and Singer (2007) is a rare example of such an endeavor. Their work, however, involves a concrete question, whether yellow rain observed in Southeast Asia was due to a biological weapon. The question of whether institutions constitutes a successful monocausal theory of development is profoundly different as it involves assessing the theory as an approximation against more complicated alternatives. It is a commonplace that successful social science theories are low dimension approximations of a complex reality. What is needed are more systematic ways of drawing such judgments.

Third, I see a need for more work on the translation of evidence on institutions into policy recommendations. It is relatively straightforward to identify institutions to be avoided. Further, there now exists a number of well-established general policy conclusions that may be drawn from the extant empirical literature. There is much consensus with respect the Acemoglu and Robinson view that institutions which 1) secure property rights so that there are incentives for productive economic activity and 2) are inclusive across broad parts of populations, appear ubiquitous when one considers sustained economic success. Wallis and Weingast (2009) and North, Wallis, Webb and Weingast (2012) have made a very compelling case for violence reduction as fundamental to how institutions facilitate development. Similarly, there is no serious disagreement about the importance of the rule of law, be it in terms of formal governance a la La Porta, Lopez-de-Silanes, and Shleifer (2008) or private rules that Dixit (2004) defines as the areas of “lawlessness and economics.” However, the evidence for these and other very broad claims, persuasive in terms of general theory, does not lead to obvious policy conclusions, once egregious institutions are avoided. Each of these perspectives identifies how institutions can incentivize productive individual activities in ways that are simultaneously socially desirable in the presence of market frictions. These theories are not tautological as they provide reasons why these individual and aggregate effects occur. But the mapping of general propositions to microeconomic specifics requires much additional argumentation. This also has implications for the

experimental side of development economics. Pande and Udry (2005) make this argument in the context of the class of decentralized interventions that are the hallmark of randomized controlled studies. While some policy interventions may be efficacious regardless of institutional environment (Banerjee and Duflo (2011)), to conclude from those successes that one should delimit policy interventions to those that are “institutions-robust” is not tenable from any decision-theoretic perspective of which I am aware.

Concretely, the policy side of the institutions and growth literature would benefit from more attention to decision theory. This is so in several respects. First, policy recommendations should be thought of in terms of portfolios. The desirable incentive effects for sustained growth can be produced by multiple configurations of institutions. The Asian tigers differ along many institutional dimensions from the UK, US, and Australasia. And policies are of course synergistic. In general, the sorts of questions that define positive research on institutions and economics usually focuses on whether particular institutions matter. My claim is a corollary of the Ogilvie and Carus (2015) critique of historical studies of institutions and growth. The one addendum I would make is to emphasize the contextual specificity of portfolio weights, whose determination requires the “thick” description that Ogilvie and Carus argue is needed in historical work.

Second, there are always questions of the second best. Are institutions that reduce corruption always growth enhancing? Presumably not, if the corruption mitigates expropriation or bureaucratic barriers to innovation. This is a variant of the standard problem of external validity: does evidence that corruption hurts growth in one context apply to another?

Third, the issues of model uncertainty, for example possible nonlinearity, that have mattered for the institutions and regression literature need to be integrated into policy assessment. Here I really am doing no more than suggesting that explicit decision-theoretic formulations be used when policy recommendations are made.

In conclusion, there are many empirical successes for the institutions, development and growth literature and at the same time many important unanswered questions. Hence this literature should continue to be vibrant.

Table 1: Regression analyses linking institutions, economic development, and growth

| Institution | Study | Respective Claims (effect, significance) |
|--|--|--|
| Competitiveness in executive recruitment | Coyne and Tan (2012) | (+,*) |
| Democracy | Acemoglu, Naidu, Restrepo, and Robinson (2015), Barro (1996), Helliwell (1994), Papaioannou and Siourounis (2008), Persson and Tabellini (2006), Persson and Tabellini (2009), Tavares and Wacziarg (2001) | (+,*), (+/,-,*), (-,0), (+,*), (+,*), (+,*), (-,*) |
| Economic, political, civic freedom | Dawson (1998) | (+,*) |
| Executive constraints | Acemoglu, Johnson, and Robinson (2002), Acemoglu, Johnson, and Robinson (2005) | (+,*), (+,*) |
| Financial development | Benhabib and Spiegel (2000) | (+,*) |
| Forced mining labor (<i>mita</i>) | Dell (2010) | (-,*) |
| Government consumption share of GDP | Barro (1991) | (-,*) |
| Institutional quality index | Bosworth and Collins (2003), Easterly and Levine (2003), Glaeser, Porta, Lopez-De-Silanes, and Shleifer (2004), Tan (2010) | (+,*), (+,*), (0,0), (+,*) |
| Meta-study, various | DELETEDefendic, Pugh, and Adnett (2011) | (+,*) |
| Political instability | Alesina, Özler, Roubini, and Swagel (1996) | (-,*) |
| Property rights | Dawson (2003) | (+,*) |
| Protection against expropriation risk | Acemoglu, Johnson, and Robinson (2001), Acemoglu, Johnson, and Robinson (2002), Gorodnichenko and Roland (2011) | (+,*), (+,*), (+,*) |
| Public investment share of GDP | Barro (1991) | (+,0) |
| Religion | Acemoglu, Johnson, and Robinson (2001,2005) | (0,0) |
| Rule of law | Dollar and Kraay (2003), Mehlum, Moene, and Torvik (2006), Rodrik, Subramanian, and Trebbi (2004) | (+,*), (+,*), (+,*) |
| Social infrastructure (government anti diversion policies, trade openness) | Hall and Jones (1999) | (+,*) |
| Use of markets | Dawson (2003) | (+,*) |

Legend:

- (,*) : significance
- (, 0) : not significant
- (+,) : positive finding
- (-,) : negative finding
- (+/-,*) : not linear significant effects
- (0,0) : evidence of a zero effect

Table 2a: Instrumental variables used in cross-country studies

| Instrumental Variable | Study |
|---|--|
| Agricultural crops that have higher production economies of scale than others like sugar, coffee, rice, and cotton | Easterly (2007), Engerman and Sokoloff (1997, 2000, 2002), Isham, Woolcock, Pritchett, and Busby (2005), Laeven and Woodruff (2007) |
| Distance from equator | Ahlerup, Olsson, Yanagizawa (2009), Bocksette, Chanda, and Putterman (2002), Burnside and Dollar (2004), Edison, Levine, Ricci, Sløk (2002), Eicher and Leukert (2009), Hall and Jones (1999), Kogel (2005), Masters and Macmillan (2001) |
| English speakers | Bocksette, Chanda, and Putterman (2002), Burnside and Dollar (2004), Dollar and Kraay (2003), Kogel (2005), Masters and Macmillan (2001) |
| Ethnolinguistic fractionalization | Aidt (2009), Clague, Keefer, Knack, and Olson (1999), Easterly and Levine (1997), Easterly, Ritzen, and Woolcock (2006), Knack and Keefer (1997), Mauro (1995) |
| European-language speakers | Alcala and Ciccone (2004), Bocksette, Chanda, and Putterman (2002), Burnside and Dollar (2004), Dollar and Kraay (2003), Eicher and Leukert (2009), Hall and Jones (1999), Kogel (2005), Masters and Macmillan (2001), Rodrik (1999), Rodrik, Subramanian, and Trebbi (2004) |
| Gravity equation for bilateral trade flows | Alcala and Ciccone (2004), Dollar and Kraay (2003), Frankel and Romer (1999), Rodrik, Subramanian, and Trebbi (2004) |
| Initial inequality | Easterly, Ritzen, and Woolcock (2006) |
| Latitude | Diamond (1997) |
| Legal origin | Acemoglu and Johnson (2005), Aghion, Howitt, and Mayer-Foulkes (2005), Ahlerup, Olsson, Yanagizawa (2009), Claessens and Laeven (2003), Clague, Keefer, Knack, and Olson (1999), Edison, Levine, Ricci, Sløk (2002), Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004), La Porta, de Silanes, and Shleifer (2008), Levine (1998), Levine, Loayza, Beck (2000), Papaioannou (2009) |
| Log of indigenous population density in 1500 | Acemoglu, Gallego, and Robinson (2014), Acemoglu and Johnson (2005), Burnside and Dollar (2004), Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004), Papaioannou (2009) |
| Malaria index | Gooch, Martinez-Vazquez, and Yedgenov (2016) |
| Mean distance from the ocean or a navigable river | Ahlerup, Olsson, Yanagizawa (2009) |
| Natural resources like oil and minerals | Isham, Woolcock, Pritchett, and Busby (2005), Sala-i-Martin and Subramanian (2008) |
| Percentage of law students in 1963 | Knack and Keefer (1997) |
| Predicted trade share | Alcala and Ciccone (2004), Bocksette, Chanda, and Putterman (2002), Kogel (2005), Masters and Macmillan (2001), Rodrik (1999), Rodrik, Subramanian, and Trebbi (2004) |
| Settler mortality | Acemoglu, Gallego, and Robinson (2014), Acemoglu and Johnson (2005), Acemoglu, Johnson, and Robinson (2001), Acemoglu, Johnson, and Robinson (2002), Acemoglu, Johnson, Robinson and |

| | |
|--------------------------------|--|
| | Thaicharoen (2003), Aghion, Howitt, and Mayer-Foulkes (2005), Alcala and Ciccone (2004), Alfaro, Kalemli-Ozkan, and Volosovych (2008), Claessens and Laeven (2003), Dollar and Kraay (2003), Easterly and Levine (2003), Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004), Rodrik (1999), Rodrik, Subramanian, and Trebbi (2004), Satyanath and Subramanian (2004) |
| State antiquity | Bardhan (2005), Bocksette, Chanda, and Putterman (2002), Kogel (2005) |
| Tropics, germs, and crops | Easterly and Levine (2003) |
| Variability in rainfall | Haber and Menaldo (2011), Nugent and Sanchez (1999) |
| Voice and Accountability Index | Aidt (2009) |

Table 2b: Instrumental variables used in case studies

| Instrument | Study |
|--|---|
| Agricultural tenancy reform | Banerjee, Gertler, and Ghatak (2002) |
| Being conquered by the British between 1820 and 1856 | Banerjee and Iyer (2005) |
| Change in eligibility for formal sector loan | Banerjee and Duflo (2013) |
| Ethnolinguistic fractionalization | Miguel and Gugerty (2005) |
| Implementation of the Trade-Related Intellectual Property Rights Agreement | Chaudhuri, Goldberg and Jia (2006) |
| Industry-location averages of bribery and taxation | Fisman and Svensson (2007) |
| Land reform | Do and Iyer (2003) |
| Political connections | Fisman (2001) |
| Reforms that are related to market liberalization, an innovative ownership form of firms, and large-scaled state owned enterprises | Qian (2003) |
| Variation across regions induced by the timing of the national tilting program and differences across target populations in level of preprogram ownership rights | Field (2007) |
| Variation in mandated political representation for disadvantaged groups | Pande (2003) |
| Variation in privatization of water services | Galiani, Gertler, and Schargrodsky (2005) |
| Variation in rural bank branch expansion | Burgess and Pande (2005) |
| Variation in spread of debt relief tribunals | Visaria (2009) |
| Variation in states which amended the Industrial Disputes Act in a pro-worker direction | Besley, and Burgess (2004) |

Table 3: Calibration studies

| Variable of Interest | Study | Claim |
|--|---|-------|
| Democracy | Seim and Parente (2013) | + |
| Entrepreneurs | Dias and McDermott (2006) | + |
| Epidemic shocks | Lagerlof (2003) | - |
| Financial development | Greenwood, Sanchez, and Wang (2013) | + |
| Financial frictions | Buera, Kaboski, and Shin (2011), Buera and Shin (forthcoming) | -, - |
| Financial market liberalization | Azariadis and de la Croix (2006), Gine and Townsend (2004) | + |
| Limited enforcement | Amaral and Quintin (2010) | - |
| Quality of legal institutions | Castro, Clementi, and Macdonald (2009) | + |
| Religious affiliation | de la Croix and Delavallade (2015) | - |
| Thai Million Baht Village Fund program | Kaboski and Townsend (2011) | - |

Table 4: Effects of various historical events on variables plausibly related to growth

| Institution | Study | Claim |
|---|---|-------|
| Atlantic trade with the New World, Africa, and Asia | Acemoglu, Johnson, and Robinson (2005) | + |
| Credit markets | Azariadis and de la Croix (2006) | + |
| Culture | Tabellini (2010) | + |
| External trade in slaves | Nunn (2008) | - |
| Investor rights protection | La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1997, 1998) | + |
| Laissez-faire autocratic rulers (vs. kleptocrats) | Seim and Parente (2013) | + |
| Mitaforded labor system | Dell (2010) | - |
| Non-landlord revenue collection institutions (vs. landlord) | Banerjee and Iyer (2005) | + |
| Podesteria (vs. consulate) | Greif (1997) | + |
| Promoting industrial policies before opening up to trade | Akerman, Nghavi, and Seim (2016) | + |
| Property rights | Acemoglu, Johnson, and Robinson (2001) | + |
| Protestantism (literacy) | Becker and Woessmann (2009) | + |
| State antiquity | Bockstette, Chanda, and Putterman (2002) | + |
| Terrorist activity | Abadie and Gardeazabal (2003) | - |
| The indirect form of the British rule (vs. direct) | Iyer (2010), Lange (2004) | +, + |
| The length of colonial rule | Feyrer and Sacerdote (2009) | + |

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