CROSS-COUNTRY QUANTITATIVE STUDIES OF POLITICAL DEVELOPMENT*

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La política comparada le ha dedicado considerable atención a la cuestión general del desarrollo político. En este trabajo reviso brevemente las tendencias en esta área de los últimos cincuenta años, y luego considero lo que podríamos haber aprendido de estos análisis en cuanto a hechos estilizados adoptando un enfoque más cuantitativo. Se le presta especial atención a través del texto al rol de la evidencia en la evaluación de los argumentos sobre el desarrollo político, aduciendo que la distinción entre análisis cuantitativo y cualitativo es menos clara de lo que se sugiere normalmente. Es de mucha importancia que el razonamiento cualitativo juega un rol central en cuestiones de diseño de investigación y medición, incluso en estudios considerados como paradigmáticos del enfoque cuantitativo. También juega un rol central en la interpretación de las regularidades generadas por los análisis cuantitativos. Dado que ambos enfoques comparten una lógica común de investigación, tenemos más confianza en nuestras inferencias cuando estudios cuantitativos y cualitativos nos llevan a conclusiones similares.

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For the past half century, comparative politics has paid considerable attention to the analysis of political development, broadly conceived. In this paper, I briefly review our collective efforts to gain traction on this topic. I then consider what we may have learned in the form of stylized facts from those analyses adopting a more quantitative approach to these issues. I pay special attention throughout to the role of evidence in the evaluation of arguments about political development. In this connection, I argue that the distinction between quantitative and qualitative analyses is more blurred than many would have us believe. Specifically, qualitative reasoning plays a central role in questions of research design and measurement, even in apparently otherwise quantitative studies. It also plays a central role in the interpretation of observed regularities generated by quantitative analyses. Further, whatever the research design, we seek answers to similar questions, and our confidence in inferences is typically enhanced when quantitative and qualitative studies point us in the same direction.

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THE LAST HALF CENTURY

As in many other areas of the social sciences, fads and fashions seem to have come and gone in the analysis of political development. Indeed, a sensible observer could readily conclude that the field as a whole has operated substantially independent of any evidence in the sense that the popularity of a particular perspective at a given point in time cannot plausibly be traced to the consistency of that perspective with observed political patterns. Consider the record.

In the 1950s and 1960s, the field is said to have been organized by the "modernization" perspective. This focus was triggered by external events in the form of the wave of decolonization of the former European empires that began after World War Two and that culminated in 1960. Confronted with a large number of "new" states, the problem was cast as analyzing political differences between old and new states. Further, there was considerable interest in learning how the newer states might rapidly come to resemble the older states more closely, as policy makers of the time equated decolonization with democratization, and because they articulated a strong interest in reducing global poverty (for example, with the founding of the Peace Corps during the Kennedy Administration).

To some extent, the modernization studies are associated with the quantitative approach to political analysis because they were seen as part of the behavioral revolution in political science (on which, see Dahl 1961). Not unreasonably, they drew on cognate fields in the social sciences like anthropology and sociology for inspiration in an effort to develop general approaches to the topic that were not mired down in what was seen as an excessive interest in the legalistic approaches of the past and that thus centered on what political actors actually do, as opposed to what the rules say they are supposed to do. However, most studies in the genre were not distinctively quantitative. Beyond this, the studies have little in common, and the modernization label is confusing because the set of studies included under the rubric is heterogeneous.

Recall that modernization studies are typically taken to include the functional and systems approaches to political development generated by Easton (1957) and Almond and Coleman (1960). While there are many parallels between these two analyses, neither was empirical in content. Instead, each offered a general framework modified from other disciplines to the analysis of political development. These frameworks were less concerned with specifying empirical hypotheses than they were with suggesting relevant areas of inquiry. For example, both studies emphasized the origins of political support for regimes and thus the importance of political socialization, an emphasis that spawned considerable empirical work on the issue. Other analyses typically included under the modernization rubric were more directly empirical. Thus, Banfield (1957) and McClelland (1961) considered the ways in which values might influence economic growth, in an effort to generalize the Protestant ethic argument. Similarly, the evaluation of political culture from Almond and Verba (1963) centered on the values among the public that might be conducive to political development, especially to democratic development. At the same time, Deutsch (1961) addressed possible consequences of the social changes associated with industrialization for political development. Taking a quite different tack, Huntington (1968) presented an influential argument for the importance of institutionalization.

Although typically cast as epitomizing the modernization approach, these studies have little in common either methodologically or empirically, beyond an interest in developing

generalizations. Banfield's book reports on largely qualitative fieldwork completed in a small village in southern Italy, while Almond and Verba is based on a large-scale set of public opinion surveys conducted in five countries. Deutsch's analysis centers on cross-country comparisons of aggregated statistical data, and has a quite populist flavor that emphasizes the value and consequences of mass political participation. Huntington's approach, by contrast, synthesizes a large number of primarily qualitative studies, and places more emphasis on an apparent need to contain political participation. Thus, these studies share neither the common methodological approach nor the unified theoretical orientation implied by the "modernization" label[1].

In the 1970s, the modernization label fell into disrepute and was supplanted by a general dependency perspective. Again, this view is somewhat heterogeneous and constitutes a largely political argument comprised of an uneasy mixture of Marxist and nationalist threads (for a classic statement of the position, see Cardoso and Faletto 1978). Among other things, the dependency perspective suggested that changes associated with decolonization were more apparent than real, that imperialism had largely survived intact, that economic growth was unlikely within dependent economies, that economic growth has limited benefits, and that political development itself was unlikely.

Dependency theory evolved with a number of ad hoc adaptations before its demise. For example, faced with the fact that some dependent economies were manifestly experiencing growth, a case was made for economic growth in semi-peripheral states, although the growth was described as uneven (see e.g., Evans 1979). But, like the so-called modernization approach, dependency theory was substantially driven by external events, in this case taking the form of widespread disaffection with American foreign policy initiatives. When that disaffection moderated, the perspective lost favor. In other words, the demise of dependency theory did not result from the use of empirical criteria in the form of some decisive disconfirmation. Instead, it was due to renewed interest in issues of democratization, an interest driven by global political events of the 1980s that culminated in the end of the Cold War. For the past several years, of course, the field has been motivated by this renewed interest in democratization, taken to represent the "third wave" of democratization (Huntington 1991)[2].

It is noteworthy that successive "new" approaches like these were often heralded as representing a new paradigm in Kuhn's (1962) sense. However, Kuhn argued that a new paradigm supplants an older one when it can (a) explain phenomena already explained by

1 This doubtless explains the difficulty critics of modernization seem to experience in their attempts to define the term. For example, in his well-known obituary for the concept, Wallerstein (1979) defined it solely in the terms employed by Banfield (1957) and McClelland (1961). Most recently, Przeworski et al (2000, 88) write that "the basic assumption of [modernization] theory is that there is one general process, of which democratization is but the final facet". Perhaps these writers take modernization to be the view that "all good things go together", to use Huntington's term (1968, 5). While this view may describe Banfield (1957), McClelland (1961), and (more arguably) Almond and Verba (1963) with some accuracy, it misrepresents other analyses typically taken as exemplars of the modernization approach, including Deutsch (1961) and Huntington (1968) himself.

2 Other approaches (for example, the resurgence of interest in the state during the 1980s and in civic culture during the 1990s) seem to be endorsed on a more cyclic basis. Indeed, the revival of state-centered analyses was explicitly cast in these terms, with the emphasis on "bringing the state back in" (Evans, Rueschemeyer, and Skocpol 1985). Similarly, studies of political culture were common in the 1960s after Almond and Verba (1963), but then languished until the early 1990s, at which point a number of scholars have sought to revive them. For further discussion on these issues, see Jackman (1993, chap. 2) and Jackman and Miller (1998).

the previous paradigm along with (b) other phenomena unexplained by its predecessor. Two points bear emphasis here. First, both (a) and (b) are required according to Kuhn. Second, there is an empirical or evidentiary basis for the abandonment of an older paradigm in favor of a newer one.

What has been striking about comparative politics in general (and the analysis of political development in particular) has been the failure of either newer or older paradigms to account for *anything*. For example, functional and systems theory approaches of the 1960s failed because they were analogies rather than theories. Although sometimes cast as organizing frameworks, the fact that they generated no distinctive empirical implications is their most obvious problem. While the dependency approach generated more concrete empirical claims, at no stage did the acceptance of the perspective depend on the evidentiary status of those claims.

Given the above, a reasonable observer might conclude that, protests to the contrary notwithstanding, questions of evidence have typically been incidental to the rise and demise of general theoretical approaches to political development.

WHAT STYLIZED FACTS HAVE WE HAVE LEARNED IN THE LAST THREE TO FOUR DECADES?

Much attention has been paid to shifting paradigms in the field, and many readers will be familiar with the chronology outlined above. However, our collective interest in paradigms has not always been accompanied by a parallel concern with evidence, even though evidence plays a key role in Kuhn's analysis of paradigms. Indeed, it is instructive to step back from the common language of paradigms. When we take this step, we observe immediately that not all scholars located themselves within a particular paradigm or approach, even if they were subsequently pigeon-holed by others. Analyses like Lipset (1960), Deutsch (1961), and Huntington (1968), for example, did not attempt to promulgate a new paradigm but instead offered interpretive syntheses of empirical research. They focused on questions of evidence, paying special attention (for the time) to questions of measurement. They reached differing conclusions so that their arguments were mutually distinctive, of course, but they had the singular virtue of being subject to disconfirmation. Indeed, many of them have been disconfirmed.

Focusing on the more quantitative studies, it is reasonable to ask what we have learned in the recent past[3]. Here, my discussion centers on "stylized facts", for want of a better term. I take a stylized fact to refer to an observed empirical regularity that typically but not invariably involves a causal argument. In some cases, current stylized facts are consistent with earlier arguments, while in other instances they turn what used to be conventional wisdom on its head.

Stylized facts do not in themselves constitute a complete explanation. Among other things, they typically address one part of a broader problem, and they are often cast at high

3 My focus on quantitative studies is not meant to imply that these studies have been the only source of useful evidence in the analysis of political development, but simply serves to focus the discussion in a manageable way.

levels of aggregation. This means that they have to be supplemented with other information, often of a qualitative form, and often generated at lower levels of aggregation. Nonetheless, they have major implications for broader arguments, and claims that are consistent with stylized facts are more compelling than those that are not[4].

Consider some examples. While I treat them separately, the following propositions are clearly interrelated.

Economic growth

Students of political development have, over the years, displayed considerable ambivalence on the desirability of economic growth. Among many scholars adopting the dependency perspective, this ambivalence reflected the view that growth outside the core countries is unlikely, and that any observed growth typically benefits a few at the expense of everyone else, in other words, that there is no trickle down effect. Beyond this group of scholars, many argued for abandoning a strong focus on growth in favor of a broader view of economic development that, it was claimed, placed greater emphasis on the fulfillment of basic human needs (e.g., Morris 1979). Other observers were persuaded that growth is destabilizing, especially in the short term (see especially Olson 1963, Huntington 1968)[5].

However, the evidence indicates that economic growth is a good thing. In particular, economic growth systematically improves performance on a number of key demographic and public health indicators that directly affect the lives of ordinary people, including life expectancy and infant mortality rates (e.g., Firebaugh and Beck 1994, Easterly and Levine 1997)[6]. Since there is absolutely no evidence that significant numbers of people anywhere prefer higher infant mortality rates, etc., growth would therefore seem to be an unambiguous benefit. This is not to contend that growth is a panacea that addresses all issues, but merely to suggest that it beats the known alternatives, which are decline or stagnation.

Corruption

With the exception of the dependency interregnum, a good deal of attention has been paid to questions of political corruption. The prevailing view in the 1960s was essentially a functionalist one, of the form: if it exists, it must serve some purpose. Thus, while corruption was not expected in the Western democracies, in the developing world it was regarded as potentially beneficial, a way of clearing hurdles in poorly institutionalized settings (see, e.g., Leff 1964, Huntington 1968).

4 In case this claim appears excessive, consider stylized facts in other settings. Many advances in medical research have begun with stylized facts generated by epidemiologists that link particular behaviors to distinctive health outcomes. The fact that the biological bases of such patterns may not be specified until much later does not undermine the significance of the patterns, which are often employed clinically to good effect absent the biological underpinnings, that is to say, absent the microfoundations. Similarly, as the parents of teenage drivers know only too well, actuaries in insurance companies use stylized facts to identify the insurance risk of particular demographic groups.

5 Perhaps these analysts might have been drawn to a different conclusion had they pondered the rhetorical question posed by Lopreato (1965): "How would you like to be a peasant?"

6 Indeed, questions of mortality rates are typically of profound importance. Consider the issue of the female population deficit addressed by Sen (1990, 1992) and Coale (1991).

However, recent quantitative studies show unambiguously that corruption is a bad thing, in the sense that it stifles economic growth. There is strong evidence that corruption inhibits growth both directly and indirectly, principally by suppressing investment (see, especially Mauro 1995). Taking this as a stylized fact, there simply is no basis for the earlier common claim that corruption might be beneficial under certain conditions.

Additionally, there is clear evidence that competition inhibits corruption (Ades and Di Tella 2000, Montinola and Jackman 2001). This competition takes both economic and political forms, and implies that transparency in decision-making is a key factor in reducing corruption. The linkage between political competition and corruption further suggests that democratic institutions more broadly are a key factor in this regard.

These two stylized facts obviously do not in themselves constitute a full theory of corruption. But they do form the basis for a more complete argument while showing that earlier more functionally based claims about corruption are wide of the mark.

Economic Development and Democracy

Lipset (1960) was the first to bring systematic data to bear on this topic, and concluded that there is a linkage between economic development and democracy, treating the former as a prerequisite to the latter. There has been considerable debate over his measurement of democracy (see below), and indeed there has been a good deal of discussion about the validity of the linkage itself. Nonetheless, the available evidence indicates that there is a systematic linkage between the two (see, e.g., Jackman 1973, Bollen and Jackman 1985, Burkhart and Lewis-Beck 1994, Londregan and Poole 1996, Barro 1999). Coming at the issue from a different tack, Londregan and Poole (1990) show that poverty is a key factor generating transitions to authoritarian rule. Paralleling this pattern, Przeworski et al. (2000, chapter 2) conclude that once democratization has taken place, it is much more likely to persist among higher-income countries, while the odds of a reversal to authoritarianism are much higher among poorer countries. Given these patterns, Geddes (1999) concludes that the linkage between economic development and democracy is "one of the few stylized facts" generated from analyses of regime transitions.

Consequences of Regime Type

It has been argued for some time from a variety of perspectives that poorer countries cannot experience growth and more competitive regimes simultaneously. Instead, some form of authoritarianism may be unavoidable, and by imposing a degree of order on otherwise fluid situations, authoritarian rule may provide the framework for effective solutions. Such arguments have been advanced about military intervention. Thus, according to Huntington,

> The coup is the extreme exercise of direct action against political authority, but it is also the means of ending other types of action against that authority and *potentially the means of reconstituting political authority*... The seizure of power, in this sense, represents the end of a political struggle and the recording of its results, just as takes place on election day in a democratic country (Huntington 1968, 219, my emphasis).

This argument implies that economic growth should follow from the reconstitution of political authority, typically in the form of a military junta. More generally, it was argued that democratic regimes in developing countries would face irresistible electoral pressures favoring short-term consumption at the expense of investment, pressures that would thereby inhibit growth (e.g., de Schweinitz 1959, Huntington and Dominguez 1975, 60). For these reasons, Huntington and Nelson concluded that "political participation needs to be held down, at least temporarily, in order to promote economic development" (1976, 23). Coming from a different tack, Evans (1979) used the Brazilian case to suggest that "dependent development" was possible with a combination of a potentially large economy and an authoritarian regime. More recently, we have seen the familiar claim that authoritarian regimes contributed to the East Asian economic miracle from the 1960s through the middle 1990s (e.g., Haggard 1990, Wade 1990).

The quantitative evidence, however, paints a quite different picture. First, there is no evidence that military juntas generate different outcomes than other kinds of regimes (see, e.g., Jackman 1976, Zuk and Thompson 1982). Second, Huntington and others to the contrary, coups are never medicinal in any sense. Instead, military intervention simply increases the odds of subsequent coups, and the attendant instability serves to depress economic growth (Londregan and Poole 1990). Third, as Geddes (1991) has shown, most studies arguing that authoritarian regimes more generally foster growth suffer from severe selection effects.

Indeed, on the basis of more recent studies we can go further. The best available evidence indicates that democratic regimes actively foster economic growth, both directly and indirectly (see, e.g., Helliwell 1994, Feng 1997, Przeworski et al. 2000)[7]. The evidence is sufficiently strong on this point that we can consider it another stylized fact[8]. This is a far cry from earlier arguments like Huntington's.

Electoral Laws

Finally, consider questions of electoral laws, a topic of great current interest given recent efforts at democratization. These laws, of course, govern the ways in which individual preferences are aggregated by the electoral system into grass-roots support for political parties. There is a vast literature on the topic, and the most notable contributions include Rae (1971), Taagepera and Shugart (1989), Lijphart (1994), Cox (1997). Further, key political players often attempt to manipulate electoral procedures in order to advance their own goals. As Sartori noted over three decades ago, the electoral system is "the most specific manipulative instrument of politics" (1968, 273).

7 While Przeworski and his colleagues report no direct effect of regime type on economic growth, they do find a pronounced regime-type effect on population growth, on the basis of which they seem to imply an indirect effect of regimes on economic growth.

8 One might object that we cannot simultaneously entertain the Lipset argument about economic development and democracy and this relationship between democracy and growth without introducing potential endogeneity problems. However, economic development (as indexed by national income levels) is not the same as growth rates, and there is no simple bivariate correlation between the two. Indeed, more completely specified models of growth routinely control for initial per capita GDP, consistent with the conditional convergence approach to growth which implies that GDP has a negative net effect on subsequent growth. For discussion of and evidence on these issues, see Barro (1997, 8-18).

Perhaps the best known stylized fact here is associated with Duverger's (1954) proposition that simple plurality rule favors the two party system. Likewise, simple plurality rule typically generates a legislative seat share for the winning party that exceeds its vote share. Of course, these patterns have long been recognized by political actors[9].

THE EVIDENTIARY BASIS FOR THESE STYLIZED FACTS

The above constitutes only a sampling of the stylized facts generated in recent years by quantitative studies of political development. Even so, they involve empirical regularities that center on important political issues. While none of them constitutes a complete theoretical statement, many of them falsify major claims that had previously enjoyed considerable acceptance. Beyond this, they point to new directions for theoretical and empirical work that have radically different implications. For example, in the 1960s one might have rejected claims made on behalf of authoritarian or corrupt regimes on ethical grounds involving a general sense of distaste. Now, we have a firmer basis for rejecting such claims on empirical grounds as well.

In light of the discussion to this point, it should be clear that I regard theory construction and evaluation as the principal task in comparative politics. Since I am focusing on the role of evidence, I am primarily concerned with the evaluation component of this, blithely leaving to one side the interesting, important, and difficult question of where good theories come from. My focus on evaluation directs our attention to the role of falsification. For those who find this label too narrow, another way of thinking about the role of evidence is to say that we are interested in theoretical statements that are consistent with the available evidence, and that we also use that evidence to help rule out plausible rival arguments. Thus, we are concerned with evaluating the preponderance of evidence.

As King, Keohane, and Verba (1994) emphasize, statistical texts seldom discuss such issues. Rather, they proceed on the assumption that an interesting idea is to be evaluated, that the investigator has identified an appropriate empirical domain within which to undertake the analysis, that the study rests on an appropriate unit of analysis, and that the key variables (including controls) to be included in the analysis have been clearly defined, justified, and measured. In other words, they assume a good research design, and proceed from there. There is nothing intrinsically wrong with this, of course, because the texts presume that the prior questions of research design have already been addressed. At the same time, it is critical to understand that the prior questions involve key substantive issues involved in effective research design. Further, at all key phases, resolving these issues typically entails drawing on qualitative information.

Of course, there remain substantial differences between quantitative and qualitative empirical research. *The key differences, however, center on the execution of the research*

9 For example, it was on the basis of these principles that Ramsay MacDonald convinced the fledgling Labour Party conference in 1914 (prior to the introduction of adult suffrage) to abandon its support for proportional representation on the presumption that adult suffrage would enfranchise a "natural" Labour majority in the United Kingdom so that plurality voting would enable the party to squeeze out the Liberals. Similarly, in face of opposition from both the British government and the Muslim League, the leaders of the Indian National Congress insisted on the implementation of plurality voting prior to independence in 1947. Congress became the dominant political party for several years thereafter.

design, not on the criteria employed in the creation and justification of the design itself. This is why statistics texts assume that a good research design has been developed, and proceed from that point. It is fundamentally in the execution of research where quantitative statistical analyses can make a contribution that can be distinguished from a qualitative contribution.

If this is the case, then it follows that analyses *employing a quantitative design necessarily involve substantial qualitative reasoning.* By the same token, it is difficult to see how these issues can be avoided in any analyses employing a qualitative design that seek to advance empirical claims. Phrased differently, all observational analyses, qualitative or quantitative, share a common logic and therefore confront the same issues of design (King, Keohane, and Verba 1994, 1995). There is a rich literature on research design in observational settings, including Campbell (1957), Eckstein (1975), Kish (1959), Lieberson (1991, 1994), Lijphart (1971), Smelser (1976), Stouffer (1950)[10]. Key design issues we all confront center on maximizing internal and external validity and maximizing the quality of measurement. My emphasis on maximizing here reflects the fact that tradeoffs are involved.

Consider some of the ways in which quantitative analyses employ substantial qualitative reasoning at various phases. One key issue in the design of empirical research involves the selection of the appropriate domains (units of analysis) within which the analyses are to be undertaken. Most of the studies of stylized facts cited above take the nation state as the basic unit of analysis. Clearly, such highly aggregated cross-country analyses derive their meaning in part from the fact of state sovereignty, on the basis of which we argue that they are meaningful political units[11]. But the choice of the appropriate unit of analysis is a substantive issue, and the nation state is not always the most effective unit. Cox (1997), for example, shows the payoffs for our understanding of electoral systems generated by blending analyses conduced at the district level with those conducted at the national level.

Similarly, qualitative reasoning underlies decisions about which set of countries to include in a particular analysis. Often, a "most similar systems" design is developed, in which attention is restricted to a subset of countries in order to gain at least a qualitative degree of control. For example, scholars have devoted considerable attention to the politics of electoral laws in the established democracies, a restriction that focuses attention on patterns in institutionalized settings. Others have examined the same laws in the newly democratizing states of eastern Europe, thereby concentrating on patterns in fluid settings. On occasion, attention is restricted to a particular region. Thus, Collier (1982) examined regime formation in sub-Saharan Africa, thereby introducing qualitative controls for wealth, colonial past, and the like[12]. In all such studies, qualitative criteria inform the research design by justifying the domain.

10 Judging by patterns of citation, several of these [Campbell (1957), Kish (1959), Lieberson (1991), Smelser (1976), Stouffer (1950)] seem to have fallen off the radar. They are well worth revisiting.

11 The best discussion of this point remains Kuznets (1951), another paper that bears revisiting.

12 Note that such an approach is distinct from one in which cases, perhaps in a given region, are selected for inclusion because of their distinctive performance on the outcome of interest (for example, an analysis of the determinants of economic growth in which attention is restricted to the high-performing cases of east Asia). Such efforts involve unacceptable selection effects (see, e.g., Geddes 1991, King, Keohane, and Verba 1994).

Qualitative reasoning is also typically involved in questions of measurement. Most social science measures are, at best, indirect indicators of concepts that can often be highly charged or whose meaning has evolved over time. The concept of democracy is a good case in point, since it is one that entails normative goals that almost everyone claims to endorse and since it means something different now than it did, say, two centuries ago[13]. Because the indicators we employ are indirect measures of the underlying concept, questions of measurement validity are of particular concern (this is true for both quantitative and qualitative work). Measurement validity, of course, centers on evaluating the extent to which indicators of a particular concept are biased, that is, subject to nonrandom error. While some quantitative tools are available for evaluating the presence of nonrandom error (see, e.g., Bollen 1993), most evaluations of measurement validity rely crucially on qualitative reasoning, in which we attempt to justify measures in terms of face validity and other criteria (Jackman 1985).

Consider Lipset's argument about economic development and democracy. The claim links two concepts, but the empirical analysis necessarily links two *measures* of each. Lipset's discussion of the criteria for democracy is couched in terms of the competitiveness of regimes, and centers on the presence of a governing party, an opposition party, and an electoral formula by which the opposition party can assume the government, and vice versa. However, his measure is much more problematic, resting as it does on a four-fold classification that first distinguishes European from Latin American regimes (where the latter are counted as less democratic), and then *within* these categories classifies them according to whether or not they are "stable democracies". Note the way in which stability is introduced into the lexicon, even though stability appeared nowhere in the conceptual definition of democracy[14]. Note further the way that Portugal or Spain in the late 1950s are classified as more democratic than Costa Rica or Chile in the same period solely because of their geographical location[15].

Lipset's analysis was originally reported over 40 years ago, but similar issues arise in more current scholarship. For example, Przeworski and his colleagues (2000, chapter 1) employ the following four rules to classify democracies: There must be an elected chief executive; there must be an elected legislature; there must be more than one political party; and (if the first three rules are met) there must be alternation in office between the parties. These are reasonable principles that are consistent with earlier approaches like Lipset's and Dahl's. But consider the following. The period analyzed by Przeworski and his colleagues includes the 40 years ending in 1990, and during this period, Japan (to take the most prominent case) is classified as democratic even though there was no alternation until 1993. This means that given the information available during the period studied,

13 In particular, as Dahl (1971) has pointed out, the emphasis on inclusiveness is a modern one that won widespread acceptance only in the twentieth century.

14 A number of analysts since Lipset have also inadvertently conflated stability with democracy in their measures. The problem then is that, when such measures are related to some other variable, we have no basis for distinguishing the effects of democracy from those of stability on the outcome of interest (for further discussion, see Bollen and Jackman 1989).

15 Note further that the original geographical basis to the classification is also a rough measure of wealth, so Lipset's subsequent claim linking national wealth to democratic performance becomes tautologically true. My treatment of his hypothesis as (by now) a stylized fact reflects evaluations of the claim that have appeared since, including those cited earlier.

Japan would not have been classified as democratic[16]. In other words, identifying whether there has been effective alternation hinges on *ex post* reasoning.

My brief discussion of these two measures of democracy has two immediate implications for present purposes. First, evaluating the validity of measures like Lipset's or Przeworski's and his colleagues along these lines necessarily involves qualitative reasoning. Second, the validity of their substantive inferences depends entirely on the outcome of this evaluation.

Consider now in somewhat greater detail Mauro's (1995) claim that corruption inhibits economic growth. Again, acceptance of this proposition requires first an evaluation of the validity of his measure of corruption (clearly, there is a high degree of consensus about the measurement of growth). Mauro draws on material collected by Business International (BI), a private company (now part of *The Economist Intelligence Unit*) that sells its information to banks, international investors, and other commercial groups. BI gathered data on some 56 "country risk" factors, by surveying its network of analysts in the countries concerned. These respondents score the country to which they have been assigned on a scale from 1 (most corrupt) to 10 (least corrupt).

Of the many features addressed in BI surveys, Mauro focuses on the following three:

1. *Legal system and judiciary.* "Efficiency and integrity of the legal environment as it affects business, particularly foreign firms."
2. *Bureaucracy and red tape.* "The regulatory environment foreign firms must face when seeking approvals and permits. The degree to which it represents an obstacle to business."
3. *Corruption.* "The degree to which business transactions involve corruption or questionable payments."

Mauro averages these three indices to form his measure of corruption.

Because they are generated from surveys of knowledgeable informants, the BI data reflect perceptions of corruption, not a count of its incidence. Thus, the data cannot be used to estimate such quantities as the monetary costs of corruption in a given setting. They are instead best taken to gauge variations across countries in the overall *climate* of corruption. While this climate may be defined by (informed) perceptions, Mauro shows that it has direct and crucial implications for quite concrete decisions, including those about the location and timing of investments by both domestic and foreign commercial interests.

One can, of course, raise objections to the BI measure. For example, it could be argued that when access to information is restricted, respondents may tend to underestimate the incidence of corruption. However, the reported high levels of corruption in such information-poor environments as Indonesia and Zaire imply that this is not a systematic problem.

16 Indeed, the classification of Japan as democratic would not have been possible had the Liberal Democratic Party lost in 2000 rather than in 1993, or had Przeworski and his colleagues published their study in 1993 rather than in 2000. Given the outcome of the federal election of 2000, one wonders whether they would reclassify Mexico as democratic from 1950-90. While a full discussion would take us far afield, in my own view, problems like these and the conundrums they generate are best avoided by treating democracy as a matter of degree rather than as a binary attribute (see Bollen and Jackman 1989. For an opposing view, see Collier and Adcock 1999).

Similarly, it might be suggested that when a major scandal surfaces shortly before the time of the survey, respondents may overestimate corruption. This potential problem is minimized in that the data refer to a four-year period, as opposed to a briefer interval of one year or less. Finally, the quality of the data hinges critically on the clarity of the guidelines for evaluating corruption given to respondents, since respondents may have different conceptions of what constitutes corruption. Here, BI goes to great lengths to ensure that the expert raters have clear guidelines on the definition, forms, and loci of corruption under consideration, and specific instructions on the meaning of each value on the scale from 0-10. Indeed, as Mauro puts it, "evidence for the accuracy and relevance of the indices is provided by the considerable price that BI's clients are willing to pay in order to obtain them" (1995, 684)[17].

In reviewing this discussion of the validity of Mauro's measure of corruption, the most striking feature is its reliance on ancillary information, much of it based on qualitative material. Indeed, *at no point above does the case made for the validity of this measure assume a quantitative form.* At the same time, Mauro's broad claim about the effect of corruption on growth hinges critically on the validity of the measure. This simply underscores the centrality of qualitative reasoning to quantitative research.

Finally, qualitative information plays a central role in the *interpretation* of observed regularities generated by quantitative analyses. Typically, we are interested in making causal *inferences.* The fact that we regard them as inferences reflects the proposition that causation can never be empirically demonstrated. We observe regularities, and on the basis of ancillary information embodied in prior theory and research, we seek to place causal interpretations on those regularities. These interpretations can often be strengthened empirically by ruling out plausible rivals so that we have more confidence in them. Nonetheless, such procedures do not constitute some form of positive proof of the inference[18].

There are many reasons for this, of course. For one thing, we can never be sure that we have fully addressed questions of endogeneity or that we have incorporated a full set of controls. Further, empirical tests typically center on the implications of theories rather than on the full theories themselves. But, perhaps most importantly, the language of theory is not the same as the language of research. If a research design is well-formulated, the two may closely parallel each other, with the latter being derived from the former. However, the language of theory is typically much richer and includes ancillary information and assumptions that never appear in research designs. And it is to this ancillary material that we typically turn when attempting to justify causal inferences (see, e.g., Smelser 1976, 229-33).

Let us return to the linkage between economic development and democracy. Lipset did not simply declare the linkage to be a causal one on the basis of the observed statistical

17 Similar issues can, of course, arise with alternative methods of collecting information about corruption. Consider the use of press reports, judicial records, and records from anti-corruption agencies to gauge the incidence of corruption. Such documentary evidence forms an imperfect measure of the actual incidence of corruption, since many incidents are never discovered or prosecuted, especially in corrupt environments. Further, press and government agencies in different countries are more likely to have varied conceptions of corruption, and varying styles of data collection across countries are likely to result in more coverage of particular types of corruption in some countries than in others. See Lancaster and Montinola (1997) for a more detailed discussion and evaluation of corruption measures.

18 For an amusing brief discussion on this point, see Thurman and Fisher (1988) who introduce time-series evidence collected by the United States Department of Agriculture to reveal that eggs cause chickens, not the other way around, at least in the United States.

association. He instead interpreted the association by invoking ancillary factors such as increased wages, greater economic security for wage earners, the emergence of middle-class values, and a muted class struggle, all of which he claims arise from economic development, and which in turn provide part of the basis for democratic institutions (Smelser 1976, 231-2). Alternatively, one might argue (following Deutsch 1961) that economic development is typically associated with social mobilization, which among other things brings improved literacy rates and patterns of communication. Insofar as political democracy includes a meaningful right to participate by the entire adult population (Dahl 1971), it is difficult to conceive of effective democratic institutions absent such mobilization. Hence the inference that economic development is an important prerequisite of democracy.

If the information we have employed is valid, and if we have not neglected other ancillary information that might undermine the case, then we have more confidence in the claim that economic development enhances democracy. Observe, however, that the original observed association does not in itself justify the inference. Observe further that qualitative reasoning plays a large role in the use of ancillary information.

In other settings, of course, the ancillary information may involve more of a blend of quantitative and quantitative material. Consider the stylized fact from Mauro (1995) that corruption inhibits growth. To account for this pattern, Mauro evaluates the effects of investment, noting that corruption could lower the growth rate in at least two different ways. First, the effect could be indirect, so that corruption lowers the investment rate which then lowers growth, and second, the effect could be direct in that corruption inhibits growth by generating inefficient investment choices. While he is able to employ quantitative data to address the first possibility, his claims concerning the second alternative are based entirely on qualitative ancillary considerations[19]. Again, additional information is used to explain the stylized fact, and much of the argument hinges on qualitative criteria.

CONCLUSIONS

This paper has examined cross-country quantitative studies of political development, paying particular attention to the use of evidence. I suggested in the first section that, too often, fads and fashions seem to have come and gone in the analysis of political development, so that one might reasonably conclude that the field as a whole has not taken evidence seriously. While newer perspectives (modernization, dependency, and statism) have been heralded as paradigm shifts, their acceptance has waxed and waned largely independently of any evidence. This is a misuse of the language of paradigms, at least as employed by Kuhn, and it has not advanced our substantive understanding of the issues at hand. Further, the perspectives identified are themselves too heterogeneous to be useful. I therefore propose that we abandon the language of paradigms, along with the perspectives I have discussed.

19 In another phase of his analysis, Mauro also employs qualitative reasoning to justify the use of a measure of ethnic fractionalization as an instrumental variable to substitute for corruption. Here his argument is less compelling, even though it probably satisfies what we might label the "bacon" condition: "A test of the predeterminedness of eggs [with respect to chickens]... would require a valid instrumental variable (correlated with eggs and uncorrelated with the chicken forecast error), perhaps bacon" (Thurman and Fisher 1988, 238).

Accordingly, in the main part of the paper I have considered what we may have learned in the form of stylized facts from those analyses adopting a more quantitative approach to political development. Throughout, I have paid special attention to the role of evidence in the evaluation of arguments about political development. In this connection, I argued that the distinction between quantitative and qualitative forms of reasoning is more blurred than many would have us believe. Specifically, qualitative reasoning plays a central role in questions of research design and measurement, even in apparently otherwise quantitative studies. It also plays a central role in the interpretation of observed regularities generated by quantitative analyses.

Of course, the quantitative and qualitative empirical research traditions are distinct. The key differences, however, center on the execution of the research design, not on the criteria employed in the creation and justification of the design itself. If it is fundamentally in the implementation of the research design that the distinction between the two traditions is most pronounced, then it follows that both traditions share a common logic of inquiry.

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