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What's Inside This Issue

FEATURE *Essay*

From settler mortality to patrimonialism: weaving the dynamics of political competition into the political economy of development

Philip Keefer, Development Research Group, The World Bank

The evidence is overwhelming that the decisions of governments in poor countries are significantly different than in developed countries. On average, developing countries are more tolerant of corruption, more apt to expand government employment opportunities than to offer high quality education, and less likely to enforce contracts and property rights. It is no surprise, then, that in striving to understand economic development scholars have come to focus closely on the role of domestic politics and political institutions.

The political explanations of persistent economic underdevelopment that have attracted greatest attention seem to boil down to three (broad) hypotheses: countries that lack political checks and balances and competitive elections cannot credibly guarantee secure property and contract rights and therefore grow slowly; interest group influence leads governments to resist development-promoting policies; and social and cultural factors, such as caste and clientelism, cripple government decision making in poor countries. This note suggests that more attention in the future should turn to a fourth factor, the dynamics of political competition and systematic differences across countries in how politicians gather voter support.

One important hypothesis emerging from this fourth line of inquiry is that the extent of voter information and the credibility of pre-electoral promises of competing politicians explain dramatic policy differences between countries. Like the first hypothesis, this line of argument is explicitly comparative. It also recognizes that political credibility and voter information are likely to have historical roots. However, it goes beyond formal institutions in framing the political economy of development. Like the second and third hypotheses, this approach is concerned with the political incentives underlying specific government decisions. However, it it is more comparative in its approach and, substantively, focuses more specifically on political strategies for obtaining citizen support.

This fourth approach can therefore explain phenomena that are taken as "exogenous" in much of the country-specific literature. For example, much of the literature evokes clientelism as a fundamental feature of countries – as a basic unit of analysis. This fourth argument suggests that clientelism is a product of deeper phenomena, and it is these phenomena that explain cross-country differences in performance.

Formal institutions and development

Formal institutions – the formal rules governing elections and the process of legislation – are an important determinant of political incentives and are a logical choice in attempting to explain why some governments act more in the public interest than others. The empirical rationale for doing so is substantial. It is surely more than a coincidence or a relic of reverse causality that all rich countries are democracies, for example. In specific areas of policy we also observe significant differences between democracies and non-democracies.

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A LETTER FROM THE *Editors*

Dear Readers:

As the new editors of *The Political Economist*, we begin by thanking Mike Hiscox and Brian Burgoon for leaving the newsletter in such fine shape. During their tenure as editors, Mike and Brian developed the newsletter into a forum for debate and discussion on the nature of political economy, covering appropriate approaches, methods, and substantive focus. As a result of their efforts, The Political Economist serves as an important resource for information on developments and controversies in the field.

We will continue to edit the newsletter with the aim of engaging salient research issues in political economy. Our basic editorial strategy will be to devote one or more issues to the accumulation of knowledge in specific areas of political economy: What do we know now in that particular area that we did not know 10 years ago? How did we learn it? How best can we move forward?

We inaugurated this approach in the Fall 2004 newsletter with an excellent critical essay by Adam Przeworski on the relationship between economic history and political science in light of recent work by authors like Engermann and Sokoloff (2001) and Acemoglu, Johnson, and Robinson (2001). Phillip Keefer continues in this vein in the Winter 2005 issue with a paper on the historical origins of well-functioning political institutions. Additionally in this issue, Daron Acemoglu, Simon Johnson, and Jim Robinson provide a response to Przeworski's provocative essay.

In future issues, we hope to attract essays that address the accumulation of knowledge in other areas of political economy, including (but certainly not limited to) social choice, behavioral and experimental methods, institutional analysis, macro-political economy and international political economy. We encourage anyone with an interest in making a contribution to contact us. As your agents, we also welcome feedback and suggestions on any aspect of the newsletter.

Sincerely,
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A LETTER FROM THE *Chair*

Dear Readers:

In the fall 2003 issue of *The Political Economist,* Elizabeth R. Gerber initiated a sequence of important reflections on the question of "What is Political Economy?" Not only did Liz do an excellent job as chair of our section, but she provided a good overview of her "big tent" approach to the definition of our subfield. She also encouraged as broad an approach as possible so as to encourage the participation of many scholars who, although they focus on separate questions, use a roughly similar scholarly approach. Genny Haufler made a thoughtful response. Charles Shipan reflected on this invigorating discussion about the nature of political economy in his outgoing message in the fall of 2004. Also in the fall of 2004, Adam Przeworski provided his distinct view of "Economic History and Political Science" and challenged the recent focus of political economists as a major cause of differential development. In the current issue, Daron Acemoglu, Simon Johnson, and James Robinson clarify their own views as distinct from those of Przeworski. Further, Philip Keefer digs into the dynamics of the political economy of development.

I deeply appreciate being chosen as your new chair at a time when these important issues are being discussed so vigorously. Several colleagues have asked me to present my own view of what is distinctive about the political economy approach. So, here goes another effort (for an earlier attempt to develop a behavioral approach to rational choice

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theory, see my APSA Presidential Address, Ostrom, 1998).

In my view, political economists are political scientists and economists, as well as a growing number of scholars from other disciplines, who use a family of related theoretical tools to analyze the *incentives* faced by decision makers in public and private spheres whose actions we want to understand and explain. We expect that incentives will be generated by the institutions within which individuals interact, but that these incentives will be further affected by both the biophysical world (nature of the goods) and the attributes of the communities (history, level of trust, size, etc.) within which individuals operate and function (Ostrom, 2005).

The theoretical tools we use vary from rigorous deductive reasoning and game theory, to broader analytical frameworks, to our more recent attention to evolutionary theory (Alford and Hibbing, 2004; Orbell et al., 2004) and agent-based modeling (Janssen, 2002). Political economists use a wide diversity of empirical methods ranging from analytical narratives, to comparative case studies, large N statistical analyses, and experimental research.

We do not focus on the actors in just one country or just one type of institutional arrangement. Members of our section have focused on Africa, Asia, Latin America, as well as the United States and Europe. Nor do we concentrate on only executives, legislatures, or national courts even though all of these are important institutional arenas of interest to many members of our subfield. We also examine the behavior of merchants, lawyers, and local and state government officials. Many of our colleagues analyze the politics of economies and the economics of polities. Thus, it is not the region, sector, or level of government that unifies us. Our driving interest is the effort to dig into the underlying institutional factors that lead to incentives, and eventually to behavior and outcomes.

We try to understand the strategies that individuals adopt given the objective payoffs they face as well as the norms they share with others and the enforced rule system within which they are interacting. The examination of norms is a more recent development. Some work of political economists at an earlier juncture focused exclusively on material incentives within externally enforced rule systems. Many of us have learned from extensive experimental and field research that it is not possible to explain behavior of actors in most nonmarket settings without paying attention to the norms or intrinsic preferences of actors as well as to their extrinsic or material payoffs. Consequently, we have found it necessary to modify the utility function overtly to include norms, when we think they are important in explaining behavior (Crawford and Ostrom, 1995; Frohlich and Oppenheimer, 1996). Otherwise, we can only wave our hands after the fact and argue that the actors must somehow have other preferences besides those in our theory. Further, we rarely find actors who have complete information about the situation in which they find themselves.

A positive development, in my opinion, is that more of our work could be characterized nowadays as "behavioral political economy." As a consequence, we now ask ourselves important questions like: How do we understand why diverse actors behave the way they do in specific kinds of situations as defined by the institutions, number and type of participants recruited and retained, the level of information generated, and extrinsic as well as intrinsic payoffs? And, in light of evidence from empirical research, we are making serious efforts to revise our theoretical foundations to improve our explanations.

This leads me to discuss some of the methods that we use. We owe a great deal to the development in formal theory that many of our colleagues have made. They have given us propositions that we can test and examine under a variety of field and experimental conditions. For many members of our section, formal models have been their primary mode of undertaking research. Some of us are active field researchers and have spent a lot of time trying to understand why patterns of behavior occur and reoccur in particular field settings. Since the 1980s, more and more political economists have responded to and participated in the vast body of experimental research that has blossomed at the borderline between economics and political science (Camerer, 2003). Once one has a strong theoretical foundation, one can then design tight experiments to examine that theory.

We have learned from experiments that a phrase frequently used by sociologists—context matters—is really true. What we as political economists must do is to identify theoretically and empirically which aspects of context matter and why. Unfortunately, this makes our theories more complex and our empirical research more challenging. We are, however, making progress in sharpening and stretching our empirical tools to treat more factors simultaneously by using both more sophisticated data collection techniques and advanced empirical modes of analysis (see, for example, Eckel, Johnson, and Wilson, 2002; Orbell et al., 2004; Riolo, Cohen, and Axelrod, 2001).

The presence or absence of variables such as: (1) how funds become available, (2) who knows what a participant does, or (3) whether other subjects are real players or computerized players programmed to make decisions (and thus do not have norms or personal intentions), make a substantial difference in observed outcomes. Frohlich, Oppenheimer, and Kurki (2004) have, for example, shown that changes in the procedures for determining the amount of funds to be distributed in a dictator game strongly affect the distribution of outcomes. When the amount of funds to be allocated is determined by the prior work effort of participants, those in the position of dictator assign awards using a normative sense of "just desserts" rather

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Economic History and Political Science: Clarifying the Questions, Methods and Answers

Response to Adam Przeworski, "Economic History and Political Science." The Political Economist. Fall 2004

Daron Acemoglu, Department of Economics, MIT
Simon Johnson, Sloan School of Management, MIT and IMF
James A. Robinson, Department of Government, Harvard University

One of the oldest and more important questions in social science is why some countries are more prosperous than others. This question attracted the attention of philosophers of the classical world, and was central to the work of such key figures as Adam Smith and Max Weber. To an economist, a country will be prosperous if it has accumulated a lot of capital, human and physical and has good technology, yet, as Douglass North and Robert Thomas pointed out some 30 years ago (North and Thomas, 1973), this explanation is only a *proximate* one. More *fundamentally*, we would like to know why some countries save and invest more and develop or adopt better technology. Once technology is endogenous, the only things left in traditional economic analysis are preferences and factor endowments. For instance, countries where people were more patient would have more saving and investment and ultimately become richer. The answer North and Thomas proposed, building on the work of Smith and many others, is that societies which have economic and political institutions which create incentives to save and invest will prosper, while those with different institutions will not. Traditional economics does not ignore institutions, there are markets for exchange and property rights, but variations in these are not explained and even not considered to be the key sources of variation in outcomes. North and Thomas emphasized that the key economic institution is secure property rights, but property rights can only be secure if political institutions limit state predation (see also the seminal paper by North and Weingast, 1989).

While there is a huge consensus that this is the right answer, many issues remain with the agenda laid out by North and Thomas. There are two key and related ones. First, how do we really know that it is institutions that cause economic development? Second, why do different countries have different institutions? The first question is an issue of statistical inference. Institutions are the humanly devised rules that organize social, economic and political behavior. Thus while the incentives created by institutions may determine whether a society becomes prosperous or not, one cannot conduct empirical work by assuming that institutions are exogenous. For instance, since prosperity and institutions are both jointly determined (or endogenous), a simple regression of income on some measure of the security of property rights will not estimate the causal effect of property rights on prosperity. Firstly, there may be reverse causation, possibly only prosperous countries can afford to have certain institutions. Second, and more important, something else, an omitted variable, may cause both institutions and prosperity and without properly controlling for such a variable we may spuriously estimate a causal effect of institutions on prosperity when there is none.

Let's illustrate these issues with a similar problem of central interest to political scientists, the claim that there is a causal effect of income on democracy. Empirical work from Lipset (1959) to Pzreworski et al. (2000) investigates the impact of income per-capita on democracy by estimating equations where some measure of democracy is the dependent variable and income per-capita one of the independent variables. Such regressions always find that higher income per-capita leads a country to be more democratic. Whether this effect works via transitions to or away from democracy is irrelevant for the basic methodological issue (see Acemoglu, Johnson, Robinson and Yared, 2004). Yet such a specification finds nothing more than what variables are partially correlated with other variables and does not rule out democracy causing prosperity. Moreover, both prosperity and democracy may be caused by something else. This is exactly what Weber had in mind when he noted (1930, p. 11)

"Montesquieu says (Esprit des Lois, Book XX, chap. 7) of the English that they "had progressed the farthest of all peoples of the world in three important things: in piety, in commerce, and in freedom". Is it not possible that their commercial superiority and their adaptation to free political institutions are connected in some way with that record of piety which Montesquieu ascribes to them?"

Hence Weber directly argued that an omitted variable, here religion, explained both democracy and capitalism in England.

To identify the causal effect of property rights on prosperity one needs a careful and convincing research design. More specifically, one needs to find an exogenous source of variation in institutions. By this we mean a variable which explains why institutions differ across countries, but is not itself a determinant of prosperity. Where could we find such a variable – what economists call an instrument? In Acemoglu, Johnson and Robinson (2001, 2002a) we proposed an approach to this which simultaneously addressed the second question, why do

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Human rights violations are lower under democracy (Mulligan, Gil and Sala-i-Martin 2004). The fiscal costs of banking crises (essentially, government bailouts of crony lenders and depositors) are all substantially lower in democracies (Keefer 2004a).

However, other large empirical puzzles are difficult to explain using formal institutions: substantial heterogeneity across democracies and worse performance among many democracies than among autocracies on important policy dimensions. For example, the median non-democracy (a country with no elections or less than competitive elections, as measured by variables in the Database of Political Institutions, Beck et al.) had the same score (four) on a six point rule of law index as the median democracy in 1997. Democratic heterogeneity is significant: the standard deviation of the rule of law score among democracies was 1.3, very high relative to the average. With respect to a similar indicator of corruption (on a 0-6 scale), the median democracy stood at 3.7 and the median non-democracy at 3 – but the standard deviation among democracies was almost twice as large as this difference, or 1.25.

Secondary school enrollment is a widely-used indicator of government willingness to provide services to the non-elite, and is a reasonable indicator as well of government inclinations to provide public goods generally to citizens. Secondary school enrollment is much greater in democracies (30 percentage points greater). However, school enrollment decisions by families are notoriously sensitive to income. Controlling for income per capita, secondary school enrollment in the median democracy is only 5 percentage points greater than in the median non-democracy. The standard deviation of enrollment in democracies, controlling for income, is 21 percentage points, however. These comparisons are the same if one compares countries according to whether they exhibit political checks and balances, using the checks variable from DPI.

From a development perspective, as important as the heterogeneity of democratic performance is the fact that poor democracies perform no better than non-democracies. If one compares the corruption, rule of law and secondary school enrollment indicators of the poorest half of countries with competitive elections in 1997 to all countries without competitive elections in 1997, the scores are nearly identical. The performance distance between rich and poor democracies is nearly the same as the distance between rich democracies and all non-democracies.

These results are at least puzzling, even troubling. Elections lower the costs to average citizens of expressing their disapproval of a regime and, one would think, should make politicians think twice before offering large payoffs to special interests. There is no particular reason to believe that this effect should collapse at lower income levels. At the same time, the theory is compelling that the formal institutions of democracy should do a better job of improving the security of property rights (not necessarily of lowering taxes imposed by the majority on the minority, but of preventing an opportunistic increase in taxation on those with the temerity to invest fixed assets in a country). Why, then, should the incentives of elected leaders vary so widely and often be less compatible with the interests of average citizens compared to unelected leaders?

One explanation is that our stylized notions of autocracy exaggerate the extent to which autocracies are unaccountable. Recent research tells us that autocracies are in fact often more accountable than their formal institutions would indicate. Work by Haber et al. (2003) on the Porfirio Diaz government (the Porfiriato) in Mexico, by Qian and Weingast (1997) on the operation of federalism in China, by Przeworski and Gandhi (2003) on the positive impact that even rubber stamp legislatures have on government performance, all tell us that autocracies can manufacture credible commitment. This credible commitment may come at a substantial cost in terms of long run growth and development, since it often involves granting inefficient privileges to the few (e.g., the monopolization of banking in Mexico). But it is sufficient to spur growth at least over a generation. Nevertheless, the ability to make credible commitments to key investors sufficient to trigger growth does not explain why many autocracies perform better than many democracies in the provision of public goods (secondary school enrollment), or in the prevention of corruption.

Another explanation is that the formal institutions of democracy are themselves too heterogeneous to be reasonably grouped together. Persson and Tabellini (2000) argue that whether a country is presidential or parliamentary, whether it has large or small electoral district magnitudes, or whether it uses plurality or proportional electoral systems can have a significant impact on policy outcomes. There is some evidence for these hypotheses. However, the only political institution that varies systematically between poor and rich democracies is regime-type: poor democracies are much more likely to be presidential. Presidential democracies are expected to provide fewer public goods – for which there is some evidence – but they are also expected to be less corrupt – for which there is little evidence. Despite good reasons to believe that politicians will behave differently depending on the specific formal institutions of decision making in a democracy, these behavioral differences seem not explain the heterogeneity we observe among democracies.

If not (only) institutions, then what?

Variation among democracies on

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BEST PAPER AWARD ANNOUNCEMENT

The committee for the Best Paper Award (Torben Iversen, Harvard; Layna Mosley, UNC Chapel Hill; Frances Rosenbluth, Yale University) received seven nominations from panel chairs and discussants. The winner of the award for the 2004 APSA meeting is William Bernhard (University of Illinois) and David Leblang (University of Colorado), "When Markets Party: Stocks, Bonds and Cabinet Formations." The runner-up is Sarah Brooks (Ohio State University), "A Competing Risks Model of Structural Pension Reform: Adoption and Diffusion of Alternative Paradigms."

Bernhard and Leblang examine the impact of political uncertainty on financial markets. Specifically, they investigate the extent to which concerns about cabinet formation in parliamentary democracies generates differences in equity and government bond prices. The paper includes an innovating pairing of the Laver-Shepsle model of cabinet formation with asset pricing models, as well as a creative and well-executed empirical analysis. The empirical results, which include an analysis of all cabinet formations in ten parliamentary democracies during the 1970s, 1980s and 1990s, as well as an analysis of daily time series data (1980-2002) for fifteen developed democracies, provide some evidence that uncertainty regarding cabinet formations is associated with abnormal returns, especially in equity markets. Bernhard and Leblang's paper raises many interesting questions about the specific effects of political uncertainty on economic events, as well as the ways in which market actors react to the resolution – via eventual cabinet formation – of political uncertainty. We look forward to the authors' future work on this project, particularly the creation of a broader theoretical conception of political uncertainty.

Brooks investigates the economic and political logic behind social security reform in various regions of the world. Her analysis is premised on the (understudied) fact that there are two models of pension reform — funded defined contribution schemes, also known as "privatization," and notional defined contribution systems, which are a public-private hybrid. Brooks argues that the factors that lead governments to adopt each of these reforms differ. For instance, the effect of "peer dynamics" (what other countries do) varies across geographic regions for FDC reforms, but not for NDC reforms. Perhaps more importantly, the empirical results, based on reforms over the last two decades, suggest that NDC reforms are more likely to be adopted where political power is more broadly shared among legislative parties. This paper has the potential to make important contributions to our theoretical understandings of economic reform, as well as to the empirical analyses of social protection policies around the world. Again, we hope to see future work on this topic.

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UPCOMING AWARDS

The **William Riker Best Book Award** is for the best book in political economy published during the past year. The Riker Award Committee will be chaired by Stephen Haggard (shaggard@ucsd.edu). The other members of the committee include Sarah Brooks (brooks@polisci.sbs.ohio-state.edu) and John Huber (jdh39@columbia.edu).

The **Mancur Olson Award** is for the best dissertation in political economy published during the past year. The Olson Award Committee will be chaired by Joe Oppenheimer (joppenheimer@gvpt.umd.edu). The other member of the committee is Scott Gehlbach (gehlbach@polisci.wisc.edu).

The **Best Paper Award** is for the best paper in political economy given at the 2004 APSA meetings. The Committee will be chaired by Layna Mosley (lmosley@email.unc.edu). The other members of the committee are Frances Rosenbluth (frances.rosenbluth@yale.edu) and Torben Iverson (iversen@fas.harvard.edu).

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institutions differ. To find an instrument one needs a theory of why institutions differ. The theory can act as a guide to find an instrument. Our theory built on a large literature by historians and social scientists and was founded on the idea that the European colonization of the non-European world was a significant determinant of the institutions of former colonies today. Europeans created different institutions in different places in a way which depended on the initial conditions they found. In places which were unhealthy for Europeans to live, which were relatively prosperous and where there were large population densities of indigenous peoples (for example Peru, Bolivia, much of sub-Saharan Africa), Europeans created an interlocking set of economic and political institutions designed to extract rents from the colonies and transfer them to the colonial power. In such societies, there was no incentive to introduce institutions guarding the property rights of the vast majority of citizens or giving them access to land or the rule of law because society was designed to exploit such peoples. In consequence, though the institutions of such societies greatly enriched an elite, they did little to create the incentives needed to induce general prosperity. In countries where initial conditions were radically different, however, such as in North America, such an 'extractive' set of institutions was infeasible and very different societies emerged. These societies had much more secure property rights and political institutions which supported them. The early struggle over institutions in the US has been well documented (Morgan, 1975, Galenson, 1994). Representative of the emphasis of scholars is Galenson's (1996, p. 143) analysis of the early development of Maryland. In design, Maryland was to be a feudal society, yet

"The extreme labor shortage ... allowed many early settlers to gain their economic independence from the manorial lords, and establish separate farms ... Thus just as in Virginia, in Maryland the colonial labor problem undermined the initial plans for a rigid social hierarchy, as Lord Baltimore's blueprints for a manorial society were largely swept away and early Maryland became an open and fluid society, which offered considerable economic and social opportunity."

The relatively benign mortality environment of North America was also attractive for Europeans. Crosby (1986, pp. 143-144) notes how the Pilgrim Fathers originally intended to colonize Guyana but switched to the US because of the absence of tropical diseases.

This theory suggests an instrument, the mortality rate faced by Europeans in different parts of the colonial world. Europeans died of tropical diseases such as malaria and yellow fever which they did not understand how to prevent or cure. The incidence of such diseases clearly influenced the types of colonial societies that developed, but the historical rate at which Europeans died of such diseases is not a determinant of the current prosperity of, say, Bolivia. We emphasize that for European mortality to be an instrument it only has to be *a* source or variation not *the*, or even *the most* important source or variation. Using data on European mortality in the colonies primarily collected by the historian Philip Curtin, our 2001 paper estimated that the causal effect of institutions accounted for fully ¾ of the income differences across countries today. Our 2002 paper also showed that initial population density, as suggested by our theory, had large impacts on institutional development and that key historical patterns of development also suggest a theory of comparative prosperity based on institutions as the fundamental causal factor, rather than say geography or culture, is the correct one. The chief such pattern we termed the "Reversal of Fortune" (Acemoglu, Johnson and Robinson, 2002a). This pattern is that countries which tended to be systematically more prosperous when the Europeans colonized them now tend to be systematically poorer. This reversal is inconsistent with simple geographical explanations for relative prosperity, but it is consistent with the hypothesis that Europeans created institutions designed to extract rents in places where this was relatively attractive and these were the places which were initially more prosperous, such as much of Latin America.

In the Fall 2004 edition of *The Political Economist* Adam Pzreworksi challenges our approach to comparative development. He does so on two grounds, first that we got the institutions wrong, and second that we got the econometrics wrong. With respect to the first point he initially claims that it is not the security of property rights that is important, but rather "institutions … that coordinate investment" (p. 3). Unfortunately, Pzreworksi does not explain what he means by this nor does he propose any evidence that this is true. His discussion suggests that it is financial institutions that are the relevant ones, yet the models of Murphy, Shleifer and Vishny which he refers to as capturing the correct approach to development feature perfect capital markets. Hence in these models the depth of financial institutions cannot possibly explain why some countries can coordinate and others not. He then argues that securing property rights is a "second-order feature" of an institutional framework. This is in fact the point that North and Thomas emphasize and is a central feature of our work. We argue that different sets of economic institutions are supported by different distributions of political power that are themselves intimately related to political institutions (see Acemoglu, Johnson and Robinson, 2004).

Pzreworski then moves to challenge our empirical approach. He first states "if institutions constitute a primary cause,

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non-institutional dimensions is therefore likely to be key to understanding the puzzle of heterogeneous, and often simply bad performance by democratic governments. Other prominent contributions to the political economy literature offer possible sources of such variation: interest groups, clientelism, caste, even leadership. However, although these are each important areas of investigation, they do not close the gap in understanding political performance in rich and poor countries.

For example, Bates (1981) traced diverse agricultural policy outcomes across African countries to differences in the organization of interest groups, and differences in organization, in turn, to the fundamental economic characteristics of countries. Frieden (1991) explains the response of countries to crisis similarly as an outcome of the organization and interests of economic sectors. This work is convincing that interest group influence, like formal institutions, plays a crucial role in policy formation. There is little evidence, however, that interest groups differ across countries in a way that would explain divergent performance among democracies.

One exception might be Acemoglu, Johnson and Robinson (2004). Although they argue for the primacy of institutions, at the heart of their arguments and similar arguments of Engerman and Sokoloff (2002) is historical evidence that the post-independence constellation of economic interests in countries (interest groups) critically influenced subsequent political and economic development. Where elites have no economic interest in opening the political system to participation by non-elites (as when there are substantial natural resource rents that elites do not wish to share), non-elites have no credible guarantees that their economic rights will be protected, refrain from investing, and growth slows, even as elites get rich. These arguments are persuasive, but explain only why democracies should perform better than non-democracies: democracies are countries where elites found it worthwhile to open up the political system to non-elites, giving non-elites credible guarantees of the security of their economic rights, and thereby spurring growth. This work was not intended to explain either significant performance heterogeneity among democracies or the frequent inferior policy performance of democracies, particularly poor democracies, relative to autocracies.

Variations in leadership quality across countries are also unlikely to provide a satisfying explanation of development. Olken and Jones (2004) show that the identity of leaders matters for growth in the short term; their rigorous examination of the question complements many investigations in the comparative politics literature of the role of leadership in the evolution of policy reform. However, the scant to non-existent evidence that short-run reform successes spur a cascade of long-run reforms is weak. It is unlikely, therefore, that leadership can explain vast performance differences among democracies.

Accountability and the search for votes

What, then, are we to make of performance heterogeneity among democracies? In his contribution to a past newsletter, Adam Przeworski argues that we should focus on "the institutions that matter for development are those that make rulers accountable, those that provide information about government's actions and permit citizens to sanction bad behavior by throwing governments out of office."(p. 3) The argument in this note is that the search for the sources accountability should concentrate especially on information and credibility failures.

Just as in private markets, political markets are bedeviled by imperfections. In particular, a wealth of research, theoretical and empirical, has shown that holding rulers to account depends on citizen information about ruler performance, about the costs to citizens of sanctioning rulers, and about the capacity of citizens to make informed choices between political competitors – and on whether those competitors can make credible promises to citizens. Absent any of these, accountability evaporates.

Uninformed citizens are entirely incapable of holding politicians accountable for performance. We would therefore expect – and we see both theoretically and empirically – that voter information plays a significant role in policy making. This is not surprising: in economic markets, lack of information about product quality also drives a wedge between buyers and sellers. Credibility has similar effects. When political competitors cannot make credible promises, citizens again are entirely incapable of holding politicians accountable unless they can coordinate on ex post performance thresholds, a la Ferejohn (1986), committting to expel incumbents who fail to meet the threshold.

The role of voter information in government policy making has been well documented. Besley and Burgess (200) have found that areas of Indian states with the highest newspaper circulation received significantly more food assistance than other areas. Strömberg (2004) uncovers similar effects on state assistance to individuals for radio ownership in the US during the Great Depression. Adserá et al., (2003) conclude that newspaper circulation significantly reduces corruption.

Keefer and Vlaicu (2004) model the behavior of non-credible politicians, allowing them to expend resources to build up their credibility, or to rely on credible intermediaries, patrons who can make credible promises to clients. Predicted policy outcomes are precisely those that one observes in democracies where the influence of patrons is thought to be significant: high corruption, low public good provision, and unyielding

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than allocating funds based on the extrinsic value of funds assigned to self (see also Frohlich and Oppenheimer's (1990) earlier work on choosing justice).

Since our initial theories of individual behavior did not posit that individuals took intrinsic valuation into account, many of our earlier studies did not overtly explore factors that would affect normative behavior. Thus, one of the threats to the internal validity that scholars have started to explore is simply how subjects are paid. If subjects (say, undergraduate students) fear that those who run the experiment (say, their faculty member) have full knowledge of all the actions they have made in an experiment, they may be trying to impress the experimenter with their behavior rather than solely responding to their internal norms and preferences. Hoffman, McCabe, and Smith (1996), as well as Cox and Deck (forthcoming), have run carefully designed experiments and found that differences exist in the behavioral patterns when the *same* experiment is run with single-blind or double-blind (where an individual's behavior is anonymous to the experimenter) payoff conditions. This enables the experimenter to examine whether a norm is fully internalized or is at least partly stimulated by external observation of their behavior. They find that double-blind behavior changed strategic behavior in predictable ways, largely by reducing "experimenter demand effect." Further, Blount (1995) and Cox (2004) have shown that subjects respond differently in experiments when the experimenter does or does not control for the belief or intentions of other players.

Digging deeper into our mental processes, Rilling and colleagues (2002) found that the brain activity of a subject connected to a magnetic resonance imaging (fMRI) scanner and playing a Prisoner's Dilemma game against either a pre-programmed computer or another real subject (not so connected to a fMRI) depended on the experimental condition. Pressing a button to indicate a choice of cooperation with a real partner who had reciprocated cooperation in the past generates the brightest reactions in the pleasure zones of the brain (the anterovential striatum and the orbitofrontal cortext). The high level of neural activation depended, however, on whether the scanned player thought she was playing a real human or a computer (see also McDermott, 2004).

Given the number of studies focusing on social dilemmas, we are learning a lot about the conditions under which individuals are more able to solve collective-action problems. We have, however, not yet developed a single theoretical explanation for the rich empirical evidence related to the level of cooperative behavior we have observed under different conditions. We have learned that the efforts so far to create one new, general theory of human behavior to replace our beloved *homo economicus* when modeling nonmarket settings have been shown so far to be inadequate to the task. One of the leading contenders for several years has been the theory by Fehr and Schmidt (1999) on inequality aversion. Studies have shown that it does not have as much bite as earlier claimed (Frohlich, Oppenheimer, and Kurki, 2004). For example, Cox and Sadiraj (2004) have shown that it is not an adequate explanation in dictator games in which both subjects in a pair are given the same endowment and the costs to the dictator of each monetary unit added to the other's payoff, costs the dictator less than the same monetary unit.

Thus, not only is this an exciting time for our section because political economists are successfully exploring a wide diversity of interesting questions with considerable success, it is also a challenging time for us given the intriguing puzzles that our strong theoretical focus identifies. Why are individuals primarily self-seeking in some settings and not in others? What are the selection, learning, and information conditions that affect the likelihood that specific kinds of intrinsic preferences are active? When intrinsic preferences lead to violence rather than cooperation, how can one break out of escalating processes leading to ever worsened outcomes? We have good beginnings to address all of these questions, but we have much more work to be done.

Besides our intellectual challenges, we also face a challenge as a section of the American Political Science Association. We are one of the largest sections of APSA. We currently have 686 members. We have, however, not had the best attendance at past APSA meetings. Thus, the number of panels assigned to us is lower than it should be given the size of our group. Thus, I hope we can address this in the 2005 meetings by all of us giving general attention to the panels being organized by William Clark. These will be announced in the spring newsletter, but we should be paying good attention to them and getting announcements of them out so that we do have good attendance at the 2005 meetings.

It is also important that we start thinking about the meetings in 2006. I would like to see us start developing a self-conscious strategy for addressing some of the crucial issues of importance to our section and doing so overtly at the 2006 meetings. I was very pleased that Liz Gerber and Rick Wilson have agreed to co-coordinate the 2006 meetings. It is not too early to start thinking about how to address some of the crucial theoretical issues related to our field as well some of the fascinating empirical issues that we now face. Thus, the following topics might be candidates to consider including in our 2006 program:

- Behavioral Political Economy
- Explaining Diverse Types of Collective Action
- Contending Theories of Individual Behavior in Diverse Contexts
- Institutions and Development
- Institutions and Economic Policy
- Factors Affecting Governmental Performance
- Legislative and Voting Behavior

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pressures to provide private goods to narrow groups of voters. The key insight, however, is that clientelism is not assumed, but emerges from politician efforts to solve their credibility problem.

The credibility of political promises is more difficult than information to measure directly. However, the theory of credibility and clientelism provides a powerful device for interpreting another robust finding in the cross-country literature, the significant effect of "years of democracy", or in Keefer (2004b), the continuous years of competitive elections, on policy outcomes. If it is the case that political competitors in younger democracies have had less opportunity to build up credibility, then we would expect younger democracies to exhibit precisely the behavior predicted in Keefer and Vlaicu (2004). This seems to be the case. Keefer (2004b) finds that young democracies engage in more targeted spending (the government wage bill and spending on targetable public investment are high relative to GDP); provide fewer public goods (secondary school enrollment, bureaucratic quality and the rule of law are all low); and high rent-seeking (corruption is high).

The evidence outlined earlier strongly suggests that many democracies exhibit a serious breakdown in accountability relations between governments and the governed. These breakdowns are not easily explained by differences across democracies in interest group behavior or in formal electoral and political institutions. They do seem to be explained by the underlying ingredients that are key in all contracting situations: citizen (consumer) information about the policy performance of the government (product); and citizen (consumer) confidence in the performance promises of the politician (seller). Nevertheless, and not surprisingly, important questions remain.

For example, what is the effect of voter information on public good provision? Government's most important activities, the ones most necessary to spur development, are related to the provision of public goods, be it control of infectious disease, provision of universal education, or pollution regulation. There is no data or theory that tell us whether informed voters are likely to spur greater public good provision, although information has a demonstrably powerful effect on other types of government decisions.

What determines the transition from unaccountable to accountable democracy? Acemoglu, Johnson and Robinson have discovered important links between colonial origins of countries and their 20th century economic performance. Is there something about the origins of countries that influences the evolution of voter information or the pre-electoral credibility of political actors? Keefer and Vlaicu (2004) suggest that Britain developed an accountable democracy following the dramatic expansion of the franchise through the 1800s because credible political competitors were in place prior to this process. In contrast, the Dominican Republic in 1961, just after the assassination of Rafael Trujillo, had no credible political parties or competitors, and has had a much more difficult political and economic trajectory since the advent of elected government. This leads one to ask whether the elite preferences and initial factor endowments described by Acemoglu, Johnson and Robinson influence not only the evolution of political institutions, but also the underlying conditions of political competition.

Finally, what are the sources of secure property rights? The prevailing wisdom, based on compelling arguments from Acemoglu, Johnson and Robinson, North and Weingast (1989) and many others, suggest that they emerge from formal institutions, particularly political checks and balances. But secure property rights have a public good element to them: expropriation of one investor drives off all investors, suppressing economic opportunities for all citizens. Do the conditions of political competition, such as the credibility of political promises and the extent of voter information, also influence the security of property rights? It is certainly the case that, even controlling for political checks and balances, the age of a democracy has a strong effect on the security of property rights.

The research agenda charted here is entirely complementary to the existing literature. It recognizes that history matters, and is likely to matter for reasons similar to those discovered by Acemoglu, Johnson and Robinson. It recognizes that political decision making is not only about the formal institutions of government, as much of comparative politics reminds us. And it gives prominence to a rigorous analysis of how the strategies of political competitors change in different institutional and non-institutional environments, consistent with the political economy tradition brought into full bloom with the recent work of Persson and Tabellini (2000). Arbitraging across these different conceptual approaches to the political economy of development seems a natural course to follow in explaining the continuing puzzles of failing democracies, successful autocracies, and the transformation of clientelist governments into accountable governments.

Note: For an extended, early version of the observations here, see Keefer (2004c).

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they cannot be caused by something else." If this statement were true, then quantitative social science would be very difficult! The central point of empirical research in social science is to achieve identification (that is, estimate causal effects) when potentially *endogenous* variables have *causal* effects on each other. The major tools of simultaneous equations estimation and instrumental variables in econometrics, for example, are about this problem.

Our whole approach is to take seriously the fact that institutions are endogenous, and that they are jointly determined with economic outcomes, and achieve reliable identification in this case. Pzreworski's definition of a primary cause seems to include only geography and climate. He suggests that (p. 10) "the embarrassingly obvious thought is that if endogeneity is sufficiently strong, institutions cannot have a causal efficacy of their own." This statement does not make sense to us. Caribbean islands and Florida are ravaged by hurricanes every year which themselves are caused by water heating up in the Atlantic Ocean. Pzreworski's claim amounts to denying that hurricanes cause devastation!

Pzreworski extends this thought (p. 10) by arguing that the institutions of a society may be efficient responses to their circumstances. This will not be heartening for Haitians, but luckily for them the instrumental variables strategy deals with precisely this issue. If institutions were efficient then changing institutions in Haiti, for example improving property rights, would have no effect on prosperity. Yet our estimates suggest that increased security of property rights would significantly raise income per-capita in Haiti. (See our 2004 paper for a discussion of whether institutions can be efficient.) The importance of an instrument here is that it is an exogenous source of variation in institutions, unrelated to factors that might determine the appropriateness of institutions to a particular society. Hence it allows us to answer the question of whether or not institutions are efficient. Our theory and empirical evidence does not explain how one could improve institutions in Haiti, clearly a difficult question, but we do believe that they establish that if institutions improved in Haiti then the country would become more prosperous.

Pzreworksi then claims that our approach is "deeply flawed" (p. 9) because (1) we have not examined institutions that coordinate investment, (2) "the assumption that institutions do not change", (3) problems with the "operationalization of the dependent variable" and (4) "problems with the use of instrumental variables estimator". As to the first point, we leave it to him to provide causal evidence that "institutions that coordinate investment," determine development. As to the second, the fact that institutions persist is not an assumption in our theory, it is a hypothesis which needs to hold if our historically based theory can explain institutional and economic divergence today. Pzreworski claims that "institutions changed in the meantime". Of course they did, but persistence does not mean that nothing changes. It seems to us obvious that institutions persist (and naturally this does not mean that we should not investigate *why* institutions persist). If they did not they could hardly structure social, economic, and political life. Any historian of Africa or Latin America would take as a basic premise the fact that the modern institutions of those societies are the result of historical processes where the past clearly influences the present. Slavery may be illegal today in Africa, but it is plausible to believe that the legacy of slavery lives on in the form of political and social institutions. This is what the persistence of institutions is about. Nevertheless, in our 2001 article we directly tested the proposition that in former colonies institutions persist, and found very strong evidence that they do.

Pzreworski then argues that Europeans brought themselves and human capital in places where institutions were good. In our 2001 paper we also tested this idea. We showed that the current fraction of people of European descent has no explanatory power for prosperity once the endogeneity of institutions has been properly controlled for. As to human capital investment, just as with the extent of financial development, these are *outcomes* not *causes*. A proximate explanation for why the US industrialized in the 19th century whereas Bolivia did not is that in the US people invested in education and there were lots of banks. But the reason they did this was because the institutions in the US stimulated such activities. Hence a regression with both institutions and human capital on the right hand side is incorrectly specified because it treats human capital as exogenous to institutions, which it is not. To the extent that such a regression finds human capital to be significant it would simply be capturing that human capital accumulation is one of the benefits of good institutions, which we certainly believe.

His final point, that there are problems with instrumental variables estimators, is of course well understood. He is correct when he quotes Nobel Laureate James Heckman (p. 11) as saying that "There is no assumption-free method of causal inference." This is both the difficulty and the challenge of quantitative social research. Unlike natural sciences, social sciences cannot undertake controlled experiments, so they have to infer causality from the data generated by real world social systems. The best practice way in econometrics of doing this is to look for natural experiments, quasi-natural experiments and instruments which represent exogenous sources of variation. Theory is useful both in guiding us in our search and in substantiating the identifying assumptions related to instruments or sources of variation. Using this methodology, we believe we

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- Why is Peace and Economic Development Achieved in Some Countries with Strong Heterogeneities When Violence Prevails in Others?

Hopefully, this will stimulate others to begin to think about our activities over the forthcoming years. Please plan to attend the 2005 APSA meetings this fall and go to as many of the Political Economy panels as you can. Please also send Elisabeth Gerber (ergerber@umich.edu) and Rick Wilson (rkw@rice.edu) any ideas you may have concerning the 2006 meetings so that we can really have sufficient lead time to develop some excellent panels. Hopefully, some of them will be cutting edge and can serve as the foundation for a special issue of a major journal.

Also, we do have three awards to be given at the 2005 APSA meetings. If you have not already thought about who would be eligible to receive the Riker or Olson awards, please do so right away. The committees reviewing nominations for these awards are:

APSA Political Economy Section Awards

William H. Riker Best Book Award, given for the best book in political economy published in the previous year.

Chair: Stephen Haggard, UCSD, IR/PS
9500 Gilman Drive
La Jolla, CA 92093-0519
Email: shaggard@ucsd.edu
Committee Members: Sarah Brooks, John D. Huber

Mancur Olson Best Dissertation Award, given for the best dissertation completed and accepted in the previous two years.

Chair: Joe Oppenheimer, Department of Government and Politics, University of Maryland
3140 Tydings Hall
College Park, MD 20742
Email: joppenheimer@gvpt.umd.edu
Committee Members: Scott Gehlbach, Gerry Mackie

Best Paper Award, given to the best paper in Political Economy presented at the previous year's APSA Annual Meeting.

Chair: Layna Mosley, Department of Political Science, University of North Carolina at Chapel Hill
361 Hamilton Hall, CB 3265
Chapel Hill, NC 27599-3265
Email: lmosley@email.unc.edu
Committee Members: Frances Rosenbluth, Torben Iverson

As you will read elsewhere in this issue—Layna, Frances, and Torben have already received nominations and made a selection of the best paper in our section for the 2004 APSA meetings.

Acknowledgments and References

Let me thank many colleagues for their suggestions to earlier, very rough drafts of some of these ideas, including Scott Adler, James Cox, Robert Franzese, Liz Gerber, Clark Gibson, Stephan Haggard, Layna Mosley, Frank van Laerhoven, Michael Schoon, and Rick Wilson.

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have made considerable progress on understanding the driving processes behind comparative development. These modern techniques are designed to deal with the difficult issues which impede causal statements in social science.

Nevertheless, we certainly regard our findings as tentative and preliminary. Many issues remain. We need much better ways of how to measure institutions. There are problems with how the security of property rights is measured and we need to understand much better the interaction between economic and political institutions. This requires, for example, the collection and coding of historical data (such as that which we attempted for early modern Europe in our 2002b article). We need to understand much better the mechanisms leading to the choice of institutions, their persistence, and sometimes their change. We also need to develop theories which will help us find instruments for institutional development outside the colonial world.

Since our approach is intrinsically political, we believe this is an exciting agenda in which we hope to engage with and learn from political scientists. One cannot understand why a society has the economic institutions it has without understanding the politics of that society (Acemoglu, Johnson and Robinson, 2004). We do not agree therefore with Pzreworski's initial statement that "Once again, economists stole our thunder" (p. 3). Over the years we have learned a terrific amount from political scientists and refer continually to their work. One of us (James Robinson) even abandoned economics and became a political scientist! This research agenda is exciting, not only because it is about big and exciting questions, but because it is genuinely interdisciplinary. We see no reason for political scientists to accept the implicit conclusion of Pzreworski's paper when he asks "is the science of comparative politics possible?"

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General Announcements

Social Science Research Network Announcement

Dear Political Economy Section Members:

I have been appointed the Editor for three journals for the Social Science Research Network at www.ssrn.com. The SSRN eLibrary is made up of two main sections: an Abstract Database comprising abstracts for over 80,500 scholarly working papers and forthcoming papers and an Electronic Paper Collection currently encompassing over 55,800 downloadable full text papers.

I wanted to encourage you and your colleagues to submit working papers to SSRN especially if the work pertains to behavioral and experimental research in finance, economics or accounting. This is the link where to submit your working papers:

http://www.ssrn.com/update/forms/absubmission.html.

Here is an updated description and a list of board members for the three journals: Behavioral & Experimental Accounting <http://www.ssrn.com/update/arn/arn_behav-exp-acctg.html>, Behavioral & Experimental Economics <http://www.ssrn.com/update/ern/ern_behav-exper-ec.html> and Behavioral & Experimental Finance <http://www.ssrn.com/update/fen/fen_behav-exper-fin.html>.

Please let me know if you have any questions or concerns.

All the best,

Vic Ricciardi, Department of Finance and Economics, Golden Gate University
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Call for Papers

Jakob de Haan, Faculty of Economics, University of Groningen and Thomas Plümper, Faculty of Political Science and Public Administration, University of Konstanz invite paper proposals for the 2005 conference, "Partisan Politics, Political Autonomy and Policy Harmonization across Europe. Does EU integration lead to a convergence of partisan politics on the national level?," to be held in het Kasteel, Groningen, The Netherlands, 19-20 May. Those interested should direct email inquiries to thomas.pluemper@uni-konstanz.de.

Upcoming Events

Summer Institute on the Empirical Implications of Theoretical Models EITM at Washington University, Summer 2005 June 12-July 1, 2005

Washington University's **Weidenbaum Center** and **Department of Political Science** will sponsor their third NSF-supported institute on the problems of testing theoretical models of politics next summer. The institute is designed for advanced graduate students and junior faculty whose research and teaching would benefit from training seminars on the link between methods of empirical anlaysis and theoretical models. Seminars combine both theory and method and several seminars address major substantive fields of application. The third NSF-EITM institute will be held in June 13-July 1, 2005. The deadline to apply for the 2005 Summer Institute is **February 15, 2005**. An **application form** is available for your perusal.

Faculty in the 2005 program include Charles Cameron, Curt Signorino, Kevin Quinn, Randy Calvert, Andrew Martin, Lee Epstein, Jeff Segal, Rick Wilson, and Gary Miller, among others.

The seminars are Theoetical and Methodological Foundations, Quantal Response Models, Experimental Tests of Theoretical Models, Operationalization of Spatial Models, and Issues in Testing Positive Theories of Judicial Politics.

More information is available at eitm@wc.wustl.edu/eitm or you can write to eitm@wc.wustl.edu.