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Constitutions, Politics, and Economics: A Review Essay on Persson and Tabellini's *The Economic Effects of Constitutions*

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In this essay, I review the new book by Torsten Persson and Guido Tabellini, The Economic Effects of Constitutions, *which investigates the policy and economic consequences of different forms of government and electoral rules. I also take advantage of this opportunity to discuss the advantages and disadvantages of a number of popular empirical strategies in the newly emerging field of comparative political economy.*

1. *Introduction*

Political economy has now emerged as an active and flourishing subdiscipline of economics, with the ambition of offering insights into the causes of the large cross-country differences in major economic outcomes and policies.[1] Torsten Persson and Guido Tabellini deserve much of the credit for making this happen. Their book, *Political Economics: Explaining Economic Policy,* which was published by MIT Press in 2000, is an excellent introduction to basic models of political decision-making and their implications for economic outcomes. The objective of their new book, *The Economic Effects of Constitutions,* also published by MIT Press, is to continue where the first book left off, and confront theory with data.

The book deserves enthusiastic reception for a number of reasons, including the authors' role in advancing the political economy research program, its ambitious objectives, its careful scholarship, and the long list of findings that come out of the research methodology. Equally, however, such a bold attempt at confronting theory with data requires a critical look. This motivates my review of their new book, focusing on the identification issues that arise both in this context and more generally in recent political economy research.

This review essay is organized as follows. Section 2 introduces the context and the objectives of Persson and Tabellini's new

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[1] While there is renewed interest in political economy, in large part in the context of understanding the sources of large cross-country differences in policies, institutions and economic performance, this work is preceded by the important results of the earlier "social choice" literature. David Austen-Smith and Jeffrey S. Banks (2000) provide an excellent introduction to the major results in this literature.

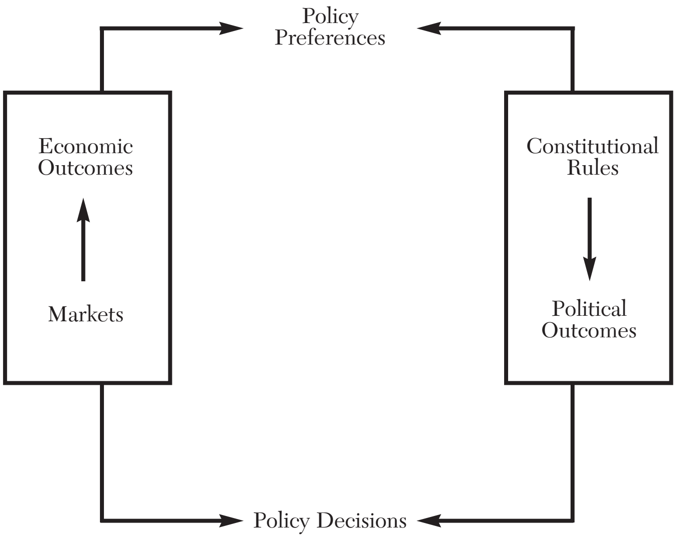

Figure 1: The Policymaking Process (Figure 1.1, page 3)

book. Section 3 provides a detailed discussion of econometric issues and identification problems that Persson and Tabellini have rightly emphasized in their work. Section 4 discusses the major empirical results of the book. Sections 5–8 discuss a number of problems and challenges facing empirical work in political economy, and reevaluate Persson and Tabellini's contribution in this light. Sections 9 and 10 discuss two sets of topics that I view as important areas for future research in political economy.

2. *Background and Objectives*

Persson and Tabellini (henceforth PT) set the scene with figure 1.1 in their book, which I replicate here. The box on the left is where much of economics falls. We have a good, though far from perfect, understanding of how different organizations of markets lead to different economic outcomes. But why are markets organized differently in different societies? Economists and other social scientists have long realized the importance of "policies." If the government imposes rent control, this will impact how the rental market works. The major advance that came with political economy is encapsulated in a new question: why do policies differ over time, across countries and more broadly across polities? Why, for example, do some governments impose rent control while others do not intervene in rental markets. Before, we had no better answer than "(some) politicians just don't get it." Political economy is about developing better answers.

This task requires a framework for thinking about why policies differ across countries. This is what much of political economy does. Starting with Kenneth J. Arrow's celebrated (im)possibility theorem (Arrow 1963, Duncan Black 1948), Anthony Downs's median voter result (Downs 1957, Harold Hotelling 1929), and George J. Stigler's work on regulation (Stigler 1970, 1972), the central working hypothesis has been that agents—as voters, lobbyists, revolutionaries, politicians—have *induced* preferences over policies. This means that they understand that different policies will map into different outcomes, and consequently their preferences over policies are shaped by their preferences over the outcomes that will be induced by the policies.[2] Political economy is then about understanding how these induced preferences over policies are aggregated. The recent work in political economy is also part of this research program, but more explicitly investigates why collective decisions and policies differ across societies.

Much work in political science, on the other hand, as succinctly captured by the box on the right in Persson and Tabellini's figure is about how different political procedures lead to different political outcomes. Let us refer to political procedures as "political institutions" and make the link between political outcomes and policies. This gives us a natural framework for thinking about why different societies choose different policies—they have different political institutions.

PT's first book, *Political Economics*, developed various models of collective decision-making and derived predictions on how differences in political institutions translate into differences of policies and economic outcomes. This new book is the next step to push this framework further by confronting its predictions with data.

Although this type of comparative analysis has been vibrant in political science, there are no systematic theoretical analyses deriving precise predictions from micro-founded models to confront with data. Here PT are much more ambitious. They take the predictions of a set of theoretical models, in particular, the probabilistic voting model of Assar Lindbeck and Jörgen Weibull (1987), extensively studied in their first book, and an extended version of John Ferejohn's (1986) model of politician accountability, studied in Persson, Gerard Roland, and Tabellini (1997, 2000), seriously. Although one can have qualms about the specific assumptions that are important to derive predictions in these models, PT's is the right approach in social science in general, and in comparative political economy more specifically. What makes this work even more impressive is the fact that the authors are investigating predictions of their earlier models.

In reality, PT have an even more ambitious goal. Many scholars attempt to document a set of correlations in the data that are related to predictions of existing models. Instead, PT are interested in more than correlations. They undertake the ambitious task of identifying the causal effects of the form of political institutions on economic and political outcomes. They write on page 7: "Our ultimate goal is to draw conclusions about the causal effect of constitutions on specific policy outcomes. We would like to answer questions like the following: if the United Kingdom were to switch its electoral rule from majoritarian to proportional, how would this affect the size of its welfare state or its budget deficits? If Argentina were to abandon its presidential regime in favor of a parliamentary form of government, would this facilitate the adoption of sound policy towards economic development?"

How would we get to the causal effects of a set of political institutions on outcomes? PT have a well-specified strategy. First, they focus on a subset of political institutions; the constitutionally determined form of electoral rules and the form of government among democracies. This leaves out a large part of political institutions, for example, those that relate to whether a country is a democracy or not, and thus a lot of interesting questions related to what makes constitutions credible in the first place.[3] Nevertheless, narrowing the scope

[2] This approach is referred to as "rational choice" in political science. To economists, it is the natural approach without apologies.

[3] See, for example, Avinash Dixit, Gene M. Grossman, and Faruk Gul (2000). This question perhaps is not first-order for PT because they focus on democracies that are "fully consolidated" and do not switch to dictatorship, and investigate how electoral rules affect policies within the democratic framework. Nevertheless, whether a society is a fully consolidated democracy is endogenous and potentially also depends on the form of government and electoral rules in democracy (see Acemoglu and Robinson 2005 for a discussion).

of the investigation is the right approach to be able to estimate causal effects. Second, they use a variety of state-of-the-art techniques developed in the micro-econometrics literature, in particular, matching estimators, propensity score methods, instrumental variables estimation, and parametric selection corrections.

Since the identification of causal effects is the major objective of this book, and also because the econometric issues that arise here are becoming increasingly important in the political economy literature, in the next section I discuss PT's econometric methodology in detail.

3. *Econometric Methodology*

3.1 *The Basic Framework*

PT's major interest is to identify the causal effect of two political institutions, presidentialism (versus parliamentary systems) and majoritarian electoral rules (versus proportional representation), on the amount and the composition of government spending, and on politicians' rents and other political outcomes. Ultimately, part of the interest is also to estimate the effect of these different constitutional forms on aggregate productivity and income per capita levels.

Following PT's exposition in chapter 5, I simplify the problem by focusing only on one binary constitutional feature, denoted by $S_i \in \{0,1\}$, and a generic outcome variable, Y_i, with i denoting country in a cross-sectional dataset.[4] PT consider a relatively general econometric framework, incorporating potentially heterogeneous effects. For the purposes of this review, let us focus on a model that is linear in the covariates and features constant (marginal) effects. Let $\mathbf{X}_i$ be a $K \times 1$ vector of (observable) exogenous covariates and suppose that the structural relationship of interest is $E(Y_i|\mathbf{X}_i) = \alpha S_i + \mathbf{X}_i'\boldsymbol{\beta}$, or:

$$(1) \qquad Y_i = \alpha S_i + \mathbf{X}_i'\boldsymbol{\beta} + u_i,$$

where α measures the effect of S_i on the outcome of interest, $\boldsymbol{\beta}$ is a $K \times 1$ vector of coefficients associated with $\mathbf{X}$, and u_i captures the effect of all unobserved determinants as well as random influences on Y_i.[5]

PT use four different empirical strategies to estimate (1) or variants: ordinary least-squares (OLS), matching and propensity score estimators to relax the linearity assumption in (1), instrumental variables (IV), and parametric selection corrections due to Heckman.

These strategies deal with the identification problem in (1) in two different ways, which can be classified as "selection on observables" and "exclusion restrictions" (see Angrist and Krueger 1999).

3.2 *Selection on Observables*

The identifying assumption for strategy 1 is that conditional on $\mathbf{X}_i$, S_i and u_i are orthogonal. This last condition implies that the OLS estimator is consistent. To see this briefly, let variables with the tildes denote the original variable after the effect of the vector $\mathbf{X}_i$ has been partialed out.[6] Then, (1) can be rewritten as:

$$(2) \qquad \tilde{Y}_i = \alpha \tilde{S}_i + \tilde{u}_i,$$

which implies that

[4] See, for example, Joshua D. Angrist and Guido W. Imbens (1995), Angrist, Imbens, and Donald B. Rubin (1996), Angrist and Alan B. Krueger (1999), James J Heckman (1997), and Jeffrey M. Wooldridge (2002), for more detailed treatments.

[5] It is worth noting that equation (1) can be estimated without the constant marginal effects and linearity assumptions to obtain "average treatment effects" with weights depending on the estimation strategy, see Angrist and Krueger (1999).

[6] In words, these are the residuals from the original variable's linear projection on the vector $\mathbf{X}_i$, given by $\tilde{\mathbf{y}} = (\mathbf{I} - \mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X})\mathbf{y}$, where $\mathbf{X}$ denotes the $K \times N$ matrix obtained from the $\mathbf{X}_i$'s.

$$\operatorname{plim}\hat{\alpha}^{OLS} = \frac{\operatorname{Cov}(\tilde{S}_i, \tilde{Y}_i)}{\operatorname{Var}(\tilde{S}_i)}$$

$$(3) \qquad = \alpha + \frac{\operatorname{Cov}(\tilde{S}_i, \tilde{u}_i)}{\operatorname{Var}(\tilde{S}_i)},$$

where the variance and covariance terms refer to population values. Consequently, the identifying assumption for strategy 1 is $\operatorname{Cov}(\tilde{S}_i,\tilde{u}_i) = 0$ (or that conditional on $\mathbf{X}_i$, u_i be statistically independent from S_i).

The fact that S_i and u_i need to be orthogonal conditional on $\mathbf{X}_i$ highlights the importance of selection on observables. It is quite possible that S_i is correlated with some other determinants of Y_i, but once we condition on the covariates, this correlation disappears. Therefore, for this class of strategies controlling for the right set of observables is of primary importance.

What distinguishes the OLS and matching strategies is the way in which they control for the effect of these observables. OLS regression, as in (1), imposes linearity, whereas the matching estimators allow for non-linear effects of observables. With matching estimators, the parameters of interest are estimated conditional on a set of observables, and then the average effect is obtained by averaging these conditional parameter estimates. The advantage of this estimator is that it allows the parameter of interest to depend flexibly on the characteristics used for matching. Propensity score methods enable consistent estimation when there are many characteristics according to which observations can be matched (see Angrist and Krueger 1999, Wooldridge 2002, chapter 18).

With both strategies, however, there are numerous reasons why $\operatorname{Cov}(\tilde{S}_i,\tilde{u}_i) = 0$ may not be valid. In this context this might be, for example, because the constitutional features of a country are partly determined by omitted factors that also influence policy choices and economic outcomes. These factors may include differences in the nature of economic opportunities across societies, the distribution of noninstitutional political power between various groups, differences in culture, and differences in other institutional features affecting potential outcomes. Since the conceptual issues arising with both linear regression and matching estimators are similar, in what follows I focus on the identification issues with OLS, which are simpler to discuss.

To highlight the potential bias in OLS estimation, suppose, again using the tilde notation:

$$(4) \qquad \tilde{u}_i = \lambda \tilde{L}_i + \omega_i,$$

where L_i is some other potential determinant of the outcomes of interest, and $\tilde{L}_i$ denotes its residuals from the projection on the $\mathbf{X}_i$'s, while ω_i satisfies $\operatorname{Cov}(\tilde{S}_i, \omega_i) = 0$. This implies that

$$\operatorname{plim}\hat{\alpha}^{OLS} = \alpha + \lambda \frac{\operatorname{Cov}(\tilde{S}_i, \tilde{L}_i)}{\operatorname{Var}(\tilde{S}_i)}.$$

Therefore, the OLS estimator will be inconsistent if there exists a potential determinant L_i of the outcome of interest (i.e., $\lambda \neq 0$), which is also correlated with the form of government or electoral rules (i.e., if $\operatorname{Cov}(\tilde{S}_i,\tilde{L}_i) \neq 0$. This corresponds to the usual omitted variable bias. This notation can also capture the measurement error (attenuation) bias whereby we observe a noisy signal of the true institutions, $\tilde{S}_i$, while what we care about is $\tilde{T}_i = \tilde{S}_i - \tilde{\tau}_i$, where $\tilde{\tau}_i$ is a classical measurement error term, with $\operatorname{Cov}(\tilde{T}_i,\tilde{\tau}_i) = 0$. In this case, $\tilde{L}_i = -\tilde{\tau}_i$ and $\lambda = \alpha$, so that $\operatorname{plim}\hat{\alpha}^{OLS} = \alpha \operatorname{Var}(\tilde{T}_i)/(\operatorname{Var}(\tilde{T}_i) + \operatorname{Var}(\tilde{\tau}_i))$.

3.3 *Exclusion Restrictions and Instrumental Variables*

The most important line of attack that researchers have against potential biases of the OLS estimator is to use an instrumental variables (IV) strategy, which is PT's third strategy. The IV strategy relies on a $M \times 1$ vector of instruments, $\mathbf{Z}_i$, which has predictive power for the endogenous regressors, but is orthogonal to u_i in (1) (where naturally,

$M \geq K+1$). The exclusion restriction refers to this last requirement, and is equivalent to the condition $E(\mathbf{Z}_i u_i) = 0$. This means that the vector of instruments, $\mathbf{Z}_i$, has no effect on the outcomes of interest other than its impact through the endogenous regressors.

A common view in the applied economics literature is that the IV strategy is "atheoretical." This view originates from the fact that typically the IV strategy does not necessitate a fully specified structural model. Nevertheless, as I will argue in greater detail below, every IV strategy requires an underlying theory. In essence, the IV strategy starts with an essentially nontestable hypothesis, that the vector of instruments, $\mathbf{Z}_i$, is orthogonal to omitted influences captured by the error term u_i. This presumption must be based on some theoretical consideration, and the IV procedure would have no justification without a compelling (a priori) theory. This underlying theory can be extremely simple, for example, the hypothesis that, everything else equal, a change in compulsory schooling laws or their enforcement should affect schooling (e.g., Angrist and Krueger 1991, Acemoglu and Angrist 2000), but have no effect on earnings through other channels; or more involved, for example, the hypothesis that all information contained in the past income realizations should already be incorporated in the current consumption decisions and should have no predictive power for consumption growth (e.g., Robert E. Hall 1978, Lars Peter Hansen and Kenneth J. Singleton 1982). I return below to the theoretical justifications of the instruments used in PT's work and in other work in political economy.

For now, let me simplify the discussion by assuming that all of the variables in $\mathbf{X}_i$ are exogenous, and there is a single excludable instrument, Z_i, for the only endogenous regressor, S_i. So the instrument vector is simply $\mathbf{Z}_i = (\mathbf{X}_i, Z_i)$. Using the tilde notation, the estimating equation can be written as (2), with a corresponding first-stage relationship of

$$\tilde{S}_i = \gamma \tilde{Z}_i + v_i, \tag{5}$$

where v_i denotes the residuals from the linear regression of $\tilde{S}_i$ on $\tilde{Z}_i$, thus $\mathrm{Cov}(\tilde{Z}_i, v_i) = 0$. The probability limit of the instrumental variable estimator of α is given by

$$\operatorname{plim} \hat{\alpha}^{IV} = \frac{\mathrm{Cov}(\tilde{Z}_i, \tilde{Y}_i)}{\mathrm{Cov}(\tilde{Z}_i, \tilde{S}_i)} \tag{6}$$

$$= \alpha + \frac{\mathrm{Cov}(\tilde{Z}_i, \tilde{u}_i)}{\gamma \mathrm{Var}(\tilde{Z}_i)}.$$

This equation encapsulates the two requirements for a valid instrument; (1) relevance, which requires that there should be a first-stage relationship, i.e., $\lambda \neq 0$;[7] (2) excludability, which means that $\mathrm{Cov}(\tilde{Z}_i, \tilde{u}_i) = 0$ (which is equivalent to $\mathrm{Cov}(Z_i, u_i | \mathbf{X}_i) = 0$, or to $E(\mathbf{Z}_i u_i) = \mathbf{0}$).

To illustrate the usefulness of the IV strategy, let us return to the model of the unobserved effect in equation (4), and suppose that $\mathrm{Cov}(\tilde{S}_i, \tilde{L}_i) \neq 0$, so that OLS is inconsistent. In this case, using the IV strategy leads to an estimate of α, $\hat{\alpha}^{IV}$, with the following probability limit:

$$\operatorname{plim} \hat{\alpha}^{IV} = \alpha + \frac{\lambda \mathrm{Cov}(\tilde{Z}_i, \tilde{L}_i)}{\gamma \mathrm{Var}(\tilde{Z}_i)}.$$

Therefore, if $\mathrm{Cov}(\tilde{Z}_i, \tilde{L}_i) = 0$, the IV estimator will be consistent (given $\gamma \neq 0$). The condition that $\mathrm{Cov}(\tilde{Z}_i, \tilde{L}_i) = 0$ is sometimes loosely referred to as the requirement that "the instrument should be orthogonal to omitted effects." Moreover, the above discussion of measurement error illustrates that a valid

[7] Note also that if $\gamma \approx 0$, then even though $\operatorname{plim} \hat{\alpha}^{IV} = \alpha$, in small samples $\hat{\alpha}^{IV}$ could systematically deviate from α, causing the "weak instruments" problem (see, for example, Douglas Staiger and James H. Stock 1997, Stock, Jonathan H. Wright, and Motohiro Yogo 2002). The sister problem is the "too many instruments" problem, which arises when the vector $\mathbf{Z}_i$ includes a large number of excluded instruments (relative to the number of observations), leading to a spurious first-stage relationship (e.g., Christian Hansen, Jerry Hausman, and Whitney Newey 2004).

instrument also removes the attenuation bias arising from classical measurement error.

In all of this, the set of covariates included in $\mathbf{X}_i$ is important because one can imagine that Z_i could be correlated with the error term in the second stage, but this correlation disappears once we condition on a set of covariates (i.e., we may have that while $\mathrm{Cov}(Z_i,u_i|\mathbf{X}_i^1) \neq 0$, $\mathrm{Cov}(Z_i,u_i|\mathbf{X}_i^2) = 0$ for some (not necessarily mutually exclusive) different sets of covariates $\mathbf{X}_i^1$ and $\mathbf{X}_i^2$). Interestingly, this issue does not arise when there is random assignment due to real experiments or because of natural experiments. This makes the identification problem more difficult and subtle in political economy models than in many other contexts, and makes the distinction between the selection on observables and exclusion restriction strategies I tried to highlight above less clear-cut.

PT include income per capita, democracy score, age of democracy, measures of trade, measures of age composition of the population, a dummy for federal structure and a dummy for OECD in $\mathbf{X}_i$ in their main specifications. The instruments are various combinations of the timing of adoption of the country's constitution, fraction of the populations that speak English or another European language, and latitude.

I will discuss the validity of these instruments below. Before doing this, however, an important estimation issue needs to be raised. The above derivation of the consistency of the instrumental variable estimator made it clear that the same set of covariates, $\mathbf{X}_i$, needs to be included both in the first and the second stage. This was made explicit by the "tilde" notation above, where the regressions include the variables after the vector $\mathbf{X}_i$ has been partialed out. Equations (5) and (6) show immediately why the IV estimator, given the identification assumption $\mathrm{Cov}(\tilde{Z}_i,\tilde{u}_i) = 0$, is consistent in this case.

PT, instead, choose a nonstandard econometric method, and exclude some of the $\mathbf{X}_i$'s from the first stage. In particular, suppose that the vector of covariates is partitioned as $\mathbf{X}_i = (\mathbf{X}_i^1,\mathbf{X}_i^2)$, and only $\mathbf{X}_i^1$ is included in the first-stage relationship (while the entire $\mathbf{X}_i$ is included in the second stage). In practice, the procedure is the following: first, S_i is predicted on the basis of $\mathbf{X}_i^1$ and Z_i (using linear regression). Denote the predicted values by $\hat{S}_i$. Then the parameters of interest are estimated using an OLS regression with $\hat{S}_i$ instead of S_i. It can be shown that for this estimator, $\hat{\alpha}^{PT}$, to be consistent the $\mathbf{X}_i$'s that are omitted from the first stage should have no predictive power for the endogenous regressors (conditional on the other covariates).[8] Otherwise, the residuals from the first stage, which mechanically go into the second stage, are no longer orthogonal to the covariates, and this biases the estimate of the vector $\boldsymbol{\beta}$, and the bias from $\boldsymbol{\beta}$ carries over to the estimate of the parameter of interest, α.[9] Therefore, the estimator used

[8] In particular, let the true (statistical) first-stage relationship be $S_i = \gamma Z_i + (\mathbf{X}_i^1)'\boldsymbol{\pi}_1 + (\mathbf{X}_i^2)'\boldsymbol{\pi}_2 + \varepsilon_i$, with $\hat{S}_i = \gamma Zi + (\mathbf{X}_i^1)'\boldsymbol{\pi}_1 + (\mathbf{X}_i^2)'\boldsymbol{\pi}_2$ denoting the predicted component of S_i. The second-stage equation of the traditional 2SLS procedure is $Y_i = \alpha\hat{S}_i + (\mathbf{X}_i^1)'\boldsymbol{\beta}_1 + (\mathbf{X}_i^2)'\boldsymbol{\beta}_2 + u_i$. The assumption $E(\mathbf{Z}_i u_i) = 0$ ensures consistent estimation of $\boldsymbol{\eta} = (\alpha,\boldsymbol{\beta})$ with the standard IV procedure. In contrast, the predicted value from the PT procedure is $\breve{S}_i = \breve{\gamma}Z_i + (\mathbf{X}_i^1)'\breve{\boldsymbol{\pi}}_1 = \hat{S}_i + (\breve{\gamma} - \gamma)Z_i + (\mathbf{X}_i^1)'(\breve{\boldsymbol{\pi}}_1 - \breve{\boldsymbol{\pi}}_1) - (\mathbf{X}_i^2)'\boldsymbol{\pi}_2$, where the second equality substitutes for the definition of $\breve{S}_i$. So the second stage in this case becomes: $Y_i = \alpha\hat{S}_i + (\mathbf{X}_i^1)'\boldsymbol{\beta}_1 + (\mathbf{X}_i^2)'\boldsymbol{\beta}_2 + \breve{u}_i$, where $\breve{u}_i = u_i - \alpha((\breve{\gamma} - \gamma)Z_i + (\mathbf{X}_i^1)'(\breve{\boldsymbol{\pi}}_1 - \boldsymbol{\pi}_1) - (\mathbf{X}_i^2)'\boldsymbol{\pi}_2)$. Defining $\breve{\mathbf{W}}_i = (\mathbf{X}_i,\breve{S}_i)$, PT's estimator is

$$\hat{\boldsymbol{\eta}}^{PT} = \left(N^{-1}\sum_{i=1}^{N}\breve{\mathbf{W}}_i\breve{\mathbf{W}}_i\right)^{-1}\left(N^{-1}\sum_{i=1}^{N}\breve{\mathbf{W}}_i Y_i\right)$$

$$= \boldsymbol{\eta} + \left(N^{-1}\sum_{i=1}^{N}\breve{\mathbf{W}}_i\breve{\mathbf{W}}_i\right)^{-1}\left(N^{-1}\sum_{i=1}^{N}\breve{\mathbf{W}}_i\breve{u}_i\right).$$

For this estimator to be consistent, we need $E(\breve{\mathbf{W}}_i\breve{u}_i) = \mathbf{0}$. However, when $\boldsymbol{\pi}_2 \neq \mathbf{0}$, $\breve{u}_i$ will be no longer be orthogonal to $\mathbf{X}_i^2$, and this will imply that the entire vector of coefficients $\boldsymbol{\eta}$ is not estimated consistently. In other words, we will typically have $E(\breve{\mathbf{W}}_i\breve{u}_i) \neq 0$ and $\mathrm{plim}\,\hat{\alpha}^{PT} \neq \alpha$, even if $E(\mathbf{Z}_i u_i) = 0$.

[9] For example, Wooldridge (2002, p. 91) notes: "In practice, it is best to use a software package with a 2SLS rather than explicitly carry out the two-step procedure. Carrying out the two-step procedure explicitly makes one susceptible to harmful mistakes. For example, the following seemingly sensible, two-step procedure is generally inconsistent: (1) regress x_K on $1, z_1, \ldots, z_M$ [the set of excluded instruments for x_K] and obtain the fitted values, say $\tilde{x}_K$; (2) run the regression [of the dependent variable on the other covariates and on $\tilde{x}_K$]..." This example is equivalent to the procedure that PT implement.

by PT may be inconsistent even when the standard identification assumption, $\mathrm{Cov}(\tilde{Z}_i, \tilde{u}_i) = 0$ or $E(\mathbf{Z}_i u_i) = 0$, is satisfied.[10] One might wonder whether this is an important source of bias in practice, and I return to this problem below.

Finally, PT's Strategy 4, parametric selection corrections, is related to the IV strategy. This is an attempt to deal with the failure of the assumption $\mathrm{Cov}(\tilde{S}_i, \tilde{u}_i) = 0$ by positing a functional form for selection on unobservables, based on the work by Heckman (1976, 1979). Specifically, PT posit that $S_i = 1$ if $G(\mathbf{Z}_i) + v_i \geq 0$, and $S_i = 0$ otherwise. Following the common practice, they assume that the v_i's are normally distributed, which enables a parametric correction for selection (see, e.g., Wooldridge 2002). Parametric selection approaches exploit the functional form assumptions on v_i, but otherwise rely on the same set of exclusion restrictions. Consequently, though they are a useful check on standard IV estimates, they will also lead to inconsistent estimates if the underlying exclusion restrictions are not valid. This motivates my focus on the linear IV approach for most of the discussion, though I will also contrast the results of the parametric selection approach to the OLS and IV estimates.

4. *Main Results and Assessment*

Using all four strategies, PT obtain a number of results that paint a broadly consistent picture. The most important results are:

1) Presidential and majoritarian systems have smaller governments than parliamentary and proportional representation systems (where government size is measured as government spending as a fraction of GDP). Majoritarian systems also appear to have smaller welfare state spendings and budget deficits.
2) While presidential or majoritarian systems in general do not have a robust effect on political rents, corruption and aggregate productivity, certain details of the electoral system, in particular the size of the electoral districts and whether voters cast their ballots for individual politicians or for party lists, do have significant effects.
3) Countries with parliamentary systems have more persistent fiscal outcomes than countries with a presidential system. Namely, in parliamentary systems, increases in government spending during downturns are not reversed during booms. There is a similar, but weaker pattern, for countries with proportional representation (relative to those with majoritarian elections). Finally, consistent with the predictions of the political business cycle models, proportional representation also appears to generate a greater expansion in welfare spending in the proximity of elections.

These results are exposed with great clarity, and PT show that they are robust. For example, the results are similar with linear regressions, or with the matching and propensity score methods. Importantly, results are also similar with the instrumental variables and parametric selection correction strategies, though sometimes less precise.

What makes these results even more important than this brief description would suggest is that they are closely related to the predictions of the models that have motivated PT's study. For example, the models that are most popular in the literature suggest that electoral rules, in particular, how broad or narrow districts are, should affect both the overall size of government, the composition of spending and the extent of political rents. These are confirmed by the empirical results in the book.

[10] PT justify this econometric method by arguing that, because the major source of bias is not reverse causality but omitted variables, excluding some second-stage covariates from the first stage might be justifiable. This may be an argument for why we should expect $\boldsymbol{\pi}_2 = 0$ in terms of the notation in footnote 8. They further suggest that this might be useful because it would lead to a stronger first-stage relationship, avoiding weak instrument problems.

In addition, models based on the importance of separation of power, such as Persson, Roland, and Tabellini (1997, 2000), suggest that presidential systems where there is typically greater separation of powers should have smaller governments, which is also the pattern that PT find in the data.

These are remarkable results. Only a very brave or uninformed scholar could attempt to write on comparative political economy without seriously studying this book, and only a very stubborn researcher would have his or her posteriors remain unchanged after studying it. PT have already achieved something very few scholars can: a body of work for not only the current generation of researchers, but also for the next generation. In fact, the impact of the book might even be greater than this discussion suggests. If the results indeed correspond to the causal effects of the form of government and electoral rules on policies and economic outcomes as PT claim, we have learned more with this book than from the entire comparative politics literature of the past fifty years.

Despite these remarkable results, there are reasons to question whether this research has successfully uncovered causal effects. The OLS and matching estimates ultimately rely on the exogeneity of political institutions. Nevertheless, political institutions are equilibrium outcomes, determined by various social factors that are not fully controlled for in the empirical models. This makes me believe that, although the OLS results uncover interesting patterns, they do not typically identify causal effects. For causal effects, we have to turn to estimates using the exclusion restrictions. Here PT's work is again the state-of-the-art, but the exclusion restrictions are not necessarily compelling, making it unlikely that the IV estimates are uncovering causal effects either. The next three sections discuss these problems in greater detail.

5. *Identification in Political Economy*

5.1 *General Issues*

PT have put identification of causal effects as the primary objective of their work. In this they are not alone. Much recent research of political economy is about understanding the effects of various policies or institutional features on economic development. The issues of identification are therefore at the forefront. I will argue that even though PT have undertaken a careful and thorough investigation of the questions at hand, they may have not estimated the causal effect of the constitutional features on the outcomes of interest. Moreover, the identification problems that arise in this work are shared by many other studies in political economy.

Let us start by recalling that the basic approach is to understand differences in policies and economic outcomes as a function of the underlying political institutions of societies. This methodological approach builds on the notion that agents have induced preferences over policies because they recognize the implications of these policies for their well-being. Although I wholeheartedly agree with this methodology, I also think that there is an immediate next step that one can (and should) take. By exactly the same token, agents should have *induced preferences over political institutions.* This is because different political institutions will generate different policies, and thus lead to different economic outcomes, and rational economic (and political) agents should understand not only the implications of different policies but also the implications of different political institutions. The logic of political economy therefore forces us to also think of political institutions as *endogenous.*

This leads to an important issue: while a large body of empirical literature argues that political institutions are "predetermined" or given by history and treats them as exogenous, the reasoning of the political economy approach implies that these institutions are also endogenous and likely determined by the same factors that make policies unappealing variables to treat as exogenous.

This point has featured in one way or another in the comparative politics literature. It is emphasized in its most general form by the Charles A. Beard's classic 1913 study, *An Economic Interpretation of the U.S. Constitution,* where he argues that the primary objective of the government and the constitution is to ensure favorable economic conditions for those holding political power, and the form of government should be seen as a (secondary) feature serving the same objective—by "secondary" Beard does not mean less important, but chosen in the same way and for the same objectives as the overall structure of political power. He writes: (1913, p. 13)

> Inasmuch as the primary object of a government, beyond the mere repression of physical violence, is the making of the rules that determine the property relations of society, the dominant classes whose rights are thus to be determined must perforce obtain from the government such rules as are consonant with the larger interests necessary to the continuance of their economic processes, or they must themselves control the organs of government. In a stable despotism the former takes place; under any other system of government, where political power is shared by any portion of the population, the methods and nature of this control become the problem of prime importance—in fact, the fundamental problem in constitutional law. The social structure by which one type of legislation is secured and another prevented—is a secondary or derivative feature arising from the nature of the economic groups seeking positive action and negative restraint.

If Beard is correct in his assessment, then many features of constitutions will be influenced by factors that also have a direct impact on policy and economic outcomes. To illustrate this possibility, imagine a world consisting of a group of politically powerful elite and citizens. The elite oppose redistribution, while the citizens favor it. When the elite are more powerful, they will be able to limit the amount of redistribution and the size of government. Imagine also that the elite prefer presidential systems to parliamentary systems. In this case, when the elite are more powerful, the equilibrium form of government is more likely to be presidential. If, in addition, the strength of the elite is persistent, there will be an empirical correlation between presidentialism and smaller governments, not only at the time the constitution is written, but also in the subsequent decades. But this correlation would not necessarily reflect the causal effect of presidentialism on equilibrium policies.[11] In terms of the econometric discussion above, the requirement that $\mathrm{Cov}(\tilde{S}_i, \tilde{u}_i) = 0$ in equation (2), is violated because there is an omitted factor, like $\tilde{L}_i$ in equation (4), in this case the political strength of the elite, which will affect both the form of government and the outcomes of interest. The same concerns generalize to the other potential outcome variables.

In this light, the OLS (non-IV) estimates of the causal effects of the form of government and electoral systems should not be interpreted as causal. Instead, they likely correspond to interesting robust correlations in the data. For causal estimates, we have to rely on IV approaches.

5.2 *The Instrumental Variables Approach in Practice*

PT attempt to estimate the causal effects of the form of government and electoral systems by using a simple IV approach. Their potential instruments are three constitutional dating variables, indicating whether a

[11] A similar argument, though with the opposite implication, is suggested by another major figure in comparative political science, Stein Rokkan. Rokkan argued in his 1970 book that the emergence of proportional representation in Continental Europe was a result of the previous elites' attempts to manage the transition to democracy in a manner that was consistent with their interests. He suggests that a key objective of the proportional representation system was to make the emergence of a socialist majority more difficult. If Rokkan is correct, then because proportional representation emerged in societies where the elites were more powerful, we may expect a negative correlation between proportional representation and redistribution, as opposed to the positive correlation documented by PT.

country adopted its current form of government and current electoral rule after 1981, between 1951–80, between 1921–50, with before 1921 as the omitted category; two language variables, indicating the fraction of the population in the country speaking one of the major European languages, and the fraction speaking English as a native language; and latitude (distance from the equator).

Although the IV approach adopted by PT is important on methodological grounds, I will argue that it is unlikely to be estimating the causal effect of the form of government and electoral rules on policy and economic outcomes for two reasons: first, because the three constitutional timing variables have little effect on the endogenous regressors, identification ultimately relies on the fraction of the population with European language as native tongue and latitude, which are not convincing instruments for institutional features in general; second, even if they were valid for some general institutional features, by implication they could not be valid instruments for specific institutional features such as the form of government or electoral rules.[12] In this subsection, I show the importance of the language and latitude instruments for identification, and also briefly look at the implications of not including second stage covariates in the first stage.

Let us begin with the reasoning for these instruments. The three timing variables could be relevant for the form of government and for the electoral system because there may have been waves (or "fads") in the type of constitutions, and different countries fell into different waves depending on when they adopted their constitution or declared their independence. The two language variables and latitude are included as proxies for "European influence" following the arguments in Hall and Charles I. Jones (1999). Hall and Jones use these variables as instruments for the overall quality of institutions (social infrastructure), and PT argue that these could also have affected the form of government and electoral rule. The reasoning for these Hall–Jones instruments is discussed in the next section.

These potential instruments are highly correlated with the form of democratic institutions. Typically, countries farther away from the equator and those with a greater fraction of the population speaking English as a native language (but not those with other European languages) are less likely to have presidential systems. For majoritarian systems, the effects of English and other European languages are reversed. There is some effect from the constitutional dating variables, but these are typically weaker, and less consistent (depending on what covariates are included in the first and second stages).

The general pattern suggests that with only the constitutional variables, there is no first stage for the instrumental variable strategy.[13] This is illustrated in table 1 where I use PT's data to replicate and further investigate their first-stage relationships. Columns 1a and 1b are identical to the first two columns of PT's table 5.1, and separately estimate OLS first-stage relationships with dummy variables for presidential and majoritarian systems on the right-hand side (respectively PRES and MAJ). Both columns include seven variables; three dummies for the dates of the adoption of the constitution, latitude, fraction of the population speaking English and fraction of the population speaking another European

[12] Another potential problem is that, as discussed in section 3, a successful IV strategy also requires $E(\mathbf{X}_i u_i)=\mathbf{0}$, i.e., the covariates that are treated as exogenous to be orthogonal to the error term. These covariates here include income per capita and trade, which are jointly determined with the size and composition of government spending, and their inclusion in the equation may lead to biased estimates. Nevertheless, reestimating PT's IV models excluding income per capita and trade leads to results similar to their baseline IV estimates.

[13] Lack of a first-stage relationship even at the conventional significance levels is an extreme form of the weak instruments problem mentioned in footnote 7.

language as native languages, and age of democracy (the latter is included as a covariate in the second stage equations as well, while the others are excluded instruments). They also report F-tests for joint significance of all the excluded instruments and the constitutional variables at the bottom.

These two columns show a reasonably strong first-stage relationships, which are then used by PT in their 2SLS estimation (the R^2 is 0.48 in the first column and 0.40 in the second column). However, we can see that the major determinants of the endogenous regressors are the Hall–Jones instruments, not the constitution timing dummies. In both columns 1a and 1b, the six instruments are jointly significant at less than 1 percent, but the three constitutional dummies are only significant at 2 percent in column 1a (for the presidential dummy) and very far from statistically significant in column 1b (for the majoritarian dummy).

Columns 2a and 2b show that the joint significance of the excluded instruments, and especially of the constitutional variables, is significantly reduced when the second-stage covariates are also included in the first stage. Recall that these second-stage covariates are log income per capita, the proportions of the population between the ages of fifteen and sixty-four, and over the age of sixty-five, the sum of exports plus imports divided by GDP, the average of political and civil liberties indices of Freedom House, and dummies for a federal structure and OECD (see PT, especially table 6.2 and the data appendix for details). As discussed above, PT include these covariates in the second stage, but not in the first-stage relationship. Once they are also included in the first stage, the constitutional dummies are no longer jointly significant for either presidential or majoritarian dummies.

I will argue in greater detail in sections 6 and 7 that latitude and European languages may not be valid instruments for the form of government and electoral rules. In this light, it is important to understand whether they are essential in the first-stage relationships reported by PT. Columns 3a,b and 4a,b show that they are, and that once these variables are left out, there is a very weak or no first-stage relationship left. Without the second-stage covariates, the constitution dummies are significant at 3 percent and 5 percent respectively in columns 3a and 3b, and when the second-stage covariates are included, they are highly insignificant.

This implies that an instrumental-variables strategy relying only on the constitutional timing dummies would not achieve identification.

Table 2 shows the corresponding 2SLS estimates of the form of government and the electoral system on the size of government. Column 1 replicates PT's results (the corresponding first stages are in columns 1a,b of table 1). Column 2 shows that the estimates are slightly less precise when the second-stage covariates are included in the first stage, though the dummy for a presidential system continues to be statistically significant. This shows that leaving the second-stage covariates out of the first stage is unlikely to have caused a major bias in this case.

Columns 3 and 4 exclude the Hall–Jones instruments and show that there is no longer a significant effect of the form of government or the electoral system on the size of government. For example, the coefficient on the presidential dummy in column 3 is –2.01 (standard error = 5.11) as compared to –8.65 (standard error = 3.61) in column 1. Evidently, the Hall–Jones instruments are essential for PT's instrumentation strategy.

Table 3 shows the 2SLS results for the other major outcome variable that PT look at in their chapter 6, the composition of spending, proxied by the share of social spending and welfare in GDP. The structure of this table is identical to that of table 2. It also shows that the IV estimates of the effect of constitutional features on policy outcomes are sensitive to excluding the Hall–Jones instruments. Furthermore, in this case, including the second-stage covariates in

TABLE 1
CONSTITUTION SELECTION: FIRST STAGE ESTIMATES

	Persson and Tabellini (2003) Sample							
	Cross-Section OLS							
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
	Dependent Variable is Constitution							
	PRES	MAJ	PRES	MAJ	PRES	MAJ	PRES	MAJ
CON2150	–0.39	–0.161	–0.070	–0.132	0.095	–0.323	0.022	–0.319
	(0.138)	(0.162)	(0.149)	(0.161)	(0.145)	(0.153)	(0.132)	(0.169)
CON5180	–0.120	0.074	–0.095	0.302	0.277	0.169	0.197	0.131
	(0.182)	(0.236)	(0.220)	(0.248	(0.167)	(0.152)	(0.165)	(0.222)
CON81	0.266	0.055	0.161	0.238	0.589	0.004	0.358	–0.029
	(0.203)	(0.252)	(0.245)	(0.263)	(0.205)	(0.183)	(0.206)	(0.272)
LAT01	–1.366	–0.884	–0.786	–0.410				
	(0.335)	(0.388)	(0.520)	(0.619)				
ENGFRAC	–0.692	0.916	–0.644	1.035				
	(0.119)	(0.122)	(0.127)	(0.174)				
EURFRAC	0.425	–0.349	0.431	–0.409				
	(0.113)	(0.134)	(0.153)	(0.186)				
AGE	0.540	0.197	0.703	0.083	0.280	0.150	0.602	0.358
	(0.308)	(0.295)	(0.320)	(0.245)	(0.401)	(0.329)	(0.326)	(0.413)
Second stage covariates	NO	NO	YES	YES	NO	NO	YES	YES
F-test on all constitution variables	3.66	0.52	1.23	1.35	3.35	2.61	1.09	2.20
	[0.02]	[0.67]	[0.30]	[0.27]	[0.02]	[0.06]	[0.36]	[0.10]
F-test on all instruments	24.39	26.69	7.97	12.07				
	[0.00]	[0.00]	[0.00]	[0.00]				
Observations	78	78	77	77	78	78	77	77
R-squared	0.48	0.40	0.55	0.48	0.18	0.07	0.41	0.23

Robust standard errors in parentheses. F-test on all constitution variables refers to the F-statistic for the joint test that the coefficients on CON2150, CON5180, and CON81 are each zero. F-test on all instrumental variables refers to the F-statistic for the joint test that the coefficients on CON2150, CON5180, CON81 , LAT01, ENGFRAC, and EURFRAC are each zero. P-value for the F-statistic is in brackets. Columns 2a, 2b, 4a, and 4b include but do not report LYP, TRADE, PROP1564, PROP65, GASTIL, FEDERAL, and OECD. PRES is a dummy variable which is equal to 1 in presidential regimes and zero otheriwse. MAJ is a dummy variable which is equal to 1 if all the lower house is elected under plurality rule and zero otherwise. See Persson and Tabellini (2003) for data and definitions.

TABLE 2
SIZE OF GOVERNMENT AND CONSTITUTIONS: SECOND STAGE INSTRUMENTAL VARIABLE ESTIMATES

	Persson and Tabellini (2003) Sample			
	Cross-Section 2SLS			
	(1)	(2)	(3)	(4)
	Dependent Variable is Central Government Expenditure as % GDP			
PRES	–8.653	–8.653	–2.005	–4.098
	(3.614)	(4.713)	(5.108)	(9.183)
MAJ	–3.901	–4.233	–9.384	–9.194
	(3.448)	(3.466)	(6.900)	(7.896)
Constitution instruments in first stage	YES	YES	YES	YES
Hall-Jones instruments in first stage	YES	YES	NO	NO
Second stage covariates in first stage	NO	YES	NO	YES
Observations	75	75	75	75

Standard errors in parentheses. All columns include but do not report AGE, LYP, TRADE, PROP1564, PROP65, GASTIL, FEDERAL, and OECD in the second stage. All columns include CON2150, CON5180, CON81, and AGE in the first stage. Columns 1 and 2 include LAT01, ENGFRAC, EURFRAC in the first stage. Columns 2 and 4 include all second stage covariates in the first stage. See Table 1 for first stage regressions. PRES is a dummy variable which is equal to 1 in presidential regimes and zero otheriwse. MAJ is a dummy variable which is equal to 1 if all the lower house is elected under plurality rule and zero otherwise. See Persson and Tabellini (2003) for data and definitions.

the first stage also matters to some extent, and the effect of a majoritarian electoral system is no longer significant at 5 percent in column 2.

This brief discussion illustrates the importance of the Hall–Jones instruments for PT's identification strategy,[14] (and also touches upon the importance of including second-stage covariates in the first stage of the IV estimation). The implication is that the IV strategy in PT's book heavily relies on the validity of the Hall–Jones instruments. Could these instruments be valid in the current context?

I believe there are two fundamental reasons for these instruments not to be valid. The first is that the Hall–Jones instruments are unlikely to be valid for the overall quality of institutions. The second is that even if these instruments were valid for the overall quality of institutions, they would *by implication* be invalid for a specific feature of the institutional structure such as presidentialism or a majoritarian electoral system. I discuss these two issues in the next two sections. This discussion is not only relevant to PT's work, but also to a number of recent papers adopting similar strategies.

[14] PT are upfront about this issue, and argue that "Admittedly, these [Hall–Jones] variables could be correlated with other unobserved historical determinants of fiscal policy or corruption . . . [As] we are confident about the exogeneity of the time dummies for constitutional adoption, we can test the validity of the additional instruments by exploiting the overidentifying restrictions" (p. 131). In addition, the parametric selection models that they report also yield very similar estimates to the linear IV estimates. Nevertheless, these checks are unlikely to be sufficient to validate the IV estimates. The overidentifying restrictions have very little power, since, as we saw above, the constitutional timing variables have little explanatory power in the first stage. The parametric selection models, on the other hand, also use the same exclusion restrictions in addition to the parametric restrictions, so the similarity in the results is not too surprising.

TABLE 3
Composition of Government Spending and Constitutions:
Second Stage Instrumental Variable Estimates

	Persson and Tabellini (2003) Sample			
	Cross-Section 2SLS			
	(1)	(2)	(3)	(4)
	Dependent Variable is Central Government Expenditure on Social Services and Welfare as % GDP			
PRES	0.296 (1.791)	0.527 (2.609)	0.615 (2.461)	1.585 (4.734)
MAJ	–3.633 (1.663)	–3.645 (1.928)	–3.920 (2.493)	–3.857 (3.041)
Constitution instruments in first stage	YES	YES	YES	YES
Hall-Jones instruments in first stage	YES	YES	NO	NO
Second stage covariates in first stage	NO	YES	NO	YES
Observations	64	64	64	64

Standard errors in parentheses. All columns include but do not report AGE, LYP, TRADE, PROP1564, PROP65, GASTIL, FEDERAL, and OECD in the second stage. All columns include CON2150, CON5180, CON81, and AGE in the first stage. Columns 1 and 2 include LAT01, ENGFRAC, EURFRAC in the first stage. Columns 2 and 4 include all second stage covariates in the first stage. See Table 1 for first stage regressions. PRES is a dummy variable which is equal to 1 in presidential regimes and zero otheriwse. MAJ is a dummy variable which is equal to 1 if all the lower house is elected under plurality rule and zero otherwise. See Persson and Tabellini (2003) for data and definitions.

6. *Are the Hall–Jones Variables Valid Instruments for Institutions?*

Hall and Jones wrote a very important and influential article emphasizing the importance of social infrastructure, or institutions on aggregate productivity and economic growth. Hall and Jones suggested that the origins of good institutions around the world lie in Europe, so "proximity" to Europe, as measured by the fraction of those speaking European languages and latitude, can be used as instruments for the quality of institutions.

Hall and Jones's paper has already become a classic. It is not only cited extensively, but their arguments are often invoked to use latitude and the fraction of the population speaking European languages as native tongues as instrument for various institutional features in the recent empirical literature.

This makes it useful to reconsider the theoretical justification of these instruments. What is the theory underlying the Hall–Jones instruments? In broad strokes, Hall and Jones's theory is as follows. "Good institutions" originated in Western Europe and spread from there to other countries. Therefore, a potential determinant of the quality of institutions is the extent to which a country has been influenced by Europe's culture, values and institutions. Hall and Jones isolate two channels of European influence. The first is through a shared language, and the second is through geography. Consequently, they argue, countries with a greater fraction of the population speaking European languages and those farther from the equator, which were less densely populated and geographically more similar to Europe hence more conducive to European migration, have benefited from a benign/beneficial European influence. Based on this theory, they use the fractions of the population speaking European languages and distance from the equator (latitude) as instruments for the overall quality of institutions.

Some studies, including PT, cite my work with Simon Johnson and James A. Robinson as supporting this view. Nevertheless, the theory, and therefore the instrumentation strategy, in Acemoglu, Johnson, and Robinson (AJR) (2001) is different. First, in AJR we focus on former European colonies where European powers had a large effect on institutional development, often by imposing the institutions and running these areas as colonies. Second, we do not argue that European influence was positive (or negative). Instead, the argument is that Europeans had very different effects on different colonies depending on what was the most attractive colonization strategy. In places where they faced high mortality rates, they could not settle, and they were more likely to opt for an extractive strategy, with associated extractive/bad institutions. In places with low mortality rates, they were more likely to settle, and good institutions were more likely to emerge, because these were institutions that Europeans themselves would live under. This contrast is illustrated by a comparison of the United States or Australia to the European colonies in Central America, the Caribbean, South Asia, or Africa. In a companion paper, AJR (2002), we documented the similar effect of indigenous population density. Europeans were more likely to settle and less likely to pursue extractive strategies in colonies that were less densely populated.

Let us now revisit the theoretical foundations of the Hall–Jones instruments in the light of this discussion. Neither the results in these papers nor historical evidence support the theory that European influence was generally beneficial (see also Stanley L. Engerman and Kenneth L. Sokoloff 1997). Indeed, the history of the Caribbean Islands clearly illustrates the adverse effects of Europeans, which set up repressive regimes based on slavery and forced labor. Instead, the evidence is more consistent with the theory that European influence could be either beneficial or harmful to institutional development. Whether or not it was beneficial depended on the European powers' colonization strategy.

Is it possible that the use of these instruments leads to consistent estimates, even though the story for identification is not entirely substantiated? There is an argument to be made here. For example, AJR (2001) show that in the sample of former European colonies, once mortality of European settlers is used as an instrument for institutions, latitude has no additional effect on income per capita today. It may then be argued that latitude could be used as an instrument and would capture the same sort of variation as settler mortality. This argument is not compelling, however, mostly because latitude is being used as an instrument for the whole world sample whereas the "experiment" in question, European colonization, applies only to the former European colonies. There is no valid theoretical argument for extending this experiment to the entire world (and for the sample of former European colonies, there is no reason to use latitude instead of settler mortality). For example, for other countries not affected by European colonization, geographic considerations may have had other, different effects, which may or may not be orthogonal to omitted determinants of the outcome variables (see, for example, AJR 2004 on the changing effect of access to the Atlantic for European nations during the early modern period). Consequently, there is no justification for using latitude, or for that matter anything to do with settler mortality rates, in a sample that includes countries that were themselves European or were never European colonies.

Overall, the theoretical foundations of the Hall–Jones instruments are not entirely compelling, especially when used for the entire world, and consequently, there are good reasons to suspect that they may not be excludable from the regressions of interest.

7. *Cluster of Institutions Versus Specific Institutions*

The above discussion centered around the question of whether the Hall–Jones variables are valid instruments for the overall quality of institutions. I now turn to a discussion of the problems involved in using these variables as instruments for *specific institutions.* This has become a common practice in the newly flourishing empirical political economy literature. PT also follow this practice and use these variables as instruments for the form of government and electoral rules (indicators of presidential or majoritarian systems). I will argue that there are serious problems in this procedure because of inherent complementarities between different types of institutions.

To develop this argument, let us put aside the concerns raised in the previous section and suppose that the Hall–Jones instruments (or perhaps settler mortality rates in the former European colonies sample) are valid for the overall quality of institutions (or the broad cluster of institutions). This means we now suppose that they are excludable from a second-stage regression of economic outcomes (such as aggregate output or productivity) with the broad measure of institutions as the endogenous regressor.

Here the distinction between a broad cluster of institutions and specific institutions is crucial. In AJR (2001), we defined a broad cluster of institutions as a combination of economic, political, social and legal institutions that are mutually reinforcing. For example, it is impossible to think of a system like the plantation economies in the Caribbean Islands until the nineteenth century together with democratic political institutions. This is because a set of economic institutions, like the plantation system, that lead to a very unequal distribution of income and wealth cannot easily survive with a set of political institutions that distribute political power equally. Those with political power would be greatly tempted to use their power to redistribute income and change the economic institutions in line with their interests. Economic institutions that lead to a very unequal distribution of income and wealth are only consistent with a similarly unequal distribution of political power, i.e., with dictatorships and other repressive regimes. In this case, sources of variation that affect a broad cluster of institutions (e.g., economic and political institutions together) would not be useful in identifying the role of specific institutional features.

As an example of the difficulty of this type of strategy to estimate the effect of specific institutions, consider the quasi-natural experiment due to international politics, the division of Korea into North and South. The two parts of Korea before the division were ethnically, culturally, economically, and socially very similar. But because of the geopolitical balance between the United States and Soviet Union, the South ended up largely capitalist, while the North became communist. In the following forty years, we witnessed a large divergence between these two countries. This is a good source of variation to understand the effect of the broad cluster of institutions at the level of "quasi-capitalist" versus "communist" systems. Suppose now that we try to use this source of variation to understand the effect of some specific institutional feature, say financial development, on economic growth. It should be clear that this strategy will lead to a highly biased estimate. It is true that South Korea is financially more developed than North Korea. It is also true that the reason for this is the division in 1946 (had it not been for the division, the North and the South would probably have similar levels of financial development). But this *does not make* the division a good experiment to understand the effect of financial development, because this division also caused many other institutional changes. It is a good laboratory for the study of broad institutions, but not for a study of the specific institutions.

Another example directly related to PT's empirical work may also be useful. In the first-stage relationships shown in table 1, the fraction of the population speaking English is a strong predictor of a majoritarian system, and this fraction may be a predetermined variable, shaped, for example, by colonial history. Nevertheless, this is not sufficient for it to be an excludable instrument in estimating the causal effect of majoritarian systems, even if as Hall and Jones claim, English influence is conducive to the development of good institutions overall. For example, as we know from Rafael La Porta, Florencio Lopez-de-Silanes, Andrei Shleifer, and Robert W. Vishny (1998), countries more influenced by the English heritage also have more developed financial markets. So the correlation between majoritarian electoral systems (instrumented by fraction of the population speaking English) and the size of government may reflect the effect of financial development, which is omitted from the regressions.

To make these issues a little more precise, consider the following structural model:

$$(7) \qquad Y_i = \alpha G_i + \mathbf{X}_i'\boldsymbol{\beta} + u_i ,$$

where Y_i denotes the outcome of interest, $\boldsymbol{\beta}$ is again a vector of coefficients associated with $\mathbf{X}$, which is now assumed to stand for a set of noninstitutional covariates. G_i is a measure of a broad "cluster of institutions." Moreover, suppose that there are $K > 1$ specific institutions, each denoted by S^k, and

$$(8) \qquad G_i = \sum_{k=1}^{K} \nu^k S_i^k + \mu_i$$

where ν^k denotes the effect of specific institution S^k on the cluster of institutions. Suppose that $\text{Cov}(u_i,\mu_i) \neq 0$ and/or $\text{Cov}(u_i,S_i^k) \neq 0$ for some k, so that equation (cluster) cannot be estimated consistently by OLS.

Next, suppose that we have an instrument Z_i, in particular a country-specific variable, which is potentially related to each of the specific institutions, i.e.,

$$(9) \qquad S_i^k = \zeta^k Z_i + \upsilon_i^k ,$$

and is a valid instrument for the cluster of institutions, G_i, i.e., $\text{Cov}(Z_i,u_i) = 0$. So if we have a good measure of G_i, equation (7) can be estimated consistently by IV.

However, the objective here is to estimate the effect of specific institutions. To simplify the discussion, also assume that $\text{Cov}(Z_i,\mu_i) = 0$, and $\text{Cov}(\mu_i,\upsilon_i^k) = \text{Cov}(Z_i,\upsilon_i^k) = 0$ for all k and $\text{Cov}(\upsilon_i^k,\upsilon_i^j) = 0$ for all $j \neq k$. Note that S_i^k's are correlated even if υ_i^k's are independent, since they are all affected by Z_i. This correlation is at the root of the identification problems facing IV estimates of the impact of specific institutions.

In the last section, I questioned whether latitude could play the role of Z_i to estimate the structural relationship in (7). Now let us put aside the issues raised there, and suppose that indeed $\text{Cov}(Z_i,u_i) = 0$. Can we then use Z_i as an instrument for a specific institution, say S^1? The answer is no. If we were to do this, all of the S^k's would also load onto S^1.

More explicitly, we are now estimating

$$Y_i = \alpha^1 S_i^1 + \mathbf{X}_i'\boldsymbol{\beta}^1 + u_i^1$$

with IV using Z_i as the excluded instrument. Given (7) above, the true value of α^1 is $\alpha^1 = \alpha\zeta^1$. Moreover, from equations (8) and (9),

$$(10) \quad u_i^1 = u_i + \alpha \sum_{k=2}^{K} \nu^k S_i^k + \alpha\mu_i .$$

We can now see that since, by construction, $\Sigma_{k=2}^{K} \nu^k S_i^k$ is correlated with $\tilde{Z}_i$, $\text{plim}\alpha^{1,IV} \neq \alpha^{1,IV}$. In particular

$$\text{plim}\hat{\alpha}^{1,IV} = \alpha^1 + \frac{\text{Cov}(\tilde{Z}_i, \tilde{u}_i^1)}{\zeta^1 \text{Var}(\tilde{Z}_i)}$$

$$= \alpha^1 + \frac{\alpha \sum_{k=2}^{K} \nu^k \zeta^k \text{Var}(\tilde{Z}_i)}{\zeta^1 \text{Var}(\tilde{Z}_i)}$$

$$= \alpha^1 + \frac{\alpha \sum_{k=2}^{K} \nu^k \zeta^k}{\zeta^1},$$

where the second equality exploits (10) and the fact that $\mathrm{Cov}(Z_i, u_i) = \mathrm{Cov}(Z_i, \mu_i) = 0$.

Note that in the most extreme case where $\zeta^1 \rightarrow 0$, even though $\mathrm{Cov}(Z_i, u_i) = 0$, we can get arbitrarily biased estimates of the effect of S^1 on the outcome of interest.

This discussion also makes it clear that the problem of instrumenting for a specific institution, such as S_i^1, is in many ways similar to the omitted variable bias, since other specific institutions that make up the cluster of institutions, G_i, are omitted from the regression. Even if we include proxies for some of them, unless we can correctly estimate the causal effects of all of those, IV regressions will fail to estimate the causal effect of the specific institution of interest, S_i^1, consistently.

For the identification strategy of using Z_i as an instrument for S^1 to be valid, we need in addition that $\zeta^k = 0$ for all $k = 2, \ldots K$. Consequently, even if we were convinced that the Hall–Jones instruments are valid for the broad cluster of institutions, there is little justification for using them as an instrument for specific institutions such as presidential or majoritarian systems. This discussion implies that in IV approaches in political economy, there is often a first-order question of *unbundling*, meaning going from an understanding of the role of a broad cluster of institutions to pinpointing which specific institutions are more important for the economic outcomes of interest. I discuss this problem further below.

8. *What Have We Learned?*

I have so far argued that some of the common empirical strategies in the political economy literature will have difficulty in uncovering the causal effect of specific institutions. Relatedly, PT's work may not have estimated the causal effect of majoritarian and presidential systems on the amount of redistribution, political rents and aggregate productivity. So one might argue that judged on the basis of their strong objective of estimating causal effects, the book is only a partial success. This would be the wrong conclusion, however.

When the body of work in the book is taken as a whole, it is a tremendous success. The correlations that PT document between the form of government and electoral systems and various economic outcomes are very important. Few comparative political economy papers can be written from now on that do not take these stylized facts seriously. PT have not necessarily estimated the causal effects of the form of government and electoral systems. But even noncausal but robust relationships are important and valuable inputs into our thinking and into our models.

Therefore, I believe that overall PT have largely achieved their ambitious aim of revolutionizing comparative political economy, and this book is the most significant contribution to this field since Lipset's work almost fifty years ago. PT have not only pushed comparative political economy forward, but they have provided a set of findings that will challenge all economists and social scientists, and likely pave the way for a large body of new work in this area. With such an important contribution, it is then the right time to wonder what will (and perhaps should) come next.

9. *Unbundling Institutions*

I argued above that a source of variation in the broad cluster of institutions is not sufficient to separately estimate the effects of specific institutional features. In other words, we can find clever instruments, from history, sometimes from geography, or international politics, that affect the whole social organization of a society, but this is only the first step. It does not enable us to conclude that one specific institution is more important than another.

However, what we want to know in practice is not only that "institutions" (defined as a broad cluster, and therefore almost necessarily as a black box) matter, but which specific dimensions of institutions matter for which outcomes. It is only the latter type of knowledge that will enable better theories of institutions to be developed and practical policy recommendations to emerge from this new area. Consequently, the issue of "unbundling institutions," that is, understanding the role of specific components of the broad bundle, is of first order importance.

We therefore need to find other strategies, even more clever instruments, or other, perhaps new, econometric techniques to decide which specific dimensions of these institutions matter. I believe that there is going to be a lot of exciting research in this area in the next ten years. Let me discuss a couple of potential questions, and in some cases, potential avenues for investigation.

1) Contracting institutions versus property rights institutions: a key question that emerges from works that emphasize the importance of institutions concerns the relative importance and specific effects of institutions that regulate interactions between private citizens ("contracting institutions") and those that constrain the behavior of political and economic elites ("property rights institutions"). Evidence that a broad cluster of institutions matters for long-run economic development is consistent with the primary role of both types of institutions. Moreover, there are many reasons to expect societies that have better property rights institutions to also have better contracting institutions, so these two types of institutions are likely to covary in practice. So unbundling in this context is very important.

   Acemoglu and Johnson (2004) attempt this type of unbundling. The idea is to combine the strategy in AJR (2001, 2002), which exploits the effect of local conditions at the colonies on institutional development together with the strategy of La Porta, Lopez-de-Silanes, Shleifer, and Vishny (1998), which exploits differences in the identity of the colonizer. While local conditions, in particular the disease environment and the indigenous population density at the time of colonization, affect the property rights institutions, the identity of the colonizer is important for the legal origin and therefore for the contracting institutions. We show that there are almost perfectly "separable" first-stages whereby the identity of the colonizer has almost no effect on property rights institutions, while local conditions have no effect on contracting institutions. This enables a multiple IV strategy to separately identify the effects of both types of institutions. The results suggest that while property rights institutions have a first-order impact on all aspects of economic and financial development, contracting institutions mainly affect the form of financial intermediation (and have no effect on long-run economic growth, on investment and on the overall amount of financial development).

2) Financial development versus contracting institutions: a similar unbundling issue arises in evaluating the role of financial development (and financial institutions) and economic development. A large literature surveyed in Ross Levine (1997, 2004) documents a robust correlation between financial development and economic growth. But is this the effect of financial development, or the direct effect of better contracting institutions that enable financial development in the first place? More explicitly, better contracting institutions will lead to better nonfinancial contracts (such as between downstream and upstream firms, firms and workers, etc.) as well as to improved financial contracts, so in the data when we see a society with greater financial development, it will also have better

nonfinancial contracts. This makes it difficult to identify the effect of financial development.

A potential strategy for unbundling in this context might be to investigate the effect of purely financial reforms that do not change the scope for nonfinancial contracts. Reforms in capital accounts that are imposed (or "encouraged") by the IMF or other international bodies, without other reforms, could provide one useful source of variation.

3) Economic versus political institutions: an even more formidable task is to separately identify the effects of economic and political institutions. There is a clear complementarity between these two types of institutions. For example, an economic system like slavery or forced labor cannot be sustained in a democratic society. Nevertheless, many relevant theoretical and policy questions require the potential effects of economic and political institutions to be unbundled. For example, what would be the effects of improving economic institutions under a given set of political institutions, for example as in China? This is an area for future research.
4) Different types of economic institutions: the question is which economic institutions matter more. Entry barriers? Labor market regulations? Property rights enforcement? Limits on government corruption? These are important, but also very difficult questions. It seems that these questions will be almost impossible to answer with cross-country data alone, and micro data investigations, for example, exploiting differences in regulations across markets and regions appear to be the most promising avenue. Interesting recent work using within-country variation to look at some of these issues include, among others, work by Timothy Besley and Robin Burgess (2004), who look at the implications of different labor market regulations across Indian states, by Thomas J. Holmes (1998), who investigates the implications of union laws on the location of manufacturing plants in the United States, by Marianne Bertrand and Francis Kramarz (2002), who look at labor market effects of product market regulations, and by Jakob Svensson (2003), who exploits differences across industries to investigate the amount of bribes that firms in Uganda have to pay to officials
5) Formal versus informal political institutions: finally, returning to the theme of PT's book, how important are formal institutions? PT's work suggests that they are essential. However, in practice, formal and informal institutions are highly correlated, and one may conjecture that changing constitutions or formal rules of political decision-making might have limited effects if society at large expects the changes not to be durable or not to be obeyed. The important results in this book notwithstanding, this is also an important area for future research.

10. *Weakly Institutionalized Polities*

Another area for future research is what might be referred to as politics in "weakly institutionalized polities." The body of work that PT have surveyed and developed further in their two books is largely for the analysis of "strongly institutionalized polities," where political institutions make politicians, at least partially, accountable to citizens. It is not surprising that this has been the first focus of Western academics, who almost all live in strongly institutionalized polities. But the same is not true for a large fraction of the population of the world. The situation in many countries in Africa, Central America, and the Caribbean corresponds much more clearly to one of "weakly institutionalized polities," where state–society relations are fundamentally different. Examples of this include, but are not limited to, the extreme kleptocratic regimes of Mobutu in the Democratic Republic of the Congo (Zaire), Rafael Trujillo in the Dominican Republic, the Duvaliers in Haiti, the Somozas in Nicaragua, Charles Taylor in Liberia, and Ferdinand Marcos in the Philippines.

Much historical evidence suggests that a systematic study of the political economy of such regimes must depart from some of the modeling approaches of politics in strongly institutionalized polities. While in strongly institutionalized polities, formal political institutions, such as the constitution, the structure of the legislature, or electoral rules, place constraints on the behavior of politicians and political elites, and directly influence political outcomes, the same does not appear to be the case in weakly institutionalized polities. Instead, the nature of politics appears to be different between strongly and weakly institutionalized polities. Most importantly, when institutions are strong, citizens have the power to punish politicians by voting them out of power; when institutions are weak, politicians pursue clientelistic policies that punish citizens who fail to support them (see Robert H. Jackson and Carl G. Rosberg 1982 and Acemoglu, Robinson, and Thierry Verdier 2004).

In modeling politics in weakly institutionalized polities, the first question that emerges is theoretical: how can we understand policy making and collective decisions in such societies? There are only a few papers that attempt to develop answers this question. In Acemoglu, Robinson, and Verdier (2004), we construct a dynamic model of politics in weakly institutionalized polities based on the idea of "divide and rule." The key focus is to understand how kleptocratic regimes that impoverish their citizens can remain in power for so long.[15] The answer we suggest is that, owing to the absence of strong institutions, rulers can deploy strategies, in particular "divide-and-rule," to defuse opposition to their regime. The logic of the divide-and-rule strategy is to enable a ruler to bribe politically pivotal groups off the equilibrium path, ensuring that he can remain in power against challenges. By providing selective incentives and punishments, the divide-and-rule strategy exploits the fragility of social cooperation in weakly-institutionalized polities: when faced with the threat of being ousted, the kleptocratic ruler intensifies the collective action problem and destroys the coalition against him by bribing the pivotal groups.

A different and innovative answer is given by Gerard Padro-i-Miquel (2004), who also constructs a dynamic model of politics. Ethnic divisions are the key feature in his model. Each ethnic group is afraid of replacing their own leader when in power, because this increases the probability of a switch of power from their own ethnic group to a rival group. This makes the standard method of controlling political elites in strongly-institutionalized polities ineffective, and enables leaders to not only exploit other ethnic groups but also their own ethnic group. Padro-i-Miquel shows how this framework can account for a puzzling feature of African politics first highlighted in Robert H. Bates' classic study *Markets and States in Tropical Africa* (1981): the simultaneous use of inefficient transfers to and taxes on the same group.[16] In the logic of Padro-i-Miquel's model, this strategy makes sense because leaders need to keep their own group happy to remain in power. This sets a limit on the amount of net taxes they can impose on their

[15] Models of strongly institutionalized polities would suggest that poorly performing leaders should be replaced more often. In contrast, many disastrous kleptocracies last for long periods; Mobutu ruled for thirty-two years, Trujillo for thirty-one, and the Somozas for forty-two years. This longevity is made even more surprising by the fact that many kleptocratic regimes lack both a core constituency of supporters and a firm command of the military.

[16] Bates described the web of inefficient transfers and policies in effect in many parts of Africa, but most notably in Ghana and Zambia. For example, the Ghanaian government heavily taxed cocoa producers, while at the same time subsidizing their inputs of seeds and fertilizers. While the simultaneous use of taxes and subsidies may be to alleviate the negative effects of taxes on investments, the interpretation in the literature is that the extent of these practices in sub-Saharan Africa is beyond what can be justified as an optimal mix of taxes and subsidies.

own group. However, taxes across groups are linked, since much lower taxes on one group will encourage other groups to switch economic activity. This then motivates leaders to inefficiently subsidize their own ethnic group so that they can increase the tax rate on their own ethnic group, and consequently tax other groups more intensively.

A second crucial area is to construct models to understand how weak institutions can be strengthened. Although there is now a number of formal models of the creation and consolidation of democracy (see the literature review in Acemoglu and Robinson 2005), many of these models are still motivated by the practice of strongly institutionalized polities.

Finally, there is almost no work on how changes in formal political institutions affect policy and economic outcomes in weakly institutionalized polities. These are all fruitful areas for future research.

11. *Conclusion*

This book is an important landmark in political economy. It takes a body of theoretical literature seriously and carefully confronts their predictions with data. PT set themselves the very ambitious goal of estimating the causal effects of constitutions, in particular, the form of government and electoral rules.

Bearing in mind the difficulties of estimating causal effects in social sciences, especially in cross-country data, the book is a tremendous success. It documents important and robust correlations between the form of government and electoral systems, on the one hand, and various policy and economic outcomes, on the other. It also uses state-of-the-art econometric techniques to estimate causal effects.

I argued above that these methods may not have been sufficient to arrive at causal effects, both because of certain conceptual and practical problems. Nevertheless, the achievement here should not be underestimated. Even noncausal robust relationships are rare in comparative political economy, and those documented by PT appear to be highly robust and of central importance for our theoretical understanding. Few political economy papers can be written from now on that do not take this book, both its methodological and empirical contributions, seriously.

Equally important, with the addition of this book, (comparative) political economy has taken one more step toward establishing itself as a major field of economics, and it offers exciting and important research areas for further inquiry.

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