Does Rigidity Matter?
Constitutional Entrenchment and Growth

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Abstract: Should procedural barriers to constitutional amendment be more onerous than those to the policy changes of ordinary politics? – i.e., should constitutions be *entrenched*? One criterion by which to evaluate these questions is economic performance. Using data on countries worldwide and constitutional adoptions from 1973 to 2017, we estimate the effect of constitutional entrenchment (rigidity) on economic growth. We employ matching methods to make causal inferences. The adoption of a constitution that is meaningfully more rigid than its predecessor defines a treatment. In our benchmark estimations (based on 19 treatments), post-treatment effects on growth are generally small and statistically insignificant. However, when we examine a subsample that excludes autocracies (13 treatments), post-treatment effects are always negative and sometimes statistically significant. The same is true when we exclude treatments associated with coups (12 treatments). Contrary to many scholars' priors, the evidence suggests that, if anything (and based on the limited number of available treatments), greater entrenchment causes less economic growth.

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1. Introduction

Should constitutional prescriptions and constraints be *entrenched* – i.e., should they be insulated from the workings of simple majoritarian democracy? While this question may be evaluated based on various criteria, surely the effect of entrenchment on a country's economic performance is one of them. *Is constitutional entrenchment good for economic growth?* In this paper we aim to inform the first question by providing a tentative answer to the latter.

We employ matching methods to estimate the effect of increases in constitutional rigidity on subsequent economic growth. As elaborated on below, entrenchment can be considered in an ex ante or an ex post sense. Ex ante, constitutions can be designed such that it is costly to amend them. Ex post, constitutions may actually endure for long periods without amendment. In this paper we focus on the ex ante sense of entrenchment and estimate the effects of procedural rigidities that are designed into constitutions.

There are reasons to believe that entrenchment can lead to higher growth, all else equal. Buchanan and Tullock (1962) emphasize the tradeoff between decision-making and external costs that is involved in any collective action. While any political decision imposes external costs (i.e., those imposed upon citizens who would have preferred a different decision), a constitution determines under what circumstances political agents can impose external costs today, tomorrow, and beyond (Salter and Young 2018). As such, the external cost side of the tradeoff will weigh heavily in constitutional design. Minimization of total (decision-making plus external) costs will require that amendment decisions are more costly than those of ordinary politics. Entrenchment, then, is consistent with efficient governance.

Relatedly, scholars emphasize that special interests can capture political processes (e.g., Stigler 1971; Peltzman 1976) leading to "institutional sclerosis" that inhibits and economy's growth (Olson 1982). Anticipating those scholars two centuries earlier, James Madison argued that

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constitutional design must guard against capture by special interests – what he referred to as "factions".[1] Students of constitutional political economy (CPE) have echoed Madison, directing attention to "the design of effective rules to [...] facilitate government production that benefits the general population rather than concentrated special interests" (Holcombe 2018, p. 1).[2] Capture is not only a concern *given* a constitutional framework; it can occur during constitutional drafting *or during subsequent constitutional amendment*.[3] Entrenchment can help to ensure that special interests cannot drive constitutional change by erecting barriers to self-serving amendments (Buchanan and Tullock 1962; Persson et al. 1997; Aghion and Bolton 2003; Ginsburg and Posner 2010).

Lastly, entrenchment can be viewed as a positive for economic performance because it promotes time-consistent policies and the benefits that go along with them (North and Weingast 1989; North 1991; Weingast 1993, 1995; Frye and Shleifer 1997; Frye 2004; Kydland and Prescott 1977). Time-consistent policies embody credible commitments – in particular, regarding the nature of property rights – that decrease uncertainty and create incentives for citizens to pursue forward-looking productive investments. Relatedly, when contemplating constitutional amendments, entrenchment promotes rational deliberation rather than heat-of-the-moment political passions (Holmes 1995; Hayek 1960).

Alternatively, there may be reasons to think that entrenchment is *bad* for economic performance. A recent paper by Versteeg and Zackin (2016) contrasts *entrenched/spare* versus *unentrenched/specific* models of constitutional design. Constraints and prescriptions based on the

[1] See *The Federalist Papers* No. 10 (Hamilton et al. 2003 [1788], pp. 72-75).
[2] This would be governance consistent with a generality norm (Buchanan and Congelton 2003 [1998]); Congelton 2004). Salter and Young (2019) specifically emphasize constitutional arrangements under which subsequent constitutional *change* will be consistent with a generality norm.
[3] Brice et al. (2019) report evidence that US states with significant resource endowments adopted longer constitutions with more explicit references to explicit industries. Tsebelis and Nardi (2014) report cross-country evidence that longer constitutions are associated with more corruption and discuss how this might arise from special interests capturing constitutional drafting (p. 472).

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former model will be relatively few, broad, and entrenched; those based on the latter model will be numerous, detailed, and subject to frequent amendment.[4] Versteeg and Zackin note that the two models provide different solutions to the same agency problem: misalignment of incentives between governance providers and the governed. Consistent with the discussion above, entrenched/spare constitutions attempt to mitigate the problem by providing the former with strict constraints that they cannot amend in their own interests. In contrast, unentrenched/specific constitutions allow the governed "ongoing constitutional micromanagement" of their agents (p. 658); they can readily adapt the rules of governance provision to fit the changing economic environment. If this approach is the more effective one, then a less entrenched constitution will be growth-enhancing.

In regard to economic performance, whether one favors the entrenched/spare model or its unentrenched/specific alternative may turn on epistemological concerns. Hayek (1937, 1945, 1960) argues that centralized governance providers will be unable to effectively utilize the dispersed knowledge that would be necessary to manage an economy. Those who find this argument convincing are likely to discourage a lot of specific, discretionary action on the part of governance providers. Presumably, the governed would face similar knowledge problems in attempting to "micromanage" the economic managers via frequent constitutional amendments. From this point of view, broad and durable constitutional constraints on governance providers are better.[5]

Of course, it may be the case that constitutional rigidity does not matter for growth. Many scholars are skeptical regarding the relevance of what Madison referred to as mere "parchment barriers". For such scholars, *de jure* constitutions only appear meaningful to the extent that they

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[4] The entrenched-vs.-unentrenched and spare-vs.-specific dimensions are distinct but also fundamentally related. More detailed constitutional prescriptions/constraints are more likely to become obsolete or inappropriate as time passes and circumstances change; hence, a greater need for them to be revisited.

[5] Hayek's emphasis of knowledge problems is fundamental to his own thinking on constitutions. See Hayek (1960) and the discussion by Martin and Wenzel (2020).

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reflect *de facto* conventions that are already (at least potentially) self-enforcing. Salter and Furton (2018, p. 38) provide a concise statement of this view:

> A written constitution can specify which set of rules, from a much larger set of feasible rules, will operate[. ...] But this is very different than the intended, and broadly agreed upon, utility of *de jure* constitutions, which is as mechanisms for binding or constraining. *De jure* constitutions cannot perform this role [...].[6]

The most compelling counterarguments involve the role of constitutions as coordination devices (e.g., Hardin 1989; Ordershook 1992; Weingast 2005, 1997; Hadfield and Weingast 2014). Accepting these arguments, constitutions may play an important role in determining *which* feasible conventions/rules are coordinated on (Young 2019). Still, unless drafters actually understand and acknowledge this role, it is unclear that constitutions will actually reflect feasible conventions/rules.

In exploring whether constitutional rigidity matters for growth, our paper contributes to a more general (though fairly small) literature on whether (*de jure*) constitutions matter for economic outcomes. For example, longer constitutions have been linked to more corruption in cross-country data (Montenegro 1995; Tsebelis and Nardi 2016; Tsebelis 2017).[7] Dove and Young (2019) report that more entrenched, shorter constitutions were associated with lower likelihood of default across nineteenth century US states. Eicher et al. (2018) find that constitutional executive constraints are positively associated social infrastructure. Minkler and Prakash (2017) report that stronger

[6] Relatedly, Caruso et al. (2012) argue that the degree to which formal constitutional rules affect ordinary politics depends on the level of *institutionalization*, by which they mean the degree to which agents expect political action to take place within a binding framework of political rules. Voigt et al. (2015) studies judicial independence and, to our knowledge, is the only study that attempts an empirical horserace between indicators of its *de jure* versus purely *de facto* relevance in economic growth regressions. (They find that only *de facto* judicial independence matters.) Voigt (1999) argues that when judicial independence leads to *de facto* constitutional change, the *de jure* constitution will be less of a constraint than the preferences of other governance bodies and the citizenry.

[7] It is unclear that these studies are identifying a causal effect of length. For example, Bjørnskov and Voigt (2014) argue and provide evidence that constitutional length is negatively related to social trust; the latter might be the relevant causal determinant of incomes and corruption.

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constitutional language on economic and social rights is negatively linked to poverty rates. Alternatively, Bjørnskov and Mchangama (2019) find that the inclusion of such rights in a constitution is unrelated to many social outcomes and negatively related to others (e.g., educational and health outcomes; de facto civil rights). Other studies have reported significant correlations between specific constitutional provisions and economic outcomes.[8]

The correlations reported in the above-mentioned studies are intriguing (and often conditional on a large number of relevant controls). However, none of those studies employ the sorts of causal inference techniques that have been associated with a "credibility revolution" in empirical research (Angrist and Pischke 2010). Making causal inferences plausible is of clear importance in present context. Constitutional design may promote governance that is more or less conducive to economic growth; however, whether or not a society is experiencing robust economic growth is also likely to influence the design of a new constitution (and, for that matter, whether or not a new constitution is proposed/adopted); also, there will inevitably be many variables that are important determinants of both constitutional design and economic growth that are unobservable to the researcher.

Ideally, we would like to (i) identify the adoptions of new constitutions that are meaningfully more rigid than their predecessors and then (ii) construct plausible counterfactuals against which to compare subsequent economic growth. We follow Hausmann et al. (2005) and Grier and Grier (2020) in employing matching methods. Matching methods are ideally suited to these tasks. We first identify countries that adopted new constitutions with higher procedural hurdles to amendment. These are "treated" countries. Using a sample of countries that adopted new constitutions, we then use a range of covariates to predict how likely each country was to have received the treatment.

[8] Voigt (2011) reviews the earlier empirical literature; see his table 2 for a summary list of studies and their findings.

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Treated countries are matched to non-treated countries, the latter which were similarly likely to adopted more rigid constitutions. The latter provide the counterfactuals against which we can reckon the causal effect of greater entrenchment on subsequent economic growth.

In **figure 1** we report plots of annual real GDP growth rates for countries that we identify as "treated" in the sense that they adopted meaningfully more rigid constitutions. (See section 3.1 below.) Vertical lines indicate the treatment dates; 10-year pre- and post-treatment growth rates are plotted. This collection of countries is a mix of richer and poorer, democratic and autocratic, countries. Eyeballing the plots, there is no clear indication that rigidity is either positively or negatively related to growth. One may take this to indicate that constitutional entrenchment has no relationship to economic growth, despite scholarly debates to the contrary. However, it may also indicate that the problems of identification alluded to above loom large. Our employment of matching methods aims to shed some light on these competing interpretations.

When examining the full set of available treatments, we find little evidence that rigidity matters for economic growth. Whether we are considering post-treatment growth over 5 or 10 years, the estimated effects of a more rigid constitution are generally small and statistically insignificant. However, when we examine a subsample that excludes autocracies, post-treatment effects are always negative and sometimes statistically significant. The same is true when we exclude treatments associated with coups; particularly for the 10-year horizon, for which most of the estimates are significant. Contrary to many scholars' priors, there is evidence to suggest that greater entrenchment harms growth in countries that are relatively democratic and politically stable.

The treatment sets we study unavoidably represent the experiences of a relatively small number of countries; as such, we are cautious about generalizing based on the results. However, the counterfactuals are constructed based on information from a much larger and globally representative sample of countries. Furthermore, the treatment sets represent cases where adoption of a

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meaningfully more rigid constitution is actually observable in cross-country data. Notwithstanding concerns regarding generalization, these cases need to be a starting point for evaluating whether rigidity matters for growth.

We proceed as follows. In section 2 we elaborate on the matching methods that we employ to identify the effect of constitutional entrenchment on per capita income growth. Then we discuss the data on which our analysis is based in section 3. The results of that analysis are reported in section 4. We make concluding remarks in section 5.

2. Matching Methods

Our goal is to identify the causal effect of constitutional rigidity on economic growth. There are two difficulties faced in attempting such an identification. First, we are concerned about selection bias. Countries that adopt meaningfully more rigid constitutions are not randomly selected; in particular, they may be likely to receive that treatment because of characteristics that are also correlated with economic growth. Second, we also have general concerns about endogeneity: growth might be a determinant of rigidity (rather than the other way around); and there may be omitted variables that are determinants of both growth and rigidity.

Due to the above concerns, standard regression analysis is unlikely to provide reliable, unbiased estimates of the effect of rigidity on growth. Instead we employ matching methods. We identify new constitutional adoptions that represent meaningful increases in rigidity over their predecessors. Those increases are defined as "treatments". For each country receiving a treatment, we create a counterfactual that is as similar as possible along a chosen set of dimensions. That set is chosen to be relevant to both the likelihood of receiving treatment (i.e., adopting a meaningfully more rigid constitution) and the outcome of interest.

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Matching methods are designed to mitigate concerns about selection bias (Rosembaum and Rubin 1983). Each treated country is matched to a set of non-treated countries that are as similar as possible in terms of how likely they were to have received the treatment. We are then comparing post-treatment growth between countries that were, in that sense, similarly selected. We estimate the average treatment effect on the treated (ATET): the difference between average income growth of treated countries and the average income growth of the matched controls.[9]

Regarding general concerns about endogeneity, we focus our analysis on estimating post-treatment *changes* in (log) real GDP per capita. By doing so, we difference-out any time-invariant heterogeneity. Similar to a panel data regression model with country fixed effects, this mitigates concerns regarding omitted variable bias.[10] Furthermore, one of the covariates that we match on is the initial (i.e., time of treatment) income per capita level. Thus, our counterfactuals are similarly likely to have received the treatment *as determined in part by economic performance*. This mitigates simultaneity concerns.

While an analogy was made above to a fixed effects regression model, with matching we are employing an alternative to the two-way fixed effects (TWFE) model with a dummy treatment variable. The TWFE model has become somewhat of the default when seeking causal inferences from panel data. The dummy variable allows staggered adoption of the treatment and it has been claimed that the estimated coefficient on that dummy is equivalent to the difference-in-differences (DID) estimator (e.g., Borusyak and Jaravel 2017; Athey and Imbens 2018; Abraham and Sun 2020).

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[9] More formally, we need some treatment indicator, observed where we can assign a value of 1 (yes, treated) or 0 otherwise. Then we need to assume that there is an expectation for an outcome conditional on the treatment indicator being 1 (as opposed to 0), as well as other variables. The additional conditionality is based on covariate values. As discussed in section 3.3 below, we include a broad set of potential covariates. The most important omitted ones are likely to be those idiosyncratic to individual countries and variable over time. By differencing the outcome variable, we hopefully mitigate the associated concerns.
[10] In doing so, we follow Grier and Grier (2020) who identify large, sustained increases in the Fraser Institute's Economic Freedom of the World (EFW) scores and, defining them as treatments, employ matching methods to estimate the post-treatment effect on economic growth.

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However, this turns out to not be the case; in particular, if there is any heterogeneity in the treatment effect over time, the estimate of it will be biased (Goodman-Bacon 2018; de Chasemartin and D'Haultfoeuille 2019). Since matching methods estimate an *average* treatment effect, even with such heterogeneity the estimate will be unbiased.[11]

We note, though, that using differences as the dependent variable in regression analysis removes between-country variation and, therefore, utilizes only within-country variation. However, this is not similarly a concern in the present context for two reasons. First, the within-country variation is what we want to focus on here. Constitutions change infrequently (relative to the 5-year and 10-year outcomes that we analyze); and we are interested in knowing, within a given country, whether adopting a more rigid constitution has effects on growth. Second, it is important to note that employment of matching methods is not regression analysis. Matching methods create a counterfactual based on comparing treated and non-treated countries in terms of pre-treatment covariate values: this implicitly takes into account within-country variation by matching to treated countries to those that are as similar as possible, *and then* comparing post-treatment changes.[12]

2.1 Propensity Score Matching

We employ two types of matching methods in this paper: *propensity score matching* and *matching by Mahalanobis distance*. The first of these is discussed in this subsection; the second in the following subsection 2.2.

[11] Matching methods can have other advantages over regression analysis. For example, propensity score matching does not extrapolate to estimate the treatment effect. When there are no non-treated units with scores sufficiently close to that of a treated unit, the latter is not included in the analysis. This restricts attention to the region of common support. (Propensity score matching is one of the methods employed in this paper.) Also, matching methods in general are less sensitive to the choice of functional form.
[12] We also note that there are two countries (Democratic Republic of Congo and Ecuador) for which we have two treatments each. This further mitigates the concern that within-country variation is not considered.

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With propensity score matching (PSM), we estimate a logit model of the probability that treatment occurs conditional on a set of covariates. After estimating the model, each country in our sample can be assigned a *propensity score* which is its estimated probability of receiving the treatment. Each treated country is then matched to a set of non-treated countries that have similar propensity scores. We will refer to a country's "neighbors" in terms of the difference between their propensity scores and that of the treated country. A country's "first nearest neighbor", then, is the other country in the sample that has the closest propensity score. (A country's "second nearest neighbor" is the other country with the second closest propensity score, etc.)

Using PSM, we report results where matching is based on (i) nearest (non-treated) neighbor, (ii) the average of the two nearest neighbors, (iii) the average of the three nearest neighbors, or (iv) matching using a normal kernel function (which uses all of the neighbors but gives larger weight to those with closer propensity scores).

2.2 Matching by Mahalanobis Distance

An alternative to matching based on propensity scores is to match directly based on the covariates. With multiple covariates that are continuous variables, exact matching (i.e., where treated and non-treated countries have equal values) is not feasible. However, the Mahalanobis distance metric provides a means for inexact covariate matching. This is metric is the Euclidian distance between the covariate vectors of two countries, adjusted for their covariance matrix.

Using the Mahalanobis distance metric, treated countries are matched according to one of the following criteria: (i) nearest (non-treated) neighbor; (ii) the average of the two nearest neighbors; (iii) the average of the three nearest neighbors.

3. Data

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The data used in this paper is divided into three categories: treatment, outcome, and covariates. We want to examine if substantially increasing the difficulty of amending constitutions leads to greater economic growth. Doing so requires a measure of constitutional rigidity. We define a treatment as the adoption of a new constitution that is meaningfully more rigid than its predecessor. The outcome of interest is post-treatment growth (i.e., change in the natural log of real GDP per capita). Lastly, we require covariates to match treated units to their non-treated controls.

3.1 Treatment: Constitutional Rigidity

We examine country-level constitutional adoptions from 1973 to 2017. The underlying data on those constitutions and their characteristics is from the Comparative Constitutions Project (CCP).[13] We employ the measure of constitutional rigidity constructed by Ginsburg and Melton (2015) (henceforth "GM"). In the spirit of Lutz's (1994) classic contribution, GM exploit information contained both in the empirical amendment rates of constitutions and in the procedures for amendment that are designed into them.

The amendment rate of a constitution is defined as the number of years in which it is amended divided by the total number of years that it has existed.[14] For the entire sample of constitutions represented in the CCP data, GM regress amendment rates on a set of variables that code amendment procedures and also numerous additional controls that are considered predictors of political reform generally (p. 695). The procedural variables are designed to reflect (i) vote thresholds for approval in the legislature; (ii) the number and type of proposers; and (iii) the number

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[13] The Comparative Constitutions Project (www.comparativeconstitutionsproject.org) data is described in Elkins et al. (2009).

[14] This represents a departure from Lutz (1994) who considers the total number of amendments divided by the number of years a constitution has existed. Ginsburg and Melton's choice is "based on a belief that the primary difficulty in amending a constitution is finding a coalition willing to pass the amendment[; o]nce the constitution is amended once, such a coalition is identified and subsequent amendments are easier to promulgate" (p. 695).

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and type of bodies involved in approval.[15] The rigidity measure is then a linear function of the procedural variables where the weights are the corresponding regression coefficients. (The function is normalized such that all constitutions have values in the range of 0 to 1.)

Intuitively, there are various dimensions of procedural rigidity (i.e., aspects of (i), (ii), and (iii)) and they can manifest in various combinations for any given constitution. The GM approach estimates, using the entire CCP sample, their relative roles in predicting how entrenched a constitution will actually be over time. The resulting estimates determine the relative weights of those various dimensions in a one-dimensional measure of constitutional rigidity.

Recall that we are defining a treatment as the adoption of a constitution that is meaningfully more rigid than its predecessor. In the context of matching methods, GM's approach might raise the concern that treatment at time t is defined in terms of events occurring at $t + 1$ and beyond. This is generally impermissible. (As the name implies, a post-treatment effect cannot be conceived of while the treatment is still occurring.) For example, for a constitution adopted in 1980, we cannot consider its *ex post* amendment rate as a treatment occurring in 1980.

However, this concern is not valid for the GM rigidity measure. Note that the rigidity of a constitution is not calculated based on *its own* amendment rate. Rather, the amendment rates of *all* constitutions are used together to estimate the weights that will be placed on procedural variables. Those weights are the same for all constitutions assigned a rigidity value. Given those weights, the rigidity of any constitution is a function of solely its amendment procedure variables. The values of the amendment procedure variables are, of course, determined at the moment of constitutional adoption.

15 For details on the regression model see the replication files for Elkins et al. (2009) (http://comparativeconstitutionsproject.org/data/endurance_of_constitutions.zip) and, in particular, the "ar_model_estimates.pdf" file.

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What constitutes a *meaningful* increase in rigidity? Our baseline definition of a treatment will be an increase of 0.128 in the GM measure.[16] Given the distribution of values in our sample, 0.128 is equal to 20% of the mean value (0.642). Based on this threshold, 19 treatments are included in our baseline estimations of 5-year post-treatment effects; 18 treatments for 10-year effects.[17] In **table 1** we report the treated countries, the size of the treatment (i.e., the increase in the GM rigidity measure), and the year in which the treatment occurred.

To check the sensitivity of results to the choice of treatment threshold, we also consider increases in rigidity of 0.176 or greater. (An increase in 0.176 is one half of a standard deviation from our sample.) This decreases our number of treatments to 16 for 5-year estimations and 15 for 10-year estimations.

Based on either threshold (0.128 or 0.176) we are obviously dealing with a treatment set that represents the experiences of a relatively small number of countries during particular times and circumstances. While the counterfactuals are created based on information contained in the global donor pool, the fact remains that results will be driven by at most 19 individual cases where meaningfully more rigid constitutions were adopted. However, if we want to take hypotheses regarding constitutional entrenchment seriously and subject them to the data, those *are* the available cases to analyze. While we cannot claim that the treatment experiences generalize, drawing on the much broader sample of countries to construct counterfactuals is about the best we can do.

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[16] We originally planned on running a similar analysis for countries with decreases in rigidity of 0.128 or larger. However, there were only 10 such cases and all but 2 of those constitutions were subsequently replaced before 5 years had passed.

[17] Ecuador adopted a meaningfully more rigid constitution in 1978 but then an even *more* rigid constitution in 1984. Since the 1984 constitution was adopted only 6 years later, the 1978 treatment can only be used in the 5-year estimations. There were also several increases in rigidity (above the 0.128 threshold) that had to be excluded entirely; these were: (1) Brazil's 1988 constitution because we did not have educational enrollment observations; (2) Thailand's 1968 constitution because it did not last for 5 years; (3) the Republic of Vietnam's 1967 constitution because that country ceased to exist in 1975 and is not included in the PWT data; (4) Yemen's 1970 constitution because PWT data for that country does not exist pre-1989; and (5) Yugoslavia's 1992 constitution because PWT data for that country is absent entirely. This jump threshold excluded twenty countries from being treated. All but three of these cases had increases of less than 0.100.

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3.2 Outcome: Economic Growth

We gather real GDP per capita data from the Penn World Table (version 9.1; henceforth "PWT"; Feenstra et al. 2015). We take the natural log of these values. Log differences over time are then (approximately) cumulative growth rates. We consider post-treatment cumulative growth rates over both 5-year and 10-year horizons.

As noted in section 2 above, by considering post-treatment *changes* in (log) real GDP per capita we are differencing out any time-invariant uncontrolled-for heterogeneity from the empirical model. In this way, we are mitigating omitted variable bias (similar to including country fixed effects in a panel regression).

3.3 Covariates

With matching, one wants to choose covariates that correlate with the outcome and/or determine the probability of receiving the treatment. Accounting for the correlates of an outcome is standard in all empirical methods. Alternatively, determinants of the treatment probability are fundamental to matching methods specifically. As discussed in section 2, the idea is to match each treated country to one or more non-treated countries that were *similarly likely to have received the treatment*. By doing so, we gain confidence that any difference in post-treatment outcomes between the treated country and its matches are the effect of the treatment.

There are ten covariates employed in our analysis. To begin with, we include five indicators of the economic environment in each country. The first of these is the level of real GDP per capita. A country's average income level may both help to determine what type of constitutional design is chosen and also correlate with the subsequent rate of income growth. For similar reasons, the other included indicators of the economic environment are the investment, export, and government

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consumption share of GDP. The covariates are all drawn from the PWT. The last economic environment indicator that we include is the inflation rate (based on the GDP deflator) which is drawn from the World Bank's World Development Indicators (WDI).[18]

Regarding the real GDP per capita covariate, we also note that there is a large regression-based literature on income convergence.[19] Much of this literature finds that, all else equal, a country's growth rate will vary inversely in relation to its initial income level. By including real GDP per capita as a covariate in our matching analyses, we are accounting for this type of effect: we examine countries that adopted a meaningfully longer constitution (i.e., the treatment) and compare their post-treatment economic growth to the growth experiences of countries that are similar in terms of their covariate values, *including the income levels that they started from*.

A country's political environment will likely factor into constitutional design and its probability of receiving a treatment. We include Freedom House's political rights and civil liberties measures.[20] Both of these measures are indices ranging from 1 through 7, with the higher values corresponding to worse rights and liberties.[21] The human capital of a country's population is also may also be important to account for. We include the WDI indicators for both primary and secondary education gross enrollment rates.[22]

Finally, we include the rigidity of the prior constitution as a covariate. When a new constitution is designed, whether or not it represents a meaningful increase in rigidity will be dependent on how rigid its predecessor was. This is true in both a tautological sense (because one

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[18] For Lithuania's 1992 constitutional episode, we use the consumer price index (CPI) inflation rate (also from the WDI) due to lack of available GDP deflator data for that country.
[19] For overviews of this literature and the underlying theory of the convergence hypothesis, see Young et al. (2008) and Johnson and Papageorgiou (2020).
[20] https://freedomhouse.org/report/freedom-world
[21] As a robustness check, we replace these political proxies for Polity IV's Polity2 score.
[22] We use these two variables instead of PWT's human capital index due to the WDIs having greater coverage with their enrollment measures.

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half of an increase in rigidity is the initial value), but also because perceptions of the virtues and vices of current constitutional design will certainly inform the drafting of its successor. Furthermore, trends in economic growth are persistent; since we hypothesizing that constitutional rigidity may be a determinant of growth, it makes sense to account for its pre-treatment level.

Summary statistics for all variables discussed above are reported in **table 2**.

4. Results

We will be primarily concerned with results based on matching methods. However, we begin this section by reporting two-way fixed effects (TWFE) regression results, where economic growth rates are related to constitutional rigidity. These are reported in **table 3**. Recall that TWFE regressions assume that the marginal effect of rigidity on growth is continuous, and that the effects over time are homogeneous (see section 2 above). Both of these assumptions are highly questionable.[23] Still, TWFE results serve as a useful benchmark against which to compare those based on matching methods.

We report four TWFE regressions: two are based on 5-year economic growth as the dependent variable, and two are based on 10-year growth; then for each of those cases, results are reported with and then without covariates. All point estimates on rigidity are negative; they always statistically significant when covariates are included. (Only for the 5-year horizon without control variables does rigidity enter insignificantly.) And the estimated effect is quantitatively meaningful. (Based on the estimates from column 4, a standard deviation increase in rigidity is associated with a

[23] The former amounts to assuming that, e.g., moving from a 75% legislature approval threshold to one of 76% makes a difference; it also amounts to assuming that one (say, legislative) burden when combined with another (say, referendum-based) burden can be translated into a clear marginal effect. These assumptions are questionable in principle. Additionally, the very construction of the rigidity variable (see section 3.1 above) calls the assumptions into question in an applied context.

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decrease in 10-year growth of 0.118, compared to the 0.342 standard deviation of 10-year growth in our sample.)

The TWFE results go against the priors of many scholars: they suggest that a more entrenched constitution is bad for economic growth. However, we are not confident to draw causal inferences from the TWFE results. Therefore, we move on to results based on matching methods.

To begin with, we report on logit estimations of the probability of receiving treatment. The results are the basis for calculating propensity scores and are reported in **table 4**. Column 1 contains results based on treatments defined as an increase in constitutional rigidity ≥ 0.128. (These are the benchmark set of 19 treatments reported in **table 1**.) Column 2 contains results based on treatments defined by the higher ≥ 0.176 threshold. The results are broadly similar across columns 1 and 2. Unsurprisingly, the rigidity of the preceding constitution enters negatively and significantly. (To the extent that a constitution is relatively rigid to begin with, it less likely to be replaced by an *even more* rigid one.) The initial government share of GDP is positively and significantly linked to the probability of treatment.[24]

4.1 Benchmark Results

In **table 5** we report estimated average post-treatment effects based on the benchmark set of treatments.[25] Estimates are based on the four types of PSM matching and three types of Mahalanobis distance-based matching described in section 2; they are reported for both 5-year and 10-year growth effects. We find no statistically significant evidence of constitutional entrenchment

[24] This could reflect that societies with larger governments, all else equal, are more likely to seek more rigid constraints on them (though this is just conjecture). Additionally in **table 4**, initial (log) GDP per capita enters negatively and significantly for the benchmark treatment definition (column 1). (It just misses the 10% statistical significance level for the larger treatment threshold.)

[25] As a point of comparison, we also run a TWFE model with our covariates. These are made available upon request (**Table B1**). We find the rigidity variable to be negative and statistically significant when examining both 5-year and 10-year growth rates as the dependent variable.

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affecting growth. The point estimates are split between positive (4) and negative (10), with all but one of the 10-year point estimates being negative.

Quantitatively, the largest (in absolute value) 5-year point estimate is a -0.078 (or 7.8% cumulative). That would be a decrease in the growth rate of approximately 1.56 percentage points annually. Though this point estimate is just over one third of a standard deviation of the 5-year growth rate (0.213; **table 2**), 1.56 percentage points annually is economically meaningful. However, most of the 5-year point estimates are considerably smaller. (The PSM nearest three neighbors estimate is -0.060 but all of the rest are smaller than 0.025 in absolute value; that is, less than 0.50 percentage points annually.) Regarding the 10-year point estimates, the largest point estimate is -0.110. (The largest 5-year and 10-year point estimates are both for the PSM nearest 2 neighbors specification.) This is approximately 1.1 percentage points annually but, again, most of the point estimates are considerably smaller.[26]

To summarize the **table 5** benchmark results, (1) the estimated effects of constitutional rigidity on economic growth are never statistically significant; (2) the signs of the point estimates are inconsistent across specifications; (3) in most cases, the estimated effects are small. There is not much evidence to suggest that rigidity matters.

Along with estimated ATET, we report Chi-square test statistics for the PSM estimations ("Cov. Balance" in **table 5**).[27] The null hypothesis of the Chi-square test is that the covariates are on average balanced between treated countries and their matches. Balance implies that the treated and matched countries are reasonably similar in terms of covariate values. A lack of balance can introduce bias into the estimates. When the null is not rejected, there is no evidence to suggest that

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[26] We also run the analysis using the benchmark treatment definition but replacing the Freedom House variables with Polity IV's Polity2 score. Doing so restricts our analysis to 17 treated units but does not change the ATET results meaningfully. We still find statistically insignificant results under each matching method for both 5-year and 10-year growth rates. These are reported in the appendix table A1.
[27] These tests are not available for Mahalanobis distance-based covariate matching.

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the estimates are biased. There is only one case here (PSM nearest neighbor; 10-year) where the null is rejected (10% level).

To reiterate a caveat, the results are ultimately driven by only 19 particular cases of constitutional adoption. Generalizing based on these results is hazardous. However, those 19 cases are the only observable instances where a meaningfully more rigid (based on the 0.128 or greater threshold) constitution was adopted. Using these cases, the evidence is consistent with constitutional rigidity not being significantly related to growth. Below, we report on whether certain changes to the threshold, treatment set, and/or covariate set affect this finding.

4.2 Higher Threshold for Treatment; Longer Time Horizon

In **table 6** we report results based on the higher (≥ 0.176) threshold for classifying rigidity increases as treatments.[28] The results are very similar to when using the benchmark threshold. (Estimated effects are never statistically significant; the signs of the point estimates are inconsistent; in most cases, the size of the estimated effects are small.) The Chi-square tests almost never reject the null. (The one exception is again the 10-year PSM nearest neighbor estimation.)

In addition to the threshold for defining treatments, another concern is that even a 10-year horizon may not pass as the "long-run" in this context. Reported in table A2 of the appendix are results based on a 20-year horizon for per capita income growth. There are 14 treatments available for this. There are no statistically significant effects reported and all of the point estimates are negative.

4.3 Excluding Autocracies

[28] For this subsample, as well as all of the others in this paper, we run a new logit equation that includes just treated and control countries that fit into the respective subsample criteria.

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Tsebelis and Nardi (2014, p. 459) emphasize that empirical studies of constitutions should "focus on constitutional systems in which the text of the document does in fact regulate political practice." We attempt to address this point by reporting estimates when excluding autocracies from our sample. Specifically, in terms of both potential treated countries and potential matches, we exclude countries with a Polity2 score of -5 or less. Doing so is quite costly in the sense that it drops our number of treatments down to 13 (for both 5-year and 10-year estimations; this is based on the benchmark ≥ 0.128 threshold.) This is a decrease of almost one third of the treatments.

The results are reported in **table 7** and are intriguing. When autocracies are excluded, all point estimates suggest that adopting a meaningfully more rigid constitution is bad for economic growth, regardless of whether we are considering the 5-year or 10-year horizon. Second, half of those estimates are statistically significant (10% level or better; whereas this was not true of a single estimate reported in **tables 5** and **6**.) Focusing on the statistically significant 10-year effects, the point estimates range from -0.219 to -0.161. The sample standard deviation for 10-year growth is about 0.342 (**table 2**), so these would be sizeable negative effects.[29]

4.4 Excluding OECD Countries or "Almost-Treated" Countries

Excluding autocracies might be reasonable because we do not want to emphasize countries where the *de jure* constitutions are mere "parchment barriers" to rulers. From another perspective,

[29] For the 10-year horizon, all of the Mahalanobis estimates are significant while only one of the PSM estimates is (normal kernel). The Chi-square tests reported in **table 6** do not suggest that covariates are unbalanced in any of PSM estimations, hence no clear reason to prefer certain specifications relative to others. In the appendix (tables A3-A6), we report individual covariate difference-in-means tests for treated countries versus (i) matched countries and (ii) unmatched countries. For each of the PSM estimations, similarity in means for treated countries and unmatched other countries is rejected (5% level or better) for six covariates ("Rigidity", "GPD per cap (log)", "Government Share", "Inflation (5-year average)", "Civil Liberties", and "Political Freedom"). In all of those cases save one ("Political Freedom"; nearest neighbor; null rejected at the 10% level) there is no evidence of different means for treated countries and their matches. Again, this suggests that covariate balance is essentially good across the PSM specifications, giving us no reason to prefer one of them relative to another.

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though, it might be the case that *de jure* constitutions - or, more relevantly, changes in them - are not particularly relevant when *de facto* norms and conventions are solidly and independently in place. To explore this possibility, we report estimates when OECD countries are excluded. The OECD, in principle, only extends membership to countries that have already firmly embraced democracy, rule of law, and civil rights. As such, *de jure* constitutional changes in OECD countries might be less meaningful. The results are very similar to the benchmark results.[30]

As an additional robustness check, we report results based on excluding countries that "almost" received the treatment from the control group of potential matches. The motivating concern is that we may be matching countries that adopted meaningfully more rigid constitutions to countries that also adopted more rigid constitutions, but just short of the ≥ 0.128 threshold. This would not make for the meaningful counterfactuals that we seek. When we report results based on excluding all non-treated countries that experienced a jump in rigidity ≥ 0.05, the results are, again, very similar to the benchmark results.[31]

4.5 Excluding Economically Free Countries

In this subsection we report on one additional robustness check that involves excluding countries with relatively high levels of economic freedom from the sample. To do this, we employ the Fraser Institute's Economic Freedom of the World (EFW) country scores (Gwartney et al. 2019). EFW scores are designed to measure "the degree to which the policies and institutions of countries are supportive of economic freedom [...] the cornerstones of [which] are personal choice,

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[30] For the sake of space, results reported from here on are included in an Appendix B, which is available upon request from the authors. See table B1 in Appendix B. The number of treatments here is 17 for 5-year estimations and 16 for 10-year estimations. Finland and Turkey are the only two treated units dropped in this analysis.

[31] See table B2 in Appendix B. The number of treatments here is the same as for the benchmark case (since we are only dropping control countries).

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voluntary exchange, freedom to enter markets and compete, and security of the person and privately owned property" (p. v). There is considerable evidence that economic freedom, conceived of as such, is associated with desirable economic outcomes (e.g., income levels and economic growth; see Hall and Lawson 2014).

For countries that have traditionally had "good" policy and institutional environments, it is possible that adopting a more rigid constitution is of little import. To the extent that constitutions matter because they constrain political agents from acting contrary to the general interests of citizenry, the governments of economically free countries are already (for whatever reasons) *de facto* limited; as such, adopting a more rigid *de jure* constitution may not matter. Therefore, we want to check how results change when those economically free countries are excluded.

EFW scores are reported on a scale of 0 to 10, with 10 presenting most economically free. For 1970, the first year for which scores are reported, the average country score is about a 6; in the most recent year, 2017, it is about a 7. Based on this, we exclude all countries that, over 1970-2017, have an average EFW score of 6.5 or higher. This limits our treatments to 12 for the 5-year estimations and 11 for the 10-year estimations. (Countries with an average EFW score of 6.5 or higher are also excluded from the pool of potential matches.) However, none of the resulting estimates are statistically significant. Furthermore, the point estimates are close to evenly split in terms of their signs (8 being negative and 6 positive); they are generally small.[32]

4.6 *Excluding Constitutional Adoptions Associated with Coups*

Military coups are often associated with the subsequent adoption of a new constitution. This is a concern here because Blum and Gründler (2020) have recently provided evidence that coups

[32] See table B3 in Appendix B.

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reduce economic growth by 2-to-3 percentage points. To make sure that coups are not confounding the estimated effect of constitutional rigidity, we produce results when coups are excluded.[33]

Even though this leaves us with only 12 treatments, the results for the 10-year horizon are intriguing. All of the point estimates are negative and they are statistically significant in 5 out of 7 of the specifications. Recall that the standard deviation of 10-year growth in our sample is about 0.342 (**table 2**). The statistically significant point estimates range from -0.368 to -0.275, suggesting that the adoption of a meaningfully more rigid constitution causes around a standard deviation *decrease* in growth over the 10-year horizon. (The 5-year point estimates are also uniformly negative but all statistically insignificant.)

4.7 Smaller Covariate Set and Separating by Income Category

There are concerns that our model could be over-specified. Past studies have found, however, that this concern is not particularly worrisome for PSM since the logit equation is itself a means of achieving covariate balance rather than inference. (Put more mechanically, the propensity scores are based on only the logit point estimates; the variance of individual coefficient estimates plays no role in either calculating the ATETs or determining their statistical significance. Furthermore, in the case of Mahalanobis-based matching there are no analogs to the initial logit coefficient estimates; hence no variances to consider at that stage.)[34]

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[33] See table B4 in Appendix B. We exclude the treatments associated with Burkina Faso (1991 following a failed coup in 1989), Dem. Reup. of Congo (1978 following a successful coup in 1976), Iraq (2005 following the 2003 US invasion), Peru (1993 following the 1992 "self-coup" (*autogolpe*), Romania (1991 following the overthrow of Ceausescu in 1989) and Thailand (1991 following a successful coup). We also exclude the Lithuanian (1992) treatment because it followed independence from the USSR in 1990.

[34] The inclusion of irrelevant variables *in and of itself* will inflate standard errors on coefficient estimates, but it will not introduce bias into the estimates. However, Basu (2020) argues that when the exclusion of relevant variables has created the potential for bias, *then* the inclusion of irrelevant variables can affect the size of the bias. In determining whether or not to include additional covariates, this creates somewhat of a *damned if you do and damned if you don't* problem. Since omitted variables loom large in empirical political economy, we have chosen to make the benchmark based on the larger covariate set and then discuss results based on a more parsimonious set in this subsection.

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Nevertheless, it may be unwise to dismiss over-specification concerns out of hand. As such, we produce results using only five covariates: lagged rigidity score, GDP per capita (logged), government share, political freedom, and primary education (% gross). (The rigidity of the prior constitution and initial income level are a bare minimum; accounting for the size of government, political institutional quality, and the citizenry's general education level also *a priori* seem critical.) The results are generally null and consistent with our baseline results, with one exception: under PSM with first nearest neighbor, we report a negative and statistically significant effect of increases in rigidity on the 10-year growth rate.[35]

We also consider results based on separate income categories. The Solow growth model points out that differences in growth rates is largely due to initial levels of income. While matching on real GDP per capita levels mitigates concerns regarding convergence effects (see section 3.3 above) considering different income categories provides another approach. We start from the World Bank's categorizations of countries by income: (i) low, (ii) low-middle, (iii) high-middle, and (iv) high. Out of our 19 treated countries, four are low-income, eleven are middle-income, and four are high-income. We produce results based on two subsamples, the first of which (i) excludes high-income countries, and the second of which (ii) excludes high-income and low-income countries. Overall, the results do not change. The results are always statistically insignificant; the point estimates are sometimes positive and sometimes negative.[36]

5. Concluding Remarks

Should the prescriptions and constraints of constitutions be entrenched? This question has been debated by political scientists, constitutional lawyers, and economists. There are many directions

[35] See table B5 in Appendix B.
[36] See tables B6 and B7 in Appendix B.

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from which to approach the question; one of them is to ask if constitutional entrenchment helps or harms a society's economic performance. In this paper, we approach the matter empirically and seek to identify causal links between entrenchment and economic growth.

This is a case where empirical analysis is clearly called for. As Versteeg and Zackin (2016) have emphasized, entrenched/spare and unentrenched/detailed models of constitutional design offer different solutions to the same agency problems faced by citizens in regard to their governance providers. The former model attempts to place constraints on governance providers that ensure they act in the general interests of the citizenry; and it entrenches those constraints such that they cannot be subsequently amended and captured by special interests. Alternatively, the latter model allows the citizenry to undertake "ongoing constitutional micromanagement" of their governance providers. There are obvious tradeoffs between the two models and the relative net benefits cannot be determined *a priori*.

A problem with providing a satisfactory empirical analysis of this matter is the endogeneity of constitutional design. Economic performance can determine the type of constitutional structures that are adopted; furthermore, there are various other (often unobservable) factors that at play in determining both economic performance and constitutional design. To confront this problem, we have here employed matching methods to make causal inferences. We have compared episodes where countries adopted significantly more rigid constitutions (i.e., received a "treatment") and then compared their subsequent economic growth to a country or set of countries who were similarly likely to have done so (but did not). Hence, for each treated country we construct a plausible counterfactual against which to compare post-treatment economic performance.

When we consider the full set of available treatments, there is very little evidence to reject the null – that constitutional entrenchment is generally not related to subsequent economic growth. Alternatively, there is compelling evidence to reject that null when we consider subsamples that

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exclude either autocracies or constitutional adoptions that were associated with coups. When considering the 10-year growth horizon in particular, we report several negative and statistically significant post-treatment effects. Those effects are also economically meaningful. Constitutional rigidity may indeed matter for growth in countries that are relatively democratic and politically stable.

Overall, our results suggest that constitutional rigidity, if anything, has a negative effect on economic growth. This may be surprising given that entrenchment has been associated credible commitments and time-consistent policies, as well as rational deliberation (as opposed to heat-of-the-moment, knee-jerk decisions). Why might this be? First, it may be that the *unentrenched/specific* model of constitutional design provides for more effective governance for the reasons discussed by Versteeg and Zackin (2016). Unentrenched/spare constitutions facilitate "ongoing constitutional micromanagement" by the governed, and that makes governance appropriately flexible to the evolving economic environment. This may lead to more growth-enhancing governance.

However, another intriguing possibility is that larger governments are more likely to impose rigid constitutional constraints upon themselves. This hypothesis is consistent with the stylized fact that high state capacity and rule of law tend to go hand-in-hand (see Johnson and Koyama 2017). Also, in our logit estimations, used to construct propensity scores, we report that government size (i.e., expenditures-to-GDP) is positively and significantly associated with the probability of adopting a more rigid constitution. However, it is also the case that high state capacity, rule of law, and *wealth* go hand-in-hand-*in-hand* (also see Johnson and Koyama 2017). And our finding that entrenchment is, if anything, bad for growth, does not jive the third part of the stylized fact.

It is also worth noting that the high state capacity-rule of law coincidence is generally accounted for by credible commitments to non-predatory governance leading to (i.e., *prior to*) governments being able to build state capacity (e.g., North and Weingast 1989). Our results would

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be consistent with large governments leading to (via entrenchment) *lower* growth. There are obvious problems here. Perhaps it is truly the case that *de facto* are largely different than (and dominate) *de jure* constitutions (e.g., Salter and Furton 2018), and that large governments impose (acquiesce to?) more rigid constitutions because they do not bind. This is pure conjecture but, perhaps, fodder for future research.

What we do know is that the degree of entrenchment for constitutional prescriptions and constraints is a fundamental dimension of constitutional design. Debates about its desirability go back to at least Madison versus Jefferson. It is a "dominant theme of the constitutional theory literature [...] that successful constitutions must not only constrain those in power, but must do so over long time horizons [...]" (Versteeg and Zakin 2016, p. 657). Here we have offered the first plausible identification of the causal effects of entrenchment on growth. To be clear: there are various criteria by which to judge the desirability of entrenchment, and economic performance is just one of them.[37] However, the role of entrenchment in determining economic performance surely has weight in the calculus.

We hope this paper demonstrates that, given the rich data that has been assembled and made available by the Comparative Constitutions Project (CCP), the application of causal inference techniques stands to provide numerous, important insights into the role of constitutional design for economic outcomes. Empirical constitutional political economy has burgeoned during the last two decades (Voigt 2011). However, the employment of causal inference techniques – such as matching methods – has lagged behind that of other fields. We hope that this contribution motivates constitutional researchers forward in this and related directions.

[37] Thomas Jefferson, for instance, argued there is no ethical justification for the dead to bind the living. This paper is silent on the merits of such a view. Bellamy and Castiglione (1997) provide a useful discussion of various arguments for and against the entrenchment of constitutional provisions from democratic influences.

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There is certainly more work to do. We acknowledge that the treatment sets analyzed above represent no more than 19 constitutional adoption experiences. Concerns regarding whether results generalize loom large. Addressing those concerns may involve employing alternative econometric techniques that can leverage variation from a greater number of constitutional episodes; and/or it may involve the construction (or eventual availability) of data on more episodes of constitutional adoption. We leave such projects to future research.

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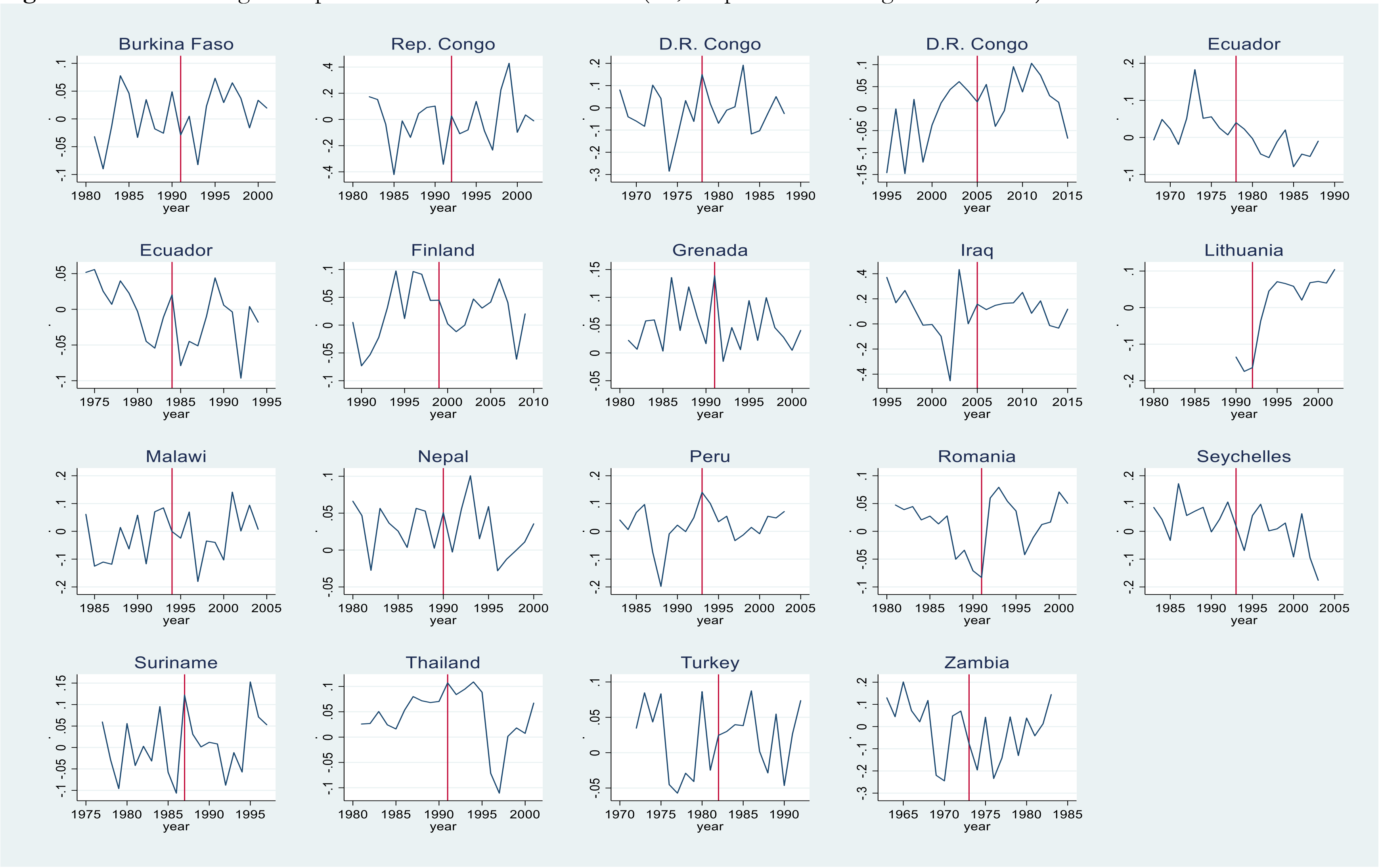
FIGURES

Figure 1: Annual GDP growth plotted over time with treatment (i.e., adoption of more rigid constitution) date indicated.



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TABLES

Table 1: Treatments in baseline estimations (adoption of a constitution with a ≥ 0.128 increase in rigidity over its predecessor).

Country	Year of Adoption	Increase	Country	Year of Adoption	Increase
Burkina Faso	1991	0.153	Malawi	1994	0.232
Congo, Republic	1992	0.168	Nepal	1990	0.347
Democratic Republic of Congo	1978	0.273	Peru	1993	0.419
Democratic Republic of Congo	2005	0.803	Romania	1991	0.407
Ecuador	1978	0.305	Seychelles	1993	0.547
Ecuador	1984	0.395	Suriname	1987	0.325
Finland	1999	0.251	Thailand	1991	0.282
Grenada	1991	0.373	Turkey	1982	0.136
Iraq	2005	0.384	Zambia	1973	0.300
Lithuania	1992	0.662			

Note: the constitutional rigidity measure is from Ginsburg and Melton (2015).

Table 2: Summary statistics.

Variable	Obs.	Mean	Std Dev	Min	Max
5-year growth (logged GDP per cap)	3328	0.114	0.213	-0.896	1.567
10-year growth (logged GDP per cap)	2847	0.230	0.342	-1.558	2.371
Rigidity	3328	0.617	0.355	0.000	1
GDP per cap (logged)	3328	8.934	1.158	6.193	12.342
Investment Share	3328	0.223	0.101	-0.102	0.718
Government Share	3328	0.199	0.093	0.017	0.804
Export Share	3328	0.239	0.208	0.002	1.376
Inflation (5-year average)	3328	39.867	308.340	-10.380	7016.733
Civil Liberties	3328	3.328	1.869	1	7
Political Freedom	3328	3.270	2.179	1	7
Primary Education (% gross)	3328	99.471	18.922	13.051	163.933
Secondary Education (% gross)	3328	67.706	31.962	1.397	161.019

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TABLES (Cont.)

Table 3: Effect of Rigidity on Economic Growth (Two-Way Fixed Effects Model)

Variables	(1) 5-year growth	(2) 5-year growth	(3) 10-year growth	(4) 10-year growth
Rigidity	-0.038	-0.181**	-0.190**	-0.332*
	(0.049)	(0.091)	(0.080)	(0.193)
GDP per cap (logged)		-0.315***		-0.698***
		(0.032)		(0.056)
Investment Share		-0.052		0.055
		(0.135)		(0.180)
Government Share		-0.373**		-0.287
		(0.143)		(0.243)
Export Share		0.186**		0.147
		(0.091)		(0.165)
Inflation (5-year avg.)		-0.000		-0.000*
		(0.000)		(0.000)
Civil Liberties		-0.008		-0.019
		(0.010)		(0.015)
Political Freedom		-0.002		-0.004
		(0.009)		(0.013)
Primary Ed. (% gross)		-0.000		-0.001
		(0.001)		(0.001)
Secondary Ed. (% gross)		0.000		-0.001
		(0.001)		(0.001)
Constant	0.126***	3.027***	0.244***	6.575***
	(0.038)	(0.300)	(0.060)	(0.504)
Observations	3,328	3,328	2,847	2,847
R-squared	0.081	0.260	0.095	0.457
Number of countries	127	127	127	127

Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Country and Year fixed effects included in each regression.

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TABLES (Cont.)

Table 4: Logit estimations: determination of treatment (i.e., adoption of constitution more rigid than its predecessor).

Variable	(1)	(2)
Rigidity	-2.867***	-3.465***
	(0.734)	(0.899)
GDP per cap (log)	-0.730*	-0.731
	(0.440)	(0.491)
Investment Share	3.569	4.282
	(2.526)	(2.932)
Government Share	5.620***	5.931***
	(1.924)	(2.146)
Export Share	-1.966	-3.062
	(1.677)	(2.023)
Inflation (5-year average)	0.0005	0.0005
	(0.0003)	(0.0004)
Civil Liberties	-0.377	-0.323
	(0.311)	(0.341)
Political Freedom	0.186	0.127
	(0.246)	(0.271)
Primary Education (% gross)	0.013	0.021
	(0.014)	(0.017)
Secondary Education (% gross)	-0.011	-0.008
	(0.012)	(0.013)

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively. Column 1 is based on treatments defined by the ≥ 0.128 rigidity increase threshold. Column 2 is based on a ≥ 0.176 rigidity increase threshold.

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TABLES (Cont.)

Table 5: Effects of increases in rigidity on economic growth (≥ 0.128 threshold).

Matching Method	5-year growth	Cov. Balance	10-year growth	Cov. Balance
PSM: Nearest Neighbor	0.017	13.31	-0.041	18.16*
	(0.102)	(0.21)	(0.168)	(0.052)
PSM: Nearest 2 Neighbors	-0.078	5.58	-0.110	7.99
	(0.092)	(0.85)	(0.142)	(0.63)
PSM: Nearest 3 Neighbors	-0.060	1.72	-0.092	0.72
	(0.089)	(1.00)	(0.130)	(1.00)
PSM: Normal Kernel	-0.019	10.78	-0.043	9.75
	(0.069)	(0.38)	(0.088)	(0.46)
Mahalanobis: NN1	0.024	-	0.016	-
	(0.096)	-	(0.181)	-
Mahalanobis: NN2	0.006	-	-0.007	-
	(0.096)	-	(0.168)	-
Mahalanobis: NN3	-0.021	-	-0.051	-
	(0.080)	-	(0.135)	-

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively. Bootstrapped standard errors are in parentheses using 200 replications for propensity score matching only. "Cov. Balance" columns report Chi-square tests where the null is that covariates are on average balanced between treated countries and their matches.

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TABLES (Cont.)

Table 6: Effects of increases in rigidity on economic growth (≥ 0.176 threshold).

Matching Method	5-year growth	Cov. Balance	10-year growth	Cov. Balance
PSM: Nearest Neighbor	0.058	10.83	0.123	16.27*
	(0.120)	(0.37)	(0.196)	(0.09)
PSM: Nearest 2 Neighbors	-0.020	4.76	0.083	5.46
	(0.104)	(0.91)	(0.170)	(0.86)
PSM: Nearest 3 Neighbors	-0.019	2.50	0.059	2.16
	(0.096)	(0.99)	(0.165)	(1.00)
PSM: Normal Kernel	-0.009	10.75	-0.050	8.77
	(0.077)	(0.38)	(0.106)	(0.55)
Mahalanobis: NN1	-0.015	-	-0.052	-
	(0.109)	-	(0.198)	-
Mahalanobis: NN2	-0.035	-	-0.058	-
	(0.111)	-	(0.187)	-
Mahalanobis: NN3	-0.042	-	-0.100	-
	(0.094)	-	(0.154)	-

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively. Bootstrapped standard errors are in parentheses using 200 replications for propensity score matching. "Cov. Balance" columns report Chi-square tests where the null is that covariates are on average balanced between treated countries and their matches.

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TABLES (Cont.)

Table 7: Excluding autocratic countries: effects of increases in rigidity on economic growth (≥ 0.128 threshold).

Matching Method	5-year growth	Cov. Balance	10-year growth	Cov. Balance
PSM: Nearest Neighbor	-0.127	8.76	-0.184	8.28
	(0.102)	(0.56)	(0.146)	(0.60)
PSM: Nearest 2 Neighbors	-0.163*	4.87	-0.191	5.63
	(0.087)	(0.90)	(0.123)	(0.85)
PSM: Nearest 3 Neighbors	-0.152*	4.15	-0.184	4.24
	(0.084)	(0.94)	(0.116)	(0.94)
PSM: Normal Kernel	-0.049	5.63	-0.161*	4.77
	(0.071)	(0.85)	(0.086)	(0.91)
Mahalanobis: NN1	-0.140	-	-0.217**	-
	(0.108)	-	(0.099)	-
Mahalanobis: NN2	-0.113*	-	-0.219***	-
	(0.066)	-	(0.067)	-
Mahalanobis: NN3	-0.096	-	-0.197***	-
	(0.064)	-	(0.062)	-

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively. Bootstrapped standard errors are in parentheses using 200 replications for propensity score matching only. We exclude treated and control countries who had a Polity2 score of -5 or worse. "Cov. Balance" columns report Chi-square tests where the null is that covariates are on average balanced between treated countries and their matches.

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APPENDIX

Table A1: Replacing Civil Liberties & Political Freedom with Polity: effects of increases in rigidity on economic growth (≥ 0.128 threshold).

Matching Method	5-year growth	Cov. Balance	10-year growth	Cov. Balance
PSM: Nearest Neighbor	0.039	11.79	-0.039	8.90
	(0.134)	(0.23)	(0.217)	(0.45)
PSM: Nearest 2 Neighbors	-0.034	8.31	-0.101	6.38
	(0.124)	(0.50)	(0.200)	(0.70)
PSM: Nearest 3 Neighbors	-0.017	9.16	0.126	5.19
	(0.112)	(0.42)	(0.185)	(0.82)
PSM: Normal Kernel	-0.012	7.70	-0.028	6.76
	(0.079)	(0.57)	(0.129)	(0.66)
Mahalanobis: NN1	-0.013	-	-0.117	-
	(0.125)	-	(0.264)	-
Mahalanobis: NN2	-0.030	-	-0.026	-
	(0.120)	-	(0.237)	-
Mahalanobis: NN3	0.034	-	-0.041	-
	(0.107)	-	(0.222)	-

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively. Bootstrapped standard errors are in parentheses using 200 replications for propensity score matching only. "Cov. Balance" columns report Chi-square tests where the null is that covariates are on average balanced between treated countries and their matches.

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APPENDIX (Cont.)

Table A2: Effects of increases in rigidity on long-term economic growth (≥ 0.128 threshold).

Matching Method	20-year growth	Cov. Balance
PSM: Nearest Neighbor	-0.244	10.50
	(0.333)	(0.40)
PSM: Nearest 2 Neighbors	-0.127	4.38
	(0.310)	(0.93)
PSM: Nearest 3 Neighbors	-0.154	1.95
	(0.296)	(1.00)
PSM: Normal Kernel	-0.124	3.36
	(0.234)	(0.97)
Mahalanobis: NN1	-0.052	-
	(0.188)	-
Mahalanobis: NN2	-0.139	-
	(0.249)	-
Mahalanobis: NN3	-0.163	-
	(0.218)	-

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively. Bootstrapped standard errors are in parentheses using 200 replications for propensity score matching only. "Cov. Balance" columns report Chi-square tests where the null is that covariates are on average balanced between treated countries and their matches.

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APPENDIX (Cont.)

Table A3: Covariate balance (≥ 0.128 increase in rigidity threshold; excluding autocratic countries; PSM nearest neighbor).

		Mean		t-test	
Variable	U/M	Treated	Control	t	p-values
Rigidity	U	0.325	0.576	-2.51**	0.012
	M	0.325	0.491	-1.33	0.197
GDP per cap (log)	U	8.3479	9.2052	-2.97***	0.003
	M	8.3479	7.8781	1.26	0.221
Investment Share	U	0.204	0.231	-1.05	0.293
	M	0.204	0.179	0.56	0.579
Government Share	U	0.274	0.186	4.24***	0.000
	M	0.274	0.270	0.09	0.930
Export Share	U	0.174	0.272	-1.63	0.103
	M	0.174	0.159	0.22	0.824
Inflation (5-year average)	U	243.47	36.693	2.59**	0.010
	M	243.47	165.27	0.36	0.722
Civil Liberties	U	3.539	2.558	2.35**	0.019
	M	3.539	4.154	-1.20	0.241
Political Freedom	U	3.385	2.372	2.07**	0.039
	M	3.385	4.615	-1.81*	0.083
Primary Education (% gross)	U	105.98	101.54	1.10	0.271
	M	105.98	106.12	-0.02	0.986
Secondary Education (% gross)	U	65.326	76.246	-1.35	0.177
	M	65.326	54.519	0.86	0.396

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively.

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APPENDIX (Cont.)

Table A4: Covariate balance (≥ 0.128 increase in rigidity threshold; excluding autocratic countries; PSM nearest 2 neighbors).

		Mean		t-test	
Variable	U/M	Treated	Control	t	p-values
Rigidity	U	0.325	0.576	-2.51**	0.012
	M	0.325	0.410	-0.81	0.428
GDP per cap (log)	U	8.3479	9.2052	-2.97***	0.003
	M	8.3479	7.990	0.94	0.359
Investment Share	U	0.204	0.231	-1.05	0.293
	M	0.204	0.238	-0.63	0.533
Government Share	U	0.274	0.186	4.24***	0.000
	M	0.274	0.259	0.36	0.723
Export Share	U	0.174	0.272	-1.63	0.103
	M	0.174	0.214	-0.52	0.606
Inflation (5-year average)	U	243.47	36.693	2.59**	0.010
	M	243.47	115.49	0.67	0.512
Civil Liberties	U	3.539	2.558	2.35**	0.019
	M	3.539	3.923	-0.73	0.472
Political Freedom	U	3.385	2.372	2.07**	0.039
	M	3.385	4.039	-0.92	0.369
Primary Education (% gross)	U	105.98	101.54	1.10	0.271
	M	105.98	101.18	0.70	0.492
Secondary Education (% gross)	U	65.326	76.246	-1.35	0.177
	M	65.326	53.767	0.93	0.361

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively.

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APPENDIX (Cont.)

Table A5: Covariate balance (≥ 0.128 increase in rigidity threshold; excluding autocratic countries; PSM nearest 3 neighbors).

		Mean		t-test	
Variable	U/M	Treated	Control	t	p-values
Rigidity	U	0.325	0.576	-2.51**	0.012
	M	0.325	0.417	-0.89	0.382
GDP per cap (log)	U	8.3479	9.2052	-2.97***	0.003
	M	8.3479	8.0300	0.80	0.430
Investment Share	U	0.204	0.231	-1.05	0.293
	M	0.204	0.240	-0.71	0.483
Government Share	U	0.274	0.186	4.24***	0.000
	M	0.274	0.258	0.39	0.701
Export Share	U	0.174	0.272	-1.63	0.103
	M	0.174	0.207	-0.42	0.676
Inflation (5-year average)	U	243.47	36.693	2.59**	0.010
	M	243.47	130.63	0.56	0.580
Civil Liberties	U	3.539	2.558	2.35**	0.019
	M	3.539	3.769	-0.42	0.677
Political Freedom	U	3.385	2.372	2.07**	0.039
	M	3.385	3.769	-0.53	0.598
Primary Education (% gross)	U	105.98	101.54	1.10	0.271
	M	105.98	102.86	0.47	0.641
Secondary Education (% gross)	U	65.326	76.246	-1.35	0.177
	M	65.326	55.522	0.79	0.436

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively.

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APPENDIX (Cont.)

Table A6: Covariate balance (≥ 0.128 increase in rigidity threshold; excluding autocratic countries; PSM normal kernel).

		Mean		t-test	
Variable	U/M	Treated	Control	t	p-values
Rigidity	U	0.325	0.576	-2.51**	0.012
	M	0.325	0.444	-1.06	0.301
GDP per cap (log)	U	8.3479	9.2052	-2.97***	0.003
	M	8.3479	8.7386	-0.87	0.391
Investment Share	U	0.204	0.231	-1.05	0.293
	M	0.204	0.228	-0.54	0.592
Government Share	U	0.274	0.186	4.24***	0.000
	M	0.274	0.219	1.29	0.208
Export Share	U	0.174	0.272	-1.63	0.103
	M	0.174	0.227	-0.71	0.487
Inflation (5-year average)	U	243.47	36.693	2.59**	0.010
	M	243.47	67.511	0.95	0.352
Civil Liberties	U	3.539	2.558	2.35**	0.019
	M	3.539	3.100	0.75	0.462
Political Freedom	U	3.385	2.372	2.07**	0.039
	M	3.385	2.936	0.62	0.542
Primary Education (% gross)	U	105.98	101.54	1.10	0.271
	M	105.98	104.25	0.30	0.769
Secondary Education (% gross)	U	65.326	76.246	-1.35	0.177
	M	65.326	68.67	-0.27	0.793

Notes: ***, **, & * indicate significance at the .01, .05, and .10 levels, respectively.

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