Received: 12 October 2020 | Accepted: 2 March 2021
DOI: 10.1111/coep.12533

ORIGINAL ARTICLE

CONTEMPORARY ECONOMIC POLICY

Does constitutional entrenchment matter for economic freedom?

Justin Callais[1] | **Andrew T. Young**[2]

[1]Department of Agricultural and Applied Economics, Texas Tech University, Lubbock, Texas, USA

[2]Rawls College of Business, Texas Tech University, Lubbock, Texas, USA

Correspondence
Andrew T. Young, Rawls College of Business, Texas Tech University, Lubbock, TX 79409, USA.
Email: a.t.young@ttu.edu

Abstract
A growing number of studies explore the determinants of economic freedom. Very few of them consider constitutional design. We study entrenchment, that is, the extent to which constitutions are more costly to change than ordinary policies and institutions. We utilize 1970–2017 data and study episodes where countries adopted meaningfully more entrenched constitutions. Using matching methods, we construct plausible counterfactuals against which to compare their post-treatment changes in economic freedom. We report no significant effects on overall freedom. There is some evidence that entrenchment leads to smaller government size, more regulation, and weaker property rights. However, none of these results are robust.

KEYWORDS
constitutional rigidity, constitutions, economic freedom, entrenchment, institution, political economy

JEL CLASSIFICATION
P00; P16; P48

1 | INTRODUCTION

"The cornerstones of economic freedom are personal choice, voluntary exchange, open markets, and clearly defined and enforced property rights" (Gwartney et al., 2019, p. 1). The extent of economic freedom in a society is defined by its policy and institutional environment. That environment arises from political processes, the structures and limits of which are provided by a society's constitution. What, then, is the relationship between constitutional design and economic freedom in a society? In this article, we focus on one important dimension of constitutional design and ask: does constitutional entrenchment matter for economic freedom?

Understanding why some societies are economically free while others are not is important. There is a considerable amount of evidence that economic freedom is linked to desirable outcomes, including higher income growth and subjective well-being (Hall & Lawson, 2014).[1] Motivated by this evidence, a growing literature seeks to identify the determinants of economic freedom (Lawson et al., 2020). Within this literature, scholars have tended to focus on economic variables and deep historical (or otherwise-exogenous) factors.[2] There has been very little work done on the constitutional determinants of economic freedom.

Abbreviations: ATET, average treatment effect on the treated; CCP, Comparative Constitutions Project; DD, democracy versus dictatorship; DID, difference-in-differences; DPI, Database of Political Institutions; EFW, Economic Freedom of the World; PSM, propensity score matching; PWT, Penn World Table; TEL, tax and expenditure limitations; TWFE, two-way fixed effects; WDI, World Development Indicators.

This neglect of constitutional determinants is surprising. If institutions provide the "rules of the game for society" (North, 1990, p. 3), then a society's constitution provides the *meta-institutions*—the higher-order framework within which the institutional and policy environment emerges. This perspective highlights a distinction between "rules within which ordinary politics proceeds, and the activity of ordinary politics itself" (Buchanan, 2003, p. 154). We expect that the rules governing ordinary politics are important determinants of the policies and institutions that it gives rise to.

In this article, we focus on one aspect of constitutional design that has generated great interest and debate among scholars: *entrenchment*. When constitutional constraints are entrenched, they are more difficult (costly) to change than ordinary-level policies and institutions. For example, simple majorities in the US Congress suffice (absent a presidential veto) to pass ordinary legislation; alternatively, Article V of the US Constitution requires large supermajorities (both in Congress and of the states) for that document to be amended. We study data on constitutions worldwide from 1960 to 2017 to estimate the effect of entrenchment on economic freedom.

The primary challenge we face is providing a plausible identification strategy. Constitutional design may lead to more or less economic freedom, but constitutions are not designed in a vacuum: they are themselves a product of a society's policy and institutional environment. Furthermore, there are undoubtedly variables—many unobservable—that can affect both constitutional design and economic freedom. Simultaneity and omitted variables work to confound attempts at causal inference.

To address these concerns, we exploit episodes when countries adopted new, meaningfully more entrenched constitutions. These are the "treated" countries in our analysis. We follow Hausmann et al. (2005) and Grier and Grier (2021) in employing matching methods to construct a plausible counterfactual for each treated country. Using the full sample of countries, we estimate how likely each country was to have received the treatment based on a set of covariates. Each treated country is then matched to a non-treated country (or countries) that was similarly likely to have adopted a meaningfully more entrenched constitution. This provides each treated country with a counterfactual case. Causal inference is then based on a comparison of post-treatment changes in economic freedom in treated countries versus the set of counterfactuals.

In Figure 1 we report plots of economic freedom for 13 countries that we identify as "treated" in the sense that they adopted meaningfully more rigid constitutions. (See Section 3.1 below.) Economic freedom levels are based on the Fraser Institute's Economic Freedom of the World (EFW) index (Gwartney et al., 2019). Vertical lines indicate the treatment dates. Eyeballing the plots, most cases support the idea that more entrenchment leads to more economic freedom, but weakly; the evidence is mixed (e.g., Ecuador's earlier constitution vs. its latter) and the timing of effects is unclear (e.g., Romania where economic freedom continued to decline after constitutional adoption but then rose notably). Figure 1 illustrates the importance of a compelling identification strategy.

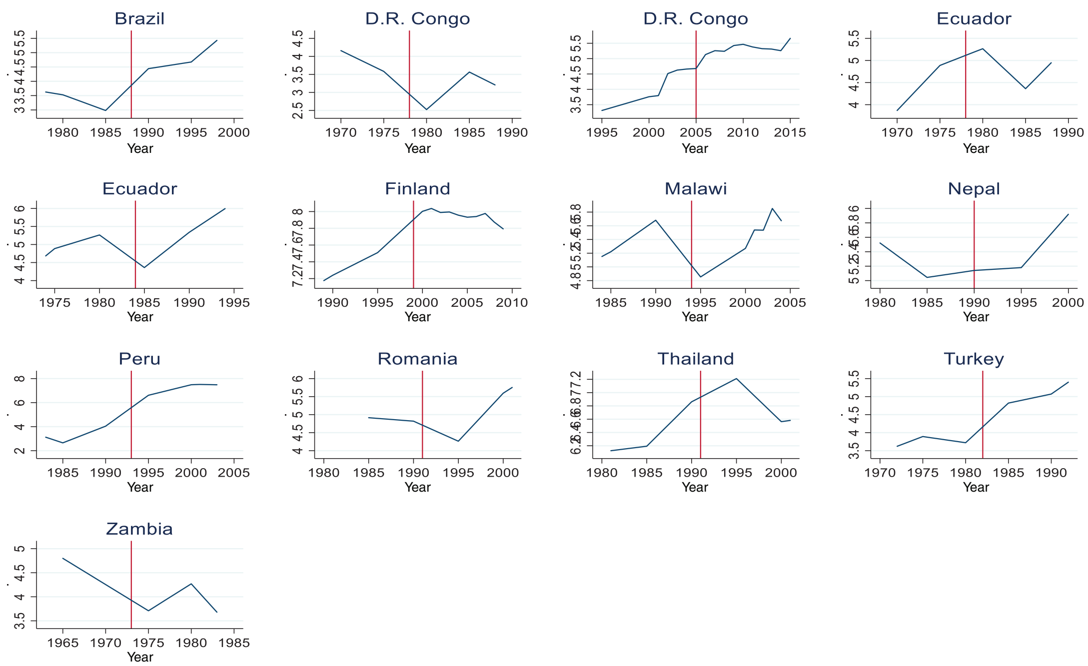

FIGURE 1 Economic freedom scores plotted over time with treatment (i.e., adoption of more entrenched constitution) date indicated

Our work contributes to the very small number of studies exploring constitutional design as a determinant of economic freedom. All of these studies utilize the Fraser Institute's Economic Freedom of the World (EFW) index (Gwartney et al., 2019) as a measure of economic freedom. Spindler and de Vanssay (2002) explore the relationship between economic freedom and whether a country's constitution prescribes (i) a bicameral legislature and/or (ii) a federalist structure. Based on cross-country OLS regressions, they report that only bicameral legislature is a robust (positive) correlate with freedom. de Vanssay et al. (2005) then employ panel data and a larger set of constitutional design variables, reporting that dummy variables for a finite executive term and a parliamentary (vs. presidential) system are the significant correlates from that set. Notably, however, de Vanssay et al. (2005) do not control for lagged economic freedom (which Spindler and de Vanssay (2002) report renders federalist structure statistically insignificant).[3]

Our work also relates to a number of empirical studies that include measures of political institutions as a potential determinant of economic freedom. These include Rode and Gwartney (2012), Sheehan and Young (2015), Hall (2016), Kotschy and Sunde (2017), O'Reilly and Murphy (2017), and Murphy (2020). The measures employed include the widely-used Polity (Marshall & Gurr, 2020) democracy/autocracy measures, the Database of Political Institutions (DPI) (Beck et al., 2001; Keefer & Stasavage, 2003) "checks" measure of veto players, and Cheibub et al.'s (2010) binary measure of democracy versus dictatorship (DD). Each of these measures reflects aspects of a country's constitution. However, each also conflates constitutional factors with ordinary-level institutions and policies. Our aim here is to focus clearly on constitutional design.[4]

The polity, "checks," and DD measures each also synthesize *de jure* (formal; codified) and purely de facto (informal; based on conventions and norms) constitutional factors. From a policy perspective on constitutional *design*, it is important to be able to isolate the role of the former. Since the adoption of the US Constitution in 1789, 95% of states have within 5 years of their establishment adopted a *de jure* constitution (Elkins et al., 2009, p. 42). Epstein (2011, p. 290) is comfortable stating that "[e]very serious student of the topic recognizes, indeed insists, that each nation, especially each emerging nation, should adopt a written constitution to shape the organization of its government."

While Epstein's statement undoubtedly rings true for most scholars of constitutional law—and certainly for policymakers who expend untold time and resources drafting constitutions—economists and political scientists are more circumspect. Voigt (2020, p. 1) notes that there is "a huge gap between constitutional text and constitutional reality."[5] Salter and Furton (2018, p. 38) argue that a *de jure* constitution can at best "specify which set of rules, from a much larger set of feasible rules, will operate"; such documents cannot actually establish constraints that bind political agents. Even accepting this, however, a codification can serve as a coordination device, providing "a focal solution […] so that citizens gain the ability to act in concert and police their government" (Weingast, 2005, p. 105; see also Hardin, 1989; Ordeshook, 1992; Weingast, 1997; Hadfield & Weingast, 2014).[6]

Whether or not constitutional design has bite when it comes to a society's policy and institutional environment, then, is an open question. Yet surprisingly few researchers have pursued it empirically. Montenegro (1995), Tsebelis and Nardi (2014), and Tsebelis (2017) report evidence that longer constitutions are associated with higher rates of corruption. However, these studies do not provide plausible identification strategies.[7] This is also true of the handful of studies (discussed above) exploring constitutional design in relation to economic freedom.

Our work is also related to a larger empirical literature exploring links between constitutional design and economic outcomes. Here we note some recent examples. Studying 19th-century US states, Dove and Young (2019) find that shorter constitutions were associated with lower likelihoods of default. Eicher et al. (2018) report that constitutional executive constraints are associated with greater social infrastructure. Minkler and Prakash (2017) find that stronger constitutional language on social and economic rights is negatively related to poverty rates. Alternatively, Feld and Voigt (2003) and Voigt et al. (2015) find that *de jure* judicial independence is unrelated to economic growth once a measure of de facto independence (which is positively related to growth) is controlled for. Likewise, Callais and Young (2020) report no significant link between constitutional entrenchment and growth. There are also several studies that estimate links between specific constitutional provisions and economic outcomes; see Voigt (2011) for a review.

Notably, aside from Callais and Young (2020), none of the above-referenced studies employ the sort of causal inference techniques associated with the so-called "credibility revolution" in empirical research (Angirst & Pischke, 2010). When possible, exploiting such techniques is desirable. Constitutional design may be an important determinant of a society's policy and institutional environment, but that environment is key to economic and political outcomes that can influence the particular type of constitution that a society adopts. There are also many other variables that can influence politics at both the constitutional and ordinary level.

We proceed as follows. In Section 2, we discuss theory that implies a link between constitutional entrenchment and economic freedom. In Section 3, we then discuss the matching methods that we employ in an attempt to identify such a link from the data. That data is described in Section 4 and the results of analyzing it are reported in Section 5. Section 6 concludes.

2 | WHAT MIGHT ENTRENCHMENT HAVE TO DO WITH ECONOMIC FREEDOM?

The entrenchment of constitutional provisions is characteristic of the "short, framework-oriented constitution" advocated for by James Madison (Hammons, 1999). Entrenchment has been identified with constitutionalism generally and classical liberalism in particular. As Holmes (1995, p. 134) states: "A liberal constitution may be minimally defined as a 'higher law' that cannot be changed through normal lawmaking procedures in the popularly elected assembly."

The existence and nature of a relationship between entrenchment and economic freedom will depend on the former's positive implications for governance. Scholars have noted that an entrenched constitution embodies credible commitments and can promote time-consistent governance patterns (Elster, 1979; Holmes, 1995; Tsebelis, 2017). Time-consistency is a critical foundation for limited government under the rule of law. This is because rule of law is itself a commitment to time-consistency—to applying legislation and regulations to individuals independently of their time, place, and social position.

Importantly, entrenchment can be a barrier to agents amending a constitution in self-serving ways (Aghion & Bolton, 2003; Buchanan & Tullock, 1962; Ginsburg & Posner, 2010; Persson et al., 1997). Requiring large supermajorities and/or allowing for multiple veto players can help to ensure that amendments occur only when they are consistent with a generality norm (Buchanan & Congleton, 2003 [1998]; Congleton, 2004). Governance innovations that are consistent with a generality norm will tend to be consistent with economic freedom.[8] These include innovations that secure individuals and their property under the rule of law; also, the provision of common-interest public goods. Alternatively, governance innovations that secure rents for special interests are inconsistent with a generality norm. Entrenchment may thus empower the protective and productive state while shackling its predatory proclivity (Buchanan, 1975).

Alternatively, there may be reasons to think that constitutional entrenchment is unrelated or even detrimental to economic freedom. For one thing, many scholars are skeptical of the efficacy of parchment barriers generally. Wenzel (2010, p. 65), for example, is adamant: "I emphatically reject the notion that good constitutional parchment is sufficient for successful constitutionalism." (Also see Salter and Furton (2018) and Voigt (2020) to the extent cited in the introduction.) The coincidence of *de jure* constraints and (actually binding) conventions and norms may be random or correlated with the skill of constitutional designers. In either case, there may be no reason to expect a general, positive relationship between entrenchment and economic freedom to exist in the data.

Moreover, Versteeg and Zackin (2016) advocate for an alternative to the Madisonian-type of constitution. Rather than an "entrenched/spare" model of constitutional design, they offer an "unentrenched/specific" model. The latter offers a very different solution to the agency problems associated with political agents. While an entrenched/spare constitution seeks to place durable constraints on political agents (that prevent them from pandering to special interests), an unentrenched/specific constitution allows for "ongoing constitutional micromanagement"—the citizenry can provide and frequently update specific instructions to guide their agents. If economic freedom is indeed associated with desirable outcomes (Hall & Lawson, 2014), then perhaps such constitutional micromanagement is the best way to ensure that political agents provide for it.

Does constitutional entrenchment matter for economic freedom? Ultimately, this ends up being an empirical question. And in what follows, we attempt to provide some empirical evidence.

3 | EMPIRICAL STRATEGY

We aim to identify a potential causal effect of constitutional entrenchment on economic freedom. In doing so, there is a concern about selection bias. Countries that adopt more entrenched constitutions are not randomly selected. They may be likely to do so because of factors correlated with their more general policy and institutional environment. There are also concerns about bias arising from simultaneity and/or omitted variables. Constitutional design may play a determining role in a society's policy and institutional environment, but that environment can also help to determine what sort of constitution the society adopts. There may also be unobserved factors that are determinants of both constitutional design and economic freedom.

Our empirical strategy is chosen to mitigate these concerns. This strategy is based on matching methods. We identify episodes where countries adopt new constitutions that are significantly more entrenched than their predecessors. Those adoptions are defined as "treatments." For each treated country, we construct a counterfactual against which to compare its subsequent change in economic freedom. The counterfactual is chosen to resemble the treated country in terms of a set of covariates. Those covariates are chosen for their relevancy to both the likelihood of receiving the treatment and the outcome of interest (i.e., in this case, the change in economic freedom).

Matching methods were developed to address problems of selection bias (Rosenbaum & Rubin, 1983). Each treated country is matched to a set of non-treated countries (which provides the counterfactual) that are similarly likely to have received the treatment. Post-treatment changes in economic freedom are compared between countries that were, in this sense, similarly selected to have received the treatment. (Of course, the treated country actually *did* receive the treatment while the countries making up the counterfactual *did not*.) The result from this comparison is an estimate of the average treatment effect on the treated (ATET): the difference between the average change in economic freedom for treated countries and the average for the matched counterfactuals.

Regarding endogeneity more generally, our covariates are also chosen to be relevant to economic freedom. Furthermore, we focus our analysis on post-treatment *changes* in economic freedom. In doing so, we difference out time-invariant heterogeneity (An & Winship, 2017). This alleviates concerns for omitted variable bias, similar to a panel data regression model with country fixed effects. Furthermore, one of the covariates that we match on is the level of economic freedom at the time of treatment. The counterfactuals are, then, similarly likely to have received treatment *as determined in part by their policy and institutional environment*. This mitigates simultaneity concerns.

Matching methods provide us with an alternative to the two-way fixed effects (TWFE) model with a dummy treatment variable. The TWFE model has become somewhat of the default causal inference using 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., Athey & Imbens, 2018; Borusyak & Jaravel, 2017; Sun & Abraham, 2020). However, this is actually not the case; in particular, if there is heterogeneity in the treatment effect over time, its estimate will be biased (Goodman-Bacon, 2018; de Chaisemartin & D'Haultfuille, 2020). This is particularly relevant in the case of this study, as only two pairs of treated countries received the treatment in the same year (Table 1). Since matching methods estimate an *average* treatment effect, the estimate will be unbiased even when such heterogeneity is present.

We employ two types of matching in this article: *propensity score matching* and *matching by Mahalanobis distance*. First, with propensity score matching (PSM), the first step is to estimate a logit model of the probability that treatment occurs, conditional on a set of covariates. Second, each country is assigned a *propensity score*, which is its estimated probability of treatment.[9] Third, each treated country is matched to a set of non-treated countries that have similar propensity scores. We refer to a country's "neighbors" in reference to the difference between their propensity scores and that of the treated country. A country's "first nearest neighbor" is the other country in the sample that has the closest propensity score. (Its "second nearest neighbor" has the second closest propensity score, etc.)

An alternative to PSM 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, inexact

TABLE 1 Treatments in baseline estimations (adoption of a constitution with a ≥0.128 increase in entrenchment over its predecessor)

Country	Year of adoption	Increase	Country	Year of adoption	Increase
Brazil	1988	0.192	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	Thailand	1991	0.282
Ecuador	1984	0.395	Turkey	1982	0.136
Finland	1999	0.251	Zambia	1973	0.300
Malawi	1994	0.232			

Notes: the constitutional entrenchment measure is from Ginsburg and Melton (2015).

covariate matching can be based on the Mahalanobis distance metric. This is the Euclidian distance between the covariate vectors of two countries, adjusted for their covariance matrix.

Using PSM, we report separate results when teach treated country is matched to (i) its nearest (non-treated) neighbor, (ii) the average of its two nearest neighbors, (iii) the average of its three nearest neighbors; also based on (iv) matching using a normal kernel function (which uses all of the neighbors but gives larger weight to those with closer propensity scores). We also report results based on (i), (ii), and (iii) when matching is based on the Mahalanobis distance metric.[10]

4 | DATA

The data used in this article can be discussed in terms of three categories: treatment, outcome, and covariates. Our treatment will be defined in terms of a meaningful increase in constitutional entrenchment. The outcome that we are concerned with post-treatment is a change in economic freedom. We aim to identify a causal effect of the treatment on the outcome.

4.1 | Treatment: Constitutional entrenchment

We examine constitutional adoptions from 1960 to 2017. We employ a measure of constitutional entrenchment constructed by Ginsburg and Melton (2015) (henceforth GM). The underlying data for this measure come from the Comparative Constitutions Project (CCP).[11] Following Lutz's (1994), GM exploit information in both the procedures for amendment and observed amendment rates.

Entrenchment is conceived of in terms of the amendment procedures designed into constitutions. However, there are many different types of procedures that are relevant (e.g., supermajority approval in the legislature versus a popular referendum; e.g., requirements on how amendments can be proposed versus how they are approved). To develop a measure of entrenchment, one needs a way to determine the relative importance of these different procedures. Observed amendment rates can inform that determination.

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.[12] For the entire sample of constitutions drawn from the CCP data, GM regress amendment rates on a set of variables coding amendment procedures while also controlling for numerous predictors of political reform generally (p. 695). The procedural variables reflect (i) vote thresholds for approval in the legislature; (ii) the number and type of proposers; and (iii) the number and type of bodies involved in approval.[13] The entrenchment measure is then a linear function of the procedural variables with weights corresponding to the regression coefficients. (The function is normalized such that all constitutions have entrenchment values in the range of 0 to 1.)

The intuition behind the construction of the GM entrenchment measure is as follows. There are various procedures that can be used to entrench constitutional provisions (i.e., which can be divided into categories (i), (ii), and (iii)). These procedures can be found in various combinations in any given constitution. Using CCP data on constitutions worldwide and over time, GM estimate the relative roles of these procedures in determining whether a constitution is or is not resistant to change. The estimates provide weights used in combining the various procedural variables into a unitary measure of constitutional entrenchment.

In the context of our analysis, one might object that time t treatments are being defined in terms of events occurring at $t + 1$ and beyond. This objection would arise based on the use of observed amendment rates to construct the entrenchment measure. For example, for a constitution adopted in 1980, it indeed would be impermissible for the *ex post* amendment rate to determine whether a treatment occurred. (A post-treatment effect is nonsensical if the treatment is still occurring!) However, this objection is not valid for the GM measure. The entrenchment value of any constitution is not meaningfully a function of its own amendment rate. Rather, the amendment rates of *all* constitutions are used in the estimation of weights on the procedural variables. Those weights are the same for all constitutions that are assigned an entrenchment value. Given those weights, the entrenchment value of any constitution is a function solely of the procedure variables. The values of those variables are determined at the moment of constitutional adoption.

To identify treatments based on the GM entrenchment measures, we have to define some threshold for a *meaningful* increase in entrenchment. Our baseline definition of a treatment will be an increase in the entrenchment measure of 0.128. An increase of 0.128 is equal to 20% of the mean entrenchment value (0.642) in the sample. Based on this threshold and the availability of other data, our baseline estimations are based on 13 treatments (Table 1). To check the sensitivity of results to choice of treatment threshold, we also consider increases in rigidity of 0.176 (about one half of a standard deviation) or greater. However, this decreases the number of treatments to 12.

4.2 | Outcome: Changes in economic freedom

As a measure of economic freedom, we employ the Fraser Institute's Economic Freedom of the World (EFW) index. This index is constructed based on five equally weighted areas: (i) government size, (ii) legal system and property rights, (iii) access to sound money, (iv) freedom to trade internationally, and (v) regulation. This measure is a comprehensive indicator of how supportive a country's institutions and policies are to economic freedom. Each country is scored on a scale of 0 to 10, with 10 indicating the greatest support for individual choice, competitive markets with free entry, and security in one's person and property.

The EFW index is reported every 5 years from 1970 to 2000, and then annually through 2017. Since (constitutional entrenchment) treatments occurring pre-2000 do not necessarily correspond to years divisible by 5, we use linear interpolation to fill in annual EFW values for 1970–2000.[14] This allows us to calculate post-treatment changes in economic freedom for all 13 of the cases listed in Table 1. We analyze post-treatment changes over both 5-year and 10-year horizons. Relying on interpolation is somewhat problematic because the time variation is what matters for the outcome of interest. However, the EFW is a measure of institutional quality and institutions evolve slowly over time (e.g., Acemoglu et al., 2001; North, 1990); and interpolation leverages the low-frequency variation in the data. Of course, one can debate the definition of "institutions" and the pace at which they can change. Still, it is the lower-frequency variation that is likely to be important in this context. As Wallis (2014, p. 31) states: "If we want to see whether institutions matter, we have to look at institutions that exist for long enough to exert an effect."

As noted in Section 3 above, because we are considering post-treatment *changes* in economic freedom, we are differencing out the time-invariant uncontrolled-for heterogeneity from the empirical model. Similar to including fixed effects in a panel regression, this serves to mitigate omitted variable bias.

In addition to the overall EFW, we also report ATET estimates for three individual areas of economic freedom (size of government, legal system and property rights, and regulation). Countries are also scored from 0 to 10 on each of these individual areas. Note that all of these sub-indices are constructed such that higher values correspond to greater freedom. Hence, while a higher legal system and property rights score implies stronger property rights under rule of law, higher size of government and regulation scores, respectively, imply *smaller* government and *less* regulation.

4.3 | Covariates

When employing matching methods, one wants to choose covariates that determine the likelihood of receiving the treatment and/or correlate with the outcome of interest. Accounting for variables that correlate with the outcome is standard for any empirical analysis. Taking into account determinants of treatment probability, however, is distinct and fundamental to matching methods. The aim is to match each treated country to a country (or countries) that were similarly likely to have received the treatment (but did not). In comparing post-treatment outcomes between treated countries and their matches, then, we gain confidence that any difference is due to the treatment.

We employ 13 covariates in matching. The first of these is the lagged entrenchment value for the constitution; and the second is the economic freedom score at the time of treatment. Since a country's policy and institutional environment is likely to display a high degree of persistence over time, it is important to account for the prior constitutional framework (under which it was shaped) and of the policy and institutional environment itself.

Next, we take into account several indicators of a country's economic conditions. These include the (log) level of GDP per capita; the (log) real capital stock per capita; also the average investment, government spending, and export shares of GDP. All of these are time-of-treatment values and they are taken from the Penn World Table (version 9.1; henceforth PWT; Feenstra et al., 2015). We further include the share of a country's population living in urban areas. Lastly, we include the average inflation rate (based on the GDP deflator) during the 5 years leading up to treatment. These final two variables are taken from the World Bank's World Development Indicators (WDIs).

While a constitution provides the framework within which a country's ordinary-level politics occurs, it is also true that the political environment can feedback into whether a new constitution is adopted and what sort of design it has. In matching, we take into account a country's Polity2 score from the Polity5 database (Marshall & Gurr, 2020). The

Polity2 score is a regime measure that ranges from −10 (strongly autocratic) to 10 (strongly democratic). Also from Polity5, we employ the regime durability measure, defined as years passed since a regime change (identified by at least a three point change in the Polity2 score over 3 years or less). As covariates, we include both regime durability and regime durability squared. Finally, we include the Polity5 measure of civil war, where positive values (1 = lowest magnitude; 10 = highest magnitude) are assigned when a civil war is occurring (and 0 otherwise). All political variables enter as pre-treatment values.

Summary statistics for covariates are reported in Table 2; the same for post-treatment outcomes are in Table 3.

5 | RESULTS

For our PSM benchmark estimations, the logit estimations are reported in Table 4. These coefficient estimates, along with a country's covariate values, are the basis for that country's assigned propensity score. There are four estimations since we will be estimating ATETs for (1) overall EFW and the (2) size of government, (3) legal system and property rights, and (4) regulation sub-indices separately. (For each estimation, "Relevant Freedom Index" refers to pre-treatment (1), (2), (3), or (4), depending on the corresponding column.)

In the benchmark logit estimation for PSM, pre-treatment entrenchment is a highly significant, negative predictor of whether a country adopts a new, meaningfully more entrenched constitution. This is consistent with the treatment being more

TABLE 2 Summary statistics (covariates)

Variable	Obs.	Mean	Std. Dev.	Min	Max
Entrenchment	3,364	0.626	0.365	0.000	1.000
EFW	3,364	6.058	1.316	1.841	8.817
Size of government	3,319	5.871	1.305	1.573	10
Legal system and property rights	3,179	5.083	1.779	0.991	9.138
Regulation	3,249	6.198	1.341	2.346	9.143
GDP per cap (logged)	3,364	8.937	1.173	6.199	12.282
Investment share	3,364	0.224	0.099	0.001	0.710
Government share	3,364	0.182	0.083	0.017	1.774
Export share	3,364	0.239	0.250	0.004	2.742
Inflation (5-year average)	3,364	42.759	332.792	−8.576	7,016.733
Percent urban	3,364	56.743	22.920	3.525	100
Civil war	3,364	0.172	0.829	0	6
Polity2	3,364	3.534	6.955	−10	10
Per-capita capital stock (logged)	3,364	10.346	1.435	5.915	13.433
Regime durability	3,364	26.469	32.620	0	203

TABLE 3 Summary statistics (outcomes)

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
5-Year change in EFW	3,364	0.224	0.502	−2.158	3.101
10-Year change in EFW	2,850	0.506	0.773	−2.629	4.189
5-Year change in size of government	3,319	0.151	0.772	−5.833	3.869
10-Year change in size of government	2,805	0.376	1.093	−6.497	4.613
5-Year change in LSPR	3,179	0.159	0.744	−4.155	6.478
10-Year change in LSPR	2,665	0.383	0.988	−3.047	5.490
5-Year change in regulation	3,249	0.230	0.538	−2.628	4.355
10-Year change in regulation	2,736	0.495	0.724	−2.699	4.383

TABLE 4 Logit estimations: Determinants of jumps in constitutional entrenchment

Variable	(1)	(2)	(3)	(4)
Rigidity	−3.757***	−3.903***	−3.431***	−4.850***
	(1.026)	(1.106)	(1.107)	(1.323)
Relevant Freedom Index	−0.318	−0.195	0.905***	−0.930**
	(0.304)	(0.285)	(0.360)	(0.422)
GDP per capita (log)	−0.271	−0.235	−1.424	0.480
	(0.836)	(0.879)	(1.006)	(0.916)
Investment share	0.442	1.772	1.627	−0.211
	(3.862)	(4.353)	(5.412)	(4.924)
Government spend share	3.896	1.560	2.173	−3.087
	(2.877)	(4.339)	(5.076)	(4.842)
Export share	−4.488	−6.035	−7.335	−3.273
	(3.591)	(4.109)	(4.888)	(4.307)
Inflation (5-year average)	0.001*	0.001*	0.001*	0.001
	(0.000)	(0.000)	(0.000)	(0.000)
Percentage urban	−0.044*	−0.032	0.010	−0.069*
	(0.026)	(0.028)	(0.031)	(0.036)
Civil war	0.121	0.191	0.238	0.175
	(0.268)	(0.277)	(0.346)	(0.287)
Polity2	0.070	0.079	0.024	0.145**
	(0.059)	(0.059)	(0.068)	(0.073)
Cap. stock per capita (log)	0.508	0.212	0.392	0.048
	(0.471)	(0.551)	(0.678)	(0.534)
Durable	−0.092*	−0.090**	−0.096*	−0.084*
	(0.047)	(0.045)	(0.049)	(0.049)
Durable squared	0.001*	0.000*	0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)

Notes: ***, **, and * indicate significance at the .01, .05, and .10 levels, respectively. Column 1 is based on treatments defined by the ≥0.128 rigidity increase threshold when examining changes in economic freedom. Column 2 is based on the treatments defined by the ≥0.128 rigidity increase threshold when examining changes in the size of government. Column 3 is based on the treatments defined by the ≥0.128 rigidity increase threshold when examining changes in LSPR. Column 4 is based on the treatments defined by the ≥0.128 rigidity increase threshold when examining changes in regulation. The Relevant Freedom Index corresponds to the pre-treatment level of (1) EFW, (2) size of government, (3) legal system & property rights, or (4) regulation.

likely when countries are replacing a relatively flexible constitution. This could be because of a lack of entrenchment is perceived to be associated with poor governance, leading to calls to "tighten the ship" with a new constitution. However, except for the regulation area (4), neither the pre-treatment freedom index nor Polity2 appears to be a significant predictor of treatment. Alternatively, the other notably significant covariates are inflation and regime durability.[15] This may be consistent with economic and political uncertainty creating a demand for a more stable governance framework via constitutional entrenchment.

In Table 5, we report estimated ATETs for overall economic freedom, measured by changes in the EFW index over either the 5-year or 10-year post-treatment horizon.[16] These are, again, considering effects of benchmark treatments (defined as an increase of 0.128 or more in the entrenchment measures).[17] For the PSM estimations, chi-square covariate balance tests are also reported. The null hypothesis is that covariate values are on average balanced between treated countries and their matches. When we cannot reject the null, there is no evidence to suggest that the matches are poor counterfactuals. All else equal, we will discount ATET estimates for which covariate balance null is rejected.

There is no evidence that increased constitutional entrenchment has any systematic, significant effect on economic freedom. None of the estimates are statistically significant, and their signs are both positive (PSM for the 5-year horizon; Mahalanobis for the 10-year) and negative (PSM for the 10-year; Mahalanobis for the 5-year). One might be concerned that the tests have low power, since inference is based on 13 or fewer treatments. However, matching methods are designed to

TABLE 5 Effects of increases in entrenchment on changes in overall economic freedom (≥0.128 threshold)

Matching method	5-Year change	Cov. balance	10-Year change	Cov. balance
PSM: Nearest neighbor	0.407	36.04***	0.039	30.50***
	(0.289)	(0.00)	(0.488)	(0.00)
PSM: Nearest 2 neighbors	0.170	7.95	−0.109	18.79
	(0.259)	(0.85)	(0.424)	(0.13)
PSM: Nearest 3 neighbors	0.032	7.30	−0.045	5.08
	(0.234)	(0.89)	(0.388)	(0.97)
PSM: Normal kernel	0.087	6.58	0.187	6.79
	(0.154)	(0.92)	(0.247)	(0.91)
Mahalanobis: NN1	−0.123	–	−0.083	–
	(0.152)	–	(0.222)	–
Mahalanobis: NN2	−0.145	–	−0.035	–
	(0.137)	–	(0.145)	–
Mahalanobis: NN3	−0.176	–	−0.064	–
	(0.142)	–	(0.164)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. Both PSM and Mahalanobis matching uses all 13 treated units in the 5-year results. PSM uses 11 of the 12 treated units in the 10-year results, while Mahalanobis matching uses all 12 treated units in the 10-year results.

leverage the information from a (much) larger sample of untreated units in constructing counterfactuals. Furthermore, not only do the individual estimates alternate in sign; the point estimates are also quite small. In Table 5, no point estimate is greater than 0.407 in absolute value (and the second largest is 0.187). Compare this to the 1.316 sample standard deviation of EFW. Even if the small number of treatments creates imprecision, the narrow range of estimates on both sides of zero is consistent with constitutional entrenchment having no significant effect on economic freedom.

The estimates when considering the size of government sub-index are reported in Table 6. In this case, the point estimates are almost always positive (the only exception being for PSM with normal kernel-based matching). For the 10-year horizon, two of the Mahalanobis estimates are statistically significant (10% level or better). A higher size of government score implies that government expenditures and transfers are a *smaller* part of an economy. Hence there is some evidence consistent with constitutional entrenchment serving to rein in government and increase that area of economic freedom. But that evidence is fairly weak and suggestive at best.

Alternatively, when considering the legal system and property rights sub-index, the results (Table 7) are consistent with entrenchment being detrimental to economic freedom over the longer run. Most of the point estimates are negative for the 5-year horizon, and this is true regarding the only statistically significant estimate (Mahalanobis nearest neighbor). This is also true when considering the 10-year horizon, but in this case none of the estimates are statistically significant. This is surprising. Albeit very weak, the evidence suggests, if anything, that politics within a more entrenched constitutional framework is *less* conducive to the enforcement of property rights under rule of law.

Lastly, the results for the regulation sub-index are reported in Table 8. The 5-year effects have mixed signs and are never statistically significant. However, the 10-year point estimates are uniformly negative and two of them (PSM and Mahalanobis nearest neighbor) are statistically significant. (A decrease in the regulation index implies more regulation, i.e., less economic freedom.) If anything, these results suggest that greater entrenchment is associated with more burdensome regulation throughout the economy. From a perspective informed by regulatory capture theory (Peltzman, 1976; Stigler, 1971), this suggestion may be also surprising. Entrenchment is advocated as a means to make politics driven by general rather than special interests. If regulation evidences successful rent-seeking by special interests, then one would expect entrenchment to be associated with less rather than more of it.

Now we report a series of robustness checks for effects on the size of government, legal system and property rights, and regulation sub-indices. We focus on the sub-indices because, unlike the overall EFW index, they are associated with some statistically significant results (reported above). However, we provide the same robustness checks for the overall EFW index in the Appendix.[18]

TABLE 6 Effects of increases in entrenchment on changes in size of government (≥0.128 threshold)

Matching method	5-Year change	Cov. balance	10-Year change	Cov. balance
PSM: Nearest neighbor	−0.252	30.32***	−0.555	27.73***
	(0.475)	(0.00)	(0.570)	(0.00)
PSM: Nearest 2 neighbors	0.073	21.26*	−0.021	16.05
	(0.416)	(0.07)	(0.547)	(0.25)
PSM: Nearest 3 neighbors	0.131	12.33	0.047	12.29
	(0.391)	(0.50)	(0.522)	(0.50)
PSM: Normal kernel	0.129	7.47	0.144	8.10
	(0.303)	(0.88)	(0.364)	(0.84)
Mahalanobis: NN1	−0.085	–	4.039**	–
	(0.335)	–	(2.001)	–
Mahalanobis: NN2	−0.010	–	0.735*	–
	(0.253)	–	(0.407)	–
Mahalanobis: NN3	−0.014	–	0.327	–
	(0.260)	–	(0.278)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. PSM uses all 12 treated units in the 5-year results and 10 of the 11 units in the 10-year results. Mahalanobis matching uses all 12 treated units in the 5-year results and all 11 treated units in the 10-year results.

TABLE 7 Effects of increases in entrenchment on changes in legal system and property right protections (≥0.128 threshold)

Matching method	5-Year change	Cov. balance	10-Year change	Cov. balance
PSM: Nearest neighbor	−0.127	27.73***	−0.670	24.95***
	(0.395)	(0.00)	(0.564)	(0.00)
PSM: Nearest 2 neighbors	−0.207	14.46	−0.541	24.95***
	(0.339)	(0.34)	(0.504)	(0.00)
PSM: Nearest 3 neighbors	−0.254	11.04	−0.568	24.95***
	(0.324)	(0.61)	(0.489)	(0.00)
PSM: Normal kernel	−0.077	3.55	−0.455	2.82
	(0.243)	(1.00)	(0.342)	(1.00)
Mahalanobis: NN1	−3.146***	–	−0.060	–
	(1.037)	–	(0.166)	–
Mahalanobis: NN2	0.571	–	0.169	–
	(0.357)	–	(0.156)	–
Mahalanobis: NN3	0.244	–	−0.301	–
	(0.231)	–	(0.185)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. Both PSM and Mahalanobis matching uses all 10 treated units in the 5-year results and all nine units in the 10-year results.

5.1 | Excluding "almost-treated" countries

In employing matching methods, we aim to identify treated countries and then construct plausible counterfactuals based on non-treated countries. In this context, treatment amounts to adopting a meaningfully more entrenched constitution. If we end up matching a treated country to one or more countries that adopted a more entrenched constitution

TABLE 8 Effects of increases in entrenchment on changes in regulation (≥0.128 threshold)

Matching method	5-Year change	Cov. balance	10-Year change	Cov. balance
PSM: Nearest neighbor	−0.320	26.29***	−0.746*	14.64
	(0.330)	(0.00)	(0.451)	(0.33)
PSM: Nearest 2 neighbors	−0.101	6.86	−0.265	10.01
	(0.290)	(0.91)	(0.418)	(0.69)
PSM: Nearest 3 neighbors	−0.032	7.74	−0.282	9.62
	(0.276)	(0.86)	(0.380)	(0.73)
PSM: Normal kernel	0.144	3.43	−0.330	5.28
	(0.173)	(1.00)	(0.288)	(0.97)
Mahalanobis: NN1	0.148	–	−0.329**	–
	(0.379)	–	(0.142)	–
Mahalanobis: NN2	−0.146	–	−0.176	–
	(0.144)	–	(0.118)	–
Mahalanobis: NN3	−0.065	–	−0.074	–
	(0.135)	–	(0.115)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. PSM uses 10 of the 11 treated units in the 5-year results and all 11 treated units in the 10-year results. Mahalanobis matching uses all 11 treated units in the 5-year results and 10-year results.

falling just short of the threshold, then the counterfactual will not be a compelling one. Since the covariates that we match on are chosen, in part, to predict the likelihood of treatment, this concern is a legitimate one.

As a robustness check, we report results based on excluding countries that were "almost" treated from the control group of potential matches. These countries adopted constitutions with higher entrenchment scores than their predecessors, and the increases were ≥0.05. (Recall that our threshold for treatment is ≥0.128.) For size of government, legal system and property rights, and regulation, the results are reported in Table 9, Table 10, and Table 11, respectively.

The results are fairly consistent with those reported in Tables 6 through 8. There is only one statistically significant point estimate for size of government and it is positive (Mahalnobis; 10-year; nearest neighbor); one for legal system and property rights which is negative (Mahalanobis; 5-year; nearest neighbor). However, there are no statistically significant point estimates associated with regulation.

5.2 | Higher threshold for treatment

Here we consider results when the threshold for treatment is a 0.176 (rather than 0.128) or greater increase in the entrenchment measure. This threshold represents about one half of a standard deviation increase. This robustness check requires us to sacrifice treatments; all estimates are based on between 9 and 10 treatments. This is less than ideal but we at least get to see whether increasing the entrenchment score threshold leads to any significant estimates in disagreement with those reported above. The results are reported in Tables 12–14.

When considering size of government or regulation, the statistically significant estimates are consistent with what has been reported above. There is one statistically significant point estimate for size of government and it is positive (Mahalanobis; 10-year; nearest two neighbors); one for regulation and it is negative (Mahalanobis; 10-year; nearest neighbor). However, for legal system and property rights (Table 13) there is one significant estimate and, unlike any reported above, it is positive (Mahalanobis; 5-year; nearest two neighbors).

5.3 | Sensitivity to the choice of covariates

In all results reported thus far, we use the same set of covariates to match. However, results can be sensitive to the choice of covariates and checking that sensitivity is desirable (Dehejia, 2005; Heckman et al., 1997; Millimet, 2011).[19]

TABLE 9 Excluding "almost-treated" countries: Effects of increases in entrenchment on changes in size of government (≥0.128 threshold)

Matching method	5-Year growth	Cov. balance	10-Year growth	Cov. balance
PSM: Nearest neighbor	−0.398	19.00*	−0.240	30.50***
	(0.478)	(0.09)	(0.661)	(0.00)
PSM: Nearest 2 neighbors	−0.061	5.78	0.351	10.79
	(0.415)	(0.95)	(0.598)	(0.63)
PSM: Nearest 3 neighbors	0.084	4.25	0.009	10.17
	(0.380)	(0.99)	(0.572)	(0.68)
PSM: Normal kernel	0.134	7.23	0.132	7.99
	(0.300)	(0.89)	(0.388)	(0.84)
Mahalanobis: NN1	0.062	–	0.795*	–
	(0.279)	–	(0.465)	–
Mahalanobis: NN2	−0.057	–	0.634	–
	(0.248)	–	(0.393)	–
Mahalanobis: NN3	0.056	–	0.327	–
	(0.259)	–	(0.278)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. As potential matches for treated countries, we exclude countries that *almost* reached the threshold necessary to considered treated. Specifically, we drop countries that had an increase in rigidity between 0.050 and 0.128 (Cameroon, Comoros, Ghana, Madagascar, Sierra Leone, South Africa, and Venezuela). Both PSM and Mahalanobis matching use all 12 treated units in the 5-year results and all 11 treated units in the 10-year results.

TABLE 10 Excluding "almost-treated" countries: Effects of increases in rigidity on changes in legal system and property rights protection (≥0.128 threshold)

Matching method	5-Year growth	Cov. balance	10-Year growth	Cov. balance
PSM: Nearest neighbor	−0.039	27.73***	−0.467	24.95***
	(0.416)	(0.00)	(0.555)	(0.00)
PSM: Nearest 2 neighbors	−0.045	8.81	−0.558	15.13
	(0.369)	(0.79)	(0.471)	(0.30)
PSM: Nearest 3 neighbors	−0.154	7.96	−0.731	10.13
	(0.334)	(0.85)	(0.457)	(0.68)
PSM: Normal kernel	−0.088	3.55	−0.466	3.13
	(0.226)	(1.00)	(0.332)	(1.00)
Mahalanobis: NN1	−3.146***	–	−0.060	–
	(1.037)	–	(0.166)	–
Mahalanobis: NN2	0.571	–	0.169	–
	(0.357)	–	(0.156)	–
Mahalanobis: NN3	0.244	–	−0.301	–
	(0.231)	–	(0.185)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. As potential matches for treated countries, we exclude countries that *almost* reached the threshold necessary to considered treated. Specifically, we drop countries that had an increase in rigidity between 0.050 and 0.128 (Cameroon, Comoros, Ghana, Madagascar, Sierra Leone, South Africa, and Venezuela). Both PSM and Mahalanobis matching uses all 10 treated units in the 5-year results and all 9 treated units in the 10-year results.

TABLE 11 Excluding "almost-treated" countries: Effects of increases in rigidity on changes in regulation (≥0.128 threshold)

Matching method	5-Year growth	Cov. balance	10-Year growth	Cov. balance
PSM: Nearest neighbor	0.003	26.29***	−0.054	21.45*
	(0.370)	(0.00)	(0.422)	(0.07)
PSM: Nearest 2 neighbors	0.017	6.93	−0.204	4.66
	(0.315)	(0.91)	(0.370)	(0.98)
PSM: Nearest 3 neighbors	0.095	5.18	−0.223	4.66
	(0.307)	(0.97)	(0.344)	(0.98)
PSM: Normal kernel	0.190	3.49	−0.374	5.28
	(0.203)	(1.00)	(0.279)	(0.97)
Mahalanobis: NN1	0.184	–	−0.046	–
	(0.278)	–	(0.188)	–
Mahalanobis: NN2	−0.103	–	−0.102	–
	(0.141)	–	(0.106)	–
Mahalanobis: NN3	−0.065	–	−0.074	–
	(0.135)	–	(0.115)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. As potential matches for treated countries, we exclude countries that *almost* reached the threshold necessary to considered treated. Specifically, we drop countries that had an increase in rigidity between 0.050 and 0.128 (Cameroon, Comoros, Ghana, Madagascar, Sierra Leone, South Africa, and Venezuela). PSM uses 10 of the 11 treated units in the 5-year results and all 11 treated units in the 10-year results. Mahalanobis matching uses all 11 treated units in the 5-year results and 10-year results.

TABLE 12 Effects of increases in entrenchment on changes in size of government (≥0.176 threshold)

Matching method	5-Year growth	Cov. balance	10-Year growth	Cov. balance
PSM: Nearest neighbor	−0.774	n.a.***	−0.504	24.95***
	(0.497)	(0.00)	(0.775)	(0.00)
PSM: Nearest 2 neighbors	−0.562	16.55	0.127	24.95***
	(0.452)	(0.22)	(0.697)	(0.00)
PSM: Nearest 3 neighbors	−0.433	10.71	0.224	13.74
	(0.397)	(0.64)	(0.658)	(0.39)
PSM: Normal kernel	−0.242	9.39	0.367	8.78
	(0.311)	(0.74)	(0.483)	(0.79)
Mahalanobis: NN1	−0.654	–	0.408	–
	(0.847)	–	(0.431)	–
Mahalanobis: NN2	0.012	–	0.820*	–
	(0.271)	–	(0.471)	–
Mahalanobis: NN3	0.019	–	0.303	–
	(0.289)	–	(0.313)	–

Notes: ***, **, and * 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. *P*-values are in parentheses; "n.a." corresponds to Stata software not returning output on very large chi-square values (and hence a rejection at better than the 1% level). PSM uses all 11 treated units in the 5-year results and 9 of the 10 treated units in the 10-year results. Mahalanobis matching uses all 11 treated units in the 5-year results and all 10 treated units in the 10-year results.

We address this concern in two ways. First, we re-estimated results for overall EFW dropping one covariate (excluding pre-treatment rigidity and EFW) at a time. Second, we re-estimated results using a parsimonious set of only six covariates: rigidity, economic freedom, GDP per capita (log), civil war, polity2, and durability.

TABLE 13 Effects of increases in rigidity on changes in legal system and property rights protection (≥0.176 threshold)

Matching method	5-Year growth	Cov. balance	10-Year growth	Cov. balance
PSM: Nearest neighbor	−0.079	24.95***	−0.026	22.18***
	(0.404)	(0.00)	(0.496)	(0.00)
PSM: Nearest 2 neighbors	−0.137	24.95***	−0.316	22.18***
	(0.393)	(0.00)	(0.417)	(0.00)
PSM: Nearest 3 neighbors	−0.240	13.02	−0.265	5.94
	(0.363)	(0.45)	(0.398)	(0.95)
PSM: Normal Kernel	−0.059	3.27	−0.264	2.10
	(0.261)	(1.00)	(0.297)	(1.00)
Mahalanobis: NN1	0.137	–	−0.177	–
	(0.568)	–	(0.164)	–
Mahalanobis: NN2	0.890**	–	−0.215	–
	(0.421)	–	(0.159)	–
Mahalanobis: NN3	0.236	–	−0.225	–
	(0.309)	–	(0.163)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. PSM and Mahalanobis matching use all nine treated units in the 5-year results. They also both use all eight treated units in the 10-year results.

TABLE 14 Effects of increases in rigidity on changes in regulation (≥0.176 threshold)

Matching method	5-Year growth	Cov. balance	10-Year growth	Cov. balance
PSM: Nearest neighbor	−0.492	24.95***	−0.290	n.a.***
	(0.375)	(0.00)	(0.488)	(0.00)
PSM: Nearest 2 neighbors	−0.330	11.49	−0.224	5.15
	(0.338)	(0.57)	(0.422)	(0.97)
PSM: Nearest 3 neighbors	−0.424	8.43	−0.265	5.10
	(0.323)	(0.82)	(0.378)	(0.97)
PSM: Normal kernel	−0.285	5.15	−0.259	4.91
	(0.245)	(0.97)	(0.295)	(0.98)
Mahalanobis: NN1	0.137	–	−0.364**	–
	(0.411)	–	(0.165)	–
Mahalanobis: NN2	−0.095	–	−0.203	–
	(0.127)	–	(0.141)	–
Mahalanobis: NN3	−0.074	–	−0.102	–
	(0.128)	–	(0.131)	–

Notes: ***, **, and * 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. P-values are in parentheses; "n.a." corresponds to Stata software not returning output on very large Chi-square values (and hence a rejection at better than the 1% level). PSM uses 9 of the 10 treated units in the 5-year results and 9 of the 10 treated units in the 10-year results. Mahalanobis matching uses all 10 treated units in the 5-year results and all 10 treated units in the 10-year results.

The results are included in an ""'Appendix B," available from the authors upon request. The results are generally consistent with those previously reported. There are no statistically significant effects over the 10-year horizon. There are a small number of statistically significant 5-year effects but only two cases (when dropping GDP as a covariate and when dropping the capital stock as a covariate) and only for Mahalanobis estimations. (There are no statistically significant PSM estimations.) In those two cases, the point estimates are all negative and very small (<0.312 in absolute value) compared to the 1.316 sample standard deviation of EFW). When we employ the parsimonious covariate set, there are no statistically significant estimates.

5.4 | Summary of results

As mentioned above, the "almost treated" and higher threshold robustness checks are reported for the overall EFW index in the Appendix (Tables A3 and A4). Furthermore, we also report estimates for the overall EFW index when OECD countries are excluded (Table A5). (We do not report this robustness check for the sub-indices because we are often left with only 7 or 8 treatments.) Within all of these results, there is only one that is statistically significant (Table A4; PSM; 10-year; Normal Kernel); it is negative. There is no strong evidence of an effect of constitutional entrenchment on economic freedom.

And the above is also a fair summary of what we have reported throughout. Despite entrenchment being perceived as fundamental to constitutionalism and classical liberalism, a causal link between increased entrenchment and economic freedom is not robustly identified in the data. Some suggestive evidence does link entrenchment to smaller government; alternatively, most statistically significant estimates link entrenchment to weaker property rights/rule of law and more burdensome regulation. To reiterate and emphasize, though, none of the results are robust across specifications.

6 | CONCLUSIONS

Given the extensive literature showing that economic freedom is robustly correlated with many desirable social and economic outcomes, identifying the determinants of economic freedom is clearly important. To date, very little work has explored constitutional design as one of the potential determinants. Specifically, no studies have examined the role of constitutional entrenchment. Entrenchment has been widely identified with constitutionalism and classical liberalism. As such, there is an expectation that entrenchment goes hand in hand with economic freedom. In this study, we have asked whether there is empirical evidence to back up that prior belief.

Identifying the causal effect of entrenchment on economic freedom, however, is tricky. Constitutional design does not take shape outside of a country's political and economic environment. Changes in economic freedom may factor into the likelihood that a country adopts a more or less entrenched constitution. Various other (unobserved) factors may also influence both economic freedom and constitutional design. Linear regression analysis is likely to produce biased estimates in this context. A more compelling identification strategy is required.

Toward this end, we have employed matching methods. Based on a large sample of countries from 1970 to 2017, we have identified countries that adopted new, meaningfully more entrenched constitutions. For this set of "treated" countries, we have constructed a set of plausible counterfactuals from non-treated countries. Treated and non-treated countries are matched according to their similarity in terms pre-treatment values of a set of covariates, chosen in part to predict the likelihood of treatment. Identification of an entrenchment-economic freedom effect is based on comparing post-treatment changes in economic freedom between treated countries and the set of matched non-treated countries.

We have found no compelling evidence that meaningful increases in entrenchment cause changes in overall economic freedom. In some specifications the estimates are positive; in others they are negative. In almost every single case—save one in the Appendix—the estimate is not statistically significant. We do find some weak evidence of entrenchment impacting changes in certain individual areas of economic freedom. This evidence is consistent with a link between entrenchment and smaller government (in terms of expenditures and transfers); but more regulation and weaker protections of property rights and rule of law. Again, these latter results do not align well with classical liberal constitutional priors. (We emphasize, however, that this evidence is weak and suggestive at best: it is not robust across specifications and we have fewer treated units to work with when dealing with the individual economic freedom areas.)

It is fair to say that the evidence provided leads to acceptance of the null: constitutional entrenchment is not related to economic freedom. While null results are often considered uninteresting, we believe that here this is not the case. Constitutional entrenchment is part of the Madisonian tradition and tightly associated with classical liberal thought. Finding that there is no empirical link between entrenchment and economic freedom is surprising. Furthermore, we are one of the only empirical studies of constitutional political economy to utilize causal inference techniques. This should be of interest to scholars in this field and institutional economics generally, suggesting applications of these methods in future research.

ACKNOWLEDGMENTS

We thank two referees for their insightful comments, which have resulted in an improved article. We also thank Jamie Bologna Pavlik for providing us with valuable advice.

ORCID

Justin Callais https://orcid.org/0000-0003-3582-2277

ENDNOTES

[1] For economic growth see Ayal and Karras (1998), Gwartney et al. (1999), Heckelman and Stroup (2000), and Young and Sheehan (2014); for subjective well-being see Ovaska and Takashima (2006), Gehring (2013), and Nikolaev (2015); other outcomes that economic freedom has been positively linked to include the extent of trust among individuals (Berggren & Jordahl, 2006), health outcomes (Stroup, 2007), and labor shares of income (Young & Lawson, 2014).

[2] Economic variables include economic growth (Heckelman & Knack, 2009; Heinemann, 2004), immigration (Clark et al., 2015; Nowrasteh et al., 2020; Powell et al., 2017) and crisis episodes (Pitlik & Wirth, 2003; Bologna & Young, 2016); historical factors include medieval representative assembly experiences (Bologna Pavlik & Young, 2020), genetic and linguistic diversity (Faria et al., 2016; Gohmann, 2018), and state and technological history (Gohmann, 2018); geographical variables and natural resource endowments have also been considered (Brown, 2014; Gohmann, 2018; Hall, 2016; O'Reilly & Murphy, 2017). An external, though not exogenous, determinant that has been explored is foreign aid (Dutta & Williamson, 2016; Heckelman & Knack, 2009; Young & Sheehan, 2014).

[3] Mudambi et al. (2002) examine a sample 29 emerging market economies. They report that a proportional (vs. plurality) representation dummy enters positively and significantly for economic freedom, as does the number of districts from which representation is drawn. However, their results are prima facie problematic. First, the number of districts is entered without accounting for the size (population or area) of a country. Second, when analyzing (1990–1995) changes in EFW, the point estimates reported on proportional representation are simply implausible (around 44 where the EFW index only runs from 0 to 10).

[4] More generally, normative arguments regarding constitutional design have often been divorced from empirical reality. For example, Shadbegian (1996) notes that tax and expenditure limitations (TELs) are widely seen as desirable to restricting the growth of US state governments; yet he harnesses data that suggests TELs have no significant effect. Likewise, López (2003) notes that term limits are viewed as desirable because they decrease the average tenure of elected officials *and therefore* make election markets more competitive and lead to greater fiscal restraint; yet his review of the literature suggests that while term limits do result in the former, there appears to be no significant, indirect link to competition and fiscal restraint. Of course, some normative arguments are (at least in part) detached from empirical outcomes. (E.g., Thomas Jefferson famously argued against entrenchment because the dead should not be allowed to bind the living. See Bellamy and Castiglione (1997) for a discussion of various normative arguments for and against entrenchment.) However, links to empirical outcomes are certainly an important part of most normative assessments of constitutional design.

[5] While we are concerned with the potential link between constitutional design and the policy and institutional outcomes of ordinary politics, Voigt (2020) is ultimately focused on the (also interesting) question of what are the determinants of the "*de jure*/de facto gap": "the non-congruence between provisions explicitly written down [...] and the behavior of the top representatives of the various government branches" (p. 4)—For example, if explicit presidential term limits are ignored by a sitting president.

[6] Young (2021) notes that such a focal solution can just as well provide political agents within a government with a means to police one another. Salter and Furton's claim is actually consistent with the view that *de jure* constitutions serve as coordination devices (Hadfield & Weingast, 2014; Hardin, 1989; Ordeshook, 1992; Weingast, 1997, 2005). The "feasible rules" that Salter and Furton refer to are presumably conventions or norms. For these to be binding in a political equilibrium, they must be supported by mutual beliefs across agents. A codification of conventions/norms provides a focal solution that evidences the mutual nature of beliefs and facilitates collective action to enforce the conventions/norms.

[7] The latter two papers are explicit in stating that causality may run the other way. Consistent with this possibility, Bjørnskov and Voigt (2014) report that constitutional length is negatively related to social trust; the latter may be a relevant causal determinant of corruption.

[8] To say that they will tend to be is not to say that they will *always* be. For example, James Buchanan was a self-identified classical liberal and advocate for of economic freedom generally; yet he supported a 100% marginal estate tax "over a relatively modest amount" (Henderson, 2013, p. 162; see also Brennan, 2013). In regard to why this was, Berggren (2013, p. 297) personally recalls "Buchanan say[ing] that he thought people *in general* shared a dislike of inherited wealth [...]" (*emphasis added*). Buchanan emphasized the importance of a generality norm and thought that a confiscatory estate tax would be consistent it; but this was the exception rather than the rule.

[9] The distribution of the propensity scores for the treated and untreated units can be found in the appendix (Figure A1). Untreated units with propensity scores lower than the lowest score of treated units are not included in the analysis. Similarly, untreated units with propensity scores higher than the highest score of the treated units are also dropped.

[10] Using a kernel function is not an option when matching directly on covariates.

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

[12] Alternatively, Lutz (1994) considers the *total number of amendments* divided by the number of years a constitution has existed. GM's use of number of years in which amendment occurs "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).

[13] For details on the regression model see the "ar_model_estimates.pdf" file in http://comparativeconstitutionsproject.org/data/endurance_of_constitutions.zip).

[14] For example, when a country has EFW scores for 1970 and 1975, we use a line between those two scores to fill in 1971, 1972, 1973, and 1974 scores.

[15] Whereas Polity2 only enters significantly for the estimation predicting treatment when considering the regulation area of EFW, inflation enters significantly *except* when considering regulation.

[16] The PSM and Mahalanobis matches that are employed for each treated country are reported in the Appendix, Table A1 and Table A2, respectively.

[17] All 13 treatments are used in producing the 5-year estimates. To consider the 10-year horizon, we have to drop one treatment. The Mahalanobis 10-year estimates are based on all 12 of the remaining treatments. The PSM 10-year estimates, alternatively, are based on only 11 of the remaining treatments. This is because (unlike Mahalanobis-based matching) PSM drops any treated units that are outside the region of common support. (Intuitively, if there is no potential match with a propensity score reasonably close to a treated unit, then that particular treated unit is not used in the estimation.) The number of treatments used in subsequent estimations will differ depending on the combination of matching method and output variable. Treatment numbers for all cases are stated in the table notes.

[18] See Tables A3–A5. The results are, again, generally insignificant.

[19] Heckman et al. (1997) show that both the choice of covariates—specifically in PSM logit estimations—and the measurement error in the covariates (also see Gerhart et al., 2000) can affect an ATET meaningfully.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of this article.

How to cite this article: Callais, J. & Young, A.T. (2021) Does constitutional entrenchment matter for economic freedom? *Contemporary Economic Policy*, 39:808–830. https://doi.org/10.1111/coep.12533

APPENDIX A.

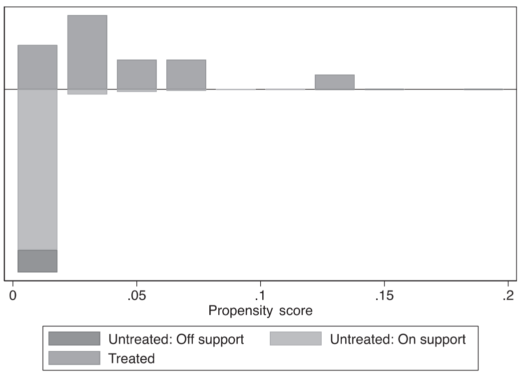

FIGURE A1 Distribution of propensity scores: Treated and untreated units

TABLE A1 Matched countries for each treated country (baseline results; propensity score matching)

Treated country (year)	1st nearest neighbor	2nd nearest neighbor	3rd nearest neighbor
Brazil (1988)	India (1971)	Congo, Dem. Rep (2003)	Zambia (1969)
Congo, Dem. Rep (1978)	Peru (1981)	Zambia (1968)	Ecuador (1977)
Congo, Dem. Rep (2005)	Iran (1986)	Zambia (1972)	Nicaragua (1992)
Ecuador (1978)	Iran (1988)	Peru (1992)	Peru (1985)
Ecuador (1984)	India (1970)	Fiji (1991)	Russia (1998)
Finland (1999)	Belgium (1979)	Malaysia (2000)	Austria (1987)
Malawi (1994)	Morocco (1975)	Iran (1979)	Zambia (1991)
Nepal (1990)	Ecuador (1972)	Nicaragua (1991)	Iran (1980)
Peru (1993)	Russia (2001)	Spain (1978)	Iran (1988)
Romania (1991)	Peru (1973)	Peru (1971)	Russia (2001)
Thailand (1991)	India (1971)	Congo, Dem. Rep (2003)	Zambia (1969)
Turkey (1982)	Israel (1973)	Ecuador (2006)	Nicaragua (1984)
Zambia (1973)	Russia (1995)	Kyrgyzstan (2012)	Congo Dem Rep (1970)

Notes: These matches are from the baseline results (propensity score matching) shown in Table 5.

TABLE A2 Matched countries for each treated country (baseline results; Mahalanobis matching)

Treated country (year)	1st nearest neighbor	2nd nearest neighbor	3rd nearest neighbor
Brazil (1988)	Brazil (1987)	Brazil (1986)	Brazil (1985)
Congo, Dem. Rep (1978)	Congo, Dem. Rep (1976)	Syria (1979)	Congo, Dem. Rep (1977)
Congo, Dem. Rep (2005)	Congo, Dem. Rep (2004)	Congo, Dem. Rep (2003)	Sierra Leone (2001)
Ecuador (1978)	Ecuador (1977)	Ecuador (1976)	Ecuador (1975)
Ecuador (1984)	Mauritius (1979)	Mauritius (1978)	Mauritius (1977)
Finland (1999)	Finland (1998)	Finland (1997)	Denmark (1998)
Malawi (1994)	India (1970)	Bangladesh (1992)	Bangladesh (1993)
Nepal (1990)	Nepal (1984)	Nepal (1986)	Nepal (1985)
Peru (1993)	Peru (1992)	Peru (1991)	Peru (1990)
Romania (1991)	Slovakia (1995)	Hungary (1994)	Croatia (2001)
Thailand (1991)	Thailand (1990)	Thailand (1989)	Thailand (1988)
Turkey (1982)	Turkey (1980)	Turkey (1981)	Turkey (1972)
Zambia (1973)	Zambia (1972)	Zambia (1971)	Zambia (1970)

Notes: These matches are from the baseline results (Mahalanobis matching) shown in Table 5.

TABLE A3 Excluding "almost-treated" countries: Effects of increases in entrenchment on changes in economic freedom (≥0.128 threshold)

Matching method	5-Year change	Cov. balance	10-Year change	Cov. balance
PSM: Nearest neighbor	0.092	36.04***	−0.066	27.53***
	(0.320)	(0.00)	(0.469)	(0.00)
PSM: Nearest 2 neighbors	−0.142	9.51	−0.498	8.72
	(0.234)	(0.73)	(0.429)	(0.73)
PSM: Nearest 3 neighbors	−0.075	7.45	−0.378	3.41
	(0.242)	(0.88)	(0.403)	(0.99)
PSM: Normal kernel	0.081	6.45	0.180	6.51
	(0.180)	(0.93)	(0.284)	(0.93)
Mahalanobis: NN1	−0.198	–	−0.083	–
	(0.174)	–	(0.222)	–
Mahalanobis: NN2	−0.145	–	−0.035	–
	(0.137)	–	(0.145)	–
Mahalanobis: NN3	−0.195	–	−0.064	–
	(0.155)	–	(0.164)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. As potential matches for treated countries, we exclude countries that *almost* reached the threshold necessary to considered treated. Specifically, we drop countries that had an increase in rigidity between 0.050 and 0.128 (Cameroon, Comoros, Ghana, Madagascar, Sierra Leone, South Africa, and Venezuela). Both PSM and Mahalanobis matching use all 13 treated units in the 5-year results. PSM uses 11 of the 12 treated units in the 10-year results, while Mahalanobis matching uses all 12 units in the 10-year results.

TABLE A4 Effects of increases in entrenchment on changes in economic freedom (≥0.176 threshold)

Matching method	5-Year growth	Cov. balance	10-Year growth	Cov. balance
PSM: Nearest neighbor	0.357	13.07	−0.635	30.50***
	(0.300)	(0.44)	(0.559)	(0.00)
PSM: Nearest 2 neighbors	−0.057	10.80	−0.472	14.88
	(0.264)	(0.63)	(0.477)	(0.32)
PSM: Nearest 3 neighbors	−0.083	7.72	−0.371	14.82
	(0.238)	(0.86)	(0.417)	(0.32)
PSM: Normal kernel	−0.028	6.48	−0.489*	12.23
	(0.182)	(0.93)	(0.269)	(0.51)
Mahalanobis: NN1	−0.144	–	−0.090	–
	(0.182)	–	(0.260)	–
Mahalanobis: NN2	−0.142	–	−0.013	–
	(0.146)	–	(0.163)	–
Mahalanobis: NN3	−0.165	–	−0.038	–
	(0.155)	–	(0.181)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. Both PSM and Mahalanobis matching use all 12 treated units in the 5-year results and all 11 units in the 10-year results.

TABLE A5 Excluding OECD countries: Effects of increases in entrenchment on changes in economic freedom (≥0.128 threshold)

Matching method	5-Year change	Cov. balance	10-Year change	Cov. balance
PSM: Nearest neighbor	−0.108	11.42	0.098	24.95***
	(0.416)	(0.49)	(0.573)	(0.00)
PSM: Nearest 2 neighbors	−0.200	9.15	0.107	7.79
	(0.349)	(0.76)	(0.509)	(0.86)
PSM: Nearest 3 neighbors	−0.217	8.31	0.118	4.17
	(0.321)	(0.82)	(0.491)	(0.99)
PSM: Normal kernel	−0.067	2.08	0.049	1.45
	(0.239)	(1.00)	(0.322)	(1.00)
Mahalanobis: NN1	−0.065	–	0.013	–
	(0.086)	–	(0.191)	–
Mahalanobis: NN2	−0.086	–	0.021	–
	(0.131)	–	(0.173)	–
Mahalanobis: NN3	−0.185	–	−0.012	–
	(0.156)	–	(0.158)	–

Notes: ***, **, and * 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. *P*-values are in parentheses. We exclude treated and control countries who were OECD during relevant years. PSM uses all 11 treated units in the 5-year results and 9 of the 10 units in the 10-year results. In the 10-year results, one unit lie outside the region of common support. Mahalanobis matching uses all 11 treated units in the 5-year results and all 10 treated units in the 10-year results.