# 5 Online Appendices

In the Appendices, we describe the data, detail our estimation strategy, display numerical results, and undertake some robustness analyses.

## 5.1 Data

### 5.1.1 Cross-national Data

Like Tsebelis (2022), we include Israel in the analysis, but we do not include the United Kingdom because its uncodified constitution renders it an exceptional case. There is debate in the literature as to when the current UK constitution was enacted, which affects the measurement of its amendment rate and length (cf. Tsebelis 2017, 2022)—a dependent variable and control in the analysis.

We also use Tsebelis' measure of constitutional rigidity, which is a sum of the approval thresholds of the institutions required to consent to a constitutional amendment based on data provided by the Constitute Project. When amendments require the approval of a bicameral legislature, the measure is based on the Euclidean distance between the partisan composition of the two chambers. That is, if one legislative chamber is composed of parties with proportions $x_1, x_2, x_3, ...x_n$, and a second legislative chamber is composed of parties with proportions $x'_1, x'_2, x'_3, ...x'_n$, then the distance between the two chambers is calculated as $\sqrt{(x_1 - x'_1)^2 + (x_2 - x'_2)^2 + (x_3 - x'_3)^2 + ... + (x_n - x'_n)^2}$. The index also includes adding or subtracting a small constant for other procedural requirements that affect rigidity, e.g., a quorum requirement or a mandate that an amendment be passed twice in the same legislative session.

The group membership variable is the total number of group types in which a respondent has either an active or inactive membership. We limit the total number of group types to subjects of inquiry on every iteration of the WVS: church or religious organizations; sport or recreational organizations; art, music or educational organizations; labour unions; political parties, environmental organizations; professional organizations; humanitarian or charitable organizations; and other organizations.

Table A1: Social Capital Indicator Correlation Matrix

| | Government Confidence | Party Confidence | Court Confidence | Group Membership |
|---|---|---|---|---|
| Party Confidence | 0.873 | | | |
| Court Confidence | 0.700 | 0.767 | | |
| Group Membership | 0.368 | 0.318 | 0.261 | |
| Civic Activism | -0.048 | 0.047 | 0.269 | 0.263 |

Consistent with other scholarship (Welzel 2013), we limit the civic activism variable to petitioning, boycotting, and protesting, as these activities are the only ones that appear on every iteration of the WVS. As displayed below, one response option given to survey participants is that they "might do" a civic activity. For our index, however, we only count instances when a respondent has engaged in a given activity. As displayed in Table A1, some of these indicators are strongly correlated (such as Government Confidence and Party Confidence), while others are not (like Government Confidence and Civic Activism).

Table A2: Correlation All WVS Data and Pre-2014 Data

| Variable | Correlation |
|---|---|
| Government Confidence | 0.959 |
| Party Confidence | 0.982 |
| Court Confidence | 0.985 |
| Group Membership | 0.938 |
| Civic Activism | 0.995 |

Data for the dependent variable end in 2013, but we use all available WVS data, including surveys since 2013. As discussed in the body of the paper, each social capital indicator is constructed by creating a country-wave average and then a cross-wave, national average for countries surveyed in multiple waves. We include all available WVS data for several reasons. First, it provides additional degrees of freedom for a small sample and broadens the diversity of democratic nations included in the analysis. Second, the dependent variable measures the amendment rate for each nation's current constitution, and no democratic nations have replaced their constitutions since then. Finally, as indicated in Table A2, there is a very strong correlation between variables constructed using all available data and pre-2014 data.

The wording for all the WVS confidence questions is as follows:

- I am going to name a number of organizations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all?
  - The government (in your nation's capital)
  - Political parties
  - The courts
- Now I am going to read out a list of voluntary organizations; for each one, could you tell me whether you are a member, an active member, an inactive member or not a member of that type of organization?
  - Church or religious organization
  - Sport or recreational organization, football/baseball/rugby team
  - Art, music or educational organization
  - Labour union
  - Political party
  - Environmental organization
  - Professional organization
  - Humanitarian or charitable organization

- Other organization

- Now I'd like you to look at this card. I'm going to read out some different forms of political action that people can take, and I'd like you to tell me, for each one, whether you have actually done any of these things, whether you might do it or would never, under any circumstances, do it.
  - Signing a petition
  - Joining in boycotts
  - Attending peaceful demonstrations

#### 5.1.2 Cross-national Longitudinal Data

The Civil Society Participation Index, courtesy of Varieties of Democracy (V-Dem), represents an aggregation of four indicators measuring the degree to which organizations engage in politics. The first indicator measures the participatory environment in which civil society organizations (CSOs) operate, with potential scores ranging from zero, where the state sponsors all organizations, to three for societies with diverse CSOs in which citizens at least occasionally participate. Next, the index includes an indicator that gauges the extent to which rulers routinely consult CSOs. The degree to which women can participate in CSOs forms another dimension of the index. Finally, the index includes a measure of the centralization of candidate nominations (Bernhard et al. 2017, 347-48).

The Political Constraint (PolCon) dataset was developed by Henisz (2000). His measure of legislative fractionalization is equal to the probability that two random draws from one legislative chamber are from different parties. Given the larger $N$, we include additional controls. Courtesy of Penn World Table, we control for real logged per-capita GDP and real GDP annual growth. Like Tarrabar and Young (2021), we also control for the Executive Constraint component of each nation's Polity IV score and ethnic fractionalization, using the Historical Index of Ethnic Fractionalization (Drazanova 2020), which corresponds to the probability that two randomly drawn individuals within a country are not from the same ethnic group. Finally, we include a dichotomous indicator of a major episode of political violence per country-year, as defined by the Center for Systematic Peace.

#### 5.1.3 State Data

*The Book of the States*, published by the Council of State Governments, provides data on the number of amendments proposed and ratified in each state, as well as the length of each state's constitution. We use *The Book of the States* and the website Ballotpedia to exclude the number of amendments that were proposed through the constitutional initiative process that exists in 18 states. This allows for a direct comparison across the states of the number of amendments proposed by state legislatures and ratified by voters in Figure A3.

We also use Ballotpedia for information regarding the partisan make up of state legislatures. Following the formula of Tsebelis (2022), we take the Euclidean distance between the two chambers every year before taking a biennial average. Nebraska, which uses nonpartisan elections to its single legislative chamber, takes a Euclidean distance value of zero.

Like Tsebelis (2022), we add 0.01 if states limit the number of amendments that can be placed on the ballot at one time, if states require an amendment to pass in multiple legislative sessions, if amendments are subject to a quorum requirement, or if a referendum requires a majority of those voting in the election, rather than those voting on the ballot measure (effectively turning an abstention into a no vote). We subtract 0.01 for states that have the constitutional initiative, periodic constitutional convention referendums, and emergency or alternative amendment provisions.

We include information on the referendum process when analyzing amendment proposals because we assume that state legislators will act strategically based on whether they anticipate an amendment will be ratified by the voters. The size of a majority required in a referendum is likely to play a part of this calculation.

Finally, Hawes, Rocha, and Meier (2013) construct their state-level social capital measure using data from the marketing research firm MediaMark Research, Inc (MRI). Every other year, MRI interviews over 20,000 respondents for its publication, *The Survey of the American Consumer*. The resulting index is constructed using a factor analysis of 22 items from the MRI survey, which measure community organizational life, engagement in public affairs, and community volunteerism.

## 5.2 Addressing Endogeneity

The cross-sectional, longitudinal structure of the V-Dem data provides an opportunity to test whether social capital trends are endogenous to constitutional reform. For high salience amendments that expand democratic rights, the causal arrows may flow in both directions. That is, social capital could mitigate the transaction costs associated with expanding democratic rights, while the creation of new rights then spurs higher levels of political trust and future citizen activism.

Thanks to Tsebelis (2022, 291-92), researchers now have a sense as to which amendments around the world are democratically significant and which are more routine. Tsebelis consulted country experts to classify every amendment event in a country-year into one of three categories: insignificant, significant, or exceptionally significant. Significant amendments "alter (but do not transform) key institutional features of the legislative, executive, or judicial bodies of government (or their relationship); expand the electorate (but not fundamentally alter it) in some way; or enhance individual rights." The Nineteenth Amendment to the US Constitution, which expanded the right to vote to women, falls under this category. Meanwhile, exceptionally significant amendments "transform how legislative bargaining or interbranch relations transpire, introduce an entirely new class of individual rights to a citizenry, or were subsequently deemed 'unconstitutional' by the country's Supreme Court." Ending slavery in the United States via the Thirteenth Amendment qualifies as exceptionally significant.

The subset of "significant" and "exceptionally significant" amendments provide a more fruitful test for an endogenous relationship with social capital than using *all* amendments. Many constitutional amendments are low salience affairs that do not impact the rights of citizens one way or the other. In these instances, there is no theoretical reason to suspect endogeneity. For example, the CSPI score for the United States remained at 0.807 consistently for several years in the 1910s. Following the ratification of the Nineteenth Amendment, the

CSPI score immediately jumped up to 0.825 and remained at that level for another decade. On the other hand, the ratification of the 27th Amendment in 1992, which imposes procedural requirements on congressional pay raises, made no impact to the CSPI score of 0.985, which held constant across the 1990s.

To identify and correct for endogeneity, we employ an instrumental variables approach using the Stata package `xtivreg2`. This package assumes that both the dependent variable and potentially endogenous regressor are continuous. In our analysis, the potentially endogenous regressor is dichotomous: whether one or more significant amendments were ratified in a given country-year. Nevertheless, assuming our amendment variable exerts a constant effect, there is no need to use logistic regression in the first stage of the analysis. Clustering the standard errors on each country is sufficient. Conducting logistic regression is theoretically more efficient, but we obtain similar results under either specification.[1]

To be effective, instrumental variables should correlate with the potentially endogenous regressor but not the dependent variable. In other words, they need to be associated with constitutional reform but not social capital. For instrumental variables, we use the length of each country's constitution, measured by the natural log of its word count, and the length of time since the constitution had last been amended. As we explain in the main text, longer constitutions tend to have a greater need for amendments, and we also predict that the length of time a constitution has been stable affects the likelihood of change at a given point in time. Of course, constitutional rigidity would have made for an effective instrumental variable, but there is only significant within-country variation in 16 nations. By excluding it, our sample spans 76 democratic nations.

Table A3: Instrumental Variables Regression Models of Social Capital Trends

| Term | (1) | (2) |
|---|---|---|
| Amendment Event | 0.002 | 0.004 |
| | (0.003) | (0.006) |
| Freedom of Expression | 0.548*** | 0.548*** |
| | (0.146) | (0.146) |
| Constant | 0.002*** | 0.002*** |
| | (0.001) | (0.001) |
| Weak Instruments (F-Stat) | 109.06*** | 54.913*** |
| Overidentification (Sargan) | 0.001 | 0.016 |
| Endogeneity (WH F-Stat) | 0.004 | 0.014 |
| $R^2$ | 0.266 | 0.266 |
| N | 2,500 | 2,500 |
| Countries | 76 | 76 |
| Amendment Type | Significant/ Exceptionally Significant | Exceptionally Significant |

The goal of this analysis is to evaluate whether the kind of positive, instantaneous effect of amendments like the Nineteenth Amendment on American social capital occurs systematically. Because of the immediate nature of the observed effect, we use a first difference

[1] See `https://www.statalist.org/forums/forum/general-stata-discussion/general/1524185-instrumental-variable-using-panel-data-with-binary-endogenous-variable`.

approach in lieu of a fixed effects model, which is preferable when the effect of the independent variable lasts over several years. Both first difference and fixed effects estimators are unbiased and consistent, but they make different assumptions about the error term. The first difference estimator assumes errors are well behaved in differences, while the fixed effects estimator assumes errors are well behaved in levels.

First difference models transform regressors $X_{i,t}$ to $\tilde{X}_{i,t} = X_{i,t} - X_{i,t-1}$ and the dependent variable $y_{i,t}$ to $\tilde{y}_{i,t} = y_{i,t} - y_{i,t-1}$. Thus, it is well-suited to capture the effect of a constitutional amendment, which takes the value of 0 at time $t$-1 and 1 at $t$, on a change in social capital between time $t$-1 and $t$.[2] However, if significant constitutional amendments occur in consecutive years, this model assumes each subsequent year with an amendment would not affect social capital, as the amendment variable takes a value of 1 at time $t$-1 *and* a value of 1 at time $t$ (leading to a difference of 1-1=0). Significant constitutional change is rare in general, and it is even rarer to find it occurring in consecutive years (81 times out of an N of 2,500). Our results remain the same whether we include these instances or drop them.

In Table A3, we report the results of two instrumental variable regression models of changes to social capital. The dependent variable is V-Dem's Civil Society Participation Index. The independent variable of interest is the adoption of one or more potentially rights-enhancing amendments. Model 1 covers any amendment deemed significant or exceptionally significant. Model 2 is limited to exceptionally significant amendments only. In both models, we include V-Dem's index measure for freedom of expression and alternative sources of information as a control, as we predict social movements will be more successful in organizing and operating in nations with more open information environments. This index is an annual composite measure of freedom of discussion, academic expression, and cultural expressionl, government censorship efforts, media bias, and self-censorship.

As predicted, the coefficients of both variables are positive in both models. However, only the Freedom of Expression variable achieves conventional levels of statistical significance. The diagnostic tests provide evidence that the amendment event variable in each model was properly instrumented. The weak instruments test statistic allows for the rejection of the null hypothesis that the instrumental variables are only weakly correlated with the endogenous regressor. The overidentification test assesses whether the instruments are "valid." The statistics in both models fail to reject the joint null hypothesis in that the instruments are uncorrelated with the error term and that the excluded instruments are correctly excluded from the estimated equation.

The primary insight from these two models are the endogeneity test statistics. The results of these tests are only valid if the models are otherwise well specified, which is why the previous diagnostic tests are important. Here, the null hypothesis is that the specified endogenous regressors can be treated as exogenous, and the test statistics are too small to reject the null. Thus, there is no evidence that even the most significant constitutional

[2] Sometimes, the change in social capital does not occur until time $t$ and $t$+1. For example, the CSPI value in the United States increased between 1865 and 1866, the *former* year being the one in which the Thirteenth Amendment was ratified. However, the CSPI increased between 1919 and 1920 - the *latter* being the year when the Nineteenth Amendment was ratified. To mitigate this, we lag the amendment variable and the two instrumental variables by one year.

amendments exert a significant effect on social capital levels.

## 5.3 Models

In the analysis that follows, we use the `brms` package (Bürkner 2017, 2018, 2021) in R (R Core Team 2021) to estimate the models. We use the `tidyverse` universe of packages for data manipulation (Wickham et al. 2019), the `posterior` package for summarizing posterior distributions (Bürkner et al. 2022; Vehtari et al. 2021) and the `ggplot2` package for plotting results (Wickham 2016). We discuss the specification and estimation of each of the three different models below.

### 5.3.1 Cross-national Models

Currently, the state of the art methodologically is Tsebelis (2022), where amendment rates are modeled with a heteroskedastic linear model to account for the changing residual variance as a function of independent variables. This strategy, while common in economics, has several disadvantages. The same problems that are evident in the linear probability model of binary variables also exist (though perhaps to a lesser degree) when modeling proportions (i.e., rates) as the dependent variable. While it would be possible to transform the probabilities into something unbounded using the logit transform (or similar) $y^* = log(\frac{p}{1-p})$, in this case 20% of the observations are boundary values of 0 or 1 such that the logit transform is undefined: e.g., $\log\left(\frac{1}{1-1}\right) = \infty$. Again, we could solve this by shrinking the range (e.g., $p^* = 0.98 \times p + .01$) and then treat $y^* = log(\frac{p^*}{1-p^*})$, but the amount by which we shrink the range (0.98 in the previous equation) is arbitrary and potentially partly determinative of the model result. While we *could* use this model in a pinch, there is no particularly good reason not to use a model that avoids making questionable assumptions.

We choose the negative binomial regression model (NBRM) as our starting point. Inherent in this model is the idea that the variance is related to the independent variables. Specifically, we use the following model:

$$\begin{aligned} y_i &\sim NB(\mu_i, \phi) \\ &= \binom{n+\phi-1}{n} \left(\frac{\mu_i}{\mu_i+\phi}\right)^{y_i} \left(\frac{\phi}{\mu_i+\phi}\right)^{\phi} \\ E(y_i) &= \mu_i \\ Var(y_i) &= \mu_i + \frac{\mu_i^2}{\phi} \end{aligned}$$

In a conventional NBRM, we parameterize the expected value as $\mu_i = e^{\beta_0+\beta_1 x_{i1}+\ldots+\beta_k x_{ik}}$. The $\phi$ parameter (or more appropriately $\frac{1}{\phi}$ )is the overdispersion parameter. In our Bayesian model we can also parameterize this term such that it is a function of other variables in a similar fashion to $\mu_i$: $\phi_i = e^{\gamma_0+\gamma_1 x_{i1}+\ldots+\gamma_k x_{ik}}$. Doing so allows both terms that contribute to the residual variance ($\mu$ and $\phi$) to be different functions of the variables of interest. We use this approach when warranted (not all models will benefit from this additional complexity).

In these models, we parameterize both $\mu_i$ and $\phi_i$ as follows:

$$\mu_i = \exp(\beta_0 + \beta_1 \text{Rigidity}_i + \beta_2 \text{Social Capital}_i + \beta_3 \log(\text{Words}_i) + \log(\text{Age}))$$

$$\phi_i = \exp(\gamma_0 + \gamma_1 \text{Rigidity}_i + \gamma_2 \text{Social Capital}_i)$$

Since the conditional variance of $y_i$ is a function of both $\mu_i$ and $\phi_i$, we can calculate how the variance relates to the variables of interest - Constitutional Rigidity and Social Capital.

Considering the model specification, note the inclusion of log(Age). This is the exposure or offset term in the model. In the data, the amendment rate is calculated by dividing the number of years in which amendments were ratified by the number of years since the adoption of the current constitution. Including an exposure term allows us to generate a model that predicts rates instead of counts, like Tsebelis (2022), but without needing to make assumptions about the errors. This is particularly useful given the considerable variation in the age of constitutions.

We use an average first difference approach, changing a variable of interest while holding all other variables at their observed values (Hanmer and Kalkan 2013). The general approach works as follows: if we are interested in the effect of social capital, we would generate a new data frame ($D_1$) where for all observations we subtract $.5 \times sd(\text{Social Capital})$. We then generate a second new dataframe ($D_2$) where for each observation we increase social capital by the same amount ($.5 \times sd(\text{Social Capital})$). We then produce the relevant design matrices for each data frame: $X_1$ and $X_2$, respectively. We take the $s \times k$ matrix of posterior draws of the model parameters ($\tilde{\beta}$) and generate predictions:

$$\tilde{\mu}_1 = e^{X_1 \tilde{\beta}'}$$
$$\tilde{\mu}_2 = e^{X_2 \tilde{\beta}'}$$

We then take the difference between the two $\Delta = \tilde{\mu}_2 - \tilde{\mu}_1$. The $\Delta$ matrix is now $n \times s$ where $n$ is the number of observations in the data and $s$ is the number of posterior draws from the model parameters. Taking the average in each column gives a vector of length $s$ of posterior average first differences in predictions. We can summarize this vector (i.e., calculate the average, quantiles, and posterior probabilities) to learn about the expected effect of the variables of interest. The procedure described above is how we would proceed to calculate the effect for the mean. We use a similar procedure to calculate the effect for the variance.

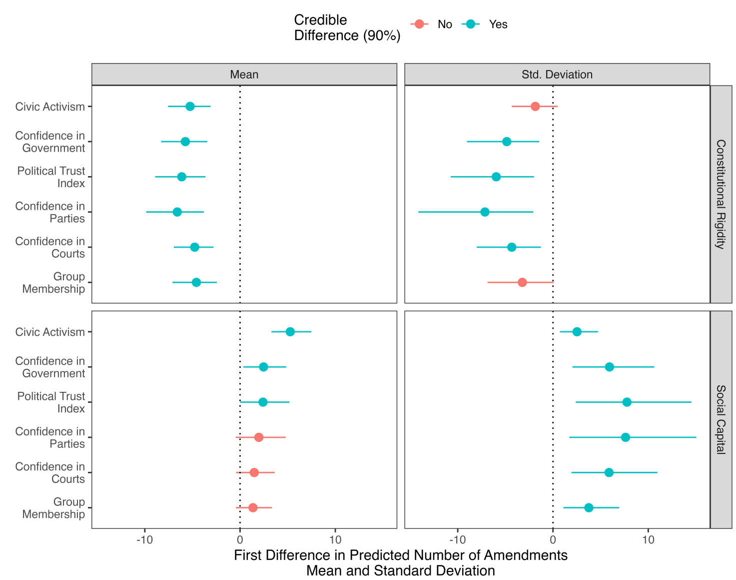
Figure A4, below, shows the average first differences for each of the variables of interest for both the mean and variance. Observations are marked as "credible" if they have at least 90% of their posterior density on the same side of zero. The idea of a two-tailed p-value is foreign to the Bayesian paradigm, so we calculate the proportion of the posterior distribution that lies on the same side of zero as the posterior mean. We refer to this as the Bayesian p-value or the posterior probability. We calculate 80% credible intervals that allow visual tests against zero at the 90% Bayesian p-value level. In the interest of parsimony, we present a subset of these models in the paper - only those on the political trust index, group activity and civic activism.

Table A4: Negative Binomial Models of Amendment Ratification

| Predictor | Government Confidence | Party Confidence | Court Confidence | Group Member. | Civic Activism | Pol. Trust Index |
|---|---|---|---|---|---|---|
| Const. Rigidity | -1.72* | -1.89* | -1.50* | -1.45* | -1.52* | -1.76* |
| | (-2.40, -1.01) | (-2.61, -1.13) | (-2.19, -0.78) | (-2.24, -0.62) | (-2.24, -0.78)) | (-2.46, -1.06) |
| Social Capital | 0.71* | 0.60 | 0.36 | 0.14 | 1.29* | 0.20* |
| | (-0.04, 1.51) | (-0.40, 1.64) | (-0.23, 0.99) | (-0.12, 0.40) | (0.80, 1.78) | (-0.04, 0.45) |
| log(# Words) | 0.55* | 0.69* | 0.55* | 0.62* | 1.02* | 0.61* |
| | (-0.02, 1.12) | (0.12, 1.29) | (-0.02, 1.13) | (0.03, 1.23) | (0.50, 1.56) | (0.03, 1.19) |
| Intercept | -3.81* | -3.81* | -3.29* | -2.98* | -5.18* | -2.38* |
| | (-6.75, -0.94) | (-7.27, -0.57) | (-6.18, -0.58) | (-5.59, -0.44) | (-7.62, -2.85)) | (-4.87, 0.04) |
| Shape: Intercept. | 7.31* | 8.01* | 7.66* | 2.45* | 1.72* | 0.88 |
| | (2.34, 12.53) | (2.63, 13.82) | (2.77, 13.44) | (0.25, 4.83) | (-0.42, 3.92) | (-0.81, 2.54) |
| Shape: Const. Rigid | -0.17 | 0.13 | 0.02 | -0.66 | -1.27 | -0.00 |
| | (-2.04, 1.70) | (-1.83, 2.10) | (-1.87, 2.05) | (-2.71, 1.47) | (-3.44, 0.90) | (-1.81, 1.86) |
| Shape: Social Capital | -2.77* | -3.72* | -2.76* | -0.73* | 0.71 | -0.99* |
| | (-4.85, -0.76) | (-6.44, -1.12) | (-5.04, -0.82) | (-1.34, -0.17) | (-0.86, 2.20) | (-1.67, -0.34) |
| LOO IC (SE) | 342.1 (18.3) | 344.3 (19.7) | 343.9 (20.2) | 345.9 (19.2) | 331.8 (18.7) | 341.4 (18.9) |
| $R_{y,\hat{y}}$ | 0.82 | 0.80 | 0.85 | 0.87 | 0.90 | 0.82 |

**Notes:**
* indicates posterior probability greater than 0.9 on the same side of zero.
Dependent variable in all models is the number of constitutional amendments amendments ratified.
Number of observations in each model is 57.
LOO IC is an approximation to leave-one-out cross-validation - a comparative measure of model fit (akin to the AIC or DIC).
$R_{y,\hat{y}}$ is the correlation between observed and posterior mean fitted values.

Figure A1: Effects of Social Capital on Mean and Residual Variance



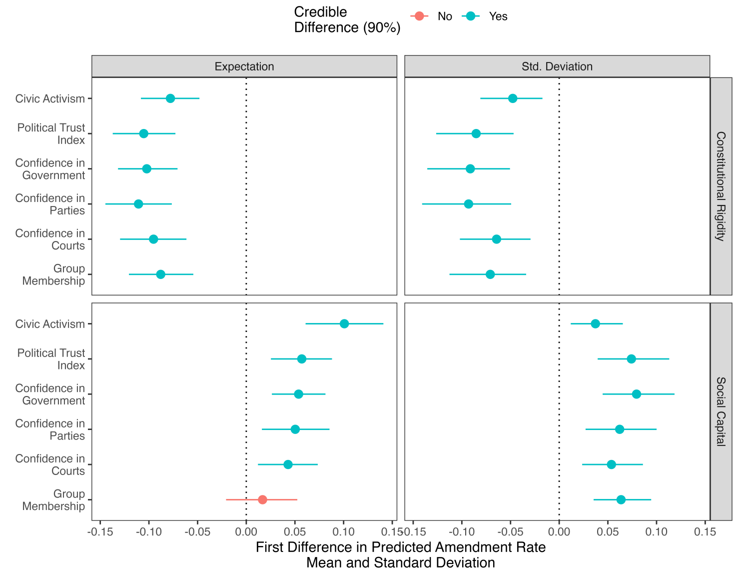

See Table A4 (models 1-6) for model results.

### 5.3.2 Replicating the Heteroskedastic Linear Model

We also produce the same results as above for a heteroskedastic linear model, like the one Tsebelis (2022) used. The main difference here is that we do so using Bayesian simulation rather than maximum likelihood estimation.

The heteroskedastic linear model produces quite similar results. In general, everything

Figure A2: Effects of Social Capital on Mean and Residual Variance from Heteroskedastic Linear Models




See Table A5 (models 1-6) for model results.

Table A5: Heteroskedastic Linear Models of Amendment Ratification - Cross-national Data

| Predictor | Government Confidence | Party Confidence | Court Confidence | Group Member. | Civic Activism | Pol. Trust Index |
|---|---|---|---|---|---|---|
| Const. Rigidity | -0.36* | -0.40* | -0.34* | -0.32* | -0.28* | -0.38* |
| | (-0.51, -0.22) | (-0.55, -0.24) | (-0.50, -0.19) | (-0.47, -0.17) | (-0.42, -0.14) | (-0.53, -0.23) |
| Social Capital | 0.19* | 0.21* | 0.13* | 0.02 | 0.31* | 0.06* |
| | (0.06, 0.33) | (0.01, 0.40) | (0.00, 0.25) | (-0.04, 0.08) | (0.15, 0.48) | (0.02, 0.11) |
| log(# Words) | 0.07 | 0.09 | 0.09 | 0.14* | 0.20* | 0.08 |
| | (-0.06, 0.19) | (-0.04, 0.23) | (-0.05, 0.24) | (-0.01, 0.28) | (0.07, 0.34) | (-0.05, 0.21) |
| Intercept | -0.13 | -0.18 | -0.15 | -0.08 | -0.52* | 0.27 |
| | (-0.77, 0.51) | (-0.91, 0.53) | (-0.91, 0.61) | (-0.72, 0.54) | (-1.16, 0.10) | (-0.34, 0.86) |
| Sigma: Intercept. | -3.21* | -2.48* | -2.43* | -1.14* | -1.22* | -0.42 |
| | (-4.67, -1.67) | (-3.86, -1.02) | (-3.84, -0.97) | (-1.81, -0.44) | (-1.89, -0.51) | (-1.03, 0.19) |
| Sigma: Const. Rigid | -1.39* | -1.37* | -1.02* | -1.12* | -0.86* | -1.33* |
| | (-2.10, -0.67) | (-2.11, -0.64) | (-1.70, -0.34) | (-1.80, -0.43) | (-1.59, -0.14) | (-1.99, -0.66) |
| Sigma: Social Capital | 1.24* | 1.09* | 0.72* | 0.36* | 0.59* | 0.34* |
| | (0.57, 1.88) | (0.36, 1.83) | (0.21, 1.24) | (0.16, 0.56) | (0.08, 1.11) | (0.16, 0.53) |
| LOO IC (SE) | -17.2 (14.5) | -7.7 (17.5) | -6.6 (17.3) | -11.6 (14.4) | -22.4 (13.5) | -13.7 (15.7) |
| $R_{y,\hat{y}}$ | 0.35 | 0.33 | 0.37 | 0.37 | 0.62 | 0.35 |

**Notes:**
* indicates posterior probability greater than 0.9 on the same side of zero.
Dependent variable in all models is the number of constitutional amendments amendments ratified.
Number of observations in each model is 57.
LOO IC is an approximation to leave-one-out cross-validation - a comparative measure of model fit (akin to the AIC or DIC).
$R_{y,\hat{y}}$ is the correlation between observed and posterior mean fitted values.

that produces credible differences in the negative binomial model does so here. However, there are some instances when the converse is not true. Both confidence in parties and confidence in courts reliably increase the expected rate of amendments. Likewise, group activity and civic activism tend to reliably decrease the residual variation here where they did not in the negative binomial models. We find these two sets of results largely mutually corroborative. To the extent that there are differences, we feel the NBRM should be preferred as it makes a more tenable set of assumptions about the data generating process.

### 5.3.3 Cross-national Longitudinal Models

In the cross-national, longitudinal models, the dependent variable is binary, indicating country-years in which *at least* one amendment was ratified. We employ a dichotomous dependent variable because other studies (Ginsburg and Melton 2015; Tsebelis 2022) assume that multiple amendments are combined into a single legislative package. This is the same raw data that motivated our previous cross-national analysis. The main difference is that in this model, we have a time-varying measure of social capital from the Varieties of Democracy (V-Dem) project (Coppedge et al. 2021). Since the dependent variable is binary, we use as a starting point the model described in Beck, Katz, and Tucker (1999). They suggest using a spline or spell dummies build a flexible model of the time dependence. Carter and Signorio (2010) suggest that for most applications a third-degree polynomial is sufficiently flexible, particularly when coupled with the lack of familiarity most quantitative scholars have with splines. Our model is well-suited to treating the spells as a random effect - this provides incredible flexibility in terms of functional form, but the shrinkage inherent in these models will keep sparsely populated spell values closer to the global mean of the random effect. Thus, it is a nice compromise between the (semi-)parametric model and the spell dummy variables leveraging the most beneficial features of each.

In the Beck, Katz and Tucker (1999) model, only the subset of the data where an $y_{i,t-1} = 0$ is used. This operationalizes the survival model that is being replicated in the logistic regression setup. As a reminder, a survival model tries to estimate the hazard - the probability that an event happens at time $t$ conditional on it not having already happened. This makes the most sense in the health settings in which this model was developed, where the event is death or illness. Presumably if you're dead at time $t$ you will continue to be dead at time $t + 1$. Illness is a bit trickier, but presumably if you are ill at time $t$ and also ill at time $t + 1$, we could consider this time-frame as belonging to the same illness. With the case of amendments, the logic is even less clear. If an amendment is ratified at time $t$ and at time $t + 1$, we know for certain those are *different* amendments. It is possible to consider amendment spells as times where a society is particularly prone to ratify amendments - enough so that it continues to ratify amendments each year for several years in a row. This may be a theoretical curiosity, though empirically it is difficult to test.

In addition to modeling the spell variable with a random effect, we also adopt the advice (at least in part) of Bell and Jones (2015). They suggest using both within- and between-transformations of the variables of interest. This permits the disentanglement of the between- and within- effects of those variables generating a more nuanced set of findings. The between-transformation of a variable $X$ with observations $i$ in groups $j$ is just:

$$\bar{x}_{j.} = \frac{1}{n_j} \sum_{i=1}^{n_j} x_{ij}$$

The within-transformation simply subtracts the between transformation from the observations value, $x_{ij}$. We will denote this as follows:

$$x_{ij}^{(w)} = x_{ij} = \bar{x}_{j.}$$

We can then specify the model as:

$$\log\left(\frac{Pr(y_{it}=1)}{1-Pr(y_{it}=1)}\right) = \beta_{0i} + \beta_1 \text{Social Capital}_{it}^{(w)} + \beta_2 \text{Const. Rigidity}_{it}^{(w)} \times \text{Time-Varying}_i + \theta_s$$
$$\beta_{0i} = \gamma_{00} + \gamma_{01}\text{Time Varying}_i + \nu_{i0}$$

where $\theta_s$ is the random effect for the spell variable. In this iteration of the model, constitutional rigidity has within-variation for 16 countries. We use an interaction with a binary indicator identifying countries that have time-varying constitutional rigidity measures. We do so in a way that is often ill-advised (Berry, Golder, and Milton 2012), though in this particular application it makes sense. The conventional advice is to include all constitutive terms in the interaction (which would have us include the non time-varying values of constitutional rigidity as well). However, in this case since there is no time-varying information to be leveraged, the effect of the within-transformed rigidity variable for the countries without time-varying information is (and must be) precisely zero. Our model captures this.

In Model 1, we estimate the effect for all countries in the data, with an interaction for those with time-varying information on constitutional rigidity. In Model 2, we subset the data to include *only* those countries with time-varying information on constitutional rigidity. This is meant as a robustness check for the effect of social capital.

In Models 3 and 4, we replicate Models 1 and 2, but include several of the control variables used by Tarabar and Young (2021) - namely, executive constraints, the log of GDP/capita, GDP growth, conflict and ethnic fractionalization. We subject these variables to a within-country transformation. The random intercept will pick up any important between-country variation that may be partly explained by the country averages of these variables. For two countries, India and Slovenia, we do not have ethnic fractionalization measures, so instead of removing them from the analysis, we simply set their within-transformed ethnic fractionalization measures to zero. This would indicate a constant ethnic fractionalization rate within country, though it is silent on the level at which that rate is constant.

We find that this effect is of roughly the same size. The only important difference exists between Models 2 and 4, where the effect of Social Capital becomes nearly zero when including controls for the countries with time-varying constitutional rigidity measures. Given that this is a small subset of countries in the main analysis, we do not see this as particularly troublesome, as the main finding holds moving from Model 1 to Model 3.

Nominally, we are estimating the probability of an amendment being ratified at time $t$, but we can also predict the expected amount of time it would take for an amendment to get ratified under different scenarios. Smaller intervals until amendments are ratified means a greater number of amendments ratified over a country's lifespan. We use a similar idea to the one described above to calculate the effect of variables in the model. This model is more complex than the previous one and as such permits the comparison of two quantities of interest.

Table A6: Estimates of Cross-national Longitudinal Binary TSCS Models

| Term | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Intercept | -1.43* | -7.77* | -1.28 | -7.92* |
| | (-3.67, 0.79) | (-13.82, -1.67) | (-3.65, 1.03) | (-14.63, -1.33) |
| Soc. Capital (within) | 4.24* | 4.57* | 3.49* | -0.28 |
| | (2.47, 6.06) | (1.69, 7.76) | (0.61, 6.55) | (-7.26, 6.98) |
| Const. Rigidity (within) | -3.46* | -3.01* | -3.77* | -3.65* |
| | (-7.49, 0.23) | (-6.63, 0.46) | (-7.95, -0.08) | (-7.62, 0.15) |
| Const. Rigidity (between) | -2.81* | -1.29* | -3.15* | -2.14* |
| | (-4.53, -1.25) | (-2.95, 0.09) | (-4.97, -1.47) | (-4.20, -0.45) |
| log(# Words) | 0.19* | 0.72* | 0.20* | 0.80* |
| | (-0.01, 0.41) | (0.16, 1.31) | (-0.01, 0.42) | (0.20, 1.44) |
| Time Varying | 0.18 | | 0.21 | |
| | (-0.61, 0.99) | | (-0.65, 1.14) | |
| Exec. Const. (within) | | | -0.29 | -0.43 |
| | | | (-0.73, 0.18) | (-1.74, 0.84) |
| log(GDP/capita) (within) | | | 0.35* | 1.16* |
| | | | (-0.08, 0.78) | (-0.01, 2.33) |
| GDP Growth (within) | | | 3.09* | 3.95* |
| | | | (-0.11, 6.34) | (-2.09, 10.24) |
| Conflict (within) | | | 0.70* | 0.59 |
| | | | (-0.28, 1.68) | (-0.95, 2.15) |
| Ethnic Frac. (within) | | | -1.94 | -4.14* |
| | | | (-6.00, 1.94) | (-10.61, 1.77) |
| sd(Country RE) | 1.23* | 0.45* | 1.32* | 0.47* |
| | (0.94, 1.60) | (0.02, 1.12) | (1.00, 1.72) | (0.03, 1.16) |
| sd(Spell RE) | 0.11* | 0.22* | 0.11* | 0.21* |
| | (0.00, 0.35) | (0.01, 0.67) | (0.00, 0.35) | (0.01, 0.66) |
| N | 2002 | 569 | 1529 | 397 |
| N Countries | 80 | 16 | 78 | 16 |
| LOO IC (SE) | 1659.6 (52.0) | 445 (30.1) | 1327.5 (44.7) | 385 (25.8) |
| PRE | 0.12 | 0.06 | 0.12 | 0.10 |

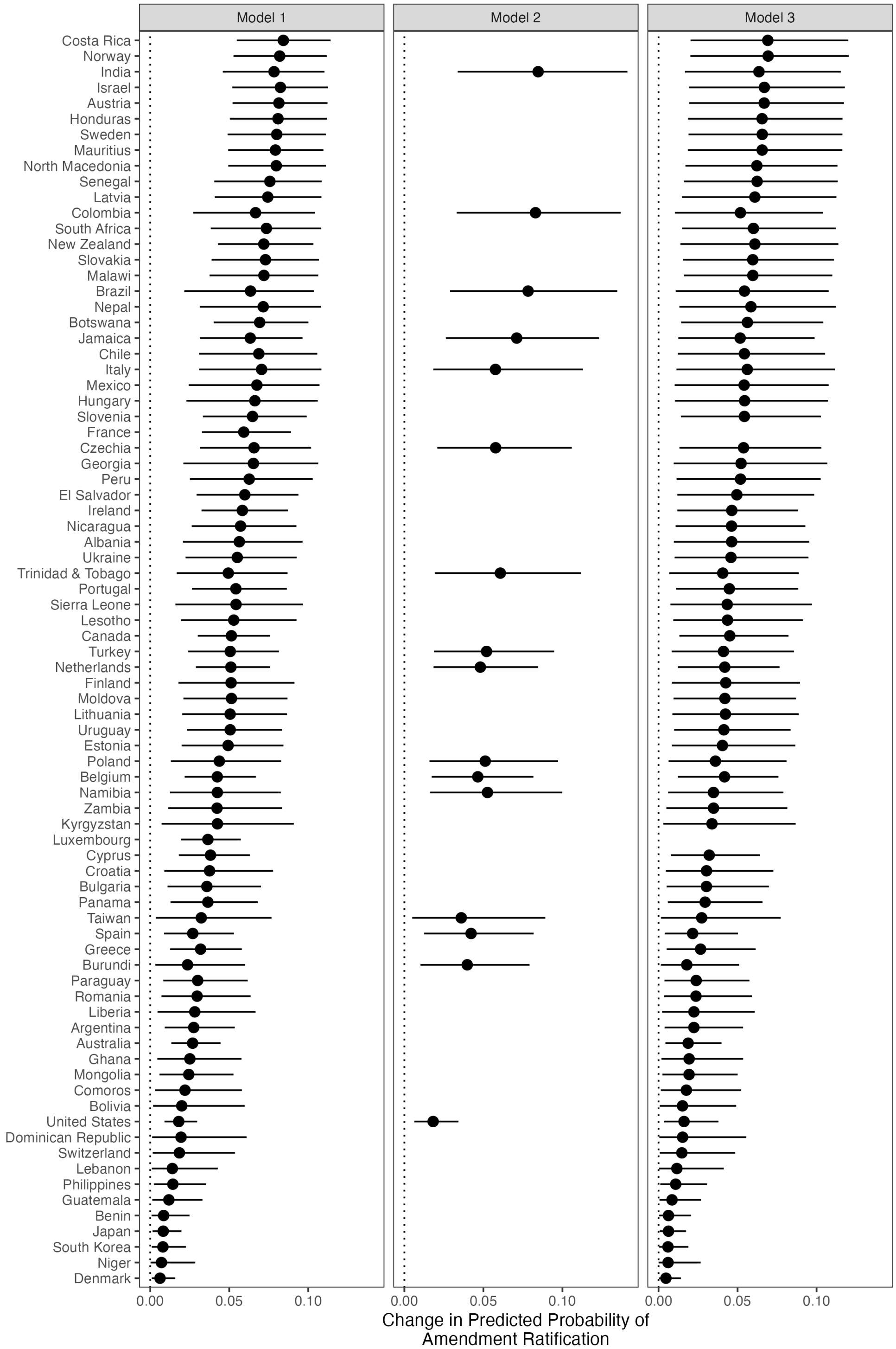
**Notes:**
* indicates posterior probability greater than 0.9 on the same side of zero.
Dependent variable in all models is whether a constitutional amendment happened in each year (0/1).
LOO IC is an approximation to leave-one-out cross-validation - a comparative measure of model fit (akin to the AIC or DIC).
PRE is the proportional reduction in error

### 5.3.4 Change in Probability of Ratification

First, we can calculate the change in the probability of ratification for each country year and average those changes across all years of the country's democratic history since the enactment of its current constitution.

Of the 80 countries included in the data, 78 have changes in predicted probability (in the aggregate) that have posterior probability greater than 0.9. The differences here range from

Figure A3: Effects of Social Capital on the Probability of Ratification

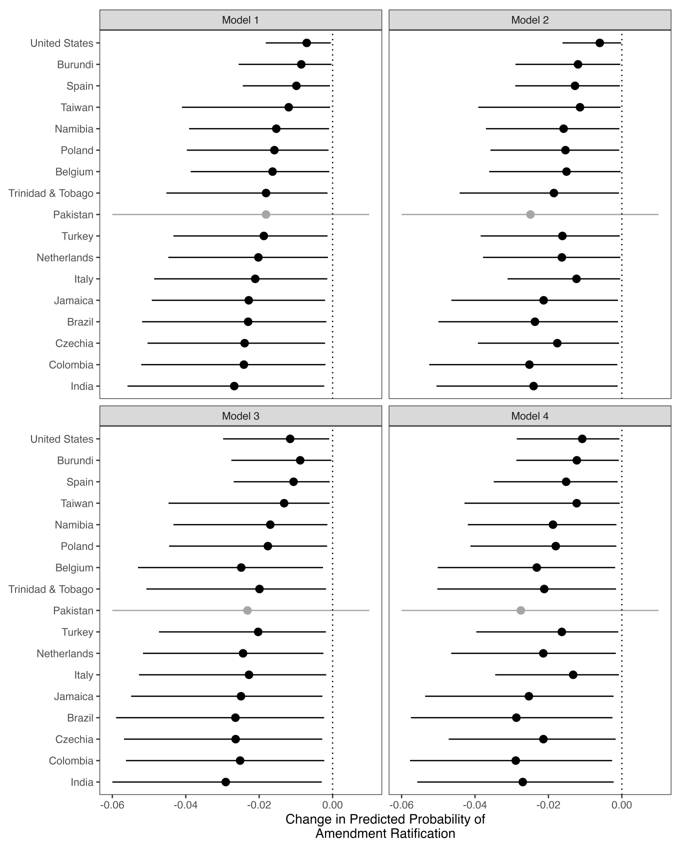



See Table A6 (models 1-3) for model results.

below 0.01 (e.g., Denmark, Niger and South Korea) to more than 0.08 (e.g., Israel, Austria, Honduras and Costa Rica). We could also look at this in the second model, using only the observations with time-varying values of constitutional rigidity. Figure A6 identifies those countries and the change in probabilities from Models 1, 2 and 3. The findings across the three models are quite similar. There are only two countries (France and Luxembourg) that had credible effects in Model 1 but not Model 3. Further, the ordering of effects is quite similar, which would suggest no major differences in the random effects across the two models. This is further corroborated by the similar standard deviations of the fixed effects across corresponding models with and without controls.

We could do the same thing for the constitutional rigidity measure. Using the within-country measure, each of the 16 countries included in the data have changes in predicted probability (in the aggregate) that have posterior probability greater than 0.9. On average these credible changes in predicted probability range from United States with a change of -0.01 to India with a change of -0.03. We also do this for Model 2, with only time-varying observations on constitutional rigidity and find that the effects are quite similar. Figure A7 also shows the same results for Models 3 and 4 which include control variables.

Figure A4: Effects of Constitutional Rigidity on the Probability of Ratification




See Table A6 (models 1-4) for model results.

The results across the four models displayed in Figure A7 are quite similar. In each panel, the results are ordered by the findings from Model 1. To the extent that the results change ordering, that indicates a change in the random effect - resulting in larger or smaller effects given the same data and similar sized coefficients. We see this is largely corroborating the importance of constitutional rigidity across models.

We also estimated models that parallel Models 1-4, but include a referendum dummy variable and an interaction with social capital. We find no strong evidence of an interaction effect. Table A7 shows the results of these models.

Table A7: Models with Refernedum-Social Capital Interaction

| Term | Model 5 | Model 6 |
|---|---|---|
| Intercept | -5.43* | -5.71* |
| | (-10.16, -1.47) | (-10.98, -1.05) |
| Const. Rigidity (within) | -3.52* | -3.88* |
| | (-7.73, 0.27) | (-8.18, -0.03) |
| Soc. Capital (within) | 3.38* | 5.12* |
| | (1.22, 5.68) | (1.07, 9.40) |
| Referendum | -0.07 | -0.36 |
| | (-0.60, 0.50) | (-1.01, 0.26) |
| Soc. Capital (within):Referendum | -0.62 | -2.36 |
| | (-5.26, 3.87) | (-8.47, 3.52) |
| Const. Rigidity (between) | -2.62* | -2.81* |
| | (-4.31, -0.98) | (-4.63, -1.05) |
| log(# Words) | 0.60* | 0.66* |
| | (0.21, 1.06) | (0.22, 1.18) |
| Time Varying | -0.03 | -0.08 |
| | (-0.83, 0.78) | (-0.96, 0.73) |
| Exec. Const. (within) | | -0.20 |
| | | (-0.75, 0.36) |
| log(GDP/capita) (within) | | 0.29* |
| | | (-0.14, 0.72) |
| GDP Growth (within) | | 3.20* |
| | | (-0.07, 6.69) |
| Ethnic Frac. (within) | | -2.20 |
| | | (-6.37, 1.79) |
| Conflict (within) | | 1.02* |
| | | (-0.09, 2.13) |
| sd(Country RE) | 1.18* | 1.24* |
| | (0.88, 1.55) | (0.92, 1.63) |
| sd(Spell RE) | 0.11* | 0.12* |
| | (0.00, 0.33) | (0.00, 0.40) |
| N | 2002 | 1529 |
| N Countries | 80 | 78 |
| LOO IC (SE) | 1570.3 (50.8) | 1259.9 (44.3) |
| PRE | 0.13 | 0.15 |

**Notes:**
* indicates posterior probability greater than 0.9 on the same side of zero.
Dependent variable in all models is whether a constitutional amendment happened in each year (0/1).
LOO IC is an approximation to leave-one-out cross-validation - a comparative measure of model fit (akin to the AIC or DIC).
PRE is the proportional reduction in error

The first indication of additive effects is that the interaction term is not statistically significant in either model, though it is in the proposed negative direction). Following Berry et. al. (2010), we calculate the second difference in probabilities with respect to the social capital and referendum. In neither model do we find that the average second difference is credible at the 90% level.

In the cross-national analysis, we added in cultural controls because it was plausible that controlling for those variables could change our results. In some cases they did and in other cases they did not. The same cannot be said of our cross-national longitudinal analysis. Here, we explicitly use a within-country estimator to estimate the effect of social capital. As such, any country-level differences are eliminated by design, including time-invariant cultural factors. Including the cultural variables in these models could reduce the country-level random effect variance, but would not change at all the within country effects estimated in the model. Since we are not interested in the between-country effects, we choose not to burden the reader with another set of results that cannot be meaningfully different from the ones we presented above.

#### 5.3.5 State Amendment Models

The final analysis uses amendment proposal and ratification rates in the US states. The benefit here is that many aspects that varied in the cross-national setting will naturally be held constant in this analysis. That said, there is still considerable variation in contexts across states. The analysis here comprises two separate models - one of amendment proposal and one of amendment ratification. In the proposal stage, a negative binomial model of event counts is estimated. In this model, there is no exposure or offset term because theoretically the number of amendments proposed is unlimited, at least in most states. Arkansas, Colorado, Illinois, Kansas, and Kentucky all have some limits on the number of amendments that can be placed on ballots. One way to deal with this is to treat the limited states as having exposure terms equal to the limit and those without limits as having very high exposure values (e.g., 10,000). This, however does not really capture reality. Even in states without limits, the time, energy and political capital required to move amendments to the ballot suggest that there is some effective limit that is well below $\infty$ or even $10,000$. So, we calculate the model with all states without an offset. We also estimated a model only on those states without ballot limits. The results are substantively similar, so the ballot limits are not driving our findings in a meaningful way.

As above, we calculate within-state variables and between-state variables for both constitutional rigidity and social capital. We started with the same specification as above for the amendment ratification model. We estimated random slopes for social capital but found that the residual standard deviations were very small. We retain the random intercept for both the expectation and overdispersion terms. Further, at the proposal stage, there is very little within-state variation for constitutional rigidity. The variation is small enough that the modeled is rendered unable to generate meaningful estimates of the variable's effect. As such, we move to including the between-state effect to capture the relevant differences in constitutional rigidity. Specifically, we estimate the model below for amendment proposals:

$$
\begin{aligned}
y_{ij} &\sim NB(\mu_{ij}, \phi_{ij}) \\
\log(\mu_{ij}) &= \beta_{0j} + \beta_1 \mathrm{VP}_{ij}^{(w)} + \beta_2 \mathrm{SC}_{ij}^{(w)} \\
b_{0j} &= \gamma_{00} + \gamma_{01} \mathrm{VP}_{j}^{(b)} + \gamma_{02} \mathrm{SC}_{j}^{(b)} + \gamma_{03} \log(\mathrm{Words}_j) + \nu_{0j} \\
\log(\phi_{ij}) &= \theta_{0j} + \theta_1 \mathrm{SC}_{ij}^{(w)} \\
\theta_{0j} &= \delta_{00} + \epsilon_j
\end{aligned}
$$

where $\mu$ is the expected value and $\alpha = \frac{1}{\phi}$ is the overdispersion parameter. Both parameters are modeled as a function of the independent variables of interest.

For the ratification stage, we estimated the following model:

$$
\begin{aligned}
y_{ij} &\sim NB(\mu_{ij}, \phi_{ij}) \\
\log(\mu_{ij}) &= \beta_{0j} + \beta_2 \mathrm{SC}_{ij}^{(w)} + \log(\#\text{Proposed Amendments}) \\
b_{0j} &= \gamma_{00} + \gamma_{01}\mathrm{VP}_j^{(b)} + \gamma_{02}\mathrm{SC}_j^{(b)} + \gamma_{03}\log(\mathrm{Words}_j) + \nu_{0j} \\
\log(\phi_{ij}) &\sim N(\delta_{00}, \sigma^2)
\end{aligned}
$$

After some model diagnostic checking, we arrived at the specification described above. We remove the within-effect of constitutional rigidity on amendment ratifications because it introduces a considerable amount of noise due to lack of variation and does not change the findings otherwise (only Florida changed its amendment rules during the timeframe covered in this dataset). We also removed the parameterization of the overdispersion parameter as none of the variables had a credible effect on the overdispersion. This suggests that the residual variance is well-captured by the parameterized mean and single overdispersion parameter.

Table A8: Results for Proposal and Ratification Models

| Term | Proposal (All Obs) | Ratification (All Obs) | Proposal (No Limits) | Ratificaiton (No Limits) |
|---|---|---|---|---|
| Intercept | -9.34* | 2.27* | -9.87* | 2.90* |
| | (-13.03, -5.63) | (0.05, 4.69) | (-13.65, -6.17) | (0.39, 5.80) |
| Const. Rigidity (within) | -0.36 | | 0.16 | |
| | (-2.01, 1.26) | | (-1.62, 1.89) | |
| Social Capital (within) | 0.10* | 0.03 | 0.07* | 0.03 |
| | (0.01, 0.19) | (-0.05, 0.11) | (-0.02, 0.17) | (-0.05, 0.11) |
| Const. Rigidity (between) | 0.36 | -4.43* | 0.47 | -5.57* |
| | (-1.50, 2.27) | (-9.15, -0.23) | (-1.41, 2.47) | (-11.27, -0.80) |
| Social Capital (between) | -0.01 | -0.10* | -0.01 | -0.11* |
| | (-0.24, 0.23) | (-0.18, -0.01) | (-0.26, 0.24) | (-0.19, -0.02) |
| log(# Words) | 0.96* | -0.03 | 1.01* | -0.04 |
| | (0.68, 1.23) | (-0.11, 0.04) | (0.73, 1.30) | (-0.11, 0.03) |
| Intercept ($\sigma$) | 2.08* | | 1.99* | |
| | (1.45, 2.92) | | (1.34, 2.89) | |
| Social Capital (within, $\sigma$) | 0.72* | | 0.72* | |
| | (0.12, 1.40) | | (0.14, 1.38) | |
| sd(State RE) | 0.56* | 0.04* | 0.54* | 0.04* |
| | (0.42, 0.74) | (0.00, 0.11) | (0.39, 0.73) | (0.00, 0.12) |
| sd(State RE [Shape]) | 1.36* | | 1.37* | |
| | (0.75, 2.21) | | (0.73, 2.27) | |
| N | 576 | 452 | 516 | 405 |
| N States | 48 | 47 | 43 | 42 |
| LOO IC (SE) | 2367.9 (50.9) | 1463.8 (26.5) | 2169.4 (48.0) | 1340.1 (25.9) |
| $R_{y,\hat{y}}$ | 0.88 | 0.97 | 0.88 | 0.97 |

**Notes:**
* indicates posterior probability greater than 0.9 on the same side of zero.
All models are negative binomial models with state random effects in both the mean and variance equations. Dependent variable in "Proposal" models is the number of proposed amendments.
Dependent variable in "Ratification" models is the number of ratified amendments.
LOO IC is an approximation to leave-one-out cross-validation - a comparative measure of model fit (akin to the AIC or DIC).
$R_{y,\hat{y}}$ is the correlation between observed and posterior mean fitted values.

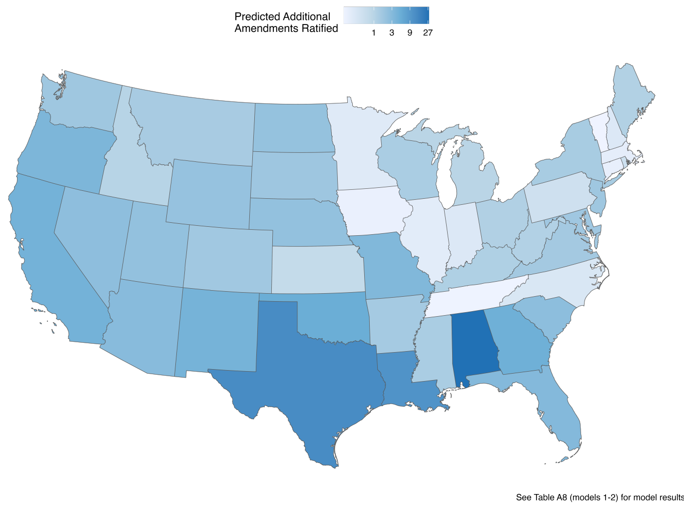
These suggest that social capital has a small-ish effect in the proposal stage, but nothing in the amendment ratification stage. What we will see below is that at the proposal stage, the small effects can really accumulate over time to produce relatively large changes in the number of amendments proposed over the life of the constitution. In the simulation below, we first simulate the effect of a one standard deviation change in social capital in the same way as above. That produces two different sets of estimates - one under a lower social capital scenario and one under a higher social capital scenario. We then use the posterior means of those two predictions (rounded to the next highest integer) as the exposures in our predictions for Model 2. Again, we generate two sets of predictions - one under a lower level of social capital for everyone, using the lower social capital predictions from Model 1 as the exposure term, and one under a higher level of social capital for everyone, using the higher social capital predictions from Model 1 as the exposure term. This captures the total effect of social capital across the two models.

The figure below gives the change in the number of amendments ratified by state (aggregating across all years) for a one standard deviation change in within-state social capital. It is largely the Southern states that see a big increase in additional amendments with Alabama predicted to see more than 28 extra amendments proposed as a function of changes in social capital. Texas and Louisiana are also expected to experience double-digit increases (13 and 11, respectively). The changes here are due primarily to the increased number of amendments that are proposed over time. In this case, 32 of the 47 effects are statistically reliable. Specifically, predicted values bigger than 0.85 have posterior probabilities greater than 0.9.

One potential problem with the analysis above is that it does not incorporate the uncertainty in the first-stage predictions. We may imagine that this would be the appropriate thing to do, but there are arguments to the contrary as well. Since we built the first-stage model to test a hypothesis and not to produce a "good" model with the smallest posterior variability, there may be some unnecessary noise in those predictions owing to extraneous variables included. Nonetheless, we estimate the simulation this time, not using the posterior means but for each draw of the posterior for the predicted number of proposed amendments.

The figure below shows the result of this different simulation. Here, we get $\hat{P}^{-}_{i,t}$ and $\hat{P}^{+}_{i,t}$, which are the predicted proposal rates for each of the $i$ states in each of the $t$ years given a half standard deviation decrease in social capital (with a $-$ superscript) or a half standard deviation increase in social capital (with a $+$ superscript). We get one of these for each posterior draw from the MCMC algorithm. Next, we calculate $\hat{R}^{-}_{i,t}$ and $\hat{R}^{+}_{i,t}$, which is the predicted number of ratified amendments given a half standard deviation plus or minus in social capital *and* accounting for the change in proposed amendments. That is, $\hat{R}^{-}_{i,t}$ is calculated using $\hat{P}^{-}_{i,t}$ as the number of proposed amendments. The same is true for the $+$ superscript variables. This provides a much better accounting of the overall effect of social capital on ratifications through the proposal process. We find that increase in proposals translate into increases in predicted changes in ratifications. Alabama is expected to ratify roughly 28 more amendments over the years covered if social capital increases by a standard deviation. Of these 47 results, 22 are statistically reliable, with posterior probabilities greater than 0.9.

Figure A5: Effect of Social Capital on State Constitutional Amendment Ratifications (alternative simulation)




## 5.4 Comparison with Tarabar and Young

One reviewer posed the question - what do we learn here that we did not already know from Tarabar and Young (2021)? To answer this question comprehensively, we must think about the nature of the social science research process in its entirety. We feel that our analysis is superior in several different ways.

### 5.4.1 Theory and Conceptualization

During the review process, there was a suggestion that our concept of social capital is really just a repackaging of Tarabar and Young's *amendment culture* concept. Theoretically, these are different ideas. Amendment culture, as defined by Ginsburg and Melton (2015) and used in Tarabar and Young (2021, 2) is "the set of shared attitudes about the desirability of amendment." We define *social capital* in this context as the "trust, reciprocity and civic activity produced by interpersonal networks [that] help elites, ordinary citizens, and social movements overcome the transactions costs" of ratifying amendments. These ideas are likely related, though culture is something that changes slowly over generations whereas social capital is more variable over time. If we are right, then higher levels of social capital *within a constitutional regime* should coincide with higher rates of constitutional amendments. Since culture is a much stickier concept, it will not be able to explain a phenomenon that changes in relatively small time-scales. The culture argument would have us believe that all of that variation across time is simply idiosyncratic. We feel that an adequate explanation of amendment rates must do better than that.

### 5.4.2 Measurement

Both our social capital measures and Tarabar and Young's amendment culture variables are derived from surveys. To some degree, the quality of the survey determines the quality of the measurement. Our social capital measures in the cross-national analysis are derived from the WVS, one of the most rigorous and longest-running cross-national surveys in the world.

The Hofstede indicators were originally collected from surveys of employees of IBM subsidiaries in 40 countries from 1966-1973.[3]. Hofstede stresses that for cross-country comparison, samples should be "matched" meaning that the same kinds of respondents should be used in each country, and while the scores may not replicate (i.e., students may have different values than grandparents), the inter-country differences can be reliably estimated. This strains credibility a bit. We could imagine that, on average, students or employees of a huge multinational corporation may exhibit similar patterns across countries, but would that hold for all groups? What about farmers, for example? If we could survey farmers in each of these countries, would these same cultural patterns obtain? Would we expect them to be present? Would not a nationally representative measure require a nationally representative sample? Assuming that students, corporate employees and farmers might respond differently, a nationally representative measure would depend on the relative prevalence of each group in society. Since societies have different proportions of students, corporate employees, farmers (and all other groups), we could not possibly expect these matched samples to produce something that is nationally representative.

Furthermore, for some unexplained reason, the Hofstede data are non-replicable. On his website, Geert Hofstede warns, "Some of the dimension scores obtained in replication studies fall outside the 0-100 continuum."[4] Finally, scholars have criticized these Hofstede measures for presuming that national culture does not change over time, overestimating the number of cultural dimensions, and misinterpreting their meaning (Ailon 2008; Baskerville 2003; Baskerville-Morley 2005; Fang 2003; McSweeney 2002, 2009; Taras, Steel, and Kirkman 2012; Venaik and Brewer 2016).

On the whole, we feel that our survey-derived measures stand on firmer ground.

### 5.4.3 Modeling

So far, we have argued that both our conceptual framework that accounts for time-varying changes in amendment rates and our measures derived from the most rigorous cross-national survey project to date are superior to those used in Tarabar and Young (2021). The next question concerns the model. Tarabar and Young use an ordinary least squares (OLS) model to estimate amendment rates. We address the problems with using OLS on amendment rates elsewhere in the appendices, particularly with respect to Tsebelis' use of the heteroskedastic linear model. Those concerns, and others, are the same here. Consider model 8 in Table 4 (p. 12 of Tarabar and Young) and model 6 in Table 5 (p. 13 of Tarabar and Young). These models have 89 and 72 observations representing 43 and 35 countries, respectively. These models have a very high ratio of estimated parameters to number of observations, or to put

[3] See: `https://geerthofstede.com/research-and-vsm/vsm-2013/`.

[4] See https://geerthofstede.com/research-and-vsm/dimension-data-matrix/.

it differently, very few residual degrees of freedom. Model 8 (Table 4) estimates an intercept, 14 coefficients and six region fixed-effects for a total of 21 parameters. This is a lot to ask of 89 data points, especially since some of the measures will not vary within country, i.e., across constitutional regimes. Model 6 (Table 5) is even worse, estimating the 21 parameters in Model 8 plus six, for a total of 27 parameters on 72 observations - fewer than 3 observations per parameter. In a simple simulation, you can show that if you randomly generate $X$ with 27 variables and $n = 72$, and than randomly generate a $y$ that has no relationship to those 27 variables, a regression of $y$ on $X$ will yield an $R^2$ of, on average around .4 with, on average, one or two variables being significant and can be as high as around 0.7 with as many as 11 or 12 variables being significant.

We use a Bayesian negative binomial model that estimates seven parameters on 57 observations, using roughly 8 observations per parameter, which seems considerably more reasonable. We extol the virtues of that model elsewhere in the appendices. However, if we had done the same kind of simulation described above with 57 observations and 7 parameters, it would produce an OLS model with an average $R^2$ of around 0.1 with, on average, a single significant variable.

Comparing model performance is difficult, as the question cannot be answered in the conventional way - a kitchen-sink model containing both sets of variables. This would simply result in models that we feel would use too many degrees of freedom. There is not enough Hofstede data available to make statistically-reliable comparisons between the two approaches. Only 31 democracies in Tarabar and Young's sample also exist in ours, and only 24 of these have data for all four Hofstede culture dimensions. Furthermore, the democracies analyzed by Tarabar and Young are not a random sub-sample of the democracies included in our sample. Several nations that are important cases in any study of democratic constitutionalism are missing from Tarabar and Young's (2021) sample, including: France, India, Israel, Japan, and the United States. Deriving insights from such a model could easily lead us in the wrong direction.

## 5.5 References

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