The Institutional Foundations of the Uneven Global Spread of Constitutional Courts

Dongwook Kim
The Australian National University, Australia

Paul Nolette
Marquette University, USA

Perspectives on Politics

Supplementary Material (Online Appendix)

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Table OA1. Hypotheses and Summary Statistics

Variables	Hypothesis	Number	Mean	Standard deviation	Minimum	Maximum
Common law system	−	8,074	0.347	0.476	0	1
Venice Commission engagement	+	8,074	0.008	0.089	0	1
Political insurance	−	8,057	0.616	0.400	0.000	1.000
Regional contagion	+	8,074	0.828	1.192	0.000	6.278
Recent democratic transition	+	8,066	0.031	0.174	0	1
Presidentialism	−	7,954	0.501	0.321	0.008	0.995
GDP per capita	+	8,074	23.672	2.575	9.411	30.498

Testing the Proportional Hazards Assumption: The Cox Model Diagnostics

In table OA2, we test the proportional hazards assumption required for our main Cox model in the article. As a member of the proportional hazards family of event history models, the Cox model makes the proportional hazards assumption that the effects of explanatory variables remain constant over time. This proportionality test is essentially a model specification test verifying that the vector of explanatory variables in our statistical model has been adequately specified (Cleves et al. 2008, 197). In essence, the key point here is that our main Cox model in the article satisfies the proportional hazards assumption and therefore it is adequately specified, as seen in table OA2 below.

We test our Cox model's proportional hazards assumption both variable-specifically and globally. We compute the Schoenfeld residuals from the Cox model, regress them on the rank transformation of the duration time,[1] and then test the null hypothesis of a zero slope (that is, no relationship between the residuals and time). *Non*rejection of the null hypothesis indicates that our model satisfies the proportional hazards assumption (Box-Steffensmeier and Jones 2004, 135–36; Cleves et al. 2008, 200–2; Park and Hendry 2015, 1074–75). The variable-specific tests verify the proportionality of the effect of each explanatory variable on the baseline hazard rate, while the global test checks that of the joint effects of all explanatory variables in the model.

Table OA2 presents the results for the proportional hazards assumption tests. In essence, in both the variable-by-variable and the global tests, p-values are never statistically significant, so that we cannot reject the null hypothesis that hazards are proportional. Therefore, we can conclude that our main Cox model in the article is an adequately specified model that satisfies the proportional hazards assumption both variable-specifically and globally.

Table OA2. Testing the Proportional Hazards Assumption: The Cox Model Diagnostics

	ρ	χ^2 statistic	P-value
Variable-specific tests			
Common law system	0.14	1.96	0.162
Venice Commission engagement	0.00	0.00	0.982
Political insurance	−0.14	2.07	0.150
Regional contagion	−0.06	0.22	0.642
Recent democratic transition	0.03	0.06	0.808
Presidentialism	0.16	2.64	0.104
GDP per capita	0.06	0.30	0.586
Global test			
The whole model	—	5.48	0.602

Note: The variable-specific and global tests verify the proportionality of the individual and combined effects of the explanatory variables, respectively. In the variable-by-variable tests, ρ reports the correlation between the Schoenfeld residuals for a given explanatory variable and the rank transformation of time.

[1] Following Park and Hendry's (2015, 1079–80) advice, we utilize the rank transformation (that is, "the observed event times placed in integer-rank order") as the functional form of the duration time.

Robustness Checks

This supplementary material presents robustness checks against alternative operationalization of the dependent variable as competing events, endogeneity bias, omitted variable bias, and alternative operationalization of control variables that cannot be reported in full details in the article's *Robustness Checks* section due to the word limit. In essence, the key point here is that the article's main findings about domestic legal systems and the Venice Commission remain highly robust statistically and substantively, as seen below.

Robustness Checks against Alternative Operationalization of the Dependent Variable: Competing Events

In addition to the article's model 5 in table 1, we conduct additional robustness checks against alternative operationalization of the dependent variable, by continuing to follow Ginsburg and Versteeg's (2013, 14–17) operationalization, and by explicitly considering constitutional legislation for the Kelsenian model of judicial review and constitutional legislation for the American model of judicial review as "competing events." The Kelsenian and the American models can be regarded as competing events because national governments can choose between the two as a constitutional review mechanism during the same observation period, and because one model's adoption either prevents governments from adopting (that is, switching to) the other model altogether or alters the likelihood of government adoption of the other model.

The extant literature on event history analysis identifies three possible situations of competing events: namely, the two-step model, the cause-specific hazard model, and the subdistribution hazard model (that is, the Fine and Gray model). In the below, we explain why the Fine and Gray model is the most appropriate for analyzing constitutional legislation for the Kelsenian and the American models of judicial review as competing events.

First, what Yamaguchi (1991, 169–71) calls the two-step model of competing events assumes that government adoption of a constitutional review mechanism occurs in two steps. At the first step, a government decides whether or not to adopt a constitutional review mechanism via constitutional enactment or amendment. Once it has decided to do so, the second step is for the government to choose whether to make the adopted constitutional review mechanism Kelsenian or American. This two-step model makes *no* real-world sense at all when one works with Ginsburg and Versteeg's (2013) and Elkins and Ginsburg's (2021) text-based constitutional review data from the Comparative Constitutions Project. Their data measure not countries' constitutional drafting process but merely the final textual outcome, that is, the enacted or amended constitutional text. As such, the two-step model presumes that the government, parliamentarians, constitutional drafters, and other stakeholders will never specify or know which between the Kelsenian and the American models of judicial review is contained in their proposed constitutional text being voted on, until that constitutional text—with the constitutional review mechanism's type left unspecified—has been passed in the legislature and the national referendum as the first step. This is because, per this stepwise conjecture, deciding whether to make the adopted-but-unspecified constitutional review mechanism either Kelsenian or American will only occur as the second step. Indeed, Yamaguchi (1991, 170) rightly criticizes the two-step model for its lack of real-life plausibility by noting that "[i]n many cases, the two steps reflect a conceptual rather than an empirical distinction, since the occurrence of an event simultaneously determines which particular event occurs."

Second, the so-called cause-specific hazard model (Box-Steffensmeier and Jones 2004, 166–72; Singer and Willett 2003, 586–95; Yamaguchi 1991, 171) presumes that once national governments have passed constitutional legislation for one of the two competing—that is, Kelsenian versus American—models of judicial review, they will never be able to switch to the other model in the future because adoption of one model removes governments from the risk set for the other model's adoption as censored observations and thereby prevents them from adopting (that is, switching to) the other model altogether. This cause-specific hazard model is equally unrealistic in that like many other politico-institutional phenomena, the process of adopting a constitutional review mechanism is far from preordained or irreversible.

Finally, Fine and Gray's (1999) subdistribution hazard model allows for the possibility that even if national governments have adopted either of the Kelsenian and the American models of judicial review via constitutional legislation, they continue to remain in the risk set and hence eligible for experiencing the competing event because they can switch to the alternative model of judicial review in the future via constitutional amendment or replacement. In other words, a country's adoption and retention of the American model of judicial review does not prevent that country's future adoption of the Kelsenian model of judicial review altogether, but instead alters the likelihood of that country's switching to the Kelsenian model. Thus, the Fine and Gray model accounts for constitutional legislation for the Kelsenian model of judicial review pursued by both countries having no constitutional review mechanism whatsoever *and* those switching from the American model of judicial review. We argue that the Fine and Gray model is the most appropriate for analyzing constitutional legislation for the Kelsenian and the American models of judicial review as competing events because it explicitly accepts the possibility of institutional change and reversal.

Table OA3 presents the results for robustness checks against alternative operationalization of the dependent variable as competing events, based on Ginsburg and Versteeg's (2013, 14–17) operationalization and Elkins and Ginsburg's (2021) constitutional review data. Model 1 is the original, unstratified Fine and Gray model (Fine and Gray 1999) that estimates the determinants of the risk of constitutional legislation for the Kelsenian model of judicial review as the event of interest in the presence of constitutional legislation for the American model of judicial review as the competing event. The dependent variable for the event of interest is measured in the same way as in the article's model 5 in table 1, focusing on whether and when a country's constitutional text explicitly assigns the formal mandate of judicial review (that is, the interpretation of the constitution) to a constitutional court or council. The competing event codes whether each country's constitutional text expressly assigns the formal mandate of judicial review to "any ordinary court," "supreme court only" or "special chamber of the supreme court" in a given year (Elkins and Ginsburg 2021). Model 2 is the stratified Fine and Gray model (Zhou et al. 2011) that extends the Fine and Gray model by stratifying the repeated constitutional-legislation events data according to event number and hence allowing for event-specific baseline hazards.

Note that because Elkins and Ginsburg (2021) concentrate exclusively on constitutional texts in measuring the existence and type of countries' constitutional review mechanisms in a given year, their data code the United States as lacking any constitutional review mechanism because the US Constitution makes no explicit mention of constitutional review, despite the fact that judicial review is an American invention. They also classify other countries like Britain and New Zealand as equally lacking one. Furthermore, Ginsburg and Versteeg (2013, 16) exclude these old common law democracies from their statistical analysis altogether, acknowledging that "[o]ur approach excludes the small number of cases, chiefly the United States and Australia, whose constitutions do not explicitly provide for constitutional review power, even though courts exercise the power in practice." In contrast with Ginsburg and Versteeg (2013), however, our analysis includes these countries as originally measured by Ginsburg and Elkins' (2021) constitutional review data. This is because, given that Ginsburg and Versteeg's (2013) operationalization and Ginsburg and Elkins' (2021) data concentrate on constitutional texts alone in measuring whether and when countries adopt what kind of a constitutional review mechanism and choose to ignore the timing of such a mechanism's actual operation, there is no analytic justification for Ginsburg and Versteeg (2013) to arbitrarily discard the US and other similar countries based on anything other than constitutional texts themselves. In fact, Ginsburg and colleagues' treatment of the US and other old common law democracies as lacking a constitutional review mechanism is another main reason why it is important to go beyond the year of constitutional legislation and to actually measure the year of operation when analyzing governments' choice of constitutional review mechanisms.

In table OA3, in all cases, the results are essentially identical to those of the article's model 5 in table 1. The article's key findings about *common law system* and *Venice Commission engagement* remain unchanged, regardless of the use of alternative measurement of the dependent variable as competing events. Also, just like in the article's model 5 in table 1, the *political insurance* variable remains *opposite* of the hypothesized negative sign and statistically significantly so across both models 1 and 2 in table OA3, thereby challenging the validity of Ginsburg and Versteeg's (2013) political insurance theory.

Table OA3. Robustness Checks against Alternative Operationalization of the Dependent Variable as Competing Events: Determinants of Constitutional Court Establishment, 1947–2019

	Model 1	Model 2
	Fine and Gray model	Stratified Fine and Gray model
Common law system	−0.771**	−0.891***
	(0.323)	(0.337)
Venice Commission engagement	2.495***	2.236***
	(0.446)	(0.602)
Political insurance	1.606***	1.860***
	(0.550)	(0.556)
Regional contagion	0.259***	0.287***
	(0.075)	(0.080)
Recent democratic transition	1.526***	1.588***
	(0.394)	(0.382)
Presidentialism	0.428	0.173
	(0.600)	(0.609)
GDP per capita	−0.041	−0.039
	(0.033)	(0.036)
Number of states	154	154
Number of adoptions	86	86
Number of observations	5,668	5,668
Log likelihood	−685.77	−630.62
Wald χ^2	85.9***	76.8***

Note: Coefficients are reported. Numbers in parentheses are standard errors. All explanatory variables use a one-year lag. *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.10$, in two-tailed tests.

Robustness Checks against Endogeneity Bias, Part I: Simultaneity Bias

In the article and the supplementary material, we address three possible sources of endogeneity bias: namely, simultaneity bias (also known as reverse causation), unobserved country-level heterogeneity, and selection bias. First of all, as we explained in the article, in all of our statistical analyses, all explanatory variables are lagged by one year to ensure the correct temporal sequencing of the explanatory and the dependent variables and, hence, to reduce endogeneity bias arising from simultaneity bias (Box-Steffensmeier and Jones 2004, 111). Thus, *common law system* and *Venice Commission engagement* in the previous year explain constitutional court (non-)adoption in the current year.

Robustness Checks against Endogeneity Bias, Part II: Unobserved Country-Level Heterogeneity

Models 1 and 2 in table OA4 show the fixed-effects logistic models to demonstrate that the article's key findings about *common law system* and *Venice Commission engagement* are robust against endogeneity bias caused by unobserved time-invariant, country-level heterogeneity. We estimate the fixed-effects model, given the possibility that unobserved heterogeneity may be not only present but also correlated with the explanatory variables, and given Ginsburg and Versteeg's (2013, 12–13) concern with country fixed effects as a confounder. In doing so, we go beyond the traditional dummy variables approach to fixed effects (for example, Ginsburg and Versteeg 2013, 12–13), and instead utilize the between-within (or hybrid) approach that integrates the strengths of the fixed-effects and random-effects approaches and handles country fixed effects through decomposing the predictor-outcome association into its cross- and within-country dimensions (Allison 2009; Neuhaus and Kalbfleisch 1998).

As the first step of the between-within approach to country fixed effects, we calculate the country-specific mean of each and every time-varying explanatory variable, whether the variable is continuous or categorical. The second step is to compute the deviation by subtracting each mean variable from the corresponding time-varying explanatory variable. As Allison (2009) notes, the two steps should apply to time-varying dummy variables like *Venice Commission engagement* and *recent democratic transition* just like continuous variables. The first step generates seven mean variables that estimate the determinants of "cross-country" variations in the prospect of constitutional court establishment: namely, *mean.common law system*, *mean.Venice Commission engagement*, *mean.political insurance*, *mean.regional contagion*, *mean.recent democratic transition*, *mean.presidentialism*, and *mean.GDP per capita*. The second step produces six deviation variables that capture the determinants of "within-country" variations: namely, *deviation.Venice Commission engagement*, *deviation.political insurance*, *deviation.regional contagion*, *deviation.recent democratic transition*, *deviation.presidentialism*, and *deviation.GDP per capita*. Note that because *common law system* has no within-country, over-time variation, we can compute only *mean.common law system* (which is the same as *common law system*) but not *deviation.common law system*. As the last step, we include both the mean and the deviation variables as predictors and estimate the multilevel logistic regression model (that is, the random-effects logistic model) as recommended by Allison (2009).

Our between-within approach to fixed effects has important advantages over the traditional dummy variables approach (for example, Ginsburg and Versteeg 2013, 12–13). First, our approach helps to avoid sample selection bias (specifically, selection on the dependent variable). The dummy variables approach causes sample selection bias in the logistic and probit models whenever the country dummy perfectly predicts the dependent variable for a country for which a constitutional court creation has never occurred and hence excludes that country's entire observations from statistical estimation.[2] Indeed, Ginsburg and Versteeg (2013, 28, fn. 50) acknowledge that their statistical inference suffers from sample selection bias, by confirming that "we effectively remove from the analysis all the countries that never adopted constitutional review, thereby limiting our estimation to those which do."

Second, our approach helps to avoid the incidental parameters problem (Lancaster 2000). The dummy variables approach forces one to include dummies for our data's 172 countries, so that the logistic and probit models will have too many statistical parameters to estimate relative to the sample size and thereby lose statistical consistency. In contrast, our between-within approach requires us to include only six additional variables (that is, total 13 explanatory variables), thereby making our statistical analysis virtually free from the incidental parameters problem.

Third, whereas the traditional dummy variables approach fixates on only the within-country dimension of the constitutional court creation process (Ginsburg and Versteeg 2013), our between-within approach enables us to examine both the cross- and within-country dimensions of the adoption process simultaneously, while controlling for confounding country fixed effects. As such, the between-within approach facilitates a fuller and deeper understanding of the determinants of governments' choice of constitutional review mechanisms.

Finally, our between-within approach to fixed effects enables incorporating and estimating time-invariant explanatory variables in the statistical model that would otherwise be dropped by the traditional dummy variables approach, notably *common law system*. Indeed, unlike our analysis, Ginsburg and Versteeg (2013, 12–13) control away "whether or not a country has a common law legal heritage" as part of country dummies. However, as Bell and Jones (2015, 134) emphasize, "models that control out, rather than explicitly model, context and heterogeneity offer overly simplistic and impoverished results that can lead to misleading interpretations."

Table OA4 presents the results for robustness checks against endogeneity bias arising from unobserved country-level heterogeneity. Model 1 does not control for temporal dependence as per Allison's (2009) original specification, and model 2 includes the cubic polynomial approximation to temporal dependence—that is, *time*, *time*^{2}, and *time*^{3}—according to Carter and Signorino (2010a) and Ginsburg and Versteeg (2013, 13–14, especially fn. 12).[3] In essence, in both models, *mean.common law system* is negatively correlated with the cross-country variation in the hazard rate of constitutional court establishment, suggesting that cross-nationally, common law countries are less likely and later than their civil law counterparts to create a constitutional court. The Venice Commission's engagement is positively associated with both the cross- and within-country variations in the hazard rate of constitutional court adoption. The result for *mean.Venice Commission engagement* means that cross-nationally, a country engaged by the Venice Commission is more likely and earlier to adopt the Kelsenian model of judicial review than another country not engaged by the organization. The result for *deviation.Venice Commission engagement* indicates that as the Venice Commission begins to engage in a country, that country becomes more likely and earlier to establish a constitutional court within the national context over time. Furthermore, *mean.common law system*, *mean.Venice Commission engagement*, and *deviation.Venice Commission engagement* all are highly statistically significant. Thus, the article's main results for *common law system* and *Venice Commission engagement* remain unchanged, regardless of the use

[2] This violates the logistic and probit models' non-quasicomplete separation assumption (Albert and Anderson 1984).

[3] Note that in model 2, we do not decompose *time*, *time*^{2}, and *time*^{3} into the mean and the deviation variables because, as Carter and Signorino acknowledge (2010b), in discrete event history analysis like the logistic and probit models with the cubic polynomial approximation to temporal dependence, temporal dependence per se is "not a theoretical variable" (Beck 2010) but actually residuals (that is, the unexplained part of the dependent variable): "in the context of grouped binary data, interpreting 'the effect of time' should not be undertaken quite so literally. Time is not an independent actor here. Rather, the hazard reflects unmodeled processes and/or omitted regressors" (Carter and Signorino 2010b, 295).

of the fixed-effects estimation method.

It should be noted that all the between-within models' results for *political insurance* raise doubts about Ginsburg and Versteeg's (2013) assertion since the variable takes the completely opposite signs across the cross- and within-country dimensions of the same constitutional court creation process. Specifically, in model 1, *mean.political insurance* is positively associated with the cross-country dimension of the hazard rate of constitutional court creation, which is *opposite* of what Ginsburg and Versteeg (2013) hypothesize. In contrast, *deviation.political insurance* is negatively correlated with the within-country dimension of the same establishment process. Furthermore, both *mean.political insurance* and *deviation.political insurance* are highly statistically significant. Likewise, in model 2, these mutually contradictory results for *political insurance* remain unchanged. Thus, our between-within approach to country fixed effects uncovers the *political insurance* variable's contradictory and hence questionable relationships with the cross- and within-country dimensions of the same constitutional court creation process, which have been obscured by Ginsburg and Versteeg's (2013, 12–13) traditional dummy variables approach that both suffers from sample selection bias and fixates exclusively on the within-country dimension of the predictor-outcome association.

Table OA4. Robustness Checks against Endogeneity Bias Due to Unobserved Country-Level Heterogeneity: Determinants of Constitutional Court Establishment, 1947–2019

	Model 1	Model 2
Mean.common law system	−2.313***	−2.044***
	(0.795)	(0.534)
Deviation.common law system	—	—
Mean.Venice Commission engagement	5.221***	2.002**
	(0.932)	(0.902)
Deviation.Venice Commission engagement	2.722***	2.802***
	(0.754)	(0.752)
Mean.political insurance	2.240***	0.855
	(0.845)	(0.652)
Deviation.political insurance	−1.860***	−1.976***
	(0.538)	(0.579)
Mean.regional contagion	0.470	0.113
	(0.297)	(0.125)
Deviation.regional contagion	0.613***	0.540***
	(0.218)	(0.180)
Mean.recent democratic transition	4.350**	1.660
	(2.018)	(1.410)
Deviation.recent democratic transition	0.602	0.399
	(0.540)	(0.520)
Mean.presidentialism	−0.120	0.099
	(0.943)	(0.693)
Deviation.presidentialism	−0.575	−1.006
	(1.373)	(1.117)
Mean.GDP per capita	−0.098	−0.007
	(0.081)	(0.054)
Deviation.GDP per capita	1.578**	2.840***
	(0.626)	(0.346)
Time		−0.235***
		(0.067)
$Time^2$		0.007***
		(0.002)
$Time^3$		−0.00008***
		(0.00002)
Constant	−5.306**	−3.249**
	(2.082)	(1.502)
Number of states	168	168
Number of adoptions	79	79
Number of observations	7,934	7,934
Log likelihood	−344.07	−316.20
Wald χ^2	95.68***	295.18***

Note: Coefficients are reported. Numbers in parentheses are robust standard errors clustered on country. The *mean* variables and the *deviation* variables capture explanatory variables' relationships with the cross- and within-country dimensions, respectively, of the constitutional court establishment process. All explanatory variables use a one-year lag. *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.10$, in two-tailed tests.

Robustness Checks against Endogeneity Bias, Part III: Selection Bias

Tables OA5 and OA6 present the results and validity tests of the two-stage estimation method to demonstrate that the key finding about *Venice Commission engagement* is robust against endogeneity bias caused by the possible selection bias that the Venice Commission may be selective and uneven in getting involved in the processes of constitutional court adoption across countries. Given that the Venice Commission has often provided expert advice and technical assistance at the request of individual countries, the Venice Commission's in-country engagement may be endogenous to a target country's willingness in the first place to establish a constitutional court. If so, this would confound the estimated relationship between *Venice Commission engagement* and the dependent variable. We solve this potential endogeneity bias resulting from selection bias by using the two-stage estimation method, specifically Maddala's (1983, 120–22) control function approach.

There are several reasons why we chose this method over other two-stage estimation approaches. Generally, the two-stage estimation method begins with the first-stage equation, where an endogenous predictor is regressed on both instrumental variables and all the other exogenous predictors from the second-stage outcome equation (Baltagi 2011, 265; Wiggins 2013). The existing approaches differ from one another in terms of how to handle the endogenous predictor in estimating the second-stage equation that already includes the other control variables. First, the conventional two-stage predictor substitution approach—for example, the two-stage least squares regression model—replaces the endogenous predictor with its predicted values from the first-stage equation and then estimates the outcome. However, this approach is inappropriate for our analysis because our limited dependent variable requires a nonlinear model but in nonlinear models the two-stage predictor substitution approach is inconsistent and biased (Terza, Bradford, and Dismuke 2008). Second, the two-stage residual inclusion approach estimates the second-stage outcome by including the first-stage residuals as the control for potential endogeneity as well as the original endogenous predictor (Terza, Basu, and Rathouz 2008). Yet, the typical two-stage residual inclusion approach is also inadequate because, although compatible with our nonlinear model, for it to work it requires the endogenous predictor to be a *continuous* variable and, as such, cannot handle our binary endogenous predictor (that is, the Venice Commission's engagement or nonengagement).

In contrast with the existing approaches, Maddala's (1983, 120–22) control function approach is appropriate and effective for solving the potential endogeneity problem with the *binary* endogenous predictor like *Venice Commission engagement*. It estimates a pair of probit models: that is, the first-stage equation about whether or not the Venice Commission engaged in a country in the previous year, and the second-stage equation about whether a country establishes a constitutional court in the current year. In doing so, Maddala's probit model also estimates and includes λ (lambda) as the control and the direct test for the potential endogeneity of the *Venice Commission engagement* variable. λ tests the null hypothesis of exogeneity. *Non*rejection of the null hypothesis (that is, *non*significance of λ) indicates that the Venice Commission's in-country engagement is *not* endogenous to a country's willingness for constitutional court adoption.

In view of these advantages, our two-stage estimation based on Maddala's (1983, 120–22) control function approach proceeds as follows. In the first-stage equation, we estimate the probit model by regressing *Venice Commission engagement* on *common law system* (that is, the exogenous predictor of our interest), instrumental variables, and all of the second-stage equation's control variables (that is, exogenous predictors). We select four instrumental variables that relate to the Venice Commission's in-country engagement but have no clear and direct link to country adoption of a constitutional court per se: namely, *urbanization*, *Sunni population*, *Venice Commission judicial independence density*, and *distance from the US*.

Urbanization considers that the urban middle class may make their government more willing to Westernize and open to Western international organizations such as the Venice Commission, whereas *Sunni population* accounts for the possibility that Islamic fundamentalism may lead to governments' ambivalence or hostility to Western-led multilateralism (Inglehart 2000; Inglehart and Norris 2003). *Urbanization* measures the percentage of total population living in cities with populations greater than 100,000 in a country in a year, using the National Material Capabilities 6.0 data (Greig and Enterline 2021; Singer, Bremer, and Stuckey 1972). *Sunni population* computes the percentage of the Sunni Muslim population in a country in a year, based on the World Religion Data, Version 1.1 (Maoz and Henderson 2013). *Venice Commission judicial independence density* taps into the claim that the degree of regional organizations' external democracy promotion reflects the extent to which their permanent member states—which often include Western great powers—are democratic (Pevehouse 2005, 46–49). Adapting Pevehouse's (2005, 70–73) operationalization, we begin by computing the yearly mean level of judicial independence (specifically, high court independence) for four Western great-power members of the Venice Commission (namely Austria, France, Germany, and Italy), using Staton and colleagues' judicial independence data (Linzer and Staton 2015; Staton et al. 2019). Then, this variable takes that yearly mean value if a country is a full or associate member of the Venice Commission in a given year, and 0 otherwise. As the data cover up to the year 2015, 2015's values are used to impute those of 2016, 2017, and 2018. *Distance from the US* accounts for the argument that the domestic politics of the US has had a ripple effect on the rest of the world, particularly the Global South, and catalyzed the latter's acceptance of the liberal international order, of which Western international organizations like the Venice Commission are a part (Dezalay and Garth 2006). It is the great-circle distance in kilometers from Washington, DC to each country's capital city, based on World Cities Database's latitude and longitude data.[4] To correct skewedness, we then transform this variable's values using the inverse hyperbolic sine transformation, that is, a variant of log transformation which, unlike the natural log transformation, can handle negative or zero values without adding a small arbitrary constant (say, 1 or 0.001) that prevents missing data (Friedline, Masa, and Chowa 2015). Given that this variable's untransformed value is zero for the US itself, we chose the inverse hyperbolic sine transformation over the natural log transformation to prevent its missingness.

In the second-stage equation, we estimate the second probit model that regresses the probability of constitutional court creation on *common law system*, *Venice Commission engagement*, all the control variables, λ, and the cubic polynomial approximation to temporal dependence (that is, *time*, *time*^{2}, and *time*^{3}) as per Carter and Signorino's (2010a) advice. As we explained earlier, λ captures the correlation between the unobservables that affect the first-stage equation's endogenous binary predictor and the unobservables that affect the second-stage equation's dependent variable, and serves as the statistical test for the possible endogeneity of *Venice Commission engagement*. Table OA5 presents the results of our two-stage estimation based on Maddala's (1983, 120–22) control function approach. Model 1 is the first-stage equation about whether or not the Venice Commission engaged in a country in the previous year, and model 2 is the second-stage equation about whether a country establishes a constitutional court in the current year. As model 2 in table OA5 shows, in essence, λ testing for the endogeneity of the Venice Commission's in-country engagement is never statistically significant. This lack of statistical significance indicates that *Venice Commission engagement* is *not* endogenous. Furthermore, the main results for *Venice Commission engagement* as well as *common law system* remain unchanged both statistically and substantively.

While our two-stage estimation model based on Maddala's (1983, 120–22) control function approach provides firm support for our theory, we conduct instrument validity tests to ensure that

[4] Available at http://simplemaps.com/static/data/world-cities/basic/simplemaps-worldcities-basic.csv (accessed August 19, 2019).

our four instrumental variables satisfy the two-stage estimation method's requirements of instrument relevance and exogeneity. First, when in the first-stage equation *Venice Commission engagement* is regressed on *common law system*, the four instrumental variables, and all of the second-stage equation's control variables, the F-test statistic for the joint significance of the four instruments is 50.34. Given that 10 is the minimum acceptable threshold of the first-stage F-test statistic in a linear two-stage least squares model (Baltagi 2011, 267), *urbanization*, *Sunni population*, *Venice Commission judicial independence density*, and *distance from the US* are highly relevant strong instruments for *Venice Commission engagement*.

Second, none of *urbanization*, *Sunni population*, *Venice Commission judicial independence density*, and *distance from the US* are correlated with the error process of the second-stage equation about constitutional court creation. Table OA6 shows the Sargan-Hansen test for instrument exogeneity by regressing the normally distributed residuals of the second-stage probit model on all of the exogenous predictors used in the first-stage equation.[5] In essence, none of them are statistically significant in the variable-by-variable Sargan-Hansen test, suggesting that *urbanization*, *Sunni population*, *Venice Commission judicial independence density*, and *distance from the US* have no direct relationship with constitutional court (non-)adoption. The fact that all of the second-stage equation's control variables are properly included in the first-stage equation minimizes the risk that *urbanization*, *Sunni population*, *Venice Commission judicial independence density*, and *distance from the US* have an indirect relationship with constitutional court (non-)adoption by being correlated with an omitted predictor of the dependent variable. Thus, the part of variation in *Venice Commission engagement* captured by the four instrumental variables can be regarded as exogenous.

[5] Table OA5 does not include *time*, $time^2$, and $time^3$ because, as we already noted in fn. 3 above, in discrete event history analysis, temporal dependence is in essence the unexplained part of the second-stage dependent variable and hence cannot be treated as an exogenous predictor (Beck 2010; Carter and Signorino 2010b).

Table OA5. Robustness Check against Endogeneity Bias Due to Selection Bias: Determinants of Constitutional Court Establishment, 1947–2019

	Model 1 First-stage equation: Venice Commission engagement	Model 2 Second-stage equation: constitutional court creation
Urbanization	0.005	
	(0.004)	
Sunni population	0.007***	
	(0.002)	
Venice Commission judicial independence density	2.448*** (0.389)	
Distance from the US	−0.204***	
	(0.036)	
Common law system	−0.155	−0.637***
	(0.156)	(0.194)
Venice Commission engagement	—	2.132**
		(0.983)
Political insurance	0.822**	−0.156
	(0.413)	(0.194)
Regional contagion	0.308***	0.073*
	(0.061)	(0.043)
Recent democratic transition	0.716***	0.589***
	(0.248)	(0.196)
Presidentialism	−0.191	0.423*
	(0.597)	(0.249)
GDP per capita	−0.021	−0.026
	(0.031)	(0.026)
λ (Test for endogeneity)		−0.414
		(0.585)
Time		−0.041
		(0.026)
Time^2		0.002**
		(0.001)
Time^3		−0.00002**
		(0.00001)
Constant	−1.694**	−1.977***
	(0.850)	(0.681)
Number of states	158	158
Number of adoptions		70
Number of observations	7,700	7,665
Log likelihood	−207.38	−343.62
Wald χ^2	146.57***	114.77***

Note: Coefficients are reported. Numbers in parentheses in model 1 are robust standard errors clustered on country, and those in model 2 are bootstrap standard errors clustered on country with 2,000 replications. λ controls and tests for the endogeneity of *Venice Commission engagement*. All explanatory variables in the second-stage equation use a one-year lag. *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.10$, in two-tailed tests.

Table OA6. Testing Instrument Exogeneity

	Sargan-Hansen test
Urbanization	0.0002
	(0.0002)
Sunni population	0.00007
	(0.0001)
Venice Commission judicial independence density	0.009
	(0.022)
Distance from the US	0.003
	(0.002)
Common law system	0.0003
	(0.006)
Venice Commission engagement	—
Political insurance	−0.0005
	(0.011)
Regional contagion	−0.0004
	(0.002)
Recent democratic transition	0.0001
	(0.024)
Presidentialism	−0.0004
	(0.015)
GDP per capita	−0.0004
	(0.002)
Constant	−0.023
	(0.049)
Number of states	158
Number of observations	7,665
Wald χ^2	0.42
R^2	0.0006

Note: Coefficients are reported. Numbers in parentheses are robust standard errors clustered on country. The dependent variable is the normally distributed residuals of the second-stage probit model. All explanatory variables use a one-year lag. *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.10$, in two-tailed tests.

Robustness Checks against Omitted Variable Bias, Part I

In models 1 to 5 in table OA7, we estimate five additional expanded specification models to demonstrate that the main findings about domestic legal systems and the Venice Commission are robust against omitted variable bias. First, model 1 accounts for the role of American rule-of-law assistance as a possible rival to the Venice Commission. With the fall of the Berlin Wall, the American Bar Association (ABA) began its Rule of Law Initiative (ROLI) to assist with foreign countries in building sustainable legal institutions. Given that ABA members come mainly from legal professionals operating in the American common law system, countries engaged by the ABA may be less likely to adopt a constitutional court. We code the *American Bar Association assistance* variable 1 if the ABA operates a ROLI program for constitutional or judicial reform in a country in a given year and 0 otherwise, by reading all official ABA documents on the ABA ROLI.[6]

Second, model 2 tests the claim that constitutional courts are created to adjudicate the distribution of powers and responsibilities between the federal and the provincial governments in a federal system (Öhlinger 2003). *Federalism* equals 1 if a country has a federal system in a year and 0 otherwise, using the Political Constraint Index 2021 data (Henisz 2002). Note that the data are unavailable for the pre-1960 period and hence decrease model 2's sample size to 6,545 observations.

Third, model 3 considers the claim that a constitutional court is more likely to be established in strong rule-of-law countries (Shapiro 1999). *Judicial independence* is a continuous variable that measures de facto judicial independence (specifically, high court independence) in a country in a year on a 0 (fully dependent) to +1 (fully independent) scale, using Staton and colleagues' (Linzer and Staton 2015; Staton et al. 2019) judicial independence data. As the data are available up to the year 2015, 2015's values are used to impute those of 2016, 2017, and 2018.

Fourth, model 4 controls for the level of democracy as an additional, possible determinant of a government's decision to adopt a constitutional court (O'Donnell 1998), although, as mentioned in the article, the *political insurance* variable already taps into the level of democracy (Ginsburg and Versteeg 2013, 24, 30). *Polyarchy* is a continuous variable that measures regime type on a 0 (fully autocratic) to +1 (fully democratic) scale, using the Varieties of Democracy (V-Dem) Dataset, Version 12 (Coppedge et al. 2022).

Finally, model 5 considers world society theory's assertion that the diffusion of the same policy models across dissimilar national states is caused by those states' embeddedness in the global cultural structures that legitimize those models as the appropriate standards of modern state behavior (Meyer 2010; Meyer et al. 1997). *Cultural globalization* proxies the degree of national embeddedness in world culture. It is a composite index measuring the overall level of cultural globalization for a country in a year on a 1 (the least globalized) to 100 (the most globalized) scale, using the KOF Globalisation Index, Version 2021 (Gygli et al. 2019). Note that the data are not available for the pre-1970 period, thereby reducing model 5's sample size to 5,453 observations.

Table OA7 presents the results of robustness checks against omitted variable bias. In essence, the article's key findings about *common law system* and *Venice Commission engagement* remain unchanged, irrespective of the inclusion of those additional possible factors. Also, although *judicial independence* and *cultural globalization* are statistically significant, both variables are *opposite* of their hypothesized positive sign.

[6] Available at https://www.americanbar.org/advocacy/rule_of_law/publications (accessed December 23, 2021).

Table OA7. Robustness Checks against Omitted Variable Bias: Determinants of Constitutional Court Establishment, 1947–2019

	Model 1	Model 2	Model 3	Model 4	Model 5
Common law system	−1.764***	−1.760***	−1.718***	−1.816***	−1.881***
	(0.465)	(0.473)	(0.466)	(0.461)	(0.488)
Venice Commission engagement	2.839***	2.386***	2.821***	3.062***	2.008***
	(0.506)	(0.484)	(0.425)	(0.444)	(0.497)
Political insurance	−0.417	−0.357	−0.719*	−0.550	−0.589
	(0.396)	(0.397)	(0.399)	(0.414)	(0.398)
Regional contagion	0.237***	0.186**	0.287***	0.231***	0.265**
	(0.085)	(0.093)	(0.096)	(0.085)	(0.127)
Recent democratic transition	1.350***	1.517***	1.288***	1.361***	1.110***
	(0.310)	(0.313)	(0.331)	(0.316)	(0.321)
Presidentialism	0.957*	0.830*	−0.082	0.368	−0.034
	(0.524)	(0.501)	(0.751)	(0.888)	(0.653)
GDP per capita	−0.066	−0.068	−0.056	−0.062	−0.097
	(0.054)	(0.061)	(0.056)	(0.054)	(0.104)
American Bar Association assistance	0.767				
	(0.819)				
Federalism		−0.977			
		(0.854)			
Judicial independence			−1.781**		
			(0.851)		
Polyarchy				−0.868	
				(1.046)	
Cultural globalization					−0.026**
					(0.011)
Number of states	168	163	166	168	156
Number of adoptions	79	76	79	79	72
Number of observations	7,934	6,545	7,669	7,934	5,453
Log likelihood	−324.47	−299.24	−317.80	−324.56	−269.99
Wald χ^2	191.32***	156.85***	184.72***	170.34***	112.77***

Note: Coefficients are reported. Numbers in parentheses are robust standard errors clustered on country. All explanatory variables use a one-year lag. *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.10$, in two-tailed tests.

Robustness Checks against Alternative Operationalization of Control Variables

In models 1 to 4 in table OA8, we show robustness checks against alternative operationalization of control variables. Model 1 replaces the article's *regional contagion* variable based on Neumayer's (2008, 256) operationalization with an alternative measurement of regional emulation to ensure that the main results for domestic legal systems and the Venice Commission are *not* an artifact of a particular operationalization of regional demonstration effects. Following Simmons' (2009, 384) operationalization and using the World Bank's regional classification, for a country in a given year, *regional adoptions* measures the percentage of all the countries with a constitutional court within that country's geographic region.

Models 2 to 4 substitute one-, five-, and ten-year windows since democratic transition for the article's *recent democratic transition* variable's three-year window, based on Boix, Miller, and Rosato's (2013, 2022) data on regime transitions. Specifically, in model 2, *democratic transition within 1 year* equals 1 if a country underwent a democratic transition in the previous year and still remains a democracy in a given year, and 0 otherwise. In model 3, *democratic transition within 5 years* is coded 1 if a country has experienced a democratic transition within the past five years and still remains a democracy in a given year, and 0 otherwise. Finally, in model 4, *democratic transition within 10 years* takes 1 if a country has experienced a democratic transition within the past decade and still remains a democracy in a given year, and 0 otherwise.

Table OA8 reports the results of robustness checks against alternative operationalization of control variables. In short, the article's main findings about *common law system* and *Venice Commission engagement* remain unchanged, regardless of the use of alternative measurements of regional emulation and democratic transition.

Table OA8. Robustness Checks against Alternative Operationalization of Control Variables: Determinants of Constitutional Court Establishment, 1947–2019

	Model 1	Model 2	Model 3	Model 4
Common law system	−1.948***	−1.785***	−1.780***	−1.783***
	(0.467)	(0.473)	(0.466)	(0.465)
Venice Commission engagement	2.051***	3.024***	3.037***	3.083***
	(0.545)	(0.418)	(0.425)	(0.416)
Political insurance	−0.353	−0.488	−0.392	−0.278
	(0.387)	(0.391)	(0.400)	(0.409)
Regional contagion	(replaced)	0.215**	0.227***	0.246***
		(0.086)	(0.083)	(0.082)
Recent democratic transition	1.542***	(replaced)	(replaced)	(replaced)
	(0.311)			
Presidentialism	0.840*	0.829	0.933*	0.996*
	(0.496)	(0.522)	(0.522)	(0.537)
GDP per capita	−0.040	−0.063	−0.061	−0.054
	(0.057)	(0.053)	(0.054)	(0.055)
Regional adoptions (%)	0.029***			
	(0.006)			
Democratic transition within 1 year		1.021**		
		(0.462)		
Democratic transition within 5 years			1.107***	
			(0.317)	
Democratic transition within 10 years				1.241***
				(0.287)
Number of states	168	168	168	168
Number of adoptions	79	79	79	79
Number of observations	7,934	7,934	7,934	7,934
Log likelihood	−317.06	−328.98	−326.26	−323.71
Wald χ^2	174.53***	134.37***	147.71***	171.64***

Note: Coefficients are reported. Numbers in parentheses are robust standard errors clustered on country. All explanatory variables use a one-year lag. *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.10$, in two-tailed tests.

Robustness Checks against Omitted Variable Bias, Part II

In models 1 to 7 in table OA9, we take into account the claim that since the mid-2000s, there appears to have emerged an important international wave toward authoritarianism and against the rule of law across many regions of the world, and that this wave may have countered the global trend toward constitutional court adoptions.[7] We explicitly address this possibility by measuring and controlling for the level of global and regional dictatorship in seven different ways as additional robustness checks against omitted variable bias.

Specifically, *foreign policy similarity to Russia* in model 1 and *foreign policy similarity to China* in model 2 measure a country's voting similarity to Russia (formerly, the Soviet Union) and China, respectively, within the United Nations (UN) General Assembly in a given year, using the United Nations General Assembly Voting Data, Version 29.0 (Voeten 2013). Note that the data on voting similarity to China are available for the years since 1971 when the UN General Assembly formally restored the People's Republic of China to the Chinese seat at the UN and expelled Taiwan from the UN.

In model 3, for a country in a year, *regional dictatorial neighbors* computes the percentage of all dictatorships in that country's geographic region *except* that country itself, that is, the annual percentage of each country's regional dictatorial neighbors, based on Boix, Miller, and Rosato's (2013; 2022) regime type data and the World Bank's regional classification. *Regional reversed polyarchy* in model 4, *regional reversed judicial constraint* in model 5, and *regional reversed rule of law* in model 6 measure the average *reverse* scores of each country's regional neighbors' polyarchy (that is, electoral democracy), judicial constraints on the executive, and the rule of law, respectively, in a year on a 0 (fully democratic) to +1 (fully autocratic) scale, using the V-Dem Dataset, Version 12 (Coppedge et al. 2022) and the World Bank's regional classification. Finally, *regional reverse judicial independence* in model 7 calculates the average *reverse* score of each country's regional neighbors' judicial independence in a year on a 0 (fully independent) to +1 (fully dependent) scale, using Staton and colleagues' (Linzer and Staton 2015; Staton et al. 2019) judicial independence data and the World Bank's regional classification. As the data cover up to the year 2015, 2015's values are used to impute 2016's, 2017's, and 2018's.

Table OA9 presents the results of robustness checks against omitted variable bias regarding the possible role the international wave of authoritarianism may play in the global spread of constitutional courts. In essence, the article's main results for *common law system* and *Venice Commission engagement* remain unchanged in all cases. Interestingly, all seven variables for global and regional dictatorship turn out to be consistently *opposite* of the hypothesized negative sign with respect to the hazard rate of a country's constitutional court creation, and five of them are statistically significant. There may be two reasons for these results. First, countries' foreign policy similarities to Russia and China, although increasing during the mid-twentieth century, peaked in 1964 and 1989, respectively, and have since remained below that level, as seen in figure OA1 in this supplementary material. Second, as figures OA2 to OA9 show, the level of regional dictatorship—despite an uptick in some regions' immediately recent years on some indicators—has actually been far lower during the past decade than during the Cold War period across all regions and all measurements. Thus, these results for global and regional dictatorship suggest that the emerging international wave of authoritarianism has *not* worked as a countermove against the global trend toward the adoption of Kelsenian constitutional courts in and beyond Europe.

In conclusion, in all the models in tables OA3, OA4, OA5, OA7, OA8, and OA9, the coefficients of *common law system* and *Venice Commission engagement* all remain unchanged

[7] We thank a reviewer for this insight.

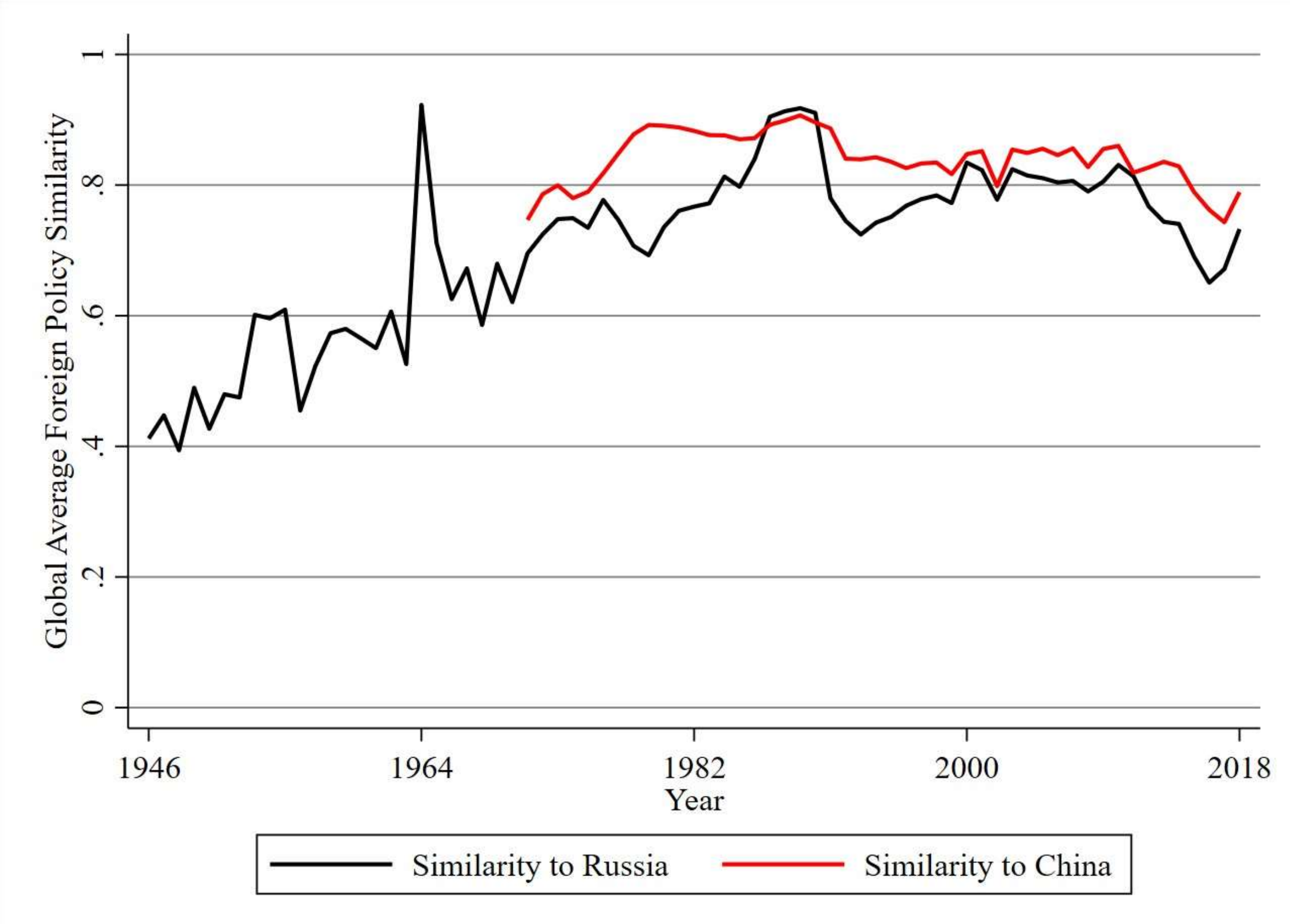
statistically and substantively. Thus, the article's key findings about domestic legal systems and the Venice Commission are robust against alternative operationalization of the dependent variable as competing events, endogeneity bias, omitted variable bias, and alternative operationalization of control variables.

Table OA9. Robustness Checks against Omitted Variable Bias: Determinants of Constitutional Court Establishment, 1947–2019

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Common law system	−1.745***	−1.812***	−1.923***	−1.811***	−1.859***	−2.034***	−1.984***
	(0.471)	(0.484)	(0.467)	(0.468)	(0.476)	(0.480)	(0.478)
Venice Commission engagement	2.789***	2.335***	2.870***	2.950***	2.770***	2.554***	2.558***
	(0.502)	(0.556)	(0.446)	(0.437)	(0.449)	(0.458)	(0.459)
Political insurance	−0.262	−0.319	−0.654*	−0.484	−0.525	−0.520	−0.656*
	(0.412)	(0.409)	(0.364)	(0.379)	(0.368)	(0.354)	(0.376)
Regional contagion	0.248**	0.274**	0.293***	0.263***	0.319***	0.432***	0.366***
	(0.101)	(0.111)	(0.080)	(0.083)	(0.081)	(0.088)	(0.097)
Recent democratic transition	1.489***	1.423***	1.377***	1.371***	1.378***	1.318***	1.339***
	(0.312)	(0.309)	(0.310)	(0.313)	(0.311)	(0.309)	(0.315)
Presidentialism	0.611	0.370	0.582	0.774	0.579	0.310	0.545
	(0.529)	(0.565)	(0.537)	(0.567)	(0.548)	(0.535)	(0.544)
GDP per capita	−0.122**	−0.164*	−0.060	−0.068	−0.064	−0.049	−0.056
	(0.051)	(0.087)	(0.054)	(0.055)	(0.055)	(0.058)	(0.057)
Foreign policy similarity to Russia	1.706*						
	(0.899)						
Foreign policy similarity to China		2.700					
		(1.695)					
Regional dictatorial neighbors (%)			0.011**				
			(0.005)				
Regional reversed polyarchy				0.514			
				(0.727)			
Regional reversed judicial constraint					1.647**		
					(0.796)		
Regional reversed rule of law						3.113***	
						(0.916)	
Regional reversed judicial independence							2.194***
							(0.780)
Number of states	162	158	167	167	167	167	165
Number of adoptions	72	69	78	78	78	78	78
Number of observations	7,351	5,294	7,889	7,889	7,858	7,889	7,626
Log likelihood	−285.01	−257.95	−318.91	−321.02	−319.57	−315.94	−313.33
Wald χ^2	191.90***	102.49***	161.66***	162.29***	167.12***	183.10***	171.59***

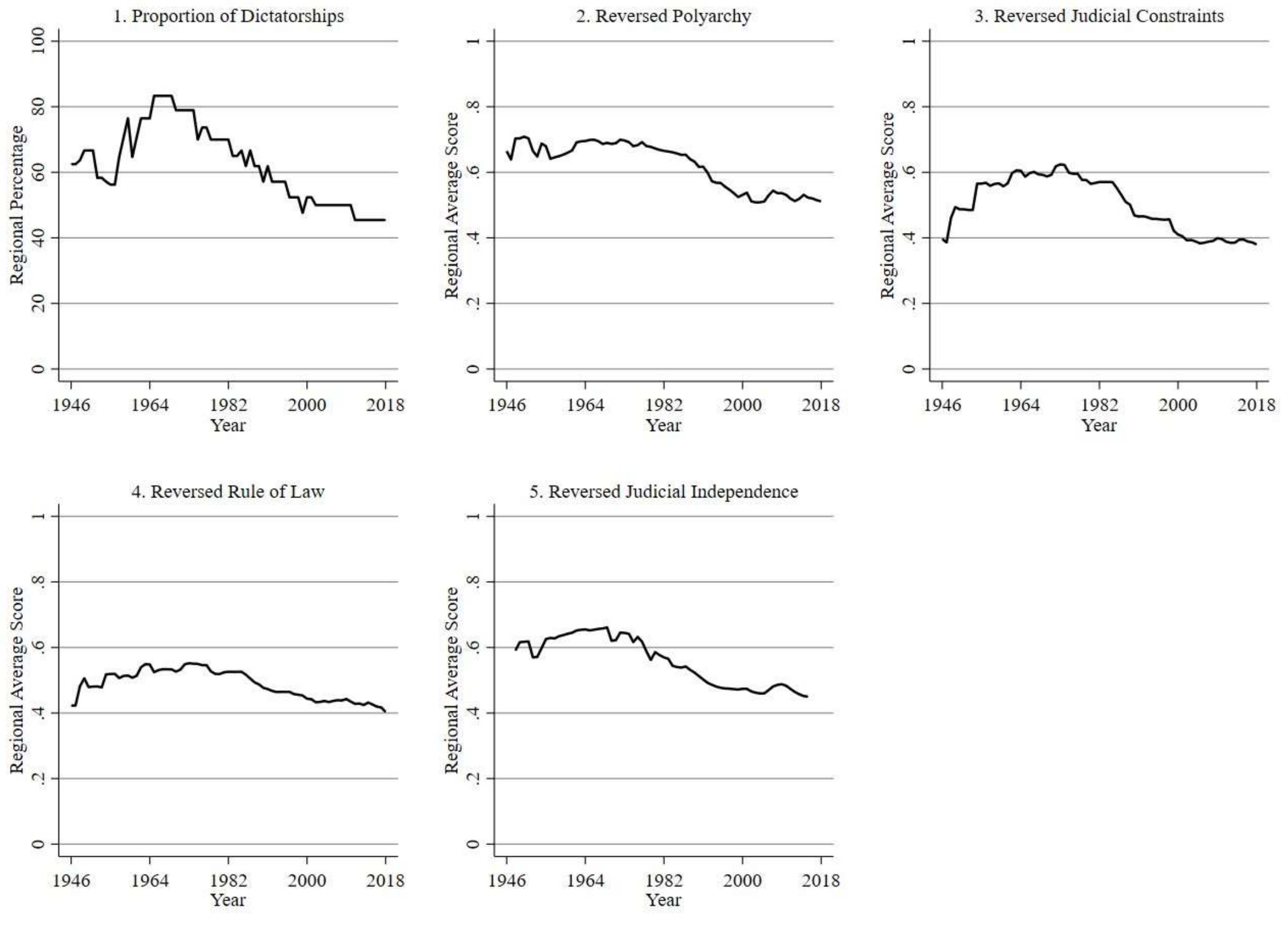
Note: Coefficients are reported. Numbers in parentheses are robust standard errors clustered on country. All explanatory variables use a one-year lag. *** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.10$, in two-tailed tests.

Figure OA1. The Level of Global Dictatorship, 1946–2018



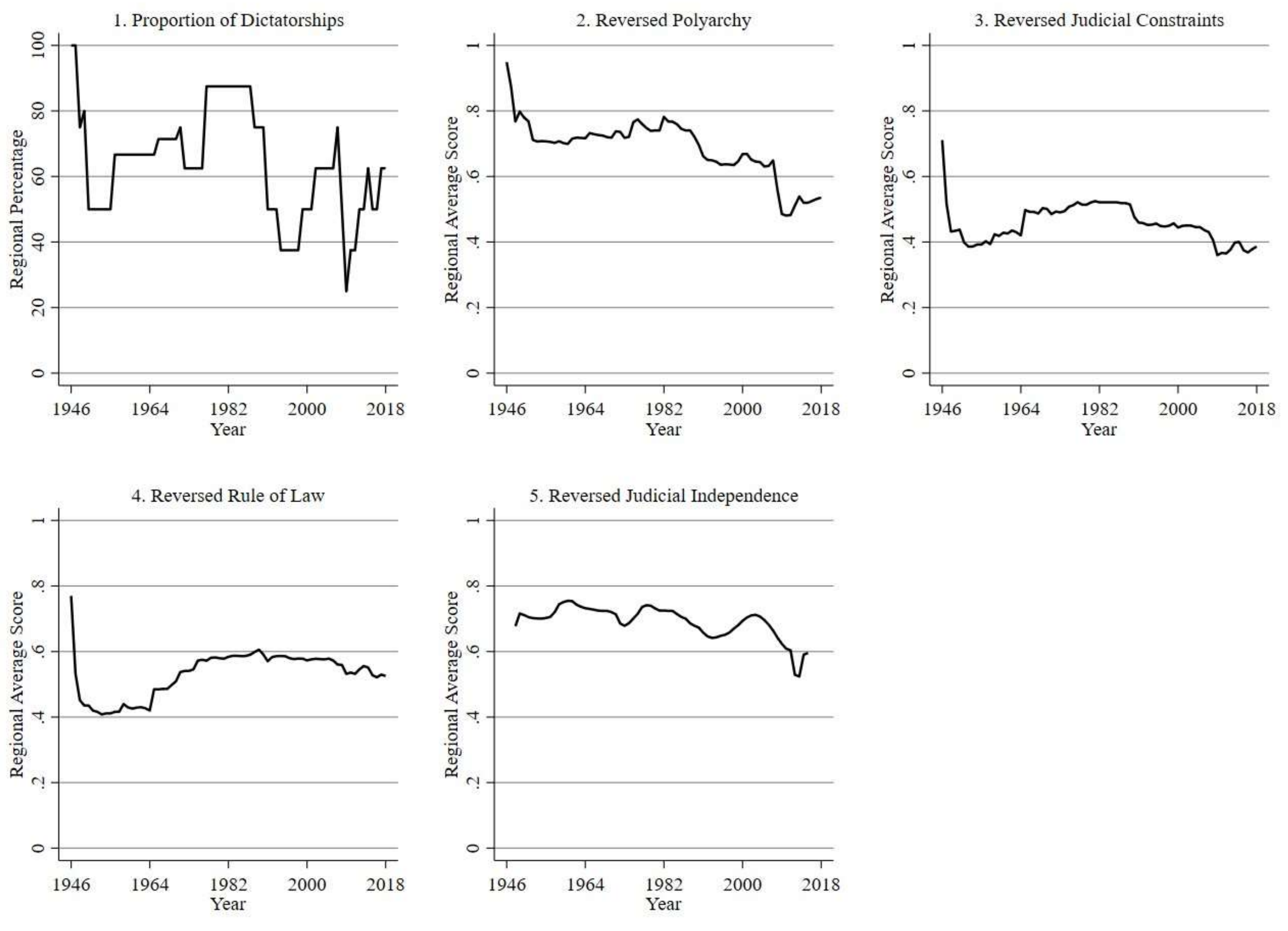
Note: In the graph, the higher levels indicate the greater foreign policy similarities to Russia and China and hence the greater levels of global dictatorship. The data on similarity to China are available from 1971 when the UN formally restored the People's Republic of China to the UN's Chinese seat and expelled Taiwan.

Figure OA2. The Level of Regional Dictatorship in East Asia and the Pacific, 1946–2018



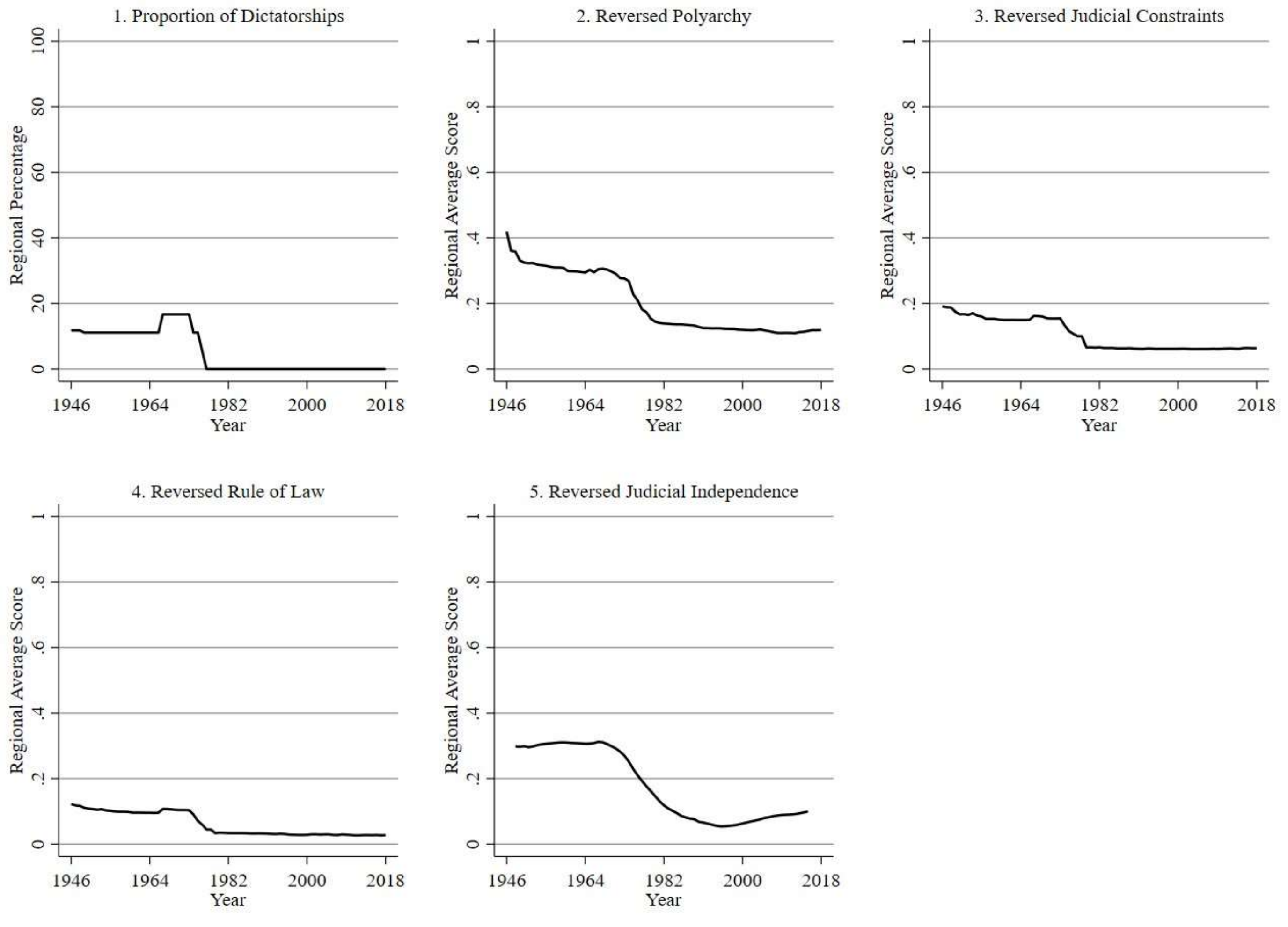
Note: In each graph, the higher level indicates the greater level of regional dictatorship.

Figure OA3. The Level of Regional Dictatorship in South Asia, 1946–2018



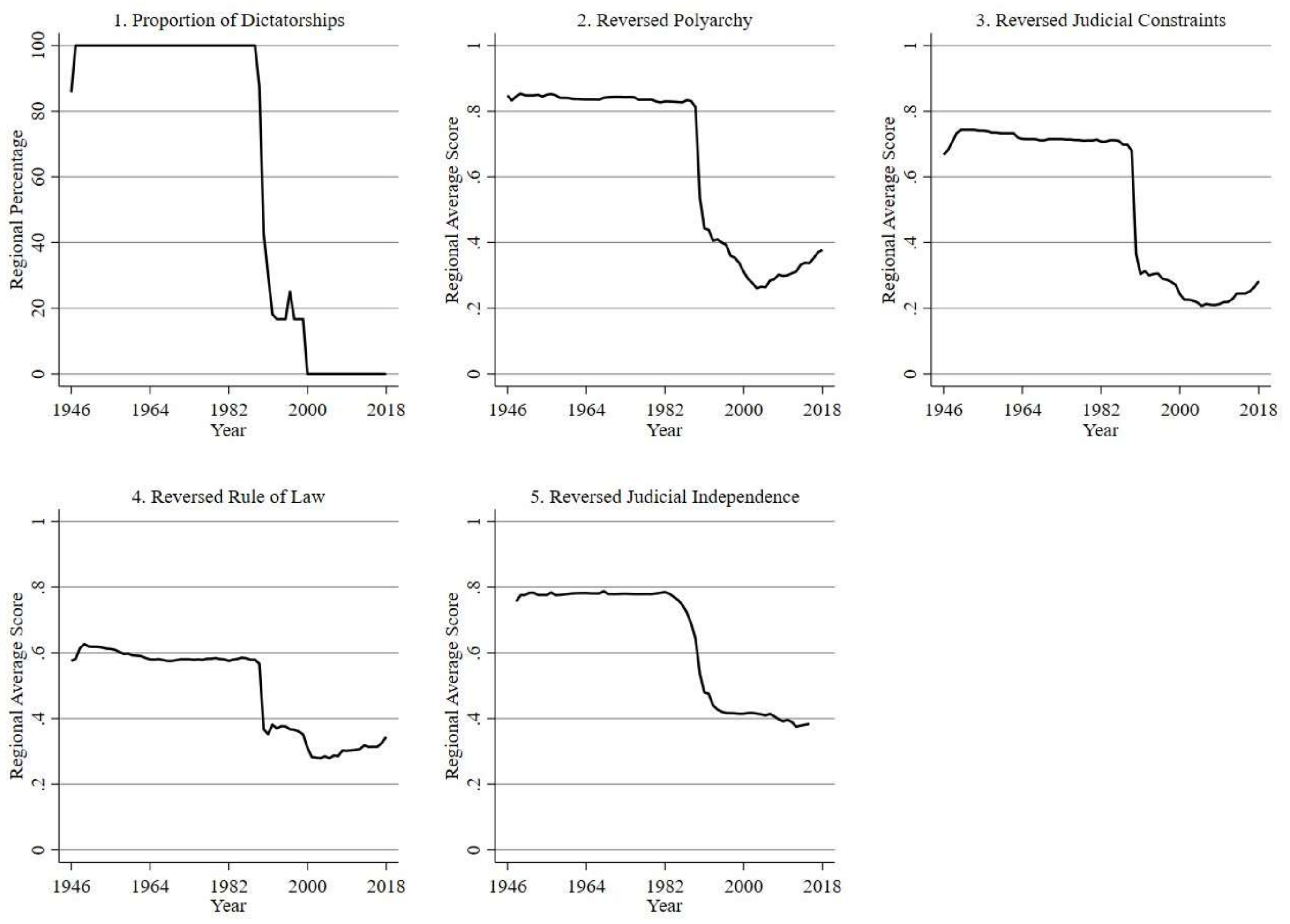
Note: In each graph, the higher level indicates the greater level of regional dictatorship.

Figure OA4. The Level of Regional Dictatorship in Western Europe, 1946–2018



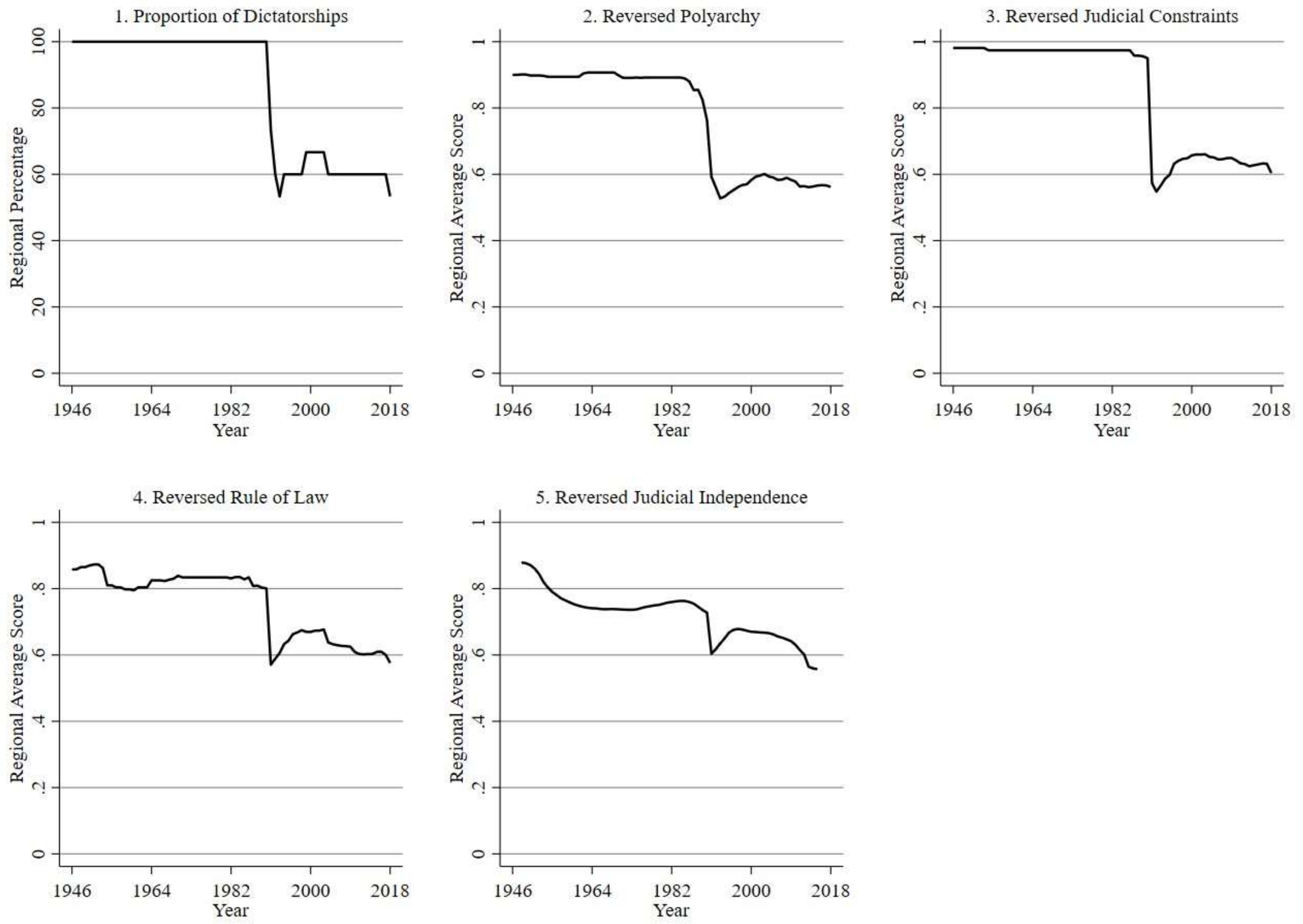
Note: In each graph, the higher level indicates the greater level of regional dictatorship.

Figure OA5. The Level of Regional Dictatorship in Eastern Europe, 1946–2018



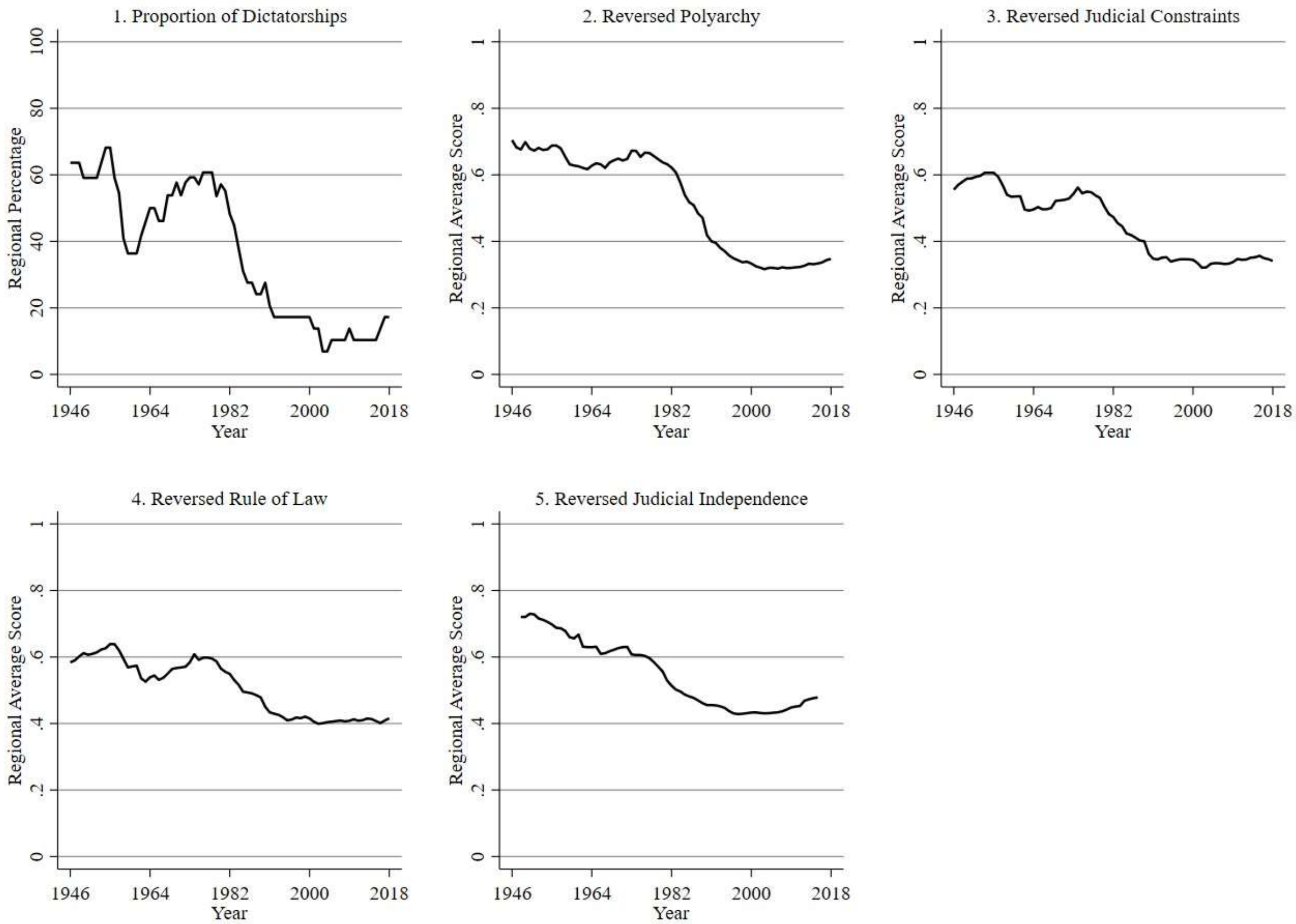
Note: In each graph, the higher level indicates the greater level of regional dictatorship.

Figure OA6. The Level of Regional Dictatorship in the Former Soviet Union, 1946–2018



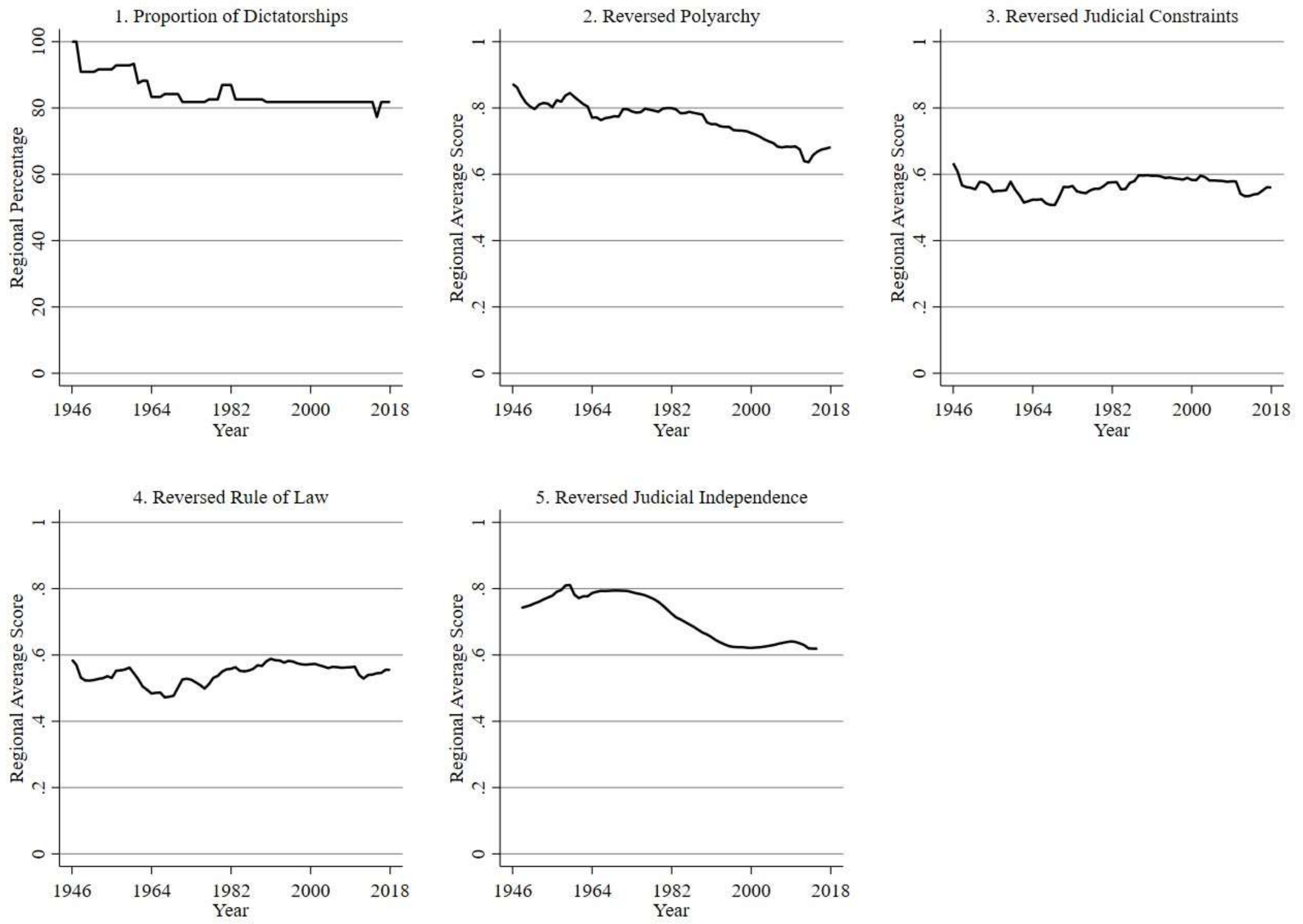
Note: In each graph, the higher level indicates the greater level of regional dictatorship.

Figure OA7. The Level of Regional Dictatorship in the Americas, 1946–2018



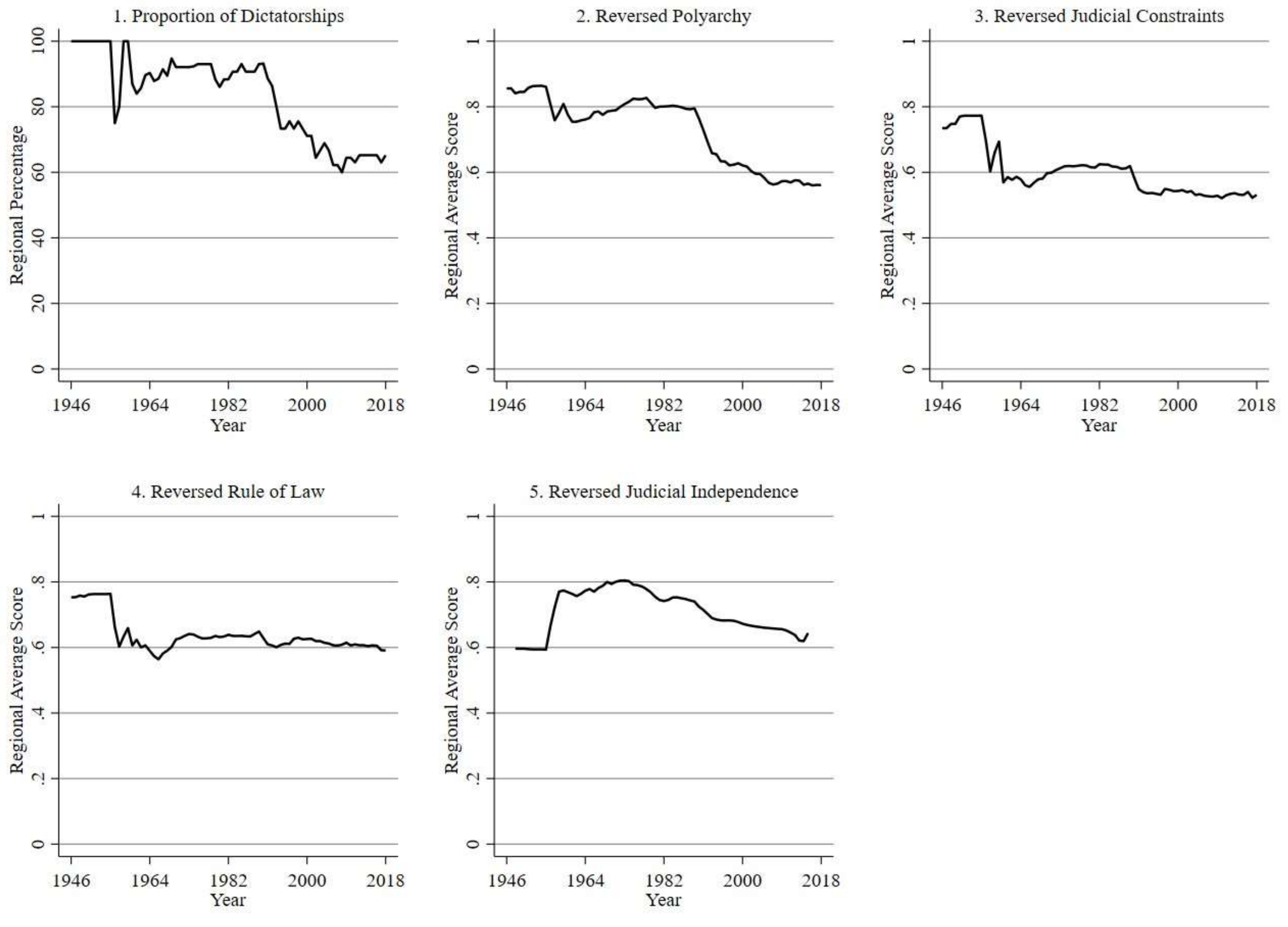
Note: In each graph, the higher level indicates the greater level of regional dictatorship.

Figure OA8. The Level of Regional Dictatorship in the Middle East and North Africa, 1946–2018



Note: In each graph, the higher level indicates the greater level of regional dictatorship.

Figure OA9. The Level of Regional Dictatorship in Sub-Saharan Africa, 1946–2018



Note: In each graph, the higher level indicates the greater level of regional dictatorship.

Supplementary Material References

Note: This section does not duplicate what has already been cited in the article, and includes only the new additions to the supplementary material.

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