Can courts in non-democracies deter election fraud? De jure judicial independence, political competition, and election integrity

ONLINE APPENDIX A

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Contents

Balance statistics for entropy weighting

Table A.1 reports the variables used in the pre-processing phase for Model 2, as well as the means for the treatment group, the control group after entropy balancing and weighting, and the unweighted control group. There are sizable improvements in balance associated with *opposition autonomy*, *Polity score*, *high-* and *low-court independence*, *transitional election*, *duration of the regime*, and *executive respect for the constitution*. All of these are theoretically important variables for predicting attacks on the judiciary, suggesting that the re-weighted data will improve estimation of the causal effect of judicial independence compared to the raw data.

	Treatment group means	Weighted control group means	Unweighted control group means
Duration of the current constitutional regime	27.031	27.032	34.821
Opposition autonomy	1.164	1.164	0.608
Opposition oversight	0.744	0.744	0.019
Latent judicial independence	0.412	0.412	0.382
GDP per capita (log)	7.559	7.559	7.926
Urbanization	0.446	0.446	0.477
High-court independence	0.421	0.421	-0.274
Low-court independence	0.673	0.673	-0.119
Transitional election	0.016	0.016	0.057
Executive respect for the constitution	0.957	0.957	0.283
Alternative information index	0.722	0.722	0.545
Education	4.395	4.395	5.510
Legislative constraints on the executive	0.590	0.590	0.433

Table A.1: Balance improvements for covariates used in entropy balancing (Model 2)

Interaction effect validation

Linear multiplicative interaction models of the kind used here can be misleading if the assumption of a linear change in the marginal effect does not hold and/or there is a lack of common support—meaning that either the treatment or control condition is available over only a limited range of the moderator. To help validate interaction results, Hainmueller et al (2019) propose dividing a continuous moderator into discrete bins, and then estimating the marginal effect of the treatment on the dependent variable for the median value of each of those bins. This has the advantage of enabling the researcher to visually inspect the assumption of linear trends (by comparing the positions of the resulting marginal effects), and of testing the marginal effects only for typical values of the moderating variable. Diagnostic plots

using this test are presented in Figure A.1. The standard marginal effect plot is overlaid by the point estimates and 95% confidence intervals of the binning method. The plots show that the general marginal effect is not driven by extreme values of the moderator, suggesting that common support is upheld. In three of the four models, the first and second tercile marginal effects are statistically significant; in Model 1 only the first tercile estimate is. They also show that the assumption of linear interactive effects is well supported, with the possible exception of Model 1 in the upper left quadrant where the point estimate falls just outside the 95% confidence interval for the linear model. Though the confidence interval of the point estimate itself overlaps with the general model, it is still worth investigating Model 1 more closely.
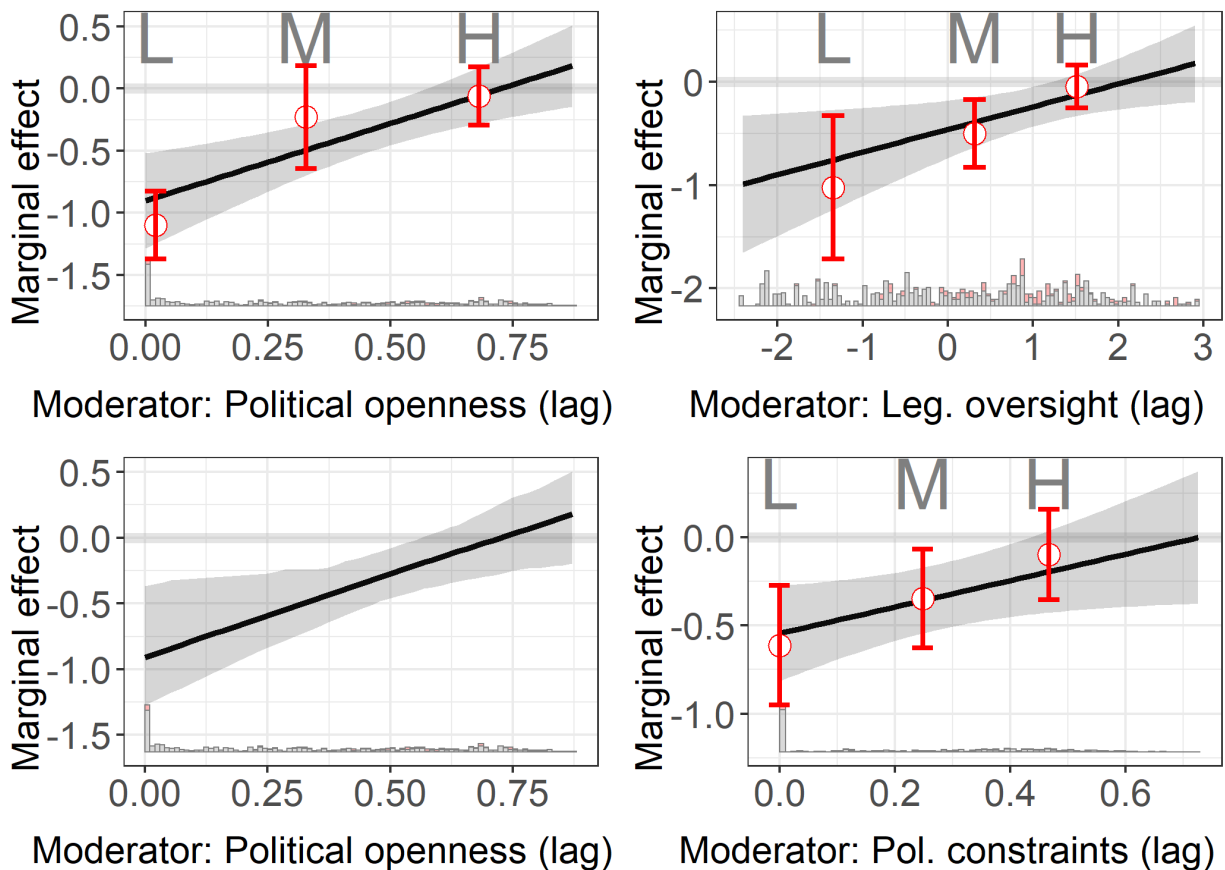

Figure A.1: Validation plots for linear interaction models

To further test the linear interaction effect assumption, Hainmueller et al (2019) propose a kernel smoothing estimator of the marginal effect. By estimating the marginal effect of the treatment at a series of small ranges of the moderator, the overall marginal effect is not confined to a linear trend; in other words, this method is able to detect non-linearities at a fairly fine-grained level of detail. Figure A.1 presents the results of this diagnostic in the lower left, and shows negligible evidence of nonlinearity. The results of these diagnostics show that the assumptions underlying the linear interactive model are well supported.

Using pro-government intimidation and vote-buying as alternative measures of manipulation

The main analysis utilizes electoral fraud as a dependent variable, but other forms of illegal electoral manipulation can also be addressed by courts; incidents of electoral violence and intimidation or vote-buying may both find their way to the courts. The variable *pro-government intimidation* is taken from V-Dem, and captures the extent to which the opposition was subject to "repression, intimidation, violence, or harassment by the government, the ruling party, or their agents" (Coppedge, Michael et al., 2017). *Vote-buying* is a measure of the degree to which parties relied on the distribution of money or gifts to turn out and/or persuade voters (Coppedge, Michael et al., 2017). As with *intentional voting irregularities* in the main text, both variables have been multiplied by -1 so that higher values indicate more severe manipulation. As Table A.2 and Figure A.2 show, a positive judicial reform has the same effect on intimidation and vote-buying in interaction with legislative opposition oversight as it does on fraud.

	Dependent variable:	
	Intimidation	Vote-buying
	(3)	(4)
Judicial reform	-0.357***	-0.274***
	(0.081)	(0.074)
Opposition oversight	-0.337***	-0.196***
	(0.034)	(0.031)
Executive election	-0.079	-0.071
	(0.067)	(0.061)
Proportional representation	0.045	0.068
	(0.082)	(0.074)
Mixed electoral system	-0.172*	-0.073
	(0.093)	(0.085)
International observers	-0.144***	-0.112**
	(0.050)	(0.045)
Negative judicial reform	0.088	-0.082
	(0.102)	(0.092)
GDP per capita (log)	-0.081	0.026
	(0.067)	(0.061)
Judicial purge	-0.116**	-0.003
	(0.050)	(0.045)
Court packing	-0.395***	-0.178***
	(0.065)	(0.059)

Positive judicial reform : Opposition oversight	0.163***	0.047
	(0.052)	(0.047)
Country fixed effects	Yes	Yes
Constant	2.298***	1.379***
	(0.559)	(0.506)
Observations	727	727
R^2	0.891	0.875
Adjusted R^2	0.870	0.850
Residual Std. Error (df = 605)	0.236	0.214
F Statistic (df = 121; 605)	41.023***	34.905***
Note:		*$p<0.1$; **$p<0.05$; ***$p<0.01$

Table A.2: OLS models of alternative measures of election manipulation. All non-electoral variables lagged one year.
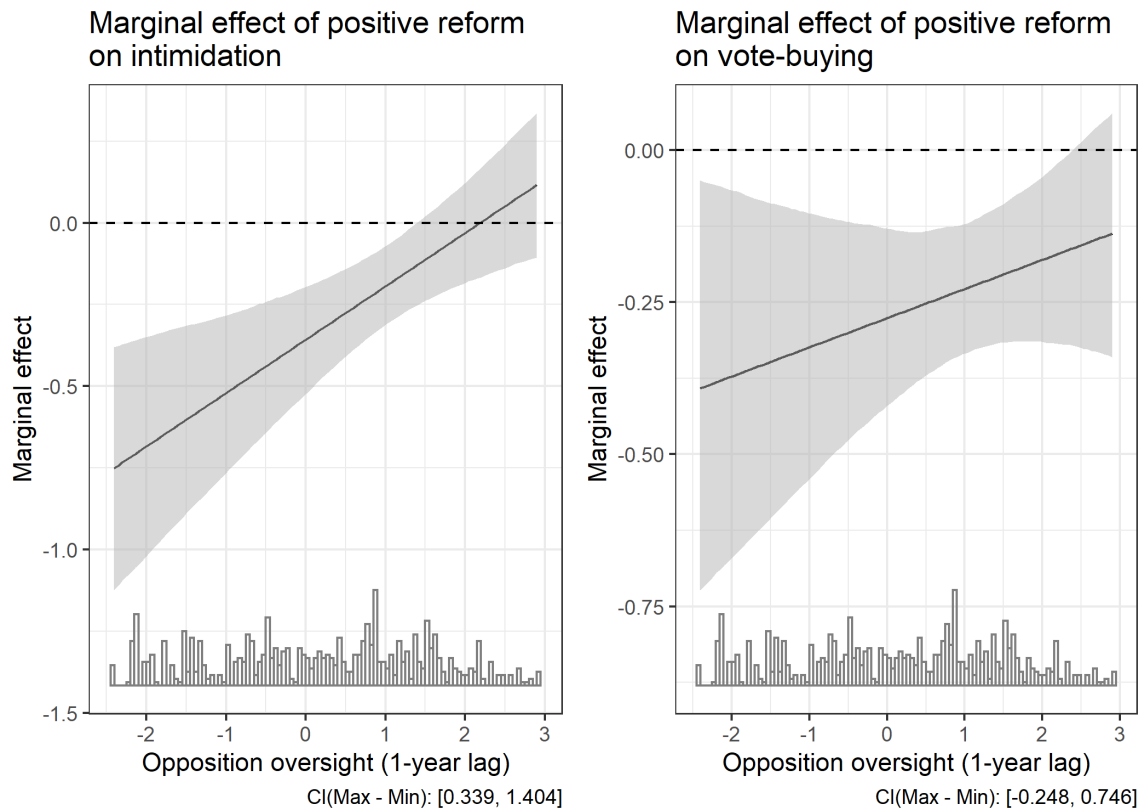

Figure A.2: Marginal effect of positive judicial reform on alternative measures of election manipulation. Shaded areas represent 95% confidence intervals adjusted for marginal effects.

Alternative pre-processing method

To demonstrate that the results are not driven by the specifics of the entropy balancing procedure, I also employ covariate balancing propensity score (CBPS) weighting as an alternative approach (Imai and Ratkovic 2014). Rather than directly estimating control weights that balance the means of covariates as in entropy balancing, the CBPS method of pre-processing estimates the propensity scores (i.e. the probability of being in the treatment group) such that weighting observations by propensity score results in covariate balance across treatment and control groups. While similar to entropy balancing, one distinguishing feature of this approach is that control observations with high propensity scores (and treatment observations with low propensity scores) take on greater weight in the subsequent regression models. This is useful since observations that more closely resemble the counterfactual are given greater weight in the subsequent models. The same variables were included in the pre-processing and analysis phases as in Models 2 and 4 in the main text. Table A.3 and Figure A.3 show that the results of models using CBPS weighting are equivalent to those using entropy balancing.

	Dependent variable:		
	Intentional voting irregularities		
	(5)	(6)	(7)
Positive judicial reform	-0.759***	-0.419***	-0.494***
	(0.110)	(0.082)	(0.090)
Political openness	-0.835***		
	(0.142)		
Opposition oversight		-0.309***	
		(0.034)	
Political constraints			-0.429***
			(0.163)
Executive election	-0.073	-0.045	-0.081
	(0.066)	(0.068)	(0.067)
Proportional representation	-0.022	0.028	-0.037
	(0.081)	(0.082)	(0.081)
Mixed electoral system	-0.061	-0.096	-0.095
	(0.092)	(0.094)	(0.093)
GDP per capita (log)	-0.232***	-0.259***	-0.337***
	(0.068)	(0.067)	(0.066)
International monitors	-0.199***	-0.210***	-0.220***
	(0.051)	(0.051)	(0.052)
Negative judicial reform	-0.110	-0.089	-0.083

	(1)	(2)	(3)
	(0.098)	(0.102)	(0.101)
Judicial purges	-0.225***	-0.162***	-0.253***
	(0.048)	(0.050)	(0.047)
Court packing	-0.117*	-0.115*	-0.146**
	(0.063)	(0.066)	(0.064)
Judicial reform : Political openness	1.009***		
	(0.187)		
Judicial reform : Opposition oversight		0.215***	
		(0.051)	
Judicial reform : Political constraints			0.745***
			(0.216)
Country fixed effects	Yes	Yes	Yes
Constant	3.967***	4.035***	4.586***
	(0.520)	(0.530)	(0.525)
Observations	796	747	795
R^2	0.856	0.868	0.854
Adjusted R^2	0.830	0.843	0.828
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A.3: OLS models of electoral fraud, using covariate-balancing propensity score weighting. All non-electoral variables lagged one year.

Marginal effect of positive judicial reform on voting irregularities

CI(Max - Min): [0.564, 1.189]

Marginal effect of positive judicial reform on voting irregularities

CI(Max - Min): [0.619, 1.659]

Marginal effect of positive judicial reform on voting irregularities
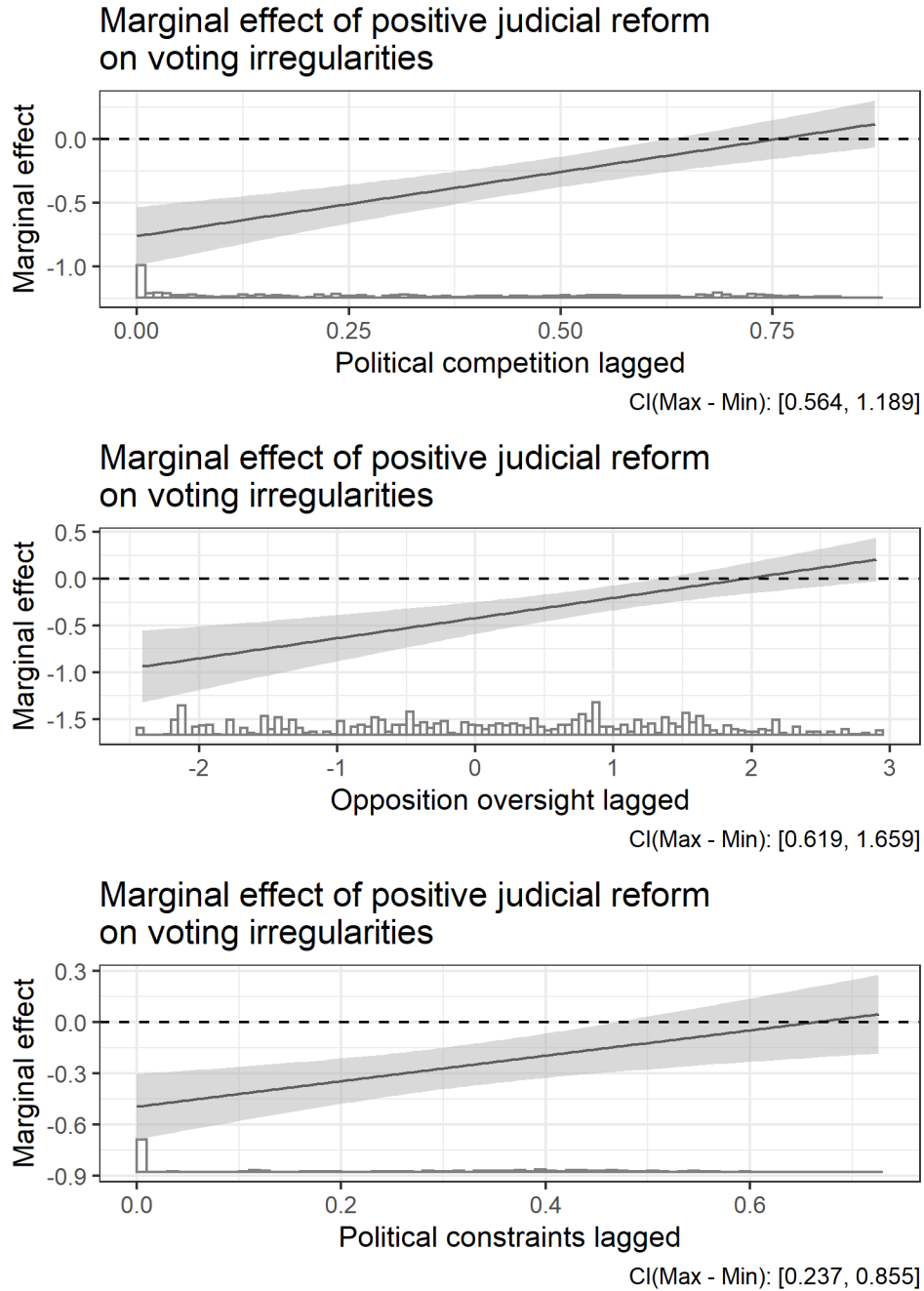
CI(Max - Min): [0.237, 0.855]

Figure A.3: Marginal effect of a major negative shock to judicial independence, conditional on Polity score and opposition oversight, using CBPS weighting. Shaded areas represent 95% confidence intervals adjusted for marginal effects

Balance improvement for CBPS method

Balance improvement statistics for the CBPS approach are shown here. The upper panel of Table A.4 shows the means and standardized means for the explanatory and control variables after balancing; the

lower panel shows the same information for the original data. The standardized means refer to the variable's mean divided by its standard deviation; balance improvements on this measure illustrate how CBPS weighting accounts for the variance as well as means of the covariates (Imai & Ratkovic, 2014). By both measures, balance between treatment and control groups is improved across all variables.

	Control means	Treatment means	Control std. means	Treatment std. means
Regime duration	26.635	26.627	0.580	0.580
Opposition autonomy	1.144	1.145	0.947	0.948
Education	4.396	4.395	1.821	1.820
Opposition oversight	0.739	0.740	0.561	0.562
GDP per capita (log)	7.575	7.575	9.423	9.422
Urban	0.450	0.450	2.215	2.215
High court independence	0.401	0.402	0.350	0.351
Low court independence	0.663	0.664	0.614	0.615
Exec. respect constitution	0.956	0.957	0.871	0.872
Alternative info	0.714	0.715	2.814	2.815
Legislative constraints on exec.	0.590	0.590	2.169	2.170
Transitional election	0.022	0.022	0.094	0.094

	Control means	Treatment means	Control std. means	Treatment std. means
Regime duration	35.755	26.627	0.779	0.580
Opposition autonomy	0.579	1.145	0.479	0.948
Education	5.454	4.395	2.258	1.820
Opposition oversight	-0.005	0.740	-0.004	0.562
GDP per capita (log)	7.920	7.575	9.851	9.422
Urban	0.475	0.450	2.336	2.215
High court independence	-0.289	0.402	-0.252	0.351
Low court independence	-0.137	0.664	-0.127	0.615
Exec. respect constitution	0.261	0.957	0.237	0.872
Alternative info	0.541	0.715	2.130	2.815
Legislative constraints on exec.	0.428	0.590	1.574	2.170
Transitional election	0.069	0.022	0.288	0.094

Table A.4: Balance statistics for CBPS procedure

Results of the selection model

The pre-processing techniques used in this study (weighting control observations by entropy balancing and covariate balancing propensity scores) can ameliorate concerns about endogeneity, but this comes at the risk that the underlying selection model may be misspecified. The CBPS approach has the advantage of producing doubly-robust estimates, meaning that the results are unbiased if either the propensity score or outcome model is correctly specified (Imai & Ratkovic, 2014), and the CBPS results mirror those of the entropy balancing models. However, in both cases, it is important to check the effectiveness of the underlying selection model. To evaluate the selection model, I report its results in the form of a standalone logit model of *positive judicial reform*, shown in Table A.5.

	Dependent variable:		
	Positive judicial reform		
	(8)	(9)	(10)
Regime duration	-0.002	-0.001	-0.001
	(0.003)	(0.003)	(0.003)
Opposition autonomy	-0.36**	-0.29**	-0.23*
	(0.14)	(0.13)	(0.13)
Political openness	1.00		
	(0.72)		
Opposition oversight		-0.07	
		(0.18)	
Political constraints			-1.12*
			(0.58)
GDP per capita (log)	-1.06***	-1.13***	-1.10***
	(0.22)	(0.22)	(0.22)
Urban	3.78***	3.85***	3.84***
	(0.83)	(0.83)	(0.84)
High court independence	0.06	0.13	0.05
	(0.16)	(0.16)	(0.16)
Low court independence	0.78***	0.74***	0.85***
	(0.18)	(0.18)	(0.18)
Transitional election	0.26	0.56	-0.04
	(0.35)	(0.38)	(0.35)
Exec. Respect for constitution	0.16	0.19	0.17
	(0.15)	(0.15)	(0.14)

Alternative info	1.57*	2.28***	2.57***
	(0.82)	(0.77)	(0.73)
Education	-0.15**	-0.13**	-0.14**
	(0.06)	(0.06)	(0.06)
Legislative constraints	-0.84	-0.61	-0.77
	(0.58)	(0.82)	(0.59)
Constant	4.63***	4.85***	4.77***
	(1.39)	(1.53)	(1.39)
Observations	869	823	868
Log Likelihood	-335.63	-319.08	-330.28
Akaike Inf. Crit.	697.27	664.16	686.56
Note:		*p<0.1; **p<0.05; ***p<0.01	

Table A.5: Logit model of positive reform of judicial independence

Each of the covariates included in the model is theoretically justified as a predictor of assaults on judicial independence, as discussed in the main text. A chi-square test using the null and residual deviances from the logit models presented in Table A.5 produces a p-value approaching zero, indicating that the model fits the underlying data well. The results of the models themselves are also compelling. We see significant relationships between the dependent variable and the political variables *alternative sources of information, low-court independence,* and *opposition autonomy*. The negative relationship seen for income, which is unexpected, could indicate that more developed countries engaged in judicial reforms prior to entry into the dataset. Most importantly, we do not observe meaningful changes in the sign or size of control variables based on the inclusion of any particular measure of political competitiveness.

Comparative constitutions project data

Following the coding rules in Melton and Ginsburg (2014), which also makes use of the CCP data, selection rules promote *de jure* independence if the appointment process involves a judicial council or two or more other institutions. Removal procedures promote *de jure* independence if judges cannot be removed, if removal requires super-majority approval in the legislature, if only the public or judicial councils can propose removal for ratification by another institution, or if judges can only be removed for crimes and other misconduct. For each country-year in the dataset, I take the sum the dummy variables indicating the presence of these features in the constitution regarding ordinary courts. The binary variable *positive constitutional reform* is then constructed by subtracting the current-year value from the lagged value for each country; it takes a value of 1 if there has been a positive change in *de jure* independence in the constitution, and 0 otherwise.

This 'treatment' indicator has advantages, namely its conceptual precision and ease of replicability for future studies, but it also has limitations. Changes to judicial institutions need not require amendments to the constitution; in Russia, for example, the specifics of judicial nomination, removal, and funding are set via statute.[1] Even in the United States, a country typically understood to have high *de jure* judicial independence, the size of the judiciary and the Supreme Court is set by legislation, and whether or not the opposition party has a voice in judicial selection is determined by Senate procedures. The V-Dem measure, by relying on coding by country experts, is more likely to capture such sub-constitutional reforms that can affect *de jure* independence. Moreover, constitutional reforms are rare; *positive constitutional reform* takes on a positive value only thirteen times in the year before an election in this dataset; the sum rises only to nineteen when the pre-election lag window is extended to two years.

This causes a lack of common support for the *opposition oversight variable*, as Figure A.4 illustrates. While positive constitutional reforms occur across most of the range of the *openness* and *constraints* mediator variables, they only occur at relatively high levels of *opposition oversight*. For this reason, I exclude *oversight* from models using CCP data as indicators of *de jure* independence.
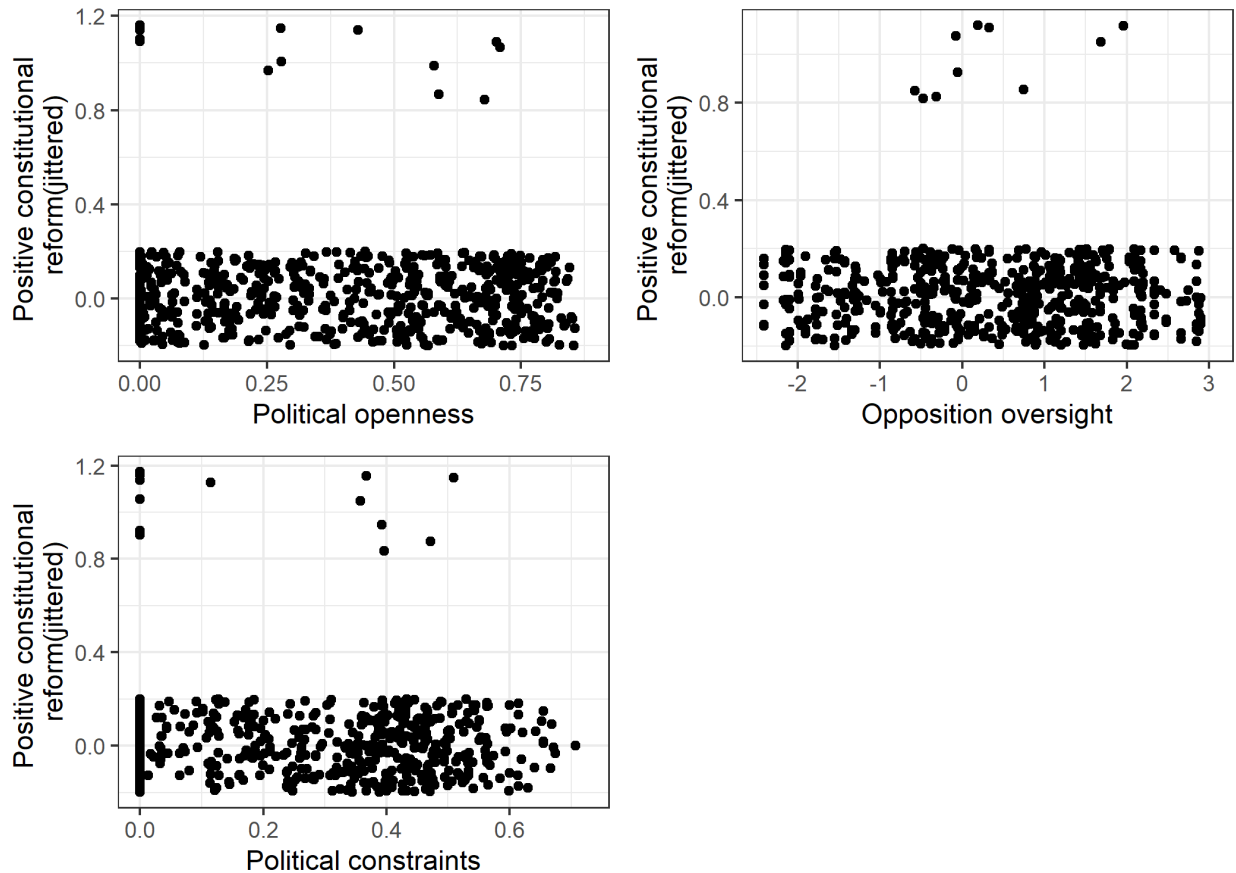

Figure A.4: Common support for treated observations across three moderator variables

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[1] See Federal Constitutional Law "On the Judicial System of the Russian Federation."

The results of the models which make use of *positive constitutional reform* as the treatment variable after entropy balancing are presented in Table A.6, with the marginal effects for these models illustrated in Figure A.5. As the figure demonstrates, the results are largely consistent with those using the V-dem indicator—marginal effects are increasing with each measure of competitiveness, and are negative for low levels of competition. However, in contrast to the previous models, there is also a positive effect at high levels of competitiveness. What accounts for this difference? While this paper does not attempt to investigate the causes of statutory versus constitutional reforms to *de jure* independence, it is reasonable to assume that changes to the constitution are more likely to occur following a reordering of elite networks (Hale, 2014) or as a result of the negotiated transitions described by Magalhães (1999). In this case, it is likely that the increase in election manipulation observed in high-competition settings can be attributed to the insurance mechanism, in which outgoing elites look to secure their interests in a more redoubtable judicial branch as their electoral power declines. If true, the positive marginal effect is in comparison to weak incumbent elites who are unable to secure their interests on the courts; these elites would leave their agents exposed to increased risks of punishment and disfavor, leading to declines in their capacity to generate manipulation (Rundlett & Svolik, 2016).

	Dependent variable:		
	Intentional voting irregularities		
	(11)	(12)	(13)
Constitutional reform (sel. and rem.)	-0.400***	0.390***	-0.428***
	(0.119)	(0.066)	(0.095)
Political openness	-0.994***		
	(0.193)		
Opposition oversight		-0.236***	
		(0.043)	
Political constraints			0.565**
			(0.228)
Executive election	0.274***	-0.083	0.211**
	(0.097)	(0.097)	(0.095)
Proportional representation	0.142	-0.143	0.140
	(0.130)	(0.120)	(0.123)
Mixed electoral system	0.032	-0.325**	0.424***
	(0.149)	(0.158)	(0.144)
GDP per capita	-0.547***	-0.325**	-0.678***
	(0.140)	(0.136)	(0.126)
International observers	-0.103	0.091	-0.011
	(0.104)	(0.093)	(0.097)

	(1)	(2)	(3)
Negative judicial reform	-0.353***	-0.517***	-0.421***
	(0.115)	(0.114)	(0.109)
Judicial purge	-0.252***	-0.472***	-0.437***
	(0.065)	(0.058)	(0.059)
Court packing	-0.105	-0.263***	0.081
	(0.071)	(0.080)	(0.066)
Constitutional reform (sel. and rem.):Political openness	1.614***		
	(0.250)		
Constitutional reform (sel. and rem.) : Opposition oversight		0.126	
		(0.099)	
Constitutional reform (sel. and rem.) : Political constraints			2.144***
			(0.296)
Country fixed effects	Yes	Yes	Yes
Constant	6.390***	4.706***	6.800***
	(2.195)	(1.675)	(1.251)
Observations	449	436	449
R^2	0.893	0.894	0.897
Adjusted R^2	0.863	0.864	0.869
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A.6: Weighted OLS models of election fraud (entropy balanced weights). All non-electoral variables lagged by one year.
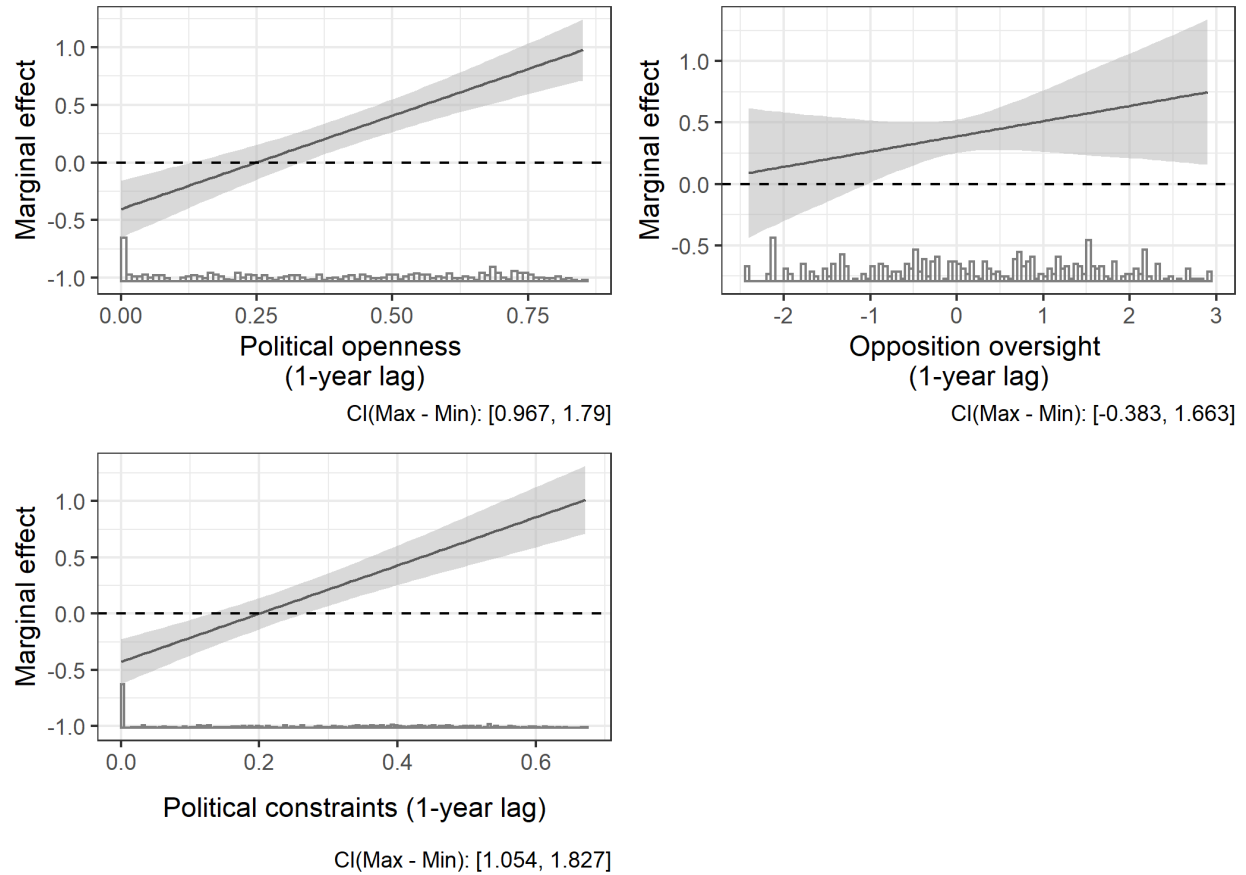
Figure A.5: Marginal effects of a positive judicial reform on intentional voting irregularities. Shaded areas represent 95% confidence intervals.

Regression table for electoral court models

Table A.7 provides the regression summaries associated with Figure 2 in the main text.

	Dependent variable:		
	Intentional voting irregularities		
	(23)	(24)	(25)
Political openness	-0.28		
	(0.23)		
Opposition oversight		-0.31***	
		(0.04)	
Political constraints			-0.20
			(0.25)

Positive judicial reform	-0.99***	-0.56***	-0.71***
	(0.16)	(0.12)	(0.12)
Electoral court	1.32***	1.09***	1.07***
	(0.36)	(0.35)	(0.39)
Electoral court independence	-0.33***	-0.38***	-0.37***
	(0.07)	(0.07)	(0.07)
Political openness : Positive judicial reform	1.26***		
	(0.28)		
Opposition oversight : Positive judicial reform		0.22***	
		(0.06)	
Political constraints : Positive judicial reform			1.15***
			(0.31)
Positive judicial reform : Electoral court	0.78**	0.92***	1.02***
	(0.33)	(0.28)	(0.31)
Political openness : Electoral court	-0.93**		
	(0.42)		
Political openness : Positive judicial reform : Electoral court	-0.75		
	(0.57)		
Opposition oversight : Electoral court		0.16	
		(0.11)	
Opposition oversight : Positive judicial reform : Electoral court		-0.41***	
		(0.16)	
Political constraints : Electoral court			-0.32
			(0.49)
Political constraints : Positive judicial reform : Electoral court			-1.48**
			(0.63)
Country fixed effects	Yes	Yes	Yes
Observations	506	478	506
R^2	0.90	0.92	0.90
Adjusted R^2	0.88	0.89	0.87

Note: $^*p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A.7: Weighted OLS models of election fraud (entropy balanced weights). All variables 1-year lagged, except *executive election*, *proportional electoral system*, *mixed electoral system*, and *international observers*. Control variables excluded from the table, but are included in the models.

Lagged election manipulation as a selection variable

One possible confounder for this study is the pre-reform ability of the regime to generate election manipulation: it may be that governments that have difficulty motivating election-manipulating agents face little cost from *de jure* reforms and are more likely to implement them as a result. To control for this possibility, the models in Table A.8 include lagged values for *intentional voting irregularities* in the pre-processing selection model. This makes positive judicial reforms conditionally independent of the level of manipulation in the prior election, at the cost of some observations. Figure A.6 shows that the results are substantively similar to those in the main text.

	Dependent variable:		
	Intentional voting irregularities		
	(30)	(31)	(32)
Positive judicial reform	-0.93***	-0.62***	-0.61***
	(0.12)	(0.09)	(0.11)
Political openness	-1.21***		
	(0.16)		
Opposition oversight		-0.41***	
		(0.04)	
Political constraints			-0.40**
			(0.19)
Executive election	-0.17**	-0.15**	-0.18**
	(0.07)	(0.08)	(0.08)
PR system	-0.14	-0.15	-0.15
	(0.09)	(0.09)	(0.09)
Mixed electoral system	-0.14	-0.20*	-0.19*
	(0.10)	(0.10)	(0.11)
GDP per capita (log)	-0.23***	-0.32***	-0.42***
	(0.09)	(0.08)	(0.08)
International monitors	-0.22***	-0.23***	-0.23***
	(0.06)	(0.05)	(0.06)
Negative judicial reform	-0.26**	-0.17	-0.16
	(0.12)	(0.13)	(0.12)
Judicial purges	-0.20***	-0.14***	-0.29***
	(0.05)	(0.05)	(0.05)
Court packing	-0.14**	-0.06	-0.15**

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	(0.07)	(0.07)	(0.07)
Positive judicial reform : Political openness	1.16***		
	(0.22)		
Positive judicial reform : Opposition oversight		0.29***	
		(0.06)	
Positive judicial reform : Political constraints			0.75***
			(0.25)
Country fixed effects	Yes	Yes	Yes
Constant	4.51***	5.01***	5.54***
	(0.67)	(0.66)	(0.67)
Observations	701	663	701
R^2	0.87	0.88	0.86
Adjusted R^2	0.84	0.86	0.83
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A.8: Weighted OLS model of election fraud (entropy balanced weights). All variables 1-year lagged, except *executive election*, *proportional electoral system*, *mixed electoral system*, and *international observers*
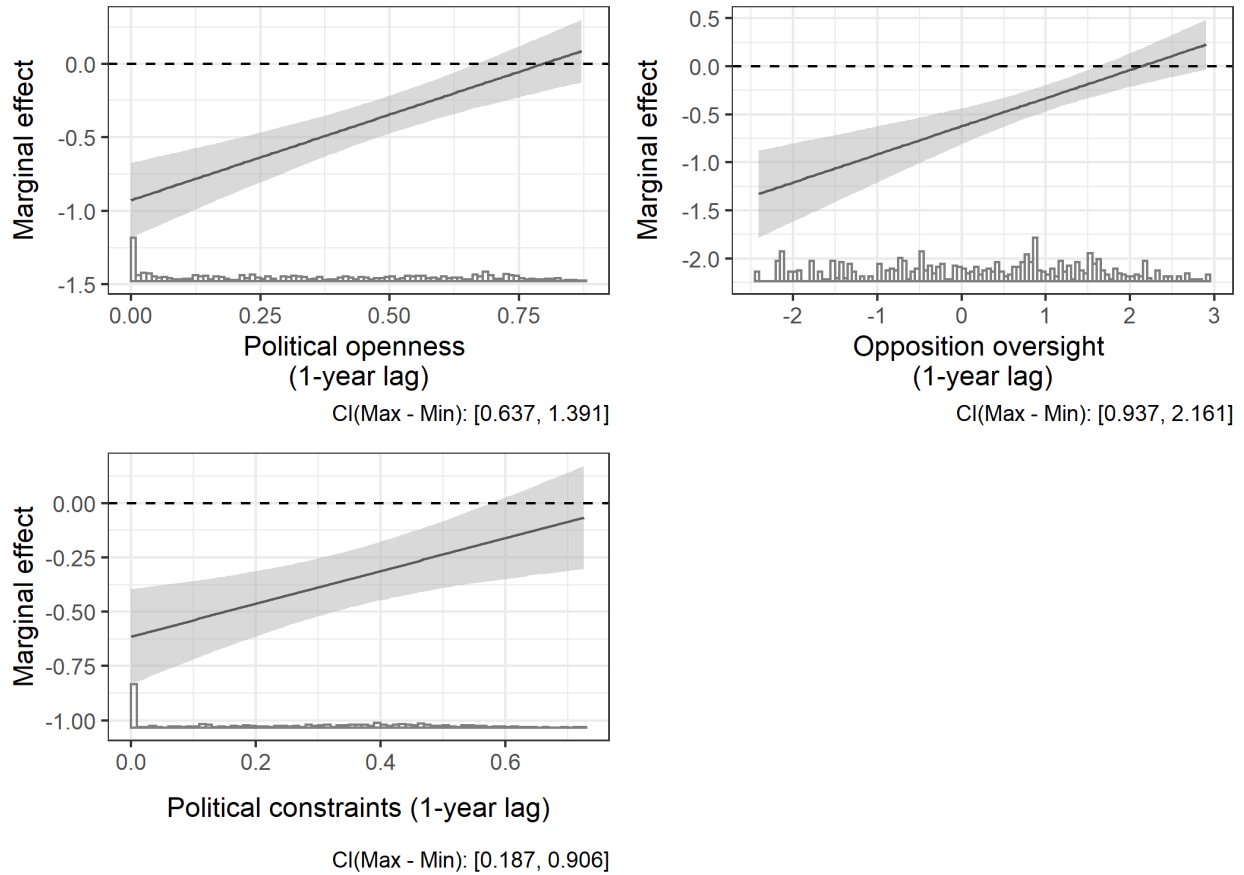
Figure A.6: Marginal effects of a positive judicial reform on intentional voting irregularities. Shaded areas represent 95% confidence intervals

Government control of the legislature as a selection variable

The following models include the variable *government seat share* in the selection models, to balance treatment and control groups across the size of the government's faction in the legislature. The variable is constructed from the Database of Political Institutions (Cruz et al., 2017), by dividing the number of pro-government seats by the total number of seats in the legislature. This reduces the sample size, but helps control for the possibility that the government's relative dominance of the legislature affects both its desire to implement judicial reforms and its ability to generate election manipulation. The results, shown in Figure A.7 and Table A.9, are consistent with those of the main models.

	Dependent variable:		
	Intentional voting irregularities		
	(33)	(34)	(35)
Positive judicial reform	-1.07***	-0.57***	-0.68***
	(0.15)	(0.11)	(0.12)

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Political openness	-1.30***		
	(0.21)		
Opposition oversight		-0.42***	
		(0.05)	
Political constraints			-0.59***
			(0.21)
Executive election	-0.08	-0.14*	-0.14*
	(0.08)	(0.08)	(0.08)
PR system	-0.16	-0.31***	-0.25**
	(0.10)	(0.11)	(0.11)
Mixed electoral system	-0.04	-0.17	-0.14
	(0.11)	(0.12)	(0.11)
GDP per capita (log)	-0.22*	-0.39***	-0.27**
	(0.11)	(0.13)	(0.12)
International monitors	-0.27***	-0.30***	-0.29***
	(0.06)	(0.06)	(0.06)
Negative judicial reform	0.02	0.04	0.02
	(0.15)	(0.18)	(0.15)
Judicial purges	-0.09	-0.07	-0.17***
	(0.06)	(0.06)	(0.06)
Court packing	-0.11	-0.04	-0.19**
	(0.08)	(0.09)	(0.08)
Positive judicial reform:Political openness	1.81***		
	(0.25)		
Positive judicial reform:Opposition oversight		0.39***	
		(0.07)	
Positive judicial reform:Political constraints			1.50***
			(0.27)
Country fixed effects	Yes	Yes	Yes
Constant	2.94***	3.72***	2.64***
	(0.77)	(0.86)	(0.79)
Observations	547	503	547
R^2	0.89	0.89	0.88
Adjusted R^2	0.87	0.86	0.86
Note:			*p<0.1; **p<0.05; ***p<0.01

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Table A.9: Weighted OLS model of election fraud (entropy balanced weights). All variables 1-year lagged, except *executive election*, *proportional electoral system*, *mixed electoral system*, and *international observers*
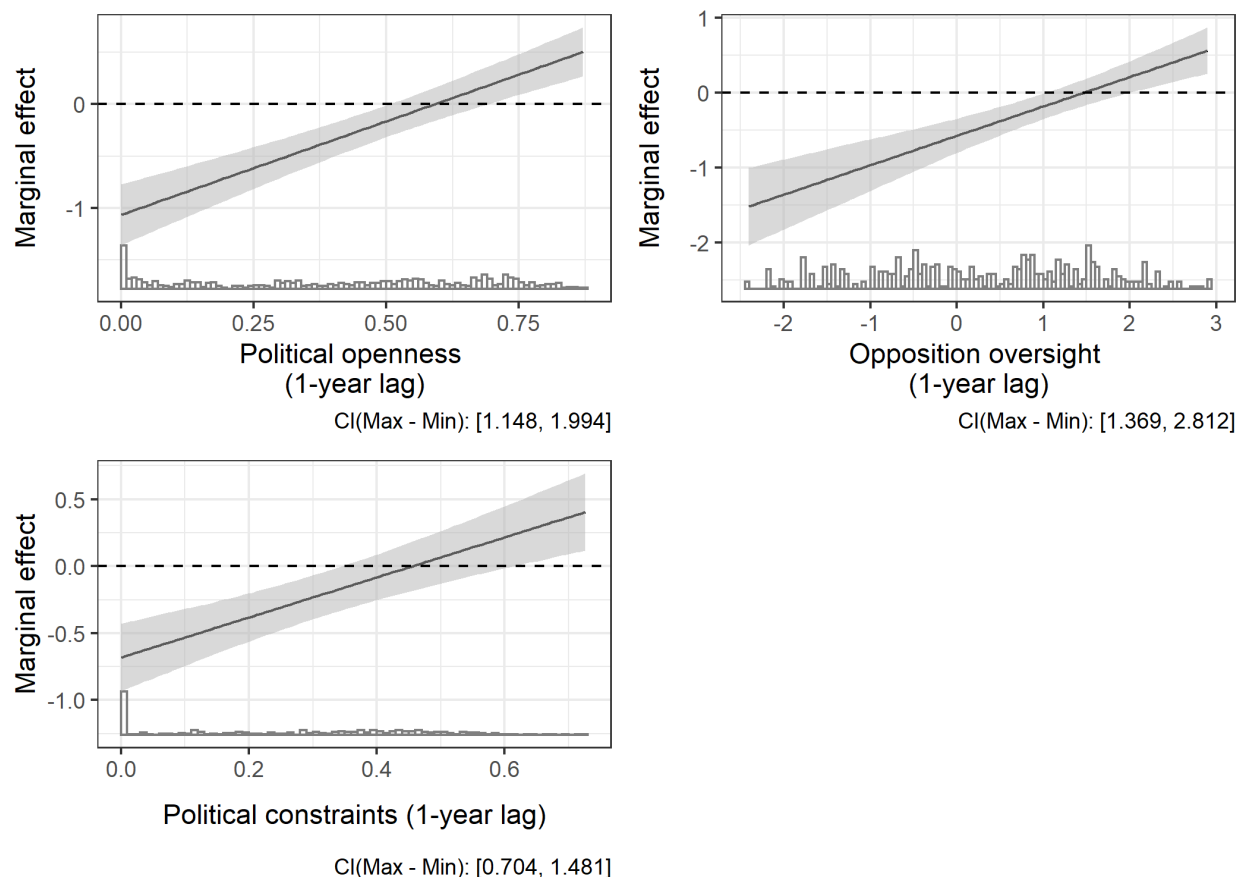

Figure A.7: Marginal effects of a positive judicial reform on intentional voting irregularities. Shaded areas represent 95% confidence intervals

Judicial independence and access to the courts

The theoretical mechanism proposed in the main text—that positive judicial reforms create incentives for more vigorous legal mobilization by regime opponents, raising the costs of electoral manipulation for ruling parties—rests in part on the assumption that citizens have access to the courts. In part, that is, because complaints about electoral malfeasance may not be brought to court only by citizens or political parties, but also by prosecutors. Nonetheless, high costs of bringing cases to court, procedural issues like questions of standing, or rules that require electoral complaints to be heard in specialized courts (see above) may inhibit access to the courts, making it harder for affected parties to bring cases and insulating the ruling party and its agents from costs associated with manipulated elections. The main explanatory variable in this study, *positive judicial reforms*, encompasses reforms that increase judicial

independence as well as judicial access, meaning that it alone does not distinguish between these two related mechanisms

While I am unaware of detailed cross-national data on judicial access for cases related to elections, the Comparative Constitutions Project offers a rough proxy that can be used to better isolate the independent effect of judicial independence (controlling for access). Specifically, CCP data records which entities are explicitly granted the right to challenge laws in court, and whether or not there is a constitutional *amparo* right—the right of any citizens to allege in court that their political or civil rights have been violated. Again, these are rough proxies; a broad right to challenge laws does not necessarily imply that allegations of electoral misconduct can be easily heard. For this reason, I do not include these variables in the main models. Still, given the unavailability of more fine-grained data, it is plausible that judicial openness of civil legal questions will be associated with judicial openness generally.

To construct this control variable, I add the values of three binary variables from the CCP dataset; these variable indicate whether the constitution formally grants the right to challenge laws to citizens or to lawyers, and whether there is a constitutional *amparo* provision. The combined variable, *citizen access*, thus ranges from zero to three. Most country-year observations in the dataset score zero on this measure, but approximately one-third of observations take a positive value. I include this variable as a control in models otherwise identical to those in the main text. As Table A.10 and the associated figures show, there is no substantive difference between the main models and those that include a proxy for judicial access; this helps improve confidence in the argument that judicial independence has an effect on electoral manipulation independent of access.

	Dependent variable:		
	Intentional voting irregularities		
	(1)	(2)	(3)
Positive judicial reform	-0.646***	-0.391***	-0.487***
	(0.141)	(0.104)	(0.117)
Political openness	-0.626***		
	(0.211)		
Opposition oversight		-0.312***	
		(0.043)	
Political constraints			-0.400*
			(0.225)
Executive election	0.011	0.044	0.001
	(0.077)	(0.075)	(0.078)

Proportional electoral system	0.036	0.097	0.025
	(0.099)	(0.096)	(0.100)
Mixed electoral system	-0.086	-0.078	-0.119
	(0.108)	(0.104)	(0.110)
Log GDP per capita (lagged)	-0.654***	-0.679***	-0.801***
	(0.119)	(0.102)	(0.104)
International monitors	-0.053	-0.067	-0.027
	(0.073)	(0.070)	(0.074)
Negative reform	-0.332**	-0.340**	-0.341**
	(0.141)	(0.141)	(0.146)
Judicial purges	-0.247***	-0.184***	-0.309***
	(0.064)	(0.060)	(0.061)
Court packing	-0.109	-0.032	-0.072
	(0.070)	(0.074)	(0.070)
Citizen access	0.116	0.083	0.218***
	(0.083)	(0.086)	(0.082)
Positive reform : Political openness	0.868***		
	(0.253)		
Positive reform: Opposition oversight		0.153**	
		(0.060)	
Positive reform : Political constraints			0.778***
			(0.267)
Constant	6.994***	7.333***	7.899***
	(0.885)	(0.805)	(0.843)
Country fixed effects	Yes	Yes	Yes
Observations	520	492	520
R^2	0.884	0.905	0.882
Adjusted R^2	0.854	0.878	0.851
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A.10: Weighted OLS model of election fraud (entropy balanced weights). All variables 1-year lagged, except *executive election*, *proportional electoral system*, *mixed electoral system*, and *international observers*
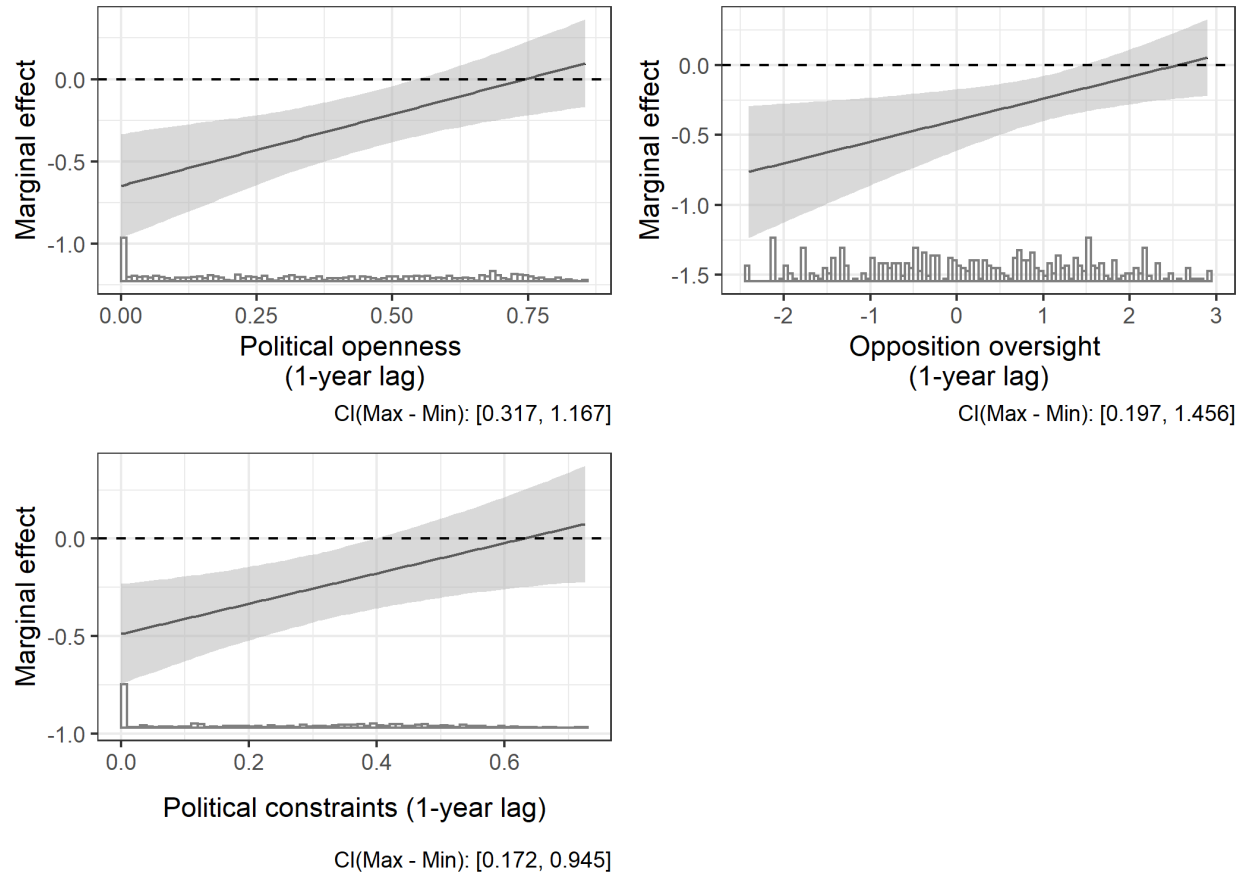
Figure A.8: Marginal effects of a positive judicial reform on intentional voting irregularities. Shaded areas represent 95% confidence intervals

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