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

ONLINE APPENDIX B

Cole J. Harvey

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Inter-temporal effects of reform within cases

A further implication of the theory presented in this paper is that *de jure* judicial reforms, by reducing electoral manipulation in some circumstances, will result in a better showing for opposition parties in the election at time one. This improved opposition strength leads to increased competitiveness of the election at time two, which in turn should cause the incumbent ruling party to dial up informal pressure on the courts. In places where the ruling party remains strong, this pressure then returns the courts to a more pliant mode, allowing for a return to baseline levels of election manipulation in the second election. In places where the opposition's success in the first election allows it to more seriously contest the ruling party, we should expect the ruling party's pressure campaign to be less effective, allowing for a more sustained reduction in election manipulation. Put in terms of an interaction effect, I expect that positive judicial reforms will have no significant effect on election fraud in the second election after reform in places where competition is low, and that the slope the marginal effects will be negative.

I check this implication by running the same models as those presented in Table 2 in the main text, with entropy-balanced weights for judicial reform, with a one-election lead value for *intentional voting irregularities* as the dependent variable. Control variables are also measured on a one-year lead, so that they refer to the election at hand in the model. Selection variables in the entropy-balancing stage remain lagged one year behind the reform. The results in Table B.1 and Figure B.1 are mixed, but generally supportive of the predictions outlined above. Model 14, which uses *political competition* as the moderator variable, is the most supportive; the effect of judicial reform at time one is insignificant for low-competition settings, but is significant and negative at higher levels of competition. Model 15 is also supportive, but with a null effect across all levels of *legislative oversight*. Model 16 does not support the predicted effects for elections at time two; a prior judicial reform is associated with reduced fraud across all levels of *political constraints*. Further research could investigate the circumstances under

which increased competition based on positive judicial reform 'tips over' into durable opposition strength, and the circumstances under which regime pressure is successful at defusing such movements.

	Dependent variable:		
	Intentional voting irregularities (time 2)		
	(14)	(15)	(16)
Positive judicial reform	-0.12	-0.13	-0.25**
	(0.14)	(0.09)	(0.12)
Political competition	-1.58***		
	(0.15)		
Opposition oversight		-0.36***	
		(0.04)	
Political constraints			-0.70***
			(0.19)
Executive election	-0.17**	-0.06	-0.08
	(0.08)	(0.08)	(0.09)
GDP per capita	-0.02	-0.24***	-0.24***
	(0.08)	(0.08)	(0.08)
International monitors	-0.31***	-0.22***	-0.36***
	(0.06)	(0.06)	(0.06)
Proportional representation	-0.21**	-0.12	-0.09
	(0.10)	(0.10)	(0.11)
Mixed electoral system	-0.03	-0.06	0.02
	(0.11)	(0.11)	(0.12)
Negative judicial reform	-0.16	-0.08	-0.14
	(0.12)	(0.12)	(0.13)
Judicial purges	-0.25***	-0.27***	-0.22***
	(0.06)	(0.06)	(0.07)
Court packing	-0.15**	-0.11	-0.25***
	(0.08)	(0.07)	(0.08)
Positive judicial reform : Pol. Comp.	-0.12		
	(0.22)		
Positive judicial reform : Opp. oversight		-0.04	
		(0.06)	

Positive judicial reform: Pol. constraints			-0.04
			(0.26)
Country fixed effects	Yes	Yes	Yes
Constant	2.76***	3.94***	3.87***
	(0.65)	(0.69)	(0.69)
Observations	648	607	649
R^2	0.88	0.89	0.85
Adjusted R^2	0.85	0.87	0.82
Note:			*p<0.1; **p<0.05; ***p<0.01

Table B.1: Weighted OLS (entropy-balanced weights) models of election fraud in second election after reforms. All non-judicial variables are lagged one year behind the second election, except *executive election* and *international monitors*.

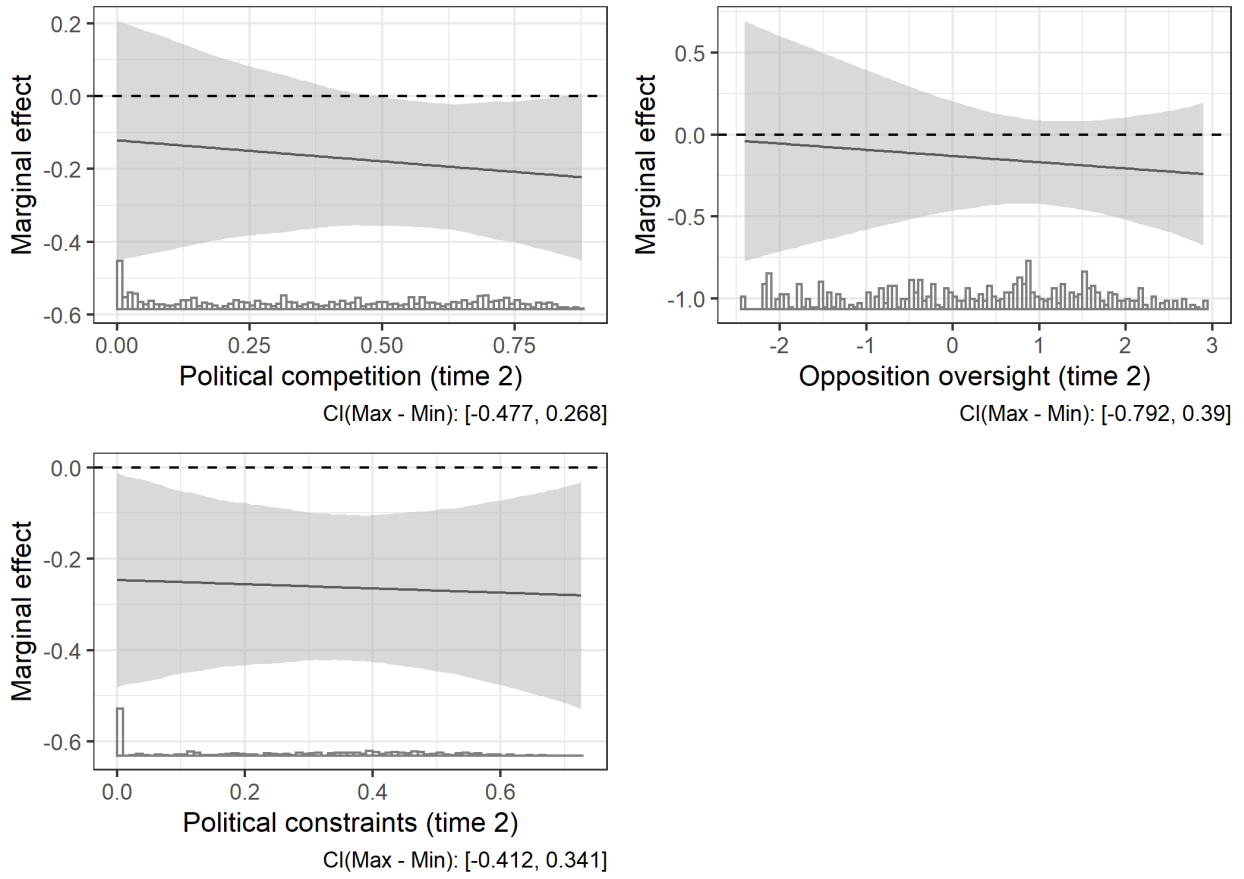

Figure B.1: Marginal effects of a positive judicial reform on intentional voting irregularities. Shaded areas represent 95% confidence intervals

Given that *de jure* judicial reforms result in reduced manipulation in low-competition settings in the short-term, but not—apparently—in subsequent elections raises another testable implication of this theory. Any potentially destabilizing effect of positive judicial reforms due to reduced election manipulation in low-competition environments is likely to be felt in the first subsequent election, rather than in later contests after pressure is applied. To test this implication, I use the binary *democratic transition* variable in V-Dem as a dependent variable (Boix, Miller, and Rosato 2013). I test three alternative models, with the dependent variable leading the reform by one year, three years, and five years. The models include control variables for *regime duration*, and *GDP per capita*. These controls lead the judicial reform by the same number of years as the dependent variable. The models also control for indicators of attacks on the judiciary. Selection variables for *positive judicial reform* remain the same. For simplification, I only run these models using the *political openness variable*. Since the destabilizing effect is posited to occur due to better-than-expected opposition results in election years, I interact the reform and openness variables with an indicator for *election years*. I expect that judicial reform will increase the risk of transition in immediately subsequent election years, but not thereafter. I also expect

there will be no effect for transitions in ordinary years, since it is elections that are the mechanism for revealing information about opposition strength in the absence of election fraud.

	Dependent variable:		
	Democratic transition	Democratic transition (3-year lead)	Democratic transition (5-year lead)
	(1)	(2)	(3)
Positive judicial reform	0.03*	0.01	0.002
	(0.02)	(0.02)	(0.02)
Political openness	-0.16***		
	(0.02)		
Election year	0.05***		
	(0.02)		
Regime duration	-0.002***		
	(0.0002)		
GDP per capita	0.03***		
	(0.01)		
Political openness (3-year lead)		-0.04	
		(0.02)	
Election-year (3-year lead)		0.11***	
		(0.02)	
Regime duration (3-year lead)		-0.001***	
		(0.0002)	
GDP per capita (3-year lead)		0.01	
		(0.01)	
Political openness (5-year lead)			-0.06**
			(0.03)
Election-year (5-year lead)			0.10***
			(0.02)
Regime duration (5-year lead)			-0.002***
			(0.0002)
GDP per capita (5-year lead)			0.01
			(0.01)

Negative judicial reform	-0.002	0.03*	0.04**
	(0.02)	(0.02)	(0.02)
Judicial purge	-0.01*	-0.02***	-0.04***
	(0.01)	(0.01)	(0.01)
Court packing	0.02**	0.01	0.02**
	(0.01)	(0.01)	(0.01)
Positive jud. Reform : Political openness	-0.05		
	(0.03)		
Positive jud. reform : Election year	0.11***		
	(0.02)		
Political openness : Election year	-0.03		
	(0.03)		
Positive jud. reform : Political openness : Election year	-0.18***		
	(0.04)		
Positive jud. reform : Political openness (3-year lead)		-0.01	
		(0.03)	
Positive jud. reform : Election year (3-year lead)		-0.05*	
		(0.03)	
Political openness (3-year lead) : Election year (3-year lead)		-0.08***	
		(0.03)	
Positive jud. reform : Political openness (3-year lead) : Election year (3-year lead)		0.02	
		(0.05)	
Positive jud. reform : Political openness (5-year lead)			0.01
			(0.03)
Positive jud. reform : Election year (5-year lead)			-0.08***
			(0.03)
Political openness (5-year lead) : Election year (5-year lead)			-0.08***
			(0.03)

Positive jud. reform : Political openness (5-year lead) : Election year (5-year lead)			0.10**
			(0.05)
Country fixed effects	Yes	Yes	Yes
Constant	-0.15	-0.08	0.21***
	(0.09)	(0.10)	(0.08)
Observations	2,903	2,547	2,316
Log Likelihood	146.58	312.29	340.79
Akaike Inf. Crit.	-29.16	-368.58	-437.58
Note:			*p<0.1; **p<0.05; ***p<0.01

Table B.2: Entropy balanced logit models

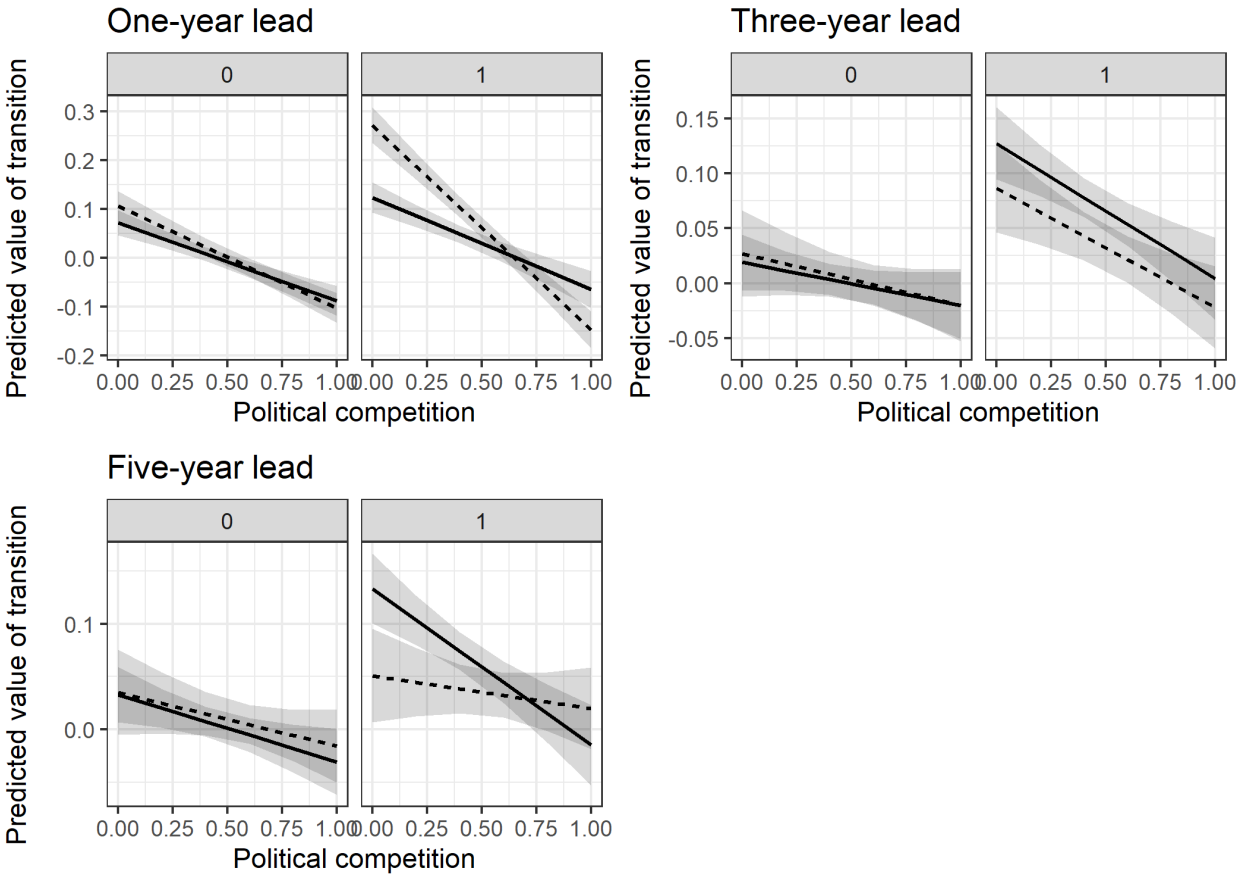

Figure B.2: Marginal effects of judicial reform on regime transition one year, three years, and five years from reform. Dashed lines indicate the presence of judicial reform; solid lines represent no reform. Left panels (marked '0') indicate ordinary years, right panels (marked '1') represent election years. Control variables held at the mean.

Figure B.2, which plots the three-way interaction effects, shows that these predictions are upheld. In election years, reform is associated with a significant and substantively large increase in the risk of regime transition in low-openness settings one year after reform. After three years, there is no significant difference between cases with and without judicial reforms during election years. Finally, by five years, the relationship is reversed—the risk of regime change is significantly lower in low-openness settings that enacted a judicial reform compared to those that did not. This is in keeping with the theory that ruling parties in non-democracies can reap long-term benefits from empowering their courts, but that such an approach entails the short-term risks described in this article.

Judicial independence and the risk of post-election protest

In the main text of the paper, the findings are interpreted as supportive of principal-agent theories of election manipulation and as unsupportive of models that posit that protest risk is the main deterrent of election fraud. To further test this interpretation, the models in this section use a measure of *anti-fraud protests* as a dependent variable. These models are not a complete review of the question, which is deserving of more thorough academic study; nevertheless, taken in concert with the findings in the main paper and other sections of this appendix, the results offer further evidence that protest-risk does not drive the outcomes in the main study.

To build the models, I combine data from V-Dem with data from the National Elections Across Democracy and Autocracy (NELDA) dataset (Hyde and Marinov 2012), which includes a measure of post-election protests that include allegations of fraud. Specifically, the variable *anti-fraud protests* takes on a value of one if the variable 'nelda30' takes on a value of one[1], and takes on a value of zero if there were no protests or if they did not include allegations of manipulation. I make use of the same selection variables for *positive judicial reform* as have been used in all prior models. Finally, I include several control variables that could also account for the occurrence of post-election protest: the winner's margin of victory, the openness of civil society participation, GDP growth rate, whether the election is for the executive or legislature, a physical repression index, and a categorical measure of regime type.[2] All of these measures are taken from V-Dem, and with the exception of *executive election* are lagged by one year. The models also include country fixed effects.

In these models, I interact the judicial reform treatment variable with the measure of *intentional voting irregularities* and the underlying level of competition. Table B.3 and Figure B.3 present the results of these models. The marginal effects in Figure B.3 are especially useful for teasing out the implications of the three-way interaction terms. In each sub-figure, the left panel holds *judicial reform* at zero, while the right panel holds it at one. The x-axis refers to the measure of competitiveness used in the model. To capture the extreme cases, the extent of election is rigging is held at its minimum (red line) and maximum (blue line) values. In all three cases, there is no evidence that more formally independent courts increase protest risk when elections are rigged. In fact, positive judicial reforms appear to *reduce* the risk of post-election protest, especially in lower-competition regimes.

[1] This variable indicates if there were riots or protests after the election that were backed by allegations of fraud.
[2] Categories include closed autocracy, electoral autocracy, electoral democracy, and liberal democracy.

	Dependent variable:		
	Anti-fraud protests		
	(20)	(21)	(22)
Positive judicial reform	-0.01	-0.02	-0.03
	(0.08)	(0.06)	(0.06)
Intentional voting irregularities	0.19***	0.21***	0.14***
	(0.05)	(0.03)	(0.04)
Political openness	0.28**		
	(0.12)		
Opposition oversight		-0.01	
		(0.03)	
Political constraints			0.22*
			(0.12)
Winner margin	0.14	0.14	0.15
	(0.10)	(0.11)	(0.10)
Civil society openness	0.13	0.27*	0.20
	(0.15)	(0.16)	(0.14)
GDP growth rate	0.26	0.21	0.22
	(0.17)	(0.17)	(0.17)
Executive election	0.01	0.01	0.03
	(0.03)	(0.03)	(0.03)
Physical integrity	0.15	0.23	0.20
	(0.14)	(0.15)	(0.13)
Closed authoritarian regime	0.16	0.10	0.07
	(0.10)	(0.10)	(0.09)
Electoral authoritarian regime	-0.09*	-0.03	-0.06
	(0.05)	(0.05)	(0.04)
Pos. judicial reform:Intentional voting irreg.	-0.15*	-0.07	-0.09
	(0.09)	(0.05)	(0.07)
Pos. judicial reform:Pol. openness	-0.06		
	(0.14)		
Intentional voting irreg.:Pol. openness	0.02		
	(0.07)		
Pos. judicial reform:Intentional voting irreg.:Pol. openness	0.10		

	(0.14)		
Pos. judicial reform : Opposition oversight		0.01	
		(0.04)	
Intentional voting irreg.: Opposition oversight		-0.02	
		(0.02)	
Pos. judicial reform:Intentional voting irreg.: Opposition oversight		0.01	
		(0.03)	
Positive judicial reform:Pol. constraints			-0.08
			(0.15)
Intentional voting irreg.:Pol. constraints			0.16*
			(0.10)
Pos. judicial reform:Intentional voting irreg.:Pol. constraints			-0.02
			(0.17)
Country fixed effects	Yes	Yes	Yes
Constant	0.06	0.02	0.05
	(0.14)	(0.15)	(0.14)
Observations	806	765	807
Log Likelihood	-533.16	-521.50	-535.37
Akaike Inf. Crit.	1,296.33	1,273.01	1,300.73
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table B.3: Logit model of post-election anti-fraud protests, with entropy balanced weights.

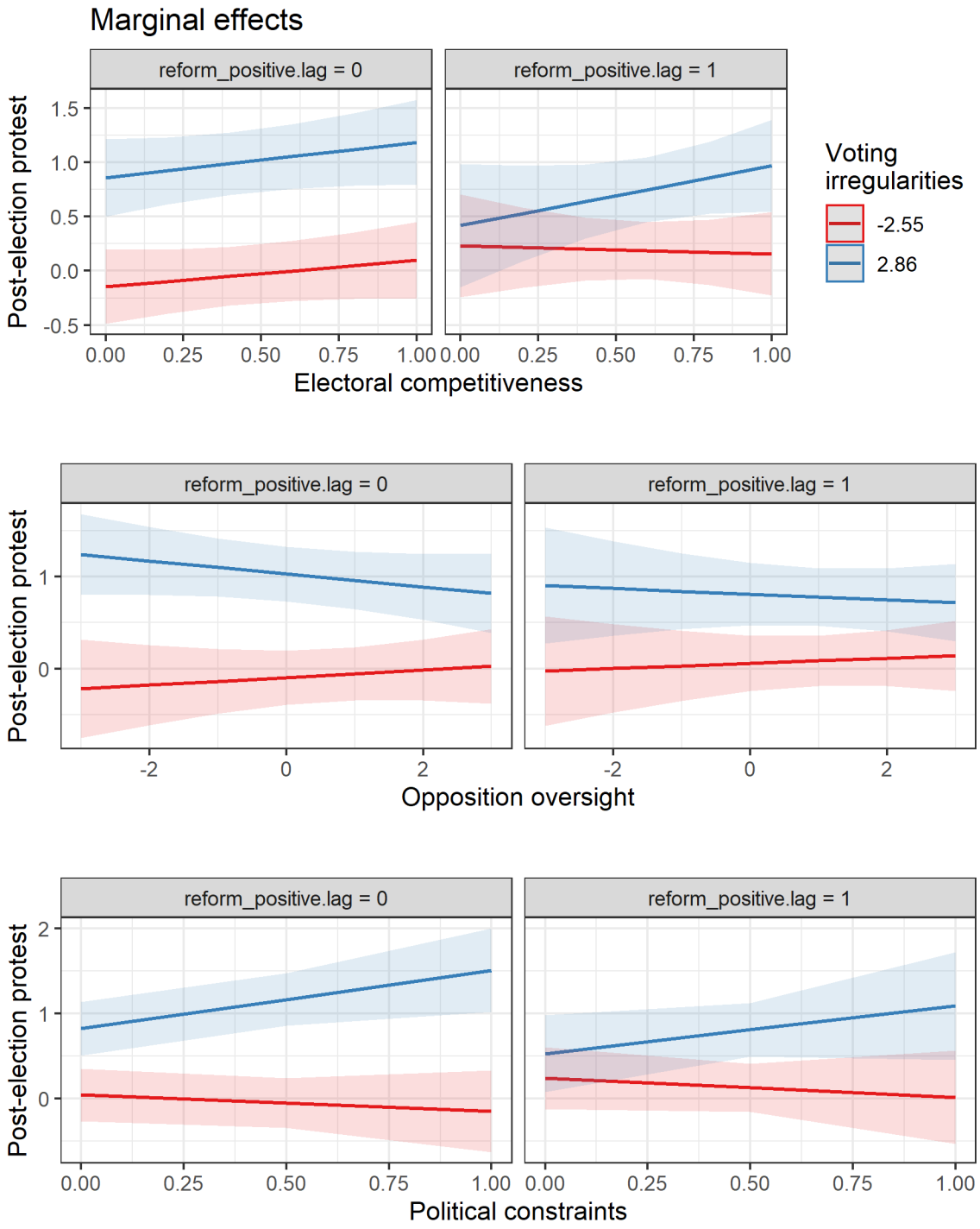

Figure B.3: Marginal effects of a positive judicial reform on anti-fraud protests. Shaded areas represent 95% confidence intervals

The post-Cold War effect

One of the foundations of the theory of judicial independence articulated in the main text is that ruling parties in non-democracies derive genuine benefits from independent courts; they will allow those courts to operate independently so long as those benefits outweigh the negative costs of unfavorable court rulings. This yields an additional testable hypothesis in the context of election manipulation. The end of the Cold War changed non-democratic governments' incentives by increasing the 'democracy premium' available in the international system (Hyde 2011). During the Cold War, democracy credentials often took second consideration behind anti-communism for the United States and other democracy-promoters. Afterward, non-democracies could gain material and symbolic benefits by at least giving the appearance of democratic practice. This shift should, in theory, increase the benefit of allowing courts to rule independently on election-fraud cases for regimes that are relatively secure. We should expect to see a larger reduction in fraud in non-competitive settings in the post-Cold War period than during the Cold War, as a result. The model below includes a dummy variable, *post-Cold War*, which takes on a value of 1 for country-years after 1989. The results in Figure B.4 show the predicted effects.

	Dependent variable:
	Intentional voting irregularities (29)
Political openness	-0.74***
	(0.20)
Positive judicial reform	-0.56***
	(0.19)
Post-Cold War	-0.25
	(0.15)
Executive election	-0.06
	(0.07)
PR system	0.02
	(0.08)
Mixed electoral system	0.05
	(0.10)
GDP per capita (log)	-0.17**
	(0.08)
Negative judicial reform	-0.07
	(0.11)
Judicial purges	-0.16***
	(0.05)

Court packing	-0.17***
	(0.06)
Political openness : Positive judicial reform	0.33
	(0.38)
Political openness : post-Cold War	-0.06
	(0.26)
Positive judicial reform : post-Cold War	-0.36
	(0.26)
Political openness : Positive judicial reform : post-Cold War	1.03**
	(0.47)
Country fixed effects	Yes
Constant	3.73***
	(0.64)
Observations	771
R^2	0.87
Adjusted R^2	0.84
Note:	*p<0.1; **p<0.05; ***p<0.01

Table B.4: 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*

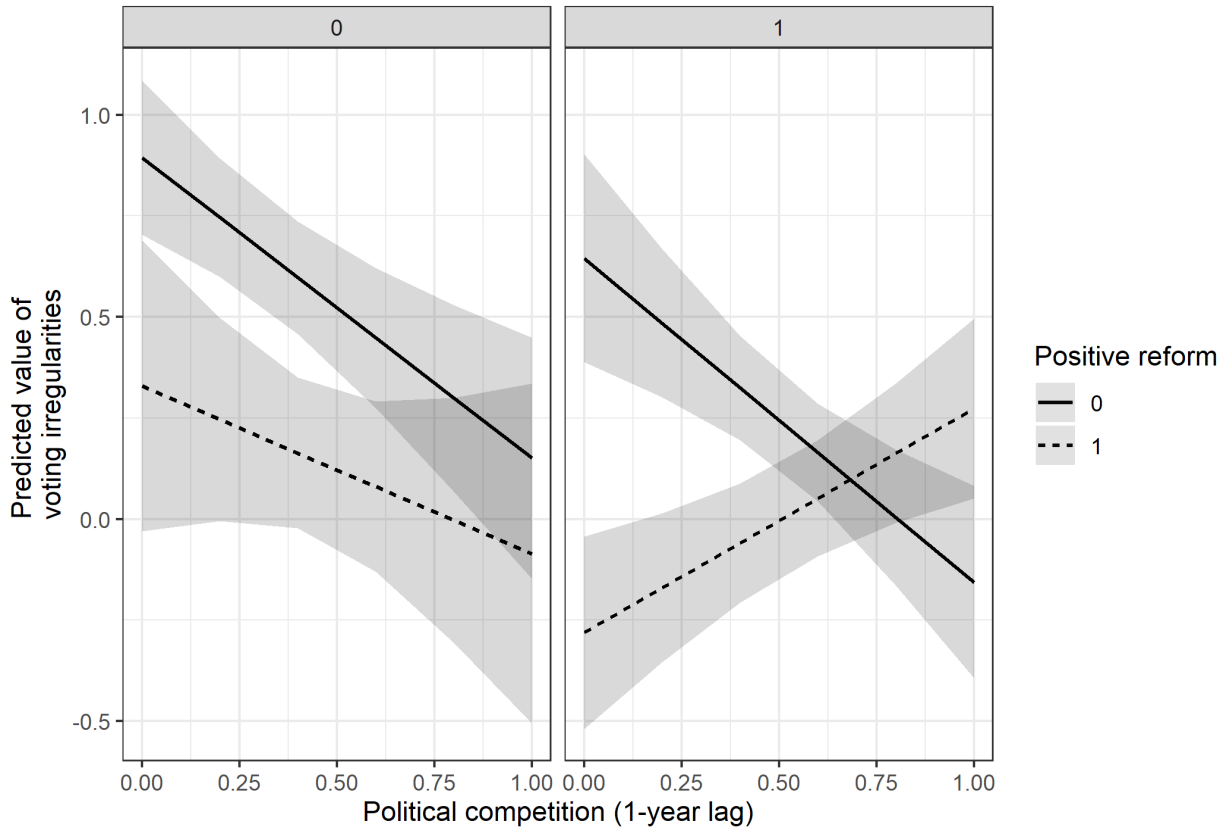

Figure B.4: Marginal effects of a positive judicial reform on intentional voting irregularities. Right-hand panel represents the post-Cold War environment. Shaded areas represent 95% confidence intervals. Control variables held constant at the mean.

Alternative coding of democracy

To confirm that the results do not depend on specific features of the dataset based on the *competitiveness of participation* measure of democracy, I also use the binary coding of democracy developed by Cheibub et al (Cheibub, Gandhi, and Vreeland 2010). Their coding scheme classifies regimes as democracies if they meet four conditions: 1) the chief executive must be popularly elected or chosen by a popularly elected body, 2) the legislature must be popularly elected, 3) there must be more than one party competing in elections, and 4) at least one alternation in power under the same electoral rules must have occurred. The following models exclude all cases that qualify as democracies under this coding scheme. As Table B.5 shows, it is a more restrictive dataset, since it will exclude some country-years where alternation under the status-quo electoral rules has not yet taken place. Nonetheless, as Figure B.5 shows, the results mirror those in the main text.

	Dependent variable:		
	Intentional voting irregularities		
	(26)	(27)	(28)
Positive judicial reform	-0.93***	-0.61***	-0.87***
	(0.15)	(0.12)	(0.13)
Political openness	-0.44		
	(0.33)		
Opposition oversight		-0.30***	
		(0.08)	
Political constraints			-1.43***
			(0.27)
Executive election	-0.03	-0.10	-0.05
	(0.06)	(0.07)	(0.06)
PR system	-0.39**	-0.48***	-0.36**
	(0.15)	(0.15)	(0.14)
Mixed electoral system	-0.04	-0.07	0.11
	(0.10)	(0.09)	(0.09)
GDP per capita (log)	0.07	0.001	0.06
	(0.08)	(0.09)	(0.08)
International observers	-0.42***	-0.37***	-0.31***
	(0.06)	(0.07)	(0.07)
Negative judicial reform	0.43**	0.40**	0.51**
	(0.17)	(0.19)	(0.20)
Judicial purges	-0.09	-0.11	-0.13
	(0.12)	(0.13)	(0.13)
Court packing	-0.03	-0.12	0.12
	(0.12)	(0.11)	(0.10)
Positive judicial reform:Political openness	0.91***		
	(0.35)		
Positive judicial reform:Opposition oversight		0.47***	
		(0.08)	
Positive judicial reform:Political constraints			1.91***

			(0.31)
Country fixed effects	Yes	Yes	Yes
Constant	1.26*	1.85***	1.05
	(0.70)	(0.71)	(0.66)
Observations	343	319	343
R^2	0.94	0.95	0.94
Adjusted R^2	0.92	0.93	0.92
Note:			*p<0.1; **p<0.05; ***p<0.01

Table B.5: 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*

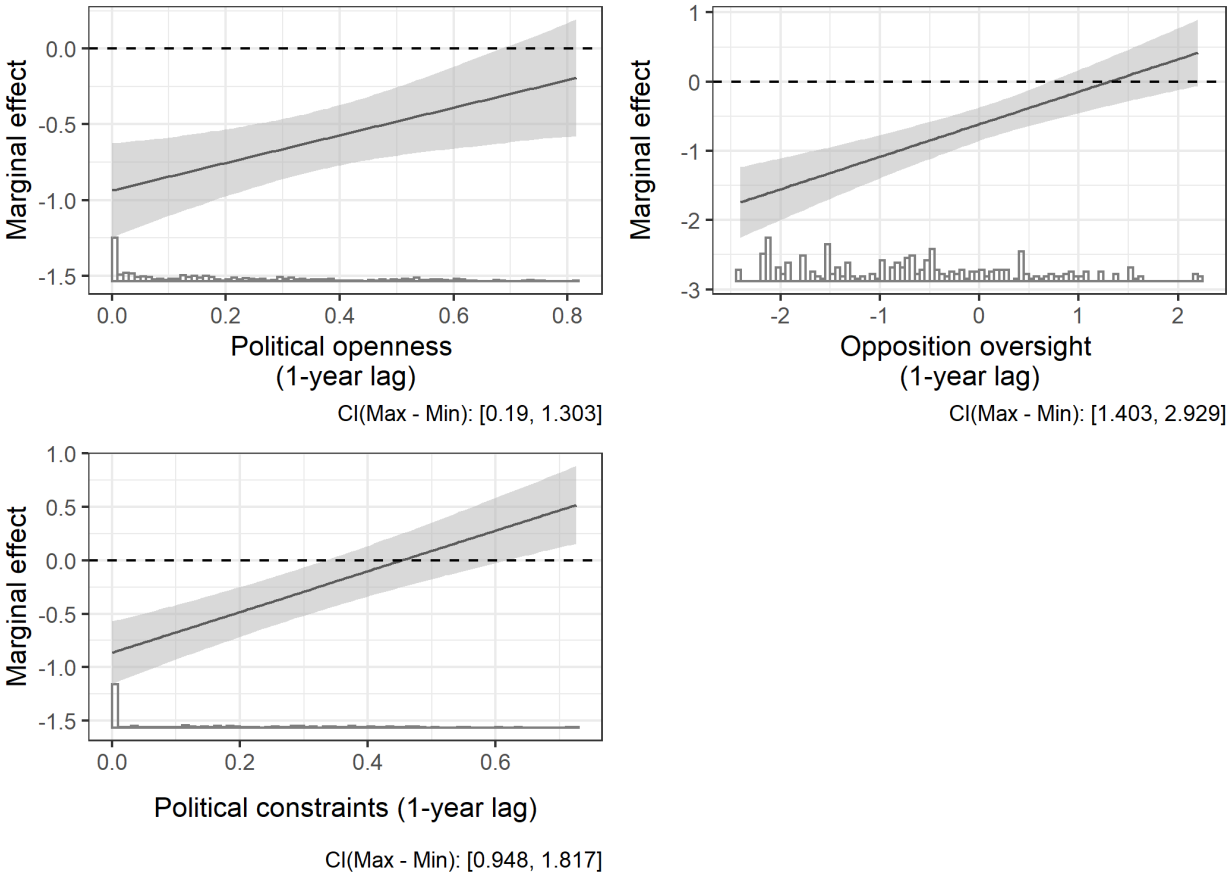

Figure B.5: Marginal effects of a positive judicial reform on intentional voting irregularities. Shaded areas represent 95% confidence intervals

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