How Executive Institutions Shape Affective Polarization: Evidence from Israel, 1975-2001

Supplementary Information

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Contents

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S1 Countries and Institutional Coding

In the main text, I describe my decision to code countries as "presidential" only if their head of government satisfies two criteria: they are elected separately from the legislature (or appointed by a head of state who is also not chosen by the legislature) and they serve a fixed term in office, with no need to rely on the legislature for confidence. This coding scheme ensures, in line with my theory, that I only consider countries presidential if their institutional design provides both separate origin and separate survival for the executive.

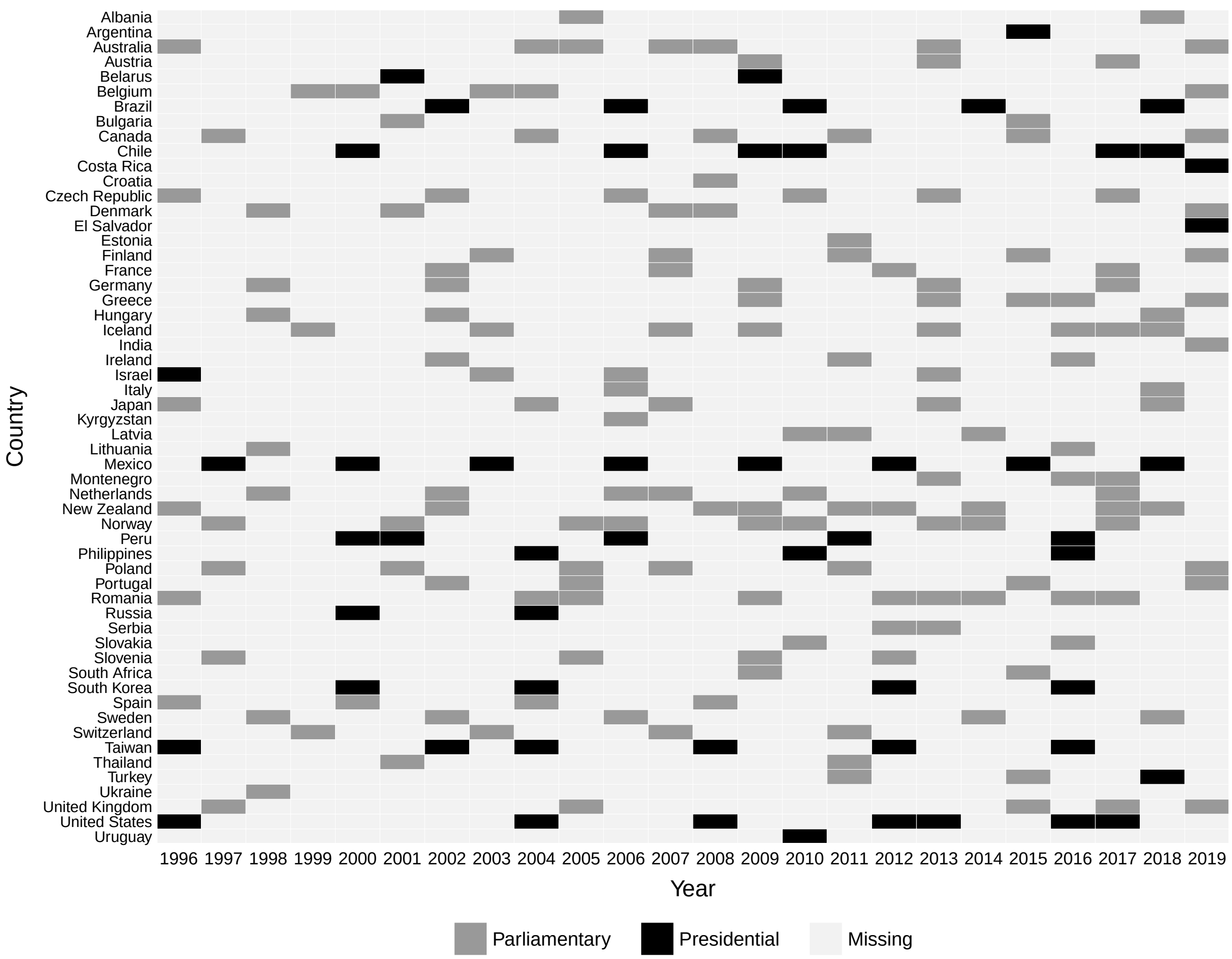

Figure S1: Country-Year Institutional Coding, Cross-National Survey Analyses

Figures S1 and S2 show the country-years included in cross-national survey and synthetic control analyses, respectively. Black cells indicate country-years coded as presidential, while dark gray cells indicate country-years coded as parliamentary. Light gray cells indicate country-years that are not included. Country-years may be missing for a different reasons. In Figure S1, missing country-years are most often due to surveys not being fielded in that year. In Figure S2, countries

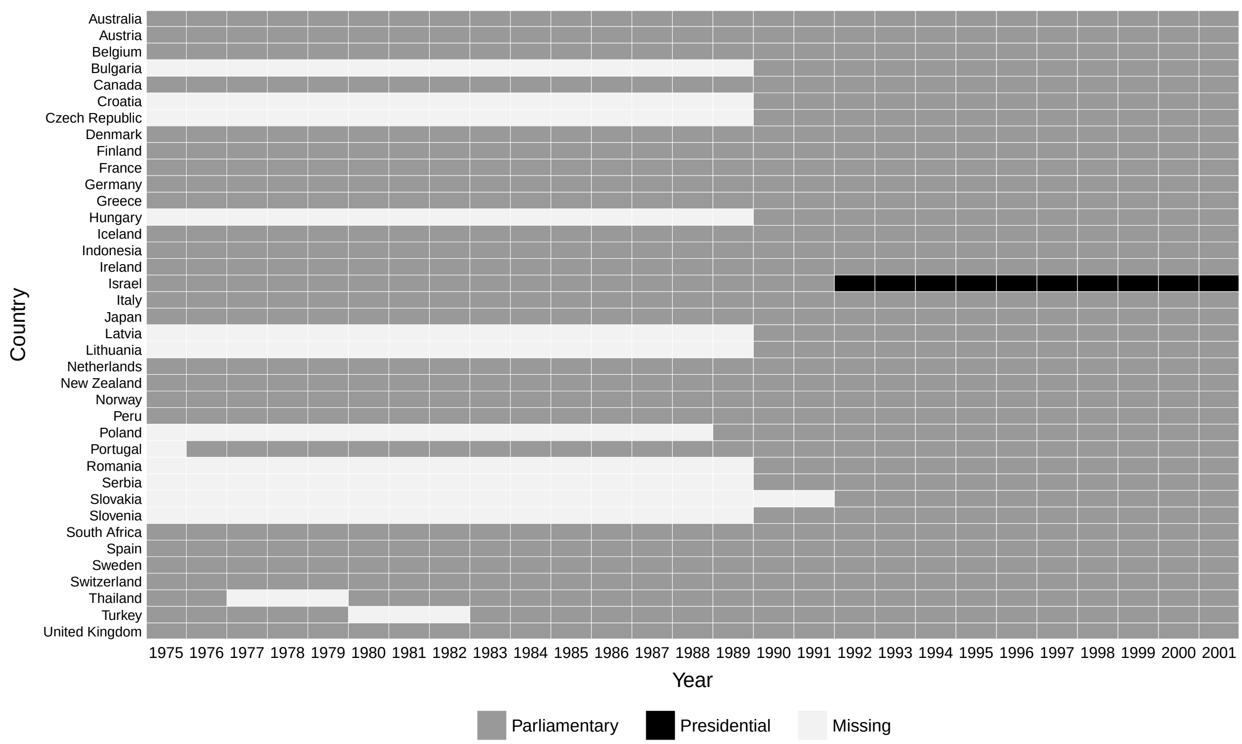

Figure S2: Country-Year Institutional Coding, Synthetic Control Analyses

are left off the plot entirely if they did not have parliamentary-style executive insitutions for the entirety of the time period under consideration (1975-2001). Countries that had presidential institutions or oscillated between the two cannot serve as control units and therefore are not included in the synthetic control analysis. Across all analyses, I do not provide any institutional coding in country-years where a state was governed by an autocratic or military regime, an exclusion which causes, for instance, the brief interregna observed in Thailand's and Turkey's time series. Finally, many countries—particularly in Eastern Europe—did not yet exist as separate states in the early part of the time series. Below, I clarify how the synthetic control esitmator handles these unbalanced panels.

An alternate coding scheme would be to consider countries with *either* separate origin or survival as presidential, thereby grouping semi-presidential systems under the "presidential" label. Samuels and Shugart (2010) recommend considering semi-presidential systems as more similar to presidencies than to parliamentary systems, which would suggest a coding scheme such as this one. Figures S3 and S4 display country-year coding under this alternate scheme, similar to Figures S1 and S2. Elsewhere in the Supplementary Information, I present results of empirical analyses using this coding scheme.

However, my decision to require both separate origin and survival as criteria for being considered presidential aligns more closely with other datasets providing cross-national coding of

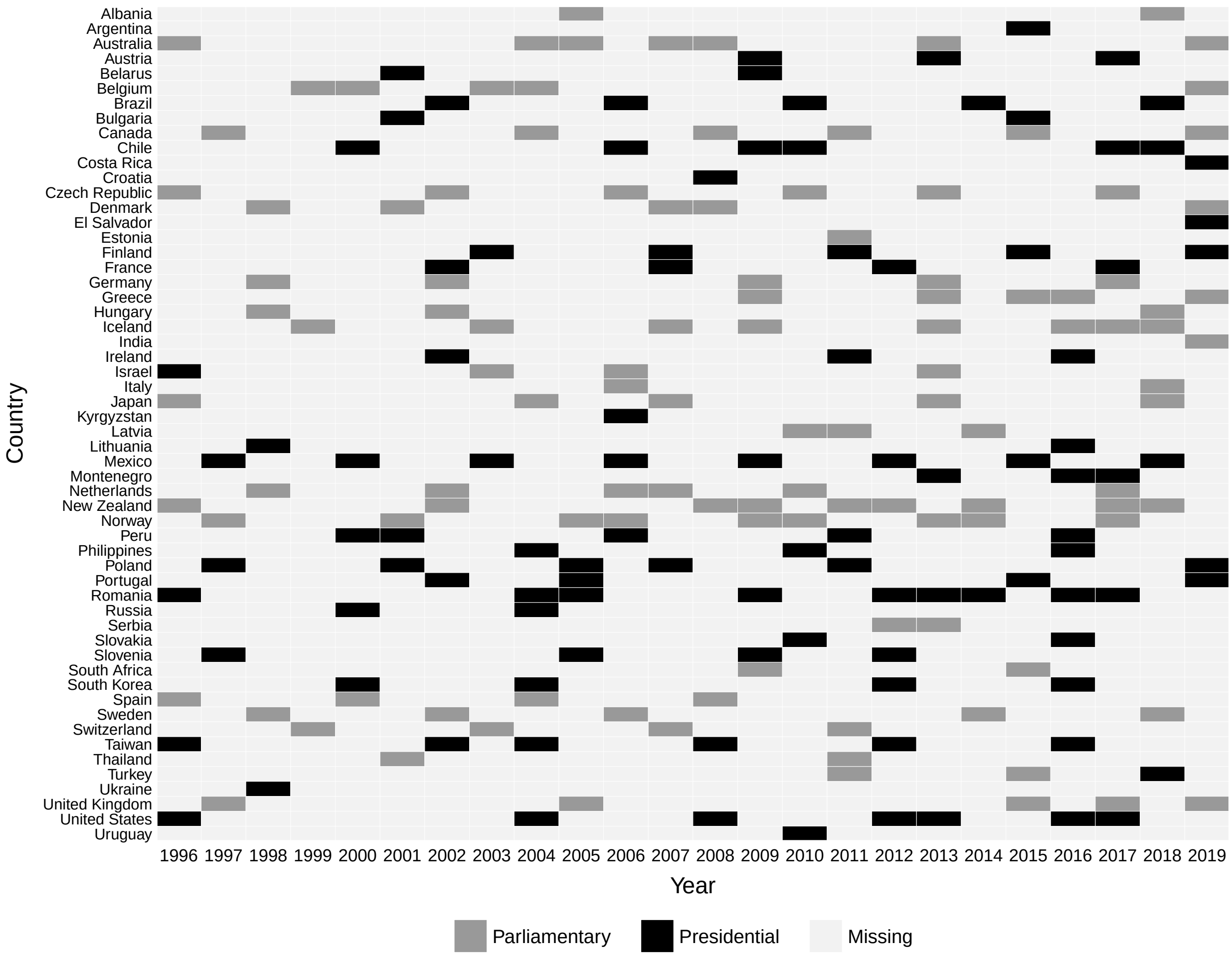

Figure S3: Alternative Country-Year Institutional Coding, Cross-National Survey Analyses

executive institutions. Two-sample t-tests assessing the value of the presidentialism index from the Varieties of Democracy project (V-Dem; Coppedge et al. 2020) shows that presidential and parliamentary institutions are better separated under this coding scheme ($\delta = 0.707$, $t = 22.2$) than under the alternate coding scheme that considers semi-presidential systems as presidential ($\delta = 0.57$, $t = 19.838$). The difference between these two differences-in-means is statistically significant ($\beta = -0.138$, $p < 0.001$). The same is true when comparing the two coding schemes to the presidentialism index from the Database of Political Institutions (DPI; Cruz et al. 2021): $\delta = 0.799$, $t = 77.674$ for the main coding scheme and $\delta = 0.685$, $t = 55.6$ for the alternate scheme. The difference between these two differences-in-means is also statistically significant ($\beta = -0.114$, $p < 0.001$).

Although I need to categorize institutional designs according to a coarse coding scheme in order to identify treatment effects, this can leave questions about the many variations on designs that do not fit neatly into one category or the other. On one hand, semi-presidential subtypes such

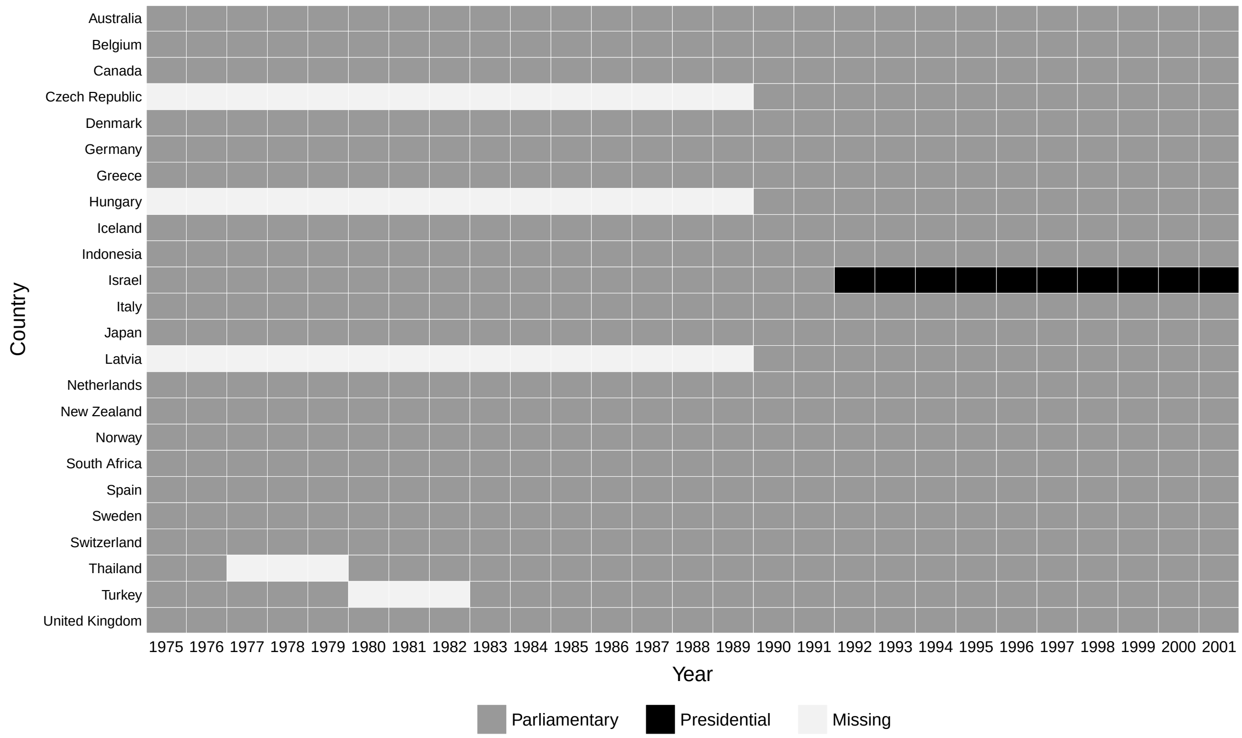

Figure S4: Alternative Country-Year Institutional Coding, Synthetic Control Analyses

as premier-presidential or president-parliamentary systems are classified as “parliamentary” under this coding scheme because they have fused survival mechanisms (Shugart and Carey 1992). On the other hand, heads of government may carry the “presidential” label while actually remaining quite accountable to the legislature, a feature that might suggest the case needs to be coded as “parliamentary” here. Below, I highlight a selection of edge cases in the main coding scheme and clarify my coding decisions in each.

Austria Austria’s head of government is the Federal Chancellor, who is appointed by the popularly elected President. However, the Chancellor requires the confidence of the National Council to remain in office and can be removed at any time (Müller 2003), thus lacking separate survival.

Bulgaria Bulgaria’s system is often described as “parliamentarized semi-presidentialism” (Siaroff 2003). Their head of government is the Prime Minister, who is formally appointed by the popularly elected President. In most cases, this is a *pro forma* appointment offered to the leader of the largest party or coalition in the National Assembly. The Prime Minister must maintain the confidence of the National Assembly and can be dismissed by a no-confidence vote (Elgie 2011). Both origin and survival are thus fused.

Croatia Under the 1990 constitution, Croatia had a strong semi-presidential system in which the President exercised considerable authority and could dismiss the Prime Minister at will. Even under this arrangement, however, the Prime Minister was formally accountable to Parliament, lacking separate survival. Constitutional amendments around the turn of the century transferred most executive authority to the Prime Minister, effectively converting Croatia to a parliamentary system (Kasapović 2012).

Finland Finland's president has been directly elected since 1994 and historically wielded significant powers, particularly in foreign policy and government formation. However, the Prime Minister is head of government and has always served at the pleasure of Parliament. The 2000 constitution further reduced presidential powers, transferring authority over government formation to Parliament and cementing the Prime Minister's dependence on parliamentary confidence (Nousiainen 2001).

France France is the paradigmatic case of semi-presidentialism (Duverger 1980). The President is directly elected, exercises substantial executive authority, and appoints the Prime Minister, who serves as the head of government. However, the Prime Minister can be dismissed by the National Assembly, effectively requiring the President to select a Prime Minister with parliamentary support. At any rate, the requirement for parliamentary confidence marks a lack of separate survival.

Lithuania Lithuania operates a premier-presidential system in which the President is directly elected but the Prime Minister serves as head of government and is elected (and can be removed) by the Seimas (Elgie 2011). Both origin and survival are thus fused.

Peru Peru is perhaps the most ambiguous case. The President is directly elected and serves as the head of government with a fixed term, which would ordinarily suggest a presidential classification. Peru's unique institutional configuration lies in the interpretation of Congress' power to impeach and remove the President. Although the 1993 constitution provides many reasons a President can be removed, impeachment proceedings in practice most often stem from a charge of "moral incapacity," a vague term left up to interpretation by Congress (Brown 2024). Parties in Congress have wielded this charge liberally, applying it to situations ranging from corruption scandals to policy disagreements to political expediency (Cassinelli 2025). As a result, "leaders without significant support in Congress face a constant threat of removal" (Sanborn and García Nice 2023). The fate of Peru's modern-day presidents shows how far off the rails this arrangement has gone in recent years: seven presidents have left office since 2018 due to impeachment or the threat thereof, leading one Latin American media outlet to describe impeachment as "a political crowbar wielded whenever Congress wants to pry open the presidency" (The Latin American Post Staff 2025). Because the President's survival depends on the confidence of Congress *in practice*, I consider Peru to have not satisfied the requirements for separate survival.

Poland Poland's Prime Minister is appointed by the directly elected President but must receive a vote of confidence from the Sejm. In the case that the confidence vote fails, the Sejm assumes the power to nominate the Prime Minister. As a consequence, the President must nominate Prime Ministers who have parliamentary support. The Prime Minister can then be removed by the Sejm through a vote of no confidence (Jasiewicz 2008). The head of government thus lacks both separate origin and separate survival from the legislature.

Portugal Portugal's popularly elected President appoints the Prime Minister, who must maintain the confidence of the Assembly of the Republic. The 1982 constitutional revision substantially curtailed the President's power to dismiss the government (Costa Lobo 2005), and the Prime Minister is typically the leader of the largest party or coalition in the Assembly. Portugal's Prime Minister thus lacks separate survival.

Romania Romania's popularly elected President nominates a candidate for Prime Minister in consultation with Parliament. The candidate must receive a vote of confidence from both parliamentary chambers to be appointed. The Prime Minister can be removed by a vote of no confidence (Roper 2002), indicating that Romania lacks separate survival.

Serbia Under the 1990 constitution, Serbia's President was directly elected with broad formal powers. Nevertheless, the Prime Minister was the head of government and formally accountable to the National Assembly (Spasojević 2022). The 2006 constitution established a more parliamentary system with a largely ceremonial presidency. Under both arrangements, the head of government lacked separate survival from the legislature.

Slovakia Before 1999, Slovakia's President was elected by the National Council, making the system straightforwardly parliamentary. A constitutional amendment in 1999 introduced direct popular election of the President (Malová and Rybár 2012). Even after this change, the Prime Minister remains the head of government and requires parliamentary confidence, thus lacking separate survival.

Slovenia Slovenia's directly elected President nominates the Prime Minister following consultation with the National Assembly (Cerar 1999). In practice, the Prime Minister is typically the leader of the largest party or coalition in the National Assembly. Because the Prime Minister must be elected by the National Assembly, they are typically the leader of the largest party or coalition in the Assembly. The Prime Minister can be removed through a vote of no confidence, thus lacking either separate origin or separate survival.

Switzerland Switzerland is a unique case, often referred to as "assembly-independent" (Shugart and Carey 1992). The Federal Council serves as a collective head of government and state, with seven members elected by the Federal Assembly. Presidential duties rotate among members, and they are not subject to parliamentary confidence. The Council is not subject to Assembly confidence, making origin fused but survival separate.

S2 Cross-National Survey Evidence

In the main text, one of the variables I use to demonstrate the association between presidential institutions and voters' perceptions of parties is the variance in survey respondents' perceptions of parties' ideological positions. In the main text, I use all parties in each survey to calculate this variance. However, respondents are likely to be less familiar with smaller parties and thus have greater uncertainty about their positions. As a result, it is possible that the results reported in the main text are larger than they would be if only major parties were considered. Here, I examine only the two largest parties in each country-year—defined as those with the largest representation in the legislature—and recalculate the variance in perceived party ideology. Table S1 shows the results of these models alongside the original models using all parties. The magnitude of the association between presidentialism and variance in perceived party ideology does go down slightly when using only the two largest parties, but the association remains statistically significant at the $\alpha = 0.05$ level and the effect magnitudes remain relatively large: 0.726 standard deviations in the model without covariates and 0.34 standard deviations when including covariates.

Table S2 replicates the models presented in main text Table 1, but instead uses the alternate scheme of coding institutional designs discussed in seciton S1. Results are broadly consistent across coding schemes. The association between presidentialism and variance in perceived party ideology is positive and statistically significant but declines slightly in magnitude. The association with difference in party affect—a rough measure of affective polarization—remains negative, though it is statistically significant only when controlling for covariates. The association with variance in own-party affect is likewise positive but slightly smaller in the main text. It does not remain statistically significant when controlling for covariates, but neither does the main text model. Finally, the association between presidentialism and the proportion of respondents identifying with

Table S1: Variance in Perceived Party Ideology

	Dependent variable:			
	Variance in Perceived Party Ideology		Variance in Perceived Party Ideology (Top 2)	
	(1)	(2)	(3)	(4)
Presidential	0.881*	0.400*	0.726*	0.340*
	(0.160)	(0.192)	(0.150)	(0.171)
Democracy		−0.641*		−0.628*
		(0.221)		(0.254)
GDP		−0.366*		−0.273
		(0.127)		(0.175)
Gini		0.221*		0.173*
		(0.052)		(0.051)
Eff. Parties		0.110		0.199
		(0.141)		(0.135)
Intercept	−0.223*	0.648*	−0.182*	0.590*
	(0.070)	(0.166)	(0.080)	(0.164)
Observations	214	208	212	206
R^2	0.140	0.419	0.089	0.287
Adjusted R^2	0.136	0.404	0.084	0.270

Note: $^*p < 0.05$. Heteroskedasticity-consistent standard errors in parentheses.

a party actually increases in magnitude under the alternate coding scheme, though it remains statistically significant only in the model without covariates.

S3 Generalized Synthetic Control Models

S3.1 Model Explication and Alternative Estimators

The generalized synthetic control method (Xu 2017) models the level of affective polarization Y_{it} in each country i and year t as a function of a binary indicator for whether that country-year was "treated" by presidential institutions D_{it}:

$$Y_{it} = \delta_{it} D_{it} + X_{it}^{\top} \beta + \lambda_i^{\top} f_t + \varepsilon_{it}. \tag{S1}$$

δ_{it} gives the heterogeneous treatment effect on country i in year t, β provides the effects of country-year covariates X_{it}, f_t is a vector of unobserved time-varying factors, λ_i is a vector of unit-specific factor loadings, and ε_{it} denotes random error. The key feature of this model is the interactive fixed

Table S2: Cross-National Survey Evidence, Alternate Coding Scheme

	Dependent variable:							
	Variance in Perceived Party Ideology		Difference in Party Affect		Variance in Own-Party Affect		Proportion with Party ID	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Presidential	0.796*	0.366*	−0.230	−0.382*	0.651*	0.189	−0.527*	−0.163
	(0.128)	(0.128)	(0.129)	(0.132)	(0.131)	(0.208)	(0.120)	(0.134)
Democracy		−0.675*		−0.311		0.012		−0.009
		(0.204)		(0.208)		(0.215)		(0.149)
GDP		−0.315*		−0.175		−0.697*		0.533*
		(0.129)		(0.145)		(0.188)		(0.125)
Gini		0.243*		0.060		0.132		0.084
		(0.051)		(0.084)		(0.088)		(0.083)
Eff. Parties		0.140		−0.521*		−0.155		−0.418*
		(0.144)		(0.218)		(0.131)		(0.109)
Intercept	−0.393*	0.538*	0.120	0.611*	−0.294*	0.503	0.325*	−0.316
	(0.070)	(0.150)	(0.082)	(0.177)	(0.089)	(0.302)	(0.072)	(0.161)
Observations	214	208	222	215	221	213	226	218
R^2	0.161	0.422	0.015	0.138	0.102	0.316	0.082	0.258
Adjusted R^2	0.157	0.408	0.010	0.118	0.098	0.299	0.078	0.241

Note: $^*p < 0.05$. Heteroskedasticity-consistent standard errors in parentheses.

effects (IFE) term $\lambda_i^\top f_t$, which allows each unit to follow its own trajectory over time by loading differentially on common latent factors.

This is a substantial generalization of the standard two-way fixed effects model, which restricts units to parallel trends conditional on covariates. The IFE estimator relaxes this assumption, but at the expense of assuming that all unobserved confounding is absorbed by the low-dimensional factor structure. That is, it assumes that any unobserved confounders are either time-invariant or vary over time in a way that is captured by the common factors. This is a strong assumption, but it is more flexible than parallel trends and allows for heterogeneous treatment effects across units and over time.

Estimation proceeds in three steps. In the first step, the model is estimated on never-treated observations to recover $\hat{\beta}$, $\hat{f}_t$, and $\hat{\lambda}_i$. The number of latent factors is selected via cross-validation, using mean squared prediction error (MSPE) based on the treated units' pre-treatment periods as the validation criterion. In the second step, factor loadings for the treated unit are estimated by projecting its pre-treatment outcomes onto $\hat{f}_t$. In the third step, $\hat{\beta}$, $\hat{f}_t$, and $\hat{\lambda}_i$ are used to impute counterfactual outcomes for the treated unit in the post-treatment period—the "synthetic control" for the treated unit. The treatment effect for each post-treatment period is the difference between the observed and imputed outcomes, and the average treatment effect on the treated (ATT) averages these period-specific effects.

Unbalanced panels are accommodated naturally within this framework. When some country-year observations are missing, an EM-type algorithm iteratively imputes missing values and re-estimates the factor model until convergence. This is important here, as Figures S2 and S4 show that I do not have complete time series for all units.

The IFE estimator relies on four main assumptions. The first is strict exogeneity; conditional on factor structure and covariates, ε_{it} must be independent of treatment assignment in all periods. This rules out time-varying unobserved confounders that are not captured by the latent factors. Second, the factor structure must be low-rank—the number of latent factors must be small relative to the number of units and time periods. The cross-validation procedure identifies four latent factors—well within the bounds of the 38 countries and 27 years. Third, the treated unit cannot be so different from the controls that its counterfactual cannot be constructed as a weighted combination of control trajectories. The strong pre-treatment fit observed in main text Figure 1 suggests the model accurately tracks Israel's affective polarization levels before treatment, and thus the control units likely provide adequate support for counterfactual imputation. Finally, the model assumes no anticipation; units do not alter their behavior in response to future treatment. As discussed in the main text, I designate 1992 as the first treatment year precisely because parties likely began adjusting their strategies immediately after the reform passed, even though the first election under the new system did not occur until 1994. Other assumptions revolve around ε_{it}: they must weakly serially dependent, cross-sectionally independent, and homoskedastic (Xu 2017).

As noted in the main text, I use a parametric bootstrap with 1,000 iterations to obtain standard errors (Xu 2017). This procedure draws a pseudo-treated unit from the pool of control units, re-estimates the model to obtain simulated treatment effects, and uses the distribution of these simulated effects to construct a variance-covariance matrix for the residuals. Bootstrap errors are then drawn from this estimated covariance structure, preserving the serial correlation present in the original residuals. This approach avoids the downward bias that would result from a standard nonparametric bootstrap when the number of treated units is small.

I test two alternative estimators in addition to the IFE estimator above. The first is the matrix completion (MC) estimator (Athey et al. 2021). Rather than imposing a factor model and estimating factors explicitly, MC treats the matrix of potential outcomes as approximately low-rank and uses nuclear norm regularization to impute the counterfactual outcomes for treated units in post-treatment periods. The regularization strength is selected through cross-validation, again using MSPE as the criterion. Like the IFE estimator, MC is robust to violations of the parallel trends assumption, as it allows for heterogeneous trends driven by latent structure in the outcome.

However, since MC does not explicitly model the factor structure, a parametric bootstrap is not feasible—there are no residuals from which to estimate a covariance matrix and resample error series. Instead, I calculate standard errors using a jackknife procedure, which iteratively leaves out one control unit at a time and re-estimates the model. Unfortunately, with only 38 units, this procedure likely produces overly optimistic standard errors. The jackknife captures variation in the ATT due to which control units compose the donor pool, but it holds the treated unit fixed and only perturbs the controls. With a single treated unit, much of the uncertainty comes from the counterfactual imputation itself, but that uncertainty is not captured by the jackknife.

The second alternative is a generalized difference-in-differences (DID) estimator with two-way fixed effects. This model replaces the interactive fixed effects term $\lambda_i^\top f_t$ with additive unit and time fixed effects α_i and ξ_t:

$$Y_{it} = \delta_{it} D_{it} + X_{it}^\top \beta + \alpha_i + \xi_t + \varepsilon_{it}. \tag{S2}$$

This is a more restrictive model that assumes parallel trends conditional on covariates and two-way fixed effects. Because it does not model unit-specific trajectories, it absorbs less systematic variation than the IFE estimator. As a result, the residuals tend to be larger and standard errors less precise. However, it serves as a useful benchmark: if the IFE and DID estimators yield similar point estimates, this suggests the treatment effect is not an artifact of the more flexible factor structure.

S3.2 Tabular Results

Table S3 presents the full tabular results of the models estimated in the main text, alongside the results of the alternative estimators. The first two columns show the results of the IFE estimator under the main coding scheme and the alternate coding scheme, respectively. The third column shows results from the MC estimator, and the fourth column shows results from the DID estimator. Estimates are consistent across all three estimators, save for a slight dropoff in effect magnitude in the model using the alternate coding scheme. As discussed above, the standard errors in the MC model should be interpreted with caution, but the consistency of results across estimators suggests the robustness of the general finding that Israel's shift to direct election of the executive led to a reduction in affective polarization.

Table S3: Effect of Presidentialism on Affective Polarization across Counterfactual Estimators

	IFE	IFE (Alternate Coding)	Matrix Completion	Diff-in-Diff
Presidential	-0.571*	-0.491*	-0.557*	-0.557*
	(0.227)	(0.238)	(0.083)	(0.273)
Democracy	-0.020	0.079	0.112	0.112
	(0.071)	(0.094)	(0.122)	(0.074)
GDP	0.092	0.399	0.017	0.017
	(0.214)	(0.243)	(0.182)	(0.270)
Gini	-0.171	-0.059	-0.088	-0.088
	(0.089)	(0.097)	(0.086)	(0.094)
Eff. Parties	-0.062	-0.268	-0.001	-0.001
	(0.139)	(0.149)	(0.140)	(0.159)
Total *N*	1026	648	1026	1026
N Units	38	24	38	38
N Years	27	27	27	27

Note: $^*p < 0.05$.

S4 Party Brand and Organization Analyses

S4.1 Tabular Results

Tables S4 and S5 present the full tabular results of the models presented in main text Figure 2. Tables S6 and S7 present the results of the same models using the alternate coding scheme discussed in section S1. All models use parametrically bootstrapped standard errors, using the procedure described in section S3.1. Results are consistent across coding schemes, with all estimates statistically significant and in the expected directions.

Table S4: Effect of Presidentialism on Uncertainty of Party Policy Positions

	Immigration	Economic	Minority	Religious
Presidential	0.172*	0.375*	0.737*	1.490*
	(0.029)	(0.033)	(0.029)	(0.067)
Democracy	-0.027*	-0.042*	-0.087*	-0.096*
	(0.011)	(0.009)	(0.014)	(0.013)
GDP	-0.119*	-0.138*	-0.513*	-0.134*
	(0.017)	(0.018)	(0.022)	(0.022)
Gini	-0.047*	-0.025*	-0.006	-0.012
	(0.006)	(0.006)	(0.006)	(0.008)
Eff. Parties	0.037*	-0.017	0.050*	-0.101*
	(0.015)	(0.011)	(0.015)	(0.020)
Total *N*	999	999	999	999
N Units	37	37	37	37
N Years	27	27	27	27

Note: $^{*}p < 0.05$.

Table S5: Effect of Presidentialism on Party Organization and Behavior

	Attack Oppo.	Local Offices	Local Activists	Personalization
Presidential	-0.606*	-0.676*	-0.353*	0.502*
	(0.028)	(0.047)	(0.033)	(0.034)
Democracy	-0.168*	0.047*	0.040*	0.092*
	(0.016)	(0.015)	(0.016)	(0.015)
GDP	-0.178*	0.393*	0.294*	-0.199*
	(0.021)	(0.026)	(0.023)	(0.025)
Gini	0.114*	-0.137*	-0.183*	0.020*
	(0.007)	(0.009)	(0.009)	(0.007)
Eff. Parties	-0.004	-0.303*	-0.180*	0.033
	(0.017)	(0.026)	(0.027)	(0.020)
Total *N*	999	999	999	999
N Units	37	37	37	37
N Years	27	27	27	27

Note: $^{*}p < 0.05$.

Table S6: Effect of Presidentialism on Uncertainty of Party Policy Positions (Alternate Coding)

	Immigration	Economic	Minority	Religious
Presidential	0.110*	0.327*	0.733*	1.720*
	(0.020)	(0.051)	(0.018)	(0.030)
Democracy	-0.120*	-0.052*	-0.100*	-0.102*
	(0.011)	(0.015)	(0.014)	(0.019)
GDP	-0.008	-0.245*	-0.223*	-0.011
	(0.021)	(0.034)	(0.022)	(0.022)
Gini	-0.010*	-0.034*	-0.040*	-0.140*
	(0.005)	(0.011)	(0.006)	(0.007)
Eff. Parties	0.117*	0.014	0.029	-0.114*
	(0.010)	(0.020)	(0.016)	(0.017)
Total *N*	648	648	648	648
N Units	24	24	24	24
N Years	27	27	27	27

Note: $^{*}p < 0.05$.

Table S7: Effect of Presidentialism on Party Organization and Behavior (Alternate Coding)

	Attack Oppo.	Local Offices	Local Activists	Personalization
Presidential	-0.659*	-0.727*	-0.252*	0.480*
	(0.023)	(0.030)	(0.028)	(0.031)
Democracy	-0.232*	0.093*	0.149*	-0.045*
	(0.016)	(0.015)	(0.021)	(0.022)
GDP	-0.264*	0.248*	0.287*	0.328*
	(0.020)	(0.032)	(0.033)	(0.032)
Gini	0.217*	-0.112*	-0.198*	-0.091*
	(0.007)	(0.008)	(0.010)	(0.011)
Eff. Parties	-0.004	-0.412*	-0.359*	0.061*
	(0.016)	(0.022)	(0.028)	(0.022)
Total *N*	648	648	648	648
N Units	24	24	24	24
N Years	27	27	27	27

Note: $^{*}p < 0.05$.

S4.2 Permutation Tests

Figure S5 presents the results of permutation tests for each of the eight dependent variables in the party brand and organization analyses. In each case, the average treatment effect across all control units is approximately zero. The estimated treatment effect in Israel is almost always among the strongest treatment effects observed, with the only exceptions being on immigration and the presence of local activists. Moreover, comparing across dependent variables reveals that even when a control unit has a large effect on one variable, that effect almost never translates to other variables. Whereas Israel exhibits results that are consistently in the expected direction, the control units do not. Taken together, these results indicate that the effects observed in Israel are not likely to be based on chance. Table S8 presents the *p*-values calculated from these permutation tests. Although many of them do not clear the $\alpha = 0.05$ threshold, I noted in the main text that these *p*-values are likely to be conservative. The fact that some control units lack a sufficient amount of pre-treatment data—as seen in Figure S2—means that the permutation *p*-values are calculated using a smaller number of units than the main analyses.

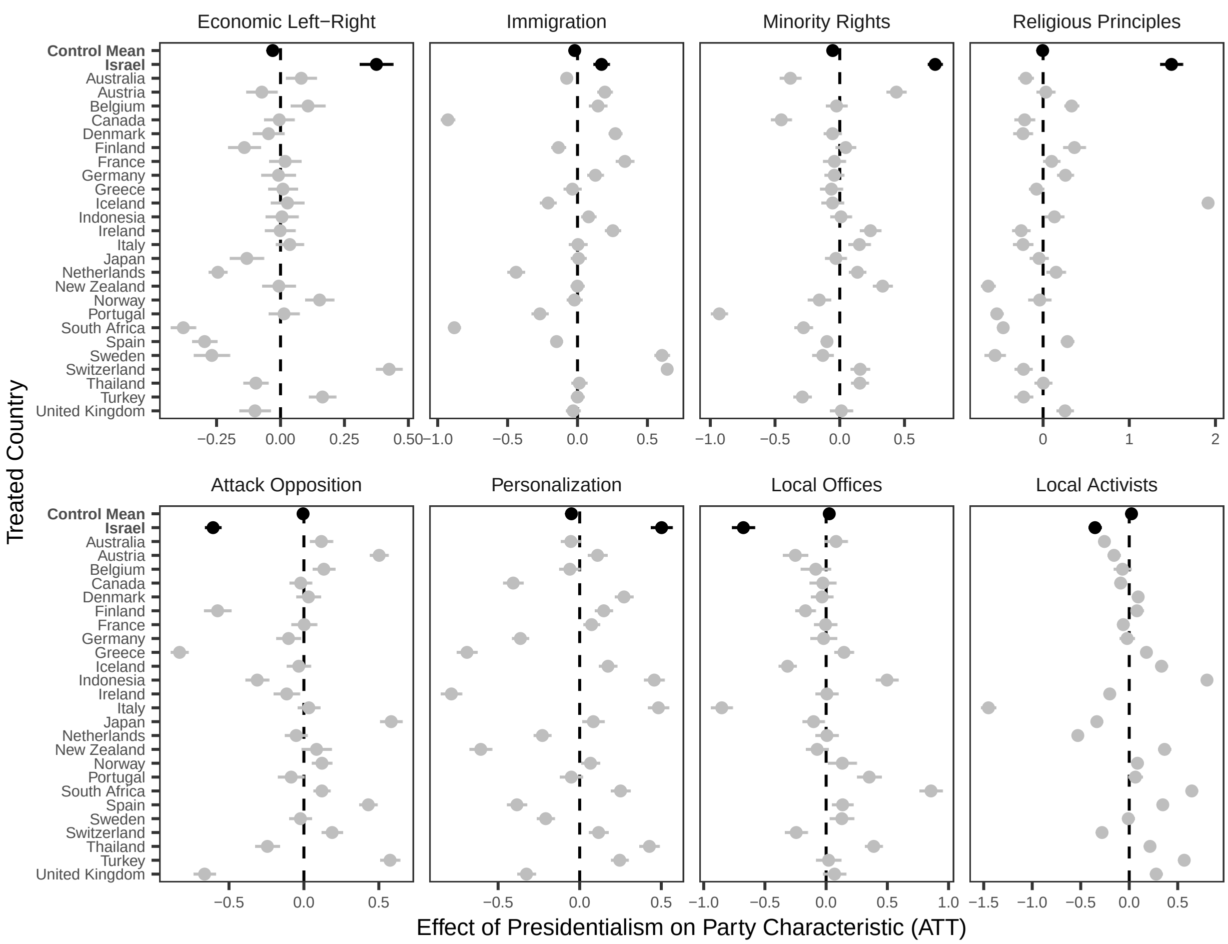

Figure S5: Permutation Test Results for Party Brand and Organization Analyses. Error bars represent 95% confidence intervals.

Table S8: Permutation Test p-Values for Party Brand and Organization Analyses

	One-Sided	Two-Sided
Immigration	0.240	0.440
Economic Left-Right	0.040	0.080
Minority Rights	0.000	0.040
Religious Principles	0.040	0.040
Attack Opposition	0.080	0.080
Local Offices	0.040	0.080
Local Activists	0.080	0.240
Personalization	0.000	0.120

S5 Newspaper Sentiment Analyses

S5.1 Data Procurement

The ProQuest Historical Newspapers database contains digitized versions of all articles published in *The Jerusalem Post*. I restricted all database searches to the time period between January 1, 1975 and December 31, 2001. I identified articles related to political parties, party leaders, and the Knesset through three separate searches. As such, some articles are represented in multiple categories because they mention multiple topics. 67,536 articles appear in only one search, while 28,798 appear in exactly two and 10,629 appear in all three.

To identify articles relevant to political parties, I used the following search parameters: "alignment" OR "likud" OR "labor party" OR "one israel." While this is not an exhaustive list of all parties that existed at any point during the time period under consideration, my aim was to capture the largest parties and coalitions, as those would have generated the most media coverage and held the greatest sway over public opinion. The same reasoning applies to party leaders, which I identified in articles using the following search parameters: "yitzhak rabin" OR "menachem begin" OR "yitzhak shamir" OR "shimon peres" OR "benjamin netanyahu" OR "ehud barak" OR "ariel sharon." Finally, I identified articles related to the Knesset simply using the search term "knesset."

Figure S6 shows the number of articles published in each category by year, broken down by topic. The number of articles related to parties and the Knesset remained relatively constant over time, while the number of articles related to party leaders increases substantially. This comports with the analysis in main text Figure 2, which shows that Israel experienced a rapid increase in party personalization following the institutional reform. The increase in leader-focused articles is likely a reflection of this trend, as media coverage shifted to focus more on individual party leaders rather than parties as organizations.

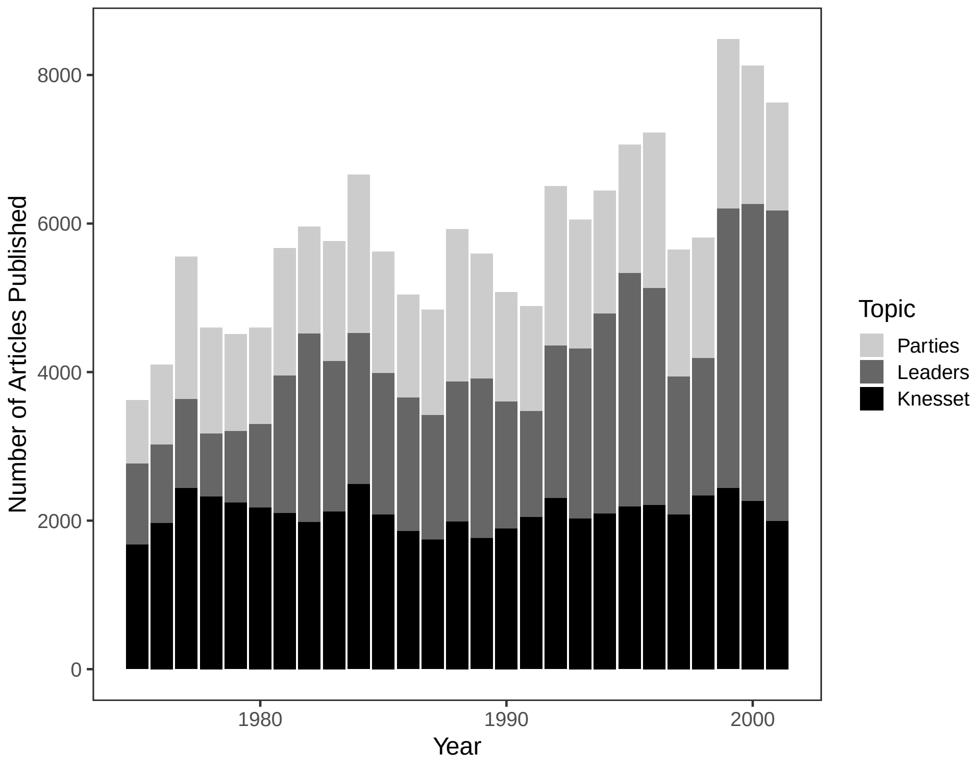

Figure S6: Number of Newspaper Articles Published by Topic

S5.2 Model Explication

To estimate the effect of the institutional reform on newspaper headline sentiment, I use interrupted time series (ITS) models. These models are designed for settings in which an outcome is observed repeatedly over time and a discrete intervention occurs at a known point, partitioning the time series into pre- and post-intervention periods (Anderton and Carter 2001; Bates et al. 2017; Cross and Bølstad 2015). Because I observe headlines at the article level across 27 years, with the institutional reform occurring in 1992, ITS is a natural fit.

The sentiment Y_t of each headline published at time t is modeled as:

$$Y_t = \beta_0 + \beta_1 \mathrm{T}_t + \beta_2 \mathrm{D}_t + \beta_3 \mathrm{P}_t + \varepsilon_t, \tag{S3}$$

where T_t is the number of years elapsed since the beginning of the observation period (January 1, 1975), D_t is a binary indicator equal to 1 during the period of direct executive elections (1992-2001), and P_t is the number of years elapsed since the reform (and is zero in pre-reform periods). β_0 is the intercept, representing the baseline level of sentiment at the start of the observation period. β_1 captures the pre-treatment trend in sentiment—that is, the rate at which sentiment was increasing or decreasing before the reform. β_2 captures the immediate level shift in sentiment at the time of the reform. β_3, the coefficient of primary interest, captures the *sustained* change in the outcome's trajectory following the reform. A statistically significant β_3 indicates that the reform altered the rate at which sentiment changed over time, above and beyond any pre-existing trend.

As discussed in the main text, I focus on β_3 rather than β_2 because the effect of institutional change on media content is unlikely to be instantaneous; rather, party behavior should gradually change over time as parties adapt to new electoral incentives. I report Newey-West standard errors to account for heteroskedasticity and autocorrelation (Newey and West 1987).

The ITS design relies on four main assumptions (e.g. Baicker and Svoronos 2019). First, time trends can be expressed as a linear combination of parameters. Second, the pre-treatment level and trend in sentiment would have been the same regardless of whether the reform had occurred. Third, if the reform had not occurred, the pre-treatment trend would have continued unchanged into the post-treatment period. This parallel trends assumption is often strong and difficult to verify. The biggest threat to the validity of this assumption is perhaps also the threat to the final assumption—that there are no other events coinciding with the reform that could have caused a change in sentiment. I discuss in the main text that the *The Jerusalem Post* changed ownership in 1989, which could have affected the newspaper's editorial stance. However, the shift to the political right that occurred in the years immediately following the change in ownership likely would have pushed the newspaper to evoke harder stances, more polarized sentiment regarding parties and political leaders, and less subjective language—the opposite of what I expect theoretically and find empirically. While the *point estimate* of the reform's effect may still be biased toward zero, the strong and statistically significant results suggest the *direction* of the effect is likely to be robust to this potential confounder.

S5.3 Tabular Results

In the main text, I presented only the results for the parameters of interest in order to keep the tables manageable. Tables S9, S10, and S11 present full results for the ITS models of newspaper headline sentiment regarding parties, party leaders, and the Knesset, respectively. I further test an alternate model to code headline sentiment. The Lexicoder Sentiment Dictionary (LSD) is specifically designed for use on political and news texts (Young and Soroka 2012), though its performance on short texts may not be as strong as VADER's. It also only allows for negative and positive sentiment, which slightly restricts the variables I am able to test. Young and Soroka (2012) report that subtracting negated positive and negative words from purely positive and negative words, respectively, produces a more accurate measure of sentiment, so I adopt that strategy. Results from ITS models using the LSD are presented in Table S12. Results are very similar to the positive and negative sentiment models using VADER, with the reform leading to a significant decreases in negative and positive sentiment following the reform.

Table S9: Interrupted Time Series Models of Newspaper Headline Sentiment (Parties)

	Dependent variable:			
	Subjectivity	Neutral	Negative	Positive
	(1)	(2)	(3)	(4)
Time	0.002	−0.002	−0.001	0.004*
	(0.001)	(0.001)	(0.001)	(0.001)
Reform	−0.011	0.004	0.006	−0.012
	(0.019)	(0.019)	(0.019)	(0.019)
Time Since Reform	−0.014*	0.016*	−0.009*	−0.012*
	(0.003)	(0.003)	(0.003)	(0.003)
Intercept	−0.001	−0.008	0.035*	−0.027*
	(0.013)	(0.013)	(0.013)	(0.013)
Observations	44,169	44,169	44,169	44,169

Note: $^{*}p < 0.05$. Newey-West standard errors in parentheses.

Table S10: Interrupted Time Series Models of Newspaper Headline Sentiment (Leaders)

	Dependent variable:			
	Subjectivity	Neutral	Negative	Positive
	(1)	(2)	(3)	(4)
Time	0.003*	−0.008*	0.007*	0.004*
	(0.001)	(0.001)	(0.001)	(0.001)
Reform	−0.005	0.033	−0.043*	0.002
	(0.018)	(0.017)	(0.017)	(0.018)
Time Since Reform	−0.012*	0.016*	−0.008*	−0.013*
	(0.002)	(0.002)	(0.002)	(0.002)
Intercept	−0.007	0.070*	−0.067*	−0.024
	(0.014)	(0.013)	(0.013)	(0.014)
Observations	56,021	56,021	56,021	56,021

Note: $^{*}p < 0.05$. Newey-West standard errors in parentheses.

Table S11: Interrupted Time Series Models of Newspaper Headline Sentiment (Knesset)

	Dependent variable:			
	Subjectivity	Neutral	Negative	Positive
	(1)	(2)	(3)	(4)
Time	0.0002	−0.005*	0.005*	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Reform	−0.009	0.038*	−0.054*	0.009
	(0.018)	(0.017)	(0.017)	(0.017)
Time Since Reform	−0.007*	0.014*	−0.010*	−0.008*
	(0.003)	(0.003)	(0.003)	(0.003)
Intercept	0.013	0.022*	−0.023*	−0.005
	(0.011)	(0.010)	(0.010)	(0.011)
Observations	56,829	56,829	56,829	56,829

Note: $^{*}p < 0.05$. Newey-West standard errors in parentheses.

Table S12: Interrupted Time Series Models of Newspaper Headline Sentiment (LSD)

	Dependent variable:					
	Parties		Leaders		Knesset	
	Negative	Positive	Negative	Positive	Negative	Positive
	(1)	(2)	(3)	(4)	(5)	(6)
Time	0.001	0.005*	0.009*	0.0001	0.006*	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Reform	0.029	−0.009	−0.027	0.070*	−0.026	0.011
	(0.020)	(0.020)	(0.018)	(0.018)	(0.018)	(0.018)
Time Since Reform	−0.018*	−0.016*	−0.019*	−0.018*	−0.017*	−0.009*
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Intercept	0.009	−0.029*	−0.070*	0.013	−0.035*	−0.014
	(0.013)	(0.013)	(0.013)	(0.014)	(0.011)	(0.011)
Observations	44,169	44,169	56,021	56,021	56,829	56,829

Note: $^{*}p < 0.05$. Newey-West standard errors in parentheses.
Sentiment labeling conducted using Lexicoder Sentiment Dictionary.

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