Party Competition and Cooperation Shape Affective Polarization:

Evidence from Natural and Survey Experiments in Israel

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Abstract

Does electoral competition increase affective polarization? Can cross-party cooperation depolarize voters? Addressing these questions is challenging since both competition and cooperation are endogenous to political attitudes. Building on social identity theory, and leveraging a natural experiment unfolding over seven Israeli election studies, we demonstrate that enhanced electoral competition, measured by interview date proximity to an election, increases affective partisan polarization. We then consider whether party cooperation can depolarize the electorate. To do so, we leveraged the ambiguity around coalition building in Israel's 22nd election and implemented a survey experiment where we credibly shaped respondents' perceptions regarding the likelihood that a unity government will form in the near future. We find that priming party cooperation in the form of a unity government, promotes more tolerance across partisan lines. Our study contributes to the affective polarization literature by causally identifying institutional causes and remedies of polarization in a comparative context.

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1 Introduction

Affective polarization, conceptualized as the gap between in-party affect and out-party dislike, is common in modern democracies (Iyengar et al. 2019; Druckman and Levendusky 2019; Gidron, Adams and Horne 2019). Scholars of American and comparative politics have suggested that political campaigns (Iyengar, Sood and Lelkes 2012; Sood and Iyengar 2016; Sheffer 2019), elite ideological polarization (Rogowski and Sutherland 2016; Andreadis and Stavrakakis 2019), economic inequality (Gidron, Adams and Horne 2018), media consumption (Levendusky 2013; Lelkes, Sood and Iyengar 2017), and majoritarian electoral institutions (Gidron, Adams and Horne 2018), all contribute to affective polarization. Nonetheless, far less attention has been allocated to how polarization may be reduced (Levendusky 2018*a*,*b*). Since polarization has many adverse social consequences, not least of which are economic discrimination and political gridlock (McConnell et al. 2018; Iyengar et al. 2019), it is crucial for political scientists to identify not only causes of, but also remedies for affective polarization.

In this paper we consider both causes and remedies of affective polarization, by focusing on party *competition* and *cooperation*. We conceptualize electoral competition broadly, as the process in which parties employ a host of strategies to achieve opposing goals, namely mobilizing voters to support party A rather than party B. In line with previous advances, we expect the salience of competition to enhance as election day approaches (Eifert, Miguel and Posner 2010; Michelitch 2015; Michelitch and Utych 2018; Singh and Thornton 2019; Sheffer 2019; Dekeyser and Freedman 2018). In contrast, we conceptualize cooperation as a process in which parties negotiate and eventually reach a shared goal. Following others who have identified electoral coalitions as a form of cooperation (Ibenskas 2016), we focus on unity governments, which are perhaps the most pronounced case of cross-party collaboration in multi-party electoral systems.

Drawing on social identity theory (Tajfel 1978), we theorize that electoral competition increases polarization, while cross-party cooperation can depolarize the electorate. Providing evidence regarding institutional causes and remedies of affective polarization in the form of party competition and cooperation is extremely challenging. This is because electoral competition and

cross-party cooperation are likely endogenous to citizens' political attitudes. To overcome this challenge we focus on the Israeli case, and introduce a natural experiment unfolding over seven election cycles (2001-2019), which we supplement with a unique survey experiment implemented following elections to Israel's 22^{nd} parliament, the Knesset.

Specifically, leveraging the random assignment of Jewish Israeli survey respondents to telephone interview-dates over seven rounds of the Israeli National Election Study (INES), we demonstrate that enhanced electoral competition, measured by interview-date proximity to the election date increases general affective partisan polarization.[1] After establishing the effects of electoral competition on affective polarization, we consider whether party cooperation can depolarize the electorate. To do so, we leverage the ambiguity around coalition building following elections for the 22^{nd} Israeli Knesset, and implement a survey experiment where we credibly shape respondents' perceptions regarding the likelihood that a unity government comprised of the two main competing parties will form in the near future. We find that priming partisan cooperation in the form of a unity government, has a modest depolarizing effect, promoting more tolerance towards voters across party lines.

Competition and cooperation are perhaps the most central dynamics of electoral politics. Nonetheless, despite the many expected democratic virtues of electoral competition, it has clear negative externalities in the form of enhanced affective polarization. This is especially the case for modern-day electoral campaigns like the ones we observe, in which politicians employ negative advertisements (Sood and Iyengar 2016), vitriolic rallies (Morrison et al. 2018), and political violence (Wilkinson 2006), in order to gain electoral support. Acknowledging the centrality of competition during electoral cycles, we consider whether cooperation following electoral cycles can depolarize the electorate and contribute to more tolerant political climates. Our results suggest that kinder and gentler interactions between parties (Lijphart 2012), may undue a portion of the harm imposed by electoral competition.

We make three contributions to the existing literature on partisan affective polarization. First,

[1] Our focus on the Israeli Jewish population follows recent advances exploring polarization in Israel (Shamir, Dvir-Gvirtzman and Vantura 2017).

we join recent advances which focuses on remedies for affective polarization (Levendusky 2018*a*,*b*; Carlin and Love 2018). Specifically, we innovate by focusing on cooperative institutional arrangements, demonstrating how they may reduce affective polarization. This novel institutional approach is particularly important given our evidence that political competition polarizes the electorate, and given more general observational findings which link competitive institutional arrangements with affective polarization (Gidron, Adams and Horne 2018).

Second, we follow Gidron, Adams and Horne (2019), and explore affective polarization in a comparative context. Focusing on Israel where political left-right divides have emerged as a central and stable political cleavage since the 1970s (Shamir and Arian 1999; Shamir, Dvir-Gvirtzman and Vantura 2017), we provide new evidence suggesting that both party competition and cooperation shape affective polarization. In doing so, we extend the study of prejudice and intergroup relations in Israel beyond ethnic and religious cleavages (Zeitzoff 2014; Barak-Corren, Feldman and Gidron 2018; Enos and Gidron 2018; Weiss 2019), to include partisan identities.

Third, we make a methodological contribution by introducing research designs previously used by scholars of electoral campaigns (Eifert, Miguel and Posner 2010; Michelitch 2015; Singh and Thornton 2019), and intergroup attitudes (Tankard and Paluck 2017; Barak-Corren, Feldman and Gidron 2018), to the literature on affective polarization. By leveraging survey sampling procedures as a natural experiment, and by exploiting ambiguity during coalition bargaining to increase realism in survey experiments, we provide new evidence regarding the causes and remedies of affective polarization. Though we focus on the Israeli case, scholars of affective polarization can adapt our empirical approach to further test the generalizability of our findings in additional contexts.

2 Competition, Cooperation, and Polarization: A Social Identity Perspective

Partisan affective polarization is most commonly measured by the gap between levels of in-party affect and out-party dislike (Iyengar and Westwood 2015; Druckman and Levendusky 2019). It follows that a pre-condition for polarization is some degree of identification with a political party (or voting bloc in a multi-party systems). Partisan identification is often thought of as a social

identity (Green, Palmquist and Schickler 2004; Iyengar et al. 2019), and empirical accounts from the Unites States and other comparative contexts including Israel suggest that partisanship as a social identity remains rather stable over time (Green, Palmquist and Schickler 2004; Shamir and Arian 1999; Shamir, Dvir-Gvirtzman and Vantura 2017).

A robust literature in social psychology demonstrates that the presence of distinct social groups such as parties, can cause in-group favoritism, bias towards out-groups, and negative stereotypes (Tajfel 1978, 2010; Huddy 2001; Kalin and Sambanis 2018), all of which may facilitate polarization. Nonetheless, one may wonder if and how competition and cooperation between groups shape intergroup attitudes. This question is particularly pertinent for scholars of partisan polarization, since competition and cooperation are central strategic dynamics in electoral politics. Building on social identity theory, we consider how these dynamics may impact affective polarization.

2.1 Competition

Social psychologists have long demonstrated that intergroup competition can fuel negative affect and discrimination towards out-groups (Cuddy, Fiske and Glick 2007). Competition is thought to increase the salience of social identification, further emphasizing an "us" vs. "them" mentality (Cikara, Botvinick and Fiske 2011). Partisanship as a social identity is not an exception to these empirical patterns.

Indeed, Miller and Conover (2015) argue that American voters view elections as a group competition between partisan identities. In line with this perspective, empirical evidence from around the world suggests that electoral competition increases party based in-group favoritism (Michelitch 2015) as well as partisan trust gaps (Carlin and Love 2018). Others show that exposure to political campaigns increases affective polarization (Iyengar, Sood and Lelkes 2012; Sood and Iyengar 2016). Taken together, these findings suggest that electoral competition may enhance polarization, by increasing (decreasing) in-party (out-party) affect. This effect is likely driven by several mechanisms, of which we consider three: information, engagement, and turnout.

First, electoral competition may increase the volume of political information to which voters are exposed. In turn, increases in information can crystallize voters' attitudes towards parties, con-

tributing to polarization. Second, electoral competition may encourage engagement in political activities. In turn, voters who attend community meetings or participate in political conversations may become more polarized, especially when partisan sorting is prevalent. Third, increased competition may enhance the likelihood that voters intend to turnout and vote. Since voting enhances partisan identification (Dinas 2014; Michelitch and Utych 2018), it is possible that competition increases polarization through a mechanism of enhanced political participation.

2.2 Cooperation

Cooperation, often thought of as the opposite of competition, has been extensively studied by scholars of intergroup relations (Allport 1954; Gaertner et al. 1999). Specifically, theoretical frameworks of intergroup contact identify cooperation as a necessary condition for prejudice reduction (Allport 1954). In addition, recent empirical analyses demonstrate that cooperation across identity groups can improve intergroup relations (Lowe 2018; Mousa 2019).

Several mechanisms may account for the link between cooperation and reduced polarization. First, cooperation creates shared goals and reward structures between competing groups. This leads to a shared fate, which may serve to bridge gaps between previously distant groups (Gaertner et al. 1999). Second, cooperation may reduce polarization through the re-categorization of social identities, and the emergence of a superordinate social category (Gaertner et al. 1999; Brewer 2000). Thus, depolarization may be driven by increased affect towards previously considered outgroups who now share an overarching identity.

A substantial portion of the literature on cooperation focuses on personal interactions. However, cross-party cooperation often occurs as an elite process, absent direct engagement between supporters of different parties. For example, congresswomen from both sides of the aisle cosponsor specific bills, and parliament members from competing parties form broad coalitions. Therefore, one may wonder if and how elite-level cooperation may affect mass-partisan polarization.

Social identity scholars suggest that elite interactions in the form of negotiations led by group leaders affect the attitudes of their group members. A necessary condition for attitudinal change in this case, is that leaders are perceived to embody the relevant group identity (Hogg 2001; Van Knip-

penberg 2011). Applied to an electoral context, cooperation (and competition) between politicians may therefore impact citizens' attitudes, especially when citizens perceive politicians as representative of their salient social category.

Building on social psychological frameworks regarding the depolarizing effects of superordinate identities (Gaertner et al. 1999; Brewer 2000), as well as the central role of elite cues in electoral politics (Zaller et al. 1992), we expect cooperation across party lines to decrease affective polarization. Perhaps the most significant and pronounced form of cross-party cooperation in multi-party electoral systems is the formation of broad unity governments, which include parties from different sides of the political and ideological spectrum. We therefore expect cooperation in the form of unity governments which create superordinate political identities and shared reward structures to attenuate the salience of party identification, and reduce affective polarization.

3 Empirical Context

To empirically evaluate our theoretical expectations we turn to the Israeli case, and focus on elections to the Knesset since 2001. Elections in Israel are held every four years,[2] and follow a nationwide proportional representation system. Following elections, the Israeli president consults with all elected party leaders and grants one member of Knesset (MK) the authority to form a coalition. The president selects the MK who is most able to form a coalition and serve as Israel's next prime minister.[3] This MK is then given 42 days to form a government. During this time frame, coalition talks are initiated in order to reach agreements with several parties (Rahat and Hazan 2005). Since the early 2000s, the number of party-lists within Israeli coalitions ranged from 5-8, varying in their ideological and partisan composition.

The right-left cleavage amongst the majority Jewish population, is central to current Israeli politics (Plesner et al. 2018). Historically, the multi-party system in Israel has been characterized by multiple salient cleavages, including intra-Jewish ethnic and religious divides. Although these cleavages play a role in Israeli politics to this day, they have become subordinate to the right-left

[2]Unless the parliament votes to conduct early elections.

[3]Typically the leader of the largest party.

collective identity cleavage which emerged in the 1970s (Shamir, Dvir-Gvirtzman and Vantura 2017). Since then, "right" and "left" partisan identities have become central in Israeli political discourse, and voters have begun to identify more commonly in terms of political blocs rather than individual parties (Arian and Shamir 1983).

In recent years, animosity between right and left-wing voters in Israel has received growing attention. 36% of participants in a recent survey conducted by the Israeli Democratic Institute have identified right-left tensions as the strongest in Israeli society, while only 28% of respondents reported that tensions between Jews and Arabs were most severe (Plesner et al. 2018). Despite these alarming statistics, little work has been done to systematically evaluate affective polarization in Israel. Tsfati and Nir (2017) focus on the Israeli case to investigate the mechanisms linking selective media exposure with polarization. Concurrently, Shamir, Dvir-Gvirtzman and Vantura (2017) analyze survey data in a longitudinal investigation of polarization trends. They find that affective polarization is relatively stable across ten election cycles (1988 - 2015). However, it remains unclear whether electoral competition itself intensifies these existing tensions. This question has become increasingly relevant in 2019, as Israel is facing a record third election after two highly competitive cycles, which eventually created a year-long political deadlock.

The centrality of partisan tensions in Israel raises an equally intriguing question, namely: what may reduce affective polarization? On September 25^{th} 2019, following a divisive election cycle for Israel's 22^{nd} Knesset, Prime Minister Benjamin Netanyahu called for the formation of a unity government, in order to "...promote reconciliation and unify the people of Israel" (Ynet 2019). Unity governments are based on broad coalitions which include parties from both right- and left-wing blocs, and are usually led by the two largest (central) parties.

Much like other multi-party electoral systems, Israel has had several unity governments over the years, most notably in times of war or as a result of a political deadlock. Arguably, such broad coalitions are the most pronounced form of cross-party cooperation, and as our theoretical framework suggests, they may minimize affective polarization. In the following sections we set out to experimentally test the effects of party competition and cooperation on affective polarization.

4 Study I: Electoral Competition Increases Polarization

Existing theory and evidence suggest that electoral competition increases polarization. Nonetheless, causally identifying this relationship is extremely challenging. This is since polarization may be a cause or an effect of political competition. To overcome this challenge, recent scholarship linking competition with polarization has leveraged spatiotemporal variation in battle-ground US presidential campaigns, as well as mismatches between State and designated media market areas (DMAs), as identification strategies. Doing so, scholars demonstrate that exposure to political campaigns increases polarization (Sood and Iyengar 2016; Iyengar, Sood and Lelkes 2012).

Building on recent advances in the partisanship and ethnic identity literatures (Eifert, Miguel and Posner 2010; Michelitch and Utych 2018; Singh and Thornton 2019), we take a different empirical approach leveraging the random assignment of respondents to interview dates in the INES. This allows us to identify a more general effect of electoral competition, measured through proximity to election dates. Under the reasonable assumption that electoral competition increases as election day approaches, we consider respondents surveyed closer to election dates as exposed to higher levels of political competition.

Our empirical strategy differs from previous advances in the polarization literature which focus on party campaigns – a central and important feature of political competition (Sood and Iyengar 2016; Iyengar, Sood and Lelkes 2012). In contrary, we focus on competition as a broader political construct which becomes more intense closer to election dates. Therefore, we consider competition to include campaigns, as well as other elements such as televised debates, increased coverage of politics in the media, rallies, and enhanced presence of political elites on social media.

4.1 Identification Strategy

The INES collects public opinion data from representative samples of Israeli voters prior to national elections. Specifically, since 2001 the INES began to implement telephone interviews, randomly selecting survey respondents from the Ministry of Interior's listing of the population, which is linked to phone number records. Random selection has been implemented by the INES

to guarantee representative samples, and the sampling of respondents is spread over several weeks preceding each election.[4]

We leverage the INES sampling procedure since 2001 as a natural experiment, in which respondents are randomly assigned to interview dates at different time-periods before elections. Building on a commonly adapted empirical strategy (Eifert, Miguel and Posner 2010; Michelitch 2015; Michelitch and Utych 2018; Singh and Thornton 2019; Dekeyser and Freedman 2018), we consider proximity to an election day as a measure of electoral competition. This is reasonable given the understanding that electoral competition increases as elections approach (Michelitch 2015; Michelitch and Utych 2018).

As demonstrated in Figure A1 of our appendix, survey interview dates range from 1-44 days prior to election dates.[5] Since in Israel electoral campaigns last approximately three months, we analyze survey responses collected in the last and most competitive lag of the election season. Therefore, one may consider the treatment effects we estimate as local, since all respondents are exposed to some degree of electoral competition. Nonetheless, respondents surveyed closer to the election, are exposed to higher levels of electoral competition.

As survey respondents are selected randomly over each INES sampling period, we assume that proximity to elections is orthogonal to respondents' characteristics. To empirically test the observable implications of this assumption, and further enhance the credibility of our identification strategy, we present a comprehensive balance test in Figure 1 as well as in Table A5 of the appendix. Figure 1 reports the bivariate as well as multi-variate correlation of age, gender, household spending, religiosity, education, and ethnicity with treatment assignment.[6] To account for the fact that treatment is assigned every election-year, we include election-year fixed effects in all

[4]In some election studies, respondents are interviewed pre- and post-election. However, in this study we focus on pre-election responses.

[5]Since elections in Israel are implemented on Tuesdays, and the INES does not operate on the Sabbath, we should not observe interviews from 3, 10, 17, 24, 31, and 38 days prior to the election. This is evident from Figure A1. However, in the 2006 (2009) election study, one interview date is listed on day 10 (17) for an unclear reason. Results are robust to omitting these two observations.

[6]We discuss the construction of covariates from multi-year surveys in Section A.1 of the appendix.

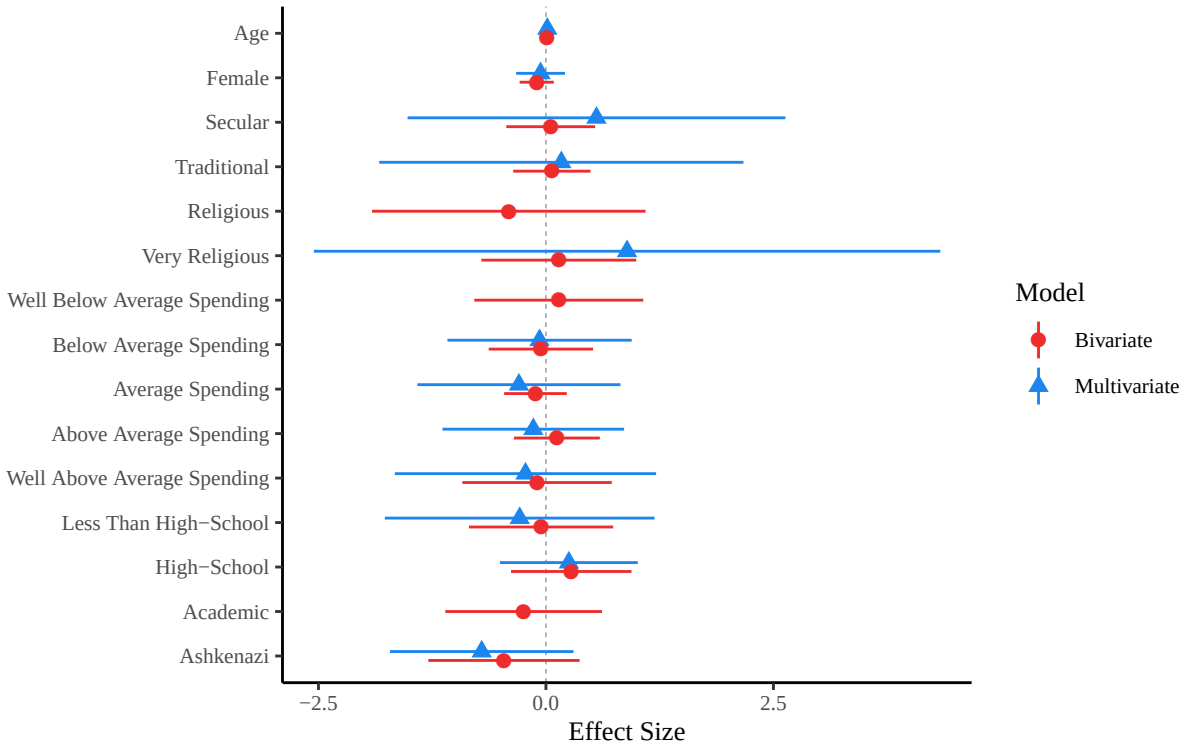

Figure 1: **Covariate Balance** - Point estimates and their corresponding 95 percent confidence intervals from OLS models with election year fixed effects, where errors are clustered by year. Coefficients in red are extracted from bivariate models identifying the association between our treatment and a given covariate. Coefficients in blue are extracted from multi-variate models identifying the association between similar variables in one model. Religious, Academic, and Well Below Average Spending are reference categories in the multi-variate regression.

models, and cluster errors by year. The results presented graphically in Figure 1 further enhance the credibility of our identification strategy, as covariates are well balanced over our treatment. The F-Statistic of our multivariate model is 2.06 ($p = 0.21$).[7] We present similar models in Table format, in Section A.2 of the appendix.

4.2 Outcome

Our main outcome of interest is affective partisan polarization between members of the right and left-wing voting blocs in Israel. We therefore focus on differences in respondents' mean affect to-

[7] In Figure A2 of the appendix we plot the distribution of respondents over days of the week. Generally the majority of telephone interviews we analyze have been implemented between Sunday and Wednesday. Though only of secondary concern for our identification strategy, we further demonstrate balance over days of the week in Table A6 of the appendix.

wards parties in the opposing voting bloc, subtracted from mean affect towards parties within one's voting bloc (See Equation 1). Higher values of this variable denote higher levels of polarization.

$$Polarization = \frac{\sum_j^n IN_j}{n} - \frac{\sum_j^n OUT_j}{n} \tag{1}$$

Calculating Equation 1 requires us to identify right- and left-wing voters, as well as their affect towards parties in each voting bloc. Therefore, to identify voters' belonging to a voting bloc, we follow Shamir, Dvir-Gvirtzman and Vantura (2017), and leverage self-reported ideology indices to classify respondents as right (left) wing voters, omitting all center respondents.[8] We further utilize party feeling thermometers to construct our measure of polarization. A list of parties divided by cycles and voting blocs is presented in Section A.1 of the appendix (Table A1), where we further elaborate on the INES surveys and the process of constructing all variables for our analyses.

It is important to stress two points regarding our outcome. First, the polarization index we employ captures differences in average affect towards multiple parties associated with left (right) wing voting blocs. We focus on voting blocs rather than specific parties, in order to capture general polarization along the left-right cleavage. Our approach is different from previous explorations of polarization in Israel which focus separately on affect towards leading (e.g Likud and Labor), and smaller (e.g. Mafdal and Meretz) parties (Shamir, Dvir-Gvirtzman and Vantura 2017). Regardless, we demonstrate that our results are robust to other measures of polarization employed in recent analyses of the Israeli electorate.

Second, in order to construct our measure of polarization we employ a commonly used ideological scale through which we classify voters as members of right (left) wing blocs. Doing so could raise concerns regarding post-treatment bias if responses to ideological scales are affected by proximity to elections. Nonetheless, like in the American case in which partisan loyalties are rather stable, recent empirical evidence suggests that Israeli voters' commitment to specific po-

[8]Most studies include a 7 item scale, however the 2006 INES data include an 11 point scale ideology measure, and in 2009 respondents were randomly assigned to either 5, 7, or 11 item scale. In all cases, respondents right (left) of center are considered right-wing (left-wing).

litical camps has been rather stable since the 1990s (Shamir, Dvir-Gvirtzman and Vantura 2017). Therefore it is unlikely that left (right) wing support will shift during the 44 days prior to elections, in response to our treatment. We present an empirical investigation of this expectation in Table A4 of the appendix. The results of this investigation further strengthen our theoretically motivated intuition.

4.3 Estimation Strategy

The random assignment of INES respondents to dates over seven election studies allows for a rather simple estimation strategy. Our preferred specification is the OLS model presented in Equation 2, where we employ election-cycle fixed-effects (γ), and cluster errors by election cycles, in order to identify the effects of electoral competition (β) on affective polarization (y_{ic}) for respondent i in cycle c.[9]

$$y_{ic} = \beta X_i + \gamma_c + \varepsilon_{ic} \tag{2}$$

Our main parameter of interest β identifies the effects electoral competition, as a function of temporal proximity to an upcoming election. Higher values of X indicate greater temporal proximity to an election (i.e. interview date later in the election cycle). Therefore, we expect β to be positive and statistically significant, indicating a positive effect of electoral competition on affective polarization.

4.4 Results

In Table 1 we report our main results, identifying the effects of electoral competition on affective polarization. Competition measured by proximity to an upcoming election has a positive and statistically significant effect on polarization. More specifically, according to our preferred specification (Column 1 in Table 1), exposure to an additional month of electoral competition accounts for over

[9] In doing so we follow others who use similar designs (Eifert, Miguel and Posner 2010; Michelitch and Utych 2018). In Section B.2.1 of the appendix we demonstrate that our results remain subantively similar when clustering by election-week, when clustering at the respondent level, or when using wild cluster bootstrapping.

a tenth of a standard deviation increase in polarization. Our results remain robust to the inclusion of demographic (age, gender, origin), as well as social controls (religiosity, education).[10]

Table 1: Effect of Proximity to Election on Polarization and Party Affect

	Polarization			In-Party Affect	Out-Party Affect
	(1)	(2)	(3)	(4)	(5)
Proximity to Election	0.012	0.011	0.011	0.011	−0.001
	(0.002)	(0.002)	(0.003)	(0.005)	(0.004)
Demographic Controls	No	Yes	Yes	No	No
Social Controls	No	No	Yes	No	No
Year FEs	Yes	Yes	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes	Yes	Yes
N	4,690	4,579	4,363	4,800	4,707

We further evaluate whether polarization increases in light of in-party affect, or out-party dislike, in the shadow of electoral competition. Columns 4-5 in Table 1 suggest that increases in polarization are mainly a consequence of increases in in-party affect as elections approach. The coefficient sign for out-party affect is negative but insignificant.

4.5 Potential Mechanisms

Our empirical strategy allows us to identify the general effects of electoral competition, broadly defined. Therefore, in this section we attempt to de-bundle our identified effects, and provide suggestive evidence for three different mechanisms relating to information, engagement, and turnout. Causally identifying mechanisms is a notoriously challenging endeavor (Imai et al. 2011), and our empirical design is suited to provide evidence regarding a general effect, rather than particular mechanisms. Thus readers should consider these additional analyses as exploratory.

With this caveat in mind, we first consider whether our identified effects are driven by increased levels of political information amongst survey respondents interviewed closer to election dates. As noted in section 2.1, increased information regarding the political system may crystallize voters'

[10]Note that questions about monthly household spending were not included in the 2013 INES, and therefore we do not employ spending as a covariate in our main results.

attitudes towards parties, leading to enhanced polarization. We test the plausibility of this mechanism by leveraging a recurring factual question from the INES (2006-2019) regarding the precise level of the electoral threshold.[11] We transform this survey item into a binary variable taking the value of 1 for voters correctly recalling the electoral threshold. In columns 1-2 of Table 2, we regress this political information binary variable, over our main treatment. Doing so, we do not find evidence that competition increases voters' information regarding the political system, raising doubt that competition increases polarization through a mechanism of political information.

Second, we consider whether electoral competition increases polarization through a mechanism of enhanced engagement in political activities. This mechanism suggests that in light of electoral competition, voters may attend political events such as community meetings, or participate in political conversations, and become more polarized. To evaluate this expectation, we leveraged a recurring question from the INES (2001-2015) regarding the frequency in which respondents engage in political conversations with friends and family (1-4 scale, where higher values indicate increased engagement). We regress our engagement indicator over our main treatment in columns 3-4 of Table 2. Our insignificant results, suggest that enhanced polarization is unlikely driven by increased political engagement.

Lastly, we consider whether our identified effects are driven by a political participation (i.e. turnout) mechanism. It is commonly established that voting enhances partisan identification (Dinas 2014), and therefore electoral competition may shape polarization by increasing voters' intention to participate in an upcoming election. To explore the plausibility of this mechanism, we leverage a recurring question from the INES (2009-2015) regarding the extent to which a survey respondent expects to vote in an upcoming election (1-4 scale, where higher values denote increased likelihood of participation). We regress our expected turnout variable over our main treatment in columns 5-6 of Table 2, and demonstrate that respondents interviewed closer to election dates report higher

[11] This question identifies respondents knowledge regarding the general political system rather than specific party platform. However, we selected this measure as a best approximation of political knowledge, which is common across multiple INES waves. Between 2006 and 2019 only 37% of survey respondents answered this question correctly.

levels of intended political participation. Therefore, it is possible that polarization is driven, at least in part, by respondents' increased intention to participate in politics.

Table 2: Potential Mechanisms

	Information		Engagment		Turnout	
	(1)	(2)	(3)	(4)	(5)	(6)
Proximity to Election	0.0019	0.0019	−0.0019	−0.0037	0.0031	0.0031
	(0.0013)	(0.0013)	(0.0027)	(0.0026)	(0.0008)	(0.0004)
Demographic Controls	No	Yes	No	Yes	No	Yes
Social Controls	No	Yes	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes	Yes	Yes	Yes
Cycles	2006-19	2006-19	2001-15	2001-15	2009-15	2009-15
N	6,332	5,599	5,973	5,565	3,735	3,266

4.6 Robustness Checks

In addition to the above examinations of our treatment, we subject our main results to a number of robustness checks. First, we model our treatment as a discrete factor variable, rather than a continuous variable in Figure 2. To do so, we divided our treatment (proximity to elections) into four categories.[12] Doing so, we demonstrate that our effects are substantively similar, and likely driven by survey respondents interviewed within 25 days prior to an election.

Second, following Singh and Thornton (2019), we demonstrate that our results hold when considering a logged measure of our treatment - days before election (See Table A7 in the appendix). Third, we demonstrate that our results remain substantively similar when focusing on strong partisans.[13] By filtering our data, and focusing on strong right (left) bloc members, we reduce our sample size by more than a third. Nonetheless, results presented in Table A7 in the appendix remain robust.

[12]Categories include: (i) 1-15 days before elections, (ii) 16-25 days before elections, (iii) 26-35 days before elections, and (iv) 36-44 days before elections. The latter category is the reference category in Figure 2.

[13]In other words, we focus on voters reporting 1-2 or 6-7 on a seven point ideology scale.

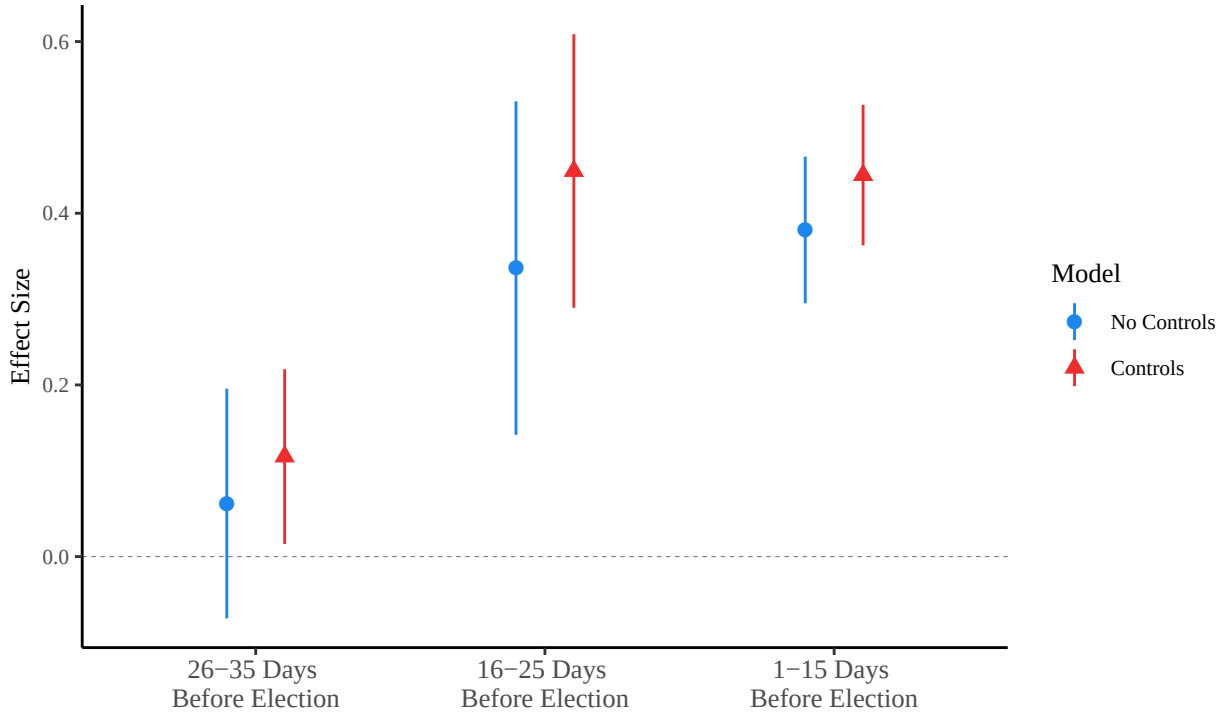

Figure 2: **Average Treatment Effects from OLS models with year fixed effects** - In both models with and without controls, affective polarization is regressed over four dummy variables representing interview dates in four periods prior to an election. Standard errors are clustered by year, and the reference category in both models is a dummy variable for respondents interviewed more than 35 days prior to an election.

Fourth, we consider whether any one election study is driving our identified effect. To do so, we re-run our preferred specification, omitting one election study at a time. Results presented in Table A8 of the appendix demonstrate that our findings are robust to the exclusion of any one election study.

Fifth, we present additional models in which we control for, and consider heterogenous treatment effects of specific week days. Our results presented in Table A9 of the appendix suggest that days of the week do not confound or moderate our main identified effects. Lastly, in Tables A11-A13 of the appendix, we consider alternative clustering procedures of our standard errors, demonstrating robustness to alternative specifications.

5 Study II: Cross-Party Cooperation Reduces Polarization

In our second study, we set out to explore a potential remedy for affective polarization – cross-party cooperation in the form of a unity government. Identifying the effects of cross-party cooperation on polarization is extremely challenging, since depolarization may be a cause or effect of cooperation. In other words, it is unclear whether politicians are more likely to cooperate and form unity governments in unpolarized societies, or alternatively, societies depolarize in light of cross-party cooperation.

5.1 Research Design

To overcome this hurdle, we adapt a novel experimental design first implemented by Tankard and Paluck (2017), who leveraged the American public's uncertainty around a court ruling relating to gay marriage, in order to credibly shape survey respondents' perceptions. In doing so, they identified the effects of court rulings on citizens' attitudes towards gay marriage. Similarly, we leverage the ambiguity around coalition formation in Israel following elections for the 22^{nd} Knesset, to shape respondents perceptions regarding the likelihood of a unity government. Doing so, we identify the effects of perceptions regarding party cooperation in the form of unity governments on affective polarization.

As mentioned above, the result of elections for the 22^{nd} Knesset created a political deadlock,

eventually forcing Israel into a third election cycle. At the time in which we implemented our study, the media reported various potential coalition compositions, ranging from narrow right (left) wing governments to broad unity governments (Levinson 2019; Press 2019). The extreme ambiguity around the political and institutional future of the Israeli parliament, provided us with a unique opportunity to credibly shape respondents' perceptions regarding the likelihood that a unity government would form following the elections for the 22^{nd} Knesset.

We fielded our experiment in October 2019, during the tail end of the first round of coalition talks led by Netanyahu. During this period, uncertainty around the political and institutional future of Israel was very strong. Though many politicians advocated for the formation of a unity government, disagreements and political conflict rendered both minority governments and a third election as possible future alternatives.

We recruited 1,524 respondents using iPanel, Israel's largest opt-in online survey company.[14] Implementing quota sampling we aimed to match our sample to the Jewish population of internet users in Israel based on gender, age, ethnicity, residential area, and religiosity. Descriptive statistics, and a comparison of our sample to the Israeli public are reported Section B.1 of the appendix.

5.1.1 Survey Instrument and Experimental Vignette

We programmed our survey in Hebrew using Qualtrics. Following a battery of demographic questions, respondents were invited to read a brief vignette about the political situation in Israel. Treated respondents read information suggesting that a *unity government* will form in the near future, whereas participants in the control condition were told that a *narrow government* is expected to form. We further randomized whether the government will be led by Benjamin Netanyahu or his opponent Benny Gantz in order to minimize concerns of "information leakage" (Tomz and Weeks 2013; Dafoe, Zhang and Caughey 2018).[15] This resulted in a fully crossed 2x2 experimental de-

[14] 1,110 participants completed all responses to our outcome measures. Table A17 in the appendix suggests that attrition was not driven by our treatments.

[15] One concern we had was that respondents in the 'narrow government' condition would be more inclined to believe that it is a right-wing government since Netanyahu held the mandate to form a coalition.

sign. Below is an English translation of our experimental vignette:

> In the last few days, significant progress has been made in the Israeli political arena. In light of the efforts made by many members of Knesset, senior political commentators estimate that a **[narrow government / broad national unity government]**, led by **[Benjamin Netanyahu / Benny Gantz]**, will form in the near future. "Many things may affect the political reality in Israel, but based on an in-depth analysis, I believe that a **[narrow government / broad national unity government]** will form soon," said a senior political commentator.

Following the vignette, respondents were presented with several questions which we employ as outcomes. First, participants were asked to report their feelings on a conventional 1-100 thermometer scale towards: left-wing voters, right-wing voters, ultra-orthodox Jews, Arab citizens of Israel, and voters of each political party.[16] Second, respondents were asked to position each of these groups along a common seven-item social distance scale, which measures preferences for social exclusion (Enos and Gidron 2018). Possible responses ranged from absolute exclusion to inclusion as a family member. We also presented participants with questions regarding nationalism and party ideology.[17] Finally, as a manipulation check, respondents were asked to report the likelihood that a unity government will form in the near future according to their personal evaluation (on a scale of 1 to 5, where 5 is most likely). Our survey instrument was posted online together with our pre-registration materials,[18] and an English description of our instrument is presented in Section A.1 of the appendix.

5.1.2 Measuring Outcomes

In order to measure affective polarization we follow a similar approach to the one formerly outlined in section 4.2, subtracting respondents' reported affect towards out-groups from their affect towards in-groups. To do so, we identify voters as left- or right-wing supporters based on their

[16] Only the nine parties that have met or exceeded the electoral threshold were included.
[17] See a discussion of these measures in the appendix.
[18] See pre-registration materials posted on EGAP: http://egap.org/registration/6161.

response to a standard pre-treatment seven-point ideology scale. In our main analyses, we omit centrist respondents who report an ideology score of 4. After classifying voters as left (right) wing supporters, we use questions on affect and social distance towards left and right wing voters, in order to construct measures of affective polarization. We also construct separate measures of general affect towards in- and out-group voters.

It is important to note that there are several differences between the outcome measures used in our experiment, and our former study of INES data. First, by designing our own survey we are able to explore two different measures of affective polarization based on social distance scales and feeling thermometers. This allows us to consider and compare different dimensions of polarization which relate to general affective dispositions, and symbolic attitudes of social exclusion (Enos and Gidron 2018; Druckman and Levendusky 2019).

Second, our main experimental measures consider affect towards *right-* and *left-wing supporters*, rather than towards specific parties which we have classified as right- or left-wing parties in study I. Therefore, the outcomes we use for our experiment are theoretically preferable, as they allow us to directly measure the cleavage we are interested in (i.e. the right-left cleavage). Specifically, we no longer need to assume how individual survey respondents classify different political parties along the right-left cleavage.[19]

Lastly, in our experiment we directly ask respondents to report their affect towards *voters*. This approach slightly differs from the INES surveys which ask about affect towards parties in a more general fashion.[20] Recent evidence from the U.S. suggests that affect towards parties is correlated with affect towards supporters of those parties (Druckman and Levendusky 2019). Nonetheless, since our experiment tests an intervention aimed to improve intergroup relations between left- and right-wing voters, we decided to use voter specific questions, which are theoretically more relevant to our case.

[19]Note that in study I, we were forced to make these assumptions since only the 2019 INES survey includes questions regarding affect to left (right) wing supporters.

[20]For exact wording see Section A.1 of the appendix.

5.1.3 Estimation Strategy

To identify our treatment effects, we estimate three pre-registered OLS models. First, we estimate a basic OLS model predicting affective polarization, as a function of our unity government treatment assignment. In addition, to enhance the precision of our estimates, we employ additional models, controlling for our second treatment arm (expected prime-minister), as well as several pre-treatment demographic controls (sex, age, ethnicity, religiosity, locality, and self-reported vote choice).

5.2 Results

Figure 3 reports results from our manipulation check, which suggests that respondents in the treatment (unity government) condition reported higher levels of certainty regarding the likely formation of a unity government. Generally, the average respondent was relatively convinced that a unity government would eventually form ($\mu = 3.2$, on a 1-5 scale). However, our difference in means test suggests that exposure to the treatment condition increased respondents' expectation that a unity government will form in the near future by approximately 15% of a standard deviation.

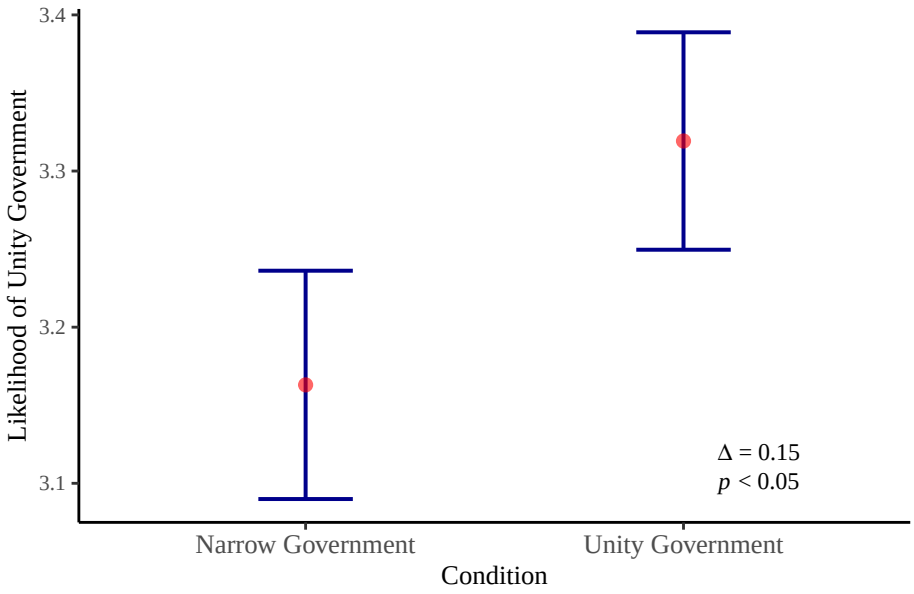

Figure 3: **Manipulation Check** - Treatment effect on perceptions of the likelihood that a unity government will form in the near future.

Results from our main analysis are depicted in Table 3, in which we identify the effects of our unity government treatment on polarization, in terms of social distance. We find that information about cross-party cooperation in the form of a unity government reduces affective polarization. Specifically, information about the expected unity government accounts for over a tenth of a standard deviation decrease in affective polarization ($p = 0.05$). Our results remain robust when we control for the second treatment arm informing respondents of the prime-minister who was reported to lead the government ($p < 0.06$). However, when we include pre-registered demographic controls to improve the precision of our estimates, results only approach conventional levels of statistical significance ($p < 0.1$).

We further evaluate whether decreases in social distance polarization are driven by an increase in out-party affect, or a decrease of in-party affect. Columns 4-5 in Table 3 suggest that the decrease in affective polarization identified above is driven for the most part by increases in out-party affect. The coefficient sign of our treatment effect on out-party affect is positive and statistically significant, suggesting that individuals are more likely to report favorable attitudes towards out-party voters when receiving information about the likely formation of a unity government. The coefficient sign for the effect on in-party affect is positive, but statistically insignificant.

Table 3: Effects of Unity Government on Polarization and Party Affect (Social Distance)

	Polarization			In-Party Affect	Out-Party Affect
	(1)	(2)	(3)	(4)	(5)
Unity	−0.243	−0.238	−0.197	0.045	0.288
	(0.124)	(0.124)	(0.118)	(0.071)	(0.128)
PM Control	No	Yes	Yes	No	No
Demographic Controls	No	No	Yes	No	No
Center Voters	No	No	No	No	No
N	1,110	1,110	1,110	1,110	1,110

In Table A16 of the appendix, we report results from similar analyses, employing alternative outcome variables measured in terms of feeling thermometers. We similarly find negative coefficient signs for our average treatment effect. However, our results are not statistically significant at

conventional levels. As further discussed in Section B.2 of the appendix, although both outcomes are highly correlated, we suspect differences in results to be driven at least in part by the diverging properties of feeling thermometers and social distance scales. Since our thermometer questions were more general and subject to personal interpretation, we worry that they are more prone to measurement error.

5.3 Robustness Checks

Finally, we subject our results to several robustness checks, presented in Figures A5 and A6 in the appendix. Specifically, we present six alternative measurement strategies for our main outcome of interest – affective polarization. In doing so, we consider several categorization schemes of voters into voting blocs based on ideology or self-reported vote choice, as well as multiple measurements of out party affect based on general attitudes towards out-groups or specific attitudes towards supporters of a given party. A detailed discussion of these measures is presented in Section B.2.1 of the appendix.

As evident in Figure A5 of the appendix, the coefficient signs for our average treatment effect remain negative across all measurement specifications. However, three of our alternative models yield statistically insignificant results. As we further discuss in Section B.2.1 of the appendix, we suspect that these weak additional results relate to decreased statistical power in models employing measures that focus on a subset of voters, or to variation across subjects in categorization of parties along the right-left cleavage, in measures that rely on party specific affect.

6 Conclusion

In this paper we draw on social identity theory and hypothesize that electoral competition intensifies affective polarization, whereas cross-party cooperation alleviates polarization. Testing these propositions is extremely challenging as party competition and cooperation are likely endogenous to public opinion. We overcome these challenges by focusing on affective polarization in Israel, and by adapting unique experimental research designs.

Leveraging the random assignment of Jewish Israeli respondents to interview dates in the INES,

we demonstrate that electoral competition, measured by temporal proximity to the election date, increases affective partisan polarization. Acknowledging the centrality of electoral competition to modern democracies, we consider the extent to which cooperative institutional arrangements in the form of unity governments depolarize the electorate. To do so, we leverage the uncertainty surrounding coalition formation following Israel's elections for the 22^{nd} Knesset, and credibly shape survey respondents' beliefs regarding cross-party cooperation in the form of a unity government. We find that information about the likely formation of a unity government has a modest depolarizing effect. Thus, our results suggest that cross-party cooperation can undo some of the negative externalities imposed by electoral competition.

We contribute to the affective polarization literature on three fronts. First, we emphasize the importance of studying not only the causes of, but also the remedies for affective polarization. In doing so, we join a nascent literature which considers how to promote tolerance across partisan lines (Carlin and Love 2018; Levendusky 2018*a*,*b*), and provide an institutional approach which focuses on party competition and cooperation as determinants of polarization. Second, we follow Gidron, Adams and Horne (2019) and extend the study of affective polarization beyond the United States. By focusing on the Israeli case, we also contribute to the literature on intergroup relations in Israel and extend it beyond ethnic and religious divides (Shamir, Dvir-Gvirtzman and Vantura 2017). Third, we make a methodological contribution to the literature on affective polarization by introducing two experimental designs, previously employed by scholars of electoral campaigns (Eifert, Miguel and Posner 2010), and intergroup attitudes (Tankard and Paluck 2017; Barak-Corren, Feldman and Gidron 2018).

Despite these contributions, our paper faces two central limitations. First, the substantive effects we identify in both studies are relatively modest. We believe this to be an artifact of relatively weak treatments. Specifically, in Study I, our treatment measures proximity to elections within a 44 day time period prior to an election. It follows that all INES respondents are exposed to some degree of electoral competition, and our treatment identifies a "local" effect of exposure to enhanced competition. In our second study, our treatment varies one word in an experimental vignette (i.e.

unity/narrow government), to avoid a bundled treatment. In addition, in our survey experiment we identify the effects of increases in the perception that a unity government will form, rather than the average treatment effect of unity government formation. We therefore interpret our findings as conservative lower bound effects.

Finally, in our research we provide a rich account of the effects of cooperation and competition on polarization. Although we provide a theoretical discussion of potential mechanisms which may drive our identified effects, and although we provide suggestive empirical evidence regarding several potential mechanism linking competition with polarization, our empirical advances fall short of providing exhaustive and conclusive evidence regarding causal mechanisms. Future advances should develop suitable research designs, and extend our study to explore why competition increases affective polarization, and what mechanisms link cooperation with depolarization. These mechanisms may relate to the emergence (decline) of nationalism as a superordinate social identity category (Levendusky 2018*a*; Carlin and Love 2018), in light of electoral cooperation (competition). Alternatively, political competition and cooperation may affect polarization, by shaping voters' perceptions of ideological ambiguity across parties. A fruitful empirical strategy to discriminate between these potential mechanisms, would employ rigorous designs which are capable of manipulating not only treatments, but also potential mechanisms. Adapting such designs in a comparative context would greatly advance our understanding of affective polarization.

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Supplementary Information

A INES Study

A.1 INES Surveys and Variable Construction

Our main analyses regarding the effects of electoral competition on polarization leverage interview dates from 7 INES waves as well as feeling thermometers towards political parties. In addition, we leverage several recurring demographic questions to create respondent level covariates and additional outcome variables. In this section we describe the construction of all variables used in our analyses. We display descriptive statistics of these variables in Table A2. We further provide a breakdown of observations and mean values for our main treatment and outcome by election cycle in Table A3.

Our treatment, `Proximity to elections` is constructed by creating a count of days between an interview date and the election, where higher values of our treatment, indicate greater proximity to an election (i.e. a smaller number of days before the election approaches). In Figure A1 we demonstrate the distribution of interview dates across different election cycles.

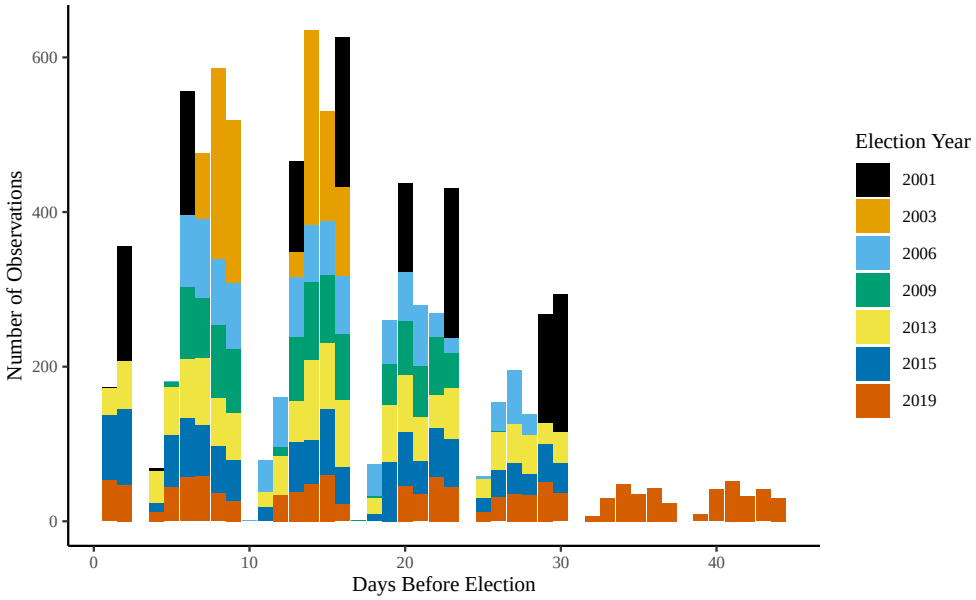

Figure A1: **Distribution of Interview Date (Treatment)** - Interviews were not implemented on days 3, 10, 17, 24, 31, and 38 prior to the election. In addition, only in 2019 (dark orange bars) did the INES interview respondents more than a month prior to the election.

Our main outcome `Polarization` leverages a recurring party feeling thermometer. Question wording for the thermometer goes as follows:

> *What is your attitude toward each of the following political parties? Rate your response on a scale from 0 to 10, where 0 is rejection/hatred, 10 is support/sympathy; and 5 is in between*

We take responses to this question and focus on affect towards clearly left (right) wing parties (see Table A1 below for a full classification of parties by election cycles and voting blocs), in order to construct our main outcome variable. In doing so, we identify voters as left (right) wing supporters, based on their response to a common question regarding political ideology. The wording of this question goes as follows:

> *There is much talk about left and right in politics. Where would you rank yourself along a left-right continuum, when 1 is the right end and 7 is the left end?*[21]

To calculate polarization, we subtract respondents' average affect towards out-parties from their average affect towards in-parties as described in Equation 1 in the main text. As detailed in Section 4.2 of the main text, to reduce concerns regarding post-treatment bias, we show that the variable used to classify voters as right (left) wing supporters is unaffected by our treatment – proximity to elections. Results of this examination are presented in Table A4

Our main demographic covariates include: `gender`, `age`, `religiosity`, `household spending`, `ethnicity`, and `education`. Possible responses to questions relating to respondents' gender, age, and household spending remain consistent over survey waves. Therefore, employing these questions as covariates is rather straightforward. However, since question wording and possible responses varied across waves with regards to religiosity, education, and ethnicity, we collapsed responses to these questions into broad categories, allowing us to maintain general consistency in our measurement. Therefore, we consider religiosity along a four point scale ranging from `Secular` to `Very Religious` (i.e. Ultra-Orthodox). Similarly, we divide education into three categories: `Less the HS`, `HS`, `Academic`. In doign so we loose some of the granularity available in several survey waves. Lastly, our measure of ethnicity (`Ashkenazi`) divides between Ashkenazi and non-Ashkenazi jews, categorizing respondents with European heritage as Ashkenazi.

In order to explore potential mechanisms, we create several additional variables. Specifically, we leverage a factual question regarding the electoral threshold, as well as questions regarding engagement in political conversations, and intended turnout, in order to consider three different mechanisms driving our identified effects. Doing so we create three variables: `information`, `engagement`, and `participation`, which are all analyzed in section 4.5 of the main text. We also create an alternative measure of polarization, focusing on strong partisans which we analyze in section B.2.1 below. Lastly, we create measures of polarization towards central parties (Likud and Labor), and non-center parties (Meretz and Jewish home parties) which are analyzed in Table A10.

[21] Most studies include a 7 item scale, however the 2006 INES data include an 11 point scale ideology measure, and in 2009 respondents were randomly assigned to either 5, 7, or 11 item scale. In all cases, respondents right (left) of center are considered right-wing (left-wing).

	Left Bloc	Right Bloc	Notes
2019	Labor and Meretz	Likud, Jewish Home, Yamin Hadash, Israel is our Home Otzma Yehudit	
2015	Labor and Meretz	Likud, Jewish Home, Israel is our Home	
2013	Labor and Meretz	Likud Betenu, Jewish Home	
2009	Labor and Meretz	Likud, Jewish Home, Israel is our Home	
2006	Labor and Meretz	Likud, Jewish Home, Israel is our Home	
2003	Labor and Meretz	Likud, National Union, Mafdal, Israel Ba'aliya	
2001	Labor	Likud	Prime-Minister Elections

Table A1: **Parties by Ideological Bloc and Electoral Cycle** - We do not consider affect towards center parties (e.g. Kadima, Yesh Atid, Kulanu). In addition, since over different election cycles right-wing parties have united and split-up, we employ the available affect measures in the INES for right wing parties.

Table A2: Descriptive Statistics - Jewish Survey Respondents (2001-2019)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Age	8,439	46.606	17.752	18.000	32.000	60.000	100.000
Female	8,665	0.508	0.500	0.000	0.000	1.000	1.000
Secular	8,581	0.526	0.499	0.000	0.000	1.000	1.000
Traditional	8,581	0.295	0.456	0.000	0.000	1.000	1.000
Religious	8,581	0.101	0.301	0.000	0.000	0.000	1.000
Very Religous	8,581	0.079	0.270	0.000	0.000	0.000	1.000
Spending Well Below Average	6,505	0.205	0.403	0.000	0.000	0.000	1.000
Spending Below Average	6,505	0.193	0.395	0.000	0.000	0.000	1.000
Average Spending	6,505	0.291	0.454	0.000	0.000	1.000	1.000
Above Average Spending	6,505	0.199	0.399	0.000	0.000	0.000	1.000
Well Above Average Spending	6,505	0.112	0.315	0.000	0.000	0.000	1.000
Education - Less than HS	8,124	0.092	0.289	0.000	0.000	0.000	1.000
Education - HS	8,124	0.372	0.483	0.000	0.000	1.000	1.000
Education - Academic	8,124	0.536	0.499	0.000	0.000	1.000	1.000
Ashkenazi	8,648	0.406	0.491	0.000	0.000	1.000	1.000
Party Polarization	4,690	2.645	3.406	−9.000	0.500	5.000	10.000
Party Polarization (Alterantive)	2,981	3.013	3.609	−9.000	1.000	5.500	10.000
Out-Party Affect	4,707	3.227	2.260	0.000	1.000	5.000	10.000
In-Party Affect	4,800	5.878	2.234	0.000	4.500	7.500	10.000
Knowledge	6,332	0.271	0.444	0.000	0.000	1.000	1.000
Intended Participation	3,735	3.691	0.721	1.000	4.000	4.000	4.000
Engagment	5,973	2.898	0.931	1.000	2.000	4.000	4.000

Spending Variable refers to Household spending.

Table A3: Observations, Treatment, and Outcome By Election Cycle

	Cycle	Obs	Mean Treatment	SD Treatment	Mean Polarization	SD Polarization
1	2001	1,248	27.361	9.528	1.565	4.477
2	2003	1,083	33.569	3.297	3.155	2.517
3	2006	1,194	30.141	6.527	2.962	2.813
4	2009	1,037	30.990	5.561	1.730	2.132
5	2013	1,457	30.001	8.146	3.469	3.230
6	2015	1,330	30.641	8.829	3.464	3.228
7	2019	1,317	22.819	13.260	3.327	3.158

Table A4: Effects of Treatment on Right Wing Identification

	Right Wing		
	(1)	(2)	(3)
Proximity to Election	−0.0003	−0.0003	−0.0003
	(0.001)	(0.001)	(0.001)
Demographic Controls	No	Yes	Yes
Social Controls	No	No	Yes
Year FEs	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes
N	6,326	6,164	5,769

A.2 Balance

Our identification strategy for the analysis of INES data leverages the random assignment of respondents to interview dates. In Section 4.1 of the main text, we demonstrate that respondent demographics are well-balanced over values of our treatment. In Table A5 we present additional Table format results of similar multi-variate balance checks, with and without household spending covariates which do not appear in all election studies.

In addition, though not directly a threat to inference in our case, we consider the distribution and demographic correlates of assignment to survey interviews during specific days of the week. In figure A2 we demonstrate that over the seven elections we study, most respondents are interviewed between Sunday and Wednesday, and interviewes are regularly not implemented on the Sabbath.[22] We further consider demographic balance over days of the week in Table A6. We find that for the most part respondents interviewed on different week-days are similar, though in certain specifications (column 2) Ashkenazi respondents are significantly less likely to be interviewed on later days of the week. That said, ethnicity is well balanced over our main treatment – proximity to elections.

A.3 Robustness Checks

In this section we provide Table format results for the robustness checks described in our main text. In Table A7 we consider an alternative functional form of our treatment, as well as an alternative measure of our outcome. Thus columns 1-2 in Table A7 demonstrate that our results remain robust when considering the log of proximity to elections, and columns 3-4 in Table A7 demonstrate that our results remain robust when considering affective polarization only amongst strong partisans.

[22] Several interviews are listed to be implemented on Sabbath for an unclear reason, which we suspect to be a documentation error. Omitting these interviews does not change our results.

Table A5: Demographic Correlations with Treatment

	Proximity to Election	
	(1)	(2)
Age	0.015	0.012
	(0.011)	(0.011)
High-School	0.252	0.256
	(0.295)	(0.313)
Less than High-School	−0.288	−0.107
	(0.577)	(0.517)
Below Average Spending	−0.070	
	(0.394)	
Average Spending	−0.297	
	(0.434)	
Above Average Spending	−0.139	
	(0.388)	
Well Above Average Spending	−0.225	
	(0.558)	
Secular	0.555	0.665
	(0.808)	(0.756)
Traditional	0.169	0.496
	(0.779)	(0.697)
Very Religious	0.892	1.020
	(1.339)	(1.037)
Female	−0.059	−0.149
	(0.104)	(0.090)
Ashkenazi	−0.706	−0.606
	(0.392)	(0.309)
Year FEs	Yes	Yes
Year Cluster	Yes	Yes
N	6,002	7,831

Table A6: Demographic Correlations with Day of the Week

	Days of Week (Sunday - Saturday)	
	(1)	(2)
Age	0.001	−0.001
	(0.002)	(0.003)
High-School	0.014	0.028
	(0.043)	(0.049)
Less than High-School	0.018	−0.018
	(0.071)	(0.070)
Below Average Spending	−0.036	
	(0.037)	
Average Spending	−0.029	
	(0.031)	
Above Average Spending	−0.020	
	(0.029)	
Well Above Average Spending	−0.006	
	(0.033)	
Secular	−0.042	−0.022
	(0.101)	(0.081)
Traditional	−0.024	−0.040
	(0.063)	(0.039)
Very Religious	−0.101	−0.049
	(0.108)	(0.095)
Female	−0.061	−0.023
	(0.034)	(0.044)
Ashkenazi	−0.081	−0.088
	(0.034)	(0.020)
Year FEs	Yes	Yes
Year Cluster	Yes	Yes
N	6,002	7,830

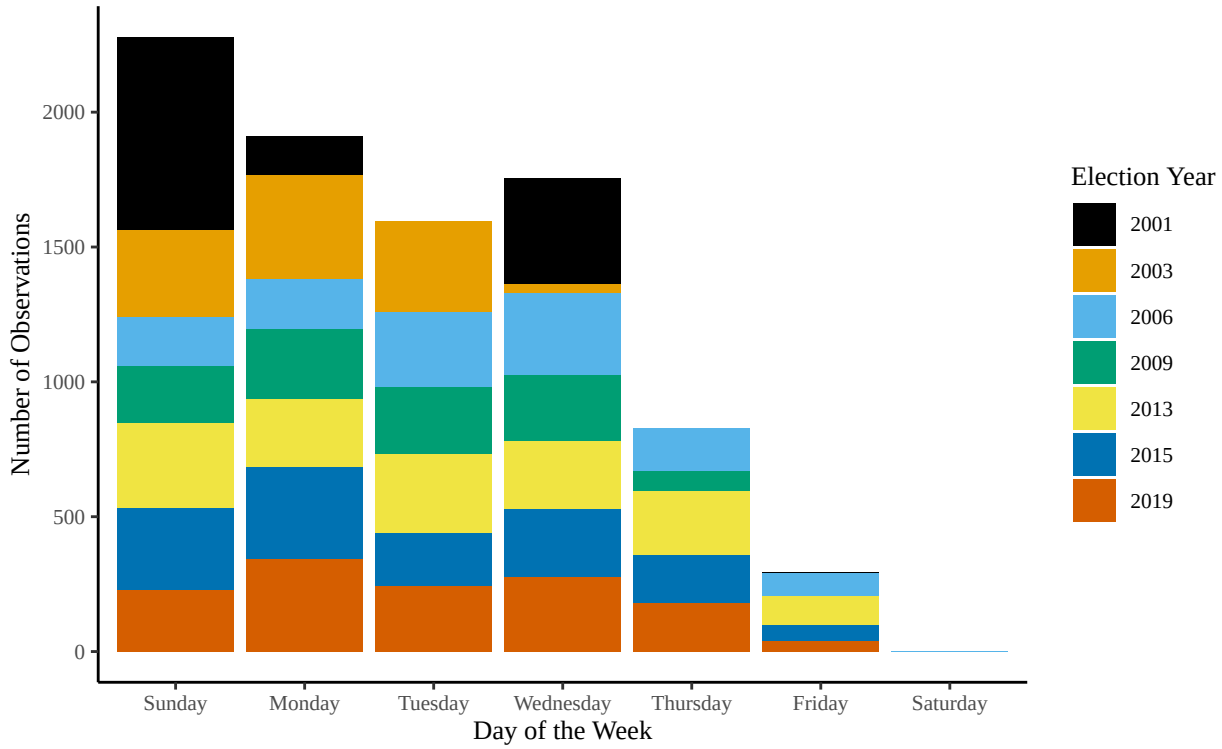

Figure A2: Observations by Day of the Week

Table A7: Election Effect - Robustness to Alternative Measurment

	Polarization		Polarization	
	Log	Log	Partisans	Partisans
	(1)	(2)	(3)	(4)
Log Proximity to Election	0.298	0.290		
	(0.078)	(0.057)		
Proximity to Election			0.013	0.013
			(0.005)	(0.005)
Demographic Controls	No	Yes	No	Yes
Social Controls	No	Yes	No	Yes
Year FEs	Yes	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes	Yes
N	4,690	4,363	2,981	2,768

In Table A8 we demonstrate the robustness of our results to the omission of any given election cycle. Doing so enhances our confidence that our results are not driven by electoral competition during any given election cycle. This could especially be a concern with regards to the 2001 election – a direct election for Israel's prime-minister in which voters' selected leaders rather than parties as part of a temporary split-ticket reform which occurred in Israel in the late 1990s. Nonetheless, even when omitting responses from the 2001 INES, results remain robust.

Table A8: Election Effect - Robustness to Cycle Ommisions

	Polarization						
	Omit 01	Omit 03	Omit 06	Omit 09	Omit 13	Omit 15	Omit 19
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Proximity to Election	0.011	0.012	0.011	0.013	0.013	0.013	0.013
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Demographic Controls	No	No	No	No	No	No	No
Social Controls	No	No	No	No	No	No	No
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3,513	3,830	3,930	4,169	4,201	4,242	4,255

In Table A9 we present results of additional models in which we control for specific weekdays in which respondents were interviewed. Specifically, by controlling for days of the week, and by interacting specific week days (i.e. day of the week, or Sunday) with our main treatment, we demonstrate that our results are not affected, or moderated by specific attributes of any given weekday.

In Table A10 we demonstrate the robustness of our results when employing the polarization measure previously used by Shamir, Dvir-Gvirtzman and Vantura (2017). Unlike our outcome which considers polarization across voting blocs, Shamir, Dvir-Gvirtzman and Vantura (2017) consider attitudes towards the central large parties (i.e. Likud and Labor) and smaller non-center parties. However, when employing their more specific measure of polarization over the seven elections we study, our results remain similar. Electoral competition seems to increase polarization to large center (small non-center) parties.

Since our data are collected from seven election cycles, and respondents are assigned to an interview date within each cycle, we cluster our errors by election year. However, a concern with our main empirical specification may be that clustering errors by election is not a suitable empirical approach. Therefore, in Table A11, we subject our analyses to an alternative specification in which we cluster our errors at the individual respondent level. In Table A12 we present results in which errors are clustered by election week. In addition, in Table A13 we present results with wild cluster

Table A9: Robustness to Day of the Week

	Affective Polarization		
	(1)	(2)	(3)
Proximity to Election	0.0157	0.0097	0.0111
	(0.0091)	(0.0034)	(0.0029)
Day of Week	0.1297		
	(0.1069)		
Proximity to Election * Day of Week	−0.0016		
	(0.0037)		
Sunday		−0.4116	
		(0.3689)	
Proximity to Election * Sunday		0.0075	
		(0.0063)	
Monday			−0.2057
			(0.1333)
Sunday			−0.3037
			(0.2927)
Thursday			−0.0377
			(0.1814)
Tuesday			−0.2692
			(0.1920)
Wednesday			0.0281
			(0.1830)
Demographic Controls	No	No	No
Social Controls	No	No	No
Year FEs	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes
N	4,690	4,690	4,690

Table A10: Robustness Check - Competition Effects on Center and Non-Center Party Polarization

	Center Party Polarization			Non-Center Party Polarization		
	(1)	(2)	(3)	(4)	(5)	(6)
Proximity to Election	0.010	0.008	0.012	0.011	0.011	0.010
	(0.004)	(0.004)	(0.003)	(0.005)	(0.005)	(0.006)
Demographic Controls	No	Yes	Yes	No	Yes	Yes
Social Controls	No	No	Yes	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes	Yes	Yes	Yes
N	4,564	4,455	4,246	3,198	3,125	2,942

bootstrapping (Graham et al. 2016). The additional results presented in Tables A11-A13 suggest that our results remain rather robust to these alternative specifications. However, in some of these alternative specifications, our standard errors slightly increase.

Table A11: Effect of Proximity to Election on Polarization and Party Affect Without Year Clusters

	Polarization			In-Party Affect	Out-Party Affect
	(1)	(2)	(3)	(4)	(5)
Proximity to Election	0.012	0.011	0.011	0.011	−0.001
	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)
Demographic Controls	No	Yes	Yes	No	No
Social Controls	No	No	Yes	No	No
Year FEs	Yes	Yes	Yes	Yes	Yes
Year Cluster	No	No	No	No	No
N	4,690	4,579	4,363	4,800	4,707

Table A12: Effect of Proximity to Election on Polarization and Party Affect Alternative Clustering

	Polarization			In-Party Affect	Out-Party Affect
	(1)	(2)	(3)	(4)	(5)
Proximity to Election	0.011	0.010	0.011	0.006	−0.005
	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)
Demographic Controls	No	Yes	Yes	No	No
Social Controls	No	No	Yes	No	No
Year FEs	Yes	Yes	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes	Yes	Yes
Week Cluster	Yes	Yes	Yes	Yes	Yes
N	3,513	3,425	3,235	3,620	3,527

B Survey Experiment

B.1 Survey Instrument and Descriptive Statistics

In this section we provide an elaborate description of our survey experiment. A pre-analysis plan with the original material is posted with EGAP.[23] As detailed in section 5.1.1 of our main text, the

[23] http://egap.org/registration/6161.

Table A13: Effect of Proximity to Election on Polarization and Party Affect Wild Cluster Bootstrap

	Polarization			In-Party Affect	Out-Party Affect
	(1)	(2)	(3)	(4)	(5)
Proximity to Election	0.012	0.011	0.011	0.011	−0.001
	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)
Demographic Controls	No	Yes	Yes	No	No
Social Controls	No	No	Yes	No	No
Year FEs	Yes	Yes	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes	Yes	Yes
Wild Cluster Bootstrap	Yes	Yes	Yes	Yes	Yes

experiment was administered online by iPanel, an Israeli survey firm. A demographic comparison between the Israeli population of internet users and our studied sample is presented in Table A15. As part of our survey, respondents were asked to report the following demographics: Sex, Age, Ethnicity, Religiosity, Locality, Self-reported vote choice, and position on Left-Right ideological scale

Following these questions, participants were randomly assigned into one of four conditions, as described in Section 5.1.1. Note that we employ two treatments – information regarding a Unity / Narrow government, and the leader of said government [Netanyahu / Gantz]. The distribution of respondents across each of these conditions is presented in Figure A3. Subsequently, respondents were presented with the following outcome questions:

- We are interested in learning about your feelings towards different groups of people. Here are a number of different groups, please place each group on a "feeling thermometer". According to the thermometer, higher numbers indicate more positive feelings. What is your attitude towards individuals from each group? Please indicate your feelings where 0 means rejection and hatred, 100 means support and sympathy, and 50 is in the middle.
  - Left-wing voters
  - Right-wing voters
  - Ultra Orthodox Jews
  - Arabs
  - Likud voters
  - Blue-white voters

  - Israel Beitenu voters
  - Labor-Gesher voters
  - United Torah Judaism voters
  - Democratic Union voters
  - Joint List voters
  - Yamina voters

- Below are some groups of people in Israel. Which is the closest relationship you would find acceptable to maintain with each group? For example, if you would accept someone from a group living on your street, but not as a close friend, then you would choose neighbors. [**Possible answers:** family member, friend, neighbor, coworker, citizen, visitor, none].

  - Left-wing voters
  - Right-wing voters
  - Ultra Orthodox Jews
  - Arabs
  - Likud voters
  - Blue-white voters
  - Israel Beitenu voters
  - Labor-Gesher voters
  - United Torah Judaism voters
  - Democratic Union voters
  - Joint List voters
  - Yamina voters

- On a scale of 1 through 7 (1 highly agree, 7 don't agree at all), how much do you agree with each of the following statements?

  - I am proud to be Israeli
  - I identify as a zionist

- Please mark where you think each of the following parties should be located on an ideological scale, ranging from right (1) and left (7):

- Likud
- Blue-White

It is important to note that the last two questions were asked in order to investigate potential mechanisms driving the effect of unity government.[24] In future follow-up studies we plan to take a more rigorous approach to address this issue, by experimentally manipulating party ideological ambiguity and nationalism, in order to credible identify their mediating effect on polarization. Descriptive statistics of all variables used in our analyses are presented in Table A14, and a comparison of demographics across treatment and control conditions is presented in Table A15.

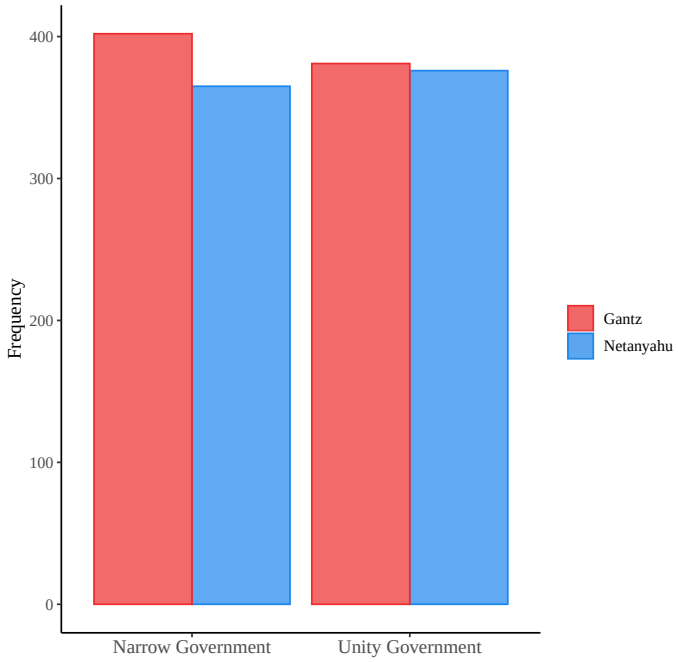

Figure A3: Distribution of Respondents Across Treatment Conditions

B.2 Additional Analysis of Feeling Thermometer

In the main text, we present the effects of our treatment on affective polarization, in terms of social distance. Here (Table A16), we report results from similar analyses, employing alternative outcome variables measured in terms of feeling thermometers. We similarly find negative coefficient signs for our average treatment effect. However, our results are not statistically significant at conventional levels.

That said, in a similar fashion to our social distance measures, we find a positive average treatment effect of our unity government treatment on general out-party affect. This effect approaches

[24] See our pre-analysis plan for more information.

Table A14: Descriptive Statistics - Survey Respondents (Study II)

Statistic	N	Mean	St. Dev.	Min	Max
Female	1,524	0.423	0.494	0	1
Male	1,524	0.577	0.494	0	1
18-22	1,524	0.117	0.322	0	1
23-29	1,524	0.194	0.396	0	1
30-39	1,524	0.249	0.432	0	1
40-49	1,524	0.182	0.386	0	1
50-70	1,524	0.257	0.437	0	1
Ashkenazi	1,496	0.352	0.478	0	1
Mizrahi	1,496	0.383	0.486	0	1
Russian	1,496	0.176	0.381	0	1
Ethiopian	1,496	0.082	0.275	0	1
Other	1,496	0.007	0.085	0	1
Secular	1,524	0.515	0.500	0	1
Traditional	1,524	0.325	0.468	0	1
Religious	1,524	0.135	0.341	0	1
Haredi	1,524	0.026	0.158	0	1
Jerusalem	1,524	0.100	0.300	0	1
Tel Aviv	1,524	0.320	0.466	0	1
North	1,524	0.266	0.442	0	1
South	1,524	0.214	0.410	0	1
Sharon	1,524	0.101	0.301	0	1
Manipulation	1,469	3.241	1.008	1	5
Polarization (Therm)	1,128	41.236	34.669	-100	100
Polarization (Socd)	1,110	1.417	2.070	-5	6
Affect out-party (Therm)	1,128	35.760	23.365	0	100
Affect in-party (Therm)	1,128	76.996	20.531	0	100
Affect out-party (Socd)	1,110	4.888	2.134	1	7
Affect in-party (Socd)	1,110	6.305	1.186	1	7

Therm refers to feelings thermometer, Socd refers to social disctance scale.

	Control	Treatment	Overall	Israeli public
Gender				
Female	341 (44.5%)	303 (40%)	644 (42.3%)	49%
Male	426 (55.5%)	454 (60%)	880 (57.7%)	51%
Age				
18-22	94 (12.3%)	85 (11.2%)	179 (11.7%)	12%
23-29	152 (19.8%)	144 (19%)	296 (19.4%)	17%
30-39	189 (24.6%)	190 (25.1%)	379 (24.9%)	22%
40-49	141 (18.4%)	137 (18.1%)	278 (18.2%)	18%
50 +	191 (24.9%)	201 (26.6%)	392 (25.7%)	31%
Locality				
Jerusalem	81 (10.6%)	71 (9.4%)	152 (10%)	11%
North	193 (25.2%)	212 (28%)	405 (26.6%)	26%
Sharon	77 (10%)	77 (10.2%)	154 (10.1%)	9%
South	163 (21.3%)	163 (21.5%)	326 (21.4%)	22%
Tel Aviv	253 (33%)	234 (30.9%)	487 (32%)	32%
Religiosity				
Haredi	22 (2.9%)	17 (2.2%)	39 (2.6%)	3%
Religious	109 (14.2%)	96 (12.7%)	205 (13.5%)	14%
Secular	388 (50.6%)	397 (52.4%)	785 (51.5%)	52%
Traditional	248 (32.3%)	247 (32.6%)	495 (32.5%)	31%

Table A15: Balance across treatments

conventional levels of statistical significance ($p < 0.7$). It follows that information about unity government formation accounts for over a tenth of a standard deviation increase in out-party affect. In contrary, the coefficient sign of our average treatment effect on in-party affect is negative but statistically insignificant.

Table A16: Effects of Unity Government on Polarization and Party Affect (Thermometer)

	Polarization			In-Party Affect	Out-Party Affect
	(1)	(2)	(3)	(4)	(5)
Unity	−2.810	−2.716	−2.089	−0.229	2.581
	(2.064)	(2.064)	(1.996)	(1.223)	(1.390)
PM Control	No	Yes	Yes	No	No
Demographic Controls	No	No	Yes	No	No
Center Voters	No	No	No	No	No
N	1,128	1,128	1,128	1,128	1,128

Although the results depicted in Table A16 are for the most part directionally comparable to

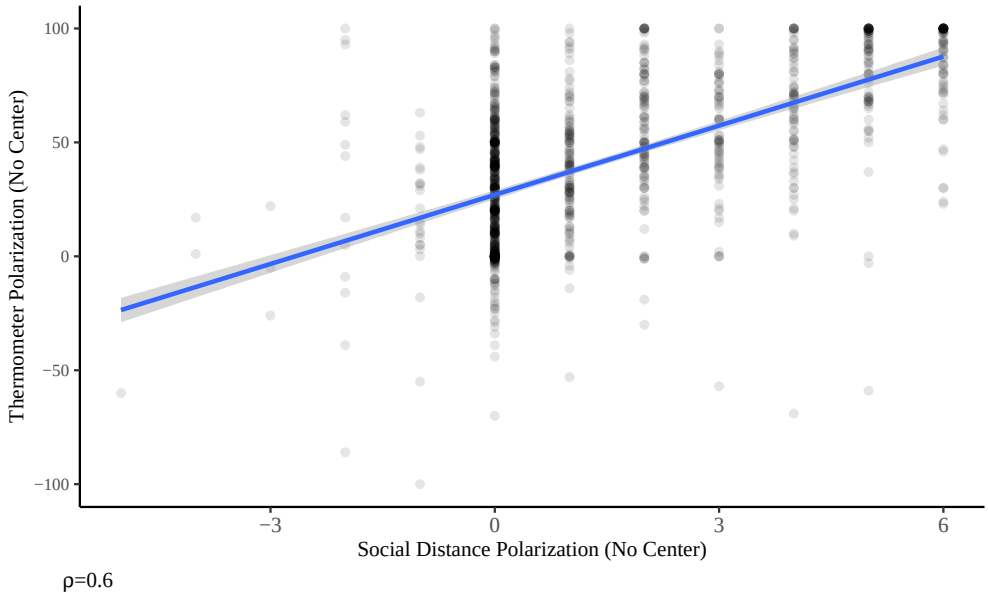

Figure A4: **Correlation between Affective Polarization measured in Feeling Thermometer and Social Distance scales**

the results presented in Table 3, it is important to consider several differences. In particular, we note that the coefficients in Table A16 are largely insignificant, which warrants further investigation. As suggested in previous work (Enos and Gidron 2018), although social distance scales and feeling thermometers share similar properties, they differ in several ways. Particularly, since feeling thermometers ask respondents to provide a general assessment of their attitudes, responses can be more arbitrary and subject to personal interpretation. Social distance scales, on the other hand, provide specific categories that are more useful in discriminating between different preferences.

Figure A4 suggests that while the two indicators are highly correlated ($\rho = 0.6$), they seem to capture somewhat different phenomena. In particular, consider the variation in thermometer polarization for individuals who scored a zero on social distance polarization. Clearly, there is much variation in affect, amongst those who report zero polarization in terms of social distance. This observation suggests that respondents who generally did not mind sharing social spaces with members of the opposing political bloc were still quick to report negative feelings towards members of these groups. In that sense, we expect the specificity of the social distance scale, which has recently been used to examine intergroup relations in Israel, to be more immune to measurement error.

B.2.1 Robustness Checks

In this section, we present figures depicting the robustness checks described in our main text. In Figure A5 we consider the effect of our treatment on six alternative measures of affective polariza-

tion, measured in terms of social distance scale. The first point estimate and corresponding 95% confidence intervals consider an outcome which includes centrist voters as left-wing voters. The second point estimate and corresponding 95% confidence intervals consider an outcome which classifies voters as left and right wing supporters based on their self-reported voting behavior. The third measure which we explore considers distance between affect towards the two central parties (Likud and Blue-White) amongst right and left wing voters.

The fourth coefficient in Figure A5 considers the distance between affect towards the two extreme parties (Yamina and Democratic Union). The fifth (INES measure) in Figure A5 considers affect towards supporters of left and right wing parties, rather than left- and right-wing voters broadly defined. Lastly, the sixth outcome we explore in Figure A5, solely considers voters of the Likud and Blue-White and their attitudes to the competing party. In Figure A6 we consider the same outcomes, employing thermometers rather than social distance items.

As evident in Figure A5, the coefficient signs for our average treatment effect remain negative across all measurement specifications. While the first three models yield similar results to our main analyses, the remaining three models yield results with relatively high p-values, suggesting that our findings may be sensitive to specific measurement approaches.

One explanation for these weak alternative results relates to statistical power. By subsetting our data to include only voters of the two major parties in our fourth measure, we lose a substantial share of observations, making it harder to identify theoretically expected effects.[25] In addition, the insignificant effect of our treatment on the party based measure (i.e. attitudes towards supporters of specific parties, and not towards right or left wing supporters), may be driven by variation across voters in perceptions of party ideology and placement along the right-left cleavage. Lastly, the insignificant effect of our treatment on polarization employing a measure of non-center parties (third measure), may further demonstrate that our results are driven by increased affect towards central parties, but not towards extreme parties.

In addition, as evident in Figure A6, the effect of our treatment on the alternative indicators measured in terms of feeling thermometers largely resemble our findings presented in Table A16. Thus the coefficients signs of our treatment are negative, but our findings are statistically insignificant for most outcome measures.

Lastly, it is important to note that in our experiment we sampled over 1,500 participants. However, approximately 400 participants did not complete our survey. This form of attrition should be concerning, if our treatment is causing attrition. In Table A17, we evaluate this concern. Specifically we show that across different specifications, attrition is not related to either of our main treatments – Unity government or the identity of the expected prime-minister.

[25] Indeed, the number of observations drops significantly when employing a two party measure $n = 878$.

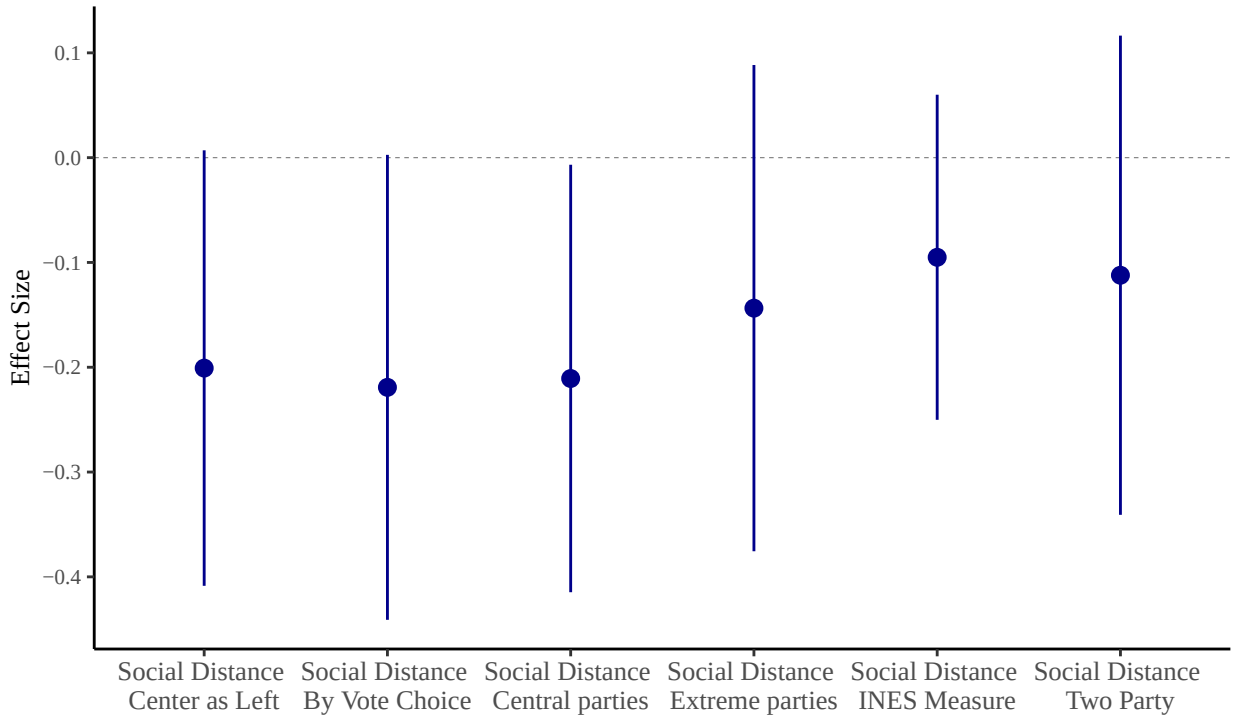

Figure A5: Robustness Check - Unity Government Effect on Polarization (Social Distance)

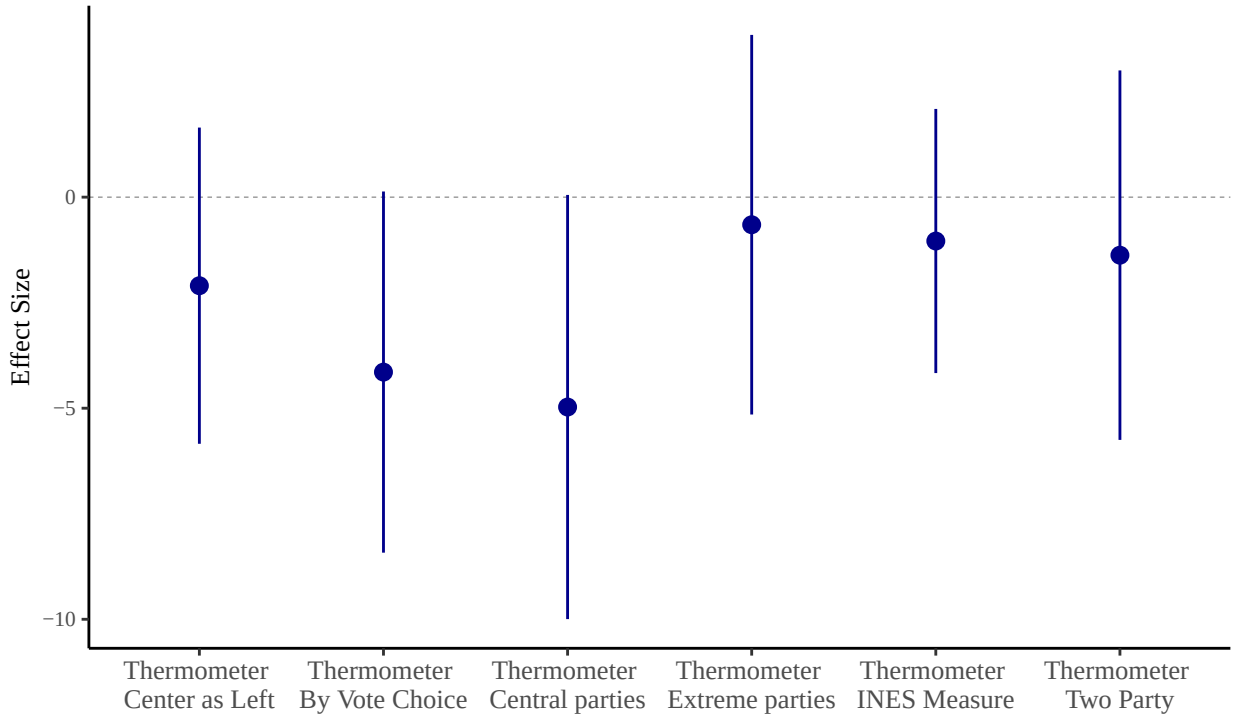

Figure A6: Robustness Check - Unity Government Effect on Polarization (Thermometer)

Table A17: Effect of Treatments on Attrition

	Attrition (Social Distance Question)		Attrition (Thermometer Question)	
	(1)	(2)	(3)	(4)
Unity	0.017	0.005	0.024	0.014
	(0.023)	(0.021)	(0.022)	(0.021)
Prime Minister		0.003		0.007
		(0.021)		(0.021)
Demographic Controls	No	Yes	No	Yes
N	1,524	1,524	1,524	1,524