Party Competition and Cooperation Shape Affective Polarization:

Evidence from Natural and Survey Experiments in Israel[*]

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June 11, 2021

Word Count: 11,523

Forthcoming, Comparative Political Studies

Abstract

Does electoral competition increase affective polarization? Can inter-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 the enhanced salience of electoral competition, increases affective polarization. We then consider whether inter-party cooperation can depolarize the electorate. To do so, we further build on theories of coalition ambivalence and party brands, and leverage the ambiguity around coalition building following elections of Israel's 22nd Knesset, to implement a survey experiment where we credibly shape respondents' perceptions regarding the likelihood that a unity government will form. We find that priming party cooperation in the form of a unity government, promotes tolerance across partisan lines. Our studies contribute to the affective polarization literature by 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 2018a,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 *electoral competition* and *party cooperation*. We conceptualize electoral competition broadly, as the process in which voters, parties, social movements, and the media engage in a host of election related activities. Building on recent evidence, we expect the salience of electoral 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 Forthcoming). This is since elections "...*are the climax of intense group competition...*" (Michelitch 2015, p. 44), and as elections approach, citizens are exposed to higher dosages of campaigns, rallies, political conversations, visible party platforms, and televised debates, all of which are central components of electoral competition (Eifert, Miguel and Posner 2010; Michelitch 2015; Dekeyser and Freedman Forthcoming; Zaller et al. 1992).[1] In contrast to electoral competition, we conceptualize

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[1]To be clear, our conceptual understanding of competition, differs from the concept of competitiveness, which is often thought to imply an underlying propensity for a close election outcome (Moskowitz, Schneer et al. 2019). The relationship between competitiveness and polarization is an

party cooperation as the process in which parties negotiate and eventually reach a shared goal.[2] Specifically, we focus on the most salient form of inter-party cooperation in multi-party systems – coalition formation (Spoon and Klüver 2017; Klüver and Spoon 2017, 2020; Ibenskas 2016; Singh and Thornton 2016; Fortunato and Stevenson 2013; Fortunato and Adams 2015).

Drawing on social identity theory (Tajfel 1978), as well as on theories of coalition ambivalence (Fortunato and Adams 2015; Singh and Thornton 2016), and party brands (Lupu 2013, 2014), we theorize that electoral competition increases polarization, while inter-party cooperation can depolarize the electorate. Providing evidence regarding institutional causes and remedies of affective polarization in the form of electoral competition and party 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 22nd 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 the salience of electoral competition, measured as temporal proximity to the election date, increases general affective partisan polarization by over a tenth of a standard deviation.[3] After establishing the effects of electoral competition on affective polarization, we consider whether inter-party cooperation can depolarize the electorate. To do so, we leverage the ambiguity around coalition building following elections for the 22nd Israeli Knesset, and implement a survey experiment where we credibly shape respondents' perceptions regarding the likelihood that a unity

exciting avenue for research which is beyond the scope of this paper. For studies identifying the effects of competitiveness on polarization see Lelkes (N.d.).

[2]Usually, inter-party cooperation occurs following, rather than during elections.

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

government comprised of the two main competing parties will form in the near future.[4] We find that priming partisan cooperation in the form of a unity government, depolarizes the electorate by over a tenth of a standard deviation, promoting more tolerance towards voters across party lines. In contrast to the polarizing effects of electoral competition which are driven by increased *in-group love*, the depolarizing effects of party-cooperation are largely driven by decreased *out-group hate*.

Competition and cooperation are perhaps the most central dynamics of electoral politics. Nonetheless, despite the many expected democratic virtues of electoral competition, its heightened salience 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 electoral competition for democracy, 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 the enhanced salience of electoral competition.

We make four central contributions to the existing literature. First, we demonstrate that the enhanced salience of electoral competition is a potent cause of affective polarization. In doing so, we expand on recent advances that show how temporal proximity to elections shapes general identification with political parties (Michelitch and Utych 2018; Singh and Thornton 2019), and how voters become less polarized following elections (Hernandez, Anduiza and Rico 2020). Our theory and evidence emphasize one negative externality of electoral competition – affective polarization.

Second, we join recent advances which focus 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 the salience of political

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[4]Throughout the paper we use the term unity government, which is common in Israeli politics, to refer to coalitions that are comprised of competing left- and right-wing parties.

competition polarizes the electorate, and given more general observational findings which link majoritarian institutional arrangements with affective polarization (Gidron, Adams and Horne 2018, 2020).

Third, we contribute to the literature on coalition ambivalence and party brands. Recent studies have demonstrated that parties' decisions to consolidate or join broad coalitions, affect voters' evaluation of party ideology (Spoon and Klüver 2017; Fortunato and Adams 2015; Singh and Thornton 2016), as well as parties' future electoral gains (Lupu 2014; Klüver and Spoon 2020). By focusing on affective polarization as an outcome, we demonstrate that party behavior, and specifically inter-party cooperation, has additional consequences which relate to intergroup relations.

Lastly, we follow Gidron, Adams and Horne (2020), and explore affective polarization in a comparative context. Focusing on Israel where political left-right divides have emerged as 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 also extend the study of prejudice and intergroup relations in Israel beyond ethnic and religious cleavages (Zeitzoff 2014; Barak-Corren, Feldman and Gidron 2018; Weiss 2019), to include partisan identities.

2 Competition, Cooperation, and Polarization

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 United 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, as well as on recent evidence regarding party brands and coalition ambivalence, 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, and emphasize 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 in economic markets (Michelitch 2015) as well as partisan trust gaps (Carlin and Love 2018). More so, following elections voters report less-polarized attitudes (Hernandez, Anduiza and Rico 2020). 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, and specifically the enhances salience of competition, may increase 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, the enhanced salience of 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, contributing to polarization (Garrett et al. 2014). Second, the enhanced salience of electoral competition may encourage engagement in political activities. In turn, voters who attend community meetings or participate in political conversations may become more polar-

ized, especially when partisan sorting is prevalent (Hutchens, Hmielowski and Beam 2019). Third, the enhanced salience of competition may increase the likelihood that voters intend to turnout and vote. Since voting enhances partisan identification (Dinas 2014), it is possible that competition increases polarization through a mechanism of enhanced political participation in elections.

2.2 Cooperation

Cooperation 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), and 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 out-groups who now share an overarching identity.

A substantial portion of the literature on cooperation focuses on personal interactions. However, inter-party cooperation often occurs as an elite process, absent direct engagement between supporters of different parties. For example, congress-members 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 (Hogg 2001; Van Knippenberg 2011). In line with this argument, scholars of comparative politics have demonstrated that party behavior shapes voters' attitudes. Particularly, when parties join coalitions or consolidate into new parties, voters' update their perceptions regarding party ideology (Lupu 2013; Fortunato and Stevenson 2013;

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Spoon and Klüver 2017; Singh and Thornton 2016). Building on these findings, as well as on psychological insights regarding the depolarizing effects of superordinate identities (Gaertner et al. 1999; Brewer 2000), we expect cooperation across party lines to decrease affective polarization.

Coalition formation is the most significant and pronounced form of inter-party cooperation in multi-party electoral systems. Specifically, unity governments which include parties from different sides of the political and ideological spectrum, are a central way through which ideologically diverse societies can promote orderly governance. 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 formally held every four years,[5] 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.[6] 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 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

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

[6]Typically the leader of the largest party.

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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 partisans in Israel has received growing attention. 36% of participants in a recent survey conducted by the Israeli Democratic Institute identified right-left tensions as the strongest in Israeli society, while only 28% of respondents reported tensions between Jews and Arabs as 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 the salience of electoral competition intensifies these existing tensions. This question has become increasingly relevant since 2019, as Israel has faced a record third election after two highly competitive cycles, which eventually created more than a year-long political deadlock.

The centrality of partisan tensions in Israel, which according to our theory are exacerbated by electoral competition, raises an equally intriguing question, namely: what may reduce affective polarization? On September 25th 2019, following a divisive election cycle for Israel's 22nd 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 inter-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 electoral competition and cooperation on affective polarization.

4 Study I: The Salience of Electoral Competition Increases Polarization

Our theory suggests that the salience of electoral competition increases polarization. Nonetheless, causally identifying this relationship is extremely challenging. This is since polarization may be a cause or an effect of salient 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 the salience of electoral competition, measured through proximity to election dates. Under the reasonable assumption that the salience of electoral competition increases as election day approaches, we consider respondents surveyed closer to election dates as exposed to higher levels of salient political competition.

Our empirical strategy differs from previous advances in the polarization literature which focus on party campaigns – a central and important feature of electoral competition (Sood and Iyengar 2016; Iyengar, Sood and Lelkes 2012). In contrast we focus on competition as a broader political construct. Therefore, we consider competition to include campaigns, as well as other elements such as televised debates, increased coverage of politics in the media, rallies, political conversations, and enhanced presence of political elites on social media. We expect All these elements of competition to become more salient closer to the election date, and therefore we theorize that respondents surveyed closer to an election, experience a higher salience of electoral competition.

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.[7]

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 Forthcoming), we consider proximity to an election day as a measure of the salience of electoral competition. This is reasonable given the understanding that electoral competition becomes more salient as elections approach (Michelitch 2015).

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

Since 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 A2 as well as in Table A5 of the appendix. The results of our balance test (further described in Section A.2 of the appendix) enhance the credibility of our identification strategy, as covariates are well balanced over our treatment.[8]

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

[8]In Figure A3 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 Sun-

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 towards 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 which cannot be intuitively linked to a specific voting bloc.[9] 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 A3), 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,

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day 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.

[9]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).

in Section A.3 of our appendix, 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 political 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, which strengthens our theoretically motivated intuition.

4.3 Estimation Strategy

The random assignment of INES respondents to dates over seven election studies allows for a 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 cycle, in order to identify the effects of enhanced salience of electoral competition (β) on affective polarization (y_{ic}) for respondent i interviewed during cycle c.[10]

$$y_{ic} = \beta X_i + \gamma_c + \varepsilon_{ic} \qquad (2)$$

Our main parameter of interest β identifies the effects of the enhanced salience of electoral competition, as a function of temporal proximity to an upcoming election. Higher values of X indicate greater temporal proximity to an election. Therefore, we expect β to be positive and sta-

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[10]In doing so we follow others who use similar designs (Eifert, Miguel and Posner 2010; Michelitch and Utych 2018). In Tables A11, A12 and A13 in the appendix we demonstrate that our results remain substantively similar when clustering by election-week, when clustering at the respondent level, or when using wild cluster bootstrapping.

tistically significant, indicating a positive effect of the enhanced salience of electoral competition on affective polarization. By employing an election-year fixed effect, we isolate the net-effect of proximity to elections, and ensure that particular occurrences during any given election-year–such as enhanced rocket launches or terror attacks (Berrebi and Klor 2008; Getmansky and Zeitzoff 2014)–are not confounding our main estimates.

4.4 Results

In Table 1 we report our main results, identifying the effects of increased salience of electoral competition on affective polarization. Our polarization outcome ranges from -9–10 ($\mu = 2.74$, $\sigma^2 = 3.43$), as reported in Table A1 of the appendix. In turn, the salience of electoral 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 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).[11]

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	0.014	0.012	0.013	0.008	−0.004
	(0.002)	(0.002)	(0.003)	(0.005)	(0.005)
Demographic Controls	No	Yes	Yes	Yes	Yes
Social Controls	No	No	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes	Yes	Yes
N	4,826	4,703	4,486	4,509	4,490

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

In columns 4-5 of Table 1, we further evaluate whether polarization increases in light of in-party affect, or out-party dislike, in the shadow of electoral competition. Recent evidence on intergroup relations often underscores the complexity of disentangling in-group love from out-group hate (Brewer 1999), both of which may contribute to affective polarization. Indeed, while some evidence from American politics points to the centrality of negative partisanship (Iyengar and Westwood 2015; Abramowitz and Webster 2016), others emphasize in-group favoritism as a key phenomenon fueling polarization and conflict (Waytz, Young and Ginges 2014; Lelkes and Westwood 2017; McConnell et al. 2018).

In line with the latter, our analysis suggests that polarization is largely driven by increases in in-party affect as elections approach (column 4), a finding which closely relates to the literature on electoral competition and increases in general partisanship (Michelitch 2015; Michelitch and Utych 2018). The coefficient sign for out-party affect is negative as expected, but substantively small and insignificant (column 5).[12] Taken together, we interpret Table 1 to suggest that electoral competition increases polarization, and that this effect is mainly driven by in-party affect. However, the smaller effect size in column 4 (in-party affect) compared with column 3 (polarization), suggests that polarization is driven at least in part by decreases in out-party affect as well.[13]

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[12]Due to some missingness in our polarization variable, which is driven by non-responses to specific feeling thermometer items, models 3-5 in Table 1 analyze different N sizes. Consequently, the number of observations in models 4-5 is slightly larger than model 3. This limits our ability to compare across the three models, and evaluate the role of in- (out-) party affect in shaping polarization. To allow for a more direct comparison of these models, we re-run models 4-5 from Table 1 amongst respondents for which our polarization measure is not missing. Results from Table A14 in the appendix, which estimates models 3-5 from Tables 1 using the exact same samples, are similar to our our main results, suggesting that polarization is largely driven by in-party affect.

[13]However, these decreases may be rather small, and for that reason, we may fail to identify them in column 5 of Table 1.

4.5 Potential Mechanisms

Our empirical strategy allows us to identify the general effects of the salience of electoral competition as a general bundle. Therefore, in this section we attempt to un-bundle our identified effects, and provide suggestive evidence for three different mechanisms which might link the enhanced salience of competition with polarization. Specifically, we focus on information, engagement, and turnout.

Properly measuring mediating variables and causally identifying mechanisms, are notoriously challenging endeavors (Bullock, Green and Ha 2010). Indeed, our theory and empirical design seek to shed light on a general effect, rather than disentangle particular mechanisms. Thus readers should consider these additional analyses as suggestive, and not interpret our exploration of mechanisms as direct causal evidence for a single theorized mechanism.

With this caveat in mind, we first consider whether our identified effects are driven by increased levels of objective political information regarding electoral institutions amongst survey respondents interviewed closer to election dates. 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.[14] We transform this survey item into a binary variable taking the value of 1 for voters correctly recalling the electoral threshold.[15] In columns 1-2 of Table 2, we regress this information variable, over our main treatment. Doing so, we do not find evidence that the salience

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[14]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.

[15]This measure which is available in recurring INES surveys allows us to consider one type of information relating to respondents' knowledge regarding electoral institutions. However, our measure does not capture other types of information (e.g. regarding social norms of participation) which may link between increased salience of electoral competition and polarization.

of competition increases voters' information regarding the political system, raising doubt that the salience of competition increases polarization through a mechanism of objective political information. In Section A.4 of the appendix, we demonstrate that this null result is consistent across different sub-groups in Israeli society.

Second, we consider whether electoral competition increases polarization through a mechanism of enhanced engagement in political activities. 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, which are consistent across a host of Israeli sub-populations (see Section A.4 of the appendix), 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. intended turnout) mechanism. 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 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. To further strengthen this argument, in Table A15 in the appendix we show that turnout which is caused by increases in the salience of electoral competition, is further predictive of affective polarization.

4.6 Robustness Checks

In addition to the above examinations of our treatment, we subject our main results to a number of robustness checks and additional analyses. First, we model our treatment as a discrete factor variable, rather than a continuous variable in Figure 1. To do so, we divided our treatment (proximity

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
N	6,332	5,599	5,973	5,565	3,735	3,266

to elections) into four categories.[16] 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 similar when focusing on strong partisans.[17] By 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.

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. These analyses provide further confidence that our results are not driven by extreme values of our treatment (e.g. interview dates which are held 45 days prior to an election in the 2019 wave of the INES).

Fifth, we present additional models in which we control for, and consider heterogenous treat-

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[16]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 1.

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

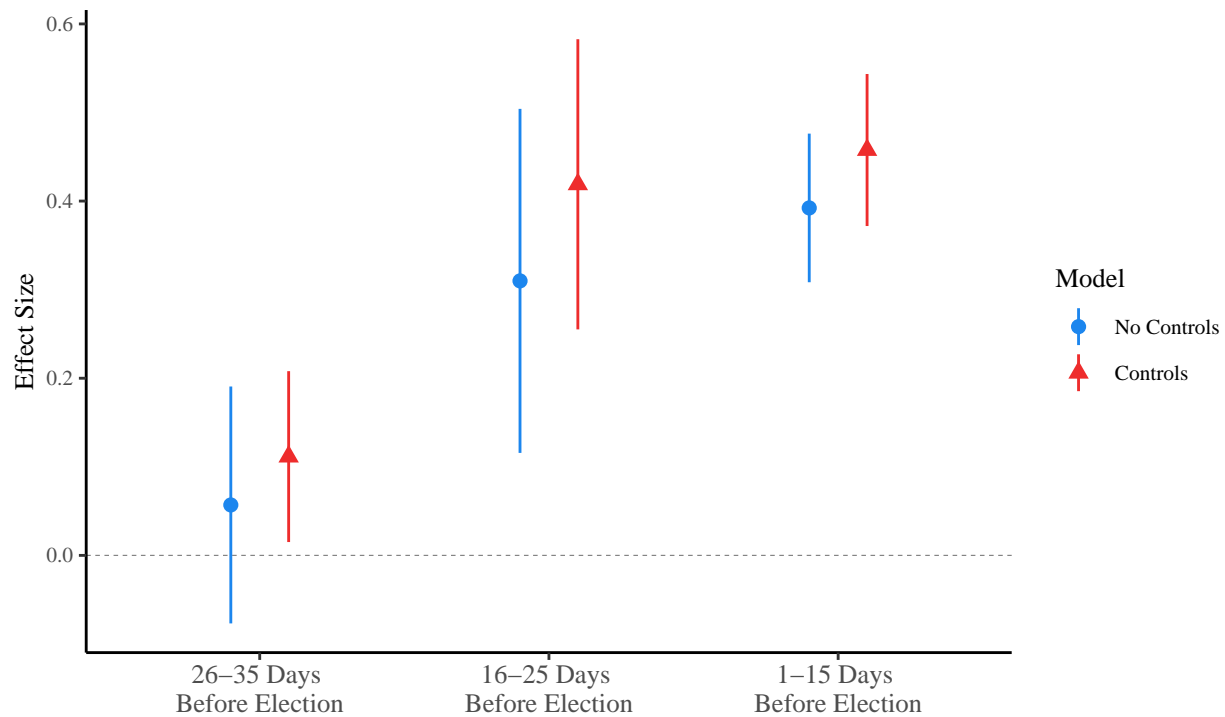Figure 1: **Average Treatment Effects from OLS models with year fixed effects** - In both models with and without controls, affective polarization is regressed over four dummy variables representing interview dates in four periods prior to an election. Standard errors are clustered by year, and the reference category in both models is a dummy variable for respondents interviewed more than 35 days prior to an election.

ment 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. Sixth, in Table A10 we demonstrate that our results remain robust when employing alternative measurements for our polarization outcome variable previously used by Shamir, Dvir-Gvirtzman and Vantura (2017). Lastly, in Tables A11-A13 of the appendix, we consider alternative clustering procedures of our standard errors, demonstrating robustness to alternative specifications.

In addition to these robustness checks, we further consider the heterogenous effect of our treatment conditional on respondent ideology in Table A16 of the appendix. We do not find any meaningful heterogenous treatment effects of electoral competition on polarization in this case. Similarly, one may wonder whether electoral competition shapes affect towards' minority parties (i.e. Ultra-Orthodox and Arab parties). In Table A17, we consider the effects of electoral competition on affect towards Shas, an Ultra-Orthodox party.[18] However, we do not find any evidence for a general, or conditional effect of our treatment on affect towards Shas.

5 Study II: Inter-Party Cooperation Reduces Polarization

In our second study, we explore a potential remedy for affective polarization – inter-party cooperation in the form of a unity government. Identifying the effects of inter-party cooperation on polarization is 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 inter-party cooperation.

5.1 Research Design

To overcome this inferential 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 regarding

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[18]We do not consider the effects of our treatment on affect towards UTJ (Ashkenazi Ultra-Orthodox party), or Arab parties, as consistently recurring feeling thermometer items towards these parties do not exist in most INES waves.

the likelihood of a future court decision.[19] 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 22nd Knesset, to shape respondents perceptions regarding the likelihood of unity government formation. Doing so, we identify the effects of perceptions regarding inter-party cooperation in the form of unity governments on affective polarization.

As mentioned above, elections results for the 22nd 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 national 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 22nd Knesset.

We fielded our experiment in October 2019, following the election, and 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.[20] 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.

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[19]For a similar application in political science see Barak-Corren, Feldman and Gidron (2018).

[20]1,110 participants completed all responses to our outcome measures. Table A27 in the appendix suggests that attrition was not driven by our treatments.

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.[21] 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), and assure that the effects we identify through our unity government treatment are orthogonal to the identity of the politician leading the government. This resulted in a fully crossed 2x2 experimental design. 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.[22] Second, respondents were asked to position each of these groups along a common seven-item social distance scale, which measures preferences for

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[21]By leveraging uncertainty around coalition formation, and by ensuring that our experimental vignette does not refer to particular commentators, we avoid the use of deception in our experiment.

[22]Only the nine parties that have met or exceeded the electoral threshold were included.

social exclusion (Bogardus 1933; 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.[23] Finally, as a manipulation check, we asked respondents 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). An English description of our instrument is presented in Section B.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 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). Specif-

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[23]See a discussion of these measures in the appendix.

ically, we no longer need to assume how individual survey respondents classify different political parties along the right-left cleavage.[24]

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.[25] Recent evidence from the U.S. suggests that affect towards parties is correlated with affect towards supporters of those parties. More so, when asked generally about affect towards parties, American voters often think of party elites (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 theory.

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. 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 2 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. This

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[24]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.

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

is a rather small shift, which is expected in light of the fact that we deliberately designed our experimental vignette as a light touch intervention, in order to avoid a bundled treatment and demand effects.
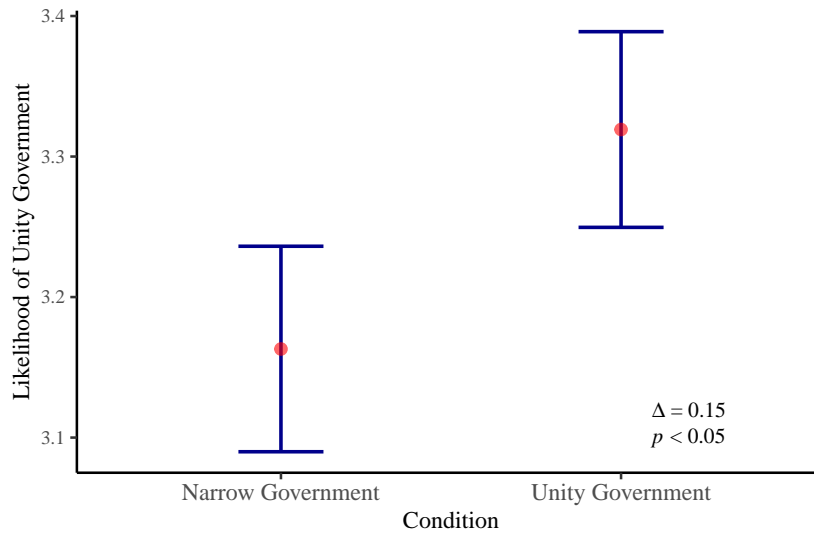
Figure 2: **Manipulation Check** - Treatment effect on perceptions of the likelihood that a unity government will form in the near future.

Results from our main analysis in which we identify the effects of our unity government treatment on polarization in terms of social distance are depicted in Table 3. Our main polarization outcome considered in Table 3 ranges from -5–6 ($\mu = 1.41$, $\sigma^2 = 2.07$, see Table A22 of the appendix). Generally, we find that information about inter-party cooperation in the form of a unity government reduces affective polarization. Specifically, as reported in model 1 of Table 3, information about the expected unity government accounts for over a tenth of a standard deviation decrease in affective polarization ($p = 0.05$).[26] 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

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[26] As reported in Table A22 of our supplementary material, our main polarization outcome ranges from -5-6, $\mu = 1.41$, $\sigma^2 = 2.07$.

$(p < 0.1)$.

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. Unlike competition which theoretically may fuel both in-group love and out-group hate,[27] our theoretical framework suggests that party cooperation depolarizes the electorate by reducing out-group animosity. This is because party cooperation may facilitate an overarching identity, or reveal elite-level interactions which shed a positive light on the out-party.

Columns 4-5 in Table 3 suggest that the depolarization 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.[28]

A careful interpretation of the patterns identified in Table 3, as opposed to the patterns identified in Table 1, lends some interesting suggestive insights. Indeed as our theory suggests, the salience of electoral competition increases polarization, and inter-party cooperation decreases polarization. More so, changes in polarization across both studies manifest in diverging ways. Specifically, the salience of electoral competition increases polarization by mainly increasing respondents' in-group

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[27]But empirically we show generates in-group love.

[28]In Table A24 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.

love, whereas inter-party cooperation depolarizes the electorate by mainly decreasing out-group hate. These findings are in line with general expectations in the social identity literature, which suggest that competition increases in-group feelings towards one's ingroup, whereas cooperation facilitates superordinate identities, which in turn reduces prejudice towards out-groups.

However, it is important to note, that readers should be cautious when comparing effects across both our studies. Even though we credibly identify the effects of competition and cooperation, we do not empirically identify the effects of competition in comparison to cooperation. More so, we do not suggest that cooperation is the opposite, or counterfactual political dynamic to cooperation. In addition, both our studies rely on different empirical strategies, observe slightly different samples, and employ mildly different outcome measures. Therefore, we suggest that comparisons across both studies should be implemented with caution.

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

5.3 Externalities for Minority Voters

One concern regarding the formation of unity governments, which facilitate superordinate identities and depolarize the electorate, relates to externalities for minority voters. Put differently, a coalition comprised of the main left- (right-) wing parties may depolarize voters. However, since this coalition does not include small minority parties, this may lead to enhanced prejudice towards those excluded minorities, thus undermining the remedial impact of unity governments.

We evaluate the plausibility of this claim in Section B.3 of our appendix. To do so, we regress

measures of social distance towards Arabs and Ultra-Orthodox Jews over our unity government treatment. We do not identify a significant effect of our treatment on these outcomes. That said, in additional sub-group analyses, we find some evidence that our unity government treatment increases prejudice towards Ultra-Orthodox Jews amongst self-identified centrist voters, and decreases prejudice towards Ultra-Orthodox Jews amongst self-identified right-wing voters. We suggest that future research should build on these patterns, and consider whether, why, and under what conditions does the formation of coalition governments shape attitudes towards minorities.

5.4 Robustness Checks

We subject our results to several robustness checks presented in Figures A6 and A7 in the appendix. Specifically, we present six alternative measurement strategies for our main outcome of interest – affective polarization. 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 discussion of these measures is presented in Section A.3 of the appendix.

As evident in Figure A6 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 A.3 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.

Lastly, we consider a series of heterogenous treatment effects in Section B.5 of our appendix. Particularly, we estimate the heterogenous effect of our treatment, conditional on ideology and self-reported vote choice. We do not find evidence that right- or left-wing voters respond differently to our treatment. In addition, we do not find any evidence that our prime-minister identity treatment (i.e. Netanyahu/Gantz) has an independent polarizing effect, or moderates the effects of information regarding the likely formation of a unity government on affective polarization (see Table A29 in the appendix).

6 Conclusion

In this paper we draw on social identity theory, as well as on recent frameworks regarding party brands and coalition ambivalence, in order to theorize about the effects of electoral competition and inter-party cooperation on affective polarization. Leveraging the random assignment of Jewish Israeli respondents to interview dates in the INES, we demonstrate that the salience of electoral competition, measured by temporal proximity to the election date, increases affective partisan polarization amongst Israeli voters. 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 22nd 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 depolarizes the electorate. Our results suggest that inter-party cooperation can undo some of the negative externalities imposed by electoral competition.

We contribute to the existing literature on four fronts. First, we demonstrate that the salience of electoral competition is a potent cause of affective polarization. Second, 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 inter-party cooperation.

Third, we contribute to the literature on coalition ambivalence and party brands by focusing on affective polarization as an outcome. Doing so, we demonstrate that party behavior, and specifically inter-party cooperation, affects additional outcomes beyond voters' perception of party ideology, and party vote share. Lastly, we follow Gidron, Adams and Horne (2020) 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.

Despite these contributions, our paper faces some central limitations that motivate future research. First, the substantive effects we identify in both studies are relatively modest. We believe this to be an artifact of our relatively weak treatments. Specifically, in Study I, our treatment measures proximity to elections within a 44 day 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 and minimize demand effects. 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.

Second, in our research we provide a rich account of the effects of cooperation and the salience of electoral 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 to rule out several alternative mechanism linking competition with polarization, our empirical advances fall short of providing direct evidence regarding causal mechanisms. Future advances should develop suitable research designs, and extend our study to explore why the enhanced salience of 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 (Imai et al. 2011). Adapting such designs in a comparative context would greatly advance our understanding of affective polarization.

Third, like other comparative studies of polarization, we face a challenge relating to the mea-

surement of outcomes in a multi-party system. To address this challenge, we follow Shamir, Dvir-Gvirtzman and Vantura (2017) and focus on polarization between Left and Right-wing partisans. We further demonstrate that our results are robust to alternative measurements of partisan polarization which omit centrist voters, or consider them as part of the left-wing bloc. Given the ongoing decline of Left-wing parties in Israel, and the rise of a robust center voting bloc, future research should further evaluate polarization along different cleavages.

Lastly, we find little evidence that information regarding the formation of unity governments shapes attitudes towards minority citizens and their parties who are excluded from power. However, a robust literature suggests that exclusion from state institutions impairs intergroup relations and causes civil conflict (Cederman, Wimmer and Min 2010). Therefore, we suggest that future studies should extend our initial exploration of prejudice towards minorities, and identify whether, and under what conditions, coalition governments which do not include minority representatives, impair intergroup relations.

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Party Competition and Cooperation Shape Affective Polarization

Supplementary Information

A INES Study

A.1 INES Surveys and Variable Construction

Our main analyses regarding the effects of the salience 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 A1. We further provide a breakdown of observations and mean values for our main treatment and outcome by election cycle in Table A2.

Our treatment, `Proximity to elections` is constructed by creating a count of days between an interview date and the upcoming 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. 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.

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 A3 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-)
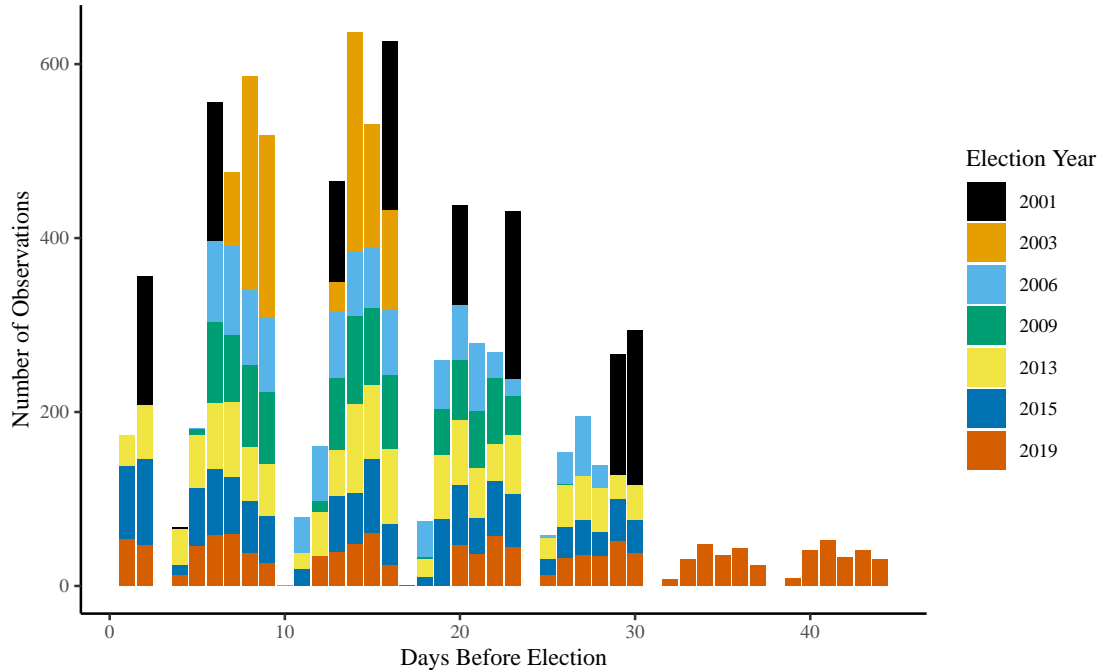
Figure A1: **Distribution of Interview Date (Treatment)** - Interviews were not implemented on days 3, 10, 17, 24, 31, and 38 prior to the election. In addition, only in 2019 (dark orange bars) did the INES interview respondents more than a month prior to the election.

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?*[29]

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

[29]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).

Table A1: 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	4,826	2.738	3.431	−9.000	0.667	5.000	10.000
Party Polarization (Alterantive)	3,063	3.111	3.603	−9.000	1.000	5.500	10.000
Out-Party Affect	4,831	3.121	2.254	0.000	1.000	4.500	10.000
In-Party Affect	4,850	5.863	2.277	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 A2: 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	2.608	2.825
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

	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 A3: **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 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

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 doing 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 A.3 below. Lastly, we create measures of polarization towards central parties (Likud and Labor), and non-centeral parties (Meretz and Jewish home parties) which are analyzed in Table A10.

A.2 Balance

Our identification strategy for the analysis of INES data leverages the random assignment of respondents to interview dates. In Figure A2 we provide supporting evidence of our identifying assumption. Specifically, we report the bivariate as well as multi-variate correlation of age, gender, household spending, religiosity, education, and ethnicity with treatment assignment. To account for the fact that treatment is assigned every election-year, we include election-year fixed effects in all models, and cluster errors by year. The results presented graphically in Figure A2
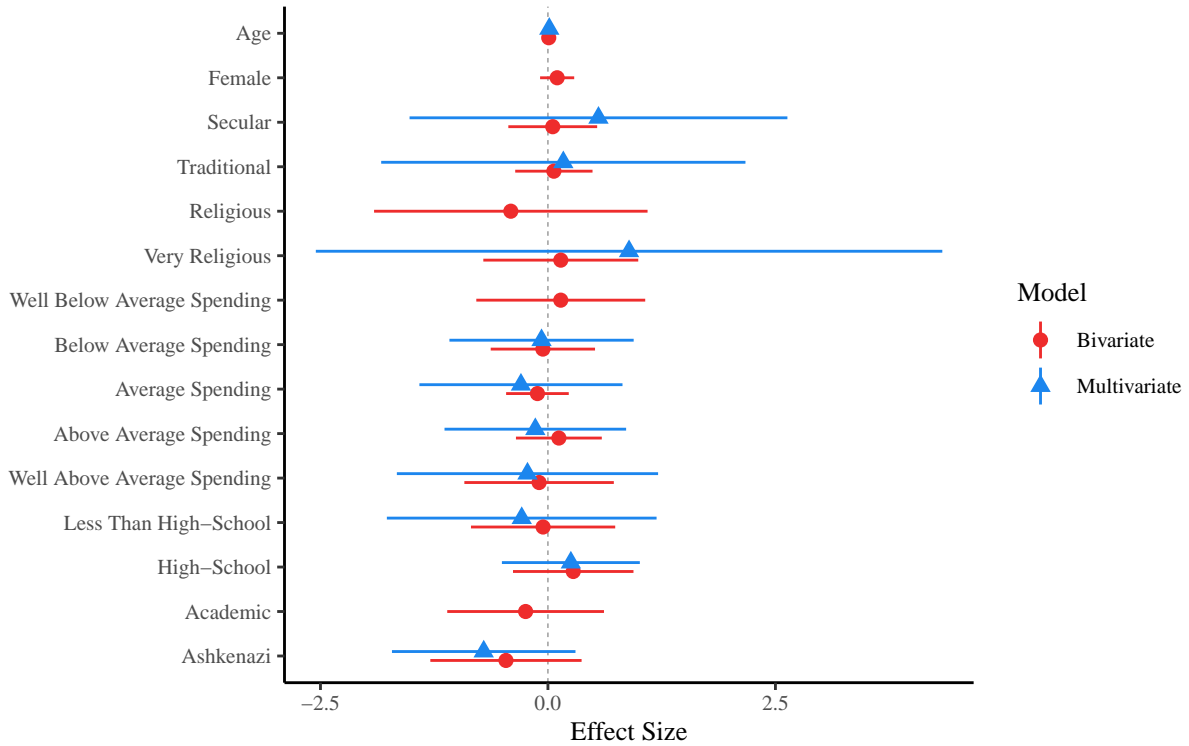
Figure A2: **Covariate Balance** - Point estimates and their corresponding 95 percent confidence intervals from OLS models with election year fixed effects, where errors are clustered by year. Coefficients in red are extracted from bivariate models identifying the association between our treatment and 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.

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 ($p = 0.21$). 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. The results presented in Table A5 lend further credibility to our central identification assumption.

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 A3 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

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
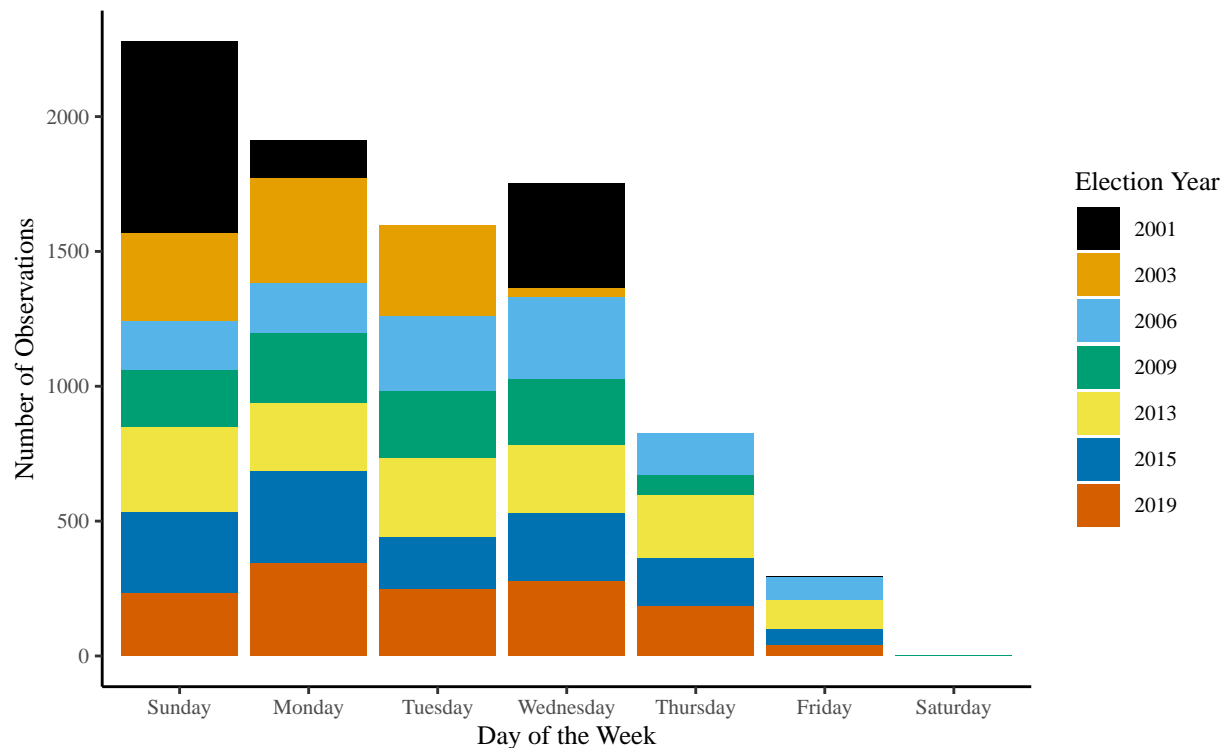
Figure A3: Observations by Day of the Week

Sabbath.[30] 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. Regardless, as shown in Section A.3 controlling for days of the week does not change
our main findings.

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

[30]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 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

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.

Table A7: Election Effect - Robustness to Alternative Measurment

	Polarization		Polarization	
	Log	Log	Partisans	Partisans
	(1)	(2)	(3)	(4)
Log Proximity to Election	0.320	0.311		
	(0.085)	(0.065)		
Proximity to Election			0.015	0.014
			(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	4,826	4,486	3,063	2,842

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.

An alternative concern may be that our results are driven by the 2019 INES study, which interviewed survey respondents up to 45 days prior to the election, unlike other studies which focus on shorter time periods. However, as demonstrated in Table A8, even when omitting the 2019 election, or results remain robust. In fact, by doing so, we increase our point estimate, suggesting that if anything, our main results provide a conservative estimate of the effects of electoral competition on cooperation.

In Table A9 we present results of additional models in which we control for specific weekdays at 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,

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	0.013	0.013	0.013	0.013	0.015	0.015	0.015
	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)	(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	3,649	3,966	4,066	4,169	4,337	4,378	4,391

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-central 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 central, as well as smaller non-central parties.

Since in our data respondents are sampled from seven election cycles, we cluster our errors by election-year, to account for what Abadie et al. (2017) call a sampling design issue. However, a concern with our main empirical specification may be that clustering errors by election-year 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 bootstrapping (Graham et al. 2016). The additional estimates 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 A9: Robustness to Day of the Week

	Affective Polarization		
	(1)	(2)	(3)
Proximity to Election	0.0179	0.0114	0.0123
	(0.0104)	(0.0033)	(0.0031)
Day of Week	0.1435		
	(0.1074)		
Proximity to Election * Day of Week	−0.0020		
	(0.0039)		
Sunday		−0.3818	
		(0.3541)	
Proximity to Election * Sunday		0.0066	
		(0.0058)	
Monday			−0.3089
			(0.1951)
Sunday			3.4975
			(0.2262)
Thursday			−0.3986
			(0.3135)
Tuesday			−0.1529
			(0.2689)
Wednesday			−0.3942
			(0.2598)
day_nameWednesday			−0.0587
			(0.2240)
Demographic Controls	No	No	No
Social Controls	No	No	No
Year FEs	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes
N	4,825	4,826	4,826

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	0.013	0.010	0.015	0.011	0.010	0.009
	(0.005)	(0.004)	(0.004)	(0.004)	(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	4,778	4,656	4,446	3,498	3,401	3,215

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	0.014	0.012	0.013	0.011	−0.003
	(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	4,826	4,703	4,486	4,850	4,831

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	0.013	0.012	0.014	0.006	−0.007
	(0.006)	(0.006)	(0.008)	(0.005)	(0.006)
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	3,649	3,549	3,358	3,670	3,651

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	0.014	0.012	0.013	0.011	−0.003
	(0.002)	(0.002)	(0.003)	(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	Yes	Yes	Yes	Yes	Yes
Wild Cluster Bootstrap	Yes	Yes	Yes	Yes	Yes

As mentioned in Section 4.4, Columns 3-5 in Table 1 are hard to directly compare, since they focus on slightly different samples. This is due to missingness in responses to feeling thermometers towards left- (right-) wing parties. Naturally, since our polarization measure employs items of affect towards both left- and right-wing parties, polarization missingness is larger than missingness in our measure of general affect towards left- (right-) wing parties.

In order to facilitate an analysis which allows for a slightly more precise comparison between models 3-5 in Table 1, we re-estimate these models amongst respondents for which there is no missingness in the polarization variable. As evident in Table A14, these models all include a similar sample size, and focus on the same respondents. However, the results in Table A14, are very similar to our main finding, suggesting that affective polarization is mainly, but not only, driven by increases in in-party affect.

In Table A15 we consider whether turnout is predictive of affective polarization, with and without controls. We find that the relationship between turnout and affective polarization is positive and statistically significant. This finding further offers suggestive evidence that competition increases affective polarization by increasing turnout intentions, as we argue in the main text.

In Table A16 we consider sub-group effects. Particularly, we explore whether ideology moderates the effects of electoral competition. We do so by interacting our treatment with self-reported

Table A14: Effect of Proximity to Election on Polarization and Party Affect omitting Respondents where Polarization = NA

	Polarization	In-Party Affect	Out-Party Affect
	(1)	(2)	(3)
Proximity to Election	0.013	0.009	−0.004
	(0.003)	(0.005)	(0.005)
Demographic Controls	Yes	Yes	Yes
Social Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes
N	4,486	4,486	4,486

Table A15: Effect of Turnout on Affective Polarization

	Polarization		
	(1)	(2)	(3)
Turnout	0.3967	0.3172	0.3048
	(0.1179)	(0.1248)	(0.0825)
Demographic Controls	No	Yes	Yes
Social Controls	No	Yes	Yes
Year FEs	No	No	Yes
N	1,573	1,408	1,408

ideology. As demonstrated in Table A16, right- and left-wing voters do not seem to respond in diverging ways to our main treatment – electoral competition.

Table A16: Effect of Proximity to Election on Polarization Conditional on Ideology

	Polarization	
	(1)	(2)
Proximity to election	0.023	0.008
	(0.007)	(0.004)
Ideological Right	−0.414	
	(0.438)	
Ideological Left		0.639
		(0.371)
Proximity to election*Right	−0.014	
	(0.009)	
Proximity to election*Left		0.016
		(0.009)
Demographic controls	Yes	Yes
Social controls	Yes	Yes
Year FEs	Yes	Yes
Year Cluster	Yes	Yes
N	4,486	4,486

In Table A17 we consider the extent to which electoral competition shapes affect towards minority parties. Particularly, we focus on attitudes towards Shas – A Mizrachi Ultra-Orthodox party. Our sole focus on Shas, and inability to consider attitudes towards other Ultra-Orthodox parties, or Arab parties, is an artifact of limited recurring feeling thermometers about those additional parties. Regardless, as demonstrated in Table A17, electoral competition does not seem to affect attitudes towards Shas. More so, we do not identify sub-group effects amongst right, left, or center voters.

A.4 Sub-Group Effects on Potential Mechanisms

In this section we consider the extent to which our results reported in Table 2 of the main text are consistent across different theoretically meaningful sub-groups. Specifically, one may expect that changes in political knowledge as a function of proximity to elections, will be more pronounced amongst survey respondents with low pre-treatment levels of political knowledge. Unfortunately, we do not have pre-treatment measures of political knowledge. Thus we cannot directly test this

Table A17: Effect of Proximity to Election on Affect towards Shas party Conditional on Ideology

	Affect towards Shas			
	(1)	(2)	(3)	(4)
Proximity to election	0.085	0.088	0.081	0.070
	(0.054)	(0.050)	(0.043)	(0.041)
Ideological Right		0.733		
		(0.358)		
Ideological Left			−1.549	
			(1.776)	
Ideological Center				−1.035
				(1.264)
Proximity to election*Right		−0.015		
		(0.019)		
Proximity to election*Left			−0.006	
			(0.044)	
Proximity to election*Center				0.055
				(0.042)
Demographic controls	No	Yes	Yes	Yes
Social controls	No	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes	Yes
N	5,049	4,391	4,391	4,562

proposition.

Instead, we use education as a proxy for political knowledge under the assumption that more educated voters are also more knowledgable about politics in Israel.[31] That said, in Table A18 we do not find supportive evidence for the expectation that changes in political knowledge as election day approaches are more pronounced amongst respondents with lower levels of education. Since we do not have a direct pre-treatment measure of political knowledge, we suggest that readers take the supplementary results of Table A18 as suggestive evidence.

Table A18: Effects of Competition on Knowledge, Moderated by Education

	Success in Factual Question	
	(1)	(2)
Proximity to Election	0.0010	0.0010
	(0.0024)	(0.0025)
Education	0.0630	0.0674
	(0.0282)	(0.0276)
Proximity to Election*Education	0.0004	0.0004
	(0.0011)	(0.0011)
Demographic Controls	No	Yes
Social Controls	No	Yes
Year FEs	Yes	Yes
Year Cluster	Yes	Yes
N	5,821	5,599

We further consider how gender, ethnicity and age moderate the effects of proximity to elections on knowledge (Table A19), engagement (Table A20), and intended turnout (Table A21). These additional models where we interact our treatment with several theoretically meaningful covariates provide limited support for the expectation that the effect of proximity to elections on the outcomes from Table 2, vary in a substantively meaning way across subgroups in the INES.

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[31]Using education is reasonable since theoertically, it cannot be affected by our treatment.

Table A19: Effects of Competition on Knowledge, Moderated by Age, Ethnicity and Gender

	Knowledge		
	(1)	(2)	(3)
Proximity to Election	0.0014	0.0020	0.0017
	(0.0008)	(0.0011)	(0.0006)
Age	0.0007	0.0010	0.0010
	(0.0009)	(0.0006)	(0.0006)
Ethnicity	0.2167	0.2166	0.2007
	(0.0252)	(0.0253)	(0.0460)
Gender	0.0195	0.0256	0.0198
	(0.0161)	(0.0386)	(0.0158)
Proximity to Election*Age	0.00001		
	(0.00002)		
Proximity to Election*Ethnicity		−0.0002	
		(0.0011)	
Proximity to Election*Gender			0.0006
			(0.0019)
Demographic Controls	Yes	Yes	Yes
Social Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes
N	5,599	5,599	5,599

Table A20: Effects of Competition on Engagement, Moderated by Age, Ethnicity and Gender

	Engagement		
	(1)	(2)	(3)
Proximity to Election	0.0004	−0.0041	−0.0002
	(0.0033)	(0.0018)	(0.0041)
Age	0.0048	0.0020	0.0019
	(0.0025)	(0.0011)	(0.0011)
Ethnicity	0.0255	0.0262	0.2342
	(0.0392)	(0.0391)	(0.0980)
Gender	−0.0705	−0.0969	−0.0723
	(0.0350)	(0.1308)	(0.0349)
Proximity to Election*Age	−0.0001		
	(0.0001)		
Proximity to Election*Ethnicity		0.0008	
		(0.0047)	
Proximity to Election*Gender			−0.0069
			(0.0035)
Demographic Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes
N	5,565	5,565	5,565

Table A21: Effects of Competition on Turnout, Moderated by Age, Ethnicity and Gender

	Turnout		
	(1)	(2)	(3)
Proximity to Election	0.0036	0.0028	0.0053
	(0.0009)	(0.0006)	(0.0011)
Age	0.0021	0.0018	0.0018
	(0.0021)	(0.0015)	(0.0015)
Ethnicity	0.0248	0.0249	0.1580
	(0.0364)	(0.0365)	(0.1030)
Gender	0.0064	−0.0243	0.0061
	(0.0227)	(0.0135)	(0.0231)
Proximity to Election*Age	−0.00001		
	(0.00002)		
Proximity to Election*Ethnicity		0.0010	
		(0.0009)	
Proximity to Election*Gender			−0.0044
			(0.0024)
Demographic Controls	Yes	Yes	Yes
Social Controls	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Year Cluster	Yes	Yes	Yes
N	3,266	3,266	3,266

B Survey Experiment

B.1 Survey Instrument and Descriptive Statistics

In this section we provide an elaborate description of our survey experiment. As detailed in section 5.1.1 of our main text, the 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 A23. 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 A4. 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. 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 A22, and a comparison of demographics across treatment and control conditions is presented in Table A23.

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 A24), 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 conventional levels of statistical significance ($p < 0.07$). 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.

Although the results depicted in Table A24 are for the most part directionally comparable to the results presented in Table 3, it is important to consider several differences. In particular, we

Table A22: Descriptive Statistics - Survey Respondents (Study II)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Female	1,524	0.423	0.494	0	0	1	1
Male	1,524	0.577	0.494	0	0	1	1
18-22	1,524	0.117	0.322	0	0	0	1
23-29	1,524	0.194	0.396	0	0	0	1
30-39	1,524	0.249	0.432	0	0	0	1
40-49	1,524	0.182	0.386	0	0	0	1
50-70	1,524	0.257	0.437	0	0	1	1
Ashkenazi	1,496	0.352	0.478	0.000	0.000	1.000	1.000
Mizrahi	1,496	0.383	0.486	0.000	0.000	1.000	1.000
Russian	1,496	0.176	0.381	0.000	0.000	0.000	1.000
Ethiopian	1,496	0.082	0.275	0.000	0.000	0.000	1.000
Other	1,496	0.007	0.085	0.000	0.000	0.000	1.000
Secular	1,524	0.515	0.500	0	0	1	1
Traditional	1,524	0.325	0.468	0	0	1	1
Religious	1,524	0.135	0.341	0	0	0	1
Haredi	1,524	0.026	0.158	0	0	0	1
Jerusalem	1,524	0.100	0.300	0	0	0	1
Tel Aviv	1,524	0.320	0.466	0	0	1	1
North	1,524	0.266	0.442	0	0	1	1
South	1,524	0.214	0.410	0	0	0	1
Sharon	1,524	0.101	0.301	0	0	0	1
Manipulation	1,469	3.241	1.008	1.000	3.000	4.000	5.000
Polarization (Therm)	1,128	41.236	34.669	-100.000	12.750	68.000	100.000
Polarization (Socd)	1,110	1.417	2.070	-5.000	0.000	3.000	6.000
Affect out-party (Therm)	1,128	35.760	23.365	0.000	18.000	50.000	100.000
Affect in-party (Therm)	1,128	76.996	20.531	0.000	62.750	94.000	100.000
Affect out-party (Socd)	1,110	4.888	2.134	1.000	3.000	7.000	7.000
Affect in-party (Socd)	1,110	6.305	1.186	1.000	6.000	7.000	7.000

Therm refers to feelings thermometer, Socd refers to social disctance scale.
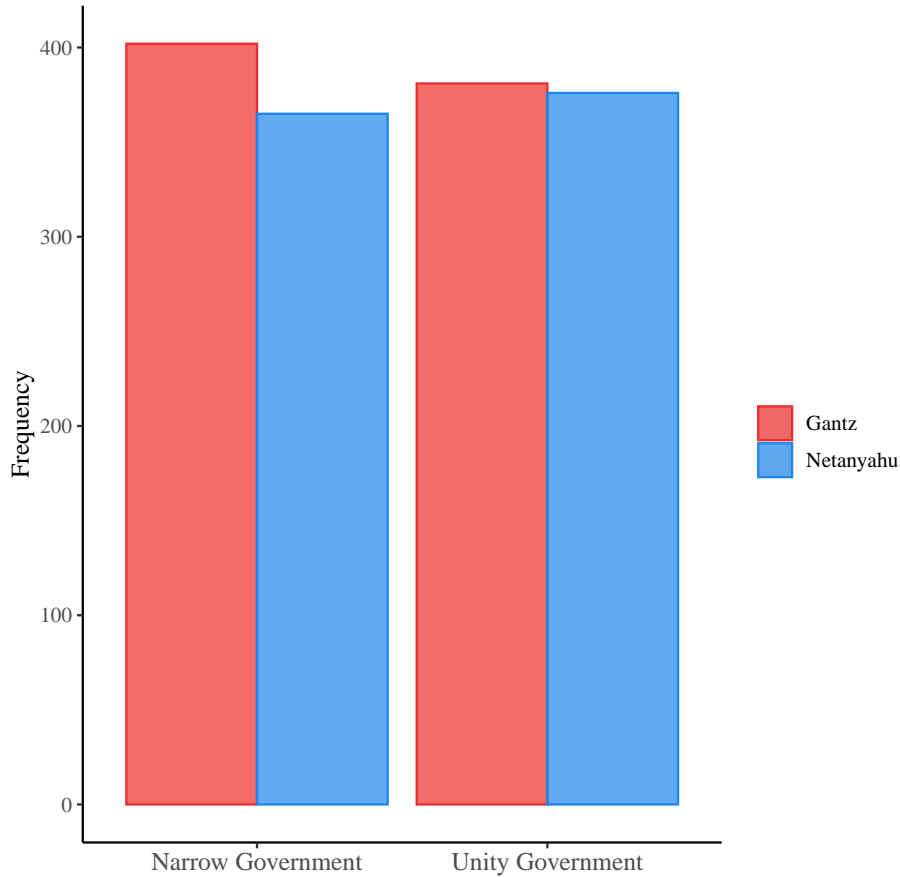
Figure A4: Distribution of Respondents Across Treatment Conditions

note that the coefficients in Table A24 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 A5 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

	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 A23: Balance across treatments

Table A24: 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

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.
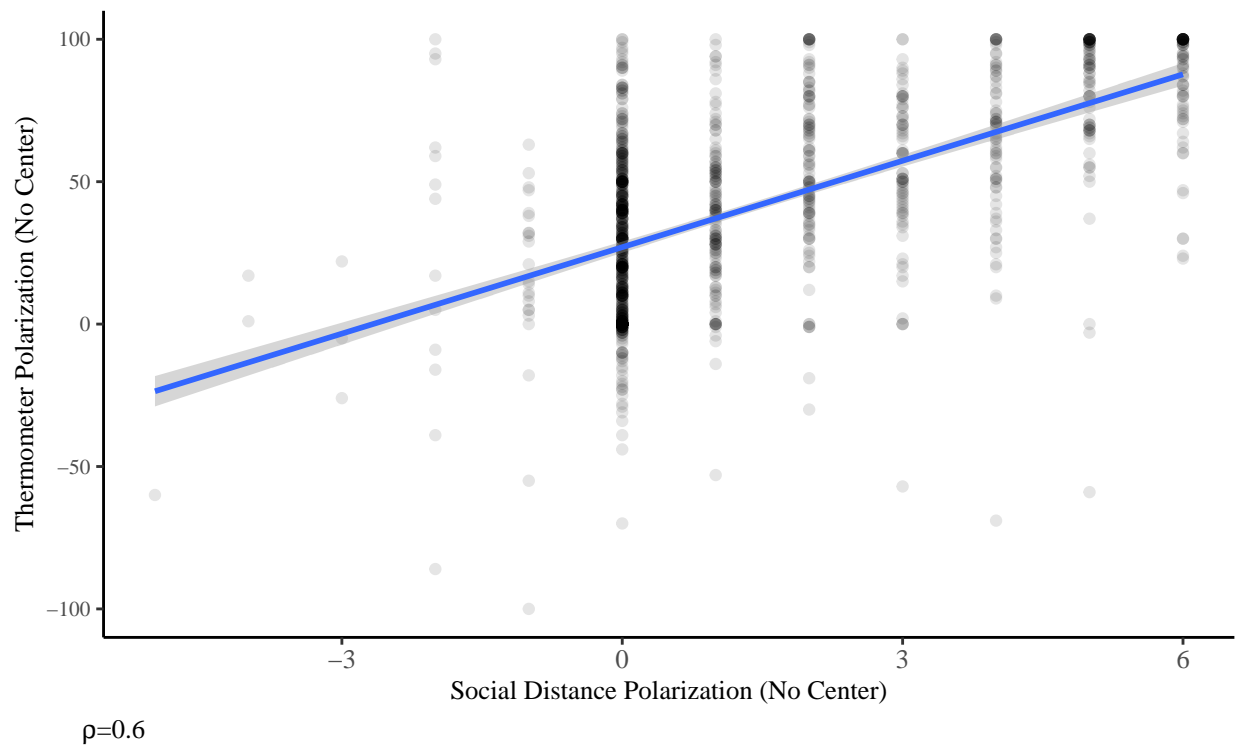
ρ=0.6

Figure A5: **Correlation between Affective Polarization measured in Feeling Thermometer and Social Distance scales**

B.3 Externalities for Minority voters

In Tables A25-A26 we report the effects of our unity government treatment on social distance towards Arabs and Ultra-Orthodox Jews. Generally, we do not identify a general effect of our treatment in these cases. This suggests that information about unity governments has limited externalities with regards to prejudice towards minorities. However, in sub-group analyses, we do find some evidence that when exposed to our treatment, right wing voters become more tolerant towards Ultra-Orthodox Jews, whereas centrist voters become more hostile towards Ultra-Orthodox Jews.

Table A25: Effects of Unity Government on Social Distance towards Arab Citizens of Israel

	Social Distance towards Arabs			
	(1)	(2)	(3)	(4)
Unity	−0.001	−0.181	−0.041	0.007
	(0.104)	(0.143)	(0.107)	(0.115)
Ideological Right		−1.430		
		(0.144)		
Ideological Left			1.362	
			(0.179)	
Ideological Center				0.569
				(0.169)
Unity*Right		0.213		
		(0.192)		
Unity*Left			−0.011	
			(0.247)	
Unity*Center				−0.295
				(0.230)
Demographic Controls	No	Yes	Yes	Yes
N	1,484	1,484	1,484	1,484

We did not develop clear expectations as part of our pre-analysis plan with regards to sub-group effects and attitudes towards Ultra-Orthodox Jews. More so, we encourage readers to interpret models 2-4 in Table A26 with a grain of salt, as they are observational in nature (i.e. ideology is not randomly assigned), and may be misleading due to limited degrees of statistical power (as identifying interactive effects requires larger sample sizes). However, with this caveat in mind,

we believe that our finding with regards to centrist voters is not surprising, given center-party campaigns in Israel which have criticized Ultra-Orthodox parties and voters for their limited integration in Israeli society. That said, our finding with regards to right-wing voters is indeed surprising, and we suggest that future research should provide a more exhaustive exploration of whether, why, and under what conditions, unity governments shape attitudes towards minorities across different segments of the population.

Table A26: Effects of Unity Government on Social Distance towards Ultra-Orthodox Jews

| | Social Distance towards Ultra Orthodox | | | |
	(1)	(2)	(3)	(4)
Unity	−0.044	−0.219	0.007	0.101
	(0.101)	(0.146)	(0.108)	(0.112)
Ideological Right		−0.288		
		(0.147)		
Ideological Left			0.286	
			(0.180)	
Ideological Center				0.136
				(0.164)
Unity*Right		0.377		
		(0.196)		
Unity*Left			−0.064	
			(0.249)	
Unity*Center				−0.431
				(0.224)
Demographic Controls	No	Yes	Yes	Yes
N	1,484	1,484	1,484	1,484

B.4   Robustness Checks

In this section, we present figures depicting the robustness checks described in our main text. In Figure A6 we consider the effect of our treatment on six alternative measures of affective polarization, measured in terms of the 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 A6 considers the distance between affect towards the two extreme parties (Yamina and Democratic Union). The fifth (INES measure) in Figure A6 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 A6, solely considers voters of the Likud and Blue-White and their attitudes to the competing party. In Figure A7 we consider the same outcomes, employing thermometers rather than social distance items.

As evident in Figure A6, 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.[32] 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 A7, the effect of our treatment on the alternative indicators measured in terms of feeling thermometers largely resemble our findings presented in Table A24. Thus the coefficients signs of our treatment are negative, but our findings are statistically insignificant for most outcome measures.

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[32]Indeed, the number of observations drops significantly when employing a two party measure $n = 878$.
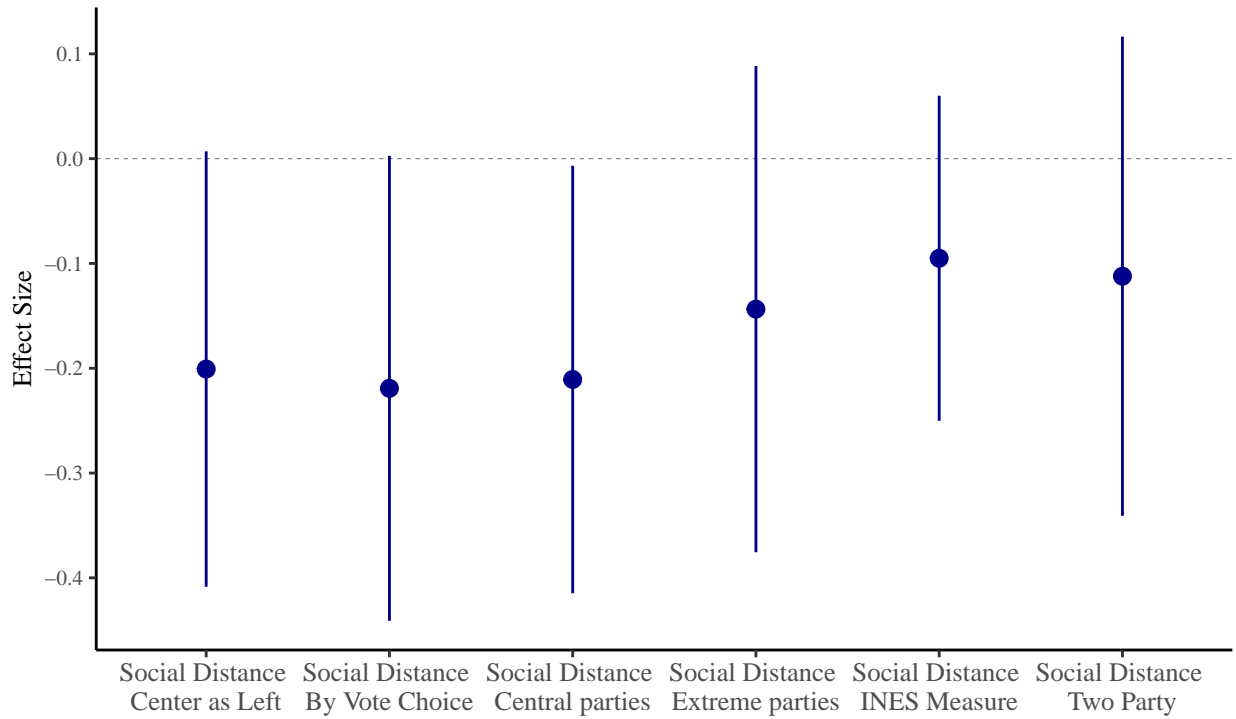
Figure A6: Robustness Check - Unity Government Effect on Polarization (Social Distance)
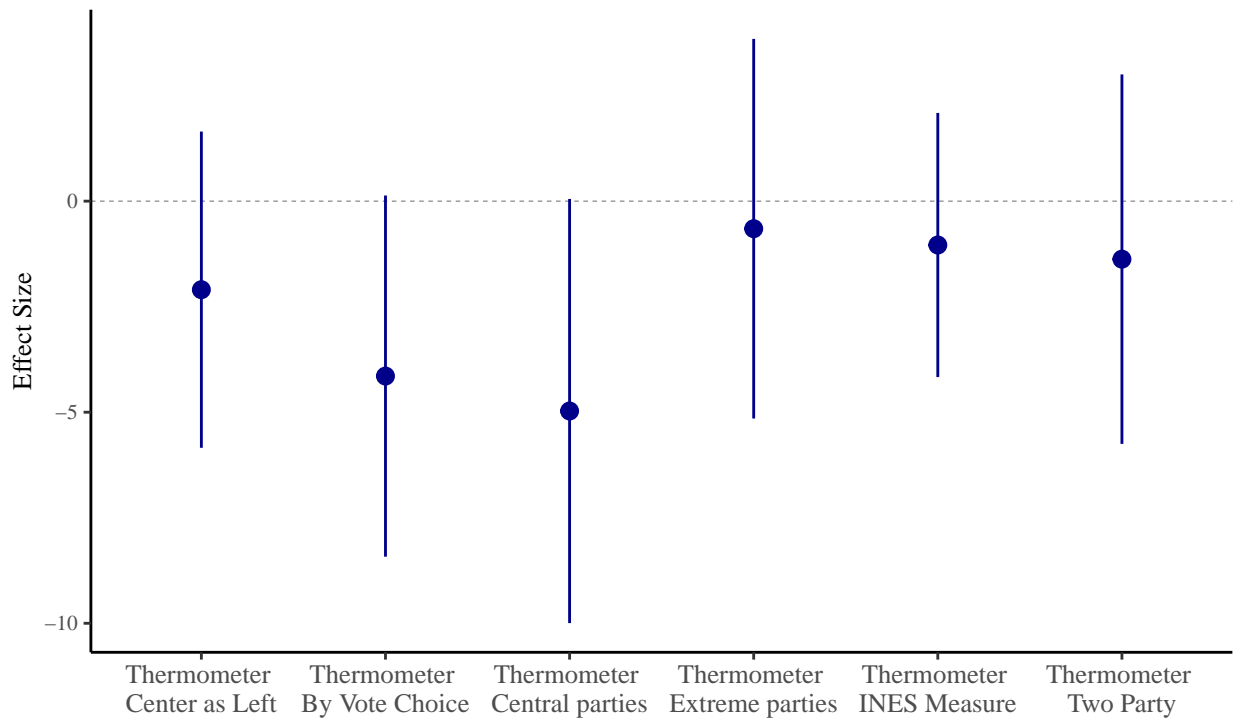

Figure A7: Robustness Check - Unity Government Effect on Polarization (Thermometer)

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 A27, 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.

Table A27: 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

B.5 Heterogenous Treatment Effects

In Table A28 we consider the heterogenous effect of our treatment, conditional on ideology and self-reported vote choice using our main polarization outcome. We do not find evidence that right- or left-wing voters (measured by responses to our ideology scale, or by self-reported vote choice) respond differently to our treatment. We also consider the extent to which the prime-minister leading the unity government (i.e. our second treatment arm), moderates the average treatment effect of information regarding unity government formation (Table A29). Results from this table, and specifically the statistically insignificant interaction term `Unity*Netanyahu`, suggest that our prime-minister identity treatment does not moderate the effects of inter-party cooperation on polarization.

This latter result may be driven by a theoretical reason, as well as by two design factors. Theoretically, as our framework suggests unity governments depolarize voters regardless of the politicians leading them, precisely because unity governments emphasize cooperation across party

lines rather than specific polarizing politicians. Empirically, it is important to remember that our prime-minister treatment is rather weak. Indeed, we demonstrate in Table A30 that the Prime-Minister treatment itself does not affect polarization. Therefore, given the weak treatment, and the increased power required to identify an interaction effect relative to an average treatment effect, our additional analyses may not be well powered to identify the moderating effects Unity Government*Netanyahu, an effect which is not of primary concern to our main theoretical argument.

Table A28: Effects of Unity Government on Polarization (Social Distance) Conditional on Self-reported vote or Ideology

	Polarization (no center)			
	(1)	(2)	(3)	(4)
Unity	−0.108	−0.230	−0.086	−0.241
	(0.143)	(0.139)	(0.238)	(0.138)
Likud	1.040			
	(0.185)			
Blue-White		−0.786		
		(0.199)		
Ideological Right			1.107	
			(0.204)	
Ideological Left				−1.107
				(0.204)
Unity*Likud	−0.208			
	(0.260)			
Unity*Blue-White		0.092		
		(0.276)		
Unity*Right			−0.155	
			(0.276)	
Unity*Left				0.155
				(0.276)
Demographic Controls	Yes	Yes	Yes	Yes
N	1,110	1,110	1,110	1,110

Table A29: Effects of Unity Government on Polarization (Social Distance) Conditional on PM Treatment

	Polarization (no center)	
	(1)	(2)
Unity	−0.147	−0.078
	(0.174)	(0.170)
Netanyahu	−0.055	−0.031
	(0.174)	(0.171)
Unity*Netanyahu	−0.185	−0.220
	(0.248)	(0.243)
Demographic Controls	No	Yes
N	1,110	1,110

Table A30: Effects of Netanyahu Treatment on Polarization (Social Distance)

	Polarization			
	(1)	(2)	(3)	(4)
PM	−0.153	−0.113	−0.173	−0.007
	(0.124)	(0.116)	(0.137)	(0.235)
Left			−0.719	
			(0.226)	
Right				0.719
				(0.226)
PM*Left			0.166	
			(0.273)	
PM*Right				−0.166
				(0.273)
Demographic Controls	No	Yes	Yes	Yes
Center Voters	No	No	No	No
N	1,110	1,110	1,110	1,110