Essays on Political Socialization and Polarization

A dissertation presented

by

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to

The Department of Government

in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy
in the subject of
Political Science

Harvard University
Cambridge, Massachusetts

May 2019
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This dissertation studies the long-term consequences of political socialization. In the first paper, I study socialization under different regime types and its implications for democratic consolidation. Using public opinion data from transitioning democracies in Sub-Saharan Africa I show that democracy may not be self-enforcing: Individuals socialized under democracy voice less support for democratic rule than individuals socialized under autocracy. In the second paper, my co-author and I analyze habituation effects through socialization among newly enfranchised 16-year-old voters in Austria. We suggest that lowering the voting age has benefits for long-term rates of political participation, but may also reinforce polarization. The third paper conceptualizes political polarization as one form of socialization and demonstrates widespread misperceptions of out-group ideologies in the US electorate. It utilizes and experimental design to test whether an information intervention can mitigate some of the negative consequences of partisan polarization. I find that individuals update their posterior beliefs about out-group ideologies. Such updating lowers negative affect and threat perceptions, but does not impact issue-based polarization.
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Acknowledgments

One of the central ideas of this dissertation is that formative periods can change personal expectations and benchmarks. My time as a PhD student at Harvard’s Department of Government has certainly been such a formative experience. My academic trajectory has been far from linear, and I am deeply grateful for the friends, family members, mentors and peers who helped me find my own path.

My mother Elfriede is the most inspiring source of moral and political socialization I can imagine. Her devotion to public service and social progress has always made me feel anchored. She and her partner Hartmut give me a sense of home wherever I am. My sister Judith and I are proof that the same socialization environment can produce two individuals who are polar opposites and highly similar at the same time. I am so grateful for her emotional support and cannot wait to see the fruits of her own academic work in medicine. My father Günther has always allowed me to explore my own path and I am grateful for this freedom.

My wife Kalila is the unconditionally supportive and compassionate partner who I did not know existed before I met her. This dissertation would not exist without her emotional and intellectual contributions. Thank you for grounding me and helping me reflect on what makes a good life.

Every research project of this scale needs fans and critics. I am extremely fortunate to have smart and compassionate friends who played both roles. Christof Brandtner, Laura Bronner, Nicol Gruber, Roswitha Harner, Hanno Hilbig, Miriam Macke, Ana Stefaner, Christoph Steininger, and Rainer Widman supported me when I doubted
myself and challenged my arguments when they needed improvement. More recently, I have become part of the wonderful residential community at Harvard’s Mather House. This dissertation owes much inspiration to hours of conversations with fellow resident tutors, students and faculty members. Mather is a good house.

I also want to thank my dissertation advisors Torben Iversen, Steve Levitsky and Gwyneth McClendon for always supporting my work and career. They taught me how to craft social science that is rigorous and relevant, and I hope that I (partially) managed to live up to this ideal. I also received invaluable guidance from Robert Bates, Ryan Enos, Teppei Yamamoto, and Daniel Ziblatt. A special thanks to Anoop Sadanandan and Abbey Steele whose mentorship made me a PhD student in the first place.

At its core, most research is collaborative and builds on what we learn from our peers. My fellow graduate students at the Department of Government are the most collaborative network I have ever been in. This includes my cohort members Jared Abbott, Alla Baranovsky-Dewey, Elizabeth Davis, Coleen Driscoll, Gabriel Koehler-Derrick, Connor Jerzak, Shelley Liu, Aseem Mahajan, Michael Morse, Matthew Reichert, Aytug Sasmaz, Austin Strange and Alice Xu, and many others at Harvard and beyond who have provided me with invaluable feedback, including Jacob Brown, Christopher Celaya, Daniel de Kadt, Drew Dimmery, Soeren Henn, Nicholas Intscher, Dominika Kruszewska, Shiro Kuriwaki, Rachael McLellan, John Marshall, Stephen Pettigrew, David Romney, Markus Wagner, Weihuang Wang, and Michael Zoorob.
Introduction

At its core, political socialization theory argues that there is a, potentially lagged, relationship between temporal context and individual political behavior. The early circumstances of individuals, such as institutions, networks and families, are seen as important predictors of political outcomes later in life, whether those are in the realm of electoral participation, civic values or ideological beliefs (Stoker and Bass 2011). This paradigm was most prominent in political science research during the 1960s and 1970s, when it was seen as a “growth stock” (Greenstein 1970). Due to a lack of robust findings, the political socialization literature then experienced a decline. However, its central ideas were gradually integrated into other research agendas, such as studies on partisan identification, electoral habituation, historical legacies of institutions, and even rational choice theories of voting.\footnote{Achen (2002) provides a rational choice justification for socialization effects. In his framework, all voters use a “running tally” to evaluate the performance of elected officials through Bayesian updating. However, young voters begin this evaluation with a blank slate. To begin the Bayesian updating process, they need to derive sensible priors from an external source. Given that their parents are often similar to them in terms of socioeconomic status, ideology, and racial/ethnic identity, it is rational for them to copy their priors. This transfer of priors is an example of a weaker form of political socialization that is postulated to fade out as individuals grow older and develop their own priors (see “life-cycle” view.)} Hyman (1959) was among the first authors to coin the term and introduce the concept of political socialization. The American Voter was another foundational building block of the political socialization literature. Campbell et al. (1960) conceptualized partisan identity as a function of both family socialization and life-cycle effects, i.e. systematic changes in partisan identification as a consequence of aging. This distinction between life-time persistence and life-cycle fluctuation points to a crucial heterogeneity in research published under the banner
of political socialization. Essentially, all socialization research argues that temporal context matters for individual behavior, but there is disagreement about the degree to which this influence is lagged.

Wasburn and Adkins Covert (2017) provide a useful framework to conceptualize this heterogeneity in socialization research. They group extant research into four categories based on the stickiness of preferences and the length of lag between temporal context and individual behavior. The persistence and life-time openness perspective represent the extremes of strong lag and no lag, while the impressionable-years and life-cycle views fall in between.

1. **Persistence.** Foundations of political behavior are shaped early by agents such as families and schools, potentially as early as pre-adolescence (van Deth, Abendschön, and Vollmar 2010) or adolescence. Beliefs and preferences underlying political behavior are seen as sticky over one’s lifetime. There is a strong lag, potentially several decades long, between temporal context and individual behavior.

2. **Life-time openness.** Foundations of political behavior stay highly malleable over the course of one’s life. Beliefs and preferences are subject to change, and vary dramatically across different individuals, depending on the specific circumstances they face. There is no lag between historical context and behavior.

3. **Impressionable years.** Beliefs and preferences are subject to ongoing change, but there is one crucial life period that leaves significant traces in the foundations of political behavior. Most authors consider young adulthood (ca. 16-30) as the period of “impressionable” or “formative years”, as individuals are incorporated into the political system through formal participation (such as voting) for the first time. The strength of the lag depends on the age of individuals: The greater the difference between one’s current age and the impressionable
years, the stronger the lag. Proponents of this view argue that socialization research has reached an impasse because it has tried to extend socialization periods to pre-adulthood and later life-stages. Niemi and Hepburn (1995) argue that socialization research should discard these other cohorts and focus on the impressionable years as the main source of socialization.

4. **Life-cycle.** Foundations of political behavior are constantly shaped by the circumstances individuals face during certain periods of their life. Thus, changes in income, unemployment or family circumstances affect individual behavior, with a small degree of lag. In contrast to the life-time openness view, there are generalizable patterns across individuals and the lag is small, but not zero. For example, a life-cycle view would argue that political ideologies become more conservatives as individuals age, and this hypothesis may hold true for many individuals. A life-time openness view would not make such general predictions across individuals based on age alone.

This dissertation is most embedded in the *impressionable years* paradigm. This view informs the coding decision in the first paper, and provides the motivation to study the behavior of newly enfranchised young voters in Austria. However, it is easily conceivable that the life-cycle view and the impressionable years view can coexist and each explain a significant share of political behavior.

**Agents of socialization**

Another way to disaggregate political socialization theory is based on the origins of socialization effects. The “agents” of socialization (Renshon 1977) that prior research has focused on include schools, family, civic associations, religious organizations and racial/ethnic identities. Humphries, Muller, and Schiller (2013) suggest that certain agents of socialization may be more influential for some subgroups than others. For example, schools may be a more important source of socialization for
young US immigrants, as classmates and teachers provide important cues for assimilation. Furthermore, variation in characteristics within agents can be used to predict socialization outcomes. For example, if informational cues are less noisy and more uniform, socialization effects may be stronger. In empirical studies, effects sizes are often largest for the most politicized agents, e.g. pairs of parents who are very interested in politics and share the same ideological views or partisan identity (Jennings, Stoker, and Bowers 2009; Sears and Brown 2013; Iyengar, Konitzer, and Tedin 2018).

Mechanisms of socialization

Furthermore, political socialization theory varies depending on the mechanisms highlighted by researchers. For example, the fundamental work of Langton and Jennings (1968) is an inquiry into the causal effects of US high school civics courses, which represents a formal dimension of schooling. However, high school can also be conceptualized as a bundle of more informal mechanisms, such as the networks of friends and teachers individuals form through classes and extracurriculars. Influential network members may have lasting impacts on the normative benchmarks of peers, even in the absence of formal enforcement mechanisms. Thus, the same agent (schools) can cause socialization effects through a variety of formal and informal mechanisms. Beck and Jennings (1982) provide a useful framework to systematize such competing mechanisms. They distinguish between three parental “pathways” of socialization (parent socioeconomic status, political activity, civic orientation) and one school-based channel (student involvement in civic activities in high school). I build on their framework and create an extended list of mechanisms that instead focuses on the different cognitive processes associated with socialization. This conceptualization allows us to see the commonalities between different agents of socialization, such as the impact of hierarchies in schools and families.

1. Information transmission. Agents of socialization, such as family and other
network members, may be relatively more informed about politics than recipients. When they transmit information to younger individuals, socialization effects ensue. The type and intensity of transmitted knowledge depends on the networks and institutions that individuals are embedded in, such as school types, political regimes and family models. The agents of transformation can range from formal (e.g. civics courses) to less formal (family hierarchies) to informal (networking effects through peers).

2. **Norm transmission.** Agents of socialization may hold internalized norms about certain behaviors that they intend to transfer to younger individuals. Socialization results from interactions between individuals with more authority (teachers, parents) and individuals with less authority (children and other recipients of socialization). Within families, such transmission may take place in every-day interactions (communal meals, conversations) or in the context of regular activities (going to church, museums). Unlike in the first mechanism, the transmission is not value-free. The goal is to shape the normative framework of younger individuals, not only to transmit "neutral" political knowledge (how to vote, how to register as a party member). This can also occur in less hierarchical structures, such as political discussions that teach younger individuals the importance of deliberative norms (Beck and Jennings 1982; McIntosh, Hart, and Youniss 2007). Norm transmission does not have to be explicit, it can also be a result of socialization in a certain historical era or regime type.

3. **Imitation.** Unlike in the transmission mechanism, socialization does not result from the transfer of knowledge or norms. Rather, the behavior displayed by agents of socialization becomes a *habit* for recipients of polarization. For example, parental political participation, such as voting, donating and debating may be directly imitated by younger individuals without instilling fundamental knowledge or values. Imitation may be the result of status-seeking (imitating
role models) or rule-following (if agents of socialization are authority figures). Gigendil and Valaste (2016) provide evidence for the imitation mechanism by combining administrative data on socioeconomic status and voting in Finland. They find that there is an association between parental education and their children’s propensity to vote. According to their estimates, the most important mediator between those two variables is whether parents were regular voters, not their income or education. Thus, imitation seems to trump other socialization mechanisms in their data.

4. Reproduction of inequalities. This mechanism has received the most attention in the sociology literature. Research on this dimension of socialization argues that lower socioeconomic status of parents translates into lower socioeconomic status for children, because they have fewer resources and opportunities available. In turn, lower socioeconomic status is associated with less political participation for various reasons, such as lack of information or self-efficacy. Thus, the observed outcome is similar to the other mechanisms, i.e. rates of political engagement are similar between parents and children. However, this similarity is not due to personal interactions between network members. Rather, it is the result of material circumstances.

Contributions

The dissertation contributes to extant knowledge about political socialization in three ways. First, it studies novel outcome variables. Classic socialization studies and the bulk of recent research focuses on political participation and civil society engagement as dependent variables (Jennings and Niemi 1974; Jennings, Stoker, and Bowers 2009). In my first paper, I show that preferences on regime types can also be partially explained as a consequence of socialization. In my second paper, I use a less common dependent variable (ideological polarization) in addition to a traditional
outcome measure (turnout).

Second, while institutions have been considered as agents of socialization by previous research, they are often situated on a meso-level of society (family, schools). In my first paper, I show that socialization can also originate in macro-level institutions, such as regime types. Specifically, I argue that socialization under democracy creates different normative benchmarks than socialization under autocracy. This approach contributes directly to the growing literature on age-period-cohort modeling that attempts to distinguish the effects of aging and socialization (Neundorf 2010).

Third, I draw theoretical connections between political socialization and two newer research agendas. In the second paper, I sketch the relationship between socialization and electoral habituation. Habituation effects are seen as self-enforcing patterns of behavior (Coppock and Green 2016). Electoral participation is modeled as a function of prior voting. These prior instances of voting can be seen as agents of socialization, as they instill normative or knowledge benchmarks in individuals. Thus, habituation theory is directly related to the impressionable years view of socialization. Turning out to vote during one’s first few eligible elections is strongly predictive of turnout patterns later in life (Stoker and Bass 2011). The third paper connects socialization and polarization theories. As mentioned above, socialization effects are strongest in families where parents are ideologically homogenous. Iyengar, Konitzer, and Tedin (2018) argue that this finding may further entrench demographic sorting in the US electorate and thus amplify partisan polarization.

The structure of the dissertation is as follows. In the first paper, I focus on normative and informational benchmarks established by variation in regime types. The institutions individuals experience during adolescence and early adulthood can affect what individuals view as feasible, and what they view as inconceivable. For example, individuals may learn what the “normal” boundaries of the political game are by observing how a political regime operates when they cast their first votes. These
benchmarks have lasting consequences for citizen preferences regarding democracy. By fitting a hierarchical age-period-cohort model on survey data from transitioning democracies in Sub-Saharan Africa, I find that citizens who were politically socialized under autocracy are more likely to become supporters of democracy later on. Socialization into democracies leads to higher benchmarks regarding political regimes. Somewhat paradoxically, these higher expectations generated a cohort of citizens that is less likely to voice strong support for democracy.

The second paper combines insights from the electoral habituation and socialization literature. In this paper, my co-author Laura Bronner and I conceptualize habituation as an intermediary step towards socialization: Once individuals begin to align with certain parties or candidates, they face cognitive dissonance costs if they do not engage in the same behavior during the next iteration of elections. We provide evidence for this claim using survey data on young voters in Austria, where the voting age was reduced to 16 in 2008. The paper directly contributes to the literature on voting habituation, and extends many of its findings to a new age group, 16-year olds, who received the right to vote in parliamentary elections in 2008. The paper can also be seen as a study of the long-term consequences of the reform, as it shows that habituation effects for newly eligible 16-year-old voters have a non-centrist bias, creating an unexpected cost for the very parties that enfranchised these voters. Thus, the paper also provides useful information for policy-makers and civil society associations that are contemplating voting age reforms.

The third paper connects socialization theory to the burgeoning literature on political polarization. The extent to which citizens are willing to update their priors, i.e. deviate from socialization, can affect the prevalence of confirmation bias and partisan-motivated reasoning, both of which have been linked to growing rates of affective polarization in the US and Western Europe. The paper suggests that changing perceived out-group homogeneity constitutes a promising pathway towards depolar-
ization. Thus, while the first two papers study the consequences of socialization, the third paper asks whether rigid socialized identities can be reshaped by informational interventions.
1 | Children of Democracy? Socialization and Political Regime Preferences

Abstract

Consolidated democracies are often seen as self-perpetuating. Democratic institutions may generate favorable public opinion, which in turn reinforces their underlying legitimacy and effectiveness. However, when countries first transition to democracy, public support for such institutions can be feeble and a lack of pro-democratic beliefs among citizens can derail consolidation efforts. Which individuals are more likely to support newly established democratic institutions in such settings? This article develops a theory of regime preferences based on historical socialization and tests its implications using Hierarchical Age-Period-Cohort (HAPC) models on a survey sample of about 160,000 respondents from Sub-Saharan African transitioning democracies. The results suggest that individuals who experienced their politically formative years under autocracy become more active supporters of democracy. Vice versa, those socialized under democracy are somewhat more likely to voice preferences for autocratic regime types. This finding has important implications for research on democratization. Younger individuals who lack personal memories of autocracy may be among the least likely to resist autocratic backsliding.

1.1 Introduction

Most research on democratization seeks to answer why transition processes and likelihoods vary across countries. Political science in particular often focuses on the causes of regime change and the emergence of hybrid regimes (Huntington 1991;
The effects of such transitions on political preferences and mass beliefs have received relatively less attention (Lindberg 2006; Moehler and Lindberg 2009). However, how individuals rank different regime types is of crucial importance in new democracies. Successful consolidation may require that support for democratic governance manages to disperse throughout society (Rustow 1970; Inglehart and Welzel 2005). One of the reasons for this is the central importance of vertical accountability in transition processes (Diamond 2015). Citizens can monitor newly elected governments more effectively if they are convinced that democracy is preferable to alternatives. As this article will show, it should not be taken for granted that citizens unconditionally prefer democracy in transitioning contexts. As Svolik (2013) argues, citizens may have to “learn to love democracy” in order to make societies less vulnerable to elite manipulation and autocratic backsliding.

How do these preferences form? What explains why some citizens are more active supporters of democracy than others? One way to tackle this question is to analyze generational differences in democratic support. Political cleavages along age groups have long received attention by scholars of comparative politics in all regions, including African politics, which this article is geographically focused on. In a recent example, Frere and Englebert (2015) argue that the growing young population of Burkina Faso was a major catalyst for mass mobilization against Blaise Compaoré’s tenure as a long-time dictator. More generally, the extant literature on democratization provides several reasons why younger individuals might be more likely to embrace democratic institutions.

1.1.1 Age and regime preferences

First, young people personally benefit from democratization, such as through the provision of free public schooling (Stasavage 2005). They may associate public goods
 provision with democratic governance and as a consequence be more vocal in their support of its institutions. Second, establishing democracy as the only viable option among young citizens can be seen as a necessary step towards consolidation. Since younger age groups have never personally experienced other regime types, they may view those alternatives as outside the realm of possibilities (Linz and Stepan 1996). Third, according to the “democratization by elections” paradigm (Lindberg 2006, 2009) democracy sustains itself through positive electoral experiences on the individual level. In a similar vein, Fuchs-Schündeln and Schündeln (2015) argue that democratization functions like capital accumulation. Citizens build trust in democracy through compounding interest derived from positive experiences, which makes them more inclined to defend democracy against alternatives. Through this lens, young citizens are more likely to begin this accumulation process with a clean slate, making it easier for democracy to take hold. Fourth, the belief that democratic change spreads through society by gaining traction among younger age groups is also embedded in classical accounts of modernization theory. Deutsch (1961) argues that younger individuals hold more pro-democratic beliefs as they are more likely to participate in inchoate political channels leading to modernization, such as political parties, labor associations and urban clubs. Fifth, previous research in political economy has investigated the link between development and democracy and argued that there are strong feedback effects between economic growth and democratic consolidation. In this view, democratic political institutions incentivize growth, which in turn helps garner support for democracy (Acemoglu and Robinson 2012; Besley and Persson 2011)

These theories span distinct research traditions, but share the assumption that democracy is at its core self-perpetuating, i.e. that citizens in transition contexts develop positive associations with democracy. These associations in turn arguably generate support for newly established regimes and gradually make democracy “the
only game in town" (Linz and Stepan 1996), particularly among young citizens. In the words of Lipset (1981, p. 29): *Once established, a democratic political system 'gathers momentum' and creates social support [...] to ensure its continued existence.* Scholars of authoritarian regimes have presented similar arguments on the self-perpetuating nature of regimes. Almond and Verba (1963) famously argued that autocracies are less likely to create “civic cultures” and thus lack an important prerequisite for democratization. Mishler and Rose (2007) build on their theory and suggest that authoritarian governments use the administrative apparatus to teach citizens values and behaviors that facilitate the maintenance of autocratic rule.

An implication of the self-perpetuating view of democratization is that, as younger cohorts replace older cohorts in the age distribution of a country, mass support for democracy should increase or at least stay constant. Thus, younger age groups are expected to be more likely to support democratic institutions. If this was not the case, democratic support would decline over time as older cohorts exit the political system. In other words, democracy would not be self-perpetuating at the individual level. How well does this assumption match survey data from Sub-Saharan Africa, one important region for research of democratization?¹ Are younger cohorts indeed more likely to actively support democratic institutions? Figure 1.1 shows the relationship between individual support for democracy and birth year. I group respondents into two age groups: “Younger" if they turned 18 after a country’s first multi-party elections, “Older" if they had their 18th birthday prior to such transitions.² The dependent variable is a binary measure of self-reported support for democracy (for more detail, see Data section). The points represent the estimated country coefficients from a logistic regression of democratic preference on a binary indicator that indi-

¹Source: Afrobarometer Rounds 1-6.

²The results are robust to using 1990 as a common cutoff for all countries. The advantage of country-specific cutoffs is that it provides a more accurate measure of the context individuals experienced in their formative years.
cates whether a respondent was 18 or older when the country transition to (partial) democracy. The vertical bars are 90 and 95 percent confidence intervals. Education level and food security as a proxy for socioeconomic status are included as control variables.

Figure 1.1: Differences in support for democracy between pre- and post-transition cohorts (Logit regression of support for democracy on younger generation indicator.)

The results cast doubt on the self-perpetuating view of democratization. All coefficients in Figure 1.1 are negative or statistically indistinguishable from zero. Across all 22 countries in the sample, being a “child of democracy” is associated with weaker support for democratic institutions. Why is this the case? What explains why younger cohorts are less likely to declare unconditional preferences for democracy? The remainder of this article seeks to answer these questions.
1.2 Age, cohort and period effects

Previous studies have empirically investigated the relationship between age and democratic support, and most have suggested a positive relationship (Anderson and Guillory 1997; Mattes and Bratton 2007; Cho 2014). By contrast, Foa and Mounk (2017) argue that the relationship has recently changed in advanced democracies. Using public opinion data from several regions, they demonstrate that younger groups have a notable preference for some aspects of authoritarian rule, particularly strong-man politics and the power of the military. What explains these conflicting findings? Under what conditions should age be positively or negatively associated with pro-democratic preferences? A useful conceptual tool is to distinguish between three distinct subcomponents: aging, period and socialization effects.

Imagine two individuals, born in 1960 and 1980 respectively. In 2000 we ask them: “Do you prefer democracy over other forms of government?” Suppose that their responses to the question differ: One individual prefers democracy more strongly than the other. This variation could be due to three different reasons. First, it could indeed be a consequence of aging. Respondents at age 20 and 40 likely differ in terms of their life-cycle on income, education and other factors that explain political preferences (age effect). Second, there may be a politically relevant event in 2000, such as an economic or environmental crisis, that affects the two age groups differently (period effect). Third, the respondents faced different socialization contexts, and those may have left traces in their political beliefs. Political contexts can change dramatically from generation to generation. For example, the older respondent (1960) may have experienced formative events under autocracy, and the younger respondent (1980) under democracy. I will refer to this last component as socialization or cohort effects.

Some previous research on democratic consolidation does not distinguish between these components. For example, the relationship described in Foa and Mounk (2017)
could be attributed to aging (younger people prefer autocracy due to their age, but this may change as they grow older) or socialization (younger people prefer autocracy due to the specific environment they experienced growing up, and their preferences persist as they grow older). To provide another example, the self-perpetuating effect of democracies Fuchs-Schündeln and Schündeln (2015) attribute to the amount of time spent under democracy in their model is indistinguishable from socialization under autocracy in a country that recently went through a transition. By definition, those socialized under autocracy are older and on average have also spent more time under democracy after a transition has occurred. This study aims to distinguish more carefully between these distinct components by focusing on democratic transition in Sub-Saharan Africa. I will refer to cohorts socialized after a democratic transition has occurred as children of democracy. The main aim of the article will be to answer the following question: Are children of democracy more or less likely to support democracy? Can the relationship between age and democratic preferences shown in Figure 1.1 be attributed to socialization and what are the agents of socialization? The following section synthesizes prior research on socialization and develops a theory of generational differences in democratic beliefs in the context of Sub-Saharan Africa’s transitions from single-party to multi-party democracy in the late 1980s and early 1990s.

1.3 Theories of political socialization

Recent research on political behavior has made progress on distinguishing the effects of aging, periods and socialization (Dinas 2013b; Chauvel and Schröder 2014; Dinas and Stoker 2014; Ghitza and Gelman 2014; Grasso 2014; Smets and Neundorf 2014; Tilley and Evans 2014; Borjas 2015). However, the small subset of these studies that focuses on regime preferences is geographically limited to the former Soviet Union and Eastern Europe. The authors of these studies argue that socialization under
autocracy is associated with anti-democratic preferences later in life, as Communist regimes could rely on the strength of the administrative apparatus and the unique opportunity structure of the Cold war to indoctrinate citizens. (Mishler and Rose 2007; Neundorf 2010; Pop-Eleches and Tucker 2011, 2014). In this section, I will argue that these theories may not generalize to conditions of lower administrative capacity and different geopolitical circumstances which did not create the same rift between Western democracy and Soviet communism. In particular, I will consider the conditions faced by Sub-Saharan Africa’s transitioning democracies.

Neundorf (2010) suggests that “younger citizens in East Europe perceive democracy more positively than older people. Younger people were not influenced by any kind of Cold War rhetoric and, hence, are more likely to share the democratic principles of Western democracies.” In the bipolar geopolitical environment at the time, the permanent security threat of Cold War politics arguably allowed communist regimes to create strong associations in the minds of citizens between Western adversaries and their typically democratic regime types. While many regions outside of Eastern Europe and Central Asia were pulled into the Cold War through proxy conflicts and the strategic use of development assistance, the indoctrination effect may be less pronounced there. In particular, African politics were somewhat less likely to be affected by Cold War rhetoric. Political commitments to African socialism were mostly nominal and did not affect social and economic structures to the same extent as in Communist regimes. African politicians acted as shrewd political negotiators and received political, economic and military assistance from Western and non-Western states. Thus, the line between Western and Soviet models of politics was more blurry. Due to these different geopolitical conditions, citizens in these settings may have been less likely to dichotomize the West as democratic and the East as non-democratic. This was reinforced by the fact that many African countries were colonized by Western states at the time or had recently liberated themselves from colonial rule. Thus,
rhetorical associations between the West and democracy were anything but clear.

Mishler and Rose (2007) present a somewhat more culturalist version of this argument. They suggest that citizens who are socialized in autocratic regimes face a salient culture that promotes non-democratic beliefs. However, as I will argue below, citizens socialized under authoritarian rule may very well have an “advantage” in evaluating the advantages of democracy. Mishler and Rose’s theory applies to settings where regimes hold a firm grip on public education, media and civil society associations. Unlike in the Soviet Union, state control over the economy was more limited in Africa’s authoritarian regimes (Bates 2014), both before and after the major wave of democratic transitions in the 1990s (Schraeder 1995). Regimes usually had less coercive capacity and were not as deeply entrenched in social life as Communist regimes in Eurasia (Bratton and van de Walle 1997; Smith 2005). Sub Saharan African single-party regimes and weak-state autocracies in other parts of the world were and are unlikely to shift public opinion on regime preferences through official channels at the same magnitude and depth as totalitarian/Communist regimes did.

Pop-Eleches and Tucker (2014) are more agnostic on whether autocratic cohorts should be more or less likely to hold pro-democratic beliefs after socialization. They explicitly mention both directions as possibilities. However, their empirical results are all but unanimous. Across the 14 communist successor states in their study, cohorts socialized under autocracy are generally less likely to state preferences for democracy after transition. Their theory does offer an alternative prediction, which they label the resistance hypothesis: “Alternatively, perhaps more exposure to communism actually leads to more resistance to the ideas of communism, precisely because life under communism could be so brutal and repressive.” This idea is an important building block of the benchmark-based theory of socialization I develop in the next section. Furthermore, Pop-Eleches and Tucker (2014)’s results are highest for formerly totalitarian (Stalinist) regimes, which is an important nuance. Are socialization effects
different in non-totalitarian autocracies? How do we expect socialization effects to influence regime preferences in autocracies with less coercive power?

To summarize, there are three main reasons why previous theories of socialization and regime preferences may not generalize to non-Communist forms of autocracy. First, communism in the Soviet Union was accompanied by a strongly centralized administrative apparatus that unraveled social and economic structures. Second, geopolitical dynamics during the Cold War drove a wedge between national identification and democratic beliefs, as Western adversaries were equated with the concept of democracy. Third, there is an important empirical consideration that may restrict the scope conditions of previous results. After the end of the Cold War political and economic transitions occurred in unison in the Soviet Union and Eastern Europe. Thus, the effects of political and economic institutional change are observationally confounded, creating a challenge for the identification of socialization under particular regime types. This article advances on previous studies both theoretically and empirically. First, it expands the scope of socialization theories to autocracies with weaker states and develops a new theory based on benchmarks set by personal experiences with autocracy. Second, it tests this theory against a survey sample drawing from Sub-Saharan African countries that transitioned to multi-party democracy in the late 1980s and early 1990s.

1.4 A benchmark-based theory of socialization under autocracy

In the absence of state indoctrination, what explains the expectations individuals form when experiencing political socialization under autocracy? To build a theory of expectation formation, I draw on studies of political socialization. One of the central findings in this research tradition is that an individual’s first few elections
have lasting impacts on their future political behavior (Jennings and Niemi 1974; Jennings 2007; Neundorf 2010; Ghitza and Gelman 2014). I will refer to the period in which individuals begin to participate in political decision-making through elections as their *politically formative years* (Visser and Krosnick 1998; Dinas 2013b; Smets and Neundorf 2014).

I argue that children of democracy - individuals who begin participating in elections after regime change - are less likely to actively support democracy than preceding generations who were socialized under autocracy. This is because individuals develop views on newly established democratic institutions based on the extent to which their expectations are fulfilled (Levi, Sacks, and Tyler 2009). Put succinctly, if expectations are higher, they are harder to meet. I expect pre-transition cohorts to have lower expectations in democratic institutions, and post-transition cohorts to have higher expectations. In the context of democratic transitions in Sub-Saharan Africa, individuals may experience their formative years under autocracy, such as single-party rule or personal dictatorship, or under (partial) democracy. Most African polities experienced a transition from full autocracy to more competitive regime types in the 1990s (Bratton and van de Walle 1997), but restricted elections were usually held before then. I posit that individuals view their formative elections differently based on whether they were held under autocracy or (partial) democracy, as distinguished by the degree of competition between parties and candidates. While democracy is a multi-dimensional concept, electoral competition is the most crucial dimension for this theory. There are two mechanisms that explain why autocratic formative years create pro-democratic preferences in the long run: Learning about the competitiveness of elections, and personal experiences with the costs of repression in autocratic regimes.

First, baseline expectations in democratic politics are lower if benchmarks are formed under autocratic institutions, so fulfilling them becomes relatively more at-
tangible. Individuals with pre-transition formative years learn first-hand that elections have relatively little consequence by participating in virtually uncontested elections. This socialization can take place either by voting in uncompetitive elections, or indirectly by learning from peers who do. During pre-transition formative years, individuals experience a lack of competition and major opposition harassment. Thus, cohorts that begin their political life under authoritarianism tend to expect very little from elections as instruments for decision-making. Crucially, such expectations are sticky and affect their later views on elections, even after a country has transitioned to a more open regime type. If expectations in elections are low, citizens are more sensitive to incremental improvements in electoral competition over time. In other words, citizens with lower expectations in democracy are harder to disappoint. Conversely, this relationship implies that younger citizens are expected to show weaker support for democracy because their baseline expectations in elections are higher. They experience their formative years under more open regime types and, other things equal, develop expectations that are more likely to be disappointed by political realities.

Second, personal experience with autocracy leads to a greater awareness of the costs of political expression in such settings. Older cohorts personally experienced the costs of political dissent in the context of authoritarianism. During their formative years they may have been subject to harassment by public officials, or have friends...
or relatives who were targeted by the regime. I argue that this personal experience with life under authoritarian rule makes them more likely to rule out non-democratic governance categorically. When they evaluate different regime types, their unique personal experience leads them to discount non-democratic options as a viable alternative. In other words, these individuals develop sticky “never again” attitudes towards autocracy, which is similar to the resistance hypothesis in Pop-Eleches and Tucker (2014). Younger cohorts lack this type of direct experience and may be more likely to have skewed views about political life under authoritarianism. This makes them more likely to prefer alternatives and, as a consequence, less likely to support democracy.

Figure 1.2 provides a summary of the theory. While the first path (lower expectations in elections) is primarily about benchmark expectations for the benefits of democracy, the second path (personal experience with repression) relates to benchmark expectations for the costs of alternatives to democracy. Both mechanisms yield the same empirical prediction: Older cohorts who experienced their politically formative years before democratic transitions are more likely to support democracy. Somewhat paradoxically, those whose politically formative years took place after democratic transitions are predicted to develop weaker preferences for democratic institutions.

1.5 Data

To test these theoretical predictions, I focus on transitions from low-competition single-party elections to higher-competition multi-party elections in Sub-Saharan Africa. While there are debates about the true extent of change following Africa’s major wave of democratization in the 1990s (Schraeder 1995), elections indeed became more contested and accessible to opposition actors, which represents one important dimension of democracy (Dahl 1971; Levitsky and Way 2010). While such a procedural defini-
tion cannot account for all dimensions of democracy, it provides a closely matched operationalization of the theoretical framework. The events most crucial for my theory are election-related experiences that form expectations for the costs and benefits of different regime types. Thus, differences between pre- and post transition cohorts in Sub-Saharan Africa provide an ideal context to test the theory. Before elaborating on the methodology, I summarize the dataset and present some descriptive statistics.

Table 1.1: Descriptive statistics on survey data (Source: Afrobarometer) and on first democratic presidential/parliamentary elections (Source: African Elections Database)

<table>
<thead>
<tr>
<th>Country</th>
<th>Respondents</th>
<th>Survey rounds</th>
<th>Cohorts</th>
<th>First multi-party election</th>
</tr>
</thead>
<tbody>
<tr>
<td>Benin</td>
<td>4,776</td>
<td>4</td>
<td>14</td>
<td>1991</td>
</tr>
<tr>
<td>Botswana</td>
<td>7,159</td>
<td>6</td>
<td>14</td>
<td>1965</td>
</tr>
<tr>
<td>Burkina Faso</td>
<td>3,533</td>
<td>3</td>
<td>14</td>
<td>1991</td>
</tr>
<tr>
<td>Cameroon</td>
<td>2,359</td>
<td>2</td>
<td>13</td>
<td>1992</td>
</tr>
<tr>
<td>Cape Verde</td>
<td>6,171</td>
<td>5</td>
<td>14</td>
<td>1991</td>
</tr>
<tr>
<td>Ghana</td>
<td>10,294</td>
<td>6</td>
<td>13</td>
<td>1992</td>
</tr>
<tr>
<td>Guinea</td>
<td>2,393</td>
<td>2</td>
<td>13</td>
<td>1993</td>
</tr>
<tr>
<td>Kenya</td>
<td>9,515</td>
<td>5</td>
<td>13</td>
<td>1992</td>
</tr>
<tr>
<td>Lesotho</td>
<td>7,051</td>
<td>6</td>
<td>13</td>
<td>1993</td>
</tr>
<tr>
<td>Madagascar</td>
<td>5,072</td>
<td>4</td>
<td>13</td>
<td>1992</td>
</tr>
<tr>
<td>Malawi</td>
<td>9,265</td>
<td>6</td>
<td>13</td>
<td>1994</td>
</tr>
<tr>
<td>Mali</td>
<td>8,052</td>
<td>6</td>
<td>13</td>
<td>1992</td>
</tr>
<tr>
<td>Mozambique</td>
<td>8,066</td>
<td>5</td>
<td>13</td>
<td>1994</td>
</tr>
<tr>
<td>Namibia</td>
<td>7,147</td>
<td>6</td>
<td>14</td>
<td>1989</td>
</tr>
<tr>
<td>Nigeria</td>
<td>15,467</td>
<td>6</td>
<td>12</td>
<td>1998</td>
</tr>
<tr>
<td>Senegal</td>
<td>5,927</td>
<td>5</td>
<td>14</td>
<td>1978</td>
</tr>
<tr>
<td>South Africa</td>
<td>14,021</td>
<td>6</td>
<td>13</td>
<td>1994</td>
</tr>
<tr>
<td>Tanzania</td>
<td>10,595</td>
<td>6</td>
<td>13</td>
<td>1995</td>
</tr>
<tr>
<td>Togo</td>
<td>2,391</td>
<td>2</td>
<td>13</td>
<td>1993</td>
</tr>
<tr>
<td>Uganda</td>
<td>14,169</td>
<td>6</td>
<td>12</td>
<td>2001</td>
</tr>
<tr>
<td>Zambia</td>
<td>7,113</td>
<td>6</td>
<td>14</td>
<td>1991</td>
</tr>
<tr>
<td>Zimbabwe</td>
<td>9,291</td>
<td>6</td>
<td>14</td>
<td>1987</td>
</tr>
</tbody>
</table>

I construct an individual-level data set by combining all six rounds of the Afrobarometer survey (Afrobarometer 2017). I exclude North African countries (Algeria, Egypt, Morocco, Tunisia) from the sample because of their different transition dynamics. I also exclude countries that were included in less than two rounds of the
Afrobarometer, as over-time variation is a crucial requirement for the validity of my methodology. Finally, I exclude countries that experienced long periods of civil war or political violence that partially overlap with transitions, and countries that have never held multi-party elections (Burundi, Liberia, Mauritius, Niger, Sierra Leone, Sudan, Swaziland).

To construct the main explanatory variable (pre- and post-transition cohorts), I use a respondent’s birth year to determine the year of their 18th birthday, which corresponds to the legal voting age in all cases, and thus marks the beginning of an individual’s politically formative years. I then determine whether the respondent experienced their formative years before or after a country’s first multi-party election, as coded in the African Elections Database (Nunley 2012). This is done by grouping respondents into 5-year cohorts based on the the distance between their 18th birthday and the transition year. Cohorts with positive values correspond to post-transition cohorts, negative numbers stand for pre-transition socialization. For example, take a country in which a democratic transition took place in 1990. The \([0, 5)\) cohort includes individuals who turned eighteen in 1990, 1991, 1992, 1993, and 1994.\(^3\) Finally, age is de-meaned and standardized in order to improve the computational efficiency of the model fitting algorithm (restricted Maximum Likelihood). In total, there are 169,827 respondents across 22 countries. Table 1.1 provides summary statistics on the number of respondents, survey rounds, cohorts, and the timing of transitions in each country.

The dependent variable is an Afrobarometer question about regime preferences that has been asked consistently since the first round of the survey. Respondents are asked to choose one of the following three statements: “(1) Democracy is preferable to any other kind of government; (2) In some circumstances, a non-democratic government can be preferable; (3) For someone like me, it doesn’t matter what kind of government we have.” I create a binary outcome variable that codes Statement

\(^3\)All results are robust to using 10-year cohort groupings.
1 as Support for democracy and the remaining two statements as Low/No support for democracy. The small number of “Don’t know” responses are also included in the “Low/No support” category. All results are robust to excluding “Don’t know” responses from the analysis.

1.6 Methodology: Identifying cohort effects

To test the theory outlined in section 1.4, I estimate differences in democratic support across formative-years cohorts. The identification of cohort effects poses a challenge for statistical inference. Socialization or cohort effects are very hard to disentangle from aging and period effects. These three factors are often highly correlated or fully dependent in survey data, a challenge referred to by methodologists as the age-period-cohort conundrum (O’Brien 2011; Smets and Neundorf 2014).

As previewed earlier, Figure 1.1 provides preliminary evidence that younger respondents are less likely to report strong support for democratic governance. This correlation could be in line with the proposed theory above, i.e. the age group differences could be the result of common socialization in a certain period. However, other interpretations are possible and observationally equivalent. The same correlation might be due to aging effects rather than socialization. Earlier generations may be more likely to support democracy because older voters are generally more likely to support democracy - independent of the period they were socialized in. This could be due to life-cycle effects, such as changes in income or wealth in the course of an individual’s life. While the difference seems subtle, distinguishing between age and cohort effects is crucial in order to test the validity of political socialization theories.

To tackle this issue, let us begin by imagining an ideal-type model. Such a model would condition on age, birth year, and survey year simultaneously and estimate coefficients for each effect. In a cross-country setting, we would also want to for country-specific and time-invariant unobserved confounding by including country fixed effects.
The following equation operationalizes such a model, where $i$ indicates individuals, $j$ survey years, $k$ countries, and $Y$ is the outcome variable measuring support for democracy:

**Ideal model**

$$Y_{ijk} = \beta_1 \cdot \text{Age}_i + \beta_2 \cdot \text{BirthYear}_i + \beta_3 \cdot \text{SurveyYear}_j + \beta_5 \cdot \text{Country}_k + \epsilon_i$$

In this model, we could single out the effect of socialization by estimating $\beta_2$, the coefficient on $\text{BirthYear}_i$. Since we are controlling for age, this coefficient would reflect the remaining variation that is not due to aging *per se*, but only comprises the result of being born at a certain time. Unfortunately the ideal model is not statistically identifiable. Including age, birth year and survey years as covariates in a regression model creates a singular or perfectly collinear design matrix and the model cannot be fit using conventional regression estimators. Algebraically, once we know an individual's age and survey year, we can also determine their survey year. However, age is an obvious confounder to socialization differences between cohorts, and survey year indicators should be included to control for time-variant unobservable factors. In the next section I describe a commonly used strategy to deal with age-period-cohort collinearity.

1.6.1 **Dealing with age-period-cohort collinearity**

To approximate the ideal model, I proceed in two steps. First, as described in section 1.5, I group respondents into 5-year cohorts according to their politically formative years. Intervals are closed on the right and open on the left. Some of the resulting cohorts have a relatively small number of observations. HAPC models require a high number of observations in each cohort for asymptotic consistency, so I drop cohorts with fewer than 2000 observations, which affects only cohorts born more than 45 years before democratic transitions. Grouping individuals into cohorts breaks
the perfect collinearity problem discussed above, as there is now variation in age within each cohort category. However, there is still a high degree of multicollinearity between age and cohort membership.

To solve this issue, a second step is required. There is growing consensus in the methodological literature (Reither et al. 2015; Smets and Neundorf 2014) that Hierarchical Age-Period-Cohort modeling (HAPC) provides an appropriate tool to disentangle socialization from aging effects. In the language of multilevel modeling, Individuals can be seen as nested into cohorts (here: formative years), periods (here: survey years) and age categories (here: their age when surveyed). By putting a structural assumption (normal distribution) on the cohort variable, collinearity can be reduced. Following this standard, I propose the following mixed effects logit specification, where individual respondents are indexed by $i$, survey years by $j$ and countries by $k$. $\alpha_{\text{cohort}}$ are the cohort-specific random effects, and $\pi_{ijk}$ is the probability of supporting democracy.

**HAPC Model**

\[
\log \left( \frac{\pi_{ijk}}{1 - \pi_{ijk}} \right) = \alpha_{\text{cohort}} + \beta_1 \cdot \text{Age}_i + \beta_2 \cdot \text{SurveyYear}_j + \beta_3 \cdot \text{Country}_k + \epsilon_i \\
\alpha_{\text{cohort}} \sim N(\mu_{\text{cohort}}, \sigma_{\text{cohort}})
\]

HAPC approaches usually model periods (survey years) in random intercepts, I move this coefficient to the main level and model them as fixed effects. This is because including country- and year-fixed effects improves the causal validity of the estimation results. In other words: While holding age, country, and survey year constant, I fit varying intercepts for each cohort. These cohort-varying intercepts ($\alpha_{\text{cohort}}$) are drawn from a normal distribution. This parametric assumption about

\[^{4}\text{To use a different terminology describing the same concept, I am modelling the generational cohorts as random effects or cohort-varying intercepts.}\]
the distribution of cohort intercepts follows the convention used in HAPC studies (Grasso 2014; Ho, Weng, and Clarke 2015; Neundorf 2010; Smets and Neundorf 2014) and is also justified given the underlying data generating process. Each cohort’s support for democracy can be seen as the mean of a random variable in a given sample (cohort). Given a sufficiently large cohort size, we can invoke the central limit theorem, which states that means of random variables converge probabilistically to a normal distribution as the number of observations in each sample (here: cohort) increases. The number of cohorts should be large to yield asymptotically unbiased results (Smets and Neundorf 2014), which is why I use 5-year instead of 10-year cohorts in the main model specification.

An alternative to HAPC modeling would be a design-based causal inference approach. For example, a Regression Discontinuity Design (RDD) could be used in countries with clear cutoffs related to democratic transitions, as demonstrated by de Kadt (2017b) in the case of South Africa. Voters who started voting briefly after regime change could be compared to slightly older individuals. However, there are two reasons why such an approach is not suitable in this case. First, to explore cohort effects, the bandwidth in a regression-discontinuity model would have to be so wide that the necessary assumption of quasi-exogeneity became indefensible. While regression discontinuities are appropriate tools to model local average treatment effects close to arbitrary thresholds, they are less well suited to explain multi-generational differences when there are no clear cutoffs and discrete running variables, such as cohort groups. Second, even in the presence of an exogenous cutoff such an approach would not be able to distinguish between age and cohort effects. A regression discontinuity design may attribute a causal effect to aging, while it may really be driven by socialization, and vice versa. Using a mixed effects model allows us to draw more conclusions about the contextual effect of political socialization.
1.7 Main Results

![Logit coefficients and Predicted probabilities](image)

Figure 1.3: The dependent variable is a binary measure of democratic preferences. The independent variable are binned five-year pre- and post transition cohorts, where the sign denotes whether a cohort was politically socialized before (negative) or after (positive) transition. The model includes country- and survey-year fixed effects. Predicted probabilities of cohort effects are generated by Monte Carlo Simulation (10,000 draws). The vertical bars represent 90 and 95 percent confidence intervals.

In this section, I fit the HAPC Model using maximum likelihood estimation and present the results. The estimated random effect coefficients (Figure 1.3) provide strong support for my theory of autocratic socialization. Later cohorts ([0, 5), [5, 10), etc.) are systematically less likely to support democracy than earlier cohorts ([−20, −15), [−15, 10), etc.) There is a pronounced downward shift in support for democracy among respondents who were socialized into politics under more open regimes. The coefficients for pre-transition cohorts are all larger than the coefficients of post-transition cohorts. Furthermore, the differences between pre- and post-

---

5Using the `glmer` function in the `lme4` package in R.
transition cohorts are statistically significant, as shown by the 90 and 95 percent confidence intervals. The results suggest that political socialization under autocratic rule fosters personal preferences for democratic institutions. Crucially, since the model controls for age, this effect is not due to life-cycle effects and can be attributed to political socialization.

To investigate the magnitude of these effects, I use a Monte Carlo Simulation to generate predicted probabilities. Following a conventional approach to simulate predicted probabilities from logit models (King, Tomz, and Wittenberg 2000), I generate 10,000 draws of the estimated random effect coefficients from a normal distribution using the estimated covariances as parameter values. I then plug the coefficient draws into the mixed effects model specification (HAPC model) and store the mean predicted probability for each cohort-draw. The results in Figure 1.3 align with the theoretical predictions: Post-transition cohorts (children of democracy) are associated with lower probabilities of unconditionally preferring democratic regimes. The difference in point estimates ranges from 71 percent for the earliest pre-transition cohorts to 66 percent among post-transition cohorts. Thus, socialization under autocracy is associated with an up to 5 percentage point increase in democratic preferences in comparison to socialization under democracy.

1.7.1 Individual-level controls

Model 1 does not include individual-level survey variables other than age in order to avoid post-treatment bias. From a causal inference perspective, cohort membership is the treatment and present-day individual covariates are post-treatment variables. This study seeks to identify the effect of common political socialization at a certain time on present-day individual preferences. This causal effect could be mediated in several ways. For example, individuals who experienced their politically formative years in the 1970s may be associated with particular levels of income and wealth
Table 1.2: Estimated results of HAPC model. There are no p-values, as differences from zero are not meaningful for these random effects coefficients. Rather, significant differences between cohorts are shown by the confidence intervals in Figure 1.3

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Pre-transition cohorts</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>[−20, −15)</td>
<td>0.168</td>
<td>0.164</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>[−15, −10)</td>
<td>0.170</td>
<td>0.182</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>[−10, −5)</td>
<td>0.126</td>
<td>0.131</td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
<td>(0.024)</td>
</tr>
<tr>
<td>[−5, 0)</td>
<td>0.137</td>
<td>0.190</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.022)</td>
</tr>
<tr>
<td><strong>Post-transition cohorts</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>[0, 5)</td>
<td>0.048</td>
<td>0.104</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>[5, 10)</td>
<td>0.023</td>
<td>0.035</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>[10, 15)</td>
<td>−0.070</td>
<td>−0.066</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>[15, 20)</td>
<td>−0.079</td>
<td>−0.043</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.024)</td>
</tr>
</tbody>
</table>

Survey Year fixed effects ✓ ✓
Country fixed effects ✓ ✓
Control variables Age Age
                   Education
                   Food Security
Observations 169,827 95,858
today. In turn, these socioeconomic variables could affect their support for democracy. Including individual level controls as measured by Afrobarometer would bias the estimated treatment effect of socialization. The main contribution of this paper is to establish that significant differences in support for democracy exist across cohorts. There are many mediators such differences could operate through, but including them as covariates in the model would induce post-treatment bias.

However, as a robustness check, I also fit a model that includes the level of education and food security as a measure of socioeconomic status. As expected, the effect size of cohort socialization decreases since some of the variation is now masked by the control variables (there is a high correlation between birth cohort membership, economic outcomes, and regime preferences). This correlation is outside the scope of my theory, though compatible. Cohorts socialized under autocracy may have higher socioeconomic status today. Thus, changes in status are one possible downstream pathway through which socialization affects regime preferences. Rather than as an alternative theory of socialization, these control variables should be seen as mediators. The substantive interpretation of the model after including controls is unchanged. As shown in Table 1.2, pre-transition cohorts are still associated with higher probabilities of democratic preferences.

One remaining concern for the causal validity of the model are confounders at the moment of socialization. For example, as established by Mishler and Rose (2007), Neundorf (2010) and Pop-Eleches and Tucker (2014), economic conditions at the moment of socialization matter for later beliefs. In the next section I introduce an alternative model that controls for GDP growth at socialization and also tests the theoretical mechanisms more directly.
1.8 Regime origins of cohort socialization

So far I have shown that there are differences in support for democracy between earlier and later cohorts. However, other than their temporal ordering, I have not yet provided direct evidence that these differences are driven by socialization under certain regime types. In this section I demonstrate a strong and significant relationship between the severity of autocracy at the time of socialization and citizen preferences today. Furthermore, I test the two proposed mechanisms (expectations for benefits, expectations for costs) directly and control for confounders of regime socialization.

My theory posits that the observed differences are due to socialization under different regime types, independent of the economic conditions at the time of socialization. To test this claim, I merge historical Polity IV score (Marshall, Ted Robert, and Jaggers 2018) and GDP growth rates (The World Bank 2018) for each country-cohort combination. Following the modeling strategy in (Pop-Eleches and Tucker 2014) I then fit a logistic regression of democratic preferences on age, Polity scores and GDP growth rates, including quadratic and interaction specifications. For the HAPC model in the previous section, it was necessary to impose structural assumptions on the cohort coefficients due to the perfect correlation between age, birth year and survey year. In this case, the age, Polity, and GDP variables are correlated, but do not impose too much collinearity to calculate coefficients via maximum likelihood estimation. This allows us to fit a conventional logit model that captures relative effects of aging, regime socialization, and economic socialization.

The main downside of this model is that there is little within-country variation between Polity scores and economic growth (they correlate strongly over time for a given country). Thus, to distinguish between the relative importance of regimes and economic conditions as drivers of socialization, cross-country variation has to be exploited to estimate coefficients. In other words, country fixed effects cannot be
modeled. However, to improve the causal validity of the results, I include cohort-level fixed effect as a control variable for cohort-specific region-wide factors such as geopolitical conditions, and survey year fixed effects. In this model, $c$ represents cohorts, $k$ countries, $i$ individuals, $j$ survey years. \( \text{Polity}_{ck} \) is the mean Polity IV score for a given country-cohort combination, \( \text{GDP}_{ck} \) is the average GDP growth for the same country-cohort, and \( X \) is a placeholder vector for control variables. A positive estimated coefficient on the Polity variable would align with the self-perpetuating view of democratization, a negative estimate would provide support for my theory of benchmark-based socialization.

**Exposure model:**

\[
\log \left( \frac{\pi_i}{1 - \pi_i} \right) = \alpha_i + \beta_1 \cdot \text{Age}_i + \beta_2 \cdot \text{Polity}_{ck} + \beta_2 \cdot \text{GDP}_{ck} + \beta \cdot X + \epsilon_i
\]

Table 1.3 provides results of several variations of the Exposure model. Across all specifications, the estimated coefficients on Polity (at socialization) are negative and significantly different from zero. GDP growth (at socialization) is statistically insignificant in all specifications. The predicted probability of preferring democracy is about 71 percent for individuals socialized in a regime setting with the minimum Polity score (-10), 67 percent for the center of the scale (0), and 63 percent for the maximum Polity score (10).\(^6\) These results are comparable in magnitude to the HAPC model results. The predicted probabilities of unconditionally preferring democracy are up to 8 percentage points higher for “children of autocracy”. Thus, the exposure model provides additional evidence for the hypothesized relationship between cohort regime type and long-lasting socialization effects.

Across the five specifications in Table 1.3, I test quadratic and interaction terms to assess the robustness of the main coefficient of interest, i.e. the estimated coeffi-

\(^6\)Using the estimated coefficient values in specification (5).
Table 1.3: Estimated fit of Exposure model.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>0.265</td>
<td>0.226</td>
<td>0.262</td>
<td>0.177</td>
<td>1.871**</td>
</tr>
<tr>
<td></td>
<td>(0.327)</td>
<td>(0.303)</td>
<td>(0.295)</td>
<td>(0.340)</td>
<td>(0.953)</td>
</tr>
<tr>
<td>Polity</td>
<td>−0.011</td>
<td>−0.011*</td>
<td>−0.023***</td>
<td>−0.023**</td>
<td>−0.023***</td>
</tr>
<tr>
<td>(at socialization)</td>
<td>(0.007)</td>
<td>(0.006)</td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>GDP growth</td>
<td>0.007</td>
<td>−0.003</td>
<td>0.005</td>
<td>0.002</td>
<td>0.002</td>
</tr>
<tr>
<td>(at socialization)</td>
<td>(0.016)</td>
<td>(0.016)</td>
<td>(0.017)</td>
<td>(0.017)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Polity²</td>
<td>0.001</td>
<td>0.004</td>
<td>0.001</td>
<td>0.003</td>
<td>0.0003</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>GDP growth²</td>
<td>0.001</td>
<td>−0.0002</td>
<td>0.0002</td>
<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Polity × GDP growth</td>
<td>0.003</td>
<td>0.003</td>
<td>0.001</td>
<td>0.0003</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.003)</td>
<td>(0.003)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.730***</td>
<td>0.731***</td>
<td>0.719***</td>
<td>0.807</td>
<td>0.839***</td>
</tr>
<tr>
<td></td>
<td>(0.095)</td>
<td>(0.105)</td>
<td>(0.098)</td>
<td>(0.223)</td>
<td></td>
</tr>
</tbody>
</table>

Cohort fixed effects ✓ Survey year fixed effects ✓ ✓
Country-Survey year cluster standard errors ✓ ✓ ✓ ✓ ✓
Observations 144,158 144,158 144,158 144,158 144,158

Note: *p<0.1; **p<0.05; ***p<0.01
cient on Polity (at socialization). The final model also includes cohort fixed effects, which captures regional socialization conditions besides economic growth. Furthermore, standard errors in all models are clustered by country and survey year. The results should be seen as an important complement to the HAPC model. The HAPC model fit in the previous section suggests that earlier cohorts are associated with a higher likelihood of democratic regime preferences, while the exposure model in this section finds a relationship between autocratic socialization contexts and democratic regime preferences. In conjunction, the two models provide strong evidence for my benchmark-based theory of regime socialization. The added value of the exposure model is to directly test for economic conditions and other cohort-specific factors (fixed effects) as potential confounders of this causal relationship.

1.8.1 Mechanisms: What sets expectation benchmarks?

My benchmark-based theory of socialization suggests two possible mechanisms explaining the observed relationship. The first mechanism relates to the benchmark expectations individuals have regarding the benefits of elections under democracy. The second mechanism relates to benchmark expectations of the costs of alternatives to democracy, created by personal experiences with violations of civil liberties. So far, I have not empirically distinguished between these two mechanisms.

In this section, I use the subcomponents of the Freedom in the World indicator (Freedom House 2018) to differentiate between the respective mechanisms. Freedom House’s Political Rights score is primarily about the competitiveness and fairness of the electoral process, and the transparency of political decision-making. Thus, it provides a suitable empirical operationalization of the theory’s first mechanism. Conversely, the Civil Liberties score can be used as a measure of the second mechanism, as it directly captures the state of personal freedom of expression and the cost of dissent at the time of socialization. Following the coding of the Polity score in the
previous section, I calculate the mean score for each country-cohort combination and interpret the estimated coefficient on these variables as the effect of the respective mechanism. The model specification is an extension of the Exposure model in the previous section. Table 1.4 presents the results. In line with the Exposure model, minor variations are fit to assess robustness against specification errors. All standard errors are clustered by country and survey year, and quadratic terms on the main variables of interest (FH Political Rights and Civil Liberties) are included to model potential nonlinearity.

Mechanism model

\[
\log \left( \frac{\pi_i}{1 - \pi_i} \right) = \alpha_i + \beta_1 \cdot \text{Age}_i + \beta_2 \cdot \text{Political Rights}_{ck} + \beta_3 \cdot \text{Civil Liberties}_{ck} + \beta \cdot X + \epsilon_i
\]

Table 1.4: Estimated results of Mechanism model.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>0.503</td>
<td>0.744*</td>
<td>2.121**</td>
<td>1.117*</td>
</tr>
<tr>
<td></td>
<td>(1.008)</td>
<td>(0.367)</td>
<td>(1.008)</td>
<td>(0.648)</td>
</tr>
<tr>
<td>FH Political Rights (at socialization)</td>
<td>-0.196</td>
<td>0.168</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.144)</td>
<td>(0.144)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>FH Civil Liberties (at socialization)</td>
<td>-0.321*</td>
<td>-0.561*</td>
<td>-0.218***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.185)</td>
<td>(0.299)</td>
<td>(0.048)</td>
<td></td>
</tr>
<tr>
<td>FH Political Rights^2</td>
<td>0.025*</td>
<td>-0.004</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.015)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>FH Civil Liberties^2</td>
<td>0.036*</td>
<td>0.049</td>
<td>0.024***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.031)</td>
<td>(0.006)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>1.148**</td>
<td>1.478***</td>
<td>1.047**</td>
<td>1.308***</td>
</tr>
<tr>
<td></td>
<td>(0.511)</td>
<td>(0.396)</td>
<td>(0.511)</td>
<td>(0.330)</td>
</tr>
</tbody>
</table>

Country fixed effects ✓
Cohort fixed effects ✓ ✓
Survey year fixed effects ✓ ✓ ✓ ✓
Country-Survey year ✓ ✓ ✓ ✓
Clustered standard errors
Observations 148,574 148,574 148,574 148,574

Note: *p<0.1; **p<0.05; ***p<0.01
There is strong evidence for mechanism 2, and no evidence for mechanism 1. After concluding that mechanism 1 does not fit the observed data well, I drop the Political Rights variable and only include Civil Liberties in column (4). The estimated coefficients on mechanism 2 (FH Civil Liberties) are significant, negative and consistent across specifications. Thus, greater exposure to violations of civil liberties at socialization is associated with a higher likelihood of democratic preferences today. The estimates on mechanism 1 (FH Political Rights) are mostly insignificant and the signs of the coefficients are inconsistent across specifications. Thus, the results provide support for the cost-based mechanism (mechanism 2), but fail to support the benefits-based explanation (mechanism 1). An important caveat of these data is that exposure to the respective mechanisms is measured by cohort membership and not directly for each individual. A richer dataset on individual experience with autocratic regimes would allow us to analyze the mechanisms directly. The difference in treatment effect of autocratic socialization could be compared between individuals who personally experienced violations of civil liberties and those who did not. However, these preliminary results suggest that exposure to repression may be more important than electoral competition when it comes to setting benchmarks for evaluating democracy.

1.8.2 Heterogeneity: Closing the expectations gap

A second way to test the validity of the mechanisms is to exploit cross-country heterogeneity in the effects of socialization. My theory posits that expectations are crucial in the evaluation of democracy and that, if the gap between expectations and reality closes, satisfaction with democracy should rise. Thus, I expect the effect of autocratic regime socialization to be weaker or even non-negative among countries that are performing well in terms of electoral competitiveness and civil liberties.

This hypothesis can be tested by estimating the interaction effect between the
Polity score at socialization and the average Polity score post transition. The interaction term captures the heterogeneity in autocratic socialization between countries where the expectation gap is small (high values of Polity\textsuperscript{post transition}) and countries where the gap is large (low values of Polity\textsuperscript{post transition}).

**Heterogeneity model:**

\[
\log \left( \frac{\pi_i}{1 - \pi_i} \right) = \alpha_i + \beta_1 \cdot \text{Age}_i + \beta_2 \cdot (\text{Polity}_{\text{at socialization}} \times \text{Polity}_{\text{post-transition}}) \\
+ \beta_3 \cdot \text{Polity}_{\text{at socialization}} + \beta_4 \cdot \text{Polity}_{\text{post-transition}} + \beta \cdot \mathbf{X} + \epsilon_i
\]

![Figure 1.4](image)

Figure 1.4

Table 1.5 shows that the interaction effect is significant across various specifications, including quadratic terms and fixed effects. Furthermore, the coefficient has the expected sign (positive). The relative magnitude of the positive relationship between autocratic socialization and democratic preferences is lower for countries and cohorts with relatively high post-transition Polity scores. This heterogeneity is compatible
Table 1.5: Estimated results of Heterogeneity model.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>0.570</td>
<td>0.530**</td>
<td>2.183***</td>
</tr>
<tr>
<td></td>
<td>(0.645)</td>
<td>(0.243)</td>
<td>(0.645)</td>
</tr>
<tr>
<td>Polity (at socialization)</td>
<td>0.002**</td>
<td>0.002*</td>
<td>0.004***</td>
</tr>
<tr>
<td>× Polity (post transition)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Polity</td>
<td>-0.007</td>
<td>-0.007</td>
<td>-0.020***</td>
</tr>
<tr>
<td>(at socialization)</td>
<td>(0.006)</td>
<td>(0.011)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Polity</td>
<td>-0.012</td>
<td>-0.010</td>
<td>0.002</td>
</tr>
<tr>
<td>(post transition)</td>
<td>(0.016)</td>
<td>(0.019)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Polity^2</td>
<td>0.002***</td>
<td>0.002**</td>
<td>0.001</td>
</tr>
<tr>
<td>(at socialization)</td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Polity^2</td>
<td>-0.005</td>
<td>-0.005</td>
<td>-0.006</td>
</tr>
<tr>
<td>(post transition)</td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.786</td>
<td>0.912***</td>
<td>-1.305</td>
</tr>
<tr>
<td></td>
<td>n.a.</td>
<td>(0.045)</td>
<td>n.a.</td>
</tr>
</tbody>
</table>

Cohort fixed effects: ✓
Survey year fixed effects: ✓ ✓
Country-Survey year: ✓ ✓ ✓
Clustered standard errors: ✓ ✓ ✓
Observations: 158,239 158,239 158,239

Note: *p<0.1; **p<0.05; ***p<0.01
with the theoretical mechanisms. In these settings, the gap between expectations and realizations is lower, and “children of democracy” are relatively more likely to actively support democratic institutions. As Figure 1.4 shows, the Heterogeneity model predicts that the socialization effect reverses for settings with a post-transition Polity score between 5 and 10. The red lines represent the main effect that we observed in the previous section - post-transition socialization in these settings is associated with a lower probability of democratic preferences. The blue lines show that in cases where the quality of democracy post-transition is fairly high, post-transition socialization is actually associated with more democratic preferences. This finding holds an important insights. While there seems to be a middle zone in which transitions dampen enthusiasm for newly established institutions, support for democracy can be reactivated if expectations of citizens are actually met.

1.9 Conclusion

What are the implications of these results for the literature on democratic backsliding and regime change? As I have argued above, many theories of democratization implicitly assume that citizens, particularly younger cohorts, prefer democratic regimes over alternatives in transition contexts. I argue that these preferences are not homogeneous and depend on the socialization context individuals experience during their politically formative years. While previous research has pointed out the importance of beliefs for democratic consolidation (Almond and Verba 1963; Lindberg 2009; Moehler and Lindberg 2009; Svolik 2013), this article provides a theory of the origins of such beliefs. The empirical results suggest that individuals who experienced their formative years under autocracy are relatively more likely to support democratic institutions. The article also provides evidence for the relative importance of competing mechanisms. Personal experience with the costs of autocratic repression seems to be more important than perceptions of the competitiveness of elections. For the
specific context of this study, personal experience with autocratic regimes in Sub-Saharan Africa between 1960 and 1990 provides individuals with unique information that makes them more likely to unequivocally support democracy.

![Figure 1.5: The dependent variable is a binary measure of collective action (“raising issues with others”). The independent variable are binned five-year pre- and post transition cohorts, where the sign denotes whether a cohort was politically socialized before (negative) or after (positive) transition. The model includes country- and survey-year fixed effects. Predicted probabilities of cohort effects are generated by Monte Carlo Simulation (10,000 draws). The vertical bars represent 90 and 95 percent confidence intervals.](image)

Political beliefs are important for the success of democratic consolidation insofar as they translate into behavior. To show that democratic preferences morph into political participation, I fit the HAPC model using a different outcome variable - the probability of participating in collective action.\(^7\) As Figure 1.5 shows, earlier generations are more likely to engage in collective action. The variation across cohorts

---

\(^7\)The wording of the survey question is as follows: *Here is a list of actions that people sometimes take as citizens. For each of these, please tell me whether you, personally, have done any of these things during the past year. If not, would you do this if you had the chance:* *Got together with others to raise an issue?* 
*Sometimes/A few times*, *Yes, several times*, *Yes, often* and *Often* are coded as 1, all other answers are coded as 0.
follows the same pattern as the democratic preference outcome variable. This suggests that post-transition cohorts are not only less content with democratic regimes, but also more likely to disengage from formal and informal modes of collective action. This finding echoes previous research on the relationship between authoritarian legacies and political participation. One the one hand, Bernhard and Karakoç (2007) find that socialization under totalitarianism makes individuals less likely to participate in collective action. On the other hand, the same study suggests that the dynamics may be different in less totalitarian non-democratic settings. The sample in Bernhard and Karakoç (2007) mostly consists of European and Latin American cases, so these results generalize the scope of their finding. Taken together, the empirical results regarding regime preferences and collective action suggest that, in order to explain the long-term consequences of autocracy for political beliefs and behavior, we should pay close attention to the specific characteristics of different regimes. Previous studies on Eastern Europe and the Soviet Union have shown that strongly institutionalized, totalitarian regimes can suppress preferences for democracy and civic activism in the long run. This study shows that cohorts socialized under less institutionalized forms of authoritarianism are, somewhat paradoxically, more likely to support democracy and raise collective issues than cohorts that were socialized after transitions.

This study has important implications for research on democratic consolidation. Mainly, theories should not abstract from individual preferences and take a close look at discontent towards newly established democratic institutions among young citizens. Frustration with slow change under democratic rule may be one reason why young democracies countries sometimes revert to authoritarianism and future research should pay attention to the historical and present causes of such discontent. Future studies could build on the results of this article in two ways. First, country experts could test the benchmark-based theory of socialization subnationally and refine the scope conditions of the argument. As an example, Berinsky et al. (2016)
study the sources of electoral discontent among young South African voters. They find that framing electoral participation as an “obligation of history” (referencing the struggle against Apartheid) does not increase the propensity to vote among the youngest generation of voters. This result is in line with the findings of this study. If individuals have fewer political memories associated with autocracy to tap into, calling attention to historical obligations may not be an effective strategy to activate enthusiasm for present democratic institutions. Second, future research could study socialization and regime preferences in regions that have not been subject to the same empirical tests so far (Latin America, South and South East Asia). In conjunction with this study and research on cohort effects in the former Soviet Union, future empirical work can be used for theory building and improve our understandings of the conditions under which democratic transitions successfully self-perpetuate, and when they are more likely to be diverted by centrifugal forces.
Voting at 16: Intended and unintended consequences of Austria’s electoral reform

Co-author: Laura Bronner (London School of Government)

Abstract
Several democracies are currently debating whether to lower their legal voting age to 16, but relatively little is known about the long-term consequences of such reforms. We contribute to this debate by studying electoral habit formation among 16-year-old voters in Austria, where the national-level voting age was decreased in 2008. We employ eligibility-based regression discontinuities to evaluate two consequences of the reform. First, we show that eligible 16-year-olds are more likely to vote in future elections. Second, we demonstrate that the political consequences of this reform were not neutral. Newly eligible young voters are more likely to support parties on the extremes of the ideological spectrum. We also simulate the cumulative long-term impact on electoral outcomes and argue that the reform was costly for the centrist parties that initially adopted it.

2.1 Introduction
Many advanced democracies are trying to counteract stagnating or falling rates of electoral participation by targeting young voters. Several countries are currently considering one particular strategy - to lower the legal voting age and introduce young voters to formal modes of participation earlier on. Three German states allow 16-year-olds to vote in regional elections (Vehrkamp, Im Winkel, and Konzelmann 2015)
and Greece lowered its voting age in national elections to 17 (Reuters 2016). Scotland temporarily decreased the voting age to 16 for its independence referendum in 2014, and in Estonia 16-year-olds can participate in local elections (European Youth Forum 2015). In 2018, Malta became the second member of the European Union to lower the federal voting age from 18 to 16. Such reforms are not restricted to Europe. Some US1 American municipalities allow citizens under 18 to vote in local elections, and Washington, D.C. is contemplating similar changes (New York Times 2017). This trend raises two important questions for political science: First, does lowering the voting age increase turnout in the long run, as governments hope? Second, are these reforms politically "neutral" or do some political actors benefit more than others?

Previous studies have shown that voters develop persistent electoral habits during their first elections. The most consistent finding in this literature relates to turnout: Young voters who vote once are more likely to turn out in future elections as well (Gerber, Green, and Shachar 2003; Denny and Doyle 2009; Meredith 2009; Dinas 2012; Coppock and Green 2016). Our methodology exploits exogenous variation in voting eligibility as a causal identification strategy. We compare eligible voters, whose birth date falls prior to an administratively set date, to citizens who are not eligible because their birth date is slightly after this cutoff date. Previous studies in this domain have focused exclusively on first-time voters at age 18, but little is known about whether habituation effects apply to other ages as well. In a recent review of the literature, Coppock and Green (2016) conclude that there is an unaddressed need to compare previous findings to other age groups and political systems outside of the United States. The case of Austria provides a highly suitable setting for such external validation. A 2007/2008 reform made Austria the first country in the European Union to allow 16- and 17-year-olds to vote in federal elections. Previous research

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1 Abbreviations used in this article: US (United States), SPÖ (Social Democratic Party of Austria), ÖVP (Austrian People's Party), FPÖ (Freedom Party of Austria), AUTNES (Austrian National Election Study), OECD (Organization for Economic Co-operation and Development).
has shown that Austrians under the age of 18 are on average as informed (Zeglovits and Zandonella 2013) and efficacious (Wagner, Johann, and Kritzinger 2012) as 18- to 21-year olds, and potentially more likely to participate in elections (Zeglovits and Aichholzer 2013). Thus, previous research suggest that the reform could have positive long-term consequences on electoral turnout through habituation. We add to this research by taking advantage of an eligibility-based identification strategy to estimate the causal impact of the voting age reform on future turnout propensity and party affinity.

2.1.1 Habituation effects in the long run

Extending the study of habituation effects to new contexts is not only important for the sake of external validity. It also allows us to evaluate how voting age reforms, such as the one in Austria, affect turnout and electoral behavior in general. One way to think about this long-term causal effect of policy change on political behavior is to distinguish between its two components—base and multiplier effects.

First, consider an individual’s initial probability to cast a vote at an election at time $t = 0$ as their base turnout rate. If individuals in a particular age brackets are relatively more likely to vote when they first are eligible, base turnout for this group is relatively higher. Some evidence suggests that 16- and 17-year-olds are more likely to participate in elections than 18-year-olds when they become eligible. For example, Verba and Nie (1987) argue that residential mobility lowers turnout. In many advanced democracies 16- and 17-year-olds are less to relocate than 18-year-olds. They also tend to be comparatively more embedded in parental, school or neighborhood-based informational networks (Franklin, Lyons, and Marsh 2004). These ties can generate a social anchoring effect which increases turnout compared to 18-year-olds, since the latter are more likely to recently have graduated from secondary schooling or have moved out of their parental home, making them less embedded into the kinds
of social networks that increase turnout (Abrams, Iversen, and Soskice 2010). In general, 18-year-olds are more likely to be in a transitional and itinerant phase of their lives in which voting is less facilitated by external circumstances. Notably, if base turnout among 16- and 17-year-olds is higher than among 18-year-olds, there is a net positive impact on turnout at \( t = 0 \) that goes beyond the mechanical effect of increasing the size of the electorate. This assumption of higher base turnout has been empirically validated by previous studies. Age-disaggregated turnout statistics are not publicly available in Austria, but Zeglovits and Aichholzer (2013) use a stratified sample of voter roles in two municipal elections (Krems, Vienna) to analyze turnout rates within different age groups. They find that base turnout is higher among those under the age of 18. In the 2012 local election in the mid-sized city Krems, 56.3 percent of 16- to 17-year-olds versus 46.3 percent of 18- to 20-year-olds turned out to vote. In the capital Vienna, 64.2 percent of 16- to 17-year-olds versus 56.3 percent of 18- to 20-year-olds participated in the 2012 local election.

The second source of long-term impacts are multiplier effects; i.e. changes in the probability of voting in future elections as a consequence of voting in one’s first election. Formally, multiplier effects are causal changes in turnout probability at time \( t = \{1, 2, 3, \ldots \} \) due to being eligible to vote at \( t = 0 \). This multiplier component is what the literature on habituation effects commonly studies. We refer to it as the multiplier because any differences in base turnout rate between newly and previously enfranchised voters at time \( t = 0 \) will be multiplied in later elections by the size of the habituation effect of eligibility. While previous studies of 16- and 17-year old voters have focused on analyzing differences in base turnout (Wagner, Johann, and Kritzinger 2012; Zeglovits and Zandonella 2013; Zeglovits and Aichholzer 2013; Kritzinger, Zeglovits, and Oberhuggauer 2013), our main contribution is to causally identify the multiplier effect and combine both types of effects to project the long-term consequences of the reform.
To preview our results, we find that 16-year-olds have higher base turnout rates than 18-year-olds at $t = 0$ in Austria. We also find that multiplier effects are positive and substantively large for 16-year-old voters, potentially larger than for 18-year-olds. Specifically, eligibility in the 2008 national parliamentary election increases an individual’s self-reported intention to vote in 2013 by 2.8 points on a 10-point scale, or by about one standard deviation. In a simulation, we show that these larger base and multiplier turnout rates positively affect turnout rates in the long run. We thus contribute to the literature by demonstrating that the timing of initial electoral habituation is crucial. If individuals experience their first elections in a context where their base turnout probability is higher, polities can attain higher steady-state turnout rates.

We also address another important question, and one often neglected in the discussion surrounding habituation effects: Are voting age reforms politically “neutral”, or are they more beneficial to some actors than others? Political scientists have shown that efforts to increase turnout can have partisan effects (Fowler 2013; Enos, Fowler, and Vavreck 2014). In the context of rising polarization in Western democracies (Baldassarri and Gelman 2008a; Jacobson 2013a; McCarty 2015; McCarty and Shor 2016; Levendusky 2016), it is particularly important to ask whether non-centrist parties and candidates benefit disproportionately. In Western Europe, polarization has been accompanied by fundamental changes in party systems, most importantly the decline of established centrist parties and the rise of populist movements. Thus, political debates on electoral reforms are intertwined with power struggles between formerly dominant parties and rising contenders. Austria exemplifies these trends, making it a suitable case study for the effects of electoral reform on polarization. In the 2016 presidential elections, for example, neither of the two established centrist party candidates progressed to the second round; instead, the run-off was held between Alexander Van Der Bellen, the former head of the Green party, and Norbert
Hofer, the candidate of the far-right Freedom Party (FPÖ). In light of these events, it is important to ask whether and how the extension of the franchise to 16-year-olds might affect partisan polarization.

Our results suggest that the reform was not politically neutral. Polarization, as measured by ideological self-placement, increases among voters who became eligible as 16-year-olds in the prior election. Furthermore, these voters are more likely to support parties on the extremes of the party system. While several parties seem to gain from the enfranchisement, the effect is most pronounced for the right-wing populist Freedom Party (FPÖ). This party experiences large gains in partisan attachment (3.5 points on a 10-point scale), while the effect is smaller or insignificant for centrist parties. As a placebo test, we also use the discontinuity design to estimate the causal effect for a party that did not exist in 2008 (Team Stronach). As expected, we find no habituation effect for this party.

The remainder of this article is structured as follows. In Section 2 we describe the background and origins of Austria’s 2008 voting age reform. In Section 3 we review the existing literature and generate a theoretical framework for our case. In Section 4 we introduce our data and describe the research design and identification strategy. Section 5 discusses the results and Section 6 offers a variety of robustness checks and placebo tests to assess the validity of our results.

2.2 Context: Austria’s 2008 Voting Age Reform

We first describe how Austria become the first European country to change its legal voting age to 16. Importantly, turnout in national parliamentary elections had dropped by almost 6 percentage points in Austria’s 2006 parliamentary elections\(^2\), which prompted the two members of the ruling coalition, the Social Democrats (SPÖ)

and the People’s Party (ÖVP) to discuss strategies to reinvigorate electoral participation. Since Austrians between 18 and 25 were particularly underrepresented among voters, this age group became a main target of mobilization efforts. Furthermore, many of these young voters had started identifying with less centrist opposition parties\(^3\) and the government needed to regain their support to sustain electoral majorities. Thus, decreasing turnout and low government support created a critical juncture that allowed for an unprecedented reform of Austria’s legal voting age.

The main opposition parties at the time, the Freedom Party (FPÖ) and the Green Party (GRÜNE), both welcomed the proposed reform. The right-wing populist FPÖ and left-wing environmentalist GRÜNE represent very different constituencies, but both draw significant support from constituencies under the age of 30 (SORA and ISA 2008, 2013). Within the governmental coalition (SPÖ, ÖVP), however, preferences were more heterogeneous. While the SPÖ hoped to gain from a lower voting age, the ÖVP draws its main support from older and more affluent voters and did not expect to benefit much from the policy change. Legislative majority decisions without the consent of all coalition members are rare in Austria and are seen as a breach of coalition norms. Thus, the governing parties had to find a compromise that would assuage both camps. They reached an agreement by adding a provision that allowed absentee voting by mail (Vehrkamp, Im Winkel, and Konzelmann 2015), which the ÖVP expected would facilitate voting among its core supporters who are more likely to have multiple residences and higher geographic mobility. The bill eventually passed in 2007, allowing 16- and 17-year-olds to vote for the first time in national parliamentary elections in 2008.\(^4\)

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\(^3\) According to a nationally representative survey (SORA and ISA 2008), 28 percent of male voters under 30 vs. 22 percent of male voters over 60 voted for the Freedom Party. 21 percent of women under 30 vs. 14 percent of women over 60 reported voting for the FPÖ. The age split is similar for the Green party. 12 percent of male and 15 percent of female voters under 30 vs. 4 percent of male and 2 percent of female voters over 60 reported casting their votes for GRÜNE.

\(^4\) Some of Austria’s local governments had already enfranchised this age group in regional and municipal elections. However, the 2007 bill was the first to include all national, regional and municipal elections.
In total, the reform added 93,000 16-year-olds and 91,000 17-year-olds as eligible voters, which increased the electorate by about 3 percent.\(^5\) While Austria uses a proportional representation (PR) electoral system and small changes in the electorate are usually less consequential than in plurality-based systems, the ongoing restructuring of the party system made this policy change highly important for election outcomes. In the post-war period, the two center parties (SPÖ and ÖVP) had dominated the political system by repeatedly forming “grand coalitions.” However, since the 1990s, like many other European party systems, Austria has witnessed the rise of right-wing populist parties such as the FPÖ and the growing importance of post-materialist political preferences, represented by the Greens. The traditional two-party system started to give way to a more complex four- or five-party system, and in the 2002 parliamentary elections the ÖVP actually fell behind the FPÖ for the first time. Thus, legislative and executive majorities for two-party centrist coalitions are becoming increasingly harder to maintain due to pressures from the left and the right. These changes make the enfranchisement of 16- and 17-year-olds an important factor in election outcomes. In systems that transition to more than two effective parties, small differences in vote shares have relatively greater consequences for coalition bargaining and can potentially swing elections by affecting which parties become part of government coalitions.

2.3 Theory

There is a growing literature in political science on electoral habituation and our study complements and extends several of its findings. In this section we review previous findings and generate hypotheses for our case. First, we define electoral habituation as follows: Behaving a certain way in one election increases the probability

\(^5\)Source: [http://derstandard.at/3408344/Voraussichtlich-6350000-Wahlberechtigte](http://derstandard.at/3408344/Voraussichtlich-6350000-Wahlberechtigte). The total size of the electorate in 2008 was 6,107,851.
of behaving the same way in a future election. Formally, if voters are eligible to vote at \( t = 0 \), they are more likely to vote or engage in other kinds of political behavior at \( t = \{1, 2, 3, \ldots\} \) than their similarly-aged peers who were not eligible to vote at \( t = 0 \).

The specific behavior examined at \( t = \{1, 2, 3, \ldots\} \) varies depending on the research question. Primarily, the literature has focused on turnout and has demonstrated that being eligible to vote at a given election makes citizens more likely to vote in later elections (Copock and Green (2016) provide a review of this research agenda). Less often, other habituation effects have also been studied. For example, voters may develop partisan identities and become increasingly attached to certain parties if they experience their first elections in a certain environment (de Kadt 2017a). In this study, we add to the existing literature in two ways. First, we extend prior turnout-related findings to a new age group. Second, we study habituation of partisanship and polarization, outcomes that have received somewhat less attention in previous research.

2.3.1 Turnout

In recent years, political scientists have found strong evidence for turnout habit formation among voters (Gerber, Green, and Shachar 2003; Denny and Doyle 2009; Meredith 2009; Franklin and Hobolt 2011; Dinas 2012, 2013a; Copock and Green 2016; de Kadt 2017a). While such findings are fairly established for 18-year-olds, this study is the first to test whether it applies to new voters under the age of 18.

In advanced industrialized democracies, the first election for voters under the age of 18 would most commonly take place while they are enrolled in secondary school, apprenticeships or other forms of job training, and while they are still living with their parents, all of which affects the context in which they are introduced to the political system (Wagner, Johann, and Kritzinger 2012; Franklin, Lyons, and Marsh 2004). In Austria, there is evidence that their base turnout rates are higher than those of
18-year-olds (Zeglovits and Aichholzer 2013). As discussed in the introduction, if base turnout among 16- and 17-year-olds is higher, then, through the existence of cumulative habituation effects at \( t = \{1, 2, 3, \ldots\} \), steady-state turnout increases can go far beyond the mechanical effects of enfranchising younger age groups.

Most previous studies of habituation effects have looked at electoral behavior in the US, and there is an open debate about whether these results generalize to other settings. Bhatti and Hansen (2012) argue that habituation is less likely in Europe, where barriers to voter registration are low. Franklin and Hobolt (2011) find that habituation exists in European elections, but only if one’s first election is salient enough. Unfortunately our data only covers two elections and thus does not have variation in the saliency of initial elections, so we cannot directly test their claim. Rather, our focus is on expanding the analysis of habituation effects to an important new group, which many countries are now considering to enfranchise.

For the case of Austria, previous studies have shown few differences in political interest, knowledge and media consumption between 16- and 18-year-olds. Zeglovits and Zandonella (2013) use cross-sectional survey data to demonstrate that political interest among 16-year olds increased from 2004 (pre reform) to 2008 (post reform). The share of respondents who are “very interested” went from 8.1 to 21.8 percent among 16-year olds. Similarly, daily news consumption in the same group increased from 19.1 to 23.8 percent. Wagner, Johann, and Kritzinger (2012) assess the “quality” of vote choice among Austrian first-time voters under 18. Using survey data from the 2009 European Parliament elections, they find that political interest, knowledge, and non-electoral participation are statistically indistinguishable between ages 16-18 and ages 18-21. Furthermore, these younger voters have slightly more trust in

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6Their finding highlights the importance of endogeneity in this context. Metrics such as political interest and non-electoral participation could be lower before a reform is adopted than after, and may provide a negatively biased estimate of the true “quality” of political engagement among those under the age of 18. Policymakers who are contemplating similar reforms should therefore not only rely on domestic statistics (pre reform), but also factor in results from countries that have adopted reforms, such as Austria.
the efficacy of political institutions. The quality of their vote choice, as measured by the congruence of personal and party ideology, is similar to older age groups. Evidence from other countries suggests that 16- and 17-year-olds may, in fact, be more likely to vote than 18-year-olds as they have lower residential mobility and firmer social anchoring in their parental, school and neighborhood environments (Verba and Nie 1987; Franklin, Lyons, and Marsh 2004). In Austria, 16-year-olds seem to have political efficacy and interest that is at least on par with 18-year-olds, and base turnout rates are likely higher (Zeglovits and Aichholzer 2013). Thus, we expect that they experience habituation effects in voting, and that such effects are at least as large as effects discovered in other settings for older first-time voters.

**Hypothesis 1** *Eligibility to vote at age 16 leads to higher turnout in future elections (positive multiplier effect).*

### 2.3.2 Partisanship and Polarization

Going beyond turnout, some scholars have investigated the effects of enfranchisement on other electoral outcomes, though they advance different theories as to what these effects might be. Mullainathan and Washington (2009) argue that being eligible to vote increases political polarization, which they measure as an increased partisan divide in approval ratings of the US president. Similarly, Dinas (2013a) finds an effect on stronger partisan identification, arguing that once voters cast a vote for a certain candidate or party, they seek to reinforce their beliefs by repeating the same behavior in future elections. Both Dinas (2013a) and Mullainathan and Washington (2009) build on cognitive dissonance theory, according to which habits form when people make decisions that are in line with their preferences (consonant votes). Consequently, electoral habit formation fails to occur when individuals do not make the same vote choices consistently (dissonant votes). This theory can be traced back to Festinger (1962) and has recently been formalized and applied to political settings by

One of the advantages of the Austrian case is that its multi-party system allows for a nuanced investigation of the political implications of such a reform. While Mullainathan and Washington (2009) operationalize polarization as approval ratings of the US president, we test directly whether habituation affects self-reported polarization and affinity to non-centrist parties. Applying the cognitive dissonance frame to Austria, we anticipate habituation to have a polarizing effect on first-time voters. We expect voters choosing more extreme parties – FPÖ and Greens – to be more “locked in” to their choice than voters choosing more moderate options for two reasons. First, choosing an extreme party at the first election makes it less likely that a voter can pick a moderate party at her second election without feeling like this represents a tacit admission that she has erred (high cognitive dissonance).

To visualize this dissonance, Figure 2.1 provides a stylized unidimensional representation of the Austrian party landscape. If we assume that a voter’s cognitive dissonance is larger the greater the ideological distance between her first pick and her second, choosing a party on the extremes of the spectrum at the first election means that she has fewer places to move in subsequent elections without incurring substantial cognitive dissonance costs. For example, a voter who chooses the Social Democrats (SPÖ) at her first election can either stick with the SPÖ in the second election, causing zero cognitive dissonance, or move one party to the left (to the Greens) or to the right (to the ÖVP) causing low cognitive dissonance; a voter who chooses the Greens in the first election, by contrast, only has the option to her right (the SPÖ). Since there is no option to her left, she is more likely to end up voting for the Greens again.

The second polarization mechanism we posit is based on parental socialization.

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Footnote: We only include parties that won at least one parliamentary seat in 2008 and 2013, which excludes three parties that held seats in one of these legislative sessions. Team Stronach and NEOS did not run in 2008, and BZÖ failed to pass the 4% threshold necessary to secure a seat in 2013.
Since older voters are more likely to vote for centrist parties, we expect those 16- to 17-year-old voters who are more likely to take cues from their parents to be more likely to choose centrist parties as well. Conversely, young voters who make their decisions in a context of lower parental influence may be more likely to pick extreme parties. In the second election, then, once voters have left the family home, those voters who selected centrist parties due to familial influence will be more likely to change their minds, while those who made their decisions more independently will be more likely to stick with their initial pick.

Regardless of the mechanism at play, we thus hypothesize that if there is indeed a habituation effect, it will increase polarization, which we measure using ideological self-placement and self-reported closeness to more extreme parties. We thus posit that the less centrist FPÖ and Greens benefit more from the enfranchisement of 16-year-old voters through habituation effects in partisan attachment.

**Hypothesis 2** *Eligibility to vote at age 16 leads to more extreme ideological self-placement.*

**Hypothesis 3** *Eligibility to vote at age 16 leads to greater affinity for non-centrist parties (FPÖ, Greens) in future elections.*

According to a different strand of the enfranchisement literature, voters may support the party that enfranchised them out of loyalty. This argument has been made about working-class and Black voters in the US (Edwards 1997), as well as about women (Przeworski 2008), though empirical support has been mixed at best (Morgan-Collins 2017). In Austria, the reform was passed by the centrist SPÖ-ÖVP coalition.
government; however, almost all parties in parliament voted for the bill, and even the far-right FPÖ, which refused to support the ultimate bill because of the provision permitting for postal voting, vocally advocated for lowering the voting age.\(^8\) This blurs the responsibility voters could assign for the passage of the reform, making it unlikely that they would reward the coalition parties specifically.

### 2.4 Data and Methodology

#### 2.4.1 Case selection

In 2008 Austria was the first European country to lower its legal voting age to 16 in all national, regional and local elections. We focus our study on Austria because it provides a unique opportunity to study habituation effects among very young voters, even while it is, in other ways, a representative case among advanced industrialized democracies. Many of its socio-economic indicators lie close to the OECD average, and like many other polities in Europe and beyond, it uses a proportional representation voting system and has faced gradually declining turnout rates in previous decades (Hoffman, León, and Lombardi 2016). Furthermore, as in most other advanced democracies, right-wing populist parties have recently experienced growing electoral support, and as noted above, establishment parties have seen their vote share drop in both legislative and presidential elections. This allows us to use Austria as a laboratory of sorts for the consequences of voting age reforms and provide the first study of habituation effects among 16-year old voters.

#### 2.4.2 Data

In order to test the effect of enfranchising 16- and 17-year-olds in Austria, we make use of the Austrian National Election Study (AUTNES) of 2013, which surveyed

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\(^8\)Source: [https://www.parlament.gv.at/PAKT/VHG/XXIII/I/I_00088/index.shtml](https://www.parlament.gv.at/PAKT/VHG/XXIII/I/I_00088/index.shtml)
Austrian voters about their political attitudes and behavior surrounding the 2013 general election. Its fine-grained data on birth months allows us to identify and estimate the causal effect of enfranchising 16-year-olds in 2008 on electoral behavior in the following election in 2013. The AUTNES data collection effort was substantial and involved several different surveys, including three different panels done face-to-face, by phone and online (Kritzinger, Zeglovits, and Oberluggauer 2013). Since our method is fairly data-intensive, we pool these three representative samples in order to obtain as many observations as possible, using only responses to questions asked identically across all three modes. This pooled dataset contains 300 respondents born between October 1991 and October 1993 and comprises exactly 150 individuals who were eligible to vote in the 2008 election (born in October 1992 or earlier) and 150 who were not. By 2013, the year of the survey, these individuals are in their early twenties and all eligible to vote. For the older half of the respondents, this is their second general election (treatment group), for the younger half this is their first general election (control group).

To measure treatment status we use birth-month data. Ideally, we would estimate turnout habituation effects twice, once for 16-year-olds in 2008 and once for 18-year-olds, the prior eligibility threshold in the previous election in 2006, and compare both the magnitude of the discontinuity-based treatment effects. Unfortunately, however, birth months are not available for older respondents, so we cannot directly compare the habituation effect at age 16 to the same effect at age 18.

The survey questions we use for our outcome measures (translated from the origi-

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9The dataset is publicly available at http://www.autnes.at.

10While the oversampling of young voters and the presence of birth month data allow us to employ a regression discontinuity design to test the effect of enfranchising new voters, the dataset has several drawbacks, the biggest of which is that respondents were not asked whether (or how) they voted in the 2008 election. This means that we cannot test whether the habituation effects we find are due to exposure to partisan mobilization or other factors which targeted all eligible voters, or due to the act of voting itself, as in e.g. Díaz (2013a). Thus, we consider eligibility, rather than voting per se, as the treatment in this quasi-experiment.
nal German) are summarized in Table A.1 in the Appendix. The turnout measure we use is self-reported intention to turn out in the upcoming election on a scale from 0 ("I definitely won’t participate"), to 10 ("I definitely will participate"). We recognize the limits of self-reported turnout as a measure. Unfortunately, administrative turnout data in Austria is not publicly available and there is no equivalent to U.S. voter files in this context. While self-reported intention may not accord perfectly with actual participation, we assume that there is a strong relationship between the two, though we note that this somewhat diminishes our confidence in the numerical accuracy of the estimated effect size. To examine polarization, we use two measures. First, we use a 0-5 scale of ideological extremism constructed by folding the 0-10 left-right self-placement question. We calculate the absolute value of the difference of a response to the mid-point of the scale. Second, we use a question that asks respondents how close they feel to each of the political parties.

2.4.3 Identification strategy

To identify the causal effect of eligibility, we use a regression discontinuity design that compares individuals who differ in eligibility status, but are similar in all other respects. This is based on the idea that by considering a sufficiently narrow window around the administratively set cutoff, the treatment is assigned quasi-randomly. In other words, we are posing the following counterfactual setup: Do those who were eligible to vote in 2008 exhibit different behavior in 2013 than they would have if they had not been eligible? Since eligibility is determined by birth date, which is unlikely to be manipulated, and the election cutoff date is set administratively years in advance, the quasi-exogeneity assumption is plausible (see Section 6 for robustness checks).

11 As our main interest is in demonstrating that a habituation effect exists for 16-year old voters, the exact estimated magnitude of this effect is somewhat secondary to its existence. We assume that, to the extent that measurement error exists, it is not systematically different between individuals in the control and the treatment group.
To model the effect of enfranchisement, we fit local linear regressions on both sides of the cutoff. In contrast to some previous studies (Mullainathan and Washington 2009; de Kadt 2017a), the finer-grained month-level nature of the Austrian data allows us to place greater weight on observations closer to the cutpoint. Thus, using local linear regression makes our design less vulnerable to spurious variation far away from the cutoff. Moreover, as voter registration is automatic for Austrian voters, we do not have to deal with the potentially confounding presence of registration effects (Meredith 2009). The narrower the window around the eligibility date, the more likely it is for the quasi-exogeneity assumption to hold. In contrast, a simple difference-in-means test would effectively treat all individuals within a 24-month-window as comparable; however, depending on a country’s educational and labor market system they may be in different school grades, vary in employment status, have moved out of their parents’ home, or vary in other factors that violate the assumptions necessary for a regression discontinuity design. We address these concerns by fitting a local linear regression model with a triangular kernel on both sides of the birth-month level cutoff. Regression discontinuity estimates can be sensitive to bandwidth choice, so we estimate Imbens-Kalyanaraman optimal bandwidths for our main results and and recalculate the estimates under alternative bandwidths.

The comparison group in all our hypotheses are those who were close to 16 years old at $t = 0$, but ineligible to vote. This comparison maps onto the treatment and control groups in our quasi-experimental design. In other words, the reference group does not cast their first vote at 16, but several years later when they are 20-21 years old. Comparisons to other reference groups - such as 18-year old first-time voters - would add further insight into the relative magnitude of habituation effects for 16-year olds. For example, a positive difference in the relative effect size between 16- and

12The exact age depends on the duration of legislative sessions. In Austria, regular national parliamentary elections are held every five years, hence the reference group casts their first vote at 20-21 years.
18-year olds could be interpreted as the marginal value of lowering the voting age for participation. Due to the above mentioned evidence suggesting the higher turnout probability of 16- and 17-year-olds, we may in fact expect the turnout habituation effect to be larger than that found among 18-year-olds. Unfortunately we are not able to perform this comparison in our data, as all 18-year olds are eligible at \( t = 0 \). One could use the habituation effect for 18-year olds at \( t = -1 \) (previous election) as a reference, but unfortunately our data does not record birth month data for these individuals, which is a necessary component of our regression discontinuity design.

2.5 Results

2.5.1 Turnout

We find that 16-year old voters are subject to habituation effects that are at least similar to, and potentially larger, than those demonstrated in previous studies for older first-time voters (see Figure 2.2, Table 2.2). Our estimate suggests a 2.8-point increase on a 0-10 scale of self-reported turnout. This is equivalent to about one standard deviation of the dependent variable.\(^{13}\) As noted above, self-reported turnout intention is an imperfect measure. However, if one is willing to assume that the 10-point scale represents an individual’s voting probability, with people who respond ‘10’ as turning out with 100 percent probability, and using a linear interpolation in between, we can interpret the effect as a sizable 28 percentage point increase in probability of participating in the 2013 elections.

The interpretation of the habituation effect further depends on the share of 16-year old eligible voters who actually participated in the 2008 election. To estimate the main habituation effect, we use eligibility status as the treatment variable (Intention-to-treat,ITT). An alternative setup would be to regard eligibility as an encouragement

\(^{13}\)The standard deviation of the dependent variable (self-reported turnout) is 2.7.
and actual voting as the treatment. To estimate the habituation effect for those who voted in 2008, we calculate the Treatment-on-the-treated (TOT) effect by dividing the ITT by the share of compliers. Using the best available estimate of turnout in this age group\(^{14}\), we estimate the TOT to be \(\frac{2.8}{0.68} = 4.1\) (1.5 standard deviations).

These results provide evidence for Hypothesis 1: The turnout habituation effect found for 18-year-olds in other studies applies to 16-year-olds as well. The magnitude of the estimated effect is sizable and similar to some of the higher-end estimates in the literature on 18-year-olds (Bedolla and Michelson 2012).\(^{15}\) Moreover, the long-term effect on turnout is magnified by the fact that 16- and 17-year-olds appear to vote at higher rates than their 18- and 19-year-old counterparts in their first election too. Franklin, Lyons, and Marsh (2004) predicts a higher base turnout rate among 16-year-olds based on his observation that voters under 18 are more likely to be embedded

\(^{14}\)We assume a turnout rate of 68% as a result of averaging all turnout rate estimates for 16-year-olds in the following paragraph.

\(^{15}\)However, the effect size should also be regarded with some caution. While it may indicate that 16-year-olds are, in fact, subject to even greater habituation effects than 18-year-olds, in the absence of more data on older Austrian voters we cannot make the direct comparison using the data available.
in turnout facilitating environments such as family households and schools. Two studies validate this prediction for the case of Austria. Kritzinger, Zeglovits, and Oberhuggauer (2013) find that 16- and 17-year-olds report turnout rates of 63% in 2013, compared to 59% for 18- to 20-year-olds. As mentioned in the introduction, using administrative data from municipal elections, Zeglovits and Aichholzer (2013) find that 16- and 17-year-olds than 18- to 20-year olds in Vienna and Krems. We provide further details on the compounding relationship between base turnout and multiplier effects in the next section.

Simulating long-term turnout

The long-term consequences of lowering the voting age can be estimated by combining the above findings on base turnout and multiplier effects. Assuming a turnout rate of around 70% (Kritzinger, Zeglovits, and Oberhuggauer 2013) among this group of second-time voters in 2013, the habituation effect alone is responsible for about 36,000 extra voters in 2013. While this sounds small, it represents a large relative share of Austria’s electorate. The estimated habituation effect for 16-year-olds accounts for 0.7% of total voters in 2013, and is larger than the margin that decided the (later overturned) presidential run-off election in May 2016. Given that Austria’s political system has recently seen the establishment of several new small parties and the parliamentary threshold is at 4 percent, this new group of voters could be of great importance for the future composition of legislatures. We note that these calculations only consider habituation, they do not take into account the direct effect of extending the franchise. Of course, the absolute number of additional votes is even larger since new voters were added to the electorate, however, our causal estimates only consider the net impact that goes beyond this mechanical effect.

If 70% of the 184,000 young people who had been enfranchised in 2008 voted, that amounts to 128,800 voters at \( t = 0 \). Based on the results of the RD, 28% of those voters turned out in 2013 \( (t = 1) \) because of the habituation effect, totalling 36,064. We use an estimate of 70% because this is the self-reported turnout rate for 21- to 29-year-olds in 2013 (Kritzinger, Zeglovits, and Oberhuggauer 2013).
To simulate the long-term consequences of the reform, we combine mechanical effects and our multiplier effect estimates to project their joint impact on future elections. Previous studies have shown that there are accumulating (Coppock and Green 2016), although marginally decreasing (Dinas 2013a) effects of voting habituation. Thus, the voting age reform is likely to have an even greater impact over the course of future elections. Each act of voting can increase turnout propensity at the following election. In Table 2.1, we simulate these cumulative effects based on the estimated habituation effect on turnout that we identified in the regression discontinuity. In 2008, the reform accounted for 2.11% of the electorate in the first election (mechanical effect only). Due to accumulating multiplier effects, we estimate that this share increased to an estimated 3 percent in the second election (which took place in 2013), and we expect it to increase to 3.76% of the electorate by the fifth election after the reform.

Our simulation makes four assumptions: First, we assume that the number of newly eligible voters stays the same as in 2008, when it was 184,000. This assumption is reasonable given Austria’s low, but non-negative population growth projections. Second, we assume that these newly eligible voters vote at the same rate as in 2008. Together, these first two assumptions allow us to calculate the mechanical effect of the voting age reform, i.e. the number of additional voters due to the simple fact that the size of the electorate grew. Third, we assume that the habituation effect gradually declines and is reduced by half at each consequent election (1, 0.5, 0.25, etc.). Fourth, we assume that no eligible citizens drop out of the electorate between elections, so that the number of first-time eligible voters at \( t = 0 \) is the same as the number of second-time eligible voters at \( t = 1 \), i.e. they are both 184,000. Together, the third and fourth assumption can be used to calculate the cumulative habituation effect.
Table 2.1: Simulating the cumulative effects of the voting age reform in Austria

<table>
<thead>
<tr>
<th>Election</th>
<th>Mechanical</th>
<th>Cumulative habituation</th>
<th>Additional voters due to reform</th>
</tr>
</thead>
<tbody>
<tr>
<td>$t = 0$ (2008)</td>
<td>$0.7 \cdot 184,000 = 128,800$</td>
<td>none</td>
<td>$128,800$ ($2.11%$ of electorate)</td>
</tr>
<tr>
<td>$t = 1$ (2013)</td>
<td>$0.7 \cdot 184,000 = 128,800$</td>
<td>$0.2827 \cdot 184,000 = 52,016$</td>
<td>$180,816$ ($2.96%$ of electorate)</td>
</tr>
<tr>
<td>$t = 2$ (2017)</td>
<td>$0.7 \cdot 184,000 = 128,800$</td>
<td>$\frac{1}{3} \cdot 0.2827 \cdot 184,000 = 78,025$</td>
<td>$206,825$ ($3.39%$ of electorate)</td>
</tr>
<tr>
<td>$t = 3$ (expected: 2021)</td>
<td>$0.7 \cdot 184,000 = 128,800$</td>
<td>$\frac{1}{3} \cdot 0.2827 \cdot 184,000 = 78,025$</td>
<td>$219,829$ ($3.6%$ of electorate)</td>
</tr>
<tr>
<td>$t = 4$ (expected: 2026)</td>
<td>$0.7 \cdot 184,000 = 128,800$</td>
<td>$\frac{1}{3} \cdot 0.2827 \cdot 184,000 = 78,025$</td>
<td>$226,331$ ($3.71%$ of electorate)</td>
</tr>
<tr>
<td>$t = 5$ (expected: 2031)</td>
<td>$0.7 \cdot 184,000 = 128,800$</td>
<td>$\frac{1}{3} \cdot 0.2827 \cdot 184,000 = 78,025$</td>
<td>$229,582$ ($3.76%$ of electorate)</td>
</tr>
</tbody>
</table>
2.5.2 Partisanship

Polarization

We have demonstrated that voting eligibility increases turnout, so the next question is how this affects the Austrian political landscape. As has been shown, state and non-state initiatives that aim to increase turnout are not necessarily politically neutral and can have substantial partisan effects (Fowler 2013; Enos, Fowler, and Vavreck 2014). In Figure 2.3 and Table 2.2 we summarize the estimated partisan habituation effects.

We find evidence for Hypothesis 2: Individuals who were eligible to vote at 16 are more likely to take more extreme positions. As described in the data section, our polarization scale ranges from 0 (centrist) to 5 (extreme), and we estimate that being eligible to vote in 2008 causes respondents to place themselves a full 1.7 points closer to the end point of the scale. Thus, our results indicate that extending the franchise to 16-year-olds may accelerate the ongoing transformation of Austria’s and Western Europe’s two-party systems, as ideological orientation and partisan identities move away from the center.

Party closeness

In order to detect whether ideological self-placement translates into party affinity, we test the impact of eligibility on how much respondents professed to like or dislike each of the main Austrian political parties on a 0-10 scale. Figure 2.4 shows the estimated effects of regression discontinuity coefficients for each of these party affinity

17 The surveys we use also asked about vote choice, but many respondents refused to answer this question. However, the closeness question asked respondents to rank how much they like each party on a scale from 0-10, and even most of those who refused to divulge their vote choice gave a response to this question. By comparing the scores they gave each party, we can deduce which party they like most, a metric which correlates with professed vote choice at around 0.85 for those who named their choice (the correlations differed slightly across the different parties, ranging from 0.79 for the Greens to 0.87 for the FPÖ). This question was only asked in the two offline surveys, so we drop the online survey for this test.
All four of the main parties have a positive habituation effect. Eligibility at 16 in 2008 causes respondents on average to like these parties more in 2013. This is not surprising given that habituation effects are partly due to repeated voting for the same party (Dinas 2013a). However, if this effect was non-partisan, we would expect it to be highest and most significant for centrist parties, which still accounted for the bulk of votes in 2008. We may even expect the effect to be greater for centrist parties if newly enfranchised voters wanted to express gratitude towards the coalition government for passing the reform.

Our results in Figure 2.5 show the opposite: The largest habituation effects exist for FPÖ and Greens, followed by SPÖ and ÖVP. These effects are sizeable; on a 10-point scale, we estimate a 3.5 points (35 percent of the total scale) gain for the FPÖ and 3.1 (31 percent) for the Greens, compared to 2.5 (25 percent) for the SPÖ. The effect on ÖVP affinity is not significant at the 0.05 level. We thus find evidence in favor of Hypothesis 3: Rather than rewarding the centrist parties who adopted the voting age reform, voters who were newly enfranchised in 2008 at age 16 develop a
Centrist parties:

(a) SPÖ like/dislike

(b) ÖVP like/dislike

Non-centrist parties:

(c) FPÖ like/dislike

(d) Greens like/dislike

Figure 2.4: The effect of 2008 eligibility on 2013 party affinity
stronger affinity to non-centrist parties, FPÖ and the Greens, in 2013. Notably, this causal effect is not due to age, as we are comparing eligible and ineligible voters of the same age.

In sum, our results indicate that the effects of the voting age reform were far from politically neutral. Rather, the act of enfranchising 16-year-olds and the electoral context they cast their first votes in makes them identify more strongly with non-centrist parties. Thus, while the effects of the voting age reform may boost overall turnout in the long run (see previous section), they may also come at a cost to the more centrist political actors in Austria’s party system.

To further validate our results, we also conduct a placebo test. Figure 2.5 shows the estimated habituation effect for Team Stronach, a political party that did not exist in 2008. Declared closeness to this party, created in September 2012 by Austro-Canadian businessman Frank Stronach, is a useful placebo outcome for our estimate of party habituation effects. Since it did not exist at $t = 0$, we would expect to see no habituation effects at $t = 1$. Indeed, we find an insignificant effect of eligibility on
affinity for Team Stronach, reinforcing our confidence in the above results for other parties that did exist at \( t = 0 \).

Table 2.2 summarizes the results of all models estimated in this section - the effect of enfranchisement on turnout, polarization, as measured by collapsed left-right self-placement, and closeness to each party. In the Appendix, we also provide a simulation of the consequences of the reform on vote shares in 2013, which illustrates that non-centrist parties gain relative to centrist parties.

Table 2.2: RD results, local linear regression with Imbens-Kalyanaraman optimal bandwidths

| Variable     | Bandwidth | Observations | Estimate | Std. Error | z value | Pr(>|z|) |
|--------------|-----------|--------------|----------|------------|---------|---------|
| Turnout      | 6.713     | 169          | 2.827    | 1.386      | 2.040   | 0.041   |
| Polarization | 3.916     | 69           | 1.659    | 0.326      | 5.096   | 0.000   |

**Closeness to:**
- SPÖ: 6.319, 115, 2.471, 1.021, 2.420, 0.016
- ÖVP: 6.514, 112, 1.652, 1.411, 1.171, 0.242
- FPÖ: 4.848, 71, 3.544, 1.710, 2.073, 0.038
- Greens: 6.311, 115, 3.100, 1.154, 2.687, 0.007
- Team Stronach: 5.093, 74, -1.021, 1.583, -0.645, 0.519

### 2.6 Robustness checks and placebo tests

We conduct three robustness checks that support the validity of our research design. First, we use the same discontinuity design as above on pre-treatment covariates as outcomes. A key assumption of RD designs is non-discontinuity in potential outcomes. A smooth relationship between the running variable and placebo covariates provides evidence for this assumption. Our data offers three suitable variables for this test: Gender as a self-reported measure of sexual identity, household size acts as a proxy for household income, and region is a binary measure of urban or rural
residency. We use Imbens-Kalyanaraman (IK) optimal bandwidths for each placebo outcome. Table 2.3 shows the LATE results. None of the effects is significant at conventional levels, which provides evidence for a smooth potential outcomes function.

Table 2.3: Placebo test on pre-treatment covariates

| Variable         | Bandwidth (IK) | Estimate | Std. Error | z value | Pr(>|z|) |
|------------------|----------------|----------|------------|---------|---------|
| Gender           | 2.996          | 0.439    | 0.420      | 1.045   | 0.296   |
| Household size   | 4.574          | 0.630    | 0.704      | 0.894   | 0.371   |
| Region           | 2.054          | -0.521   | 0.368      | -1.417  | 0.157   |

Table 2.4: Placebo test with alternative cutoffs

| Bandwidth (IK) | Estimate | Std. Error | z value | Pr(>|z|) |
|----------------|----------|------------|---------|---------|
| LATE           | 3.372    | -0.481     | 1.634   | -0.294  | 0.769   |
| Half-BW        | 1.686    | -0.569     | 1.224   | -0.465  | 0.642   |
| Double-BW      | 6.745    | -0.670     | 1.004   | -0.667  | 0.505   |

Second, in order to make sure that the discontinuity we pick up is not just a result of the respondents being of different ages in general - but rather, of them falling on either side of the cut point - we discard the portion of the data above the cut point and estimate the effect on turnout among the younger half of voters, using as the new cut point a date six months after the actual election date.\(^{18}\) Like in the main analyses above, we use the midpoint of the running variable window as the cutoff. Again, we find no indication that the assumptions of our research design are violated (Table 2.4). The LATE at this placebo cutoff is actually negative, although very small and not statistically significant at conventional levels, as are alternative bandwidth specifications.

Third, we explore whether there is sorting of individuals around the eligibility

\(^{18}\)We chose to discard the older half of the voters because there was a regional election in Lower Austria, one of the biggest states, six months prior to the 2008 general election, which might potentially have caused similar habituation effects among young voters from Lower Austria.
date. This would be the case if birth months were strategically chosen based on electoral cutoffs. Such sorting is unlikely, not just because the lowering of the voting age was only decided in 2007, but also because the eligibility cutoff was moved from January to October in the early 2000s. These changes could not possibly have been anticipated by the parents of 2008 first-time voters. Furthermore, there is no evidence of administrative manipulation of birth records in Austria. However, we additionally provide empirical evidence for this assumption. A McCrory test of birth months, our running variable, returns a p-value of 0.641, indicating strategic sorting is unlikely.

2.7 Conclusion

This paper has shown that the extension of the franchise to 16- and 17-year-old voters can have far-reaching political consequences, potentially making it a way to address falling turnout. In the first instance, we showed that 16-year-olds are subject to the same kind of habituation effect as older first-time voters, being 28 percentage points more likely to say they will vote in the following election than similarly-aged but previously ineligible voters. The combination of 16-year-olds’ higher initial turnout levels and this sizable habituation effect means that this reform does indeed have the potential to combat the crisis of turnout in advanced democracies in the longer run.

In the second instance, however, we found that not all parties benefit equally from this increase in turnout. Rather, the two centrist parties responsible for the reform, SPÖ and ÖVP, were actually harmed by it, relatively speaking, while the two parties further from the ideological center, FPÖ and the Greens, benefited substantially. Indeed, we find that this reform seems to accelerate the centrifugal tendencies that have characterized the party systems of advanced democracies over the past few decades.

However, we are not able to identify why enfranchisement polarizes younger voters in this way. There are several conceivable mechanisms. In one set of explanations,
younger voters simply have different (more extreme) preferences than older voters, perhaps because they are temperamentally more drawn to more polarizing rhetoric and more radical solutions, or because they were not socialized into the Proporz system that dominated the post-war period in Austria, in which SPÖ and ÖVP controlled access to many professional positions. If younger voters have different preferences, cognitive dissonance theory suggests that those who vote according to these preferences (thereby casting “consonant” votes) exhibit more habit formation than those who cast dissonant votes (Dinas 2013a; Mullainathan and Washington 2009). In a second set of explanations, younger voters do not intrinsically have different preferences, but cast votes for more extreme parties at higher rates for external reasons. For example, more extreme parties, which do not have as much of an established (older) voter base, may be better at targeting new (young) voters. Alternatively, it may be a question of formative elections; according to this explanation, the FPÖ and Greens were on an upswing in 2008, and so among citizens who were eligible to vote for the first time, those with more extreme preferences were more likely to turn out than those with more moderate preferences, who were less mobilized by the campaign. In this model, though younger voters do not start out with different preferences, they are mobilized differentially, and so habit formation occurs as a result of their more extreme voting behavior. Additionally, since FPÖ and Greens did relatively better in 2008 than the two centrist parties, it is conceivable that positive reinforcement would lead those who had chosen them initially to stick with their choice, while centrist voters, who did not experience this reinforcement, would be likelier to switch in the following election. Going even further, it is conceivable that being eligible to vote affects voting behavior through mechanisms other than the act of voting itself, such as being targeted more heavily by parties once one is of voting age, or social network effects of being part of a peer group of voters. Thus far, as our data has not allowed us to separate out these potential mechanisms, we have considered the joint effect
of eligibility, but we hope to tackle these distinctions in future work with more data becoming available.

This study also leaves open other avenues for future research. While we have shown that the turnout habituation effect exists for 16-year-olds, we have not been able to directly compare the magnitude of this effect to that for 18-year-old first time voters in Austria, for example. Finally, despite the wealth of studies on the turnout habituation effect, more research needs to be done on the the political consequences of this effect, and the role it plays structural changes of party systems. Since many countries have taken steps to reduce the voting age at lower-level elections, future studies should also explore whether the effects we identify at the national level also apply to the state and municipal level.
3  Perceived Unity and Partisan Polarization

Abstract

Partisan polarization has become the modus operandi of politics in the United States and other advanced democracies. This study contributes to extant research on polarization in two ways. First, it shows that individuals in the US electorate strongly misperceive the distribution of ideological beliefs in their political out-groups. Second, it designs and implements a series of experimental interventions that aim to reduce such misperceptions. Providing factual information about out-group ideologies significantly shifts prior beliefs and reduces affective polarization. Furthermore, the study investigates heterogeneous effects by partisan subgroups and consequences for individual willingness to accept bipartisan compromises and partisan-motivated reasoning.

3.1 Introduction

Partisan polarization has become the modus operandi of politics in the United States and other advanced democracies. A growing body of research discusses the origins of polarization and its impact on political preferences, belief systems, cognitive processing and intergroup relations (Baldassarri and Gelman 2008b; Jacobson 2013b; Druckman, Peterson, and Slothuus 2013; Prior 2013; Gentzkow 2016; Smidt 2017). Public opinion polls suggest that affective polarization has reached unprecedented levels in the US electorate. In 2016, 91 percent of Republicans and 86 percent of Democrats held “unfavorable” or “very unfavorable” views of the opposing party (Pew Research Center 2016). In 1994, it was only 74 percent of Republicans and 59
percent of Democrats (Pew Research Center 2014). Laboratory and survey experiments have repeatedly shown that US voters have internalized negative affect toward non-partisans, and some suggest that inter-partisan hostility even exceeds negative racial affect (Iyengar, Sood, and Lelkes 2012; Iyengar and Westwood 2015).

Some political scientists argue that polarization is limited to the realm of emotions, and that citizens substantively agree on many policy issues (Fiorina 2011). Others suggest that polarization goes beyond affect and has changed how individuals process information and form policy preferences. Several studies have shown that US voters interpret factual information with partisan-motivated biases (Bullock 2009; Nyhan and Reifler 2010; Taber and Lodge 2006; Druckman, Peterson, and Slothuus 2013). If voters develop divergent political preferences even if faced with the same facts, polarization is likely not restricted to the realm of emotions. Rather, it has direct consequences for the political positions citizens take. Public opinion research provides additional evidence for this more encompassing view of polarization, as the gap between median liberal and conservative ideologies in the US electorate has substantially widened (Pew Research Center 2014).

In light of this growing social distance between partisan groups, what can be done to mitigate the negative consequences of polarization? Can we move towards depolarization? I argue that an important leverage point for depolarization efforts is the degree to which individuals perceive political groups as homogeneous.¹ In polarized settings, individuals think of out-group members as homogeneous and unified, even if their preferences and identities actually cover a wide range of beliefs (perceived out-group unity). I use existing survey data to demonstrate the existence of such misperceptions and provide original experimental evidence of the relationship between perceived unity and polarization. To preview the main results, I find that mispercep-

¹An earlier branch of research in social psychology coined the term entitativity for perceived group unity (Grant and Hogg 2012; Lickel et al. 2000). Today this term is used infrequently, so for ease of understanding I refer to this concept as “perceived unity” or “perceived homogeneity” interchangeably.
tions are greater for out-groups than in-groups, and that supplying new information on out-group unity significantly scales back affective polarization. Furthermore, misperceptions are not symmetrical across partisan groups. Out-group misperceptions are stronger among Republicans, and some treatment effects are more pronounced among Republicans.

The article is structured as follows. First, Section 3.2 presents descriptive statistics on misperceptions of unity in the US electorate. Next, Section 3.3 reviews the relevant literature, and develops a theoretical framework with mechanisms that link perceived unity to different dimensions of polarization. Section 3.4 summarizes the experimental research design and Section 3.5 discusses the results. While most of the paper is focused on out-group misperceptions, Section 3.6 discusses the role and importance of in-group misperceptions. The article concludes with implications for policy and future research.

3.2 Perceptions versus placement

To demonstrate the discrepancy between external perceptions and ideological self-placement, I analyze data from the 2016 American National Election Studies (ANES 2016). Figure 3.1 plots the ideological self-placement of respondents on a seven-point scale against the placement of their own party as viewed by their respective partisan out-group, i.e., how Republicans are viewed by Democrats and how Democrats are viewed by Republicans. Unfortunately, there is no survey question in ANES that directly refers to out-group supporters rather than elites. In the absence of such a question, I use a question that refers to out-group parties in general terms as a proxy.\(^2\)

\(^2\)Where would you place yourself on this scale, or haven’t you thought much about this? 1. Extremely liberal, 2. Liberal, 3. Slightly liberal, 4. Moderate, middle of the road, 5. Slightly conservative, 6. Conservative, 7. Extremely conservative (Question number: V161126)

\(^3\)Left plot: Where would you place the Democratic Party on this scale? (Question number: V161130); Right plot: Where would you place the Republican Party on this scale? (Question number: V161131). I validate this proxy measure in my experimental sample, where I ask respondents directly about their views
The left plot shows a marked contrast between how Democrats place themselves and how they are perceived by Republicans. Most Republicans think that Democrats generally stand at the far liberal end of the ideological spectrum (red density curve). However, many Democrats actually identify with more moderate positions at the center (blue density curve). The right plot applies the same contrast to Republicans. The results are less pronounced than for Democrats, but point in the same direction. Most Democrats think that Republicans stand at the far conservative end of the ideological spectrum (red density curve), but a fair number of Republican respondents actually identify with more moderate ideologies (blue density curve).

of out-group members. The same misperceptions exist in this sample (see section 3.5).
3.2.1 Policy preferences vs. perception

The mismatch between perceived and actual homogeneity also extends to specific policy issues. In Figures B.1-B.3 (Appendix), I use the same data source to compare self-declared preferences and out-group perceptions on three issues: Public health insurance (Figure B.1), the role of government in providing guaranteed jobs and incomes (Figure B.2), and the scope of government services (Figure B.3). For each question, respondents were asked to place themselves and the out-group on a seven-point scale of policy options. Unfortunately, the wording of the out-group survey questions is slightly different than in the previous section. Instead of “the Democratic/Republican Party”, respondents were asked to place the 2016 presidential candidates of each party (Hillary Clinton for the Democratic Party, Donald Trump for the Republican Party). Donald Trump’s campaign was characterized by some major deviations from conventional Republican messages, which makes these results somewhat harder to interpret.

Notwithstanding this data constraint, the results confirm the trend of Figure 3.1. In terms of general ideology, as well as along specific policy issues, Republicans perceive most Democrats to be on the far progressive end of the spectrum (red density curves in the left plots) and, vice versa, Democrats perceive most Republicans to be on the far conservative end (red density curves in the right plots). Once we look at self-placement, however, we see that partisans on both sides often have moderate views on these issues. Their frequency distributions are generally wider and centered at more moderate positions (blue density curves).

3.2.2 Causes of perceived unity and polarization

The main objective of this study is to conceptualize perceived unity as an important dimension of polarization and a potential entry point for depolarization strategies. The causal origins of the divergence between perceived and actual identities are beyond the scope of this article, but there are several plausible upstream factors.
For example, some media channels portray partisan out-groups in an overly stylized fashion (Mutz 2006). As a consequence, perceived unity may be more salient among those who consume such media. Furthermore, Americans are becoming increasingly less likely to personally interact with non-partisans due to geographical sorting (Mason 2015), which can exacerbate skewed perceptions of out-group unity. Conversely, perceived unity may itself be a cause of affective polarization, as it increases the emotional distance between partisans and their out-groups. Individuals who perceive out-groups as less moderate and diverse than they actually are may be less willing to accept bipartisan compromises. I discuss the theoretical links between perceived partisan unity and polarization in the next section.

3.3 Theory

Research in the tradition of contact theory (Allport 1958) has long studied how inter-group conflict can be overcome by exposure and the establishment of social ties. Recently, scholars have applied similar ideas to inter-partisan contact. Hochschild (2016) proposes personal conversations as a tool to reestablish meaningful contact across partisan lines. In her view, such conversations can help decrease affective polarization by making individuals on either side of the political divide feel heard and understood. Her argument is emblematic of a more general view in civil society according to which divisive electoral politics can be overcome by facilitating conversations between liberals and conservatives (Local Voices Network 2019; The Civil Conversations Project 2019). Meta-analyses of prior research suggest that the success of such initiatives critically depends on the settings in which contact takes place. There is abundant empirical evidence in favor of the contact hypothesis, but less clarity on its prerequisites and scope conditions (Paluck, Green, and Green 2018).

This article aims to establish one such scope condition for inter-partisan contact. It argues that exposure to out-group members has differential effects depending on
the composition of the set of out-group members one is exposed to. As shown above, many Americans have skewed perceptions of the ideological composition of their partisan out-group. Take an individual who believes that most out-group supporters are more to the left/right than they actually are. Now, suppose that she interacts with several new out-group members, for example by meeting new co-workers or being introduced to the family members of a partner. If this new set of out-group members is relatively homogeneous and ideologically unified, her preconceived notions about the composition of the out-group can become reinforced. By contrast, if the set of out-group members is more ideologically diverse, she may be more likely to update her perceptions of the out-group. The experimental design of this study simulates such differential exposure and investigates its effects on affective and issue-based polarization.

Previous political science research on efforts to curb polarization has studied monetary incentives (Bullock et al. 2015) and framing techniques (Levendusky 2017; Druckman, Peterson, and Slothuus 2013) as interventions. However, as Kahan (2016) points out, direct material incentives are largely absent from individual political participation, which decreases their utility for policy-makers and civil society. While monetary incentives may prove effective in the laboratory, their policy implications are less clear. Second, while framing techniques can be effective in mitigating polarization (Levendusky 2017), their application may be limited to issues where there is a common supra-identity to tap into, which is not the case for many divisive policy issues. Another branch of research on polarization has studied the impact of elite-level unity, for example in congressional debates and bipartisan lawmaking (Druckman, Peterson, and Slothuus 2013). This study contributes to extant research by focusing on the citizen-level, where the mismatch between perceived and true homogeneity may be even greater.

Ahler (2014) and Ahler and Sood (2018) have shown that out-group mispercep-
tions also extend to socio-demographic attributes. For example, Democrats overestimate the share of high-income Republicans and Republicans overestimate the share of BGLTQ+ Democrats in the electorate. While there are important parallels between misperceptions of attributes and ideology, this study contributes to extant theories in two ways. First, it focuses directly on misperceptions of political positions instead of socio-demographic attributes. Misperceived policy positions have direct consequences for polarization in the electorate. If individuals believe that there is large overlap of moderate policy positions across partisan lines, they may be more likely to accept bipartisan compromises. As this study shows, one aspect of polarization is that individuals underestimate the size of this overlap. Such opportunities for compromise may exist even when partisans are strongly sorted in terms of socio-demographic background. In other words, misperceptions about out-group attributes may be less consequential for affective polarization if they are orthogonal to misperceptions about ideology. Second, Ahler (2014) and Ahler and Sood (2018) focus on out-groups. This study also considers in-group misperceptions and weighs their relative importance for polarization.

Previous research has also demonstrated that social sorting aggravates partisan polarization. Mason (2015) shows that the electorate is more strongly sorted by social attributes than by substantive issues. Social sorting diminishes the effectiveness of cross-cutting cleavages as a strategy for partisan reconciliation, despite much overlap on policy preferences in the electorate (Mason 2016). While existing studies demonstrate that the electorate is mostly sorted along social lines, I show that this distinction between social identities and issue preferences does not apply people’s perceptions. The evidence discussed above suggests that, in the realm of perceptions, individuals view each other as strongly sorted along socio-demographic lines (Ahler and Sood 2018) as well as ideology (Figure 3.1) and policy preferences (Figures B.1, B.3, B.2). This insight further complicates the effectiveness of cross-cutting cleav-
ages. Even if there was less social sorting in the electorate, skewed perceptions of substantive overlap at the center would make it challenging to identify opportunities for compromise.

3.3.1 Perceived out-group unity

In this section, I introduce a theoretical framework linking perceived unity and polarized affect and behavior. I focus on perceived out-group unity, which occurs when individuals view partisan out-groups as more unified than they truly are. I also consider the possibility of perceived in-group unity in section 3.6. There are several ways in which perceived out-group unity is connected to polarization. I summarize the resulting hypotheses below and, in the next section, introduce an experimental research design to test them empirically.

Social distance. First, overestimated out-group unity increases the average social distance between in-group members and “typical” out-group members, because individual conceptions of the out-group are skewed to political extremes. These extreme points are seen as undesirable by in-group members due to their large social distance. As a consequence, perceived social distance results in negative affect towards partisan out-groups. This channel constitutes a direct link between misperceived unity and affective polarization.

Out-group threat. A second consequence of overestimated out-group unity is that out-groups are attributed a higher capacity for collective action. If an out-group is seen as more unified and extreme, individuals view their likelihood of implementing a political agenda as more likely. Such capacity is tied to out-group threat if the preferences of the out-group are distinct. Thus, misperceived unity generates perceptions of relatively greater out-group threat.

Partisan-motivated reasoning. Third, as an extension of the first channel, affect is an important filter for how individuals process information. Previous studies
have shown that individuals with negative affect toward partisan out-groups are more likely use partisan-motivated reasoning (Taber and Lodge 2006; Erisen, Lodge, and Taber 2014). Through this mechanism, skewed perceived out-group unity can dis-incentivize accuracy in informational processing. As a consequence, individuals may develop more extreme preferences if they repeatedly use motivated reasoning. Thus, perceived unity may increase issue-based polarization in the long run. Conversely, reducing such misperceptions could decrease the prevalence of partisan-motivated reasoning.

**Accepting bipartisan compromise.** Perceived out-group unity further reinforces polarization by framing non-partisans as unwilling to compromise. The more an out-group is successfully portrayed as homogeneous, the easier it is for political entrepreneurs and media to promote beliefs about insurmountable political differences among in-group members. As a consequence, individuals perceive the overlap between their own positions and those of out-group members as relatively small. This makes bipartisan compromises less likely, as in-group members view the cost of reaching such agreements as very high. Lowering perceived out-group unity might also make individuals less receptive to polarizing messages by in-group elites and potentially correct partisan-motivated biases and increase their willingness to accept bipartisan compromises.

### 3.3.2 Hypotheses

The theory yields the following hypotheses regarding the relationship between entitativity and specific aspects of partisan polarization. All hypotheses and their according empirical tests are summarized in Table 3.1

**Hypothesis 1** Providing information about out-group unity effectively changes perceptions, i.e. individuals update their beliefs about the distribution of ideology in the out-group.
**Hypothesis 2** Reducing perceived out-group unity decreases negative affect and increases positive affects towards the out-group.

**Hypothesis 3** Reducing perceived out-group unity lowers perceptions of out-group threat.

**Hypothesis 4** Reducing perceived out-group unity makes individuals more likely to accept bipartisan compromise and moderate policy positions.

**Hypothesis 5** Reducing perceived out-group unity decreases an individual’s tendency to use partisan motivated reasoning.

Table 3.1: Summary of Hypotheses and empirical operationalizations.

<table>
<thead>
<tr>
<th>Hypothesis</th>
<th>Experiment</th>
<th>Outcome variable</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>H1: Perception updating</strong></td>
<td>1, 2</td>
<td>Perception of average out-group position (tax policy, racial justice); Posterior estimated share of Moderates in out-group</td>
</tr>
<tr>
<td><strong>H2: Affect</strong></td>
<td>1, 2</td>
<td>Feeling Thermometer, Positive/Negative Traits</td>
</tr>
<tr>
<td><strong>H3: Threat</strong></td>
<td>2</td>
<td>Perceived likelihood of out-group implementing its policy agenda</td>
</tr>
<tr>
<td><strong>H4: Bipartisan compromise</strong></td>
<td>1, 2</td>
<td>Distance to centrist policy preference (tax policy, racial justice); Interaction effect of treatment and randomized group-loss cue (Outcome: Support for First Step Act)</td>
</tr>
<tr>
<td><strong>H5: Partisan motivated reasoning</strong></td>
<td>2</td>
<td>Interaction effect of treatment and randomized group-support cue (Outcome: Support for Energy Act)</td>
</tr>
</tbody>
</table>
3.4 Methodology

I test these hypotheses in two randomized controlled online experiments. In each experiment, the goal of the treatment was to correct misperceptions about out-group unity. A battery of outcome measures was used to test whether the treatments successfully mitigate polarization. The first experiment used infographics and the second experiment used interactive sliders to instruct participants about the actual distribution of ideology in the out-group.

3.4.1 Experiment 1

In this experiment, participants were asked to estimate the distribution of moderate and extreme ideologies in their out-group. After a set of demographic questions, each respondent was asked to state their prior belief about unity in the out-group by choosing one of two infographics showing proportions of moderates and extreme ideologues among supporters of the out-group party (see Figure B.7 and B.8 in the Appendix). Only one of the two choices was correct. The correct option was constructed by generating a waffle chart from the blue density curves (actual self-placement) in Figure 3.1. The incorrect option was derived from the red density curves (out-group perception) in the same figure. The graphic was customized to each respondent based on their self-identified in-group (Democrat or Republican).

While the control group proceeded directly to the outcome measurements, the treatment group was informed about the true degree of unity in the out-group by revealing which info-graphic was correct.\(^4\) In addition, a brief text was used to highlight the difference between how their out-group is perceived and how its members place themselves (see Figure B.9 and B.10 in the Appendix).

A total of 2,293 respondents were recruited through Amazon Mechanical Turk.

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\(^4\)To avoid deception, the correct answer was also revealed to subjects in the control group at the end of the experiment.
(MTurk) on October 24, 2018. 2,088 could be associated with a political in-group and thus received information about their out-group in the treatment condition. In-groups were assigned for (1) Respondents who declared a party affiliation, (2) Respondents who “lean” towards one of the major parties, and (3) Independents who identify with liberal or conservative ideologies. Since the theory only applies to individuals who have an in-group identity, the analysis below focuses on these respondents. The remaining 205 respondents are Independents with Moderate or undetermined ideologies. For the purpose of the analysis “Democrat”/“Republican” refers to an individual who is considered a Democrat/Republican for the experiment. 68 respondents did not state a party affiliation, but could be assigned an in-group based on their ideology. All results are robust to excluding these 68 respondents and including only individuals who explicitly stated a party identity. As expected from prior research on the socio-demographic background of MTurk workers, a disproportionate number of respondents are liberal (1,238 Democrats, 833 Republicans). Treatment assignment took place conditional on partisanship, i.e. was assigned randomly within each partisan group. For additional descriptive statistics and balance tables, see Appendix B.8.

3.4.2 Experiment 2

Three reasons warranted conducting a follow-up experiment. First, Figure 3.1 suggests that misperceptions are asymmetric across partisan groups. As I show below, out-group misperceptions are relatively more common among Republicans. This heterogeneity suggests potential differences in treatment effects. The intervention may be more effective among partisan groups with greater misperceptions.

While the info-graphics in Experiment 1 provided an intuitive way to communicate the difference between perceptions and self-placement, it was not possible to calculate the numeric difference between perceived shares and actual percentages and investi-
gate heterogeneity along different degrees of prior misperceptions. For this reason, the second Experiment asked respondents to provide numeric estimates of the percentage of out-group members who are Very Conservative/Liberal, Conservative/Liberal, Slightly Conservative/Liberal and Moderate (see screenshot in Appendix B.7). These percentages were constructed by focusing on the out-group members in these four ideological categories and defining their relative shares to sum up to 100 percent. Survey responses also had to add up to 100 percent.  

Second, there were two hypotheses (out-group threat, partisan-motivated reasoning) that Experiment 1 did not address due to budget constraints. These hypotheses were tested in Experiment 2. Third, in Experiment 1, updating was measured by perceptions of out-group preferences on two policy issues. This method allows for a comparison between treatment and control groups, but not allow for a direct comparison of the estimated share of out-group members in certain ideological categories before and after treatment. Experiment 2 improved on this by asking respondents to estimate the share of Moderates in the out-group before (prior) and after the treatment (posterior).

As with Experiment 1, respondents were recruited on MTurk (Date: March 26, 2019). The total number of respondents was 3,072. 2,825 respondents could be assigned an in-group, using the same decision rule as in Experiment 1. As the theory only applies to individuals who underestimate the share of Moderates in the out-group, all analyses below focus on those respondents whose prior guess of Moderates was below the actual share (n =1,956). Among those, there are 1,133 Democrats and 823 Republicans. The procedure to assign respondents into Democratic/Republican in-group conditions was the same as in Experiment 1.

5In other words, I discarded the small number of Democrats who declared a conservative ideology and Republicans who declared a liberal ideology (about 5 percent of ANES respondents).
3.4.3 Outcome measurement

A battery of outcome measurements was used to capture both affect- and issue-based dimensions of polarization. The exact question wordings can be found in Appendix B.2.

Measures of updating

Two methods were used to measure whether respondents updated their beliefs about the out-group (Hypothesis 1). First, in Experiment 1 respondents were asked to estimate the policy preference of an “average” out-group member for two political issues (tax policy, racial justice). These policy issues and their wordings were borrowed from the most similar existing study Ahler and Sood (2018). This variable was then transformed to measure polarization by calculating the absolute distance between a response and the mid-point of the scale. Thus, a smaller value on this variable would suggest a more centrist perception of the out-group.

Second, in Experiment 2 updating was also assessed more directly by asking respondents to guess the share of out-group Moderates before and after the treatment. While the first method captures changing beliefs by measuring the difference between treatment and control groups, the second method is a within-subject measurement of updating.

Measures of affective polarization

To test Hypothesis 2, respondents were asked two types of questions. First, a feeling thermometer with a scale of 0-100 was used to assess emotions towards out-group politicians, out-group supporters and President Donald Trump.

Second, following Levendusky (2017), respondents were asked to rate whether the following attributes describe the out-group: American, Intelligent, Honest, Open-minded, Generous, Hypocritical, Selfish, Mean. The order of these adjectives was
randomized in the questionnaire. Each adjective’s applicability was measured on a three-point scale (Applies to most, Applies to some, Does not apply). For the analysis, I add the scores of the first five (“positive”) and last three (“negative”) adjectives, which yields two composite indices of positive and negative traits.

**Measures of compromise and reasoning**

To test Hypothesis 3, respondents were asked to consider an issue that they associate with the out-group and then rate the likelihood that the out-group will implement their agenda on this issue. A seven-point Likert scale was used to capture this perceived likelihood.

**Measures of compromise and reasoning**

To test Hypothesis 4, two distinct methods were used across the experiments. The first method asks respondents to state their preference on two policy issues (tax policy, criminal justice). Analogously to Hypothesis 1, the variable is transformed by calculating the absolute distance of a response to the mid-point of the scale. For example, a respondent who chooses the centrist option is assigned a value of zero, respondents who choose the most liberal or most conservative option are both assigned a value of two.

The second method leverages a factorial treatment design. All respondents were asked about their level of support for the 2018 First Step Act, a bipartisan criminal justice bill. A randomly selected group of respondents was then primed to consider in-group losses by adding the following cue: Republicans/Democrats had to make concessions to reach this compromise. This treatment was assigned orthogonally to the main treatment (correction of misperceptions). Hypothesis 1 is tested by estimating the interaction effect between the main treatment and the n-group loss cue.
Finally, Hypothesis 5 is only tested in Experiment 2 and also builds on the factorial design of the study. All respondents were asked about their level of support for the 2007 Energy Act, a bill that received support from Republican and Democratic lawmakers. Randomly selected respondents then received the following in-group cue: *It was widely supported by Republican/Democratic representatives* This treatment was assigned orthogonally to the main treatment (correction of misperceptions). This method of estimating partisan-motivated reasoning is modeled after Bolsen, Druckman, and Cook (2014). Hypothesis 1 can be reformulated as: The causal effect of partisan-motivated reasoning is smaller in the main treatment group, which can be implemented by estimating the interaction effect between the main treatment and the in-group support cue.

### 3.5 Results

#### 3.5.1 Experiment 1

In this section, I present the empirical findings of the first experiment. To facilitate comparisons across different outcomes, the outcome variables are standardized. All effect sizes can be interpreted as units of standard deviations on the respective outcome. Average treatment effects are estimated by fitting linear regressions of the standardized outcome variables on the binary treatment indicator $T_i$ and a vector of pre-treatment covariates $X_i^7$.

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \epsilon$$

Covariates are included to improve precision, however, as balance across treatment

---

6By demeaning their values and dividing them by their respective standard deviations in the full sample

7The pre-treatment covariates are age, gender, race, ideology, party, education, and prior misperceptions. 

8"Prior misperceptions" is a binary indicator representing whether respondents selected the correct answer in the initial question about out-group ideologies (see Figures B.9, B.10)
Out-group perception  
Affect  
Issues  

Racial justice (Perception of out-group)  
Tax policy (Perception of out-group)  
Feeling Thermometer (Out-group politicians)  
Feeling Thermometer (Out-group supporters)  
Feeling Thermometer (President Trump)  
Negative traits (Out-group)  
Positive traits (Out-group)  

Average Treatment Effect (in SD)  

Figure 3.2: OLS estimates of average treatment effects. Vertical bars indicate 90/95 percent confidence intervals.

Reducing perceived out-group unity

Figure 3.2 plots the main results of the out-group experiment. Estimated coefficients and standard errors are also summarized in column 1 of Table 3.2. The outcome variables fall into three categories. The first two estimates from the left are “first-stage” measures that capture whether respondents update their perceptions of out-group members after receiving the informational treatment. The estimates in the center box capture affective polarization towards the out-group. The remaining two effects on the right are measures of issue-based polarization. More detail on the construction of these outcome variables can be found in section 3.4.3.

First, in support of Hypothesis 1, the results suggest that respondents indeed update their posterior beliefs about out-group members after receiving the treatment. Using two distinct policy issues (tax policy, racial justice), this outcome is measured by calculating the absolute distance between the placement of an average out-group
Table 3.2: Out-group version of Experiment 1. OLS estimates of average treatment effects by partisan subgroups.

<table>
<thead>
<tr>
<th>Estimated treatment effects (in SD)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Full sample</td>
<td>Democrats</td>
<td>Republicans</td>
</tr>
<tr>
<td>(n = 2,071)</td>
<td>(n = 1,238)</td>
<td>(n = 833)</td>
<td></td>
</tr>
<tr>
<td>Feeling Thermometer</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Out-group politicians</td>
<td>0.099**</td>
<td>0.042</td>
<td>0.174***</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.049)</td>
<td>(0.066)</td>
</tr>
<tr>
<td>Out-group supporters</td>
<td>0.126***</td>
<td>0.079</td>
<td>0.165**</td>
</tr>
<tr>
<td></td>
<td>(0.041)</td>
<td>(0.050)</td>
<td>(0.064)</td>
</tr>
<tr>
<td>President Trump</td>
<td>−0.004</td>
<td>−0.009</td>
<td>−0.019</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.029)</td>
<td>(0.050)</td>
</tr>
<tr>
<td>Traits associated with out-group</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Negative</td>
<td>−0.196***</td>
<td>−0.155***</td>
<td>−0.272***</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.053)</td>
<td>(0.068)</td>
</tr>
<tr>
<td>Positive</td>
<td>0.145***</td>
<td>0.173***</td>
<td>0.109</td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
<td>(0.050)</td>
<td>(0.071)</td>
</tr>
<tr>
<td>Own policy preferences</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Racial justice</td>
<td>−0.043</td>
<td>−0.087</td>
<td>0.019</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.055)</td>
<td>(0.066)</td>
</tr>
<tr>
<td>Tax Policy</td>
<td>−0.068</td>
<td>−0.085</td>
<td>−0.037</td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
<td>(0.052)</td>
<td>(0.073)</td>
</tr>
<tr>
<td>Perceived average out-group policy preferences</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Racial justice</td>
<td>−0.215***</td>
<td>−0.254***</td>
<td>−0.181***</td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
<td>(0.058)</td>
<td>(0.061)</td>
</tr>
<tr>
<td>Tax policy</td>
<td>−0.133***</td>
<td>−0.162***</td>
<td>−0.103*</td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
<td>(0.058)</td>
<td>(0.062)</td>
</tr>
</tbody>
</table>

Note: *p<0.1; **p<0.05; ***p<0.01
member and the mid-point of the scale for the same issue. Thus, a negative coefficient suggests that the treatment causes perceptions of out-group preferences to become more centrist, or less polarized. Learning about true out-group heterogeneity makes respondents more likely to place an average out-group member closer to the center on each issue. The estimated effect sizes are -0.133 SDs for tax policy \((p = 0.002)\) and -0.215 SDs for racial justice \((p < 0.001)\). This suggests that factual information can be effective in changing perceptions of out-group beliefs. The estimated effect size is larger for the racial justice question, which is possibly due to relatively greater misperceptions of out-group preferences on this issue.

Second, the feeling thermometer estimates are statistically significant, which suggests that learning about the true degree of heterogeneity in the out-group reduces the prevalence of negative affect (in support of Hypothesis 2). The estimates are significant for feelings towards citizens (out-group supporters) as well as elites (out-group politicians). The effect sizes correspond to about 10 \((p = 0.015)\) and 13 percent \((p = 0.002)\) of a standard deviation, which corresponds to 2.3 to 3.0 points on the 100-point thermometer. Notably, changing perceptions of out-group unity has no effect on emotions towards President Trump \((p = 0.898)\), which highlights an interesting distinction in affect towards Trump versus other Republican politicians.

The positive and negative traits estimates provide additional support for Hypothesis 2. These measures reflect the willingness of respondents to ascribe certain adjectives to out-group members. Both measures are statistically significant with coefficients in the expected directions, suggesting that changing perceptions of unity can reduce negative affect, and increase positive affect towards the out-group. The likelihood of ascribing positive traits (American, Intelligent, Honest, Open-minded, Generous) is estimated to increase by about 0.15 standard deviations \((p < 0.001)\) and the likelihood of assigning negative traits (Hypocritical, Selfish, Mean) is estimated to decrease by about 0.2 standard deviations \((p < 0.001)\).
Figure 3.3: Prior rate of misperceptions, as measured by the share of respondents who select the incorrect info-graphic.

Third, the results on issue-polarization outcomes provide no evidence for Hypothesis 4. The two variables in the right box in Figure 3.2 measure issue-based polarization by calculating the absolute distance between a respondent’s policy preference and the center of the scale. Thus, a negative effect would suggest that respondents move closer to the center of the scale, which can be interpreted as a reduction in issue-based polarization. Overall, the results do not suggest that the treatment is effective in scaling back issue-based polarization. While the perception of average out-group members moves to the center (see above), respondents’ own policy preferences do not change to the same extent. The estimates have the expected sign (negative, i.e. moving towards the center), but fall short of statistical significance at the 95 percent confidence level. The estimated effect sizes are close to zero for tax policy ($-0.068 \, \text{SD}, p = 0.115$) and the racial justice issue ($-0.049, p = 0.310$). These null effects may mask heterogeneity between partisan groups, which I investigate in the next section.
Ahler and Sood (2018) find that misperceptions are more pronounced among Republicans. The same trend is present in the data underlying this experiment. Figure 3.3 shows the share of respondents who selected the incorrect answer when asked to estimate the ideological distribution of the out-group. Misperceptions are more than 20 percentage points more prevalent among Republicans (62.5%) than Democrats (39.4%). This asymmetry across partisan groups begs the question: Are informational treatments more effective in partisan groups that have higher misperception rates to begin with (in this case Republicans)? Figure 3.4 replicates the same analysis as above within Democratic and Republican subgroups. Columns 2-3 in Table 3.2 summarize the subgroup estimates. In each case, the linear model specification is equivalent to the main specification above.

The left box in Figure 3.4 shows that the heterogeneous effects of out-group perception are substantively similar to the full sample estimates, which suggests that
the treatment successfully updated beliefs about out-group unity for each subgroup. There is, however, one exception. The effect on tax policy perceptions is not significant for Republican respondents, which could be due to the lower number of observations in this subgroup, resulting in limitations on statistical power. As mentioned above, the sample contains about 50 percent more many Democrats (1,238) as Republicans (833).

Similarly, the results on affective polarization within partisan subgroups largely mirror the findings in the full sample. In most cases, estimated effect sizes are very similar for Democrats and Republicans. As in the full sample, there is no evidence of changes in affect towards Donald Trump. For Republicans this might be a logical consequence of how the treatment is constructed, i.e. receiving information about out-group unity should not affect their views of an in-group President. The fact that the estimated effect size is close to zero for Democratic respondents is substantively more interesting. Democrats may view Trump as an out-group representative who is distinct from other Republican politicians.

There is some suggestive evidence that reductions in affective polarization are larger for Republican respondents. The estimated increase in feelings towards out-group supporters is numerically higher for Republicans (0.165 SD) than Democrats (0.079 SD) and is only statistically different from zero for the former. Similarly, the estimated effect for out-group politicians is larger for Republicans (0.174 SD) than Democrats (0.045 SD). The difference is particularly striking for the likelihood of ascribing negative adjectives to the out-group. The effect size for Republicans (-0.272 SD) is the largest estimated coefficient among all outcomes. It is about 75 percent larger than the effect size for Democrats (-0.155 SD). The only inconsistency in these partisan differences is the measure on positive traits, which falls just short of statistical significance for Republicans. There are two possible explanations for this. First, Republican respondents are willing to let go of strongly negative associations
without an according increase in positive associations. Second, the inconsistency might be the result of limited statistical power in this relatively smaller subgroup. Aside from sample size limitations, the treatment itself could be a source of statistical noise. Misperceptions are defined as binary (respondents either chose the correct infographic or not), which makes it hard to communicate how far off their perceptions were. As I discuss in greater detail below, Experiment 2 provides a more fine-grained quantification of misperceptions, which provides an additional opportunity to explore partisan heterogeneity.

In sum, Experiment 1 provides suggestive evidence of partisan heterogeneity. Respondents in either group update their beliefs about out-group perceptions, but the estimated effect sizes are larger for Republicans than Democrats. First, the feeling thermometer measures are significant for Republicans, but not for Democrats. Second, the magnitudes of the traits effects are larger for Republicans. However, as can be seen in the overlapping confidence intervals in Figure 3.4, there is no conclusive evidence that the estimated effect sizes are statistically distinguishable from one another.

3.5.3 Experiment 2

Before discussing the results of Experiment 2, I assess whether respondents took up the treatment of this experiment as expected. In other words, do respondents update their beliefs about the out-group after receiving the informational treatment (Hypothesis 1)? To answer this question, I calculate the absolute mean distance between prior and posterior estimates of the share of Moderates in the out-group. As Figure 3.5 shows, the perception error decreases by almost half after treatment (13.8% to 7.0%), which suggests that respondents successfully update their beliefs about the out-group. The theory above applies only to those who underestimate the share of Moderates in the out-group ("under estimators"). For this reason, all analyses
Figure 3.5: Prior and posterior misperceptions, as measured by the absolute value of mean differences between the perceived share of out-group Moderates and the truth.

below exclude “overestimators” from the sample. In accordance with the ANES (2016) results in Section 3.2, there are far more underestimators (2,117) than overestimators (955) in the data. While the focus in the analyses is on underestimators, updating also occurs among overestimators whose absolute perception error decreases from 16.8% pre-treatment to 6.8% post-treatment.

To test Hypotheses 2-3, I fit linear regression models of each outcome variable on the binary treatment indicator $T_i$, and a vector of pre-treatment covariates $X_i^8$.

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + \epsilon$$

To test Hypotheses 4-5, I fit a treatment-interaction model that leverages the factorial design of the experiment. In each case, I estimate the interaction effect between the misperception treatment and a group cue $G_i$. To measure acceptance of

\footnote{The pre-treatment covariates are age, gender, race, ideology, party, education, and priors. Priors are the perceived shares of out-group members who are Very Conservative.Liberal, Conservative.Liberal and Slightly Conservative.Liberal.}
bipartisan compromise, I asked respondents whether they support the First Step Act, a 2018 bipartisan criminal justice bill, and randomly assigned a group-loss cue depending on their in-group ("Republicans/Democrats had to make several concessions to reach this compromise"). To measure partisan-motivated reasoning, respondents were asked whether they support the Energy Act, a bipartisan bill proposed in 2007, and randomly assigned to a group-support cue depending on their in-group ("It was widely supported by Republican/Democratic representatives."). Thus, the coefficient of interest in this model is $\beta_3$.

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 G_i + \beta_3 T_i \times G_i + \beta_2 X_i + \epsilon$$

**Main results**

In regards to Hypothesis 2, the results are mostly analogous to Experiment 1. In terms of affect, the estimated average effects suggest that the treatment reduced the association between the out-group and negative traits by 0.17 SD ($p < 0.001$) and
Table 3.3: Out-group version of Experiment 2. OLS estimates of average treatment effects by partisan subgroups.

<table>
<thead>
<tr>
<th></th>
<th>Estimated treatment effects (in SD)</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Full sample</td>
<td>Democrats</td>
<td>(2)</td>
<td>Republicans</td>
</tr>
<tr>
<td></td>
<td>(n = 1,956)</td>
<td>(n = 1,133)</td>
<td></td>
<td>(n = 823)</td>
</tr>
<tr>
<td><strong>Feeling Thermometer</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Out-group politicians</td>
<td>0.053 (0.042)</td>
<td>0.060 (0.052)</td>
<td>0.056 (0.067)</td>
<td></td>
</tr>
<tr>
<td>Out-group supporters</td>
<td>0.090** (0.042)</td>
<td>0.058 (0.053)</td>
<td>0.149** (0.066)</td>
<td></td>
</tr>
<tr>
<td>President Trump</td>
<td>0.035 (0.028)</td>
<td>-0.002 (0.028)</td>
<td>0.065 (0.050)</td>
<td></td>
</tr>
<tr>
<td><strong>Traits associated with out-group</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Positive</td>
<td>0.194*** (0.043)</td>
<td>0.177*** (0.053)</td>
<td>0.219*** (0.071)</td>
<td></td>
</tr>
<tr>
<td>Negative</td>
<td>-0.170*** (0.043)</td>
<td>-0.140** (0.056)</td>
<td>-0.208*** (0.067)</td>
<td></td>
</tr>
<tr>
<td><strong>Partisan-motivated reasoning</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction effect of treatment and in-group support cue (Energy Act)</td>
<td>-0.032 (0.083)</td>
<td>-0.054 (0.097)</td>
<td>0.041 (0.144)</td>
<td></td>
</tr>
<tr>
<td><strong>Accepting bipartisan compromise</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction effect of treatment and in-group loss cue (First Step Act)</td>
<td>0.085 (0.088)</td>
<td>0.077 (0.111)</td>
<td>0.072 (0.143)</td>
<td></td>
</tr>
<tr>
<td><strong>Perception of out-group threat</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Likelihood of out-group implementing agenda</td>
<td>-0.090** (0.044)</td>
<td>0.031 (0.053)</td>
<td>-0.259*** (0.076)</td>
<td></td>
</tr>
</tbody>
</table>

*Note:* 
*p<0.1; **p<0.05; ***p<0.01
increased the association with positive traits by 0.19 SD ($p < 0.001$). Furthermore, the treatment is estimated to increase feelings towards out-group supporters by 0.09 SD ($p = 0.031$), or about 1 point on the thermometer scale. In contrast to Experiment 1, the change in affect does not seem to extend to out-group politicians ($p = 0.207$).

The results also suggest that the treatment significantly reduces the prevalence of out-group threat (Hypothesis 3). Threat perception is measured by asking respondents about the likelihood that the out-group party succeeds in implementing its agenda. The treatment group experiences an estimated 0.09 SD reduction in out-group threat ($p = 0.043$). As I discuss below, this finding is primarily driven by Republican respondents.

To test Hypotheses 4-5, I estimate interaction effects as described above. Both interaction effects are statistically insignificant and close to zero, which further corroborates the results of Experiment 1. Providing information about out-group ideologies seems to be effective at shifting out-group perceptions and affect, but does not significantly reduce issue-based polarization, which was measured in Experiment 1 by surveying preferences on tax policy and racial justice.

**Heterogeneous effects**

In Experiment 1, misperceptions were more widespread among Republican respondents. Experiment 2 allows us to quantify misperceptions in each group by calculating the absolute difference between a respondent’s guess of the share of out-group Moderates and the correct share. Experiment 2 used these numerical values to give participants in the treatment group tailored feedback on how far off their guesses were from the correct shares. Respondents were shown their own guess, the actual share, and whether they had under- or overestimated the proportion (see Figure B.12 in Appendix).

This continuous measure of misperceptions makes it somewhat harder to classify
Figure 3.7: Prior rate of misperceptions, as measured by the share of respondents whose prior estimates of Moderate out-group members are within +/- 5 percentage points of the truth.

Figure 3.8: Out-group version of Experiment 2. OLS estimates of average treatment effects by partisan subgroups. Vertical bars indicate 90/95 percent confidence intervals. Subgroups include respondents who are partisan leaners.
respondents into two groups (those who have misperceptions and those who do not.) Virtually no respondent precisely guessed the share of out-group Moderates, and arguably, small deviations should not be interpreted as misperceptions. I consider all guesses within $\epsilon = 5$ percentage points as correct, and only those that fall outside of this band as misperceptions. Using this definition, I show the prevalence of misperceptions across partisan subgroups in Figure 3.7. As in Experiment 1, misperceptions are somewhat more common in the Republican subgroup. Notably, when I increase the error tolerance ($\epsilon = [7.5, 10]$), the share of Republicans with misperceptions stays relatively constant, while the proportion decreases among Democrats. This suggests that those Republicans who have misperceptions of Democrats have very strong misperceptions.

Table 3.4: Average perceived out-group shares and true shares. Underestimators are those who underestimate the share of Moderates in the out-group. Overestimators are those who overestimate the share of Moderates in the out-group.

<table>
<thead>
<tr>
<th></th>
<th>Average perceived share</th>
<th>True share</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Underestimators</td>
<td>Overestimators</td>
</tr>
<tr>
<td>Democratic respondents</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Republican Moderates</td>
<td>6.6%</td>
<td>32.8%</td>
</tr>
<tr>
<td>Republican Very Conservatives</td>
<td>41.5%</td>
<td>18.6%</td>
</tr>
<tr>
<td>Republican respondents</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Democratic Moderates</td>
<td>10.9%</td>
<td>47.5%</td>
</tr>
<tr>
<td>Democratic Very Liberals</td>
<td>38.8%</td>
<td>13.0%</td>
</tr>
</tbody>
</table>

This asymmetry in misperceptions motivates the investigation of heterogeneous effects by partisanship (Figure 3.8). Most subgroup estimates are similar to the main results. However, the results on out-group threat suggest a noticeable difference between Republicans and Democrats (right box in Figure 3.8). Learning about the true distribution of out-group ideologies is not associated with a significant change in threat perception among Democrats. However, the result is significant for Republi-
cans who experience an estimated decrease in out-group threat of 0.26 SD ($p = 0.001$). Furthermore, the difference between the effect among Democrats and Republicans is statistically significant, as indicated by the 95 percent confidence intervals in the plot. This result is particularly interesting since Republicans arguably had a higher capacity to implement their policy agenda at the time the survey was conducted, as they controlled the presidency and the Senate. One possible explanation is that out-group threat is higher among Republicans to begin with. The data actually suggest that the opposite is the case, as average threat perceptions in the control group are lower among Republicans (4.9 on 7-point scale) than Democrats (5.25). A two sample t-test suggests that this difference is statistically significant ($p < 0.001$). Thus, the finding is not a consequence of lower baseline threat among Republicans. Rather, it is likely that the treatment is more impactful for Republicans for two reasons. First, prior misperceptions are more widespread among Republicans (Figure 3.7) and second, the true share of Moderates is higher among Democrats than Moderates (Table 3.4).

### 3.6 Perceived in-group unity

So far, I have focused on the relationship between out-group misperceptions and polarization. In this section, I investigate whether similar misperceptions exist for in-groups. If citizens believe that fellow in-group members are more ideologically extreme or unified than they actually are, it could impact their publicly expressed preferences and willingness to deviate from group norms. In polarized settings, members of in-groups might perceive the preferences and attributes of co-partisans as relatively homogeneous due to sorting and communication echo chambers. Figure 3.9 compares perceptions of in-groups to self-placement of in-group members. The left plot contrasts how Democrats view other Democrats against how they place themselves. Analogously, the right plot compares how Republicans view other Republicans against their self-placement.
A pronounced difference in the red and blue density curves would be evidence for misperceived in-group unity. However, a comparison of Figure 3.1 and 3.9 suggests that in-group misperceptions are very small in comparison to out-group unity. There are some signs of in-group misperceptions among Democrats for specific policy issues such as healthcare and government services (see Appendix B.1.2), but overall, individuals have accurate perceptions of the ideologies and preferences of co-partisans, which aligns with research on in-groups in social psychology (Linville, Fischer, and Salovey 1989). This result yields an important empirical prediction. Since there is little misperception about in-group members, we should expect providing information about in-group to be insignificant in comparison the out-group treatment.

3.6.1 Hypotheses

Previous research has shown that perceived in-group unity can reduce individual uncertainty in decision-making and provide social status and group validation (Grant
and Hogg 2012; Lickel et al. 2000). To the extent that such misperceptions exist, they could affect partisan polarization in two ways.

Costs of disagreeing. In groups with high perceived unity, individuals think that the average position on a policy issue is shared by a large number of peers. As a consequence, diverging from the perceived group consensus means disagreeing with a large number of people. Disagreeing becomes doubly costly as one diverts not only from the perceived consensus, but also from a position that is shared by a large number of co-partisans. Thus, the cost of diverging from a group consensus is higher the more unified an in-group is perceived to be. Lowering the costs of “defecting” from the group norm by correcting views of actual group unity may thus incentivize in-group members to divert from group loyalty and accept bipartisan compromises.

Clarity of consensus. If perceived in-group unity is high, it is also clear what the in-group consensus is. In less unified partisan groups, identifying a “party position” is more challenging due to increased variance of preferences. While the first mechanism is about the number of people one might diverge from, this mechanism is about the ability to identify the opinion one would diverge from. In settings of perceived unity, this opinion is less ambiguous and easier to identify. Partisan cues are more effective when such group signals are stronger. By reducing perceived in-group entitativity, partisan cues may become less effective, thereby mitigating polarization.

Hypothesis 6 Reducing perceived in-group unity facilitates more moderate policy preferences because the costs of defection are lower.

Hypothesis 7 Reducing perceived in-group unity facilitates more moderate policy preferences due to increased noise around the group consensus.

3.6.2 Methodology

The experimental design to test these additional hypotheses is analogous to Experiment 1, except that it focuses on in-group rather than out-group shares. In this ver-
sion, the treatment group received information about the true degree of unity in their own party, whereas in the out-group version of Experiment 1 respondents received information about the party they do not identify with. To correct the degree of perceived in-group unity, Democrats were shown information about other Democrats, and Republicans were shown information about other Republicans. Specifically, Democratic respondents were shown waffle charts representing the proportion of Democrats in the moderate and far-left group (Figure B.9 in Appendix). Republicans were shown analogous charts with the proportion of Republicans in the moderate and far-right group (Figure B.10 in Appendix).

Respondents were recruited through MTurk on December 21, 2018. The resulting sample contains 2,256 who could be associated with an in-group. Self-identify as partisans. 1,374 identify as Democrats, 763 as Republicans. Treatment assignment again took place conditional on partisanship and ideology, with the same procedure for centrist Independents as described above. For additional descriptive statistics and balance tables, see Appendix B.8.

Reducing perceived in-group unity

Figure 3.10 and the first column in Table 3.5 summarize the main results of this version of the experiment. While the out-group version of Experiment 1 (Section 3.5) successfully shifted some aspects of affective and issue-based polarization, the in-group version does not point to similar results.

On most outcome measures, the estimates are null effects, with one exception. The estimates suggest that the treatment shifted perceptions on in-group preferences towards racial justice. This might be because in-group misperceptions are greater for this issue than the tax policy issue, making the treatment relatively more informative. Importantly, as can be seen in Figure 3.11, this effect is driven almost entirely by the Democratic subset of the sample. There are no other notable heterogeneities in
Table 3.5: In-group version of Experiment 1. OLS estimates of average treatment effects by partisan subgroups.

<table>
<thead>
<tr>
<th></th>
<th>Estimated treatment effects (in SD)</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Full sample (n = 2,246)</td>
<td>(2) Democrats (n = 1,428)</td>
<td>(3) Republicans (n = 818)</td>
<td></td>
</tr>
<tr>
<td>Feeling Thermometer</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Out-group politicians</td>
<td>-0.028 (0.039)</td>
<td>-0.061 (0.049)</td>
<td>-0.002 (0.064)</td>
<td></td>
</tr>
<tr>
<td>Out-group supporters</td>
<td>-0.037 (0.039)</td>
<td>-0.020 (0.048)</td>
<td>-0.075 (0.069)</td>
<td></td>
</tr>
<tr>
<td>President Trump</td>
<td>-0.045 (0.028)</td>
<td>-0.034 (0.027)</td>
<td>-0.065 (0.055)</td>
<td></td>
</tr>
<tr>
<td>Traits associated with in-group</td>
<td>0.050 (0.041)</td>
<td>0.059 (0.051)</td>
<td>0.023 (0.069)</td>
<td></td>
</tr>
<tr>
<td>Negative</td>
<td>-0.057 (0.041)</td>
<td>-0.068 (0.052)</td>
<td>-0.040 (0.070)</td>
<td></td>
</tr>
<tr>
<td>Positive</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Own policy preferences</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Racial justice</td>
<td>-0.014 (0.040)</td>
<td>-0.004 (0.052)</td>
<td>-0.026 (0.064)</td>
<td></td>
</tr>
<tr>
<td>Tax Policy</td>
<td>0.076* (0.042)</td>
<td>0.077 (0.052)</td>
<td>0.086 (0.069)</td>
<td></td>
</tr>
<tr>
<td>Perceived average in-group policy preferences</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Racial justice</td>
<td>-0.089** (0.042)</td>
<td>-0.150*** (0.052)</td>
<td>0.004 (0.071)</td>
<td></td>
</tr>
<tr>
<td>Tax policy</td>
<td>-0.022 (0.043)</td>
<td>-0.011 (0.046)</td>
<td>-0.020 (0.085)</td>
<td></td>
</tr>
</tbody>
</table>

Note: *p<0.1; **p<0.05; ***p<0.01
the results. As in the full sample, the in-group treatment does not seem to change measures of affective or issue-based polarization.

There are two possible explanations for the lack of findings in the in-group version of the experiment. First, as discussed above, misperceptions of unity within groups are much less pronounced than for out-groups. Thus, the treatment carries less informational value, which decreases its potential to shift priors. Second, to the extent that in-group misperceptions exist, individuals may be more reluctant to incorporate new information and update their perception of in-group unity. While the data do not allow us to distinguish between these two explanations, there is some evidence that the first reason is driving the results. As noted above, the in-group treatment was effective in shifting in-group perceptions on the racial justice issue. In-group misperceptions are likely to be greater on this issue than on tax policy. Thus, it is possible that the treatment is effective in in- and out-group settings, but that the set of policy domains where in-group misperceptions exist is much smaller.
3.7 Conclusion

This study has investigated the extent of misperceptions about out-group ideologies in the US electorate and implemented experimental interventions to correct these misperceptions. In general, most citizens underestimate the share of moderates in their political out-group, thereby overestimating the degree of unity in the out-group. As shown in the experimental data, these misperceptions are more pronounced among Republicans than Democrats.

The experiments successfully led participants to update out-group perceptions and reduced absolute perception error by about 50% in the treatment condition (Experiment 2). As a consequence, negative affect towards out-group representatives decreased, and positive affect increased. These results are particularly robust for affect towards out-group supporters. Furthermore, the treatment significantly reduced out-group threat, as measured by the perceived likelihood of out-group success at implementing their agenda. While the effects on affect and threat are significant and

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Figure 3.11: OLS estimates of average treatment effects by partisan subgroups. Vertical bars indicate 90/95 percent confidence intervals. Subgroups include respondents who are partisan leaners.
robust, there is no equivalent decrease in issue-based polarization. The treatment did not cause participants to choose more centrist policy positions, and it did not affect their likelihood to accept bipartisan compromises and use partisan-motivated reasoning.

These results provide two avenues for future research. First, laboratory (in-person) studies could be used to replicate these results. A lab setting would allow researchers to use alternative outcome measures, such as behavioral games and speech analysis. Second, while the experimental interventions in this study focused on the out-group distribution of ideology, misperceptions also exist with respect to specific policy issues (see Appendix B.1.1). Future experimental studies could be designed to correct misperceptions in the realm of different policy issues, which could potentially have heterogeneous impacts on polarization.

Lastly, the results provide useful insights for policy-makers and civil society. While programs that “bring together” citizens from different partisan groups may be an effective way to reduce polarization, their effects are probably strongest when careful attention is paid to the composition of the out-groups that individuals encounter in such settings. This study suggests that inter-partisan contact is more effective when individuals are exposed to out-group members who represent a broad range of views. Misperceptions about the distribution of moderate out-group members are high, but can be overcome by demonstrating that Moderates are more common than assumed. As opposed to previous findings on “backfiring” effects, this study shows that such information successfully leads to accurate updating, which gives hope to future efforts aiming to depolarize the US electorate.
A.1 Consequences of the reform on electoral outcomes

Ultimately, how did the voting age reform affect electoral outcomes? In other words, what would a counterfactual 2013 election without this reform have looked like? In order to answer this question, we perform another set of simulations. Since
young people vote differently than older people in Austria (SORA and ISA 2013), and we have shown above that habituation effects are heterogeneous by party affiliation, shifts in electoral consequences are to be expected. The effect of the voting age reform in 2008 on election outcomes in 2013 comes from two distinct groups: (1) the estimated 128,800 16- and 17-year-olds who voted in 2013 and would not have been able to vote absent the reform (the mechanical effect) and (2) the 52,016 21- and 22-year-olds who likely would not have turned out in 2013 had they not been enfranchised in 2008 (the habituation effect). Using the vote choices expressed by respondents under 30 in a SORA and ISA (2013) survey as providing an indication of party strength among young voters, we find that altogether, the youth vote does seem to have affected the 2013 election result (see Figure A.1). The upper figure contrasts the actual election results with a counterfactual scenario in which the voting age had not been lowered to 16 in 2008 and thus, the mechanical and habituation effect would not have altered the composition of voters in 2013. The lower figure shows the difference between counterfactual and actual results in percentage points. Particularly the FPÖ and the Greens benefited from the reform, while the impact on the two parties that granted the enfranchisement, SPÖ and ÖVP, was negative. While these effects are not large, and calculations are very approximate, one should keep in mind that these indicate the effects of the reform on the very next election only. Over time, as the cumulative habituation effect grows with each election (see e.g. Coppock and Green (2016)), this has the potential to substantially reshape the Austrian party system. We simulate the cumulative effects on turnout above, but in the absence of knowledge about future election results we are unable to replicate the same simulation for vote choice.

A.2 Survey questions
Table A.1: Survey questions used for dependent variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Wording</th>
<th>Scale</th>
</tr>
</thead>
<tbody>
<tr>
<td>Turnout</td>
<td>From today’s perspective, how certain is it that you will participate in the national elections in September? Please choose a number between 0 and 10, where 0 means “I definitely won’t participate” and 10 means &quot;I will definitely will participate.&quot; You can fine-tune your response with the numbers in between.</td>
<td>0 to 10</td>
</tr>
<tr>
<td>Self-placement</td>
<td>In politics we often talk about “left” and “right” Where would you place yourself on a scale from 0 to 10, where 0 means “left” and 10 means “right.”</td>
<td>Original scale: 0-10. Modified to measure polarization by calculating absolute deviations from the midpoint, i.e. scale 0-10.</td>
</tr>
<tr>
<td>Party affinity</td>
<td>In Austria some people feel close to a political party, although they may now and then vote for a different party. In general, do you feel close to a political party? [If yes:] Which party is that? [If no:] Is there one particular party you feel a little closer to than other parties?</td>
<td>0 to 10</td>
</tr>
</tbody>
</table>
Appendix to Paper 3

Perceived unity of policy preferences

Perceived out-group unity

Figure B.1: The density curves are smoothed by a kernel with bandwidth 0.5. The red curves are derived from Question number V161185/V161186: Where would you place [Hillary Clinton/Donald Trump] on this issue?. The blue curves are derived from Question number V161184: Where would you place yourself on this scale, or haven’t you thought much about this?. All variables are measured on the same seven-point scale: 1. Government insurance plan, 2.-6. unlabeled, 7. Private insurance plan. Source: ANES (2016)
Figure B.2: The density curves are smoothed by a kernel with bandwidth 0.5. The red curves are derived from Question number V161190/V161191: Where would you place [Hillary Clinton/Donald Trump] on this issue? The blue curves are derived from Question number V161189: Where would you place yourself on this scale, or haven’t you thought much about this? All variables are measured on the same seven-point scale: 1. Government should see to jobs and standard of living, 2.-6. unlabeled, 7. Government should let each person get ahead on own. Source: ANES (2016)
Figure B.3: The density curves are smoothed by a kernel with bandwidth 0.5. The red curves are derived from Question number V161179/V161180: Where would you place [Hillary Clinton/Donald Tramp] on this issue?. The blue curves are derived from Question number V161178: Where would you place yourself on this scale, or haven’t you thought much about this?. All variables are measured on the same seven-point scale: 1. Government should provide many fewer services, 2.-6. unlabeled, 7. Government should provide many more services Source: ANES (2016)
B.1.2 Perceived in-group unity

![Graph showing density curves for Democrats and Republicans](image)

Figure B.4: The density curves are smoothed by a kernel with bandwidth 0.5. The red curves are derived from Question number V161185/V161186: Where would you place [Hillary Clinton/Donald Trump] on this issue?. The blue curves are derived from Question number V161184: Where would you place yourself on this scale, or haven’t you thought much about this?. All variables are measured on the same seven-point scale: 1. Government insurance plan, 2.-6. unlabeled, 7. Private insurance plan. Source: ANES (2016)
Figure B.5: The density curves are smoothed by a kernel with bandwidth 0.5. The red curves are derived from Question number V161190/V161191: Where would you place [Hillary Clinton/Donald Trump] on this issue? The blue curves are derived from Question number V161189: Where would you place yourself on this scale, or haven’t you thought much about this? All variables are measured on the same seven-point scale: 1. Government should see to jobs and standard of living, 2.-6. unlabeled, 7. Government should let each person get ahead on own. Source: ANES (2016)
Figure B.6: The density curves are smoothed by a kernel with bandwidth 0.5. The red curves are derived from Question number V161179/V161180: Where would you place [Hillary Clinton/Donald Trump] on this issue?. The blue curves are derived from Question number V161178: Where would you place yourself on this scale, or haven’t you thought much about this?. All variables are measured on the same seven-point scale: 1. Government should provide many fewer services, 2.-6. unlabeled, 7. Government should provide many more services Source: ANES (2016)
B.2 Outcome measures: Question wordings

B.2.1 Affect

Feeling thermometer
Please rate how favorable and warm you feel towards these people: [Democratic/Republican Party supporters, Democratic/Republican Party Politicians, President Donald Trump]. Ratings between 50 degrees and 100 degrees mean that you feel favorable and warm toward the person. Ratings between 0 degrees and 50 degrees mean that you don’t feel favorable toward the person and that you don’t care too much for that person. You would rate the person at the 50 degree mark if you don’t feel particularly warm or cold toward the person. (Iyengar, Sood, and Lelkes 2012)

Traits
Rate whether the following attributes describe [Democrats/Republicans]: American, Intelligent, Honest, Open-minded, Generous, Hypocritical, Selfish, Mean. 1. Describes most [Democrats/Republicans], 2. Describes some [Democrats/Republicans], 3. Does not describe [Democrats/Republicans]. (Levendusky 2017)

B.3 Issues

Issue self-placement (Tax policy)
What do you personally believe about taxes? 1. Decrease federal income taxes on just the highest earners, keeping the tax rate the same on all others; 2. Decrease federal tax rates for all income groups; 3. Maintain current levels of federal income taxes on all; 4. Increase federal income taxes on the highest earners, keeping the tax rate the same on all others; 5. To address inequality, establish a national maximum income by taxing all income over a certain amount at 100 percent.] (Ahler and Sood
**Issue self-placement (Racial justice)**

What do you personally believe about racial and civil rights policy? 1. Any laws protecting racial groups should be repealed, including all voting rights and civil rights legislation; 2. Non-discrimination laws in universities and workplaces should be repealed; 3. The government should investigate and punish racial discrimination by universities and employers, but hiring or admissions based on race should be illegal; 4. Universities and employers should be encouraged to consider applicants’ backgrounds to improve diversity, but no quotas should be set; 5. The government should mandate an affirmative action program in education and the workplace to ensure that certain numbers of underrepresented minorities are hired/admitted; 6. In addition to affirmative action, the government should provide cash payments to minority groups as reparations for slavery and other past injustices. *(Ahler and Sood 2018)*

**Accepting bipartisan compromise**

We are next going to ask you what you think about parts of the First Step Act, a criminal justice reform bill that passed the House and Senate in December 2018 with bipartisan support.

[Randomized group-loss cue]. It includes the following provisions:

- Eases the three strikes rule: People with three or more convictions, including for drug offenses, get 25 years instead of life.

- Good time credits allow well-behaved inmates to cut their prison sentences by an additional week for each year they’re incarcerated.

- Earned time credits for more vocational and rehabilitative programs. Those credits will allow them to be released early to halfway houses or home confine-
Randomized group-loss cue: Democrats/Republicans had to make several concessions to reach this compromise.

Partisan motivated reasoning
We are next going to ask you what you think about parts of the 2007 Energy Independence Act.

[Randomized group-support cue]. It includes the following provisions:

- Requires U.S. automakers to boost gas mileage to 35 miles per gallon for all passenger cars by 2020, which is a 40% increase.
- Funds for research and development of solar and geothermal energy, and for the increased production of biofuels.
- Provides small businesses loans toward energy efficiency improvements.

Randomized group-support cue: It was widely supported by Republican/Democratic representatives.

B.4 Out-group threat

Likelihood of implementing agenda
For this question, please think about what you consider as the Democratic/Republican political agenda.

How likely do you think it is that Democrats/Republicans will implement their agenda? (1) Extremely likely, (2) Moderately likely, (3) Slightly likely, (4) Neither likely nor unlikely, (5) Slightly unlikely, (6) Moderately unlikely, (7) Extremely unlikely.
B.5 Out-group perception

**Average out-group member (Tax policy)**
Which of the following statements do you think comes closest to what the average [Democratic/Republican] Party supporter believes about taxes? 1. Decrease federal income taxes on just the highest earners, keeping the tax rate the same on all others; 2. Decrease federal tax rates for all income groups; 3. Maintain current levels of federal income taxes on all; 4. Increase federal income taxes on the highest earners, keeping the tax rate the same on all others; 5. To address inequality, establish a national maximum income by taxing all income over a certain amount at 100 percent. (Ahler and Sood 2018)

**Average out-group member (Racial justice)**
Which of the following statements do you think comes closest to what the average [Democratic/Republican] Party supporter believes about racial and civil rights policy? 1. Any laws protecting racial groups should be repealed, including all voting rights and civil rights legislation; 2. Non-discrimination laws in universities and workplaces should be repealed; 3. The government should investigate and punish racial discrimination by universities and employers, but hiring or admissions based on race should be illegal; 4. Universities and employers should be encouraged to consider applicants’ backgrounds to improve diversity, but no quotas should be set; 5. The government should mandate an affirmative action program in education and the workplace to ensure that certain numbers of underrepresented minorities are hired/admitted; 6. In addition to affirmative action, the government should provide cash payments to minority groups as reparations for slavery and other past injustices. (Ahler and Sood 2018)
B.6 Experiment 1: Graphics and treatments

Figure B.7: Screenshot of prior information question which asks respondents about their prior perceived unity of Democrats. Shown to all Republican respondents in the out-group version of the experiment, and to all Democratic respondents in the in-group version.
Figure B.8: Screenshot of prior information question which asks respondents about their prior perceived unity of Republicans. Shown to all Democratic respondents in the out-group version of the experiment, and to all Republican respondents in the in-group version.

We now reveal which image is correct.

The left image shows where Republicans think Democrats stand politically.
The right image shows where Democrats actually stand.

Figure B.9: Screenshot of treatment which informs respondents about the true degree of unity among Democrats. Shown to all Republican respondents in the out-group version of the experiment, and to all Democratic respondents in the in-group version. In the in-group version, the headline in the left pane is altered to: “You may think that most Democrats are relatively liberal.”
Figure B.10: Screenshot of treatment which informs respondents about the true degree of unity among Republicans. Shown to all Democratic respondents in the out-group version of the experiment, and to all Republican respondents in the in-group version. In the in-group version, the headline in the left pane is altered to: “You may think that most Republicans are relatively conservative.”
B.7 Experiment 2: Graphics and treatments

Screenshot below are for respondents whose in-group are Democrats. The treatment for Republicans is analogous (different numbers on out-group shares).

![Figure B.11: Screenshot of prior information question which asks respondents about their prior perceived unity of Republicans.](image)

Please guess what percentage (%) of these Republicans falls into each of the following categories. In other words, out of 100, how many do you think fall into a given category?

The total of your answers should add up to 100%

<table>
<thead>
<tr>
<th>Percentage (%) of Republicans</th>
<th>0</th>
<th>10</th>
<th>20</th>
<th>30</th>
<th>40</th>
<th>50</th>
<th>60</th>
<th>70</th>
<th>80</th>
<th>90</th>
<th>100</th>
</tr>
</thead>
<tbody>
<tr>
<td>Very Conservative</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0</td>
</tr>
<tr>
<td>Conservative</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0</td>
</tr>
<tr>
<td>Slightly Conservative</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0</td>
</tr>
<tr>
<td>Moderate</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0</td>
</tr>
<tr>
<td>Total</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.0</td>
</tr>
</tbody>
</table>

Figure B.11: Screenshot of prior information question which asks respondents about their prior perceived unity of Republicans.
We now reveal what percentages are correct.

<table>
<thead>
<tr>
<th>You guessed:</th>
<th>In reality:</th>
</tr>
</thead>
<tbody>
<tr>
<td>7.6% of Republicans are <em>Moderate</em>.</td>
<td>16.5% of Republicans identify as <em>Moderate</em>.</td>
</tr>
<tr>
<td>30.4% of Republicans are <em>Very Conservative</em>.</td>
<td>10.1% of Republicans identify as <em>Very Conservative</em>.</td>
</tr>
</tbody>
</table>

**You underestimated the number of Moderates among Republicans.**

In general, the majority of Democrats in the US electorate underestimate the number of Moderates among Republicans.

(Source: American National Election Survey 2016, a large nationally representative independent survey)

---

Figure B.12: Screenshot of treatment which informs respondents about the true degree of unity among Republicans. Shown to all Democratic respondents in the treatment group.
## B.8 Experiment 1: Balance tables

Table B.1: Out-group version of experiment 1. Balance table of pre-treatment covariates. All values rounded to two decimal places. T-tests are conducted for the null hypothesis of no difference in proportions between treatment and control group.

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Treatment group</th>
<th>Control group</th>
<th>t-test (p-value)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Proportion of sample</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age (18-24)</td>
<td>0.08</td>
<td>0.07</td>
<td>0.38</td>
</tr>
<tr>
<td>Age (25-34)</td>
<td>0.38</td>
<td>0.38</td>
<td>0.87</td>
</tr>
<tr>
<td>Age (35-44)</td>
<td>0.26</td>
<td>0.27</td>
<td>0.55</td>
</tr>
<tr>
<td>Age (45-54)</td>
<td>0.16</td>
<td>0.15</td>
<td>0.57</td>
</tr>
<tr>
<td>Age (55-64)</td>
<td>0.08</td>
<td>0.09</td>
<td>0.62</td>
</tr>
<tr>
<td>Age (65 or over)</td>
<td>0.03</td>
<td>0.04</td>
<td>0.51</td>
</tr>
<tr>
<td>Male</td>
<td>0.44</td>
<td>0.46</td>
<td>0.38</td>
</tr>
<tr>
<td>Female</td>
<td>0.56</td>
<td>0.54</td>
<td>0.34</td>
</tr>
<tr>
<td>Asian</td>
<td>0.06</td>
<td>0.05</td>
<td>0.45</td>
</tr>
<tr>
<td>Black/African American</td>
<td>0.09</td>
<td>0.10</td>
<td>0.38</td>
</tr>
<tr>
<td>Hispanic/Latino</td>
<td>0.09</td>
<td>0.10</td>
<td>0.79</td>
</tr>
<tr>
<td>Native American</td>
<td>0.01</td>
<td>0.01</td>
<td>0.80</td>
</tr>
<tr>
<td>Other</td>
<td>0.01</td>
<td>0.02</td>
<td>0.33</td>
</tr>
<tr>
<td>White</td>
<td>0.77</td>
<td>0.76</td>
<td>0.99</td>
</tr>
<tr>
<td>Less than high school</td>
<td>0.00</td>
<td>0.00</td>
<td>0.25</td>
</tr>
<tr>
<td>High school graduate</td>
<td>0.12</td>
<td>0.09</td>
<td>0.04</td>
</tr>
<tr>
<td>Some college</td>
<td>0.22</td>
<td>0.24</td>
<td>0.28</td>
</tr>
<tr>
<td>2-year degree</td>
<td>0.13</td>
<td>0.13</td>
<td>0.97</td>
</tr>
<tr>
<td>4-year degree</td>
<td>0.38</td>
<td>0.37</td>
<td>0.64</td>
</tr>
<tr>
<td>Advanced degree</td>
<td>0.14</td>
<td>0.16</td>
<td>0.19</td>
</tr>
</tbody>
</table>
Table B.2: In-group version of experiment 1. Balance table of pre-treatment covariates. All values rounded to two decimal places. T-tests are conducted for the null hypothesis of no difference in proportions between treatment and control group.

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Treatment group</th>
<th>Control group</th>
<th>t-test (p-value)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Proportion of sample</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age (18-24)</td>
<td>0.15</td>
<td>0.17</td>
<td>0.31</td>
</tr>
<tr>
<td>Age (25-34)</td>
<td>0.35</td>
<td>0.36</td>
<td>0.51</td>
</tr>
<tr>
<td>Age (35-44)</td>
<td>0.23</td>
<td>0.23</td>
<td>0.92</td>
</tr>
<tr>
<td>Age (45-54)</td>
<td>0.14</td>
<td>0.13</td>
<td>0.59</td>
</tr>
<tr>
<td>Age (55-64)</td>
<td>0.09</td>
<td>0.07</td>
<td>0.16</td>
</tr>
<tr>
<td>Age (65 or over)</td>
<td>0.03</td>
<td>0.03</td>
<td>0.88</td>
</tr>
<tr>
<td>Male</td>
<td>0.45</td>
<td>0.47</td>
<td>0.33</td>
</tr>
<tr>
<td>Female</td>
<td>0.54</td>
<td>0.53</td>
<td>0.45</td>
</tr>
<tr>
<td>Asian</td>
<td>0.08</td>
<td>0.09</td>
<td>0.11</td>
</tr>
<tr>
<td>Black/African American</td>
<td>0.12</td>
<td>0.10</td>
<td>0.08</td>
</tr>
<tr>
<td>Hispanic/Latino</td>
<td>0.07</td>
<td>0.06</td>
<td>0.49</td>
</tr>
<tr>
<td>Native American</td>
<td>0.01</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>Other</td>
<td>0.01</td>
<td>0.02</td>
<td>0.29</td>
</tr>
<tr>
<td>White</td>
<td>0.69</td>
<td>0.71</td>
<td>0.26</td>
</tr>
<tr>
<td>Less than high school</td>
<td>0.00</td>
<td>0.00</td>
<td>0.57</td>
</tr>
<tr>
<td>High school graduate</td>
<td>0.11</td>
<td>0.09</td>
<td>0.26</td>
</tr>
<tr>
<td>Some college</td>
<td>0.27</td>
<td>0.26</td>
<td>0.31</td>
</tr>
<tr>
<td>2-year degree</td>
<td>0.13</td>
<td>0.12</td>
<td>0.47</td>
</tr>
<tr>
<td>4-year degree</td>
<td>0.36</td>
<td>0.38</td>
<td>0.14</td>
</tr>
<tr>
<td>Advanced degree</td>
<td>0.13</td>
<td>0.14</td>
<td>0.42</td>
</tr>
</tbody>
</table>
### B.9 Experiment 2: Balance tables

Table B.3: Balance table of pre-treatment covariates. All values rounded to two decimal places. T-tests are conducted for the null hypothesis of no difference in proportions between treatment and control group.

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Treatment group</th>
<th>Control group</th>
<th>t-test (p-value)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Proportion of sample</td>
<td>Proportion of sample</td>
<td></td>
</tr>
<tr>
<td>Age (18-24)</td>
<td>0.14</td>
<td>0.14</td>
<td>0.48</td>
</tr>
<tr>
<td>Age (25-34)</td>
<td>0.41</td>
<td>0.38</td>
<td>0.13</td>
</tr>
<tr>
<td>Age (35-44)</td>
<td>0.23</td>
<td>0.24</td>
<td>0.84</td>
</tr>
<tr>
<td>Age (45-54)</td>
<td>0.11</td>
<td>0.12</td>
<td>0.18</td>
</tr>
<tr>
<td>Age (55-64)</td>
<td>0.09</td>
<td>0.08</td>
<td>0.32</td>
</tr>
<tr>
<td>Age (65 or over)</td>
<td>0.02</td>
<td>0.03</td>
<td>0.14</td>
</tr>
<tr>
<td>Male</td>
<td>0.45</td>
<td>0.45</td>
<td>0.83</td>
</tr>
<tr>
<td>Female</td>
<td>0.55</td>
<td>0.55</td>
<td>0.71</td>
</tr>
<tr>
<td>Asian</td>
<td>0.08</td>
<td>0.07</td>
<td>0.48</td>
</tr>
<tr>
<td>Black/African American</td>
<td>0.09</td>
<td>0.09</td>
<td>0.97</td>
</tr>
<tr>
<td>Hispanic/Latino</td>
<td>0.07</td>
<td>0.07</td>
<td>0.90</td>
</tr>
<tr>
<td>Native American</td>
<td>0.01</td>
<td>0.01</td>
<td>0.98</td>
</tr>
<tr>
<td>Other</td>
<td>0.01</td>
<td>0.02</td>
<td>0.39</td>
</tr>
<tr>
<td>White</td>
<td>0.73</td>
<td>0.73</td>
<td>0.78</td>
</tr>
<tr>
<td>Less than high school</td>
<td>0.01</td>
<td>0.00</td>
<td>0.60</td>
</tr>
<tr>
<td>High school graduate</td>
<td>0.10</td>
<td>0.10</td>
<td>0.58</td>
</tr>
<tr>
<td>Some college</td>
<td>0.24</td>
<td>0.25</td>
<td>0.63</td>
</tr>
<tr>
<td>2-year degree</td>
<td>0.12</td>
<td>0.11</td>
<td>0.65</td>
</tr>
<tr>
<td>4-year degree</td>
<td>0.38</td>
<td>0.38</td>
<td>0.83</td>
</tr>
<tr>
<td>Advanced degree</td>
<td>0.15</td>
<td>0.15</td>
<td>0.85</td>
</tr>
</tbody>
</table>
Bibliography


Cho, Youngho. 2014. “To know democracy is to love it: A cross-national analysis of democratic understanding and political support for democracy.” *Political Research Quarterly* 67 (3): 478–488.


Grasso, Maria T. 2014. “Age, period and cohort analysis in a comparative context: Political generations and political participation repertoires in Western Europe.” Electoral Studies 33 (March): 63–76.


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