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Essays on Democratic Resilience in an Era of Demographic Change

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*A Víctor,
por esta etapa, y las que vienen.*

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Deciding to pursue a PhD was not a straightforward decision for me. It took me time to understand whether this was the right path, and even more time to accept what such a path would demand from me and from those around me. These five years have not been easy. They have been intellectually demanding, emotionally difficult, and often uncertain. But they have also been formative in ways I could not have anticipated.

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Introduction

Democracies worldwide are facing profound challenges shaped by demographic shifts, cultural transformations, and political polarization. Chief among these is the rise of illiberalism and authoritarianism, which has become a global threat to democratic institutions and norms ([Merkel & Lührmann, 2021](#)). As [Levitsky and Ziblatt \(2018\)](#) argue in *How Democracies Die*, modern democratic erosion is often subtle and incremental, occurring not through abrupt coups but rather through the systematic weakening of democratic checks and balances by leaders who rise to power through elections. These leaders, often cloaked in populist rhetoric, consolidate power by undermining independent judiciaries, sidelining parliaments, and exploiting state institutions to entrench their authority.

Populist rhetoric plays a central role in corroding trust in democratic systems by attacking pluralism, delegitimizing opposition, and fostering disillusionment with traditional governance structures ([Norris & Inglehart, 2019](#)). This dynamic is evident in the erosion of democratic norms and practices under leaders like Viktor Orbán in Hungary, Erdoğan in Turkey, and Donald Trump in the United States. While these figures claim to represent the “true will of the people,” their actions often erode liberal institutions and imperil the rule of law. This process rarely involves outright democratic collapse but instead relies on the incremental erosion of democratic norms through actions that are often legally justified but fundamentally illiberal ([Bermeo, 2016](#)).

The proliferation of literature on democracy’s erosion, decline, and crisis underscores a shared concern: the contemporary threat to democracy is not external but internal, emanating from within systems designed to protect it and advancing at a slow pace. Demographic changes, such as increased migration and ageing populations, further exacerbate these trends. [Norris and Inglehart \(2019\)](#) describe these dynamics through a “cultural backlash” thesis, which interprets the rise of populism as a reaction by socially conservative groups to rapid cultural and demographic shifts. By tapping into these grievances, populist leaders challenge the legitimacy of democratic institutions, portray elected representatives and the judiciary as disconnected elites, and deepen societal polarization.

Migration and population ageing have become especially salient in this regard. Migration, often framed by populist leaders as a threat to national identity, can fuel xenophobic rhetoric that polarizes societies and weakens social cohesion ([Mudde, 2019](#)). Ageing populations, in turn, challenge welfare systems and contribute to perceptions of intergenerational imbalance, prompting debates about whose preferences and interests elected institutions represent ([Buchmeier & Vogt, 2024](#); [Hess, Nauman, & Steinkopf, 2017](#); [Stockemer & Sundström, 2023](#)). These demographic trends generate political reactions that can be exploited by elites and that shape attitudes toward institutions, representation, and democratic norms. As these reactions unfold, they may reinforce polarization, heighten

intergroup tensions, and erode mutual toleration, a foundational democratic principle emphasized by [Levitsky and Ziblatt \(2018\)](#). Mutual toleration, or the recognition of political opponents as legitimate actors, is increasingly under strain, and its weakening is often accompanied by discriminatory behaviours, rising hostility toward minorities, and challenges to the peaceful coexistence of diverse communities.

This dissertation investigates the resilience of democracies by examining how demographic change shapes political behaviour, institutional trust, and social cohesion. It does so by analysing three interconnected dimensions: the political consequences of migration, the representational implications of generational divides, and the societal effects of far-right electoral success. Together, these chapters trace an arc that begins with how demographic pressures influence electoral behaviour, continues with how demographic imbalances alter perceptions of democratic performance, and concludes with how political outcomes affect tolerance and minority rights. This full sequence illustrates the broader argument of the dissertation: demographic change interacts with political institutions and elite strategies in ways that can either reinforce or undermine the foundations of liberal democracy.

The central analytical argument uniting the three essays is that demographic change does not mechanically produce democratic backsliding. Instead, the relationship between demographic pressures and democratic outcomes is mediated by political institutions, party competition, and the normalization dynamics of the far-right ([Arzheimer, 2009](#); [Golder, 2003](#); [Meguid, 2005](#); [Mudde, 2019](#); [Valentim, 2024](#)). Whether migration fuels exclusionary politics depends on which parties claim the issue and how voters interpret it within a given competitive environment ([Meguid, 2005](#); [Van der Brug, Fennema, & Tillie, 2005](#)). Whether age gaps undermine democratic legitimacy depends on whether citizens read them as a failure of representation or as a correctable institutional deficit ([Claassen & Magalhães, 2022](#); [Clayton, O'Brien, & Piscopo, 2024](#); [Stockemer & Sundström, 2023](#)). Whether far-right electoral success generates hostility toward minorities depends on the broader political environment in which that success occurs and on which groups are most directly targeted by exclusionary rhetoric ([Dancygier, 2023](#); [Valentim, 2024](#)). Across all three chapters, the recurring finding is that it is not the demographic shock itself, but the political mediation of that shock, that determines whether democratic resilience holds or erodes. This has a concrete implication for the study of democratic resilience: understanding how democracies protect themselves from backsliding requires attention not only to structural conditions, but also to the institutional, competitive, and discursive mechanisms through which demographic change is converted into, or insulated from, exclusionary politics (e.g. [Golder, 2003](#); [Kitschelt & McGann, 1997](#); [Levitsky & Ziblatt, 2018](#); [Meguid, 2005](#); [Norris & Inglehart, 2019](#)).

The narrative uniting these essays centres on the interplay between demographic diversity and political systems. The first chapter investigates how migration influences far-

right electoral outcomes, showing that recent migration waves in Chile do not inevitably fuel far-right support. Instead, increases in migrant shares tend to reduce support for the far-right and strengthen the centre-right, indicating that voters distinguish between exclusionary reactions and more moderate, policy-oriented responses. Where mainstream parties retain credibility on questions of regulation and state capacity, migration-related concerns are channelled toward them rather than toward radical exclusion. The second chapter examines generational gaps in political leadership, demonstrating that visible age disparities between political elites and the broader public weaken perceptions of democratic representation and performance. Crucially, these effects are not confined to younger citizens but reflect a more general public aversion to representation deficits, suggesting that age gaps pose a broad legitimacy challenge to democratic institutions rather than a narrowly intergenerational conflict. The third chapter turns from the sources of far-right support to its social consequences. It examines whether far-right electoral victories can embolden hostility toward minorities by signalling that exclusionary views have gained democratic legitimacy. Focusing on U.S. House elections from 2006 to 2022, the chapter shows that far-right victories do not produce a uniform increase in hate crimes against all minority groups. Instead, their clearest effects are concentrated on LGBTQ+ individuals, especially in contexts where far-right rhetoric has become more normalized. Together, these essays illuminate the forces that determine whether democracies can withstand or succumb to the pressures of demographic change and political extremism.

This research makes several contributions to the fields of political science and political economy. It advances migration literature by providing causal evidence on how migrant inflows reshape competition within the right, challenging the assumption that migration inherently fuels far-right success (e.g. [Barone, D’Ignazio, De Blasio, & Naticchioni, 2016](#); [Edo, Giesing, Öztunc, & Poutvaara, 2019](#); [Halla, Wagner, & Zweimüller, 2017](#)). It enriches debates on representation by demonstrating how demographic gaps, such as age, influence trust in democratic institutions and perceptions of democratic performance, while showing that such effects operate through a general norm of descriptive representation rather than through purely self-interested generational reactions (e.g. [Clayton, O’Brien, & Piscopo, 2019](#); [Clayton et al., 2024](#); [Stockemer & Sundström, 2023](#)). Finally, it expands the literature on far-right politics by offering causal evidence that far-right legislative victories can heighten hostility toward minorities in selective, rhetoric-driven ways, even in the absence of direct local policy change, illuminating how electoral success interacts with social identity dynamics to reshape the conditions under which minority rights are exercised (e.g. [Bischof & Wagner, 2019](#); [Riaz, Bischof, & Wagner, 2024](#); [Valentim, 2024](#)). By addressing these issues, this dissertation provides a nuanced understanding of the tensions and opportunities involved in maintaining democratic resilience in the face of demographic change.

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Chapter 1

Skills, Diversity, and the Politics of Immigration: Electoral
Effects on the Far-Right in Chile

Skills, Diversity, and the Politics of Immigration: Electoral Effects on the Far-Right in Chile*

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Abstract

Does immigration increase far-right support? The migration literature has debated this question for decades, with little consensus on whether local exposure to migrants fuels anti-immigrant voting or through which mechanisms. Most existing evidence comes from Western Europe and the United States, while far less is known about how migration shapes electoral outcomes in Global South democracies, which host the majority of the world’s migrants and have increasingly experienced far-right mobilisation. This paper revisits the debate using recent Venezuelan and Haitian migration waves to Chile, a setting in which major migrant groups differ markedly in cultural proximity, skill composition, language, and social visibility, offering leverage on the electoral consequences of different migration profiles within a single national context. Using administrative migration records and a shift-share instrumental variables strategy, I estimate the causal effect of local migrant exposure on electoral outcomes in the 2017 and 2021 presidential elections. The results do not support the view that migration drives far-right voting. In local labour markets, increases in migrant shares reduce support for the far-right and increase support for the centre-right; at the municipal level, migration does not expand far-right support and mainstream parties continue to benefit. Culturally distant Haitian inflows do not generate a cultural-threat backlash and are consistently associated with lower far-right support across specifications. Overall, the findings indicate that exposure to migration reshapes competition within the right rather than fuelling far-right mobilisation.

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1 Introduction

In the past few decades, migration has become a central topic in political economy and political science, largely because of the rise of far-right parties that openly advocate anti-immigrant policies across Europe and the United States ([Alesina & Tabellini, 2024](#); [Arzheimer, 2018](#); [Cools, Finseraas, & Rogeberg, 2021](#); [Moriconi, Peri, & Turati, 2022](#); [Otto & Steinhardt, 2014](#)). Despite this growing interest, there is still no academic consensus as to whether immigration drives the far-right vote or the mechanisms by which it does so. While a few argue that contact with the migrant population reduces hostility and weakens support for exclusionary parties ([Lonsky, 2021](#); [Pagliacci & Bonacini, 2022](#); [Steinmayr, 2021](#); [Vertier, Viskanic, & Gamalerio, 2023](#)), others emphasise that fears of job competition ([Halla, Wagner, & Zweimüller, 2017](#)), pressure on public goods ([Barone, D’Ignazio, De Blasio, & Naticchioni, 2016](#); [Cremaschi, Retzl, Cappelluti, & De Vries, 2024](#); [Gennaro, 2025](#)), or cultural anxieties about neighbourhood change ([Brunner & Kuhn, 2018](#); [Mendez & Cutillas, 2014](#)) drive anti-immigrant voting.

A central limitation of this literature is that it is overwhelmingly concentrated in Western Europe and the United States, even though developing countries host roughly 85% of the world’s migrants ([United Nations High Commissioner for Refugees, 2022](#)). In these long-standing immigration destinations, migration has unfolded over decades, labour markets are highly regulated, welfare states are relatively expansive, and party systems often feature entrenched immigration cleavages. In such settings, political responses to immigration are shaped not only by local exposure but also by pre-existing attitudes, partisan cues, and media narratives, which complicates efforts to assess how migration influences electoral behaviour outside of these contexts. At the same time, even within industrialised democracies, economic and cultural mechanisms are sometimes difficult to disentangle empirically, since migrant inflows often bundle low-skilled status with cultural distance and involve multiple overlapping waves. As a result, natives may react differently depending on whether migrants are perceived as economic competitors, culturally distant outsiders, humanitarian victims, temporary residents, or permanent settlers ([Alrababa’h, Masterson, Casalis, Hangartner, & Weinstein, 2023](#); [Beaman, Onder, & Onder, 2022](#); [Dustmann & Görlach, 2016](#); [Newman, Hartman, Lown, & Feldman, 2015](#)). Much of what we know about these distinctions comes from survey experiments and conjoint designs that vary migrant attributes directly ([Alrababa’h et al., 2021](#); [Bansak, Hainmueller, & Hangartner, 2016](#); [Valentino et al., 2019](#)), rather than from observational evidence on how migration is experienced in local communities.

This paper contributes to the debate by turning to a newer and analytically advantageous setting, recent migration within Latin America. Since the early 2010s, a region traditionally characterised by emigration has experienced large-scale inflows from Venezuela and Haiti. As of May 2024, 7.7 million Venezuelans have fled their coun-

try (roughly 25.5% of the 2015 population)¹, and more than 78 percent have settled in Spanish-speaking neighbours (R4V, 2024). During the same period, over 650,000 Haitians left their country after the 2010 earthquake (UN, 2020), and a substantial share settled in Chile, partly because, until 2018, Chile uniquely allowed visa-free entry for Haitian nationals.² As Figure 1 shows, the migrant population in Chile increased ten-fold in less than a decade, reaching nearly nine percent of the population by 2020.

Crucially for assessing how different migration profiles shape electoral outcomes, Venezuelan and Haitian migrants differ sharply in attributes that theory identifies as politically relevant. Venezuelans share language, religion, and many cultural traits with Chileans, and they display substantial variation in skill levels. Haitians, by contrast, are overwhelmingly low-skilled, speak Haitian Creole which is not mutually intelligible with Spanish, and are racially distinct from most of the native population. These flows arrived rapidly and recently, and their contrasting attributes across cultural proximity, skill composition, language, and social visibility offer a degree of analytical leverage within a single national context that is rare in long-standing immigration destinations, where these dimensions tend to move together.

Chile is also a context of rising far-right mobilisation, a pattern mirrored in many democracies around the world, including Brazil, Argentina, and Turkey. The far-right *Partido Republicano*, which did not exist before 2015, received nearly 29% of the vote in the first round of the 2021 presidential election. This development raises a central question in the comparative literature: whether recent migration is contributing to the electoral success of exclusionary actors, or whether other political dynamics are at play. Addressing this question requires examining how migration shapes local electoral outcomes in places where migrants settle and become part of the local social and economic environment, rather than relying solely on aggregate national trends or attitudinal measures. Far-right mobilisation may still operate through fear, media narratives, or symbolic threat, making it essential to assess whether areas that experience greater migration are systematically more likely to support far-right candidates.

To answer this question, I combine administrative migration records by nationality with presidential election results measured at both the municipal and local labour market levels. This allows me to examine migration at a finer residential scale, where visibility and everyday exposure may be more salient, and at the level of economically integrated labour markets, where employment opportunities and commuting patterns are more likely to be shared. I implement a shift-share instrumental variables strategy following Altonji and Card (1991) and Card (2001). Because migrants are not randomly allocated across Chilean localities, the instrument isolates variation driven by national-level inflow shocks

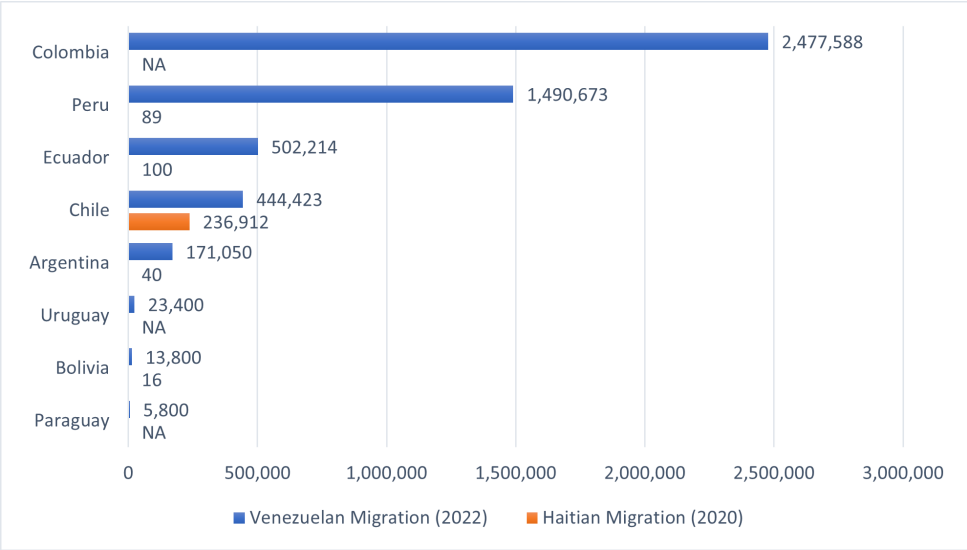
¹Based on World Bank population estimates for 2015: https://data.worldbank.org/indicator/SP.POP.TOTL?name_desc=true&locations=VE

²Haitians could enter Chile as tourists and subsequently apply for work permits and residence, unlike in most other Latin American countries.

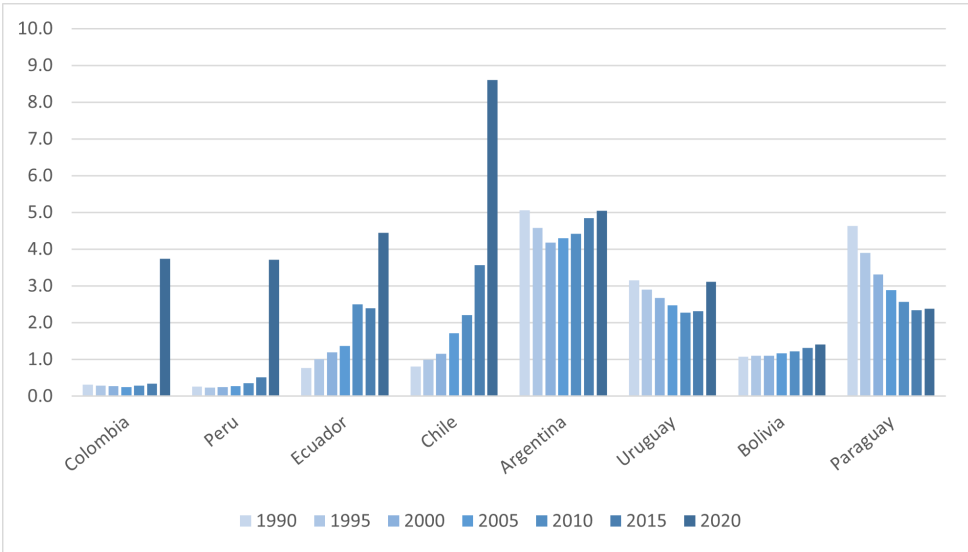
interacting with historical settlement patterns that predate the recent migration waves.

Figure 1: Distribution of Venezuelan and Haitian migrants by hosting country as of December 2022, and hosting countries migrant percent population from 1990 to 2020

(a) Distribution of Venezuelan and Haitian migrants by host country as of December 2022



(b) Trend of migrants as percent of population by country, 1990-2020



Source: Information compiled by R4V: Inter-Agency Coordination Platform for Refugees and Migrants from Venezuela jointly led by the UN Refugee Agency (UNHCR) and the International Organization for Migration (IOM) for Panel A. United Nations Department of Economic and Social Affairs, Population Division (2020). International Migrant Stock 2020, for Panel B.

The results show that immigration does not increase support for anti-immigrant far-right candidates in this new context. Instead, migration reduces far-right support and increases support for the centre-right. The decline in far-right support appears most clearly in local labour markets, where economic integration and shared employment pools make migration most directly relevant to labour market conditions. At the municipal level,

where residential proximity and everyday exposure to migrants are more salient, migration does not expand far-right support and is associated with gains for mainstream parties. Culturally distant Haitian inflows do not generate a cultural-threat backlash and, when compared to low-skilled Hispanic inflows within the same skill segment, continue to be associated with lower far-right support. Across specifications, the centre-right consistently benefits, suggesting that migration may channel aggregate support toward policy-oriented parties that promise control and order without endorsing radical exclusion. Turnout analyses indicate that these shifts reflect changes in partisan support rather than changes in participation.

Taken together, these findings indicate that recent migration to Chile has not translated into a far-right backlash in areas that experience higher migrant inflows. Rather than mechanically increasing support for exclusionary candidates, local exposure to migration appears to reorganise electoral competition within the right. This pattern is difficult to reconcile with accounts that treat immigration as an automatic trigger of far-right mobilisation, but it is consistent with the scope conditions emphasised earlier. When migration is recent and attitudes are still forming, local exposure may weaken exclusionary appeals through everyday interaction or familiarity, particularly when migrants are perceived as vulnerable. Moreover, in contexts where immigration-related concerns are framed less around redistribution and more around regulation, legality, and administrative control, aggregate support may shift toward parties that signal governing capacity rather than radical exclusion. Finally, where far-right actors have not fully consolidated ownership over immigration as a political issue, migration-related concerns may be expressed through shifts within the right-wing camp rather than through uniform gains for exclusionary parties.

This study advances the literature in three main ways. First, it provides new evidence on the relationship between migration and far-right voting in a Global South democracy where both large-scale immigration and far-right mobilisation are recent. By moving beyond the European and North American cases that dominate existing research, the paper speaks directly to the external validity of theories linking migration to far-right support and shows that such links are not inevitable in newer destination contexts. Second, by exploiting variation in the characteristics of major migrant groups within the same national setting, the analysis offers more leverage on the electoral consequences of different migration profiles than contexts in which economic disadvantage and cultural distance are tightly bundled. Venezuelan and Haitian migrants differ markedly across cultural proximity, skill composition, language, and social visibility, and the contrasting aggregate electoral responses to each group speak to debates about cultural threat, labour market competition, and local exposure in settings where these dimensions usually move together. Third, the findings highlight the importance of political mediation in shaping the electoral consequences of migration. Even when migration becomes salient and far-

right actors mobilise around exclusionary narratives, local exposure to migrants need not benefit the far-right if alternative political responses are available. This suggests that the electoral impact of migration depends not only on who migrates and where, but also on how migration-related concerns are articulated and absorbed within the party system, with broader implications for understanding when and why far-right mobilisation succeeds.

Understanding the conditions that connect migration to political behaviour is essential for designing policies that improve integration, reduce social tensions, and prevent the consolidation of xenophobic or exclusionary movements. This task is especially urgent in contexts where migration is recent, party systems are still adapting, and political responses to immigration are not yet fully institutionalised. The Chilean case, marked by unprecedented migration, rapid party-system transformation, and sharp variation in migrant characteristics, provides a rare opportunity to examine how local exposure to migration reshapes electoral competition before attitudes and political alignments fully crystallise. By showing that migration can reorganise competition within the right rather than mechanically fuelling far-right mobilisation, the paper sheds light on a broader global challenge facing many democracies that are becoming migration destinations for the first time.

2 Background

2.1 Attitudes toward immigration: established mechanisms

The literature offers several explanations for why immigration elicits hostile or supportive reactions among natives, and how these attitudes may translate into political behaviour. Following [Hainmueller and Hiscox \(2010\)](#), these accounts are often organised around economic mechanisms, sociocultural considerations, and concerns related to public order and security. Other work highlights the role of interpersonal contact and humanitarian considerations in shaping attitudes toward migrants.

A first set of accounts emphasises job market competition. Drawing on Heckscher–Ohlin models, early work argues that natives fear wage losses and job displacement when migrants compete for similar positions ([Dustmann & Preston, 2007](#); [Hainmueller & Hiscox, 2010](#); [Kessler, 2001](#); [Mayda, 2006](#); [Scheve & Slaughter, 2001](#)). Under this view, low-skilled natives should be particularly opposed to low-skilled immigration, and highly educated natives should be more hostile to high-skilled inflows. Empirical evidence, however, has challenged this simple mapping. [Hainmueller and Hiscox \(2007\)](#) and [Hainmueller and Hiscox \(2010\)](#) show that there is a general preference for high-skilled migrants, regardless of natives’ own skill levels. Other work suggests that labour market concerns are strongly sector specific rather than purely based on education. [Malhotra, Margalit, and Mo \(2013\)](#)

show for the United States that opposition to H1-B visas is concentrated in sectors where immigrants directly compete with natives. Similarly, [Dancygier and Donnelly \(2013\)](#) find that European natives employed in expanding sectors tend to support immigration more than those in declining sectors, indicating that perceptions of job scarcity in particular labour markets are central to the formation of anti-immigrant attitudes.

A second set of economic mechanisms is sociotropic. Here, natives worry less about their individual job prospects and more about the perceived burden of immigration on the state and public finances. Immigration may be associated with higher taxes, increased social spending, or concerns about the general state of the economy ([Citrin, Green, Muste, & Wong, 1997](#); [Facchini & Mayda, 2009](#); [Valentino et al., 2019](#)). The literature highlights several channels. One concerns fears that immigration worsens macroeconomic performance or increases fiscal costs ([Facchini & Mayda, 2009](#); [Hanson, Scheve, & Slaughter, 2007](#); [Valentino et al., 2019](#)). Another emphasises congestion of public goods such as health, education, and housing, concerns that may be particularly salient among low socioeconomic status natives in settings with limited provision ([Colantone & Stanig, 2018](#); [Cremaschi et al., 2024](#); [Dickson, Hobolt, De Vries, & Cremaschi, 2024](#); [Hainmueller & Hiscox, 2010](#)). Concerns over the distribution of scarce public resources, and perceptions of unfairness in access to them, may further politicise immigration even where labour market competition is limited, including through competition over public housing ([Gennaro, 2025](#)).

A third set of accounts focuses on ethnic and cultural threat. These approaches emphasise that attitudes toward migrants often reflect broader patterns of ethnocentrism and identity politics, and that natives may oppose immigration even when it does not represent a direct economic threat ([Valentino et al., 2019](#)). A large body of work in Western industrialised democracies documents persistent hostility toward culturally distant or visibly distinct groups ([Chandler & Tsai, 2001](#); [Citrin et al., 1997](#); [Dustmann & Preston, 2007](#); [Fetzer, 2000](#); [McLaren, 2003](#); [Sides & Citrin, 2007](#)). Studies emphasise different dimensions of difference. Some focus on physical appearance and racial cues ([Brader, Valentino, & Suhay, 2008](#); [Lee & Ottati, 2002](#); [Valentino et al., 2019](#)), while others highlight language and religion, especially attitudes toward Muslim migrants in predominantly Christian societies ([Adida, Lo, & Platas, 2019](#); [Bansak et al., 2016](#); [Chandler & Tsai, 2001](#); [Hainmueller & Hopkins, 2015](#)). Another strand stresses concerns that immigration may transform national culture in ways that natives perceive as threatening or incompatible with their values ([Campbell, Wong, & Citrin, 2006](#); [Card, Dustmann, & Preston, 2012](#); [Dustmann & Preston, 2007](#); [Sniderman, Hagendoorn, & Prior, 2004](#)).

A growing strand of work emphasises security concerns as a channel through which immigration may shape attitudes, alongside economic and sociocultural considerations. In this view, natives may oppose immigration because they perceive migrants, or particular migrant groups, as associated with crime, disorder, terrorism, or broader insecurity.

Some studies examine whether migrant inflows change objective levels of crime or violence (Bianchi, Buonanno, & Pinotti, 2012; Bove & Böhmelt, 2016; Zhou & Shaver, 2021), while others stress that perceptions of threat often respond strongly to selective exposure, media coverage, and stereotypes, even in the absence of corresponding increases in crime (Fasani, Mastrobuoni, Owens, & Pinotti, 2019; Helbling & Meierrieks, 2022; Ward, 2019). Fasani et al. (2019) notes that in many countries natives are more concerned that immigrants increase crime than that they affect unemployment or taxes, and that these concerns frequently rest on misperceptions about both crime rates and the size of the migrant population. In Europe, fears about crime are a central dimension of anti-immigration attitudes (Bianchi et al., 2012) and have been linked to support for exclusionary and far-right parties (Dinas & van Spanje, 2011). In Chile, this channel has received direct attention in recent work evaluating the relationship between migration and crime, which finds no systematic link between migrant inflows and criminal activity, despite the prominence of security narratives in political debate (Ajzenman, Dominguez, & Undurraga, 2023).³

Finally, a further set of arguments highlights the role of contact and humanitarian considerations in shaping attitudes toward immigration. Building on Allport (1954), this literature argues that sustained, meaningful contact with migrants can reduce prejudice and increase support for inclusion. Direct interactions in workplaces, schools, and neighbourhoods may challenge stereotypes and lower perceived threat. Related work distinguishes exposure from contact, showing that the political consequences of hosting migrants depend on whether natives experience direct interaction rather than mere proximity (Steinmayr, 2021). In parallel, several studies show that humanitarian frames and information about persecution or conflict in sending countries can generate more favourable attitudes toward refugees and forced migrants (Alrababa'h et al., 2021; Bansak et al., 2016; Newman et al., 2015). Natives may be more willing to accept migrants when they are perceived as victims of violence, repression, or natural disasters.

Taken together, this literature identifies multiple pathways through which immigration may shape native attitudes. These mechanisms are analytically distinct, but in practice, they often overlap because migrant groups differ simultaneously in skill composition, race, language, legal status, perceived vulnerability, and public visibility. The next question is how these attitudinal responses translate into electoral outcomes, and whether relationships documented in long-standing immigration destinations travel to contexts where migration is more recent and political responses are still forming.

³In Latin America more broadly, the association between migration and criminal violence is sometimes amplified by the involvement of gangs and cartels in irregular migration routes. For example, media coverage has reported interactions between Venezuelan migrants and Colombian armed groups in border regions. “Crisis in Venezuela: How Colombian mafias and armed groups are taking advantage of Venezuelan migrants,” BBC Mundo.

2.2 From attitudes to votes: immigration and far-right electoral support

A large body of research examines whether immigration affects electoral outcomes, especially support for far-right parties. Most of this evidence comes from Europe and North America, where immigration has been politically salient for decades and where far-right actors have often built durable electoral coalitions around nativist and exclusionary appeals (Barone et al., 2016; Caselli, Fracasso, & Traverso, 2021; Dinas, Matakos, Xefteris, & Hangartner, 2019; Edo, Giesing, Öztunc, & Poutvaara, 2019; Gerdes & Wadensjö, 2008; Mayda, Peri, & Steingress, 2022; Moriconi et al., 2022; Otto & Steinhardt, 2014; Vasilakis, 2018). These studies analyse cases such as the National Front in France and Vlaams Belang in Belgium (Edo et al., 2019), Brexit in the United Kingdom (Langella & Manning, 2016), and the election of Donald Trump in the United States (Mayda et al., 2022). The common premise is that local exposure to migrants can translate into electoral support for parties that mobilise around immigration.

Survey-based research complements this evidence by showing that anti-immigrant attitudes are a strong predictor of far-right voting in both single-country and comparative settings (Billiet & De Witte, 1995; Cutts, Ford, & Goodwin, 2011; Kai, 2008; Mayer & Perrineau, 1992; Mughan & Paxton, 2006; Norris, 2005; Van der Brug & Fennema, 2003; Van der Brug, Fennema, & Tillie, 2000). These studies establish a robust individual-level link between exclusionary attitudes and support for far-right parties, suggesting that immigration-related concerns can be politically mobilised when appropriate electoral vehicles are available.

Early work related far-right vote shares to the size of local migrant populations and reached mixed conclusions. Some studies find that larger migrant shares are associated with higher support for far-right or radical right parties (Golder, 2003; Knigge, 1998; Lubbers, Gijsberts, & Scheepers, 2002; Lubbers & Scheepers, 2002; Swank & Betz, 2003). Others report no clear relationship (Arzheimer & Carter, 2006; Lucassen & Lubbers, 2012; Norris, 2005; Rydgren, 2008), or suggest that far-right parties perform better where minority groups remain relatively small (Bustikova, 2014). A central concern in this literature is endogeneity, since migrants are not randomly distributed across communities and often settle in areas with stronger labour markets, more tolerant populations, and distinct political trajectories (Cools et al., 2021; Golder, 2016).

More recent work therefore relies on research designs aimed at causal identification, including instrumental variables strategies and quasi-experimental approaches (Barone et al., 2016; Dustmann, Vasiljeva, & Piil Damm, 2019; Edo et al., 2019; Halla et al., 2017; Moriconi et al., 2022; Otto & Steinhardt, 2014; Schaub, Gereke, & Baldassarri, 2021). Many of these studies find that immigration increases support for far-right or radical right parties, but the evidence also highlights substantial heterogeneity in both magnitude

and mechanisms. Some contributions attribute effects to a combination of economic and cultural channels. For example, [Barone et al. \(2016\)](#) show that increases in the immigrant share in Italian municipalities raise support for the Lega Nord and interpret this as reflecting labour market competition, pressure on local services, and cultural distance. [Edo et al. \(2019\)](#) find that low-skilled immigration in France increases support for the National Front, especially when inflows originate from non-Western countries.

A related strand emphasises economic mechanisms more directly. Several studies argue that labour market and welfare-state concerns are central to far-right mobilisation ([Bredtmann, 2022](#); [Halla et al., 2017](#); [Mayda et al., 2022](#); [Pieroni, Roig, & Salmasi, 2023](#); [Roupakias & Chletsos, 2020](#)). For Austria, [Halla et al. \(2017\)](#) show that local immigration explains a non-trivial share of variation in support for the Freedom Party and interpret this effect as driven by fears of adverse labour market consequences. For the United States, [Mayda et al. \(2022\)](#) find that low-skilled immigration increases Republican vote shares, while high-skilled immigration has the opposite effect, consistent with job competition and fiscal concerns.

Another strand highlights cultural mechanisms, arguing that the composition of migrant inflows, rather than their overall size, is crucial for understanding electoral effects ([Brunner & Kuhn, 2018](#); [Devillanova, 2021](#); [Harmon, 2018](#); [Mendez & Cutillas, 2014](#); [Rozo & Vargas, 2021](#); [Sekeris & Vasilakis, 2016](#)). [Mendez and Cutillas \(2014\)](#) show that in Spain, inflows from culturally similar Latin American countries increase support for left-wing parties, while migration from North Africa benefits the mainstream right. [Brunner and Kuhn \(2018\)](#) similarly find that culturally distant migration is more strongly associated with anti-immigrant voting.

At the same time, a growing set of studies reports null or even negative effects of immigration on far-right support ([Gessler, Tóth, & Wachs, 2022](#); [Hennig, 2021](#); [Lonsky, 2021](#); [Pagliacci & Bonacini, 2022](#); [Russo, 2021](#); [Usta, 2022](#); [Vertier et al., 2023](#)). Many interpret these patterns through contact mechanisms, arguing that sustained interpersonal interaction can weaken prejudice and reduce the appeal of exclusionary political appeals ([Allport, 1954](#); [Steinmayr, 2021](#)). Much of this work focuses on refugee inflows, where humanitarian frames and reception arrangements may facilitate contact or sympathy. [Vertier et al. \(2023\)](#) show that the redistribution of asylum seekers across French municipalities reduced support for the National Front, particularly where reception centres were small. Only a few studies using general migration flows and shift-share instruments find negative effects on far-right parties ([Lonsky, 2021](#); [Pagliacci & Bonacini, 2022](#)).

Taken together, existing research offers conflicting predictions. Economic and cultural threat accounts suggest that immigration, especially when low-skilled or culturally distant, should boost support for far-right parties. Contact and humanitarian mechanisms allow for the possibility that local exposure may temper hostility or even erode far-right support. Importantly, however, most of this evidence is drawn from contexts

where immigration has long been politicised and where party systems already contain well-defined electoral vehicles for exclusionary reactions. This matters because the electoral consequences of migration depend not only on how citizens perceive migrants, but also on which parties claim the issue, how they frame it, and whether voters view those parties as credible vehicles for responding to migration related concerns.

2.3 Why context matters: new migration destinations and political aggregation

The mixed evidence from Europe and North America suggests that the political consequences of immigration are not determined solely by the presence of migrants. Rather, they depend on how migration-related concerns are filtered through institutional settings, party systems, and state capacity. Existing theories and empirical regularities are largely built on a set of shared contextual features that characterise long-standing immigration destinations in the Global North. These contexts typically combine sustained immigration over long periods, high levels of state capacity and welfare provision, the presence of institutionalised far-right parties, and well-defined political cleavages organised around immigration. Within such environments, immigration has been politicised over extended periods, stereotypes and group boundaries have hardened, and far-right actors have often established ownership over immigration as a political issue, offering voters a clear electoral option that links migration to cultural decline, economic threat, or insecurity (Golder, 2016; Mudde, 2019).

These conditions need not hold in newer migration destinations, particularly in emerging democracies. Where immigration is recent and rapid, attitudes toward migrants may be less crystallised and more responsive to local interaction. Lower state capacity and weaker welfare systems can further reshape how migration is perceived (Hainmueller & Hopkins, 2014; Valentino et al., 2019). In contexts characterised by high levels of labour informality, migrants may contribute little to public revenues through taxation, while still increasing demand for social services, housing, or public order. As a result, concerns about immigration may centre less on long-term redistribution and more on regulation, legality, and the ability of the state to manage inflows effectively. At the same time, party systems in these settings are often still adapting to immigration as a political issue. Far-right actors may be present, but they do not necessarily monopolise exclusionary appeals, and mainstream parties may retain credibility on issues of control and governance (Meguid, 2005).

The legal and social incorporation of migrants is also likely to shape these dynamics. Migrants who can work formally, access residence permits, enroll children in schools, or use public health services may become visible not only as workers and neighbours, but also as users of state services. This can generate perceptions of competition over resources and services, particularly in contexts where welfare access is already perceived as scarce

or unequally distributed (Dancygier, 2010; Hainmueller & Hopkins, 2014; Hopkins, 2010). At the same time, irregular or precarious legal status may reduce migrants' access to formal employment and benefits, shifting public concern toward legality, border control, and administrative capacity rather than toward welfare competition alone (Guiraudon & Lahav, 2000; Huysmans, 2000). For this reason, the political meaning of migration in newer destination contexts may depend on how states regulate entry, residence, employment, and access to social rights (Koopmans, 2010; Sainsbury, 2012).

Cultural considerations further complicate the comparison between long-standing and newer migration destinations. On the one hand, societies in the Global North may be more accustomed to cultural heterogeneity due to decades of immigration, which can make attitudes less responsive to marginal changes in migrant composition. On the other hand, migration to these countries often involves large cultural, linguistic, or religious distances, which can make cultural threat particularly salient when activated. In contrast, migration in the Global South frequently occurs between neighbouring countries and within regions, where cultural, religious, or linguistic differences may be less pronounced. In such contexts, cultural distance may be a weaker or more ambiguous trigger of exclusionary reactions, especially when migration is recent and everyday interaction is common.

Taken together, these features generate a set of conditional expectations that differ from those commonly derived from the Global North literature. Economic threat may matter more in settings with weak safety nets and informal labour markets, but it may also be politically redirected toward demands for regulation rather than exclusion. Cultural threat may be less deterministic when migrants are culturally proximate or when attitudes are still forming. Humanitarian considerations may persist longer when displacement is recent, visible, and framed as temporary or crisis-driven. Security concerns, while potentially salient, may focus attention on state capacity, border control, and enforcement rather than on support for far-right parties per se. Since these contextual features shape both attitudes and the political options available to voters, I expect the link between immigration and far-right support to be more contingent and mediated in newer migration destinations than in long-established ones.

This perspective implies that the mapping from immigration to attitudes, and from attitudes to votes, is context-dependent rather than mechanical. The same underlying concerns about jobs, culture, or security may be expressed through different political channels depending on which parties are perceived as capable of responding to them. As a result, immigration may shift support within the party system, particularly within the right, rather than uniformly benefiting far-right actors.

2.4 Party competition and immigration in Chile

Chile exemplifies many features of a new migration destination in an emerging democratic context. Large-scale immigration is a recent phenomenon, and the speed of demographic change has outpaced the development of stable political cleavages around migration. At the same time, Chile combines a growing but fiscally constrained welfare system, decreasing but persistent labour market informality, and a party system that, despite more than three decades of democratic competition, is still adapting to new social and political challenges. Historically understood as a culturally homogeneous society, Chile has experienced a rapid diversification of its population without a corresponding consolidation of political routines or partisan alignments around immigration.

The institutional context of migration policy is important for understanding this process. During the period studied, Chile did not operate a reception system comparable to asylum dispersal or accommodation systems in parts of Europe. Migrants were not hosted by the state in reception centres or hotels while their status was processed. Instead, most entered through visa exemptions, temporary permits, tourist entry, followed by residence applications, employment-based residence applications, or later through more restrictive consular visa procedures.⁴ Once in Chile, migrants had access to core public services, especially health care and education, regardless of whether their status was regular, pending, or irregular. At the same time, access to residence and formal labour market incorporation depended on legal status and administrative procedures, including temporary residence applications based on an employment contract or a formal job offer. This meant that migration was politically visible both as a question of labour market incorporation and public service access, and as a question of state capacity, regularisation, and control.

As in other emerging democracies that have recently become migration destinations, these structural features are likely to shape how immigration enters political competition. Immigration has become a salient political issue, and parties across the right have sought to articulate the concerns and anxieties it provokes. To provide descriptive evidence on party positioning, I draw on the Manifesto Project, which codes party platforms by policy emphasis, including positive and negative references to immigration.⁵ This evidence shows that the far-right expressed the most consistently restrictive position on immigration, while the centre-right adopted a more limited restrictive emphasis in 2021. Yet, despite the salience of the issue, far-right ownership over immigration may remain incomplete. The recency of large-scale migration, combined with the absence of long-standing exclusionary

⁴For Haitian nationals, the main change introduced in April 2018 was the requirement of a consular tourist visa, requested at the Chilean consulate in Haiti. Before this reform, Haitian nationals could enter Chile without applying for a tourist visa in advance. A similar consular tourist visa requirement was later introduced for Venezuelan nationals in June 2022.

⁵See Appendix A for the Manifesto Project coding of immigration related statements in Chilean party platforms.

cleavages, may limit the extent to which anti-immigrant attitudes consolidate into a stable and exclusive electoral base. At the same time, centre-right parties may actively contest the issue by advancing policy-based responses centred on regulation, enforcement, and state capacity, rather than adopting overtly exclusionary rhetoric. Under these conditions, immigration may be less likely to be monopolised by the far-right than in many long-standing immigration destinations, where exclusionary frames have hardened over time, and far-right parties have become the dominant electoral vehicle for migration-related discontent.

Within this competitive environment, far-right actors have nevertheless placed immigration at the centre of their discourse, portraying recent inflows as a source of disorder, criminality, and cultural incongruity. This strategy aligns with broader comparative patterns in which far-right parties mobilise around nativism and the protection of an essentialised national community (Mudde, 2019; Pirro, 2023). In Chile, far-right leaders have emphasised precisely these themes, drawing explicit links between migration, organised crime, and a breakdown of social order.⁶

The mainstream centre-right, by contrast, has adopted a more complex and policy-oriented stance. Its rhetoric is more moderate, yet it has combined this moderation with concrete and visible interventions aimed at managing migration and reasserting state control. As the governing coalition during the 2017 and 2021 election cycles, the centre-right introduced reforms requiring visa applications to be initiated abroad, created new residence categories, enabled expedited expulsions, and introduced the Democratic Responsibility Visa for Venezuelan nationals.⁷ At the same time, it maintained a generally pro-migration discourse toward Venezuelans, justified in part by ideological opposition to the Venezuelan authoritarian regime.⁸ These combined signals gave the centre-right a basis for presenting itself as capable of restoring order and managing migration without fully embracing the far-right's more radical framing.

This pattern of competition has important implications for how immigration translates into electoral outcomes. In contexts where welfare provision is constrained, labour markets remain partially informal, and party systems are still consolidating, concerns about immigration may be channelled toward demands for regulation and control rather than toward outright exclusion. Voters who prioritise order and governance, but are wary of extreme positions, may therefore find centre-right parties more attractive than far-right actors. I therefore expect that, in Chile, immigration is more likely to reallocate aggregate support within the right than to generate uniform gains for exclusionary parties. More

⁶For example, far-right leaders frequently link crime policy to migration policy. One illustration is the Republican Party's announcement of the plan "Cero Narcotráfico" to complement the fight against organised crime and irregular migration; see <https://partidorepublicanodechile.cl>.

⁷Presidential address announcing the reform: <https://prensa.presidencia.cl>.

⁸For example, President Piñera stated "Vamos a seguir recibiendo venezolanos en Chile" in an interview with Deutsche Welle; see <https://www.dw.com/es>.

broadly, this expectation may travel to newer migration destinations where mainstream parties retain credibility on questions of state capacity, legality, and migration control.

The empirical analysis that follows evaluates these expectations by examining how recent migration is associated with electoral outcomes across two theoretically relevant spatial scales. Local labour markets capture economically integrated areas where employment opportunities, commuting patterns, and potential labour market interactions are more likely to be shared. Municipalities capture a finer residential scale where migrant visibility, neighbourhood composition, and everyday local exposure may be more salient. I also exploit variation in the characteristics of major migrant groups in Chile, particularly differences in skill levels and cultural proximity, to assess whether electoral responses vary across types of migrant exposure. Because Venezuelan and Haitian migrants differ along several dimensions simultaneously, including cultural distance, race, language, and perceptions of vulnerability, the analysis is designed to identify whether exposure to each type of inflow produces distinct electoral responses at the aggregate level, rather than to trace effects back to a single migrant attribute. This is, in itself, a contribution relative to contexts where economic disadvantage and cultural distance are tightly bundled and difficult to separate even partially. The design cannot directly observe individual perceptions of economic competition, cultural threat, or interpersonal contact, but it can show whether aggregate electoral patterns are more consistent with a far-right backlash, a reallocation of support toward more moderate right-wing alternatives, or a limited electoral response to migration, and whether those patterns vary systematically with the skill composition and cultural profile of the incoming population.

3 Data and Measurement

This study combines administrative and census data to examine how migration relates to electoral outcomes in the 2017 and 2021 presidential elections in Chile. The analysis relies on three primary sources: individual-level immigration records from the National Migration Service, population and demographic data from the National Statistics Institute (INE), and municipal-level electoral results from the Electoral Service (SERVEL).

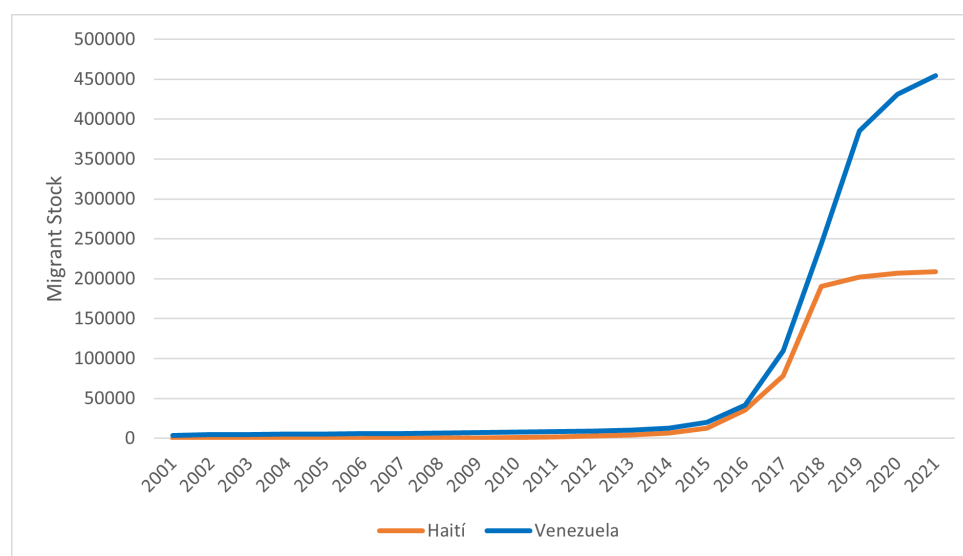
Migration to Chile To measure the magnitude and composition of migration at the local level, I use administrative records on 1,826,719⁹ visa applications to the National Migration Service of the Chilean Department of State from 2000 to 2021. These records include basic demographic information such as nationality, date of birth, municipality of residence, gender, occupation, educational level, and date of application. To account for

⁹The original dataset contains 2,674,391 visa applications. I removed duplicates based on nationality, date of birth, gender, education, occupation, and professional activity. Also, the dataset contains 749,729 permanent residence permits granted to foreigners who have remained in the country for at least 12 months with a temporary visa.

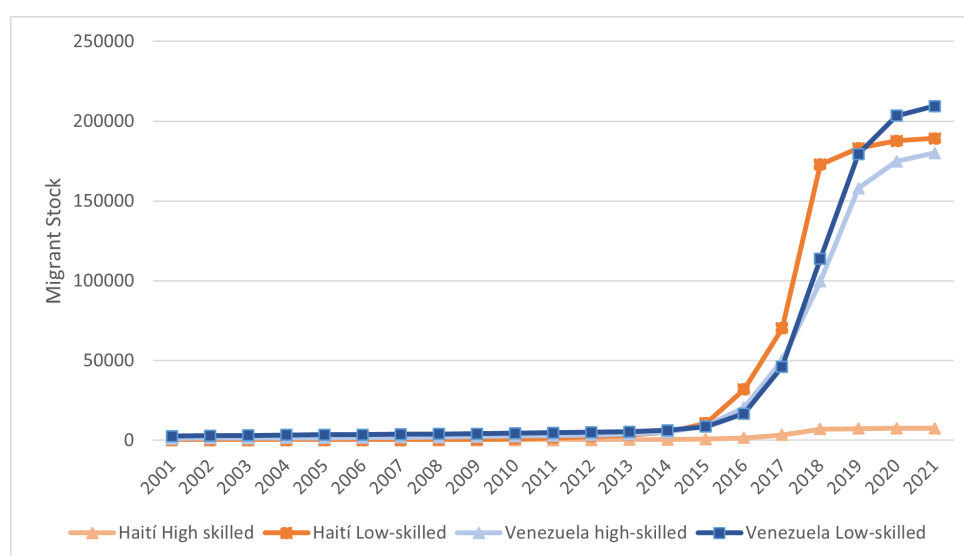
the initial migration shares, I use the 2002 Population and Housing Census prepared by INE as a baseline. Conducted in April 2002, the census recorded 187,521 migrants among the 15.1 million residents of Chile, incorporating variables such as education, age, and year of arrival. Figure 2a shows the stock of migrants for the two main migratory waves of the last 15 years. Following the 2017 election, immigration restrictions were introduced for Haitian nationals, substantially reducing regular entry channels. To compute the shares of migrant populations, I use yearly municipal population estimates reported by INE for the years 2000 to 2035, based on projections from census data.

Figure 2: Stock of Migrants by Nationality (2001 - 2021)

(a) Venezuelan and Haitian Migrant Stock



(b) Migration Stock by skill levels



Source: Author's calculation using the visa and permanent resident requirement dataset from the Chilean Department of State (National Migration Service) 2000 - 2021.

Skill Level The census and migration records include self-reported educational attainment for each applicant. I classify migrants as high-skilled if they have completed any form of higher education, and as low-skilled if they have completed only primary or secondary schooling. Because educational information is missing for 682,067 individuals, I impute missing values following the multiple imputation methodology of [Rubin \(1987\)](#). The imputation model incorporates gender, age, country of origin, occupation, professional activity, year of arrival, and municipality of residence across 20 imputed datasets. Appendix B reports a test of the Missing at Random assumption and a comparison of observed and imputed education distributions; the results confirm that missingness is predicted by variables already incorporated in the imputation model, supporting the validity of the strategy. Figure 2b illustrates the evolution of migrant stocks by nationality and by skill level.

Local labour markets To study economic mechanisms such as labour market competition, I complement the municipal data with a second level of geographic aggregation based on local labour markets. These units correspond to the *Areas Funcionales de Trabajo* constructed by the National Statistics Institute (INE) in collaboration with the Ministry of Housing and Urban Development of Chile (Minvu). Their definition follows the OECD functional urban area methodology ([OECD, 2012](#)), which identifies integrated labour markets by combining population density with commuting flows. This method groups municipalities that share common labour dynamics regardless of administrative boundaries. Using these functional areas allows the analysis to measure migration at the spatial scale where employment opportunities, commuting patterns, and labour market interactions are more likely to be shared. This complements the municipal-level analysis, which captures a finer residential scale where migrant visibility and everyday local exposure may be more salient. Together, these two levels of aggregation allow the analysis to examine whether the electoral consequences of migration vary across theoretically relevant spatial contexts.

Party Families and Outcome Construction To classify political actors into coherent ideological coalitions, I rely on the party family typology of the Manifesto Project, which groups parties according to their programmatic profiles and long-term ideological commitments ([Manifesto Project, 2011](#)). Appendix A provides the detailed mapping between Chilean parties and the corresponding Manifesto Project families. Based on this classification, I distinguish four electoral blocs. The centre-right corresponds to *Chile Vamos*, a coalition placed within the Conservative party family. The left-wing bloc aggregates the parties in the former *Nueva Mayoría* coalition together with *Frente Amplio*, which the Manifesto Project categorises in the Socialist and Social Democratic families. The far-right bloc consists of the *Republican Party* and *Partido Social Cristiano*, classified as part of the Nationalist and Radical Right family. Finally, I group the remaining

candidates in an “other parties” category that includes small, personalistic, and locally anchored movements that do not map cleanly onto any established party family. These four blocs form the basis of the vote-share outcomes used throughout the analysis.

Party Ideology To measure the ideological positions of Chilean political parties, and particularly their stance toward migration, I draw on the Manifesto Project’s coding of the 2017 and 2021 elections. The Manifesto Project analyzes party manifestos and records their emphasis on specific policy themes, including immigration. Appendix Table A.1 reports the immigration-related scores for each coalition. Consistent with the discussion in the Background section, the table shows that the Republican Party expressed the most consistently restrictive position on immigration among the major blocs, while the centre-right incorporated a smaller amount of restrictive immigration rhetoric in 2021. The left-wing bloc expressed neutral or inclusionary positions on migration. These programme-based differences provide a descriptive basis for analysing vote shares separately for the far-right, the centre-right, the left-wing, and the set of other parties.

Election Results Electoral outcomes are measured using the first-round results from the 2017 and 2021 presidential elections published by the Electoral Service (SERVEL). The 2017 election marks the first appearance of a far-right presidential candidate, José Antonio Kast, who led a new coalition distinct from the traditional centre-right alliance headed by Sebastián Piñera¹⁰. For each municipality or local labour market, I compute turnout and aggregate vote shares for the far-right, centre-right, left-wing coalition, and other parties. This structure allows the analysis to examine how migration relates to the territorial distribution of support for established coalitions and non-traditional political actors. Descriptive statistics for these electoral variables are presented in Table 1.

Municipal Characteristics To include relevant controls, I use demographic information from the National Statistics Institute. These data include municipal-level age distributions and the proportion of women. From these variables I construct an elderly dependency rate, defined as the ratio of the population aged sixty and older to the population aged fourteen and younger in each municipality. Summary statistics for these variables are reported in Table 1.

4 Methods

This study examines how exposure to immigration affects electoral outcomes in Chile. I follow two complementary identification strategies. First, I estimate a set of linear models that describe the association between local migration and voting patterns. Second, I apply

¹⁰The Chilean political system is a presidential system, so the leaders of the coalitions are not necessarily the party presidents, but rather the presidential candidates.

Table 1: Descriptive Statistics by Municipality

Variable	2017				2021			
	Mean	SD	Min	Max	Mean	SD	Min	Max
Outcome								
Far-right vote (%)	7.98	2.93	1.19	30.56	27.36	8.18	11.57	72.34
Center-right vote (%)	36.05	8.49	23.05	74.10	12.35	4.52	1.10	34.35
Left-wing vote (%)	55.10	8.82	18.70	71.97	45.41	9.81	4.42	64.04
Other parties vote (%)	0.87	0.42	0.00	5.56	14.88	7.29	0.60	60.43
Turnout (%)	46.69	5.26	11.31	69.09	47.38	5.62	19.07	69.27
Migration								
Migration (%)	6.41	8.41	0.08	62.28	9.84	10.76	0.12	62.90
Hispanic migration (%)	5.25	7.41	0.04	61.92	8.00	9.85	0.06	62.19
Haitian migration (%)	0.43	0.73	0.00	4.41	1.06	1.22	0.00	5.95
Low-skilled migration (%)	4.41	5.80	0.00	56.94	6.55	7.05	0.00	57.24
High-skilled migration (%)	1.34	2.01	0.00	10.01	2.17	2.77	0.02	14.48
Controls and Municipal Characteristics								
Population	182,847	151,824	123	604,744	199,492	165,264	141	655,033
Aging rate	83.35	23.77	19.96	360.00	95.78	28.03	24.67	343.75
Women (%)	50.73	0.65	36.36	51.89	50.66	0.67	34.75	52.23

Source: Electoral results dataset for the presidential elections of 2017 and 2021 from the Chilean Electoral Service (SERVEL), visa applications dataset from the Chilean Department of State (National Migration Service), Population projections 2000-2035 from the National Statistics Institute (INE). Ageing rate corresponds to the proportion of 60 years old or older, over the young 14 years old or younger in a given municipality. Author’s calculation by municipality (346).

an instrumental variables approach that uses historical migrant settlement and national-level inflow shocks to generate plausibly exogenous variation in local migrant exposure. The analysis is conducted at two levels of geographic aggregation. I begin with local labour markets, which capture economically integrated areas where employment opportunities and commuting patterns are more likely to be shared. I then turn to municipalities, which capture a finer residential scale where neighbourhood composition, migrant visibility, and everyday local exposure may be more salient.

4.1 Benchmark linear model

Local labour markets I first examine whether changes in the share of immigrants within local labour markets affect voting outcomes. Labour markets provide a relevant geographic unit for studying potential economic competition, as workers are mobile across municipalities within the same commuting zone and employers recruit from a shared pool of potential employees. For each labour market l and election year t , I estimate:

$$y_{lt} = \beta migr_{lt} + \rho_l + \rho_t + \gamma X_{lt} + \epsilon_{lt} \quad (1)$$

Where y_{lt} is the vote share for a given political coalition in labour market l ; $migr_{lt}$ is the share of migrants in the local labour market population; ρ_l and ρ_t are labour market

and year fixed effects; X_{lt} is a vector of demographic controls at the local level, and ϵ_{lt} is the error term. The coefficient β captures the conditional correlation between local migration levels and electoral outcomes.

To explore whether electoral responses vary with the skill composition of migration, I decompose overall migration into high-skilled and low-skilled components. This decomposition follows the literature on labour market competition, which predicts stronger native reactions when migrants and natives compete for similar types of jobs. I estimate the same model with skill-specific migrant shares both separately and jointly.

Municipalities I next examine the effects of migration at the municipal level. Municipalities capture a finer residential scale at which neighbourhood composition, cultural diversity, and everyday social interactions may be more salient than at the level of local labour markets. For each municipality m and year t , I estimate:

$$y_{mt} = \beta migr_{mt} + \rho_m + \rho_t + \gamma X_{mt} + \epsilon_{mt} \quad (2)$$

Where all variables are defined analogously. These models provide descriptive evidence on how aggregate migration relates to voting outcomes and on whether these relationships differ across local residential contexts.

To examine whether electoral responses differ by the cultural proximity of migrant inflows, I separate migration into two components. The first captures inflows from culturally similar countries, primarily Spanish-speaking Latin American origins. The second captures inflows from culturally different countries, with the Haitian migration wave as the dominant example. I estimate models using each component separately, and then jointly, to assess which type of migrant inflow is more strongly associated with aggregate electoral change.

4.2 Causal Identification and IV model

Although the benchmark regressions provide an informative description of the data, migrant settlement patterns are not random. Migrants may choose to locate in areas with better economic opportunities, more accessible public services, or more welcoming political environments. These factors may also correlate with voting behaviour, which complicates any causal interpretation.

To address this issue, I use a shift-share instrumental variables strategy following the logic of [Altonji and Card \(1991\)](#), [Card \(2001\)](#) and the subsequent literature. The approach exploits two sources of variation. The first is the historical geographic distribution of migrants across Chile. The second is the national-level change in the number of migrants from each origin country between 2017 and 2021, a period characterized by large and

plausibly exogenous supply-driven outflows from several sending countries. The instrument has predictive power because new migrants are likely to settle where earlier cohorts from the same origin were already present, and the shift component captures changes in national inflows that are driven primarily by conditions in the sending countries rather than by local political dynamics within Chile.

General construction

The instrumental variables strategy builds on the first-difference version of equations 1 and 2. For each geographic unit u , defined as either a local labour market or a municipality, I take within-unit differences in the migrant share between the 2017 and 2021 elections. Let $\Delta migr_u$ denote this change. The goal is to isolate the component of $\Delta migr_u$ that is driven by national inflow shocks rather than by local characteristics. Following the logic of [Card \(2001\)](#), I decompose changes in local migrant shares into nationality-specific components.

Let $\Delta migr_u^n$ be the change in the share of migrants from origin country n in unit u between the two elections. This change can be written as a weighted sum of nationality-specific shifts:

$$\Delta migr_u = \sum_n \theta_{u,2008}^n \cdot \Delta migr_u^n \quad (3)$$

Where $\theta_{u,2008}^n$ is the share of all migrants from origin n who lived in unit u in 2008. Formally,

$$\theta_{u,2008}^n = \frac{MIGR_{u,2008}^n}{\sum_u MIGR_{u,2008}^n}$$

Which allocates to each unit the proportion of migrants from origin n who resided there before the main migration waves. This decomposition separates the influence of the historical settlement pattern from the contemporaneous shifts in migration.

The instrument replaces the local shift $\Delta migr_u^n$ with the national-level shift for that origin, denoted $\Delta migr_{2017-2021}^n$. The predicted change in the migrant share in unit u is therefore:

$$\widehat{\Delta migr_u} = \sum_n \theta_{u,2008}^n \cdot \Delta migr_{2017-2021}^n \quad (4)$$

This construction predicts local inflows by combining the pre-existing geographic distribution of migrants with national inflow shocks from each origin. Since the shocks are measured at the national level, they are less likely to be driven by economic or political

conditions in any single locality. This provides a source of plausibly exogenous variation in the change in migrant exposure across Chilean localities.

In constructing the instrument, I include the 35 Latin American and other major origin countries that account for the largest migrant populations in Chile in 2021¹¹. Although Chile receives migrants from more than 160 origins, this set accounts for more than 95.4% of the migrant population in 2021 and therefore captures the full extent of the inflow shocks relevant for the period under study.

Skill-specific instruments for labour markets

To examine whether electoral responses vary with the skill composition of migrant inflows in local labour markets, I construct separate instruments for high-skilled and low-skilled migrant inflows. This approach follows recent applications such as [Moriconi et al. \(2022\)](#) and [Mayda et al. \(2022\)](#), who distinguish between different types of migrants in order to isolate heterogeneous effects on political outcomes.

Let $\theta_{l,2008}^{n,HS}$ and $\theta_{l,2008}^{n,LS}$ be the shares of high-skilled and low-skilled migrants from origin country n residing in labour market l in 2008, and let $\Delta migr_{2017-2021}^{n,HS}$ and $\Delta migr_{2017-2021}^{n,LS}$ be the corresponding national-level changes for each skill group. Using the same logic as in the general shift-share instrument, I construct two predicted inflows:

$$\begin{aligned}\widehat{\Delta migr}_l^{HS} &= \sum_n \theta_{l,2008}^{n,HS} \cdot \Delta migr_{(2017-2021)}^{n,HS} \\ \widehat{\Delta migr}_l^{LS} &= \sum_n \theta_{l,2008}^{n,LS} \cdot \Delta migr_{(2017-2021)}^{n,LS}\end{aligned}$$

These instruments allow me to examine whether exposure to migrants who are more likely to compete with natives for similar jobs produces distinct electoral effects. Because high-skilled and low-skilled migrants from the same origin do not necessarily concentrate in the same labour markets, the two instruments draw on partially distinct complier populations. Differences in estimated coefficients across skill groups should therefore be read as estimates of the electoral response to each type of inflow in the markets where that group concentrates, rather than as a direct test of labour market competition as an isolated mechanism.

Cultural-specific instruments for municipalities

To examine whether electoral reactions vary across migrant inflows with different cultural profiles, I decompose the general shift-share instrument into two origin-based compo-

¹¹These are Argentina, Australia, Belgium, Bolivia, Brazil, Canada, China, Colombia, Costa Rica, Cuba, Dominican Republic, Ecuador, El Salvador, France, Germany, Haiti, India, Israel, Italy, Japan, Mexico, Netherlands, Panama, Paraguay, Peru, Russia, South Korea, Spain, Sweden, Switzerland, Taiwan, United Kingdom, United States, Uruguay, and Venezuela.

nents. These components compare exposure to Spanish-speaking Latin American inflows with exposure to Haitian inflows, while recognising that these groups differ along several dimensions beyond cultural proximity, including race, language, skill composition, and perception of vulnerability.

Let $\theta_{m,2008}^{Sim}$ represent the share of migrants from culturally similar origins who lived in municipality m in 2008. This group includes migrants from Spanish-speaking American countries whose linguistic and cultural backgrounds are relatively close to those of native Chileans. Let $\Delta migr_{2017-2021}^{Sim}$ be the corresponding national-level inflow change for this group. The predicted inflow of culturally similar migrants is:

$$\widehat{\Delta migr}_m^{Sim} = \sum_n \theta_{m,2008}^{n,Sim} \cdot \Delta migr_{(2017-2021)}^{n,Sim}$$

For the culturally distinct group, I focus specifically on migration from Haiti, which represents a clear cultural and linguistic contrast with the native population. Let $\theta_{m,2008}^{Hai}$ be the share of Haitian migrants residing in municipality m in 2008, and let $\Delta migr_{2017-2021}^{Hai}$ be the national-level change in the Haitian migrant population. The predicted inflow is:

$$\widehat{\Delta migr}_m^{Hai} = \theta_{m,2008}^{Hai} \cdot \Delta migr_{(2017-2021)}^{Hai}$$

These two instruments allow me to assess whether exposure to each type of inflow is associated with distinct electoral responses. Because the two instruments draw their identifying variation from different sets of municipalities, those where Haitian migrants historically concentrated and those where Hispanic migrants settled, they involve distinct complier populations. Estimated coefficients for the two groups, therefore, capture electoral responses to each type of inflow in the localities where that group tends to settle. This is informative about whether different migration flows are associated with different electoral outcomes; it does not permit a clean attribution of those differences to cultural distance as an isolated causal channel, since the two groups also differ in race, language, and humanitarian framing, as noted in Section 2.

4.3 Validity of the IV Shift-Share Model

The internal validity of the shift-share design depends on the assumption that the pre-existing migrant shares are exogenous to later changes in electoral outcomes. [Goldsmith-Pinkham, Sorkin, and Swift \(2020\)](#) show that Bartik or shift-share estimators can be written as a generalized method of moments estimator in which the local shares act as the instruments, while the national shocks operate as weights. In this formulation, identification relies on the exogeneity of the shares rather than the shocks themselves. When a pre-period is available, this requirement is equivalent to a difference-in-differences assumption, where the pre-period shares capture differential exposure to a common subsequent

shock. Testing whether those shares predict differential pre-trends in the outcome therefore becomes central to establishing internal validity.

Following their recommendations, I begin by computing the Rotemberg weights for each origin-specific component of the instrument. These weights measure the proportional contribution of each origin country to the identifying variation. The results, presented in Appendix H Table H.12, show that a small set of origins accounts for almost all the identifying variation. Venezuela and Haiti receive the largest weights, followed by Peru, Bolivia, and Colombia. All remaining countries contribute only marginally, which helps isolate the origins whose pre-trends are most important to examine.

Using these weights as guidance, I test whether the 2008 migrant shares predict electoral changes before the start of the period analysed in the main models. Specifically, I regress changes in presidential vote shares in the 2005 election, as well as changes between 1989 and 2005, on the main shift-share instrument and the origin-specific shares that receive the highest Rotemberg weights. These regressions include the same controls used in the main specifications. The results for cities and municipalities are reported in Appendix Tables H.13 and H.14. Across all outcomes, the coefficients on the overall instrument and on the key origin-specific shares are small and statistically indistinguishable from zero. There is no evidence that areas with higher initial exposure to Venezuelan, Haitian, Peruvian, or other large migrant groups experienced different political trends in the pre-treatment period.

Taken together, the Rotemberg weights and the absence of discernible pre-trends provide support for the identifying assumption. The 2008 migrant shares do not predict earlier changes in voting behaviour, which is consistent with the interpretation that they influence later electoral outcomes through differential exposure to the post-2017 immigration shocks. These findings strengthen confidence in the internal validity of the shift-share strategy used in this study.

5 Results

5.1 Benchmark Results

Before turning to the instrumental variables strategy, I begin by examining the association between migration and electoral outcomes using the two-way fixed effects specification in Equation 1. Table 2 reports the estimates for local labour markets in Panel A and for municipalities in Panel B. The migration ratio is standardized, so the coefficients capture the change in vote share associated with a one standard deviation increase in the migrant share.

Across both local labour markets and municipalities, higher migration is consistently associated with lower support for the far-right. The estimated coefficients are sizeable in

Table 2: Two-way fixed effects estimates: Migration and vote shares

Panel A: Local Labour Markets								
	Far-Right		Center-Right		Left-Wing		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Migration Ratio	-0.070*** (0.022)	-0.054** (0.022)	0.019 (0.025)	0.014 (0.026)	-0.054*** (0.014)	-0.043*** (0.014)	0.105*** (0.020)	0.082*** (0.020)
Observations	570	570	570	570	570	570	570	570
Adjusted R^2	0.900	0.905	0.944	0.945	0.756	0.767	0.819	0.845
N° of IDs	285	285	285	285	285	285	285	285
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Municipalities								
	Far-Right		Center-Right		Left-Wing		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Migration Ratio	-0.079*** (0.019)	-0.051** (0.020)	0.042* (0.023)	0.016 (0.024)	-0.030** (0.014)	-0.033** (0.014)	0.067*** (0.023)	0.068*** (0.022)
Observations	690	690	690	690	690	690	690	690
Adjusted R^2	0.889	0.897	0.934	0.940	0.746	0.767	0.792	0.828
N° of IDs	345	345	345	345	345	345	345	345
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports estimates from two-way fixed effects regressions for the 2017 and 2021 presidential elections (Equation 1). The dependent variable is the vote share of each political bloc in a given year. The main independent variable is the migration ratio, defined as the share of immigrants in the local population, then standardized. Panel A presents results at the level of local labour markets; Panel B presents results at the municipality level. All models include year fixed effects and unit fixed effects. Columns with controls include the proportion of women and the elderly dependence rate in each locality. Robust standard errors clustered at the appropriate geographic level are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

both panels and remain relatively stable when demographic controls are added. At the municipal level, for example, a one standard deviation increase in the migrant share is associated with roughly a five percentage point decline in the far-right vote. The centre-right coalition shows no clear pattern in response to migration. Coefficients are small in magnitude in both levels of aggregation, suggesting that changes in the migrant share do not meaningfully shift support toward the traditional right.

For the left-wing parties, the estimates also point to a negative association with migration. Although the magnitudes are smaller than for the far-right, municipalities and labour markets with higher migrant shares tend to display lower support for left-wing candidates. In contrast, the vote for other parties moves in the opposite direction. The estimates for this group are consistently positive and comparatively large, indicating that localities with larger migrant shares also record higher aggregate support for independent

or minor candidates who fall outside the traditional party families.

Appendix C provides additional evidence on how these associations vary with the composition of migration. Appendix Table C.3 separates migration into high-skilled and low-skilled inflows. High-skilled migration is associated with increases in both centre-right and left-wing vote shares and with declines in support for non-traditional parties, while the far-right shows little systematic response. Low-skilled migration presents a different pattern. The estimates point to lower left-wing support and substantially higher support for parties outside the main party families, along with modest declines in the far-right vote.

These contrasting patterns may reflect different mechanisms linked to the skill composition of incoming migrants. Locations that attract more high-skilled immigrants are often economically dynamic and socially integrated, which may reinforce support for the mainstream coalitions on both the centre-right and the left. In contrast, increases in low-skilled migration may intensify perceptions of labour market competition or strain in local services, which could weaken support for established coalitions and redirect votes toward non-traditional options. Although these benchmark correlations do not identify causal channels, they suggest that the political consequences of migration vary not only with its scale but also with its composition.

Appendix Table C.4 examines heterogeneity by cultural proximity. Hispanic migration, which captures inflows from Spanish-speaking American countries, is negatively associated with support for both the far-right and the left-wing, and positively associated with support for non-traditional parties. Haitian migration shows a distinct pattern. Areas with larger increases in Haitian inflows tend to exhibit lower far-right support and modestly higher vote shares for both mainstream coalitions on the centre-right and the left-wing, together with declines in support for parties outside the main party families. These differences by cultural proximity suggest that aggregate electoral responses to migration may depend not only on the volume of incoming flows but also on the social and cultural profile of the new arrivals.

Taken together, these benchmark correlations show that higher migrant shares are associated with lower aggregate support for the far-right and, to a lesser extent, the left-wing, and with higher support for parties outside the dominant coalitions. At the same time, these patterns rely on observational variation and may reflect non-random settlement choices by migrants. For this reason, the next section turns to the shift-share instrumental variables strategy to estimate the causal effect of migration on electoral outcomes.

5.2 Instrumental Variables Results

The instrumental variables strategy offers a way to assess the research question while accounting for the concerns of self-selection and reverse causality raised in the previous section. Using a shift-share design, I exploit historical settlement patterns and national inflow shocks to isolate changes in migrant exposure that are plausibly unrelated to local political dynamics. I begin with results at the level of local labour markets, a spatial scale aligned with economically integrated areas where employment opportunities and commuting patterns are more likely to be shared. I then analyse results at the municipal level, a finer territorial scale where neighbourhood composition, migrant visibility, and everyday local exposure may be more salient. Finally, I examine turnout to assess whether the electoral changes identified in the analysis reflect changes in vote allocation across blocs or changes in political participation.

Local labour markets

Table 3 reports the instrumental variables estimates for changes in the migrant share at the level of local labour markets. The results suggest that, once migrant sorting is addressed, larger migrant inflows are associated with a marked decline in support for the far-right. A one standard deviation increase in the migrant share is linked to a reduction of roughly four percentage points in the far-right vote. In contrast, the centre-right coalition appears to benefit from immigration. The estimated effect is close to six percentage points, indicating that inflows into local job markets may strengthen support for the mainstream right. In general, migration increases support for the broader right-wing coalition but reshapes its internal composition, reallocating aggregate support away from more exclusionary and toward more moderate right-wing alternatives. These findings stand in contrast to the expectation that increases in the local migrant share would strengthen far-right support, and instead suggest that in Chile the political consequences of migration operate primarily within the right.

For the left-wing coalition, the estimates suggest that they are largely unaffected by migration inflows. Parties outside the traditional party families, however, tend to lose support as immigration increases, with estimates pointing to a decline of roughly four percentage points. Overall, the labour market results imply that migration does not produce a uniform reaction across coalitions but instead redistributes support within the right and weakens non-traditional electoral alternatives.

To explore whether these patterns vary with the composition of migration, Table 4 separates inflows into high-skilled and low-skilled components. The estimates show that both types of inflows are associated with reductions in far-right support, although the magnitudes differ. Instrumented high-skilled inflows are associated with modest declines in the far-right vote and clear gains for the centre-right coalition. Instrumented low-

Table 3: 2SLS estimates: Migration changes and vote share changes in local labour markets

Dependent Variable % vote	Models		
	OLS	IV	IV - Control
Panel A: Far right			
Migration ratio change	-0.0235*** (0.00636)	-0.0458*** (0.00502)	-0.0397*** (0.00505)
Controls	No	No	Yes
Observations	285	285	285
Panel B: Center right			
Migration ratio change	0.00518 (0.00717)	0.0612*** (0.00654)	0.0599*** (0.00592)
Controls	No	No	Yes
Observations	285	285	285
Panel C: Left wing			
Migration ratio change	-0.0192*** (0.00403)	0.0151 (0.0135)	0.0195 (0.0127)
Controls	No	No	Yes
Observations	285	285	285
Panel D: Other			
Migration ratio change	0.0375*** (0.00591)	-0.0304 (0.0196)	-0.0398** (0.0175)
Controls	No	No	Yes
Observations	285	285	285
Panel E: First stage regression			
<i>Endogenous variable: Migration ratio change</i>			
Instrument		3.32e-06*** (6.97e-07)	3.16e-06*** (6.01e-07)
F-Test		22.71	27.65
Partial R ²		0.0111	0.0115

Notes: The table reports OLS and 2SLS estimates for the effect of changes in migration on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variable is the migration ratio change, defined as the change in the number of immigrants divided by the local population and standardized. The shift-share instrument is constructed following Equation 4. All results are estimated at the level of local labour markets. Columns with controls include the proportion of women and the elderly dependence rate in each labour market in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results, including the Kleibergen–Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

skilled inflows produce larger shifts: a one standard deviation increase in the low-skilled migrant share is associated with a sizeable reduction in far-right support and a marked increase in votes for the centre-right. These differential patterns are consistent with prior

Table 4: 2SLS estimates: Skill-specific migration changes and right-wing vote share changes in local labour markets

Dependent Variable % vote	Models			
	OLS	IV	IV	IV
Panel A: Far right				
High-skilled migrant share	-0.00952 (0.00696)	-0.0198*** (0.00278)		-0.0147** (0.00662)
Low-skilled migrant share	-0.0132 (0.00885)		-0.0551*** (0.00752)	-0.0156 (0.0183)
Controls	Yes	Yes	Yes	Yes
Observations	285	285	285	285
Panel B: Center right				
High-skilled migrant share	0.0242*** (0.00682)	0.0316*** (0.00220)		0.0394*** (0.00554)
Low-skilled migrant share	-0.0106 (0.00867)		0.0820*** (0.0129)	-0.0237 (0.0153)
Controls	Yes	Yes	Yes	Yes
Observations	285	285	285	285
Panel E: First stage regressions				
<i>Endogenous variables: High-skilled and Low-skilled migrant share</i>				
Instrument for high-skilled		2.43e-05*** (2.55e-06)		-0.000135 (0.000111)
Instrument for low-skilled			3.68e-06*** (9.42e-07)	0.000152*** (2.89e-05)
F-Test		90.55	15.28	8.940
Partial R ²		0.0407	0.00592	
Partial R ² (High-skilled)				0.0467
Partial R ² (Low-skilled)				0.0383

Notes: The table reports OLS and 2SLS estimates for the effect of changes in high-skilled and low-skilled migration on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variables are the high-skilled migrant share and the low-skilled migrant share, each defined as the change in the number of immigrants in the corresponding skill group divided by the local population and standardized. The shift-share instruments are constructed following Equation 4. All results are estimated at the level of local labour markets. All columns include the proportion of women and the elderly dependence rate in each labour market in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results for both endogenous variables, including the Kleibergen–Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

work by [Halla et al. \(2017\)](#), [Moriconi et al. \(2022\)](#), and [Mayda et al. \(2022\)](#) showing that aggregate electoral responses to migration vary with the skill composition of incoming flows, though as discussed in Section 4, differences in coefficients across skill groups also reflect differences in the types of labour markets where each group concentrates. The results for the left-wing and for parties outside the traditional families are reported in Appendix Table D.5, since the political agenda around migration has been more central to the far-right and centre-right coalitions.

When both high-skilled and low-skilled inflows are instrumented jointly, the estimates continue to indicate that the electoral response to migration differs by skill profile. High-

skilled inflows are associated with higher centre-right support and, in the full appendix results, higher left-wing support, while low-skilled inflows are associated with weaker support for the left-wing and, in some specifications, stronger support for non-traditional parties. The joint specification should be interpreted with caution, however, because the first-stage relation for low-skilled migration is relatively weak, with an F statistic of around nine. Even so, the overall pattern across models indicates that the political consequences of migration in local labour markets vary with the skill profile of the incoming population.

Municipalities

Table 5 presents the instrumental variables estimates at the municipal level. Unlike labour markets, which aggregate economic interactions, municipalities offer a finer territorial scale where neighbourhood composition and everyday exposure to migrants are more likely to shape political perceptions. Here, the IV estimates suggest that changes in the total migrant share have a limited effect on far-right support at this more granular spatial level. One possible interpretation is that aggregate far-right support may be more responsive to migration when it affects local labour market conditions than when it operates through day-to-day residential proximity.

For the centre-right coalition, the municipal results are clearer. A one standard deviation increase in the migrant share is associated with gains of roughly three percentage points for this coalition. The left-wing coalition also displays positive responses, although the changes are smaller in magnitude, generally in the range of one to two percentage points. Parties outside the traditional families tend to lose support as migration increases, in values very similar to those observed in local labour markets, suggesting that the dynamics shaping support for non-traditional parties operate similarly across spatial scales. The first stage statistics for this model are not particularly strong, which raises concerns about instrument weakness. As a robustness check, I estimated the corresponding limited information maximum likelihood model, reported in Appendix G. The LIML results are virtually identical to the 2SLS estimates, which mitigates the concern that the IV results are being driven by weak first stages. Overall, the municipal evidence indicates that immigration at the neighbourhood level does not expand far-right support. Instead, it appears to benefit both mainstream coalitions while reducing support for smaller, non-traditional electoral alternatives.

To examine whether cultural distance plays a role in shaping these responses, Table 6 separates migration from Spanish-speaking American countries and migration from Haiti. For the far-right, the estimates indicate that only Haitian inflows are clearly associated with declines in support. A one standard deviation increase in the Haitian migrant share is linked to a reduction of roughly three percentage points in the far-right vote, whereas the estimates for Hispanic migration are not statistically relevant. One interpretation is that neighbourhoods exposed to culturally distinct migrants may be less receptive to far-right

Table 5: 2SLS estimates: Migration changes and vote share changes at the municipal level

Dependent Variable % vote	Models		
	OLS	IV	IV - Control
Panel A: Far right			
Migration ratio change	-0.0211*** (0.00510)	-0.0147 (0.0158)	-0.0124 (0.0129)
Controls	No	No	Yes
Observations	345	345	345
Panel B: Center right			
Migration ratio change	0.0120** (0.00525)	0.0344** (0.0143)	0.0307** (0.0125)
Controls	No	No	Yes
Observations	345	345	345
Panel C: Left wing			
Migration ratio change	-0.00730 (0.00468)	0.0180** (0.00907)	0.0166*** (0.00638)
Controls	No	No	Yes
Observations	345	345	345
Panel D: Other			
Migration ratio change	0.0164** (0.00818)	-0.0376** (0.0182)	-0.0349*** (0.0123)
Controls	No	No	Yes
Observations	345	345	345
Panel E: First stage regression			
<i>Endogenous variable: Migration ratio change</i>			
Instrument		4.60e-05*** (1.71e-05)	4.77e-05*** (1.62e-05)
F-Test		7.238	8.697
Partial R ²		0.124	0.140

Notes: The table reports OLS and 2SLS estimates for the effect of changes in migration on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variable is the migration ratio change, defined as the change in the number of immigrants divided by the municipal population and standardized. The shift-share instrument is constructed following Equation 4. All results are estimated at the municipal level. Columns with controls include the proportion of women and the elderly dependence rate in each municipality in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results, including the Kleibergen–Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

narratives portraying these inflows as harmful, though the design cannot isolate which specific features of Haitian migration drive this pattern. This stands in contrast to findings from other settings, where culturally distant migration often strengthens support for

Table 6: 2SLS estimates: Origin-specific migration changes and right-wing vote share changes at the municipal level

Dependent Variable % vote	Models			
	OLS	IV	IV	IV
Panel A: Far right				
Migrant share Hispanic	-0.0124*** (0.00470)	-0.00703 (0.0129)		-0.0109 (0.0116)
Migrant share Haiti	-0.0144*** (0.00438)		-0.0447** (0.0220)	-0.0333*** (0.0123)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel B: Center right				
Migrant share Hispanic	0.00424 (0.00558)	0.0251** (0.0107)		0.0289** (0.0119)
Migrant share Haiti	0.0186*** (0.00406)		0.0630* (0.0332)	0.0329*** (0.00901)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel E: First stage regressions				
<i>Endogenous variable: Migrant share of Hispanic countries and Haiti</i>				
Instrument Hispanic		5.95e-05** (2.31e-05)		5.65e-05** (2.32e-05)
Instrument Haiti			7.22e-05*** (1.02e-05)	9.03e-05*** (1.15e-05)
F-Test		6.626	50.27	18.85
Partial R ²		0.148	0.0297	
Partial R ² (Hispanic)				0.150
Partial R ² (Haiti)				0.0423

Notes: The table reports OLS and 2SLS estimates for the effect of changes in migration from Hispanic countries and Haiti on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variables are the migrant share from Hispanic countries and the migrant share from Haiti, each defined as the change in the number of immigrants from the corresponding origin group divided by the municipal population and standardized. The shift-share instruments are constructed following Equation 4. All results are estimated at the municipal level. All columns include the proportion of women and the elderly dependence rate in each municipality in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results for both endogenous variables, including the Kleibergen-Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

exclusionary or right-wing parties (e.g. [Brunner & Kuhn, 2018](#); [Mendez & Cutillas, 2014](#)), and suggests that cultural distance does not uniformly generate far-right support, with the specific social and economic position of incoming groups shaping aggregate electoral responses in context-dependent ways.

For the centre-right coalition, both Hispanic and Haitian inflows are associated with increases in vote shares, with somewhat larger gains linked to the Haitian inflow. This pattern suggests that centre-right parties may be able to accommodate concerns about immigration without endorsing the more radical positions of the far-right, attracting ag-

gregate support in municipalities experiencing growing diversity regardless of the racial or cultural composition of the inflow. Appendix Table D.6 shows that both types of migration tend to increase support for the left and reduce votes for non-traditional parties, although the magnitudes vary across specifications. Taken together, these findings highlight that the absence of a far-right backlash in areas experiencing Haitian inflows is not driven by an absence of electoral response to migration more broadly, but rather by a pattern in which mainstream parties, and particularly the centre-right, absorb the electoral consequences of growing diversity.

Table 7: 2SLS estimates: Low-skilled migration changes by origin and right-wing vote share changes at the municipal level

Dependent Variable % vote	Models			
	OLS	IV	IV	IV
Panel A: Far right				
Migrant share low skilled Hispanic	-0.0124** (0.00525)	-0.0144 (0.0124)		-0.0179 (0.0126)
Migrant share low skilled Haiti	-0.0147*** (0.00428)		-0.0469** (0.0229)	-0.0341*** (0.0114)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel B: Center right				
Migrant share low skilled Hispanic	0.000673 (0.00628)	0.0309*** (0.0111)		0.0349*** (0.0134)
Migrant share low skilled Haiti	0.0191*** (0.00394)		0.0643* (0.0333)	0.0393*** (0.0117)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel E: First stage regressions				
<i>Endogenous variable: Low skilled migrant share, Hispanic countries and Haiti</i>				
Instrument Hispanic		0.000107*** (2.82e-05)		0.000107*** (2.90e-05)
Instrument Haiti			6.88e-05*** (8.85e-06)	8.13e-05*** (9.43e-06)
F-Test		14.46	60.41	22.48
Partial R ²		0.139	0.0260	
Partial R ² (Hispanic)				0.139
Partial R ² (Haiti)				0.0342

Notes: The table reports OLS and 2SLS estimates for the effect of changes in low-skilled migration from Hispanic countries and Haiti on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variables are the low-skilled migrant share from Hispanic countries and the low-skilled migrant share from Haiti, each defined as the change in the number of immigrants from the corresponding origin group divided by the municipal population and standardized. The shift-share instruments are constructed following Equation 4. All results are estimated at the municipal level. All columns include the proportion of women and the elderly dependence rate in each municipality in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results for both endogenous variables, including the Kleibergen-Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Since Haitian migration is overwhelmingly low-skilled, a natural concern is that the differential electoral responses documented in Table 6 reflect differences in the skill compo-

sition of the two groups rather than differences in their cultural profile. Table 7 addresses this by comparing the electoral response to low-skilled inflows from culturally similar and culturally distinct origins, holding skill composition constant across the two groups. The results show that increases in the low-skilled Haitian share continue to be associated with declines in far-right support and gains for the centre-right, while low-skilled Hispanic inflows mainly reinforce the centre-right with limited implications for the far-right. The differential pattern therefore persists within the low-skilled comparison, suggesting that the contrasting electoral responses are not driven by skill composition alone and that the cultural profile of incoming groups contributes to shaping aggregate electoral outcomes, even if the design cannot isolate cultural distance as a single causal channel.

Taken together, the municipal results indicate that migration does not trigger a far-right backlash at the neighbourhood scale, even when inflows come from culturally distant and economically vulnerable groups. Instead, the estimates are consistent with the idea that exposure to culturally distinct newcomers does not automatically strengthen exclusionary platforms and may instead coincide with greater aggregate support for moderate alternatives. At the same time, the centre-right consistently benefits from both culturally similar and culturally different inflows, which suggests that these parties can absorb migration-related concerns in the electoral arena without adopting the more radical positions characteristic of the far-right¹².

5.2.1 Turnout

To assess whether the electoral changes documented above reflect shifts in political preferences or changes in political participation, Tables 8 and 9 report the instrumental variables estimates for turnout at the local labour market and municipal levels. Since the analysis focuses on vote choice rather than on participation per se, the turnout results serve as a diagnostic check to ensure that observed changes in vote shares are not simply driven by changes in the size of the voting electorate.

At the labour market scale, the results point to a modest positive association between migration and turnout. A one standard deviation increase in the migrant share is linked to an increase of about two percentage points in participation, with both high-skilled and low-skilled inflows displaying similar patterns. This suggests that immigration may have heightened the political salience of local conditions, increasing aggregate participation independently of the partisan reallocation documented above. However, the magnitude of this turnout increase is small relative to the six percentage point gains observed for the centre-right in the labour market analysis, which indicates that participation changes alone cannot account for the electoral shifts documented above.

At the municipal level, the estimated effects are smaller. Increases in the total mi-

¹²See Appendix A for party manifestos related to migratory topics

Table 8: 2SLS estimates: Migration changes and turnout changes in local labour markets

Dependent Variable % turnout	Turnout		
	IV	IV	IV
Total migrant share	0.0197*** (0.00372)		
High-skilled migrant share		0.00997*** (0.00230)	
Low-skilled migrant share			0.0274*** (0.00440)
Controls	Yes	Yes	Yes
Observations	285	285	285
First Stage Regressions			
<i>Endogenous variables: Total, high-skilled, and low-skilled migration</i>			
Instrument Total	3.16e-06*** (6.01e-07)		
Instrument high-skilled		2.43e-05*** (2.55e-06)	
Instrument low-skilled			3.68e-06*** (9.42e-07)
F-Test	27.65	90.55	15.28
Partial R ²	0.0115	0.0407	0.00592

Notes: The table reports 2SLS estimates for the effect of changes in migration on turnout between 2017 and 2021. The dependent variable is the change in the turnout rate in each local labour market. The main independent variables are the total migrant share, the high-skilled migrant share, and the low-skilled migrant share, each defined as the change in the number of immigrants divided by the local population and standardized. The shift-share instruments are constructed following Equation 4. All results are estimated at the level of local labour markets. All columns include the proportion of women and the elderly dependence rate in each labour market in 2021. The upper panel presents the second stage estimates. The lower panel reports the first stage results for each endogenous variable, including the Kleibergen-Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

grant share and in Hispanic migration correspond to slight increases in turnout, while Haitian migration seems not to affect participation. These patterns indicate that immigration does not substantially alter turnout at the neighbourhood scale. Taken together, the turnout results suggest that the electoral shifts observed in the labour market and municipal analyses are driven more by changes in vote allocation across coalitions than by broad changes in participation. Migration appears to reconfigure the distribution of votes rather than the size of the electorate that turns out to vote.

Table 9: 2SLS estimates: Migration changes and turnout changes at the municipal level

Dependent Variable % turnout	Turnout		
	IV	IV	IV
Total migrant share	0.0166* (0.00889)		
Hispanic migrant share		0.0172* (0.00892)	
Haitian migrant share			0.0124 (0.00919)
Controls	Yes	Yes	Yes
Observations	345	345	345
First Stage Regressions			
<i>Endogenous variables: Total, Hispanic, and Haitian migration</i>			
Instrument Total	4.77e-05*** (1.62e-05)		
Instrument Hispanic		5.95e-05** (2.31e-05)	
Instrument Haitian			7.22e-05*** (1.02e-05)
F-Test	8.697	6.626	50.27
Partial R ²	0.140	0.148	0.0297

Notes: The table reports 2SLS estimates for the effect of changes in migration on turnout between 2017 and 2021. The dependent variable is the change in the turnout rate in each municipality. The main independent variables are the total migrant share, the Hispanic migrant share, and the Haitian migrant share, each defined as the change in the number of immigrants from the corresponding origin group divided by the municipal population and standardized. The shift-share instruments are constructed following Equation 4. All results are estimated at the municipal level. All columns include the proportion of women and the elderly dependence rate in each municipality in 2021. The upper panel presents the second stage estimates. The lower panel reports the first stage results for each endogenous variable, including the Kleibergen–Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

5.3 Robustness checks

The instrumental variables strategy rests on several modelling choices that may shape the results. In this subsection, I summarise a set of robustness exercises designed to assess the sensitivity of the main findings and to be transparent about the remaining limitations.

A first concern is that the labour market estimates may be driven disproportionately by Santiago, which is treated as a single local labour market. Santiago concentrates around 36.5% of the national population and 58.3% of all migrants, so it could dominate the identifying variation. Appendix E reports models estimated after excluding Santiago from the sample (Tables E.8 and E.9). The IV coefficients remain very similar in sign and magnitude: increases in migration are still associated with lower support for the far-

right and higher support for the centre-right, and the patterns by skill composition are preserved. The results for the left-wing coalition and for parties outside the traditional families show more variation once Santiago is removed, which is consistent with the fact that non-traditional parties tend to be stronger outside the capital. These shifts, however, do not alter the main interpretation of the right-wing results. The first stage remains reasonably strong, and the substantive conclusions of the labour market models do not depend on the inclusion of Santiago.

A second concern is the relatively modest first-stage strength in the municipal models, which raises the possibility of weak instrument bias. To address this issue, I re-estimated the municipal specifications using limited information maximum likelihood. The corresponding estimates, reported in Appendix G (Table G.11), are virtually identical to the 2SLS results for all four coalitions. This similarity reduces the concern that the main IV results at the municipal level are being driven by weak instruments.

Finally, I assess whether the results are sensitive to the choice of base year used to construct the historical migrant shares. The main analysis relies on shares measured in 2008, well before the large inflows that followed 2015. Appendix F reports municipal models that instead use 2012 pre-shares as the basis for the shift-share instrument (Table F.10). The estimates are very similar to those obtained with the 2008 baseline: migration continues to be associated with higher support for the centre-right and the left, lower support for other parties, and no clear expansion of the far-right. Using 2012 shares strengthens the first stage, but it also moves the pre-period closer to the treatment window and therefore makes it harder to ensure that earlier electoral dynamics do not influence subsequent migrant settlement. Choosing 2008 as the main baseline therefore aligns the empirical strategy with the identification logic of the shift-share model, since historical settlement patterns are measured well before the recent migration surge, which reduces the risk that pre-treatment political trends contaminate the instrument. The stability of the results across the 2008 and 2012 baselines suggests that the substantive conclusions are not driven by the particular choice of pre-period, while the main specifications retain a more conservative distance between the historical shares and the post-2015 migration shocks.

6 Discussion and Conclusion

A large literature in political economy and political behaviour has examined how migration shapes electoral support for anti-immigrant and far-right parties, with most evidence drawn from Western Europe and the United States. This focus contrasts sharply with the global distribution of migration, as developing and middle-income countries host the vast majority of the world's migrants ([United Nations High Commissioner for Refugees, 2022](#)). In these newer destination contexts, migration is often recent, welfare states are

more limited, labour markets are more informal, and party systems are still adapting to demographic change. Understanding how migration shapes electoral politics in such settings is therefore essential, both for assessing the scope conditions of existing theories and for explaining contemporary patterns of far-right mobilisation beyond the Global North.

This paper provides evidence from Chile, a middle-income democracy that experienced rapid inflows from two large and distinctive diasporas. Venezuelan migrants share language, ethnicity and many cultural traits with Chileans and span a broad skill distribution, while Haitian migrants differ sharply in language, race, and cultural visibility. Because these inflows arrived recently and at speed, Chile offers leverage on the electoral consequences of different migration profiles in a setting where attitudes and partisan alignments around migration are still forming. The Chilean case is also politically consequential. A major far-right force emerged during the same period, raising the question of whether migration exposure in local communities contributed to its electoral rise.

The core findings show that, in areas that experienced larger migrant inflows, immigration did not increase support for far-right presidential candidates. In local labour markets, migration is associated with a marked decline in far-right support and a corresponding increase in support for the centre-right. At the municipal level, migration does not significantly affect far-right support but continues to benefit the centre-right, indicating that the reorganisation of electoral competition within the right operates across both spatial scales. Haitian inflows, the main culturally distinct migration wave during this period, did not generate a cultural-threat backlash and, when compared to low-skilled Hispanic inflows within the same skill segment, continue to be associated with lower far-right support. These results align with a growing set of studies documenting null or negative effects of immigration on far-right voting ([Lonsky, 2021](#); [Pagliacci & Bonacini, 2022](#)), and they suggest that the political consequences of local migration exposure can diverge sharply from expectations derived from long-standing immigration destinations.

A central implication is that local exposure to migration does not mechanically translate into far-right mobilisation, even in contexts where exclusionary rhetoric is salient. One plausible interpretation is that when migration is recent and attitudes are still forming, local exposure may weaken or fail to activate exclusionary appeals, especially when migrants are perceived as vulnerable or as fleeing severe hardship. This logic is consistent with work distinguishing exposure from contact and showing that direct interaction can temper support for exclusionary politics ([Steinmayr, 2021](#)), as well as with evidence that humanitarian frames and perceptions of migrant vulnerability can elicit more favourable attitudes among natives ([Alrababa'h et al., 2021](#); [Bansak et al., 2016](#)). A second interpretation emphasises how migration-related concern is politically expressed in newer destination contexts. Where public debate centres on regulation, legality, and state capacity rather than redistribution alone, migration-related concerns may be channelled

toward parties perceived as capable of governance rather than toward radical exclusion. Finally, where far-right actors have not fully consolidated ownership over immigration as a political issue, those concerns may be expressed through aggregate reallocation within the right-wing camp rather than through uniform gains for far-right candidates.

These interpretations should be read with appropriate caution. The empirical design identifies aggregate electoral responses to local migrant exposure, not individual attitudes, interpersonal contact, or voter transitions between parties. It also compares migrant inflows that differ across multiple dimensions at once. Venezuelan and Haitian migrants differ not only in cultural proximity, but also in race, language, skill composition, social visibility, and humanitarian framing. The subgroup analyses therefore show whether different migrant profiles are associated with distinct aggregate electoral responses; they do not isolate the causal effect of any single migrant attribute. This limitation is important, but it does not undermine the central finding: across spatial scales and specifications, migration exposure does not increase far-right support in Chile.

These findings point to several directions for future research, particularly in the Global South and other newer migration destinations. First, work combining local exposure designs with survey data could clarify whether observed electoral shifts reflect changes in perceived economic threat, cultural threat, humanitarian concern, or evaluations of governing competence, and help adjudicate among the interpretations outlined above. Second, further research is needed on the role of political supply. Textual analysis of party manifestos, campaign speeches, legislative debates, and media statements could show more directly how mainstream and far-right actors frame migration, and whether those frames condition the electoral effects of local exposure. This is especially important for understanding the role of security narratives and elite framing in contexts where crime is highly salient and media coverage amplifies perceptions of disorder. Third, the present analysis captures a relatively short window during which migration was recent and attitudes were still forming. Future work should examine whether these effects persist over time, since long-term settlement may change how migrants are perceived as they become more embedded in labour markets, schools, neighbourhoods, and public service systems. Finally, cross-national comparison within Latin America could help identify which features of the Chilean case are generalisable to other new destination contexts that share similar structural characteristics, and which are specific to Chile's particular party system and migration history.

Beyond Chile, the results have broader implications for the generalizability of existing theories linking migration to far-right voting. Chile shares several structural features with other democracies that have recently become migration destinations, including rapid and geographically concentrated inflows, constrained welfare provision, persistent labour market informality, and volatile party systems. In such contexts, local exposure to migration may reorganise competition within the right rather than generate automatic

far-right gains. At the same time, these dynamics are unlikely to be universal. The electoral consequences of migration should depend on the scale and visibility of inflows, local labour market conditions, cultural distance, and whether mainstream parties can credibly respond to migration-related concerns without embracing exclusionary rhetoric. Chile should therefore be viewed as informative for a broader class of newer destination democracies, particularly in Latin America, rather than as a universal template.

This study advances the migration-politics literature in three ways. First, by examining a Global South democracy where both large-scale immigration and far-right mobilisation are recent, it provides new evidence on the scope conditions under which migration does, or does not, strengthen exclusionary actors. Second, by analysing a setting in which major migrant groups differ markedly across cultural proximity, skill composition, language, and social visibility, it offers more leverage on the electoral consequences of different migration profiles than contexts in which these dimensions are tightly bundled. Third, the findings underscore the importance of political mediation in shaping the electoral consequences of migration. Even when immigration becomes salient and far-right actors mobilise around exclusionary narratives, local exposure to migrants need not benefit the far-right if alternative political responses are available and credible.

Understanding how migration shapes political behaviour is essential for designing policies that improve integration, reduce social tensions, and prevent the consolidation of xenophobic or exclusionary movements. The Chilean case shows that the electoral effects of migration depend not only on who migrates and where, but also on how migration-related concerns are channelled through political competition in newer destination contexts. As migration within the Global South continues to rise, explaining when local exposure fuels exclusionary politics and when it instead shifts support toward more moderate responses will remain a central challenge for research and policy.

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Appendix

A Party Families, Coalitions, and Immigration Rhetoric

This appendix provides additional detail on the ideological classification of the four major political coalitions used in this paper, based on The Manifesto Project. Table A.1 reports the average left–right positions and the immigration-related manifesto scores for each coalition in the 2009–2021 period. These classifications follow the Manifesto Project Handbook (Manifesto Project, 2011), which distinguishes Conservative, Social Democratic, Socialist, and Nationalist or Radical Right party families and provides a consistent framework for comparing coalitions over time.

The left and centre-left coalitions correspond to parties that the Manifesto Project places in the Socialist, Social Democratic, and Christian Democratic traditions. These coalitions historically emphasise redistribution, welfare expansion, labour rights, and culturally liberal positions. The centre-right coalition corresponds to Chile Vamos and its successor alliances, which belong to the Conservative family and combine market-oriented economic positions with cultural conservatism. The far-right coalition is represented by the Republican Party and the Partido Social Cristiano, classified in the Nationalist and Radical Right family, which places greater emphasis on cultural homogeneity, national identity, and restrictive immigration stances.

The immigration scores in Table A.1 summarise the direction and salience of immigration-related rhetoric in each coalition’s manifesto. Positive values in the “Immigration Negative” column indicate more restrictive positions, while positive values in the “Immigration Positive” column reflect inclusionary positions. Consistent with their party family profiles, the far-right coalition expresses the strongest anti-immigration rhetoric in 2017 and 2021, the centre-right shows a modest restrictive shift in 2021, and both the left and centre-left coalitions consistently adopt neutral or inclusionary positions throughout the period.

Table A.1: Manifesto Project Party Characteristics

Coalition	Right - left	Immigration Negative				Immigration Positive			
	Index	Score				Score			
	Average	2009	2013	2017	2021	2009	2013	2017	2021
Left	-35.37	0.00	0.00	0.00	0.00	0.00	0.03	0.27	0.07
Center-Left	-21.04	0.00	0.00	0.00	0.00	0.00	0.47	0.00	0.15
Center-Right	-11.26	0.00	0.00	0.00	0.09	0.00	0.00	0.00	0.32
Far-right	0.40			0.88	1.19			0.00	0.00

Source: Manifesto Project Version 2024a. Parties by coalition are: (a) Frente Amplio, Apruebo Dignidad and Partido Progresista for the Left Coalition; (b) Concertación de Partidos por la Democracia y Nueva Mayoría (Socialist Party and Christian Democrat Party) for the center-left Coalition; (c) Chile Vamos and Chile Podemos más (Renovación Nacional y Unión Demócrata Independiente) for the center-right Coalition; and (d) Partido Social Cristiano y Partido Republicano for the far-Right Coalition.

B Validation of the Educational Attainment Imputation

Educational attainment is missing for 682,067 individuals, approximately 37 percent of all visa records. To address this, I impute missing values using multivariate normal regression following the multiple imputation methodology of [Rubin \(1987\)](#). The imputation model incorporates gender, age, country of origin, occupation, professional activity, year of arrival, and municipality of residence across 20 imputed datasets. This appendix presents two validation exercises: a test of the Missing at Random (MAR) assumption, and a comparison of observed and imputed education distributions.

Missing at Random Test The validity of multiple imputation rests on the assumption that missingness in the target variable depends only on observed covariates, not on the unobserved values themselves. To assess this, Table B.2 reports a logistic regression of a binary missingness indicator on the variables included in the imputation model. The results show that missingness is systematically predicted by occupation type, gender, and nationality, while age is not a significant predictor. Importantly, this pattern is not problematic for the imputation strategy: the MAR assumption does not require missingness to be random, only that its systematic component is fully captured by observed covariates. Since every variable that significantly predicts missingness is incorporated in the imputation model, the estimates are consistent with the MAR condition.

Table B.2: Predictors of Missing Educational Attainment (MAR Test)

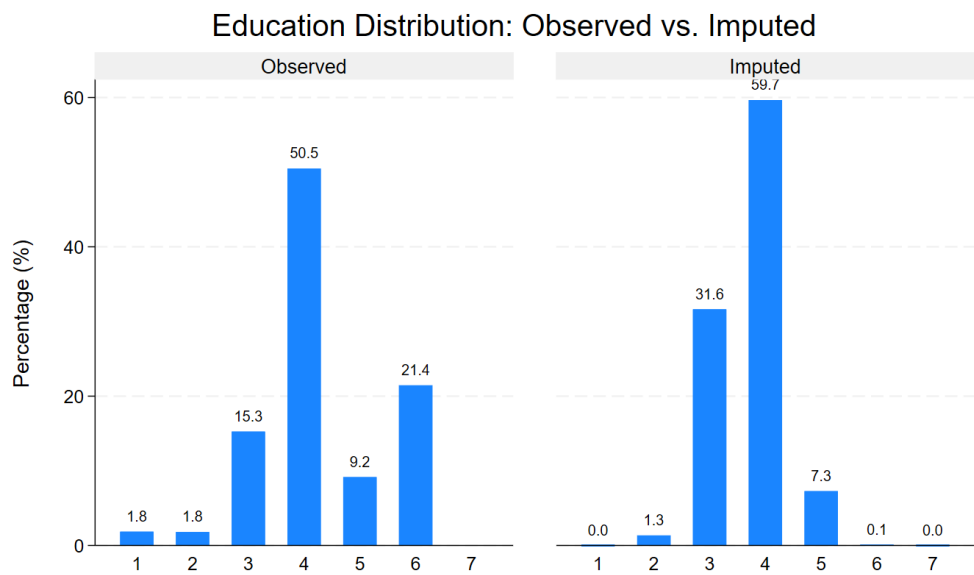
	Missing Education
	(1)
Female	0.063*** (0.003)
Activity type	0.088*** (0.001)
Profession type	0.000*** (0.000)
Age	-0.000 (0.000)
Observations	1.762.633
Country FE	Yes
Municipality FE	Yes

Notes: The dependent variable is an indicator equal to one if educational attainment is missing for a given migrant record. The table reports logistic regression estimates for the four individual-level predictors included in the imputation model. Regression have Country and municipality fixed effects.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Distribution Comparison Figure B.1 compares the distribution of educational attainment for observations with non-missing records and for observations whose education was imputed. The imputed distribution aligns closely with the observed distribution across the lower education categories, which account for the large majority of cases. The highest education category, corresponding to university-level education, is underrepresented in the imputed values relative to its share among observed cases. This pattern is consistent with the known tendency of imputation models to regress predicted values toward the central category of the distribution, particularly when the relevant predictors are occupational and administrative records that are more strongly correlated with intermediate education levels. As a consequence, the high-skilled migrant share may be modestly understated among observations with missing education records. The main findings on aggregate migrant exposure and far-right support, reported in Tables 3 and 5, do not rely on the skill decomposition and are unaffected by this limitation.

Figure B.1: Education Distribution: Observed vs. Imputed Values



Notes: The figure compares the distribution of educational attainment for migrants with non-missing records (Observed) and for migrants whose education was imputed (Imputed). Education categories range from 1 (no formal education) to 6 (university degree). For the skill decomposition in Tables 4 and 7, categories 1 to 4 are classified as low-skilled and categories 5 to 6 as high-skilled.

C Two-way fixed effects models: Heterogeneous Migration

Table C.3: Two-way fixed effects model: Vote share and high-skilled and low-skilled migration

	Far-Right		Center-Right		Left-Wing		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High-skilled Migr. Ratio	-0.025 (0.019)	-0.021 (0.018)	0.060*** (0.018)	0.059*** (0.018)	0.032** (0.013)	0.035*** (0.013)	-0.067*** (0.014)	-0.073*** (0.013)
Low-skilled Migr. Ratio	-0.055* (0.031)	-0.042 (0.030)	-0.025 (0.030)	-0.029 (0.029)	-0.079*** (0.019)	-0.069*** (0.019)	0.159*** (0.023)	0.140*** (0.023)
Observations	570	570	570	570	570	570	570	570
Adjusted R^2	0.901	0.905	0.947	0.948	0.761	0.775	0.833	0.863
N° of IDs	285	285	285	285	285	285	285	285
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports estimates from two-way fixed effects regressions for the 2017 and 2021 presidential elections (Equation 1). The dependent variable is the vote share of each political bloc in each year. The key independent variables are the high-skilled migration ratio and the low-skilled migration ratio, defined as the shares of immigrants in each skill group relative to the local population, then standardized. All models include year fixed effects and city fixed effects. Columns with controls include the proportion of women and the elderly dependence rate in each locality. Robust standard errors clustered at the city level are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table C.4: Two-way fixed effects model: Vote share and cultural similar and different migration

	Far-Right		Center-Right		Left-Wing		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hispanic Migr. Ratio	-0.063*** (0.020)	-0.038* (0.019)	0.018 (0.024)	-0.002 (0.024)	-0.048*** (0.016)	-0.046*** (0.015)	0.093*** (0.026)	0.086*** (0.023)
Haiti Migr. Ratio	-0.019*** (0.006)	-0.016*** (0.006)	0.024*** (0.006)	0.018*** (0.005)	0.013*** (0.004)	0.009** (0.004)	-0.018*** (0.005)	-0.011** (0.005)
Observations	690	690	690	690	690	690	690	690
Adjusted R^2	0.890	0.898	0.937	0.941	0.759	0.775	0.807	0.836
N° of IDs	345	345	345	345	345	345	345	345
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports estimates from two-way fixed effects regressions for the 2017 and 2021 presidential elections (Equation 2). The dependent variable is the vote share of each political bloc in each year. The key independent variables are the migration ratios from culturally similar countries and culturally different Haiti, defined as the shares of immigrants from each country group relative to the local population, then standardized. All models include year fixed effects and city fixed effects. Columns with controls include the proportion of women and the elderly dependence rate in each locality. Robust standard errors clustered at the municipal level are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

D Two Stage Least Square Models: Full Regressions and Heterogeneous Migration

Table D.5: 2SLS model: Skilled migration changes and vote share changes in local labour markets

Dependent Variable % vote	Models			
	OLS	IV	IV	IV
Panel A: Far right				
High-skilled migrant share	-0.00952 (0.00696)	-0.0198*** (0.00278)		-0.0147** (0.00662)
Low-skilled migrant share	-0.0132 (0.00885)		-0.0551*** (0.00752)	-0.0156 (0.0183)
Controls	Yes	Yes	Yes	Yes
Observations	285	285	285	285
Panel B: Center right				
High-skilled migrant share	0.0242*** (0.00682)	0.0316*** (0.00220)		0.0394*** (0.00554)
Low-skilled migrant share	-0.0106 (0.00867)		0.0820*** (0.0129)	-0.0237 (0.0153)
Controls	Yes	Yes	Yes	Yes
Observations	285	285	285	285
Panel C: Left wing				
High-skilled migrant share	0.0153*** (0.00434)	0.0125*** (0.00367)		0.0374*** (0.00968)
Low-skilled migrant share	-0.0244*** (0.00529)		0.0246 (0.0211)	-0.0757*** (0.0263)
Controls	Yes	Yes	Yes	Yes
Observations	285	285	285	285
Panel D: Other				
High-skilled migrant share	-0.0300*** (0.00487)	-0.0243*** (0.00425)		-0.0621*** (0.0112)
Low-skilled migrant share	0.0482*** (0.00646)		-0.0515* (0.0302)	0.115*** (0.0310)
Controls	Yes	Yes	Yes	Yes
Observations	285	285	285	285
Panel E: First stage regressions				
<i>Endogenous variables: High-skilled and Low-skilled migrant share</i>				
Instrument for high-skilled		2.43e-05*** (2.55e-06)		-0.000135 (0.000111)
Instrument for low-skilled			3.68e-06*** (9.42e-07)	0.000152*** (2.89e-05)
F-Test		90.55	15.28	8.940
Partial R ²		0.0407	0.00592	
Partial R ² (High-skilled)				0.0467
Partial R ² (Low-skilled)				0.0383

Notes: The table reports OLS and 2SLS estimates for the effect of changes in high-skilled and low-skilled migration on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variables are the high-skilled migrant share and the low-skilled migrant share, each defined as the change in the number of immigrants in the corresponding skill group divided by the 2021 local population and standardized. The shift-share instruments are constructed following Equation 4. All results are estimated at the level of local labour markets. All columns include the proportion of women and the elderly dependence rate in each labour market in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results for both endogenous variables, including the Kleibergen–Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table D.6: 2SLS model: Migration changes by origin and vote share changes at the municipal level

Dependent Variable % vote	Models			
	OLS	IV	IV	IV
Panel A: Far right				
Migrant share Hispanic	-0.0124*** (0.00470)	-0.00703 (0.0129)		-0.0109 (0.0116)
Migrant share Haiti	-0.0144*** (0.00438)		-0.0447** (0.0220)	-0.0333*** (0.0123)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel B: Center right				
Migrant share Hispanic	0.00424 (0.00558)	0.0251** (0.0107)		0.0289** (0.0119)
Migrant share Haiti	0.0186*** (0.00406)		0.0630* (0.0332)	0.0329*** (0.00901)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel C: Left wing				
Migrant share Hispanic	-0.00924* (0.00481)	0.0129** (0.00622)		0.0154** (0.00732)
Migrant share Haiti	0.0111*** (0.00278)		0.0374* (0.0206)	0.0214*** (0.00775)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel D: Other				
Migrant share Hispanic	0.0174** (0.00842)	-0.0309** (0.0121)		-0.0334** (0.0135)
Migrant share Haiti	-0.0153*** (0.00382)		-0.0557* (0.0328)	-0.0209** (0.0104)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel E: First stage regressions				
<i>Endogenous variable: Migrant share of Hispanic countries and Haiti</i>				
Instrument Hispanic		5.95e-05** (2.31e-05)		5.65e-05** (2.32e-05)
Instrument Haiti			7.22e-05*** (1.02e-05)	9.03e-05*** (1.15e-05)
F-Test		6.626	50.27	18.85
Partial R ²		0.148	0.0297	
Partial R ² (Hispanic)				0.150
Partial R ² (Haiti)				0.0423

Notes: The table reports OLS and 2SLS estimates for the effect of changes in migration from Hispanic countries and Haiti on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variables are the migrant share from Hispanic countries and the migrant share from Haiti, each defined as the change in the number of immigrants from the corresponding origin group divided by the municipal population and standardized. The shift-share instruments are constructed following Equation 4. All results are estimated at the municipal level. All columns include the proportion of women and the elderly dependence rate in each municipality in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results for both endogenous variables, including the Kleibergen–Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table D.7: 2SLS model: Low-skilled migration changes by origin and vote share changes at the municipal level

Dependent Variable % vote	Models			
	OLS	IV	IV	IV
Panel A: Far right				
Migrant share low skilled Hispanic	-0.0124** (0.00525)	-0.0144 (0.0124)		-0.0179 (0.0126)
Migrant share low skilled Haiti	-0.0147*** (0.00428)		-0.0469** (0.0229)	-0.0341*** (0.0114)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel B: Center right				
Migrant share low skilled Hispanic	0.000673 (0.00628)	0.0309*** (0.0111)		0.0349*** (0.0134)
Migrant share low skilled Haiti	0.0191*** (0.00394)		0.0643* (0.0333)	0.0393*** (0.0117)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel C: Left wing				
Migrant share low skilled Hispanic	-0.0125*** (0.00471)	0.0100 (0.00780)		0.0128 (0.00818)
Migrant share low skilled Haiti	0.0111*** (0.00272)		0.0369* (0.0195)	0.0277** (0.0108)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel D: Other				
Migrant share low skilled Hispanic	0.0243*** (0.00792)	-0.0265** (0.0125)		-0.0298** (0.0137)
Migrant share low skilled Haiti	-0.0156*** (0.00369)		-0.0543* (0.0306)	-0.0329*** (0.0126)
Controls	Yes	Yes	Yes	Yes
Observations	345	345	345	345
Panel E: First stage regressions				
<i>Endogenous variable: Low skilled migrant share, Hispanic countries and Haiti</i>				
Instrument Hispanic		0.000107*** (2.82e-05)		0.000107*** (2.90e-05)
Instrument Haiti			6.88e-05*** (8.85e-06)	8.13e-05*** (9.43e-06)
F-Test		14.46	60.41	22.48
Partial R ²		0.139	0.0260	
Partial R ² (Hispanic)				0.139
Partial R ² (Haiti)				0.0342

Notes: The table reports OLS and 2SLS estimates for the effect of changes in low-skilled migration from Hispanic countries and Haiti on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variables are the low-skilled migrant share from Hispanic countries and the low-skilled migrant share from Haiti, each defined as the change in the number of immigrants from the corresponding origin group divided by the municipal population and standardized. The shift-share instruments are constructed following Equation 4. All results are estimated at the municipal level. All columns include the proportion of women and the elderly dependence rate in each municipality in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results for both endogenous variables, including the Kleibergen–Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

E IV Results Robustness Analyses without Santiago

Table E.8: 2SLS model: Migration changes and vote share changes in local labour markets excluding Santiago

Dependent Variable % vote	Models		
	OLS	IV	IV - Control
Panel A: Far right			
Migration ratio change	-0.0233*** (0.00639)	-0.0572*** (0.0168)	-0.0480*** (0.0179)
Controls	No	No	Yes
Observations	284	284	284
Panel B: Center right			
Migration ratio change	0.00475 (0.00717)	0.0456** (0.0196)	0.0446** (0.0196)
Controls	No	No	Yes
Observations	284	284	284
Panel C: Left wing			
Migration ratio change	-0.0195*** (0.00403)	-0.0450*** (0.00998)	-0.0429*** (0.0116)
Controls	No	No	Yes
Observations	284	284	284
Panel D: Other			
Migration ratio change	0.0381*** (0.00591)	0.0566*** (0.0155)	0.0463*** (0.0164)
Controls	No	No	Yes
Observations	284	284	284
Panel E: First stage regression			
<i>Endogenous variable: Migration ratio change</i>			
Instrument		7.25e-05*** (2.25e-05)	6.48e-05*** (1.71e-05)
F-Test		10.40	14.40
Partial R ²		0.0501	0.0442

Notes: The table reports OLS and 2SLS estimates for the effect of changes in migration on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variable is the migration ratio change, defined as the change in the number of immigrants divided by the local population and standardized. The shift-share instrument is constructed following Equation 4. All results are estimated at the level of local labour markets, excluding the city of Santiago. Columns with controls include the proportion of women and the elderly dependence rate in each labour market in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results, including the Kleibergen-Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table E.9: 2SLS model: High-skilled and low-skilled migration changes and vote share changes in local labour markets excluding Santiago

Dependent Variable % vote	Models			
	OLS	IV	IV	IV
Panel A: Far right				
High-skilled migrant share	-0.00935 (0.00713)	-0.0353*** (0.00939)		-0.0321*** (0.0118)
Low-skilled migrant share	-0.0133 (0.00889)		-0.0524** (0.0216)	-0.00843 (0.0189)
Controls	Yes	Yes	Yes	Yes
Observations	284	284	284	284
Panel B: Center right				
High-skilled migrant share	0.0238*** (0.00697)	0.0429*** (0.00911)		0.0544*** (0.00999)
Low-skilled migrant share	-0.0105 (0.00872)		0.0447** (0.0220)	-0.0299* (0.0153)
Controls	Yes	Yes	Yes	Yes
Observations	284	284	284	284
Panel C: Left wing				
High-skilled migrant share	0.0149*** (0.00446)	-0.0177** (0.00770)		0.00635 (0.0108)
Low-skilled migrant share	-0.0243*** (0.00531)		-0.0542*** (0.0142)	-0.0629*** (0.0201)
Controls	Yes	Yes	Yes	Yes
Observations	284	284	284	284
Panel D: Other				
High-skilled migrant share	-0.0294*** (0.00498)	0.0100 (0.0116)		-0.0286** (0.0141)
Low-skilled migrant share	0.0480*** (0.00646)		0.0619*** (0.0191)	0.101*** (0.0265)
Controls	Yes	Yes	Yes	Yes
Observations	284	284	284	284
Panel E: First stage regressions				
<i>Endogenous variables: High-skilled and Low-skilled migrant share</i>				
Instrument for high-skilled		0.000340*** (9.84e-05)		0.000155 (0.000114)
Instrument for low-skilled			8.86e-05*** (2.05e-05)	0.000158*** (2.95e-05)
F-Test		11.96	18.71	9.322
Partial R ²		0.0628	0.0351	
Partial R ² (High-skilled)				0.0719
Partial R ² (Low-skilled)				0.0456

Notes: The table reports OLS and 2SLS estimates for the effect of changes in high-skilled and low-skilled migration on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variables are the high-skilled migrant share and the low-skilled migrant share, each defined as the change in the number of immigrants in the corresponding skill group divided by the local population and standardized. The shift-share instruments are constructed following Equation 4. All results are estimated at the level of local labour markets, excluding the city of Santiago. All columns include the proportion of women and the elderly dependence rate in each labour market in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results for both endogenous variables, including the Kleibergen–Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

F IV Results with an alternative base year (2012)

Table F.10: 2SLS model: Migration changes and vote share changes in municipalities using 2012 pre-shares

Dependent Variable % vote	Models		
	OLS	IV	IV - Control
Panel A: Far right			
Migration ratio change	-0.0211*** (0.00510)	-0.0166 (0.0128)	-0.0135 (0.0105)
Controls	No	No	Yes
Observations	345	345	345
Panel B: Center right			
Migration ratio change	0.0120** (0.00525)	0.0307*** (0.0113)	0.0277*** (0.0100)
Controls	No	No	Yes
Observations	345	345	345
Panel C: Left wing			
Migration ratio change	-0.00730 (0.00468)	0.0142** (0.00684)	0.0143*** (0.00516)
Controls	No	No	Yes
Observations	345	345	345
Panel D: Other			
Migration ratio change	0.0164** (0.00818)	-0.0282** (0.0134)	-0.0285*** (0.00956)
Controls	No	No	Yes
Observations	345	345	345
Panel E: First stage regression			
<i>Endogenous variable: Migration ratio change</i>			
Instrument		5.32e-05*** (1.57e-05)	5.40e-05*** (1.50e-05)
F-Test		11.53	12.98
Partial R ²		0.171	0.185

Notes: The table reports OLS and 2SLS estimates for the effect of changes in migration on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variable is the migration ratio change, defined as the change in the number of immigrants divided by the municipal population and standardized. The shift-share instrument is constructed using 2012 migrant shares and follows Equation 4. All results are estimated at the municipal level. Columns with controls include the proportion of women and the elderly dependence rate in each municipality in 2021. The upper panels present the second stage estimates. Panel E reports the first stage results, including the Kleibergen–Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

G IV Results with LIML

Table G.11: LIML model: Migration changes and vote share changes at the municipal level

Dependent Variable % vote	Models		
	OLS	IV	IV - Control
Panel A: Far right			
Migration ratio change	-0.0211*** (0.00510)	-0.0147 (0.0158)	-0.0124 (0.0129)
Controls	No	No	Yes
Observations	345	345	345
Panel B: Center right			
Migration ratio change	0.0120** (0.00525)	0.0344** (0.0143)	0.0307** (0.0125)
Controls	No	No	Yes
Observations	345	345	345
Panel C: Left wing			
Migration ratio change	-0.00730 (0.00468)	0.0180** (0.00907)	0.0166*** (0.00638)
Controls	No	No	Yes
Observations	345	345	345
Panel D: Other			
Migration ratio change	0.0164** (0.00818)	-0.0376** (0.0182)	-0.0349*** (0.0123)
Controls	No	No	Yes
Observations	345	345	345
Panel E: First stage regression			
<i>Endogenous variable: Migration ratio change</i>			
Instrument		4.60e-05*** (1.71e-05)	4.77e-05*** (1.62e-05)
F-Test		7.238	8.697
Partial R ²		0.124	0.140

Notes: The table reports OLS and limited information maximum likelihood (LIML) estimates for the effect of changes in migration on electoral outcomes between 2017 and 2021. The dependent variable in each panel is the change in the vote share of the corresponding political coalition. The main independent variable is the migration ratio change, defined as the change in the number of immigrants divided by the municipal population and standardized. The shift-share instrument is constructed following Equation 4. All results are estimated at the municipal level. Columns with controls include the proportion of women and the elderly dependence rate in each municipality in 2021. The upper panels present the LIML second stage estimates. Panel E reports the corresponding first stage results, including the Kleibergen-Paap Wald F statistic and the partial R^2 . Robust standard errors are shown in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

H Goldsmith–Pinkham Validity Checks

This appendix presents the robustness exercises used to assess the internal validity of the shift–share instrumental variables strategy. Following [Goldsmith-Pinkham et al. \(2020\)](#), I report the Rotemberg weights associated with each origin-specific component of the instrument and estimate a set of pre-trend regressions for cities and municipalities. These tests evaluate whether the baseline migrant shares predict electoral changes in the pre-treatment period.

Table H.12: Rotemberg weights for the migrant shares

Country	Cities		Municipalities	
	No controls	With controls	No controls	With controls
Venezuela	0.42396002	0.43575713	0.32907793	0.36058816
Haiti	0.18111949	0.18467205	0.19407199	0.17779111
Peru	0.10095579	0.09891052	0.17991684	0.17382301
Bolivia	0.10839509	0.09525903	0.09109958	0.07839336
Colombia	0.09838177	0.09757843	0.11235263	0.11361538
Ecuador	0.01974995	0.01974060	0.02347674	0.02299945
Argentina	0.01090224	0.01112717	0.00759283	0.00796305
Cuba	0.01073014	0.01095566	0.01349934	0.01395571
Dominican Republic	0.00968813	0.00946065	0.01348429	0.01335410
Brazil	0.00771583	0.00789297	0.00660935	0.00707289
China	0.00730771	0.00709302	0.01233011	0.01201252
Spain	0.00366983	0.00380348	0.00253341	0.00298700
United States	0.00345887	0.00358391	0.00251590	0.00292452
Paraguay	0.00250695	0.00244576	0.00216741	0.00213559
Mexico	0.00240131	0.00244779	0.00190060	0.00211542
Uruguay	0.00176846	0.00180176	0.00153295	0.00164497
France	0.00125779	0.00129907	0.00086010	0.00104458
Italy	0.00119127	0.00123445	0.00083541	0.00096941
India	0.00074839	0.00071221	0.00085532	0.00086884
Germany	0.00054814	0.00057581	0.00030282	0.00039322
El Salvador	0.00045850	0.00047009	0.00045914	0.00050169
South Korea	0.00044612	0.00045396	0.00042003	0.00046342
Russia	0.00035188	0.00036931	0.00029669	0.00034125
United Kingdom	0.00031796	0.00033115	0.00021753	0.00025984
Canada	0.00031342	0.00032044	0.00022541	0.00025134
Japan	0.00030066	0.00030418	0.00021131	0.00024929
Costa Rica	0.00029912	0.00030799	0.00019462	0.00023239
Panama	0.00025087	0.00026012	0.00031503	0.00033301
Australia	0.00022662	0.00023032	0.00022594	0.00024084
Netherlands	0.00014757	0.00015336	0.00010244	0.00011828
Taiwan	0.00011927	0.00011649	0.00012845	0.00012837
Switzerland	0.00010485	0.00011304	0.00005821	0.00007350
Belgium	0.00010456	0.00010800	0.00006249	0.00007501
Sweden	0.00006425	0.00007182	0.00004661	0.00005399
Israel	0.00003717	0.00003828	0.00002056	0.00002550

Notes: This table reports the Rotemberg weights associated with the shift–share instruments used in the analysis. The weights measure the proportional contribution of each origin country to the identifying variation in the migrant share instrument. Columns separate specifications estimated at the local labour market and municipality levels, with and without covariates. The covariates are the proportion of women and the elderly dependence rate in each municipality in 2008.

Table H.13: Pre-trend regressions for Local Labour Markets (cities)

Variables	Dependent variable: change in % vote			
	Right-Wing (2005)	Right-Wing (1989-2005)	Left-Wing (2005)	Left-Wing (1989-2005)
Panel A: Main Instrument with 2008 migrant shares				
Instrument	0.000586 (0.000447)	0.000912 (0.000570)	-0.000586 (0.000447)	-0.000912 (0.000570)
Controls	Yes	Yes	Yes	Yes
Observations	283	282	283	282
R-squared	0.058	0.098	0.058	0.098
Panel B: Venezuela Share				
Venezuela migrant share 2008	0.000573 (0.000444)	0.00111 (0.000677)	-0.000573 (0.000444)	-0.00111 (0.000677)
Controls	Yes	Yes	Yes	Yes
Observations	283	282	283	282
R-squared	0.058	0.099	0.058	0.099
Panel C: Haiti Share				
Haiti migrant share 2008	0.000377 (0.000299)	0.000853 (0.000523)	-0.000377 (0.000299)	-0.000853 (0.000523)
Controls	Yes	Yes	Yes	Yes
Observations	283	282	283	282
R-squared	0.058	0.098	0.058	0.098
Panel D: Peru Share				
Peru migrant share 2008	0.000493 (0.000369)	0.000587 (0.000537)	-0.000493 (0.000369)	-0.000587 (0.000537)
Controls	Yes	Yes	Yes	Yes
Observations	283	282	283	282
R-squared	0.058	0.098	0.058	0.098
Panel E: Top five origin countries				
Top five migrant share 2008	0.000590 (0.000459)	0.000594 (0.000567)	-0.000590 (0.000459)	-0.000594 (0.000567)
Controls	Yes	Yes	Yes	Yes
Observations	283	282	283	282
R-squared	0.058	0.098	0.058	0.098

Notes: This table reports OLS regressions of the instrument and the migrant shares of the origin countries that receive the largest Rotemberg weights on the pre-period electoral changes. The dependent variables are changes in right-wing and left-wing vote shares in the 2005 election, and changes between 1989 and 2005. All specifications include demographic controls. Standard errors are robust.

Table H.14: Pre-trend regressions for municipalities

Variables	Dependent variable: change in % vote			
	Right-Wing (2005)	Right-Wing (1989-2005)	Left-Wing (2005)	Left-Wing (1989-2005)
Panel A: Main Instrument with 2008 migrant shares				
Instrument	-0.000599 (0.00117)	0.000136 (0.00236)	0.000599 (0.00117)	-0.000136 (0.00236)
Controls	Yes	Yes	Yes	Yes
Observations	341	334	341	334
R-squared	0.070	0.120	0.070	0.120
Panel B: Venezuela Share				
Venezuela migrant share 2008	-0.000596 (0.00128)	0.00151 (0.00254)	0.000596 (0.00128)	-0.00151 (0.00254)
Controls	Yes	Yes	Yes	Yes
Observations	341	334	341	334
R-squared	0.070	0.120	0.070	0.120
Panel C: Haiti Share				
Haiti migrant share 2008	-0.000794 (0.000530)	-0.000947 (0.00113)	0.000794 (0.000530)	0.000947 (0.00113)
Controls	Yes	Yes	Yes	Yes
Observations	341	334	341	334
R-squared	0.070	0.120	0.070	0.120
Panel D: Peru Share				
Peru migrant share 2008	-0.000248 (0.000920)	-0.000573 (0.00158)	0.000248 (0.000920)	0.000573 (0.00158)
Controls	Yes	Yes	Yes	Yes
Observations	341	334	341	334
R-squared	0.070	0.120	0.070	0.120
Panel E: Top five origin countries				
Top five migrant share 2008	-0.000508 (0.00108)	-0.00111 (0.00188)	0.000508 (0.00108)	0.00111 (0.00188)
Controls	Yes	Yes	Yes	Yes
Observations	341	334	341	334
R-squared	0.070	0.120	0.070	0.120

Notes: This table reports OLS regressions of the instrument and the migrant shares of the origin countries that receive the largest Rotemberg weights on the pre-period electoral changes. The dependent variables are changes in right-wing and left-wing vote shares in the 2005 election, and changes between 1989 and 2005. All specifications include demographic controls. Standard errors are robust.

Chapter 2

When Politicians Are Older Than the Public: Age Gaps and
Support for Democracy

When Politicians Are Older Than the Public: Age Gaps and Support for Democracy ^{*}

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Abstract

Many democracies elect political leaders who are older than the public. Drawing on original surveys from Italy, South Korea, and the United States, we find evidence that these age gaps can weaken confidence in representation and reduce support for democracy. Further analysis of cross-national surveys from dozens of democracies reinforces these findings. However, in a conjoint experiment, we also show that age gaps weaken evaluations of representational quality less so than gender gaps. Results are similar across age cohorts, suggesting that age gaps constitute a general challenge to democratic support rather than fueling intergenerational conflict over democracy. These findings imply that democracies can modestly improve public satisfaction by increasing representation of younger generations.

1 Introduction

Prominent politicians in democracies are typically older than the average adult citizen in their countries ([Stockemer & Sundström, 2023](#)), because older people are more likely to run for office and older politicians benefit from the political capital that comes with experience and incumbency ([McClean & Ono, 2024](#); [Roberts & Wolak, 2023](#)). This pattern has received increasing attention in recent years, as ageing demographics have further strengthened the political power of older generations ([Berry, 2014](#)). A major concern in this discussion is that ageing political leadership could weaken confidence in democracy by undermining perceptions of effective representation of the population ([Buchmeier & Vogt, 2024](#); [Stockemer & Sundström, 2023](#)). In the United States, for example, the advanced ages of former President Biden, President Trump, and much of the Congressional leadership have prompted a flurry of warnings that the American government has become

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an unresponsive gerontocracy (Jackson, Newall, & Rollason, 2024; Moyn, 2024; Smith, 2023; Xiao, 2023).

Age matters for how people think about politics and the kinds of policies they tend to prefer (Roberts & Wolak, 2023). As a result, people may notice when politicians are older than the public at large, especially if the average political leader is relatively advanced in age. These perceived age gaps may lead people to conclude that the political system is failing to give certain age groups a voice in the government and will not represent their interests well (Buchmeier & Vogt, 2024). Since representation is a core function of modern democracy, people who believe the political system is not adequately representing certain age groups may become less satisfied with the quality of democracy in their country; furthermore, if they conclude that age gaps are a recurring feature of democratic governance, they may become less supportive of democracy more generally. These declines in *specific* and *diffuse* support for democracy can both be problematic for democratic stability (Claassen, 2020a; Claassen & Magalhães, 2022; Norris, 2011a). Furthermore, we might expect these reductions in democratic support to be particularly pronounced among younger people, who are most underrepresented as a result of their age (Stockemer & Sundström, 2022b). This reaction by younger people could explain why this group has, in some countries, shown less robust support for democracy in recent years (Claassen & Magalhães, 2023; Foa, Klassen, Wenger, Rand, & Slade, 2020).

Do age gaps between political leaders and the public undermine support for democracy, and are these effects most consequential for the democratic attitudes of younger people? Despite growing interest in the effects of ageing demographics on representation (Buchmeier & Vogt, 2024; Stockemer & Sundström, 2023), as well as increasing demand to understand the resilience of popular support for democracy at a moment of global democratic stagnation (Adserà, Arenas, & Boix, 2023; Braley, Lenz, Adjodah, Rahnama, & Pentland, 2023), there has been little empirical research into the consequences of older political leaders for the public's views of democracy. We consider these questions with experimental and descriptive data from two rounds of original surveys conducted in the ageing democracies of Italy, South Korea, and the United States. These countries vary in the size of the age gap between political leaders and the voting-age public, offering a useful test of whether attitudes shift depending on how pronounced these patterns are. We complement this analysis with additional cross-national data from dozens of democracies over several decades.

We study age gaps specifically in terms of national legislators, who are meant to represent the public at large, and we identify deficiencies in age-based representation by the average age of legislators as well as the difference between legislators and the adult public. We find evidence that age gaps can weaken satisfaction with democracy and may undermine more principled democratic support, though the effects appear relatively modest and may be less important than other types of representational imbalances. In

the first round of our original surveys, respondents who perceive political leaders in their country as too old report lower satisfaction with the quality of their country’s democracy, though they express somewhat higher support for democracy as a system of government. An experiment conducted in the second round of surveys aligns with this pattern. Respondents provided with information about the underrepresentation of younger people in their country’s political leadership become more negative about the performance of their democracies, but do not express less principled support for democracy. We conduct further analyses of cross-national data, which show that having older legislators – both in absolute terms and relative to the adult public – is associated with reduced satisfaction with democracy, as well as lower principled support for democracy. Finally, we return to our original survey data to assess the relative importance of age-based representation in a conjoint experiment. Here, we find that age gaps can undermine perceptions of how well legislatures will represent the public, but less so than some other representation deficits, particularly gender. Across these analyses, the negative reactions to age gaps are not exclusively driven by younger respondents, but reflect a broader aversion that is detectable among other age groups.

In short, we find modest evidence that the dominance of older politicians can undermine satisfaction with democracy in one’s own country, and may weaken support for democracy more generally. The similarity of these results across age groups suggests that the public response reflects a general dislike of deficient descriptive representation rather than substantive concerns over the policies promoted by political leaders of different ages. This consistency also implies that age gaps do not necessarily fuel intergenerational conflict over democracy, but may constitute a broader challenge for sustaining the public’s confidence in how democracies are performing. As such, the findings imply that democratic legitimacy and stability can be strengthened by mechanisms that enhance effective representation of all age groups in key political offices. In the context of ongoing debates about the justifiability of age-based legislative quotas or age limits for important public offices ([Bidadanure, 2015](#); [Gramlich, 2023](#); [Schachtschneider, 2024](#); [Trantidis, 2016](#)), this link between age gaps and democratic satisfaction highlights an important benefit that could follow from such quotas. At the same time, the effects of age gaps on democratic attitudes appear to be relatively modest, perhaps because age is less salient than other descriptive characteristics – especially gender – in shaping perceptions of representational quality. More generally, these findings contribute to the understanding of how and in which contexts representation can affect government legitimacy and support for democracy ([Clayton, O’Brien, & Piscopo, 2026](#)).

The paper proceeds as follows. We first expand on why age gaps may influence attitudes toward democracy and whether we should expect these effects to be more pronounced among some people than others. We then proceed through our four analyses, beginning with observational and experimental analyses of our original survey data, then

moving to our cross-national analysis using the World Values Survey and the pooled Barometer Surveys (Eurobarometer, Latinobarometer, Afrobarometer, and Asianbarometer), and ending with our conjoint experiment assessing the importance of age relative to other characteristics. We conclude with further discussion of implications for democratic resilience as democracies continue to age over the next several decades.

2 Age, Representation, and Democratic Support

Alongside characteristics such as race and gender, age is an important political trait that can shape how political leaders govern and how individuals engage with democratic politics (Kurz, Wurthmann, & Gross, 2025; McClean & Ono, 2024; Stockemer & Sundström, 2022b). Age groups often differ in values, worldviews, and material interests, leading to divergent political preferences and priorities, particularly in relation to government spending and social and economic policy (Sorensen, 2013). Reflecting these differences, younger and older political leaders tend to prioritize different policy issues and to make different governance decisions (Curry & Haydon, 2018; McClean, 2026). Age also matters in the electorate. Individuals of different ages tend to vote differently, and candidates' ages can affect the level of support they receive from voters (McClean & Ono, 2024; Roberts & Wolak, 2023). In addition, older people typically possess more time and resources, allowing them to vote and participate more regularly in politics (Blais & Rubenson, 2013).

Populations in many democracies are trending older, and researchers have posited that age will become increasingly important for politics in these contexts (Goerres & Vanhuysse, 2021). High-level political leaders already tend to be older than the average adult citizen in their countries due to the advantages that come with experience, incumbency, and greater resources (Belschner, 2024; McClean & Ono, 2024; Roberts & Wolak, 2023; Stockemer & Sundström, 2023). As populations age, these advantages may become stronger, further expanding the political power of older generations (Berry, 2014). In several countries, this shift has contributed to the emergence of political leadership that is visibly older than the country at large (Kurz & Ettensperger, 2023; Stockemer & Sundström, 2022b), of which the United States is the most prominent example, even as evidence suggests that voters would prefer to be represented by younger candidates (Eshima & Smith, 2022).

The growing political power of older generations, along with the potential for age-based representation deficits, has motivated concerns about the consequences for democratic support in affected countries (Silva, 2025). We argue that such effects are plausible, given the important relationship between age, representation, and democracy. Representation of the public is meant to be a core function of modern democratic governance (Wessels, 2009), so dissatisfaction with the quality of representation may plausibly un-

dermine support for democracy (Clayton et al., 2026). There are different aspects to representational quality. For one, people tend to value descriptive representation, or the degree to which the identities of representatives align with those of the public. When this descriptive representation is present, people are more likely to perceive decisions as legitimate and democratic (Arnesen & Peters, 2018; Bush & Zetterberg, 2021; Clayton, O’Brien, & Piscopo, 2019). Since age is a salient characteristic of political leaders (Hainmeuller, Hopkins, & Yamamoto, 2014; Roberts & Wolak, 2023; Sevi, 2021), it is plausible that people would value descriptive representation based on age. People also care about substantive representation, i.e., the extent to which their interests are effectively reflected in policymaking. As discussed, older political leaders often have different worldviews and priorities than younger citizens, and they govern in ways that conflict with the demands of this group (Buchmeier & Vogt, 2024; Chrisp & Pearce, 2019). Generational divides are especially pronounced on major issues such as climate change (Ross & Rouse, 2022), and policies more often reflect what older voters want. Faced with these differences, people may conclude that the political system will provide less effective substantive representation when there is a mismatch between the ages of important government officials and the ages of the public.

If age gaps weaken confidence in the quality of descriptive and substantive representation, we argue that they could lead to lower *specific* and *diffuse* support for democratic governance (Grant, 2021; Silva & Wrátil, 2023). The former refers to a decline in satisfaction with how democracy is performing in one’s own country (Linde & Ekman, 2003; Norris, 2011a), while the latter reflects a weakening of principled support for democratic governance (Claassen & Magalhães, 2022). We expect that age gaps would produce stronger effects on specific democratic support as opposed to diffuse democratic support.¹ Individuals who believe that an age gap has stifled representation in their country may become less satisfied with how their democracy is working, while also perceiving this lack of representation as a violation of democratic principles and therefore irrelevant to their broader support for democracy. However, if people conclude that age gaps are a common flaw of democratic political systems, they might also become less supportive of democratic governance in principle. Both outcomes can be important for democratic stability. Higher principled support for democracy is associated with more durable democratic governance, and higher satisfaction with democratic performance can strengthen this principled support as well as regime stability (Claassen, 2020a; Claassen & Magalhães, 2022; Norris, 2011a). This leads to our first two expectations:

E1: Age gaps between political leaders and the public reduce satisfaction with how democracy is performing in one’s own country (specific democratic support).

E2: Age gaps between political leaders and the public may also reduce prin-

¹This difference was specified in our pre-analysis plan.

ciplined support for democratic governance in general (diffuse democratic support), though we expect this effect to be weaker than E1.

These negative reactions to age gaps may be more pronounced among younger people, who are frequently underrepresented in key positions in legislatures and cabinets (Kurz & Ettensperger, 2023; Stockemer & Sundström, 2022b). Feeling that they are excluded from the political process and lack both descriptive and substantive representation in the halls of power, younger people may become especially dissatisfied with their democracies and more likely to conclude that democracy does not serve its intended purpose. There is evidence that younger people in some countries have become relatively less supportive of democracy in recent years (Claassen & Magalhães, 2023; Foa et al., 2020), and both scholarly accounts and the popular press have suggested this pattern may be rooted partly in frustrations with ageing political leaders who are unwilling or unable to respond to their preferences (Berthin, 2023; Rosenblum, 2023). Along these lines, Silva (2025) finds that younger people are less satisfied with democracy when the party they favor is led by an older politician, and vice versa for older people. In general, however, there is little evidence as to whether younger people may be less satisfied with democracy *because* their age group is underrepresented. This leads to our third expectation:

E3: The negative effects of age gaps on both specific and diffuse democratic support will be more pronounced among younger citizens, who are most directly affected by age-based representation deficits.

Yet, even if age gaps shape attitudes toward democracy, it is possible that they matter less than other potential deficiencies in representation. Compared to other individual characteristics that are politically and socially salient, age is somewhat different, insofar as most people expect to become older and can remember being younger (North & Fiske, 2012). As a result, people may react less strongly to age-related political issues because they can expect to reap the political benefits of older age themselves at some point. This possibility contrasts with race or gender, for instance, where society places most people in fixed categories that are less likely to change significantly throughout their lives. Thus, concerns about representational quality, and by extension democracy, may be more pronounced when representation gaps arise for these other characteristics. Certainly in the United States, deep rooted issues of race and gender have been more salient and polarizing in recent years, even as high levels of the US government have become increasingly unrepresentative along generational lines. On the other hand, the intersectional nature of these generational issues could lead to widespread emphasis on age as a particularly important driver of dissatisfaction with representation and democracy. If younger people of various gender, racial, and other backgrounds share a sense of grievance fueled by a perceived political exclusion based on age, they may place significant emphasis on this factor when they evaluate representation in their democratic political system (Stockemer & Sundström, 2022b). As such, it is important to investigate the influence of age

gaps on perceptions of representational quality relative to possible deficits based on other demographic characteristics.

The analyses that follow assess our three expectations. We test whether age gaps reduce specific and diffuse democratic support, and we analyze whether these effects are concentrated among younger citizens. As our final step, we investigate how age gaps compare to other representation deficits in shaping perceptions of representational quality, particularly relative to gender and race.

3 Research Design

To assess how and for whom age gaps affect support for democracy, we conduct several analyses, consisting of two parts: an analysis of original surveys fielded in Italy, Korea, and the United States including both observational and experimental modules, and a cross-national study using the World Values Survey and the pooled data from four regional Barometer surveys (Afrobarometer, Latinobarometer, Asianbarometer, and Eurobarometer). We preregistered our experiments on OSF.²

Deficiencies in age-based representation may become visible to the public in different ways. In some cases, people may be aware of differences between the average ages of political leaders and adult citizens in the country. However, if people are not aware of the country’s demographic patterns, they may still intuit problems with age-based representation by observing that important political leaders tend to be from older generations. We use measures that reflect both possibilities. In some of our analyses, we focus on the difference between the ages of important political leaders and the ages of adult citizens in a given country. In other analyses, we focus on the ages of the politicians themselves, without reference to the age distribution of the public. By operationalizing age gaps in both relative and absolute terms, we can better assess how mismatches between the age of political elites and the public are associated with support for democracy.

We chose to run our surveys in Italy, South Korea, and the United States because they vary in the extent of their age gaps, as documented in Appendix A. The United States displays the largest gap between the average age of legislators and the adult public among liberal democracies, while South Korea sits closer to the middle of the distribution and Italy has a comparatively small gap (Figure A.2). However, similar age gaps can mask important differences in representation. Despite its smaller overall gap, South Korea strongly underrepresents younger citizens and overrepresents older ones, much like the United States, while Italy shows more moderate underrepresentation across age groups (Figure A.3). The three countries also differ in their demographic trajectories: Italy has

²We preregistered the experiments in Open Science Framework (OSF) on February 11, 2024. Our OSF pre-registration protocol is registered at the following link: <https://osf.io/6rcq2>. The text is included in the Supplementary Materials Section I

had one of the oldest populations in the world since the 1990s, South Korea is one of the fastest-ageing societies globally, and the United States follows a more gradual ageing path (Figure A.1). This variation in both the size and the nature of age-based representation deficits, combined with differences in region, culture, democratic history, and institutions, makes these three countries a useful set of cases for assessing whether similar patterns pertain across different contexts.

Two rounds of surveys were conducted in each of the three countries. Data collection for the first round of surveys occurred between December 18, 2023 and January 2, 2024. Data collection for the second round of surveys occurred between February 16, 2024 and February 22, 2024. For both rounds, the samples were acquired by Cint, with quotas for age, educational attainment, and gender. We acquired approximately 2,000 responses in Italy and 1,750 responses in both South Korea and the United States for each round.³ The first round of surveys included descriptive questions about attitudes toward age-based representation, democracy, and ageism. The second round of surveys included an experiment in which some respondents were randomly assigned to information about the age gap in their country prior to describing their attitudes toward democracy, in addition to a conjoint experiment designed to assess the relative importance of age gaps in perceptions of representational quality.

For our cross-national analysis, we rely on global survey data on attitudes toward democracy, as well as data on national legislator ages, to analyze the country-level correlation between age gaps and support for democracy. We provide more detailed information about each analysis prior to reporting our results in the next section.

4 Results

We report results from four analyses. First, we use our initial wave of original survey data from Italy, South Korea, and the United States to analyze whether people who believe politicians are too old in their countries demonstrate less satisfaction with and support for democracy. Next, we rely on the second wave of original surveys in the three countries to analyze results from our experiment in which we randomize exposure to information about age gaps in the country. We then turn to the cross-national data on democratic support and parliamentary ages to study the country-level relationship between age gaps and attitudes toward democracy. Finally, we return to our original survey data, drawing on the conjoint experiment to test how age gaps influence perceptions of representational effectiveness relative to other representation gaps. We proceed through each of these analyses in turn.

³The surveys were implemented on Qualtrics and programmed by the authors. The Italian and Korean surveys were translated by professional translators hired through Cint. They were then checked by native speakers associated with the authors.

4.1 People Who Think Politicians Are Too Old Are Less Satisfied with Their Democracies

If age-based representation deficits prompt more negative views of democracy, we should observe that perceptions of politicians' ages are related to attitudes toward democratic governance. If age gaps primarily affect the attitudes of younger citizens, these relationships should be more pronounced among younger adults.

We assess these expectations with our first wave of original survey data from Italy, South Korea, and the United States. Our primary independent variable is a question that asks respondents whether they think politicians in their country are generally too old, too young, or about the right age. Answers to this question should capture whether respondents believe an age-based representation deficit exists in their country. In particular, we create a dichotomous variable equal to 1 if respondents select "too old" and 0 otherwise. Between 1-2 % of respondents believe politicians are too young, whereas 53% of respondents believe politicians are too old, and the remaining 45% think politicians are about the right age. Younger respondents are not more likely than older ones to believe politicians are too old.

Our outcome variables include measures of both *specific* and *diffuse* support for democracy.⁴ Regarding specific support, we record respondents' level of satisfaction with how democracy is working in their country on a ten-point scale. Regarding diffuse support, we ask respondents how important it is that they live in a democracy. We also create an index capturing general support for democracy, which draws on four questions: whether democracies do not perform well economically, whether they are indecisive, whether they are ineffective at maintaining order, and whether democracy is the best system of government. Descriptive statistics for all outcome variables, before standardization, are reported in Table B.1.

We regress these measures of democratic support, all standardized, on the indicator for believing politicians are too old, controlling for relevant demographic characteristics and political views. We control for age, gender, education, religiosity, unemployment, and ideology on a liberal to conservative scale⁵. We also control for anxiety about ageing, which is a predictor for ageism, as well as respondent ethnocentrism, to capture a more general tendency toward prejudice. These latter controls should address the possibility that a link between perceiving politicians as too old and viewing democracy less favorably

⁴Full question wording is provided in Appendix B2.

⁵One concern in studies of this kind is that more politically sophisticated respondents may be simultaneously more attentive to descriptive representation deficits and more critical of their democracy, potentially confounding the relationship of interest. While our surveys do not include a dedicated political knowledge or interest scale, we include education as a control variable. Education is strongly and consistently associated with political knowledge, interest, and engagement, and is widely used in the literature as a proxy for cognitive resources and political awareness when direct measures are unavailable (Le & Nguyen, 2021; Persson, 2015; Verba, Schlozman, & Brady, 1995).

Table 1: Belief that Politicians are Too Old and Support for Democracy

	Dem. Satisfaction <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Politicians Too Old	-0.330*** (0.026)	-0.330*** (0.030)	0.100*** (0.026)	0.069** (0.029)	0.006 (0.025)	-0.024 (0.030)
Young	0.015 (0.028)	0.015 (0.037)	-0.441*** (0.030)	-0.503*** (0.042)	-0.448*** (0.026)	-0.508*** (0.036)
Politicians Too Old $\times$ Young		0.000 (0.055)		0.118** (0.060)		0.114** (0.051)
R ²	0.11	0.11	0.11	0.12	0.19	0.19
Observations	5,685	5,685	5,691	5,691	5,681	5,681
Individual Controls	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. The results include country fixed-effects, and control variables, such as gender, education, age, working status, religion, income, ideology, anxiety about ageing and ethnocentrism. Robust SE in parenthesis. Signif.: ***: 0.01, **: 0.05, *: 0.1.

is motivated by negative views of older people and their capacity to govern rather than concerns about representation (Roman & Williamson, 2026). We also run the same regressions but with an indicator variable for younger respondents interacted with the measure of whether politicians are too old, which allows us to analyze whether the relationship is stronger among younger people, who we define as those under the age of 35.⁶

The results are reported in Table 1, with data aggregated across Italy, South Korea, and the United States and dummies included for the countries and younger respondents.⁷ Columns 1, 3, and 5 show the respective relationships between support for democracy and the belief that politicians are too old. As expected, we find a large, negative relationship with specific support for democracy (column 1). Respondents who believe politicians are too old are one-third of a standard deviation less likely to be satisfied with how democracy is working in their country, equivalent to a decline of approximately 0.8 points on the original ten-point scale.⁸ By contrast, we do not find that respondents who believe politicians are too old show less diffuse support for democracy. Instead, the results show a small but positive and statistically significant relationship with viewing democracy as important (column 3) and no relationship with the democracy index (column 5). In Section D1 of the Supplementary Materials, we show in Table D1 that these results are consistent in all three countries, especially regarding the negative relationship with specific

⁶Definitions of “youths” can range up to 35, so we use this age to capture younger versus older people. Results are similar with other cut-points.

⁷In Section D1 in the Supplementary Materials, we report similar results separately for each country, and we include results of a more parsimonious estimation without covariates to avoid suppression effects in Table D.4 in the same section.

⁸Please refer to Table B.1 for statistics before standardization.

democratic support.⁹

Columns 2, 4, and 6 include the interaction between believing politicians are too old and being under the age of 35. The interaction is precisely zero for the outcome of specific support for democracy, indicating that both younger and older people are similarly less satisfied with how democracy is performing in their country when they perceive politicians as too old. For the two outcomes capturing diffuse support for democracy, the interaction is positive, suggesting that believing politicians are too old has a stronger positive relationship with general democratic support among younger rather than older people. These patterns hold when using age as a continuous variable rather than a young/old dummy, as shown in Table D.5 in the Supplementary Materials.

The results are consistent with age gaps weakening specific support for democracy without undermining diffuse support. This pattern reflects our expectation that representational problems rooted in age would be more likely to harm satisfaction with democracy in one's own country without necessarily affecting views of democracy more generally. A plausible interpretation of the slight positive correlation with diffuse democratic support is that people view age gaps as contrary to democratic values, and so they believe the solution is to strengthen democratic elements of the political system to address deficits in representation based on age. However, this design cannot rule out reverse causation or bias from omitted variables, and it is possible that individuals dissatisfied with democracy for other reasons would be motivated to describe politicians as too old. We address causation next.

4.2 Information about Age Gaps Lowers Confidence in Democratic Representation

The preceding analysis reveals a substantively important relationship between perceiving the existence of an age-based representation deficit and feeling that democracy is not working well in one's own country. Does exposure to information about age gaps between political leaders and the public cause more negative views of democracy? To probe causality, we implemented a survey experiment on our second round of surveys in Italy, South Korea, and the United States. The experiment randomly assigned respondents to a control group or a treatment group. The control group proceeded directly to outcome questions measuring specific support for democracy in their country and diffuse support for democratic governance¹⁰. We asked three questions to gauge satisfaction with democracy in one's own country. First, we asked respondents if their democracy is good at giving citizens what they want. Second, we asked if their democracy is effective at

⁹Results are also consistent when using an alternative construction of the democracy index, as shown in Table D4 in the Supplementary Materials.

¹⁰Specifically, the control group answers the outcome questions first, after which they also read the same information and answer the same questions about the age gap.

addressing major challenges. Third, we asked if they are satisfied with how democracy is working in their country. We viewed the first two questions as measures of satisfaction with how representation was working in the political system, while the third question provided a measure of general satisfaction with democratic performance. To measure more principled support for democracy, we then asked respondents if it is important for them to live in a democracy, and we asked the same four questions discussed previously that we transform into an index of general democratic approval. Descriptive statistics for all outcome variables in this round of surveys, before standardization, are reported in Panel B of Table B.1.

In the treatment group, respondents were first exposed to information about the age gap between national legislators and adults in their country before answering the questions about democratic support. The treatment included a short paragraph that described the average age of national legislators, the average age of an adult citizen, and the implications of this gap for underrepresentation of adults under the age of 40. This underrepresentation was also presented graphically with a bar chart.¹¹ Thus, the treatment drew attention both to how old legislators were in the country and to how much these ages differed from typical adults. We used neutral language to describe the age gap rather than trying to persuade respondents of its negative implications. After reading the information, respondents were asked if they found it surprising and if they found it concerning. Respondents then proceeded to the outcome questions. The experiment was pre-registered before implementation. We expected stronger effects on the first three outcomes indicating satisfaction with democracy in one’s own country, and we had weaker expectations for effects on diffuse support for democracy.

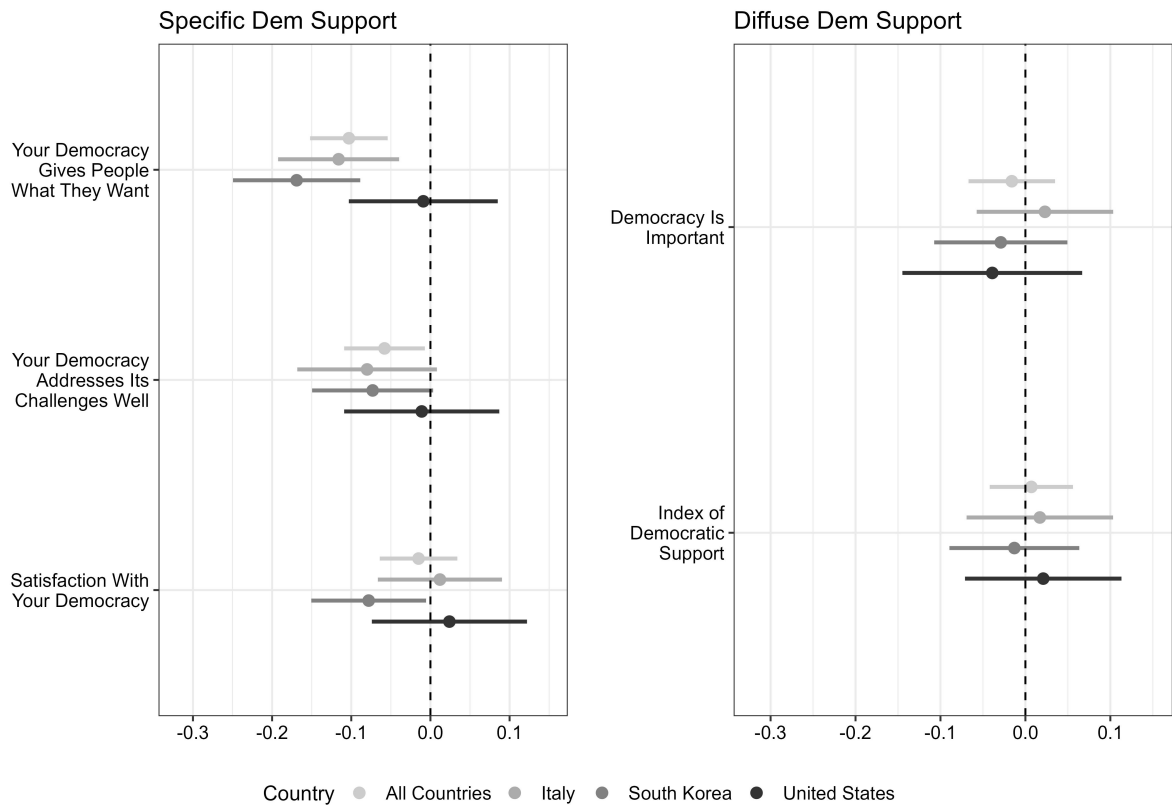
The treatment effects on the five democracy outcomes are presented in Figure 1. The left plot shows the effects on the specific support outcomes, and the right plot shows the effects on the diffuse support outcomes. Results are reported with the full sample across the three countries and within each country, with pre-registered control variables for age, education, gender, region of residence, employment, religion, income, and political ideology to increase precision.¹² We find evidence that providing information about the age gap between legislators and the public reduces specific support for democracy but not diffuse support.

First and foremost, the treatment lowers respondents’ confidence in their democracy’s ability to give the people what they want. The effect is pronounced in South Korea and

¹¹For an example of the vignette, see Section E1 in the Supplementary Materials.

¹²In our pre-registered pre-analysis plan, we indicated that race would be included as a control variable in the USA model. However, to ensure consistency in model specifications across countries, we decided not to include race as a control. This decision reflects the intention to keep all contexts comparables. Importantly, sensitivity analyses confirm that including race as a control in the USA model does not alter the results or conclusions of the study. Effects are similar when dropping control variables. See Section E2 in the Supplementary Materials. Full regression tables underlying Figure 1, including results by country, are reported in Tables E3 in the Supplementary Materials.

Figure 1: Main Effects of Age Gap Treatment on Democracy Outcomes



Notes: OLS estimates and 95% confidence intervals of the average treatment effects of the information experiment. Outcome variables are standardized. The results include region and country fixed-effects, and control variables, such as gender, education, age, working status, religion, income, and ideology. Results without controls are reported in Figure E.5 and full regression tables are reported in Tables E3 in the Supplementary Materials.

Italy, but not in the United States. When pooling all countries together and including country fixed-effects, the effect size is approximately 0.11 standard deviations ($p < 0.01$). The age gap treatment also weakens respondents' confidence that their democracies are effective at addressing major challenges. This effect is present in the aggregated data ($p < 0.05$) and is also observed in Italy and South Korea ($p < 0.10$), but not in the United States. Thus, we find support for the notion that information about age gaps weakens perceptions of their democracy's ability to represent people well by providing them with what they want and responding to important challenges. Effects are less consistent for general satisfaction with democracy in one's country, but the treatment does reduce expressed satisfaction in South Korea ($p < 0.05$). However, the treatment produces no significant effects in any of the countries or in the pooled results on the two outcomes reflecting diffuse support for democratic governance, including how much importance they place on living in a democracy and the index of general democratic approval.

These results align with our pre-registered expectations and the observational findings in the previous section. They indicate that individuals exposed to information about

age-based representation deficits express lower confidence in how well democratic representation is functioning in their country, suggesting a causal effect (though one modest in magnitude) on specific support for democracy. However, this informational exposure does not appear to reduce support for democracy as a political system, indicating no detectable effect on more principled democratic attitudes.

We also explore subgroup effects among younger and older respondents (see Table E.6 in Section E4 in the Supplementary Materials). The treatment effects are relatively consistent by age group. If anything, they are somewhat more consistent among older respondents. This similarity aligns with the correlational findings in the previous section, which show that perceived age gaps are associated with lower specific support for democracy among younger and older respondents alike. This pattern contrasts with the expectation that the negative consequences of age gaps for democratic attitudes would be driven principally by younger citizens who will feel the effects of diminished representation most directly. However, this pattern does align with prior research indicating that people generally view representation deficits as problematic for democracy and legitimate decision-making, even when they themselves are not part of the group that lacks representation (Clayton et al., 2019, 2026).

We had anticipated that effects would be stronger in South Korea and the United States, since their age gaps are larger than the age gap in Italy. However, the results are consistently null in the United States, whereas effects exist for the specific democratic support outcomes in both South Korea and Italy. One explanation for this unexpected result could be that concerns about ageing politicians are already highly salient in the United States, and have therefore been factored into views of the political system, including the quality of democratic governance (Sevi, 2021). Because the age gaps are smaller in South Korea and Italy, respondents may have thought less about this element of the political system, in which case the treatment effects would plausibly be larger (Druckman & Leeper, 2012). Alternatively, broader political polarization and dissatisfaction with democracy in the United States could also play a role, though it is worth noting that US respondents in our sample report higher baseline satisfaction with democracy than respondents in Italy and South Korea (Table B.1), which argues against a simple ceiling effect in democratic dissatisfaction. We return to this question in the discussion section, where we consider the implications of the US null result alongside the broader pattern of findings.

4.3 Larger Country-Level Age Gaps are Associated with Less Democratic Support

We have shown evidence that perceptions of age-based representation deficits are associated with reduced specific support for democracy, and that exposure to information about age gaps in an individual’s country lowers confidence in the ability of their democracy

to represent the people well. Do these individual-level patterns extend to countries more generally? Are people less satisfied on average with the quality of their democracy in countries where older politicians are more dominant? And does this loss of democratic satisfaction extend to less principled support for democracy as well?

We merge several existing datasets to answer these questions. To measure gaps in age-based representation, we use age data on all members of a country’s legislature in a given year from the Worldwide Age Representation in Parliaments Dataset (WARP) developed by [Stockemer and Sundström \(2022a\)](#),¹³ and population data projections prepared by the Department of Economic and Social Affairs of the United Nations in 2022 ([United Nations & Social Affairs, 2022](#)).¹⁴ Since legislators are meant to function as the public’s representatives in a democracy ([J. Chu & Williamson, 2025](#)), the age data on legislators provides a natural test of the consequences of age gaps. We create two independent variables. First, we measure the difference between legislators and adult citizens by subtracting the average age of adults from the average age of legislators. Second, we use the average age of the legislators, since the public may not be familiar with their country’s age distribution and may assume that visibly older political leaders equate to a more pronounced age-based representation deficit.

To measure attitudes toward democracy, we use multiple waves of the World Values Survey (WVS) as well as a pooled dataset of four regional Barometer surveys: the Eurobarometer, Latinobarometer, Afrobarometer, and Asianbarometer. We limit the sample to countries rated as electoral or liberal democracies by Varieties of Democracy ([Coppedge, Gerring, Knutsen, et al., 2023](#); [Pemstein et al., 2023](#)), since the argument that age gaps will reduce support for democracy applies most plausibly to countries that are already democratic.¹⁵ The WVS sample includes responses from 70 countries between 1994 and 2022, incorporating long-established and new democracies as well as countries at varying levels of development from all regions of the world. The Barometer surveys add a substantial number of respondents across regions and years, covering 77 countries between 1995 and 2023 with much more frequent waves¹⁶.

¹³The dataset includes information on the electoral results of parliaments between 1945 and 2023 for 195 countries or territories. Furthermore, it contains information about the composition of elected parliaments, such as the distribution of parliamentarians by age group and gender, the average and median age in each parliament, the number of elected parliamentarians, and coverage information on the data collected.

¹⁴The dataset includes population estimates for all countries and regions of the world between 1950 and 2021. We accessed the data on April 16th, 2024, at <https://population.un.org/wpp/Download/Standard/Population/>

¹⁵If citizens of an authoritarian regime are disgruntled with older political leadership, they might plausibly become less satisfied with how their political system is working. Since autocracies try to convince their citizens that they govern democratically, this dissatisfaction could spill over into attitudes toward democracy ([J. A. Chu, Williamson, & Yeung, 2024](#)). Conceptually, however, we are interested in how democracies perform at representing different age groups, so we focus only on democratic countries.

¹⁶Details about the countries and number of respondents included in each analysis are reported in Tables F.15 and G.22 in Appendices F and G respectively.

We regress several measures of democratic support on our two measures of age-based representation deficits in the dozens of democracies in our sample. We also interact the age gap measures with a dummy variable for respondents under 35 to establish whether the pattern is more pronounced among younger people. For the WVS, we examine one measure of specific democratic support and two measures of diffuse democratic support. For the former, respondents rate how democratically their country is being governed. This question is not the same as the measure used in our previous two sections, but it should capture perceptions of how well democracy is functioning in one’s own country, which reflects our interest in specific support for democracy (Norris, 2011a). For diffuse support, the World Values Survey asks the same questions that we implemented in our original surveys. We use the question about how important it is for respondents to live in a democracy, and we create an index of general democratic support based on the four questions about whether democracies do not perform well economically, if democracies are indecisive and full of problems, if democracies are not effective at maintaining order, and if democracy is the best system of government.¹⁷

For the Barometer surveys, we use comparable indicators of democratic attitudes. In all survey waves, respondents are asked to evaluate how satisfied they are with the way democracy works in their country, which provides insights into specific democratic support. To capture diffuse support for democracy, we rely on a question that presents respondents with a strict choice: preferring democracy above all other types of regimes, or endorsing the view that authoritarian rule may sometimes be preferable. These items make it possible to examine whether cross-national variation in the age of political elites is associated with differences in both satisfaction with democracy and principled support for it as a system of government, extending the analysis beyond the WVS sample.

Our specifications include several country- and individual-level covariates that may correlate with support for democracy and age gaps. We control for the quality of governance with the Political Corruption Index from V-Dem and GDP per capita.¹⁸ We also control for inequality using the Gini Index,¹⁹ as well as the poverty headcount ratio capturing the percent of the population making less than \$6.85 per day.²⁰ Together, these variables should account for the most plausible alternative explanation, which is that countries governed by older political leaders are less effective at producing desirable outcomes and, therefore, produce lower levels of democratic satisfaction and support. Regarding individual characteristics, we control for gender, whether the respondent is young

¹⁷As in the previous sections, we create the index by averaging responses to the four questions. These questions are asked on different waves of the WVS, so our sample composition differs. Results for each of the four components of the index separately, as well as for satisfaction with democracy measured in a smaller subset of waves and countries, are reported in Table F.16 in Appendix F3

¹⁸We accessed the data on May 10th, 2024 at <https://data.worldbank.org/indicator/NY.GDP.PCAP.PP.CD>

¹⁹We accessed the data on May 10th, 2024 at <https://data.worldbank.org/indicator/SI.POV.GINI>

²⁰We accessed the data on May 10th, 2024 at <https://data.worldbank.org/indicator/SI.POV.UMIC>. Because these variables do not cover all years or countries, we impute information for the missing years using linear interpolation between the years for which we have data.

(15-34 years old), has not received substantial education, and is unemployed, which together should help to account for individual resources and grievances with the political system.²¹ We keep the individual controls consistent across the World Values Survey and Barometer analyses to ensure comparability between the two datasets.²²

Our main results for the World Values Survey are reported in Table 2. Panel A shows the relationship between legislator-public age differences and perceptions of how democratic one's country is, the importance respondents place on living in a democracy, and an index of general support for democracy. Panel B reports analogous models using the mean age of legislators as the independent variable.

Across both sets of models, the age difference and mean legislator age variables are negatively associated with the three outcomes, although the size and reliability of the coefficients vary. Larger age differences are more clearly linked to lower reported importance of democracy and lower scores on the democracy index, while the relationship with perceptions of the country's level of democracy is negative but not statistically significant. When using the mean age of legislators, the pattern is similar: the coefficients are negative across all outcomes, and statistically significant for perceptions of how democratic their country is being governed and the democracy index, though not for the importance of democracy.²³ To ease interpretation, in all models, the outcomes are standardized, where higher values reflect greater satisfaction with democracy, stronger support for democratic principles, and more positive assessments of democratic governance. Substantively, moving one standard deviation in the legislator-public age difference (roughly +6.2 years) or in mean legislator age (+3.5 years) is associated with a reduction in democratic support of approximately one-fifth to one-seventh of a standard deviation, depending on the model, equivalent to roughly 0.4 points on the ten-point scales used in the World Values Survey outcomes.²⁴ These results are consistent with the idea that older political elites are viewed as less representative and may undermine confidence in democratic institutions.

In columns 2, 4, and 6, we also examine whether these patterns are more pronounced among younger individuals by interacting the age difference and mean age variables with

²¹One potential concern is that politically sophisticated respondents may be simultaneously more attentive to age-based representation deficits and more critical of their democracy. Both the World Values Survey and the Barometer surveys include questions that allow us to proxy for political sophistication; specifically, a dummy for respondents reporting high political interest in the World Values Survey, and a dummy for respondents who frequently discuss politics with family and friends in the Barometer surveys. Robustness checks including these controls are reported in Appendices F8 and G5 and are substantively unchanged from the results presented here.

²²Measures of income, religious affiliation, and political ideology are not systematically available across all Barometer survey waves. We replicate the World Values Survey models with these additional controls in Appendix F4. The inclusion of ideology and income reduces the sample size by approximately 15 percent due to missing values. Results are substantively unchanged from those presented in the main text.

²³Results are also consistent when using an alternative construction of the democracy index, as shown in Table F.20 in Appendix F7.

²⁴see Table C.2 for descriptive statistics of all outcome variables

Table 2: Cross-National Differences in Legislator Ages and Support for Democracy: World Value Survey

Panel A: Age Differences

	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator-Public Age Difference	-0.009 (0.009)	-0.008 (0.009)	-0.018** (0.005)	-0.019*** (0.005)	-0.032** (0.008)	-0.029** (0.008)
Young	-0.039* (0.017)	0.026 (0.036)	-0.196*** (0.035)	-0.234*** (0.055)	-0.090** (0.030)	0.062 (0.070)
Young x Age Difference		-0.005* (0.002)		0.003 (0.004)		-0.011* (0.004)
Observations	92195	92195	93238	93238	25557	25557
Adjusted R^2	0.093	0.093	0.045	0.046	0.096	0.097
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Panel B: Mean Legislator Age

	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator Mean Age	-0.020* (0.009)	-0.018† (0.009)	-0.013 (0.008)	-0.008 (0.007)	-0.051** (0.014)	-0.043** (0.014)
Young	-0.041* (0.016)	0.282† (0.159)	-0.199*** (0.035)	0.333 (0.450)	-0.091** (0.030)	0.971* (0.398)
Young x Mean Age		-0.007* (0.003)		-0.011 (0.009)		-0.022* (0.008)
Observations	92195	92195	93238	93238	25557	25557
Adjusted R^2	0.095	0.095	0.044	0.044	0.097	0.097
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Standard Errors clustered by country. Controls include individual controls for gender, education, age, working status, income; and country controls like corruption index, gini index, % poverty and GDP-per capita.
Signif.: ***: 0.001, **: 0.01, *: 0.05, †: 0.1.

the dummy for respondents who are under the age of 35. While the interaction terms show some variation across outcomes, they generally indicate that young people respond more negatively to larger age gaps. This pattern is most pronounced for the democracy index and for perceptions of how democratic one's country is, where the marginal effects for younger respondents become more strongly negative as the age gap increases and legislators become older. Figures F.7 and F.8 in the Appendix illustrate these patterns graphically, showing the predicted levels of democratic support for young and older respondents across the full observed range of age gap values. For the democracy index in particular, young respondents start at comparable predicted levels to older respondents when age gaps are small, but their support declines more steeply as gaps grow, such that at larger age gap values young respondents fall below older ones. This pattern suggests

that the consequences of age gaps become particularly pronounced for younger citizens when gaps are large. However, it is also important to note that older respondents also tend to be more negative toward democracy where age gaps are larger, so these negative reactions are not limited to younger people only. These results are robust to alternative specifications, including models using age as a continuous variable and models with country fixed-effects alongside year fixed effects.²⁵

The analysis of the Barometer surveys, presented in Table 3, reveals a broadly similar set of patterns. In this case, we focus on satisfaction with democracy and support for democracy as the preferred form of government. As in the WVS, both the legislator-public age difference and the mean age of legislators are negatively associated with democratic attitudes. The relationship with democratic satisfaction is negative but insignificant for the age difference, and it is strongly negative and significant with mean legislator age. The relationship with principled democratic support is negative and significant for both age variables.²⁶ To contextualise these findings, descriptive statistics for the Barometer outcome variables are reported in Table C.2.²⁷ The Barometer results also indicate that the marginal effects of larger age differences and older politicians are more pronounced for younger people for the democratic satisfaction outcome. However, younger people react similarly to older people in these surveys when it comes to principled democratic support. Figures G.9 and G.10 in the Appendix complement these findings by showing that for satisfaction with democracy, young respondents begin with higher predicted levels than older respondents when age gaps are small, but their satisfaction declines more steeply as gaps grow, with the two groups converging and young respondents eventually falling below older ones at larger age gap values. For principled support for democracy, the two groups follow nearly parallel trajectories throughout the full range, consistent with the null interaction terms in the regression table.

Taken together, the results from both the World Values Survey and the Barometer data suggest that across countries, support for democracy will be lower where political leaders are older and diverge more from the ages of the public. In contrast to the previous results, this negative relationship appears for both specific and diffuse support for democracy. We consider the implications of this difference in the discussion section.

²⁵See Table F.19 in Appendix F6 for age as a continuous variable; and Table F.18 in Appendix F5 for country fixed-effects. We treat the country fixed-effects models as a robustness check rather than our preferred specification, because the argument in this section is fundamentally cross-national. Country fixed effects absorb the between-country variation that is central to that argument and identify coefficients only from within-country changes over time. In our setting, those within-country changes are limited because legislative age profiles evolve slowly, and because many countries contribute only a small number of survey waves over roughly twenty-five years, leaving the estimates to be identified from relatively little temporal variation.

²⁶These results are robust to using age as a continuous variable rather than a young/old dummy (Appendix G4) and to controlling for political sophistication (Appendix G5)

²⁷We also estimated models with country fixed effects alongside year fixed effects (Appendix G3). As discussed above for the World Values Survey models, we view these as a useful robustness check rather than our preferred specification, since they rely only on limited within-country variation over time.

Table 3: Cross-National Differences in Legislator Ages and Support for Democracy: Barometers

Panel A: Age Differences

	Satisfaction Democracy <i>Specific</i>		Support Democracy <i>Diffuse</i>	
	(1)	(2)	(3)	(4)
Legislator-Public Age Difference	-0.007 (0.007)	-0.005 (0.007)	-0.005* (0.002)	-0.005* (0.002)
Young	0.065*** (0.015)	0.118*** (0.025)	-0.045*** (0.007)	-0.057** (0.019)
Young x Age Difference		-0.005*** (0.002)		0.001 (0.001)
Observations	1156002	1156002	208000	208000
Adjusted R^2	0.099	0.100	0.070	0.070
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Panel B: Mean Legislator Age

	Satisfaction Democracy <i>Specific</i>		Support Democracy <i>Diffuse</i>	
	(1)	(2)	(3)	(4)
Legislator Mean Age	-0.029*** (0.008)	-0.026** (0.008)	-0.005* (0.002)	-0.005* (0.003)
Young	0.063*** (0.015)	0.395* (0.193)	-0.046*** (0.007)	-0.103† (0.056)
Young x Mean Age		-0.007† (0.004)		0.001 (0.001)
Observations	1156002	1156002	208000	208000
Adjusted R^2	0.105	0.105	0.070	0.070
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Standard Errors clustered by country. Controls include individual controls for gender, education, age, working status; and country controls like corruption index, gini index, % poverty and GDP-per capita.
Signif.: ***: 0.001, **: 0.01, *: 0.05, †: 0.1.

4.4 Age Gaps Undermine Confidence in Representation Less Than Gender Gaps

We have found evidence that age-based representation deficits can result in more negative views of democracy, though the relationship is relatively modest and appears more consistent across our analyses for satisfaction with democratic performance in one's own country. However, age gaps are only one type of representation deficit that can become entrenched in a democracy. Women, for instance, are chronically underrepresented in elected positions around the world (Erlich & Beauvais, 2022; Masad, 2020). Ethnic, religious, and other minority groups also face representation deficits in many democratic contexts. To some extent, it is inevitable that descriptive representation in important

government positions will not precisely reflect the population in a democracy, at least for some characteristics. How do age gaps compare to these other potential deficits in shaping concerns about democratic representation?

To answer this question, we also implemented a conjoint experiment on our second round of surveys in Italy, South Korea, and the United States. The experiment randomized descriptive representation across several demographic groups in pairs of hypothetical legislatures and asked respondents to choose which legislature they would prefer and how well each legislature would represent the public. Unlike the information experiment in Section 4.2, which addresses the causal effect of age gaps on democratic satisfaction, the conjoint is designed to assess the relative importance of age-based representation deficits compared to other types of descriptive representation deficits. Because conjoint experiments randomize attribute values across multiple dimensions, they are ideal for understanding which attributes people prioritize in making multidimensional choices (Hainmeuller et al., 2014). Since descriptive representation can occur across many different characteristics and will not align on all of them, the design is useful for identifying whether age is relatively more or less important than other salient components of identity.

Prior to beginning the experimental module, respondents were given information about the demographic composition of their country in terms of gender, ethnicity, educational attainment, sexual orientation, religious affiliation, and age. Details varied slightly across countries; for instance, sexual orientation was dropped from South Korea due to data limitations. Respondents were told that citizens typically want the national legislature to represent this diversity, but that some characteristics will be represented better than others. They were then presented with pairs of hypothetical legislatures that reflected the aforementioned demographic characteristics more or less closely. A key feature of the design is that the actual demographic composition of the country was always displayed alongside the two hypothetical legislatures, so that respondents could directly compare each option against the real population benchmark. For each of these attributes, we randomized whether the legislature underrepresented it, represented it correctly in terms of the national population, or overrepresented it. For instance, with regards to age, respondents were told the actual percentage of adults in the country who are 60 years or older, and they were given a standardized percentage of legislators in each hypothetical legislature that indicated whether the body under, over, or correctly represented older people. Respondents completed the exercise three times. The full conjoint design, including an example task, is shown in Figure H.13 in Section H in the Supplementary Materials.

The results are reported in Figure 2, where we show the effects on the ratings of how well each legislature would represent the public.²⁸ We report the Average Marginal

²⁸We use the rating outcome because it more directly reflects perceptions of representation. The forced choice preference outcome is shown in Figure H.16 in the Supplementary Materials H5 and leads to similar

Component Effect (AMCE), which is the effect of each attribute level on representation ratings, relative to the reference category and averaged across the other attributes.²⁹ In all cases, the reference category is the correct representation of the national population. The figure displays the results for each country, including all respondents. Figure H.15 in the Supplementary Materials shows the analysis considering younger respondents and older respondents (with 35 as the cut-point) separately.

In Italy and South Korea, overrepresentation of older people reduces perceptions that the legislatures would represent the public well. The effect sizes on the representation rating are -0.11 in Italy ($p < 0.05$) and -0.09 in South Korea ($p < 0.10$). These effects are consistent with our other analyses suggesting that age gaps favoring older people can reduce confidence in representation. In the United States, however, the overrepresentation of older people does not affect the representation ratings, a null effect that is similar to the lack of movement in the information experiment. Furthermore, in all three countries, the age effects are smaller than the effects of gender, and in some cases they are smaller than the effects of religion and education. As such, while our previous results indicate that age gaps can weaken satisfaction with democracy and perhaps support for democracy overall, other descriptive representation deficits may have stronger effects on how citizens perceive the quality of representation in their democracies.

Furthermore, as with our previous results, the negative effects are not driven primarily by the reactions of younger survey respondents. As shown by the second and third rows of the Figure H.15 in the Supplementary Materials, the effect sizes associated with overrepresentation of older people are similar in magnitude among respondents of both age groups. Thus, we do not find evidence that views of representational quality are particularly affected for younger people when they are the ones excluded from elected office.

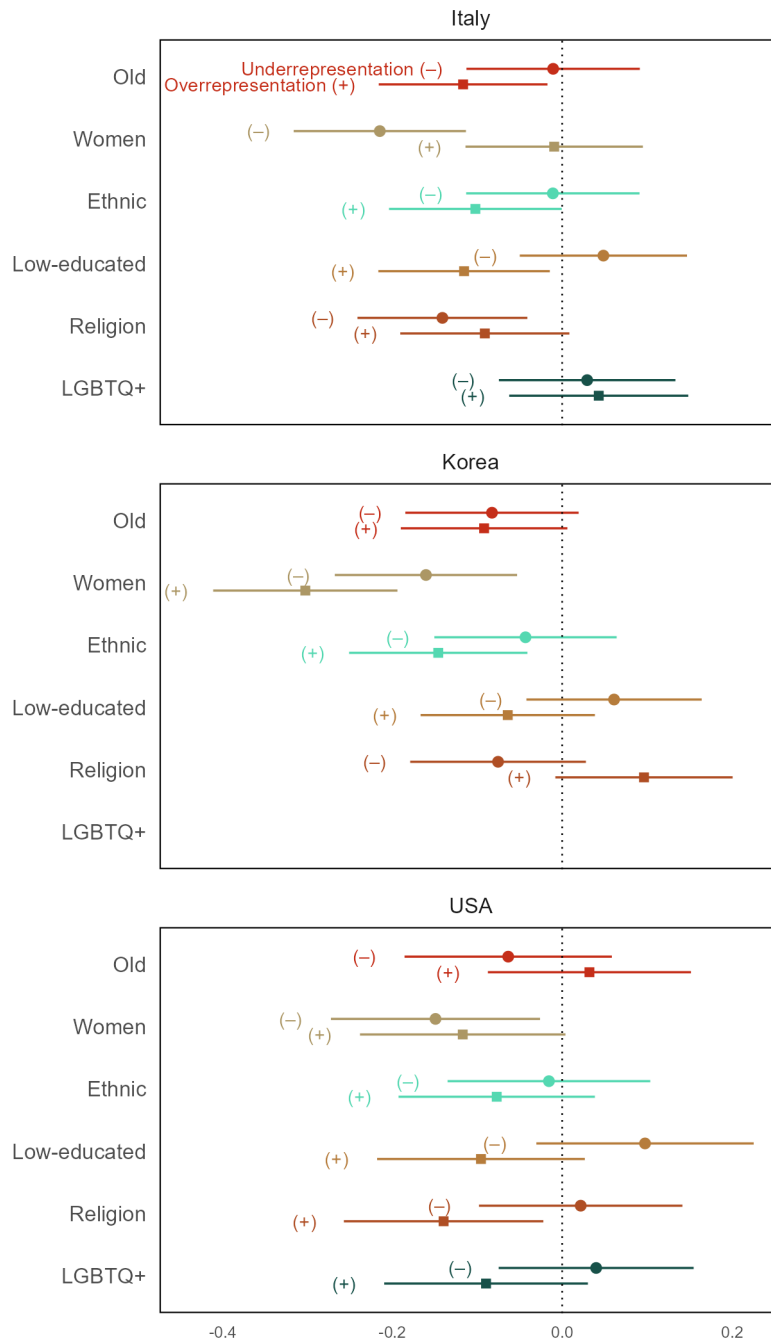
5 Discussion

Democracies often elect leaders who are older than the public, and these age gaps have become more salient recently as demographics have aged in a number of these countries (Berry, 2014; Stockemer & Sundström, 2023). Because age cohorts tend to view politics very differently, a number of scholars and social commentators have raised concerns that age gaps between political leaders and the public may increasingly undermine faith in democratic representation, sour attitudes toward democracy, and push younger people, in particular, toward distrust of democratic governance (Buchmeier & Vogt, 2024; Jackson et al., 2024; Stockemer & Sundström, 2023; Xiao, 2023). We find some evidence that there is merit to these concerns. Our results suggest that age gaps can weaken specific support

conclusions about the lower relative importance of age. In the forced choice, the effect of overrepresenting older people is negative for Italy but null in South Korea and the United States.

²⁹Marginal means are also reported in Section H6 in the Supplementary Materials.

Figure 2: Conjoint Results with rating outcome (AMCE)



Notes: AMCES estimates and 95% confidence interval. The reference group is always the correct representation. The outcome is a 10-point rating of how well the legislature would represent the public. Young is 18-34 years old.

for democracy, increasing frustrations with how democracy is performing in one's own country. Consistent with our expectations, we find weaker evidence that age gaps reduce diffuse support for democracy as a system of government. Our cross-national analysis shows that larger age gaps are associated with lower expressions of principled democratic support, but the observational and experimental analyses from our surveys only indicate a relationship between age gaps and satisfaction with democratic performance in the respondents' specific countries.

As mentioned, a possible explanation for these findings is that age gaps decrease satisfaction with democracy by reducing perceptions of representational effectiveness; however, because these age gaps are seen as contrary to democratic principles, frustrated citizens perceive the solution to be reforms that strengthen rather than abandon democracy. However, it is also possible that age gaps may undermine diffuse support for democracy over time, which could explain the association we find between larger age gaps and lower general approval of democracy in our cross-national analysis. Prior research suggests that diffuse support for democracy is a stickier attitude that changes more slowly than satisfaction with democracy (Claassen & Magalhães, 2022). As such, our experimental treatment in the context of a short survey may have affected perceptions of democratic performance in one's own country without shifting expressions of general support for democracy, even if the persistence of large, visible age gaps can gradually erode principled support for democracy within a country.

We also find relatively weak evidence that the negative effects of age gaps are especially relevant to younger people. Both older and younger respondents in our data tended to react negatively to age gaps. In some of the analyses, these negative reactions were more pronounced among younger people, but in others the reactions were similar in intensity. That both age groups tended to react negatively points toward a broader mechanism: rather than being driven primarily by self-interested reactions to personal underrepresentation, the negative responses to age gaps appear to reflect a more general value placed on descriptive representation as a democratic norm, where citizens evaluate age-based representation deficits as part of their broader assessment of how well democracy is functioning (Clayton et al., 2019; Verge, Wiesehomeier, & Espírito-Santo, 2020). Yet, our results still suggest that mitigating age gaps in political leadership could strengthen confidence in democratic representation among younger people, while also shaping democratic attitudes among older generations as well. In that sense, our study aligns with findings suggesting that even if political conflicts in ageing democracies are heavily shaped by this demographic change, the conflict will not necessarily be intergenerational in nature (Emery, 2012).

Thus, it does not appear to be the case that age gaps provide a strong explanation for why younger people have become relatively less committed to democracy in recent years (Frederiksen, 2024). Instead, other explanations for this divergence in democratic

support between age groups may relate to thermostatic models of democratic support, changing patterns of political socialization and education over time, or disapproval of rising inequality and other policy outcomes that have disproportionately affected younger citizens (Claassen, 2020b; Foa et al., 2020). The experience of the COVID-19 pandemic may also have contributed to this divergence. Research suggests that crises of this kind increase dissatisfaction with democracy, particularly where governments adopted measures that were perceived as economically damaging (Quaranta & Martini, 2017; Su, 2026). Younger generations faced particularly severe economic consequences from the pandemic, including job losses and disrupted education, and this may have reinforced perceptions among them that democratic governments were unresponsive to their needs and priorities, contributing to declining democratic support in this group independently of age-based representation gaps.

Our observational results showing that people are less satisfied with democracy when they believe politicians are too old were consistent across the three countries in which we conducted our original surveys. However, the treatment effects related to age gaps in the vignette and conjoint experiments were visible in Italy and South Korea but not in the United States. We had expected the effects to be strongest in the United States, which has the largest age gap of the three. As discussed, one explanation for the weaker effects in the United States could be that concerns about the ages of political leaders were already exceptionally salient in the American public debate at the time of our surveys. As we note in the introduction, the advanced ages of former President Biden, President Trump, and much of the Congressional leadership had prompted widespread discussion about whether the United States had become an unresponsive gerontocracy (Jackson et al., 2024; Moyn, 2024; Xiao, 2023). As a result, pre-treatment exposure may have been stronger in the United States than in Italy or South Korea, with many US citizens already having developed fixed attitudes toward the issue before participating in our survey, leaving little room for our treatment to move attitudes further (Druckman & Leeper, 2012; Sevi, 2022). Alternatively, political and social polarization around other identities may overshadow age effects in the United States. This possibility aligns with the conjoint results, where other identities appear to matter more than age for how US citizens evaluate the quality of representation.

More generally across the three countries, we find evidence that mismatches between the ages of political leaders and the public matter less than gender gaps for shaping perceptions of representative bodies. This pattern does not negate our other findings, which suggest that age gaps on their own can directly reduce satisfaction with democracy to some extent. However, it does suggest that policies designed to mitigate gender disparities, such as quotas for female legislators, may have relatively stronger effects in shaping evaluations of democratic representation (Bush & Zetterberg, 2021; Clayton et al., 2026). At the same time, it is also true that demographic deficits are often intersectional. Older

political leaders tend to be male, and they tend to be from other dominant groups in society (Stockemer & Sundström, 2018). As such, policies designed to encourage more representation of younger people in important political offices may also contribute to more robust satisfaction with democracy by helping to address these other demographic deficits as well.

Our results are mixed as to whether age gaps reduce principled support for democracy. However, the evidence that age gaps can undermine satisfaction with democracy suggests the issue has longer-term implications for democratic stability that can be addressed by empowering younger people. Specific support for the political system in a given country can be strengthened by better governance that improves outcomes, strengthens perceptions of procedural fairness, and mitigates underlying grievances (Christmann, 2018; Hobolt, 2012; Magalhães, 2016; Norris, 2011b). In turn, this higher satisfaction can increase regime stability as well as principled support for democracy (Claassen & Magalhães, 2022; Norris, 2011a).

Our findings should nevertheless be interpreted in light of several limitations. First, our original surveys were collected through online opt-in panels. Although recruitment was guided by quotas for age, gender, and education, respondents may still differ from national populations in ways that we cannot fully observe, and some caution is therefore warranted when assessing generalizability. This concern is partly mitigated by the fact that the core associations we document appear consistently across multiple demographic subgroups, and by the fact that our cross national analyses draw on large multi-wave survey datasets covering dozens of democracies. Even so, the experimental component of the study remains limited to three countries surveyed at a specific moment in time, which may constrain how far those experimental results can be generalized across settings or periods.

A second limitation concerns the range of democratic contexts captured by our experimental studies. The WVS and barometer analysis includes a global sample of countries, but our original data is collected in established and ageing democracies. If we focused on democracies with very young populations, it is possible the experimental results would be different, depending on how salient age is politically and how visible the age gap is. In such contexts, age-based underrepresentation may be interpreted through different political lenses, and generational grievances may interact more strongly with other forms of exclusion or dissatisfaction than our data allow us to observe. The fact that our results from the larger set of countries in the cross-national analysis differ in some aspects from the results in our original surveys could suggest that the consequences of age gaps will differ in other democratic contexts. An important avenue for future research is therefore to examine whether the relationship between age gaps and democratic attitudes works similarly in these younger democracies.

Taken together, our results suggest that democracies should pay attention to age

gaps and may benefit from policies that improve representation of younger generations in important public offices. Scholars have argued previously that deficits in age-based representation merit similar treatment to gender deficits, including specific policies to address them ([Trantidis, 2016](#)). Advocates for better representation of younger people have proposed policies including legislative quotas and age limits for certain public offices ([Bidadanure, 2015](#); [Schachtschneider, 2024](#)), and such policies can have significant public support ([Gramlich, 2023](#)). Strengthening confidence in democracy should be an important justification for giving further consideration to these policies that aim to limit misrepresentation of age groups.

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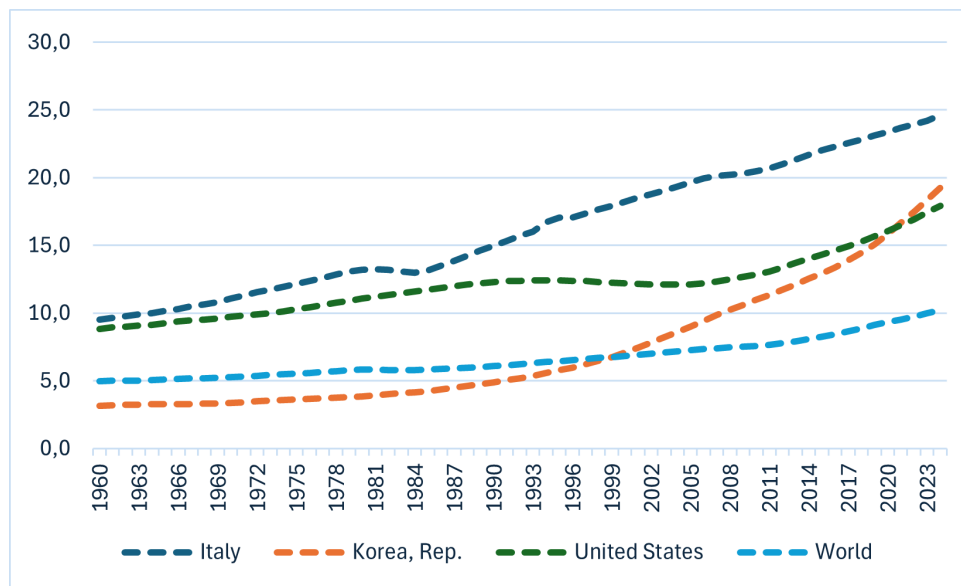
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Appendix

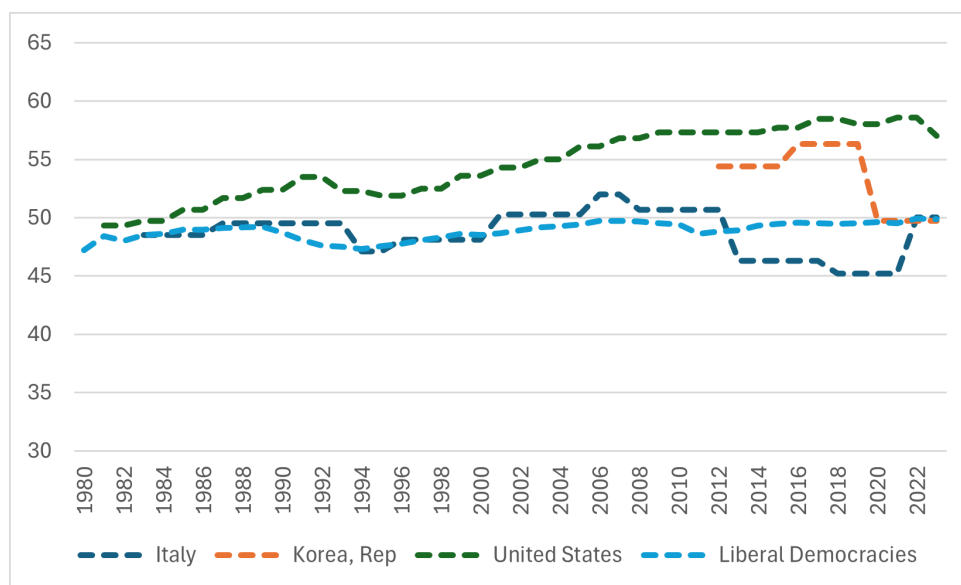
A Country Selection: Age Gaps Between Politicians and the People

Figure A.1: Population and Age of Legislators Evolution - Italy, United States and South Korea

Panel A: Population Ages 65 and above (% of total population)

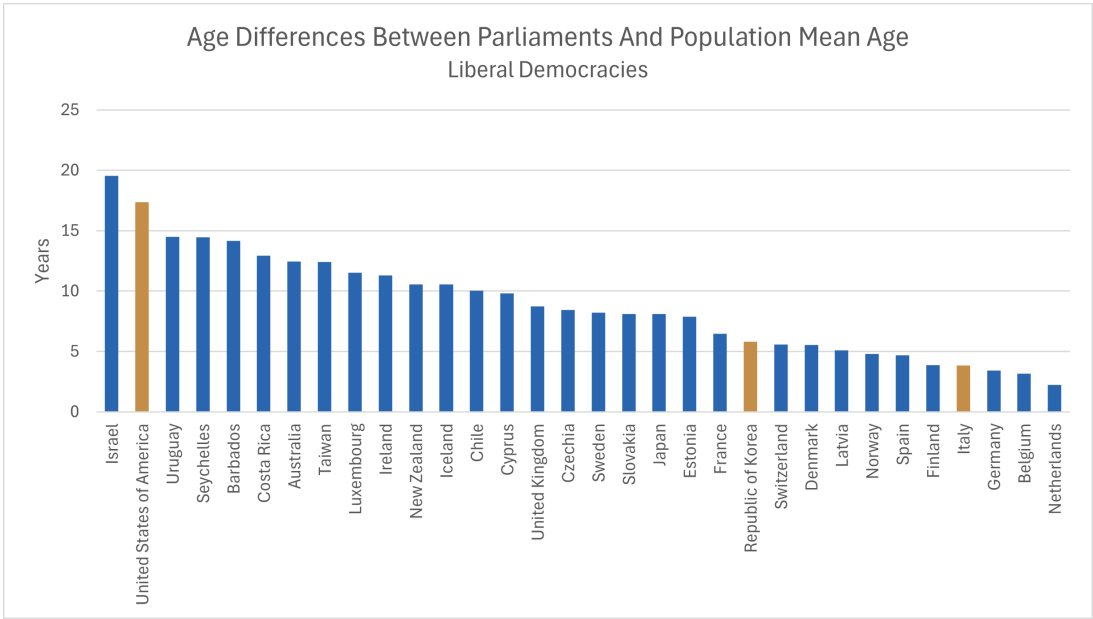


Panel B: Mean Age of Legislators



Source: Panel A: World Population Prospects, United Nations (UN), publisher: UN Population Division. Panel B: Author's calculations using WARP database and countries considered as Liberal Democracies according to the Varieties of Democracy dataset elaborated by [Coppedge et al. \(2023\)](#) and [Pemstein et al. \(2023\)](#)([United Nations & Social Affairs, 2024](#))

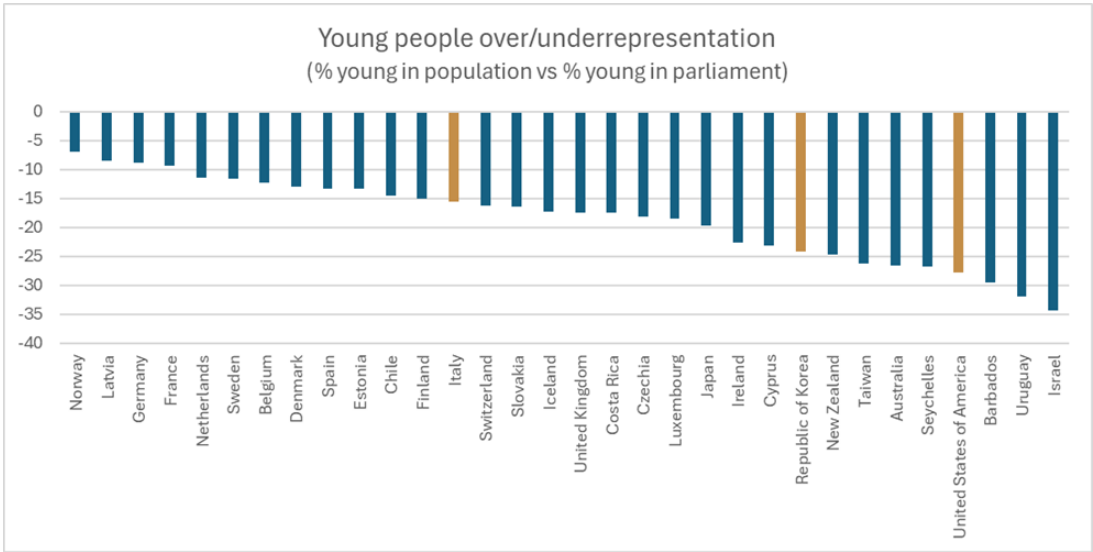
Figure A.2: Age Differences Between Parliaments And Population Mean Age



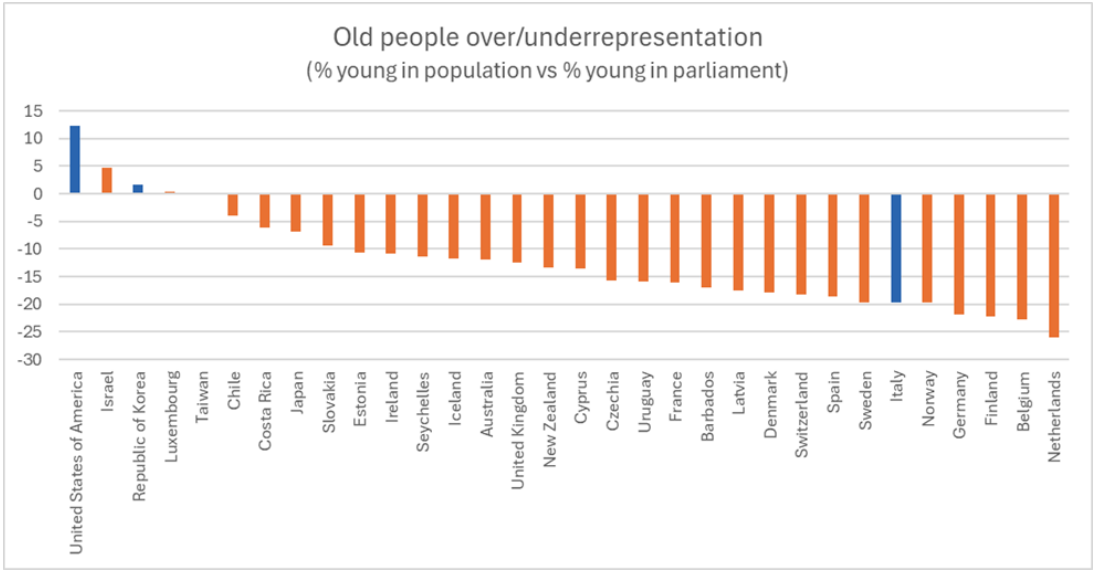
Source: Author’s calculations using WARP database and countries considered as Liberal Democracies according to the Varieties of Democracy dataset elaborated by [Coppedge et al. \(2023\)](#) and [Pemstein et al. \(2023\)](#)

Figure A.3: Over/underrepresentation of Age Groups

Panel A: Over/underrepresentation of Young People



Panel B: Over/underrepresentation of Old People



Source: Author's calculations using WARP database and countries considered as Liberal Democracies according to the Varieties of Democracy dataset elaborated by [Coppedge et al. \(2023\)](#) and [Pemstein et al. \(2023\)](#). Young people is 35 years old or younger. Old people is 60 years old or older.

B Original Surveys Questions and Descriptives

B1 Original Survey Descriptives

Table B.1: Outcome Descriptives for the 2 Rounds of Original Surveys

Panel A: Descriptives Round 1 (Section 4.1)

	Sample	Dem. Satisfaction		Dem. Important		Dem. Index		Politicians Too Old	
	N	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Italy	2059	5.878	2.420	8.737	1.858	3.452	0.896	0.471	0.499
Korea	1846	6.016	2.216	8.433	1.772	3.491	0.822	0.605	0.489
USA	1798	6.525	2.540	8.285	2.357	3.376	0.994	0.526	0.499
Total	5703	6.127	2.411	8.496	2.013	3.441	0.907	0.532	0.499

Panel B: Descriptives Round 2 (Section 4.2)

	Sample	Dem. Gives		Dem. Effective		Dem. Satisfaction		Dem. Important		Dem. Index	
	N	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Italy	2197	2.387	0.790	3.399	1.160	5.544	2.424	8.510	1.969	3.516	0.957
Korea	1856	2.633	0.828	3.687	0.947	5.962	2.207	7.867	1.818	3.322	0.796
USA	1786	2.710	0.889	3.477	1.188	6.298	2.624	8.231	2.356	3.452	0.968
Total	5839	2.564	0.845	3.514	1.112	5.908	2.442	8.220	2.068	3.435	0.916

Notes: Author's calculations based on the Original Surveys fielded in Italy, South Korea and the United States

B2 Original Survey Questions

This subsection reports the survey items used to measure support for democracy in the United States, Italy, and South Korea. These questions were included in the first wave of our original survey data and correspond to the dependent variables analyzed in Section 4.1.

1. **Politicians too old:** Do you think that politicians in your country are generally...

- Too old
- About the right age
- Too young

2. **Importance of Democracy:** How important is it to you that your country is governed by a democracy?

Scale: 1 = Not at all important; 10 = Very important

1 2 3 4 5 6 7 8 9 10

3. **Satisfaction with Democracy:** On the whole, how satisfied are you with how democracy works in your country?

Scale: 1 = Not at all satisfied; 10 = Fully satisfied

1 2 3 4 5 6 7 8 9 10

4. **General Attitudes Toward Democracy:** Please indicate how much you agree or disagree with the following statements.

	Strongly agree	Somewhat agree	Neither agree nor disagree	Somewhat disagree	Strongly disagree
Democratic systems are not effective at maintaining order and stability.	(1)	(2)	(3)	(4)	(5)
Democratic governments are indecisive and full of problems.	(1)	(2)	(3)	(4)	(5)
Under a democratic system, the country's economic performance is weak.	(1)	(2)	(3)	(4)	(5)
Democracy is better than any other system of government.	(1)	(2)	(3)	(4)	(5)

C General Surveys Descriptives: World Value Survey and Barometers

Table C.2: Outcome Descriptives for the General Surveys

	N	Mean	SD	Min	Max
<i>World Values Survey</i>					
How Democratic	238864	6.163	2.551	1.000	10.000
Dem. Important	238864	8.391	2.078	1.000	10.000
Dem. Index	238864	2.791	0.604	1.000	4.000
<i>Barometers</i>					
Dem. Satisfaction	1926550	2.476	0.873	1.000	4.000
Support for Democracy	1926550	0.668	0.471	0.000	1.000

Notes: Author's calculations based on the World Value Survey data, and Latinbarometer, Eurobarometer, Afrobarometer, and Asianbarometer.

D Additional Analyses: Belief that Politicians Are Too Old and Support for Democracy

D1 Belief that Politicians Are Too Old and Support for Democracy: Country-specific Results

Table D.3: Belief that Politicians Are Too Old and Support for Democracy. Country-specific results.

	Dem. Satisfaction <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Italy						
Politicians Too Old	-0.297*** (0.043)	-0.315*** (0.049)	0.071 (0.044)	-0.031 (0.048)	-0.099** (0.042)	-0.148*** (0.048)
Young	-0.058 (0.050)	-0.099 (0.069)	-0.432*** (0.054)	-0.662*** (0.072)	-0.464*** (0.045)	-0.576*** (0.058)
Politicians Too Old × Young		0.081 (0.100)		0.455*** (0.105)		0.223** (0.089)
Individual Controls	✓	✓	✓	✓	✓	✓
R ²	0.10	0.10	0.07	0.08	0.16	0.17
Observations	2,052	2,052	2,055	2,055	2,050	2,050
Panel B: Korea						
Politicians Too Old	-0.226*** (0.046)	-0.205*** (0.056)	0.191*** (0.047)	0.206*** (0.053)	0.176*** (0.045)	0.203*** (0.056)
Young	0.056 (0.046)	0.093 (0.063)	-0.399*** (0.050)	-0.372*** (0.075)	-0.362*** (0.045)	-0.315*** (0.065)
Politicians Too Old × Young		-0.065 (0.090)		-0.046 (0.099)		-0.083 (0.087)
Individual Controls	✓	✓	✓	✓	✓	✓
R ²	0.15	0.15	0.16	0.16	0.23	0.23
Observations	1,842	1,842	1,842	1,842	1,841	1,841
Panel C: United States						
Politicians Too Old	-0.403*** (0.046)	-0.389*** (0.053)	0.043 (0.046)	0.060 (0.050)	-0.040 (0.043)	-0.059 (0.050)
Young	0.020 (0.056)	0.052 (0.067)	-0.440*** (0.060)	-0.402*** (0.077)	-0.425*** (0.051)	-0.468*** (0.066)
Politicians Too Old × Young		-0.062 (0.102)		-0.076 (0.109)		0.084 (0.090)
Individual Controls	✓	✓	✓	✓	✓	✓
R ²	0.10	0.10	0.14	0.14	0.21	0.22
Observations	1,791	1,791	1,794	1,794	1,790	1,790

Notes: OLS estimates and robust SE in parenthesis. Outcome variables are standardized. The results include country fixed-effects, and control variables, such as gender, education, age, working status, religion, income, ideology, anxiety about ageing and ethnocentrism. Signif.: ***: 0.01, **: 0.05, *: 0.1.

D2 Belief that Politicians Are Too Old and Support for Democracy: Results without individual-level controls

Table D.4: Belief that Politicians Are Too Old and Support for Democracy: Results without individual-level controls.

	Dem. Satisfaction <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Politicians Too Old	-0.391*** (0.026)	-0.380*** (0.030)	0.135*** (0.027)	0.090*** (0.029)	0.130*** (0.027)	0.066** (0.032)
Young		0.019 (0.040)		-0.558*** (0.045)		-0.631*** (0.038)
Politicians Too Old $\times$ Young		-0.040 (0.058)		0.137** (0.062)		0.205*** (0.054)
R ²	0.05	0.05	0.01	0.06	0.01	0.06
Observations	5,710	5,710	5,714	5,714	5,701	5,701
Country FE	✓	✓	✓	✓	✓	✓

Notes: OLS estimates. Robust SE in parenthesis. No controls included. Outcome variables are standardized. Signif.: ***: 0.01, **: 0.05, *: 0.1.

D3 Belief that Politicians Are Too Old and Support for Democracy: Results with Age as Continuous Variable

Table D.5: Belief that Politicians Are Too Old and Support for Democracy. With Age as Continuous Variable

	Dem. Satisfaction <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Politicians Too Old	-0.329*** (0.026)	-0.366*** (0.080)	0.114*** (0.026)	0.260*** (0.084)	0.020 (0.024)	0.267*** (0.072)
Age	0.001 (0.001)	0.001 (0.001)	0.017*** (0.001)	0.019*** (0.001)	0.018*** (0.001)	0.021*** (0.001)
Politicians Too Old $\times$ Age		0.001 (0.002)		-0.003* (0.002)		-0.005*** (0.002)
R ²	0.11	0.11	0.14	0.14	0.22	0.22
Observations	5,680	5,680	5,686	5,686	5,676	5,676
Individual Controls	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓

Notes: OLS estimates and robust SE in parenthesis. Outcome variables are standardized. The results include country fixed-effects, and control variables, such as gender, education, age, working status, religion, income, ideology, anxiety about ageing and ethnocentrism. Signif.: ***: 0.01, **: 0.05, *: 0.1.

D4 Belief that Politicians Are Too Old and Support for Democracy: Results For Alternative Democracy Index

The following table shows the results when we take out the variable "Democracy is always better" from the Democracy Index reported in Table 1

Table D.6: Belief that Politicians Are Too Old and Support for Democracy. For Alternative Democracy Index

	Dem. Index (simple) <i>Diffuse</i>	
	(1)	(2)
Politicians Too Old	0.019 (0.025)	-0.006 (0.029)
Young	-0.413*** (0.027)	-0.465*** (0.038)
Politicians Too Old $\times$ Young		0.097* (0.052)
R ²	0.20	0.20
Observations	5,681	5,681
Individual Controls	✓	✓
Country FE	✓	✓

Notes: OLS estimates and robust SE in parenthesis. Outcome variables are standardized. The results include country fixed-effects, and control variables, such as gender, education, age, working status, religion, income, ideology, anxiety about ageing and ethnocentrism. Signif.: ***: 0.01, **: 0.05, *: 0.1.

E Age Gap Information Experiment

E1 Experiment Set-up

Figure E.4: Treatment group vignette used in the US Survey

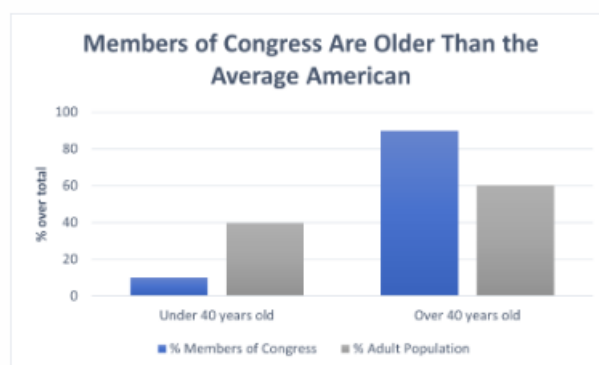
Politicians Are Older Than Their Constituents

Political leaders in the United States tend to be older than the average citizen. The average age of a US cabinet secretary is 61 years old, and the average age of a member of Congress is 57 years old. In comparison, the average age of an adult US citizen is 39 years old.

As you can see in the graph, this age gap between politicians and the public is more pronounced for certain age brackets.

For example, 40 percent of the general adult population is under the age of 40, but only 10 percent of legislators in Congress are under the age of 40.

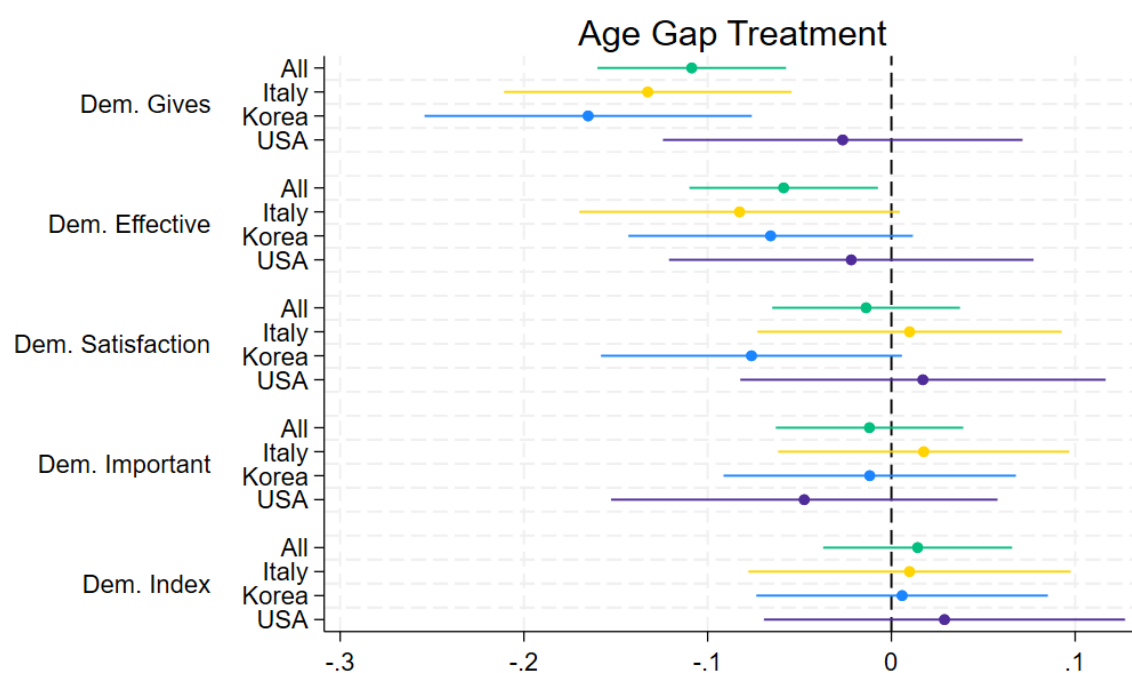
Meanwhile, 60 percent of the general adult population is older than 40, whereas 90 percent of legislators are older than 40.



Notes: Screenshot of the Qualtrics' US survey, December 2023 .

E2 Age-gap Information Experiment: Regression results without controls

Figure E.5: Main Effects of Age Gap Treatment on Democracy Outcomes



Notes: OLS estimates and 95% confidence intervals of the average treatment effects of the information experiment. Outcome variables are standardized. No controls.

E3 Age-gap Information Experiment: Regression Tables

The tables on the following pages report results for the age-gap information experiment.

Table E.7: Age-gap Information treatment effect on Democracy appreciation

	Dem. Gives		Dem. Effective		Dem. Satisfaction		Dem. Important		Dem. Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age gap Treatment	-0.109*** (0.026)	-0.103*** (0.025)	-0.059** (0.026)	-0.058** (0.026)	-0.014 (0.026)	-0.015 (0.025)	-0.012 (0.026)	-0.016 (0.026)	0.014 (0.026)	0.007 (0.025)
Observations	5836	5615	5837	5616	5831	5610	5833	5612	5839	5618
Adjusted R^2	0.003	0.143	0.001	0.069	-0.000	0.149	-0.000	0.061	-0.000	0.138
Individual Controls		Yes		Yes		Yes		Yes		Yes
Country Controls		Yes		Yes		Yes		Yes		Yes

Note: OLS estimates and standard errors in parentheses. The results with individual controls include gender, education, age, working status, religion, income, region, and ideology. Country controls are

country fixed-effects. Pooled results for the 3 countries: Italy, South Korea, and the United States.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table E.8: Age-gap Information treatment effect on Democracy appreciation: USA

	Dem. Gives		Dem. Effective		Dem. Satisfaction		Dem. Important		Dem. Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age gap Treatment	-0.026 (0.050)	-0.009 (0.048)	-0.022 (0.051)	-0.011 (0.050)	0.017 (0.051)	0.024 (0.050)	-0.047 (0.054)	-0.039 (0.054)	0.029 (0.050)	0.021 (0.047)
Observations	1784	1736	1784	1736	1783	1735	1783	1735	1786	1738
Adjusted R^2	-0.000	0.131	-0.000	0.077	-0.000	0.093	-0.000	0.080	-0.000	0.180
Individual Controls		Yes		Yes		Yes		Yes		Yes
Country Controls		Yes		Yes		Yes		Yes		Yes

Note: OLS estimates and standard errors in parentheses. The results with individual controls include gender, education, age, working status, religion, income, region, and ideology. Country controls are country fixed-effects.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table E.9: Age-gap Information treatment effect on Democracy appreciation: Korea

	Dem. Gives		Dem. Effective		Dem. Satisfaction		Dem. Important		Dem. Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age gap Treatment	-0.165*** (0.045)	-0.169*** (0.041)	-0.066* (0.039)	-0.073* (0.039)	-0.076* (0.042)	-0.078** (0.037)	-0.012 (0.041)	-0.029 (0.040)	0.006 (0.040)	-0.013 (0.039)
Observations	1856	1800	1856	1800	1854	1798	1856	1800	1856	1800
Adjusted R^2	0.007	0.238	0.001	0.074	0.001	0.266	-0.000	0.062	-0.001	0.113
Individual Controls		Yes		Yes		Yes		Yes		Yes
Country Controls		Yes		Yes		Yes		Yes		Yes

Note: OLS estimates and standard errors in parentheses. The results with individual controls include gender, education, age, working status, religion, income, region, and ideology. Country controls are country fixed-effects.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

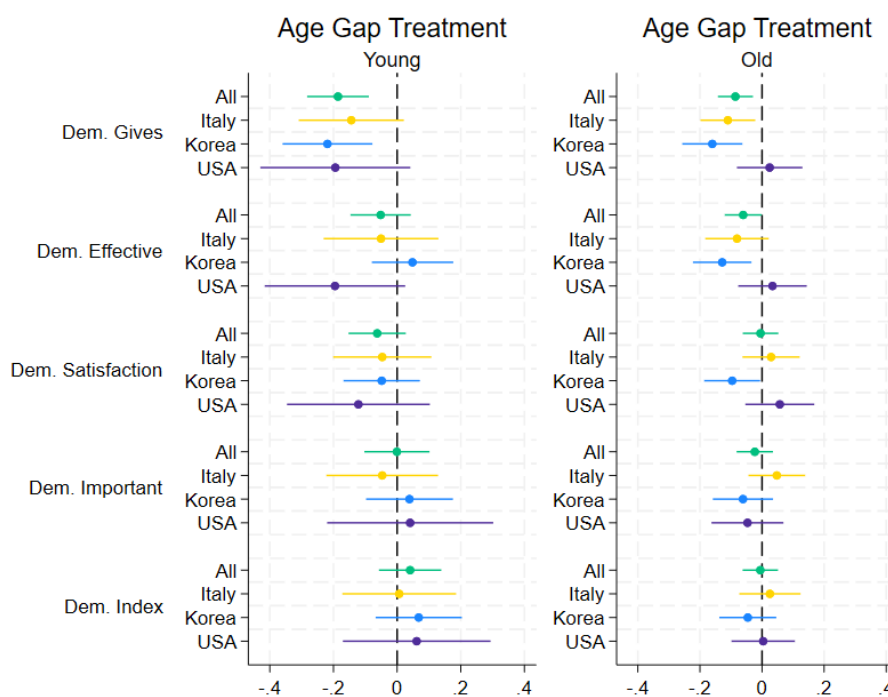
Table E.10: Age-gap Information treatment effect on Democracy appreciation: Italy

	Dem. Gives		Dem. Effective		Dem. Satisfaction		Dem. Important		Dem. Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age gap Treatment	-0.133*** (0.040)	-0.116*** (0.039)	-0.083* (0.044)	-0.080* (0.045)	0.010 (0.042)	0.012 (0.040)	0.018 (0.040)	0.023 (0.041)	0.010 (0.045)	0.017 (0.044)
Observations	2196	2079	2197	2080	2194	2077	2194	2077	2197	2080
Adjusted R^2	0.005	0.088	0.001	0.042	-0.000	0.139	-0.000	0.044	-0.000	0.115
Individual Controls		Yes		Yes		Yes		Yes		Yes
Country Controls		Yes		Yes		Yes		Yes		Yes

Note: OLS estimates and standard errors in parentheses. The results with individual controls include gender, education, age, working status, religion, income, region, and ideology. Country controls are country fixed-effects.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

E4 Age Gap Information Experiment: Young vs Old

Figure E.6: Age-gap vignette treatment effect for our model with covariates: Young vs Old

Notes: OLS estimates and 95% confidence intervals of the average treatment effects of the information experiment. Outcome variables are standardized. The results include region and country fixed-effects, and control variables, such as gender, education, age, working status, religion, income, and ideology

Table E.11: Democracy Value and Age gap Vignette (Young vs. Old)

	Dem. Gives		Dem. Effective		Dem. Satisfaction		Dem. Important		Dem. Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age gap Treatment	-0.186*** (0.049)	-0.086*** (0.029)	-0.052 (0.048)	-0.061** (0.030)	-0.062 (0.046)	-0.005 (0.029)	-0.001 (0.052)	-0.023 (0.030)	0.041 (0.050)	-0.006 (0.029)
Observations	1352	4263	1353	4263	1351	4259	1351	4261	1354	4264
Adjusted R^2	0.221	0.116	0.155	0.051	0.249	0.124	0.051	0.052	0.107	0.129
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subgroup	Young	Old	Young	Old	Young	Old	Young	Old	Young	Old

Note: OLS estimates and standard errors in parentheses. The results with individual controls include gender, education, age, working status, religion, income, region, and ideology. Country controls are country fixed-effects. Pooled results for the 3 countries: Italy, South Korea, and the United States.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table E.12: Democracy Value and Age gap Vignette USA (Young vs. Old)

	Dem. Gives		Dem. Effective		Dem. Satisfaction		Dem. Important		Dem. Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age gap Treatment	-0.194 (0.120)	0.025 (0.054)	-0.195* (0.112)	0.034 (0.056)	-0.122 (0.114)	0.057 (0.057)	0.041 (0.133)	-0.047 (0.059)	0.062 (0.118)	0.004 (0.052)
Observations	319	1417	319	1417	319	1416	318	1417	320	1418
Adjusted R^2	0.163	0.124	0.214	0.059	0.179	0.075	0.045	0.070	0.049	0.188
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subgroup	Young	Old	Young	Old	Young	Old	Young	Old	Young	Old

Note: OLS estimates and standard errors in parentheses. The results with individual controls include gender, education, age, working status, religion, income, region, and ideology. Country controls are country fixed-effects.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table E.13: Democracy Value and Age gap Vignette Korea (Young vs. Old)

	Dem. Gives		Dem. Effective		Dem. Satisfaction		Dem. Important		Dem. Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age gap Treatment	-0.219*** (0.072)	-0.160*** (0.049)	0.049 (0.065)	-0.128*** (0.048)	-0.048 (0.061)	-0.096** (0.046)	0.039 (0.069)	-0.061 (0.049)	0.068 (0.069)	-0.046 (0.047)
Observations	559	1241	559	1241	558	1240	559	1241	559	1241
Adjusted R^2	0.231	0.209	0.119	0.053	0.341	0.220	0.132	0.029	0.076	0.089
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subgroup	Young	Old	Young	Old	Young	Old	Young	Old	Young	Old

Note: OLS estimates and standard errors in parentheses. The results with individual controls include gender, education, age, working status, religion, income, region, and ideology. Country controls are country fixed-effects.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table E.14: Democracy Value and Age gap Vignette Italy (Young vs. Old)

	Dem. Gives		Dem. Effective		Dem. Satisfaction		Dem. Important		Dem. Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age gap Treatment	-0.144*	-0.110**	-0.051	-0.081	-0.047	0.029	-0.047	0.048	0.007	0.026
	(0.084)	(0.045)	(0.092)	(0.052)	(0.079)	(0.047)	(0.089)	(0.047)	(0.091)	(0.050)
Observations	474	1605	475	1605	474	1603	474	1603	475	1605
Adjusted R^2	0.115	0.081	0.063	0.038	0.185	0.135	0.003	0.046	0.118	0.108
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Subgroup	Young	Old	Young	Old	Young	Old	Young	Old	Young	Old

Note: OLS estimates and standard errors in parentheses. The results with individual controls include gender, education, age, working status, religion, income, region, and ideology. Country controls are country fixed-effects.

*** p<0.01, ** p<0.05, * p<0.10.

F World Values Survey

F1 World Values Survey Countries and Years

The following table shows the number of respondents per Country and Survey Wave of the World Values Survey (WVS).

Table F.15: World Values Survey Number of Respondents by Country and Survey Wave

Country	Chronology of EVS-WVS waves					Total No.
	1994-1998	1999-2004	2005-2009	2010-2014	2017-2022	
	No.	No.	No.	No.	No.	
Andorra	0	0	981	0	998	1,979
Argentina	1,052	1,240	963	1,010	987	5,252
Armenia	0	0	0	0	1,161	1,161
Australia	1,992	0	1,390	1,441	1,764	6,587
Bangladesh	1,441	0	0	0	0	1,441
Bolivia	0	0	0	0	1,998	1,998
Bosnia and Herzegovina	1,149	1,127	0	0	0	2,276
Brazil	1,124	0	1,451	1,415	1,613	5,603
Bulgaria	990	0	957	0	0	1,947
Burkina Faso	0	0	1,323	0	0	1,323
Canada	0	1,811	2,070	0	4,018	7,899
Chile	982	1,158	963	979	950	5,032
Colombia	0	0	2,830	1,472	1,520	5,822
Cyprus	0	0	1,047	989	967	3,003
Czechia	1,102	0	0	0	1,192	2,294
Dominican Republic	409	0	0	0	0	409
Ecuador	0	0	0	1,193	1,183	2,376
El Salvador	1,128	0	0	0	0	1,128
Estonia	950	0	0	1,478	0	2,428
Finland	940	0	997	0	0	1,937
France	0	0	985	0	0	985
Georgia	0	0	0	1,145	0	1,145
German Federal Republic	1,998	0	0	0	0	1,998
Germany	0	0	2,031	2,013	1,502	5,546

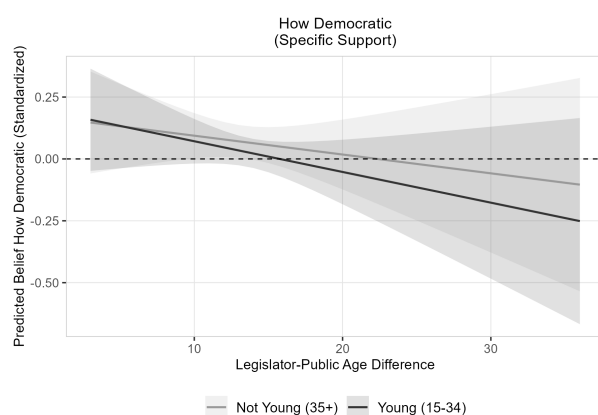
Country	1994-1998 No.	1999-2004 No.	2005-2009 No.	2010-2014 No.	2017-2022 No.	Total No.
Ghana	0	0	1,518	1,552	0	3,070
Great Britain	0	0	990	0	2,505	3,495
Greece	0	0	0	0	1,180	1,180
Guatemala	0	0	0	0	1,201	1,201
Hungary	600	0	971	0	0	1,571
India	1,549	1,489	1,483	4,033	0	8,554
Indonesia	0	965	1,834	0	3,115	5,914
Italy	0	0	973	0	0	973
Japan	856	1,147	998	2,065	1,193	6,259
Latvia	1,167	0	0	0	0	1,167
Lithuania	917	0	0	0	0	917
Macau SAR	0	0	0	0	1,015	1,015
Macedonia	908	0	0	0	0	908
Maldives	0	0	0	0	1,029	1,029
Mali	0	0	1,392	0	0	1,392
Mexico	1,357	1,290	1,490	1,973	1,722	7,832
Moldova	939	895	0	0	0	1,834
Mongolia	0	0	0	0	1,638	1,638
Montenegro	226	0	0	0	0	226
Netherlands	0	0	1,014	1,739	1,868	4,621
New Zealand	1,047	0	0	809	1,007	2,863
Nigeria	0	0	0	1,759	1,228	2,987
Northern Ireland	0	0	0	0	436	436
Norway	1,121	0	1,012	0	0	2,133
Peru	0	1,455	1,430	1,176	1,368	5,429
Philippines	1,193	1,195	0	1,200	0	3,588
Poland	1,025	0	917	911	0	2,853
Puerto Rico	1,146	710	0	0	1,112	2,968
Romania	1,165	0	1,590	1,416	1,144	5,315
Serbia	0	1,100	1,172	0	0	2,272
Slovakia	1,048	0	0	0	1,190	2,238
Slovenia	958	0	950	1,035	0	2,943
South Africa	2,636	2,781	2,901	3,453	0	11,771
South Korea	1,244	1,112	1,200	1,197	1,245	5,998
Spain	1,150	1,157	1,180	1,156	0	4,643
Sweden	996	1,004	2,040	1,183	0	5,223
Switzerland	1,151	0	1,216	0	0	2,367
Taiwan ROC	734	0	1,217	1,173	1,223	4,347
Trinidad and Tobago	0	0	983	912	0	1,895
Tunisia	0	0	0	1,103	1,180	2,283
Turkey	1,778	3,183	1,282	1,574	0	7,817
Ukraine	2,311	0	937	0	1,209	4,457
United States of America	1,490	1,181	1,193	2,153	2,541	8,558
Uruguay	967	0	972	963	962	3,864
Venezuela	1,186	1,190	0	0	0	2,376
Zambia	0	0	1,451	0	0	1,451
Total	48,122	27,190	54,294	47,670	52,164	229,440

Notes: World Value Survey survey waves by [Inglehart et al. \(2022\)](https://www.worldvaluessurvey.org/wvs.jsp) <https://www.worldvaluessurvey.org/wvs.jsp>

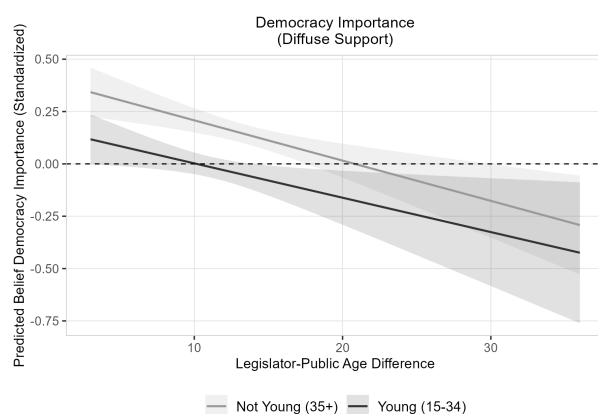
F2 World Values Survey Results: Interaction figures

The following figures display the predicted values of democratic support for young respondents (aged 15–34) and older respondents (aged 35 and above), across the observed range of the age gap variables, based on the interaction models reported in Table 2. These figures complement the regression results by illustrating how the predicted levels of democratic support for each age group evolve across the full distribution of age gap values.

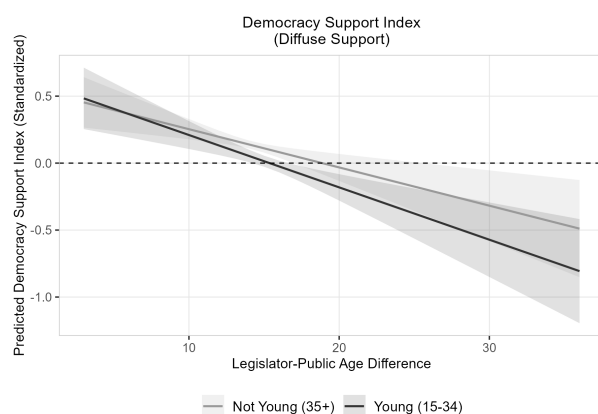
Figure F.7: World Values Survey: Predicted Values by Age Group across the Range of Legislator-Public Age Differences



Notes: Predicted values from OLS regressions with 95% confidence intervals.
All other covariates held at their means.



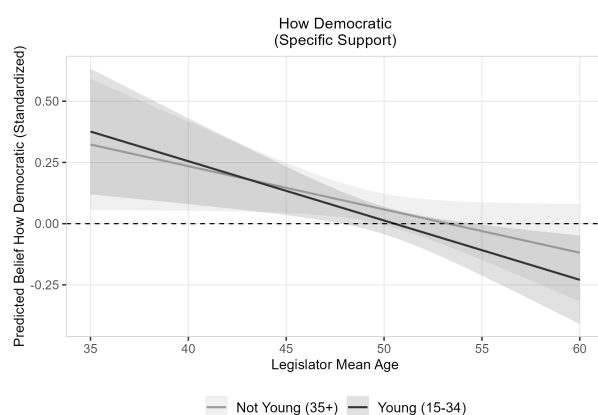
Notes: Predicted values from OLS regressions with 95% confidence intervals.
All other covariates held at their means.



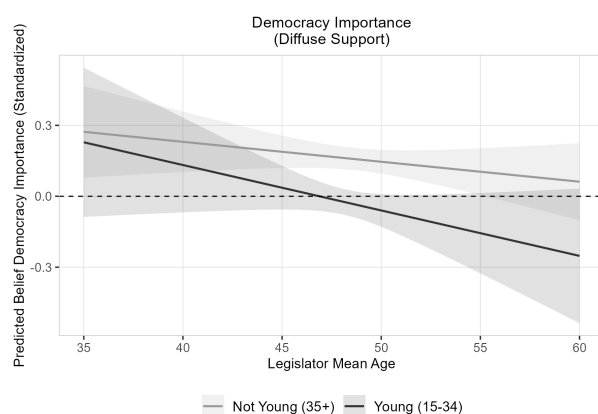
Notes: Predicted values from OLS regressions with 95% confidence intervals.
All other covariates held at their means.

Notes: Predicted values from OLS regressions with 95% confidence intervals, estimated separately for young respondents (aged 15–34) and older respondents (aged 35 and above) across the observed range of the Legislator-Public Age Difference variable. All other covariates are held at their means. Models include year fixed effects and all controls reported in Table 2.

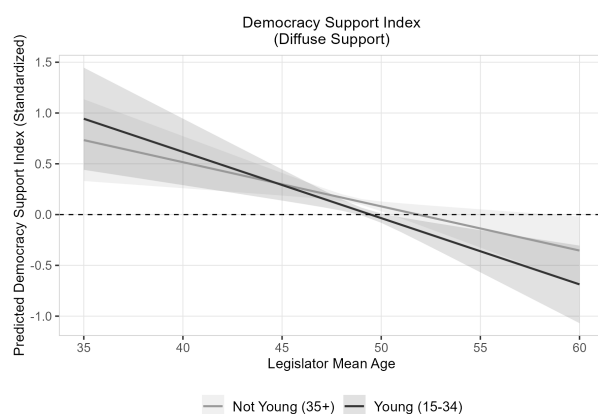
Figure F.8: World Values Survey: Predicted Values by Age Group across the Range of Mean Legislator Age



Notes: Predicted values from OLS regressions with 95% confidence intervals.
All other covariates held at their means.



Notes: Predicted values from OLS regressions with 95% confidence intervals.
All other covariates held at their means.



Notes: Predicted values from OLS regressions with 95% confidence intervals.
All other covariates held at their means.

Notes: Predicted values from OLS regressions with 95% confidence intervals, estimated separately for young respondents (aged 15–34) and older respondents (aged 35 and above) across the observed range of the Mean Legislator Age variable. All other covariates are held at their means. Models include year fixed effects and all controls reported in Table 2.

F3 World Values Survey: Results for other outcomes

The following table shows the results considering all other democratic outcomes in the World Value Survey.

Table F.16: Cross-National Differences in Legislator Ages and Support for Democracy: Other outcomes with World Value Survey

Panel A: Age Differences										
	Satisfaction Democracy <i>Specific</i>		Dem. Economic <i>Diffuse</i>		Dem. Indecisive <i>Diffuse</i>		Dem. Order <i>Diffuse</i>		Dem. Better <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Legislator-Public Age Difference	-0.023*** (0.001)	-0.020*** (0.001)	-0.020*** (0.004)	-0.018*** (0.004)	-0.013 (0.013)	-0.011 (0.013)	-0.023** (0.006)	-0.019* (0.006)	-0.037* (0.014)	-0.035* (0.014)
Young	0.000 (0.021)	0.087 (0.062)	-0.071** (0.021)	0.029 (0.054)	-0.038 (0.027)	0.084 (0.075)	-0.003 (0.027)	0.181*** (0.042)	-0.167*** (0.033)	-0.094 (0.063)
Young x Age Difference		-0.007 (0.004)		-0.007* (0.003)		-0.008† (0.004)		-0.013*** (0.002)		-0.005 (0.004)
Observations	9211	9211	24369	24369	24728	24728	24455	24455	24892	24892
Adjusted R^2	0.011	0.012	0.090	0.090	0.083	0.084	0.062	0.063	0.039	0.040
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Panel B: Mean Legislator Age										
	Satisfaction Democracy <i>Specific</i>		Dem. Economic <i>Diffuse</i>		Dem. Indecisive <i>Diffuse</i>		Dem. Order <i>Diffuse</i>		Dem. Better <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Legislator Mean Age	-0.032*** (0.002)	-0.030*** (0.003)	-0.024*** (0.004)	-0.021*** (0.005)	-0.055* (0.018)	-0.049* (0.019)	-0.020 (0.014)	-0.013 (0.014)	-0.049† (0.027)	-0.040 (0.028)
Young	0.000 (0.021)	0.414 (0.519)	-0.071** (0.021)	0.319 (0.294)	-0.041 (0.026)	0.681 (0.450)	-0.003 (0.027)	0.991† (0.459)	-0.168*** (0.033)	0.981 (0.615)
Young x Mean Age		-0.008 (0.010)		-0.008 (0.006)		-0.015 (0.009)		-0.020† (0.009)		-0.023† (0.013)
Observations	9211	9211	24369	24369	24728	24728	24455	24455	24892	24892
Adjusted R^2	0.011	0.011	0.090	0.090	0.085	0.085	0.061	0.062	0.039	0.040
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Standard Errors clustered by country. Controls include individual controls for gender, education, age, working status; and country controls like corruption index, gini index, % poverty and GDP-per capita.
Signif.: ***: 0.01, **: 0.05, *: 0.1.

F4 World Values Survey Results: Model with all control variables

The following table replicates the main World Values Survey analysis reported in Table 2, adding controls for income, religious affiliation, and political ideology. These variables are not included in the main specification in order to maintain comparability with the Barometer surveys, where income is not available in every wave, and religious affiliation and political ideology are not systematically measured across all survey waves. The inclusion of political ideology reduces the sample size by approximately 15 percent due to missing values. Results are substantively unchanged from those reported in the main text.

Table F.17: Cross-National Differences in Legislator Ages and Support for Democracy: World Value Survey - with all individual controls

Panel A: Age Differences

	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator-Public Age Difference	-0.013 (0.008)	-0.012 (0.008)	-0.020*** (0.005)	-0.021*** (0.005)	-0.054*** (0.005)	-0.051*** (0.004)
Young	-0.036* (0.016)	0.028 (0.039)	-0.188*** (0.035)	-0.228*** (0.057)	-0.080* (0.035)	0.103 (0.083)
Young x Age Difference		-0.005* (0.002)		0.003 (0.005)		-0.012* (0.004)
Observations	77522	77522	77945	77945	20304	20304
Adjusted R^2	0.105	0.106	0.064	0.064	0.117	0.119
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Panel B: Mean Legislator Age

	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator Mean Age	-0.021* (0.008)	-0.020* (0.008)	-0.015* (0.007)	-0.011 [†] (0.006)	-0.063*** (0.008)	-0.056*** (0.008)
Young	-0.037* (0.015)	0.144 (0.159)	-0.191*** (0.035)	0.336 (0.469)	-0.080* (0.035)	0.966* (0.386)
Young x Mean Age		-0.004 (0.003)		-0.011 (0.010)		-0.021* (0.008)
Observations	77522	77522	77945	77945	20304	20304
Adjusted R^2	0.107	0.107	0.062	0.062	0.117	0.117
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Controls include individual controls for gender, education, age, working status, political interest, income, religion, and ideology; and country controls like corruption index, gini index, % poverty and GDP-per capita.

Signif.: ***: 0.001, **: 0.01, *: 0.05, [†]: 0.1.

F5 World Values Survey Results with Country-Fixed Effects

The following table shows the results when we consider Country-Fixed Effects instead of Country and regional covariates in our regression model.

Table F.18: Cross-National Differences in Legislator Ages and Support for Democracy: World Value Survey - Country Fixed Effects Models

Panel A: Age Differences						
	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator-Public Age Difference	0.002 (0.014)	0.004 (0.014)	-0.032*** (0.008)	-0.033*** (0.009)	-0.065*** (0.002)	-0.063*** (0.002)
Young	-0.049** (0.015)	0.007 (0.032)	-0.193*** (0.032)	-0.224*** (0.050)	-0.087* (0.030)	0.066 (0.068)
Young x Age Difference		-0.004* (0.002)		0.002 (0.004)		-0.011* (0.004)
Observations	92195	92195	93238	93238	25557	25557
Adjusted R^2	0.016	0.016	0.017	0.017	0.023	0.024
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓

Panel B: Mean Legislator Age						
	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator Mean Age	-0.011 (0.019)	-0.008 (0.019)	-0.035*** (0.008)	-0.031** (0.009)	-0.088*** (0.003)	-0.081*** (0.003)
Young	-0.049** (0.015)	0.266 [†] (0.135)	-0.193*** (0.032)	0.259 (0.405)	-0.087* (0.030)	1.118* (0.383)
Young x Mean Age		-0.006* (0.003)		-0.009 (0.008)		-0.025** (0.008)
Observations	92195	92195	93238	93238	25557	25557
Adjusted R^2	0.016	0.016	0.017	0.017	0.023	0.024
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Country FE	✓	✓	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Controls include individual controls for gender, education, age, working status; and country controls like corruption index, gini index, % poverty and GDP-per capita.

Signif.: ***: 0.01, **: 0.05, *: 0.1.

F6 World Values Survey Results with Age as Continuous Variable

Table F.19: Cross-National Differences in Legislator Ages and Support for Democracy: World Value Survey - Age as Continuous Models

Panel A: Age Differences

	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator-Public Age Difference	-0.009 (0.009)	-0.015 (0.010)	-0.017** (0.005)	-0.019 [†] (0.010)	-0.031** (0.008)	-0.052*** (0.010)
Age	0.002* (0.001)	-0.000 (0.001)	0.006*** (0.001)	0.006*** (0.002)	0.002* (0.001)	-0.004* (0.002)
Age x Age Difference		0.000* (0.000)		0.000 (0.000)		0.000** (0.000)
Observations	92195	92195	93238	93238	25557	25557
Adjusted R^2	0.093	0.093	0.049	0.049	0.095	0.097
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Panel B: Mean Legislator Age

	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator Mean Age	-0.020* (0.009)	-0.031** (0.009)	-0.012 (0.008)	-0.030 [†] (0.017)	-0.050** (0.014)	-0.084*** (0.019)
Age	0.002* (0.001)	-0.011* (0.005)	0.007*** (0.001)	-0.014 (0.013)	0.002* (0.001)	-0.036** (0.010)
Age x Mean Age		0.000* (0.000)		0.000 (0.000)		0.001** (0.000)
Observations	92195	92195	93238	93238	25557	25557
Adjusted R^2	0.095	0.096	0.047	0.048	0.096	0.096
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Controls include individual controls for gender, education, age, working status; and country controls like corruption index, gini index, % poverty and GDP-per capita.

Signif.: ***: 0.01, **: 0.05, *: 0.1.

F7 World Values Survey Results for Alternative Democracy Index

The following table shows the results when we take out the variable "Democracy is always better" from the Democracy Index reported in Table 2

Table F.20: Cross-National Differences in Legislator Ages and Support for Democracy: World Value Survey - Alternative Democracy Index

Panel A: Age Differences

	Dem. Index (simple) <i>Diffuse</i>			
	(1)	(2)	(3)	(4)
Legislator-Public Age Difference	-0.020** (0.006)	-0.017* (0.006)	-0.032*** (0.002)	-0.030*** (0.002)
Young	-0.044 (0.027)	0.108 (0.065)	-0.041 (0.026)	0.106 (0.063)
Young x Age Difference		-0.011** (0.003)		-0.010** (0.003)
Observations	25302	25302	25302	25302
Adjusted R^2	0.100	0.101	0.021	0.022
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country FE			✓	✓

Panel B: Mean Legislator Age

	Dem. Index (simple) <i>Diffuse</i>			
	(1)	(2)	(3)	(4)
Legislator Mean Age	-0.038*** (0.008)	-0.032** (0.009)	-0.044*** (0.002)	-0.038*** (0.002)
Young	-0.045 (0.027)	0.713 (0.433)	-0.041 (0.026)	0.808 [†] (0.385)
Young x Mean Age		-0.016 [†] (0.009)		-0.017* (0.008)
Observations	25302	25302	25302	25302
Adjusted R^2	0.100	0.101	0.021	0.021
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country FE			✓	✓

Notes: OLS estimates. Outcome variables are standardized. Controls include individual controls for gender, education, age, working status; and country controls like corruption index, gini index, % poverty and GDP-per capita.

Signif.: ***: 0.01, **: 0.05, *: 0.1.

F8 World Values Survey Results: Model with political interest/sophistication

The following table shows the results when we include as a control variable the political interest or sophistication of the respondent. For political sophistication we consider a dummy variable equal to one for respondents who report being highly interested in politics, based on the political interest question included in the World Values Survey. Respondents who report being 'very interested' in politics are coded as one, and those reporting little or no interest are coded as zero.

Table F.21: Cross-National Differences in Legislator Ages and Support for Democracy: World Value Survey - with Political Interest/Sophistication

Panel A: Age Differences

	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator-Public Age Difference	-0.009 (0.009)	-0.007 (0.009)	-0.018** (0.005)	-0.019*** (0.005)	-0.054*** (0.004)	-0.050*** (0.004)
Young	-0.038* (0.017)	0.029 (0.036)	-0.184*** (0.033)	-0.217*** (0.055)	-0.074* (0.032)	0.092 (0.076)
Young x Age Difference		-0.005* (0.002)		0.002 (0.004)		-0.011* (0.004)
Observations	91787	91787	92813	92813	24486	24486
Adjusted R^2	0.093	0.093	0.054	0.054	0.108	0.110
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Panel B: Mean Legislator Age

	How Democratic <i>Specific</i>		Dem. Important <i>Diffuse</i>		Dem. Index <i>Diffuse</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Legislator Mean Age	-0.020* (0.009)	-0.017† (0.009)	-0.012 (0.008)	-0.008 (0.007)	-0.066*** (0.007)	-0.058*** (0.007)
Young	-0.039* (0.017)	0.284† (0.160)	-0.187*** (0.033)	0.315 (0.443)	-0.074* (0.032)	1.012* (0.392)
Young x Mean Age		-0.007* (0.003)		-0.010 (0.009)		-0.022* (0.008)
Observations	91787	91787	92813	92813	24486	24486
Adjusted R^2	0.095	0.096	0.052	0.052	0.108	0.109
Controls	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Controls include individual controls for gender, education, age, working status, political interest; and country controls like corruption index, gini index, % poverty and GDP-per capita.

Signif.: ***: 0.001, **: 0.01, *: 0.05, †: 0.1.

G Barometers

G1 Barometer Surveys Countries and Years

The following table shows the number of respondents per Country and Survey Wave of the four Barometer surveys: Eurobarometer, Latinobarometer, Afrobarometer, and Asianbarometer.

Table G.22: Number of Respondents by Country and Year Group (1995–2023)

Country	Chronology of Barometer waves						Total No.
	1995– 1999 No.	2000– 2004 No.	2005– 2009 No.	2010– 2014 No.	2015– 2019 No.	2020– 2023 No.	
Argentina	0	0	0	0	4,800	1,200	6,000
Australia	0	0	0	0	1,630	0	1,630
Austria	0	0	0	3,998	10,089	11,082	25,169
Belgium	7,215	8,107	3,025	8,165	10,211	11,487	48,210
Benin	0	0	0	0	1,200	0	1,200
Bolivia	0	0	0	0	4,800	0	4,800
Bosnia	0	0	0	0	0	4,062	4,062
Botswana	0	0	0	1,200	2,398	0	3,598
Brazil	4,281	5,404	6,020	3,612	4,858	1,204	25,379
Bulgaria	0	959	2,790	8,003	9,977	11,336	33,065
Burkina Faso	0	0	1,200	1,200	2,400	0	4,800
Cabo Verde	0	0	0	2,408	2,400	0	4,808
Chile	4,840	5,953	6,000	3,600	4,800	1,200	26,393
Colombia	0	0	0	0	4,800	1,200	6,000
Costa Rica	0	0	0	0	4,000	1,000	5,000
Croatia	0	0	0	2,983	10,204	11,147	24,334
Cyprus	0	0	0	3,497	4,970	5,533	14,000
Czechia	0	1,054	3,175	8,005	10,036	11,399	33,669
Denmark	6,406	7,595	3,031	8,013	10,052	11,154	46,251
Dominican R.	0	0	0	3,000	4,000	1,000	8,000
Ecuador	0	0	0	1,200	4,800	1,200	7,200
Estonia	0	0	0	6,783	9,321	11,185	27,289
Finland	7,307	8,017	3,035	7,919	9,968	11,320	47,566
France	0	5,060	2,992	7,923	9,948	11,040	36,963
Gambia	0	0	0	0	1,200	1,200	2,400
Germany	14,290	15,504	4,504	12,261	15,248	16,810	78,617
Ghana	0	0	0	2,400	4,800	0	7,200
Greece	7,053	7,779	2,996	8,020	10,067	11,176	47,091
Guatemala	0	0	0	1,000	4,000	1,000	6,000
Hungary	0	0	0	2,120	6,184	0	8,304
Iceland	0	0	0	517	0	2,029	2,546
Indonesia	0	0	0	0	3,090	0	3,090
Ireland	6,612	7,588	2,837	7,763	9,830	11,177	45,807
Italy	7,361	8,001	2,971	8,101	10,022	11,222	47,678
Japan	0	0	0	0	2,126	0	2,126
Kenya	0	0	0	2,397	1,599	0	3,996
Latvia	0	0	0	7,824	9,720	11,133	28,677
Lesotho	0	0	1,200	2,397	1,200	1,200	5,997
Lithuania	0	0	0	5,894	9,715	11,062	26,671
Luxembourg	0	0	0	1,995	4,967	5,685	12,647
Malawi	0	0	0	2,400	1,200	0	3,600

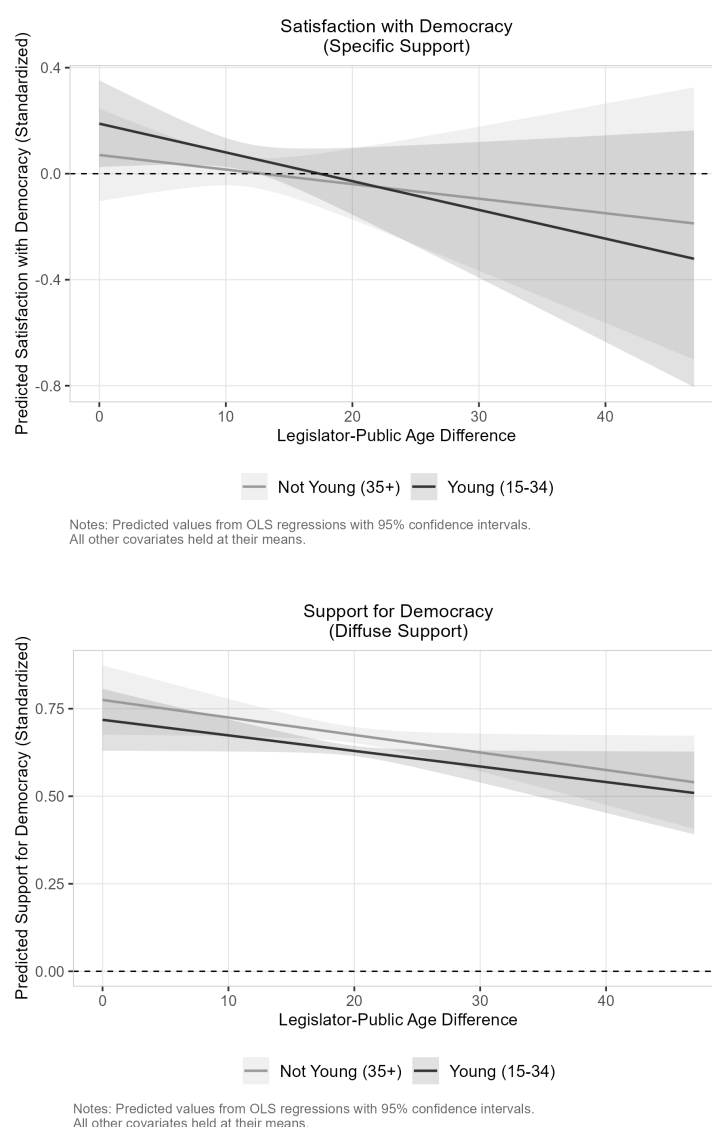
Country	1995– 1999 No.	2000– 2004 No.	2005– 2009 No.	2010– 2014 No.	2015– 2019 No.	2020– 2023 No.	Total No.
Mali	0	0	0	1,200	1,200	0	2,400
Malta	0	0	0	1,914	4,760	5,607	12,281
Mauritius	0	0	0	2,400	1,200	1,200	4,800
Mexico	0	0	0	1,200	4,800	1,200	7,200
Mongolia	0	0	0	2,438	1,284	0	3,722
Montenegro	0	0	0	0	0	1,535	1,535
Namibia	1,183	1,199	2,400	2,400	2,400	0	9,582
Netherlands	0	0	0	6,046	10,197	11,146	27,389
Nigeria	0	0	0	0	1,681	1,599	3,280
North Macedonia	0	0	0	0	992	5,171	6,163
Norway	801	938	0	0	0	4,184	5,923
Panama	0	0	0	0	4,000	1,000	5,000
Paraguay	0	0	0	1,200	4,800	1,200	7,200
Peru	0	0	0	2,400	4,800	1,200	8,400
Philippines	0	0	0	1,200	0	0	1,200
Poland	0	947	2,863	7,497	9,446	11,212	31,965
Portugal	6,997	7,850	2,831	8,056	10,186	11,270	47,190
Romania	0	0	0	6,130	10,104	11,580	27,814
Senegal	0	0	0	2,400	1,200	1,200	4,800
Sierra Leone	0	0	0	0	1,200	1,200	2,400
Slovakia	0	1,230	3,246	7,988	10,027	11,310	33,801
Slovenia	0	0	0	2,070	10,000	11,116	23,186
South Africa	0	0	0	0	4,230	1,600	5,830
South Korea	0	0	0	0	2,468	0	2,468
Spain	0	17,598	12,799	12,906	9,958	11,049	64,310
Sweden	7,042	7,830	2,981	8,139	10,119	11,602	47,713
Switzerland	0	0	0	0	0	4,202	4,202
Taiwan	0	0	0	1,657	1,259	0	2,916
Togo	0	0	0	1,200	0	0	1,200
Tunisia	0	0	0	1,200	2,399	1,200	4,799
Turkey	0	988	2,754	951	0	0	4,693
United Kingdom	4,926	7,885	2,942	7,884	9,890	5,590	39,117
Uruguay	0	0	0	0	4,800	1,200	6,000
Zambia	0	0	1,200	8	0	0	1,208
Total	86,314	127,486	79,792	237,082	376,030	332,816	1,239,520

Notes: Respondent counts from national surveys conducted each year from 1995 to 2023

G2 Barometer Survey Results: Interaction figures

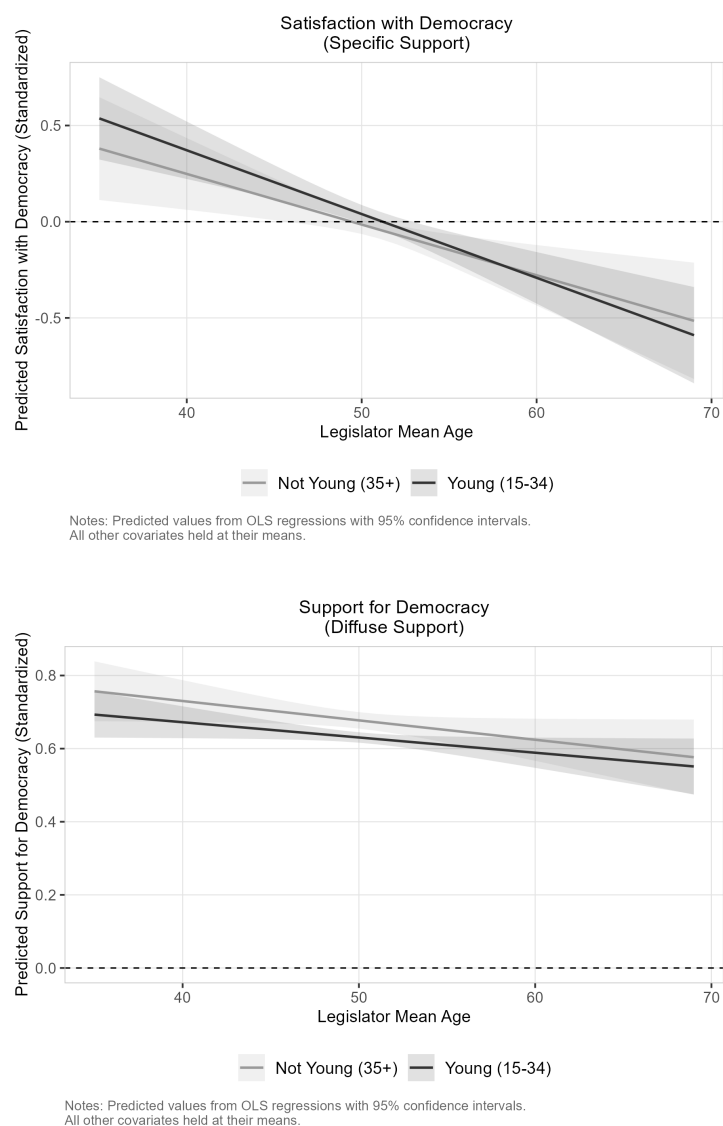
The following figures display the predicted values of democratic support for young respondents (aged 15–34) and older respondents (aged 35 and above) across the observed range of the age gap variables, based on the interaction models reported in Table 3. These figures complement the regression results by illustrating how the predicted levels of democratic support for each age group evolve across the full distribution of age gap values.

Figure G.9: Barometer Surveys: Predicted Values by Age Group across the Range of Legislator-Public Age Differences



Notes: Predicted values from OLS regressions with 95% confidence intervals, estimated separately for young respondents (aged 15–34) and older respondents (aged 35 and above) across the observed range of the Legislator-Public Age Difference variable in the pooled Barometer sample. All other covariates are held at their means. Models include year fixed effects and all controls reported in Table 3.

Figure G.10: Barometer Surveys: Predicted Values by Age Group across the Range of Mean Legislator Age



Notes: Predicted values from OLS regressions with 95% confidence intervals, estimated separately for young respondents (aged 15–34) and older respondents (aged 35 and above) across the observed range of the Mean Legislator Age variable in the pooled Barometer sample. All other covariates are held at their means. Models include year fixed effects and all controls reported in Table 3.

G3 Barometer Survey Results with Country-Fixed Effects

Table G.23: Cross-National Differences in Legislator Ages and Support for Democracy: Barometers - Country Fixed Effects Models

Panel A: Age Differences

	Satisfaction Democracy <i>Specific</i>		Support Democracy <i>Diffuse</i>	
	(1)	(2)	(3)	(4)
Legislator-Public Age Difference	-0.009 (0.008)	-0.008 (0.008)	0.001 (0.003)	0.001 (0.003)
Young	0.056*** (0.014)	0.110*** (0.024)	-0.044*** (0.006)	-0.049** (0.018)
Young x Age Difference		-0.005*** (0.001)		0.000 (0.001)
Observations	1159880	1159880	208000	208000
Adjusted R^2	0.022	0.023	0.012	0.012
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country FE	✓	✓	✓	✓

Panel B: Mean Legislator Age

	Satisfaction Democracy <i>Specific</i>		Support Democracy <i>Diffuse</i>	
	(1)	(2)	(3)	(4)
Legislator Mean Age	-0.012 (0.010)	-0.010 (0.011)	0.001 (0.003)	0.001 (0.003)
Young	0.056*** (0.014)	0.438** (0.162)	-0.044*** (0.006)	-0.074 (0.047)
Young x Mean Age		-0.008* (0.003)		0.001 (0.001)
Observations	1159880	1159880	208000	208000
Adjusted R^2	0.022	0.023	0.012	0.012
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Country FE	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Standard Errors clustered by country. Controls include individual controls for gender, education, age, working status; and country controls like corruption index, gini index, % poverty and GDP-per capita.

Signif.: ***: 0.01, **: 0.05, *: 0.1.

G4 Barometer Survey Results with Age as continuous variable

Table G.24: Cross-National Differences in Legislator Ages and Support for Democracy: Barometers - Age as continuous Models

Panel A: Age Differences

	Satisfaction Democracy <i>Specific</i>		Support Democracy <i>Diffuse</i>	
	(1)	(2)	(3)	(4)
Legislator-Public Age Difference	-0.007 (0.007)	-0.013* (0.007)	-0.005* (0.002)	-0.004 (0.003)
Age	-0.002** (0.000)	-0.003** (0.001)	0.001*** (0.000)	0.002 (0.001)
Age x Age Difference		0.000* (0.000)		-0.000 (0.000)
Observations	1155930	1155930	208000	208000
Adjusted R^2	0.099	0.099	0.070	0.070
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Panel B: Mean Legislator Age

	Satisfaction Democracy <i>Specific</i>		Support Democracy <i>Diffuse</i>	
	(1)	(2)	(3)	(4)
Legislator Mean Age	-0.029*** (0.008)	-0.038*** (0.007)	-0.005* (0.002)	-0.003 (0.002)
Age	-0.002** (0.001)	-0.012† (0.006)	0.001*** (0.000)	0.004 (0.003)
Age x Mean Age		0.000† (0.000)		-0.000 (0.000)
Observations	1155930	1155930	208000	208000
Adjusted R^2	0.105	0.105	0.070	0.070
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Standard Errors clustered by country. Controls include individual controls for gender, education, age, working status; and country controls like corruption index, gini index, % poverty and GDP-per capita.

Signif.: ***: 0.01, **: 0.05, *: 0.1.

G5 Barometer Survey Results: Model with political interest/sophistication

The following table shows the results when we include as a control variable the political interest or sophistication of the respondent. For political sophistication we consider a dummy variable equal to one for respondents who report discussing politics frequently with family and friends, based on the political discussion question available across Barometer survey waves. Respondents who report discussing politics often are coded as one, and those who discuss politics rarely or never are coded as zero.

Table G.25: Cross-National Differences in Legislator Ages and Support for Democracy: Barometers - With Political Interest/Sophistication

Panel A: Age Differences

	Satisfaction Democracy		Support Democracy	
	<i>Specific</i>		<i>Diffuse</i>	
	(1)	(2)	(3)	(4)
Legislator-Public Age Difference	-0.007 (0.007)	-0.013* (0.007)	-0.005* (0.002)	-0.004 (0.003)
Age	-0.002** (0.000)	-0.003** (0.001)	0.001*** (0.000)	0.002 (0.001)
Age x Age Difference		0.000* (0.000)		-0.000 (0.000)
Observations	1155930	1155930	208000	208000
Adjusted R^2	0.099	0.099	0.070	0.070
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Panel B: Mean Legislator Age

	Satisfaction Democracy		Support Democracy	
	<i>Specific</i>		<i>Diffuse</i>	
	(1)	(2)	(3)	(4)
Legislator Mean Age	-0.029*** (0.008)	-0.038*** (0.007)	-0.005* (0.002)	-0.003 (0.002)
Age	-0.002** (0.001)	-0.012† (0.006)	0.001*** (0.000)	0.004 (0.003)
Age x Mean Age		0.000† (0.000)		-0.000 (0.000)
Observations	1155930	1155930	208000	208000
Adjusted R^2	0.105	0.105	0.070	0.070
Controls	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Notes: OLS estimates. Outcome variables are standardized. Standard Errors clustered by country. Controls include individual controls for gender, education, age, working status, political interest; and country controls like corruption index, gini index, % poverty and GDP-per capita. Signif.: ***: 0.001, **: 0.01, *: 0.05, †: 0.1.

H Conjoint Experiment

H1 Conjoint Sources

We attained the demographic information for each country from the following sources.

1. United States

- Information on the country's composition in gender, ethnicity, and educational level was obtained from the US Census Quick Facts dataset. We accessed the data on September 7th, 2023 at <https://www.census.gov/quickfacts/fact/table/US/PST045222>
- The country's age composition was obtained from the population projections prepared by the United Nations Department of Economic and Social Affairs in 2022. We accessed the data on October 30th, 2023 at <https://population.un.org/wpp/Download/Standard/Population/>
- The country's religious composition was obtained from surveys conducted by the PEW Research Center in 2021. We accessed the data on October 30th, 2023 at <https://www.pewresearch.org/religion/2021/01/14/measuring-religion-in-pew-research-centers-american-trends-panel/>
- The country's composition in LGBTQ+ identification was obtained from a Gallup survey in 2022. We accessed the data on October 30th, 2023 at <https://news.gallup.com/poll/389792/lgbt-identification-ticks-up.aspx>

2. Italy

- The country's gender composition was obtained from the Istituto Nazionale di Statistica (ISTAT). We accessed the data on September 30th, 2023 at https://esploradati.istat.it/databrowser/#/it/dw/categories/IT1,POP,1.0/POP_POPULATION/DCIS_POPORESBIL1/IT1,22_315_DF_DCIS_POPORESBIL1_1,1.0
- Information on the country's ethnicity composition and LGBTQ+ identification was obtained from Special Eurobarometer 493: Discrimination in the EU. We accessed the data on September 7th, 2023 at https://data.europa.eu/data/datasets/s2251_91_4_493_eng?locale=en
- The country's educational composition was obtained from the OECD Education at a Glance 2023. We accessed the data on September 7th, 2023 at https://www.oecd-ilibrary.org/education/education-at-a-glance-2023_e13bef63-en
- The country's religious composition was obtained from Eurobarometer 2021 516: European citizens' knowledge and attitudes towards science and technology. We accessed the data on September 7th, 2023 at <https://europa.eu/>

eurobarometer/surveys/detail/2237

- The country's age composition was obtained from the population projections prepared by the United Nations Department of Economic and Social Affairs in 2022. We accessed the data on October 30th, 2023 at <https://population.un.org/wpp/Download/Standard/Population/>

3. South Korea

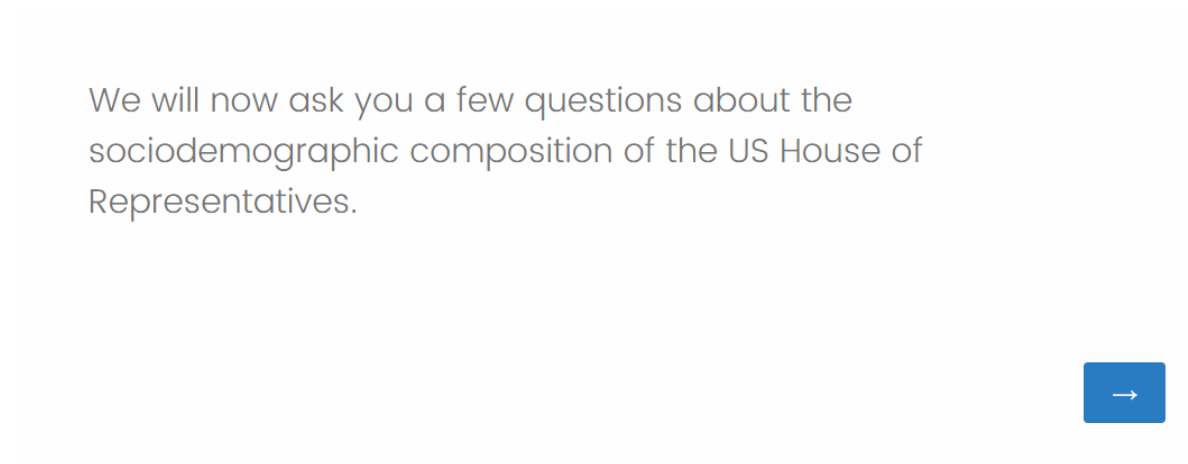
- The country's gender composition was obtained from World Bank staff estimates based on age/sex distributions of United Nations Population Division's World Population Prospects: 2022 Revision. We accessed the data on September 30th, 2023 at <https://data.worldbank.org/indicator/SP.POP.TOTL.FE.ZS?locations=KR>
- The country's ethnic composition was obtained from the migration statistics of the Ministry of Justice of the Government of South Korea 2023. We accessed the data on September 7th, 2023 at <https://www.moj.go.kr/moj/2412/subview>
- The country's educational composition was obtained from the OECD Education at a Glance 2023. We accessed the data on September 7th, 2023 at https://www.oecd-ilibrary.org/education/education-at-a-glance-2023_e13bef63-en
- The country's religious composition was obtained from the KOSIS statistics office of South Korea, based on the 2015 census. We accessed the data on September 7th, 2023 at https://kosis.kr/statHtml/statHtml.do?orgId=101&tblId=DT_1PM1502&conn_path=I2
- The country's age composition was obtained from the population projections prepared by the United Nations Department of Economic and Social Affairs in 2022. We accessed the data on October 30th, 2023 at <https://population.un.org/wpp/Download/Standard/Population/>

H2 Conjoint Set-up

The conjoint experiment followed this set-up within the survey:

1. First page of introduction

Figure H.11: Introduction to the topic



Notes: Screenshot of the Qualtrics' US survey, December 2023 .

2. Set Baseline: We informed the respondents about the demographic characteristics of the adult population in their country

Figure H.12: Demographics baseline

The United States is a diverse country when considering aspects like ethnicity, sexual diversity, or religion. Many citizens want Congress to represent this diversity, but some aspects are represented better than others. The current demographic composition of the United States is as follows:

	Actual US Demographics 2023
Sexual Orientation	• 7% Identify as LGBTQ+ • 93% identify as heterosexual
Religious Affiliation	• 61% Christian (Catholic, Protestants & others) • 39% Other & No Religion
Age	• 72% Under 60 • 28% Over 60
Gender	• 50% woman • 50% man
Ethnicity	• 59% White • 41% ethnic minorities (latino, african american, asian)
Educational Attainment	• 66% High School Degree or lower • 34% Bachelor's Degree or higher

The task on the next page will ask you to choose between two hypothetical compositions of the House of Representatives, each of which will represent some groups better than others. You will be asked to indicate which elected legislature you would prefer.



Notes: Screenshot of the Qualtrics' US survey, December 2023 .

3. Conjoint: Forced choice between 2 congress alternatives

Figure H.13: Conjoint forced choice

Please consider the following two hypothetical US Congresses.

Which one would you prefer as the elected body of the US people?

	Actual US Demographics 2023	Possible Congress 1	Possible Congress 2
Sexual Orientation	7% Identify as LGBTQ+ 93% identify as heterosexual	13% identify as LGBTQ+ 87% identify as Heterosexual	7% identify as LGBTQ+ 93% identify as Heterosexual
Religious Affiliation	61% Christian (Catholic, Protestants & others) 39% Other & No Religion	61% Christian 39% Other & No Religion	76% Christian 24% Other & No Religion
Age	72% Under 60 28% Over 60	87% under 60 13% over 60	57% under 60 43% over 60
Gender	50% woman 50% man	35% women 65% men	65% women 35% men
Ethnicity	59% White (latino, african american, asian) 41% ethnic minorities	44% white 56% ethnic minorities	74% white 26% ethnic minorities
Educational Attainment	66% High School Degree or lower 34% Bachelor's Degree or higher	51% High school Degree 49% Bachelor Degree	51% High school Degree 49% Bachelor Degree

Which one of the two parliaments would you prefer as the elected body of the US people?

☐ Congress 1

☐ Congress 2

Notes: Screenshot of the Qualtrics' US survey, December 2023 .

4. Conjoint: rating the 2 congress alternatives

Figure H.14: Conjoint forced choice

On a scale from 1 to 10, where 1 = not well at all and 10 = extremely well, and considering the House of Representatives' sociodemographic characteristics, **how well do you think each congress will represent the US people?**

Not well at all	Slightly well	Moderately well	Very well	Extremely well					
1	2	3	4	5	6	7	8	9	10

Congress 1

Congress 2

Notes: Screenshot of the Qualtrics' US survey, December 2023

H3 Conjoint Attributes and Levels

The following tables report the baseline demographic information as well as the possible attributes for the conjoint experiment, by country.

Table H.26: Baseline information USA - Italy - South Korea

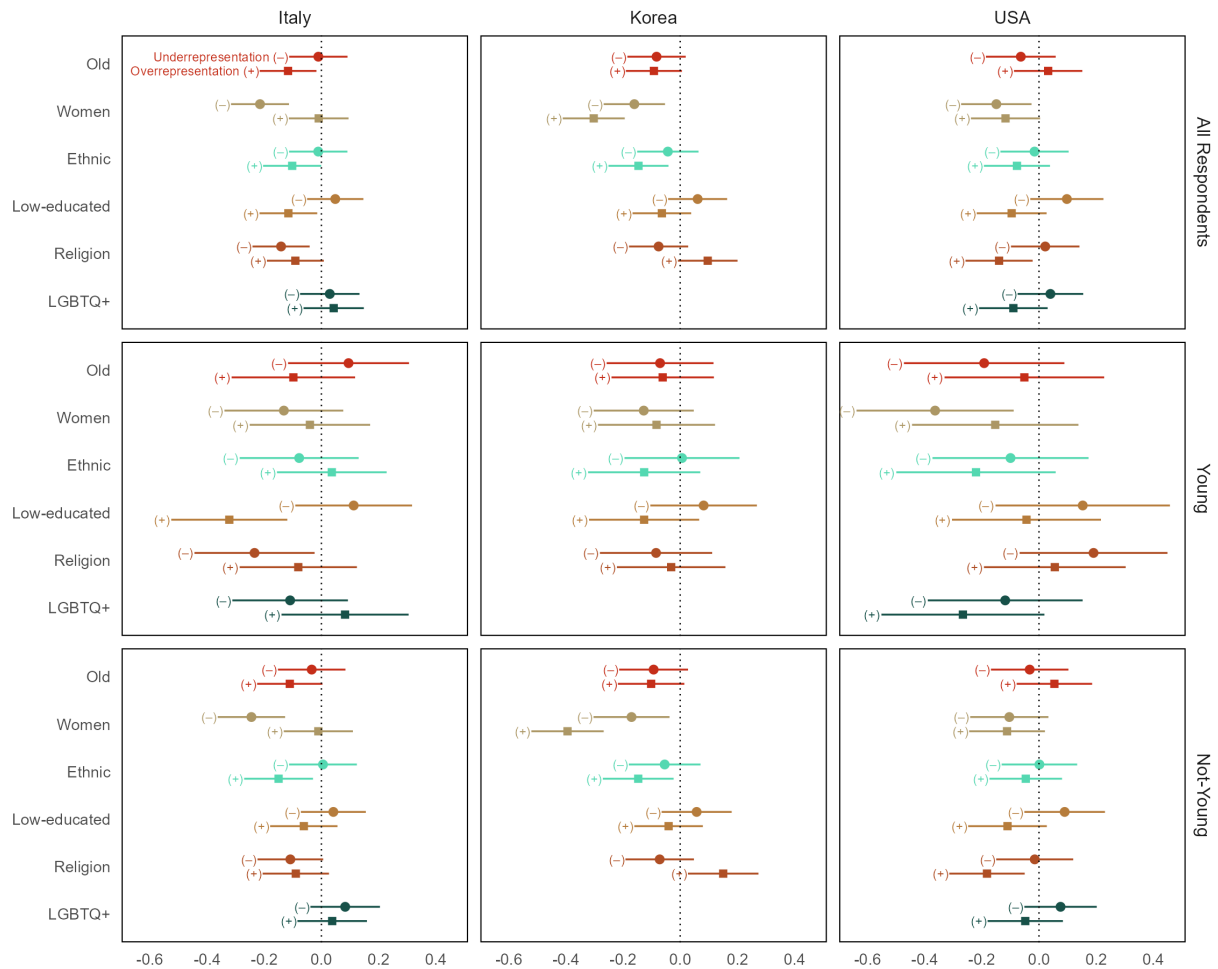
Attributes	US 2023	IT 2023	SK 2023
Gender representation	50% women 50% men	51% women 49% men	50% women 50% men
Ethnic representation	59% White 41% Other (african american, latino, asian)	91% Italian origin 9% Non-italian origin (African, South american, others)	95% Korean origin 5% Non-korean origin (Chinese, Japanese, others)
Education representation	66% High School Degree or lower 34% Bachelor Degree or higher	80% High School Degree or lower 20% Bachelor Degree or higher	46% High School Degree or lower 54% Bachelor Degree or higher
LGBTQ+ Minority representation	7% Identifies as LGBTQ+ 93% identifies as heterosexual	2% Identifies as LGBTQ+ 98% identifies as heterosexual	Not official information or survey
Religion representation	61% Christians 39% Other religion or no religion	79% Catholics 21 % Other or no religion	43% Religious (catholics,buddhist, other) 57 % No religious
Age representation	72% Under 60 28% Over 60	66% Under 60 34% Over 60	75% Under 60 25% Over 60

Table H.27: Conjoint Attributes and Levels

Attributes	#	Levels USA Content	Levels Italy Content	Level South Korea Content
Gender representation	3	35% women & 65% men 50% women & 50% men 65% women & 35% men	35% women & 65% men 50% women & 50% men 65% women & 35% men	35% women & 65% men 50% women & 50% men 65% women & 35% men
Ethnic representation	3	74% white & 26% ethnic minority 59% white & 41% ethnic minority 44% white & 56% ethnic minority	99% Italian Origin & 1% Non-italian origin 91% Italian Origin & 9% Non-italian origin 83% Italian Origin & 17% Non-italian origin	99% Korean Origin & 1% Non-Korean origin 95% Korean Origin & 5% Non-Korean origin 91% Korean Origin & 9% Non-Korean origin
Education representation	3	49% Bachelor Degree or Higher & 51% High School degree or lower 34% Bachelor Degree or Higher & 66% High School degree or lower 19% Bachelor Degree or Higher & 81% High School degree or lower	35% Bachelor Degree or Higher & 65% High School degree or lower 20% Bachelor Degree or Higher & 80% High School degree or lower 5% Bachelor Degree or Higher & 95% High School degree or lower	79% Bachelor Degree or Higher & 21% High School degree or lower 54% Bachelor Degree or Higher & 46% High School degree or lower 39% Bachelor Degree or Higher & 61% High School degree or lower
LGBTQ+ Minority representation	3	13% Identifies as LGBTQ+ & 87% identifies as Heterosexual 7% Identifies as LGBTQ+ & 93% identifies as Heterosexual 1% Identifies as LGBTQ+ & 99% identifies as Heterosexual	3.5% Identifies as LGBTQ+ & 96.5% identifies as Heterosexual 2% Identifies as LGBTQ+ & 98% identifies as Heterosexual 0.5% Identifies as LGBTQ+ & 99.5% identifies as Heterosexual	Do not apply
Religion representation	3	76% Christian Religions & 24 % Other or no religion 61% Christian Religions & 39 % Other or no religion 46% Christian Religions & 54 % Other or no religion	94% Catholics & 6 % Other or no religion 79% Catholics & 21 % Other or no religion 64% Catholics & 36 % Other or no religion	58% Religious & 42 % no religious 43% Religious & 57 % no religious 28% Religious & 72 % no religious
Age representation	3	43% Over 60 & 57% Under 60 28% Over 60 & 72% Under 60 13% Over 60 & 87% Under 60	49% Over 60 & 51% Under 60 34% Over 60 & 66% Under 60 19% Over 60 & 71% Under 60	40% Over 60 & 60% Under 60 25% Over 60 & 75% Under 60 10% Over 60 & 90% Under 60

H4 Rating: Conjoint by age group

Figure H.15: Conjoint Results by Age Group (AMCE)

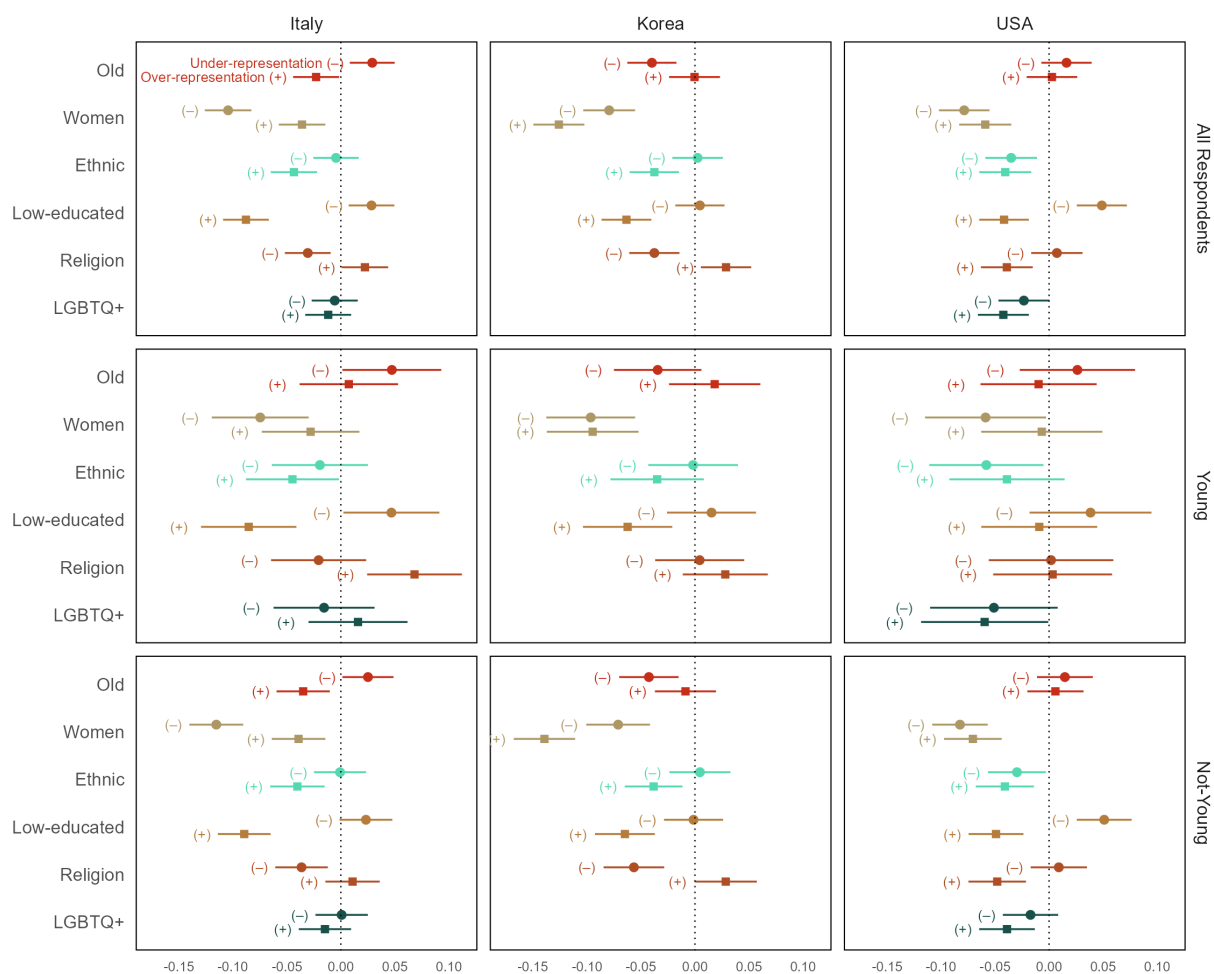


Notes: AMCES estimates and 95% confidence interval. The reference group is always the correct representation. The outcome is a 10-point rating of how well the legislature would represent the public. Young is 18-34 years old.

H5 Forced Choice: Conjoint by age group

This section shows the results of our forced choice, where the question was: *Please consider the following two hypothetical US Congresses. Which one would you prefer as the elected body of the US people?*

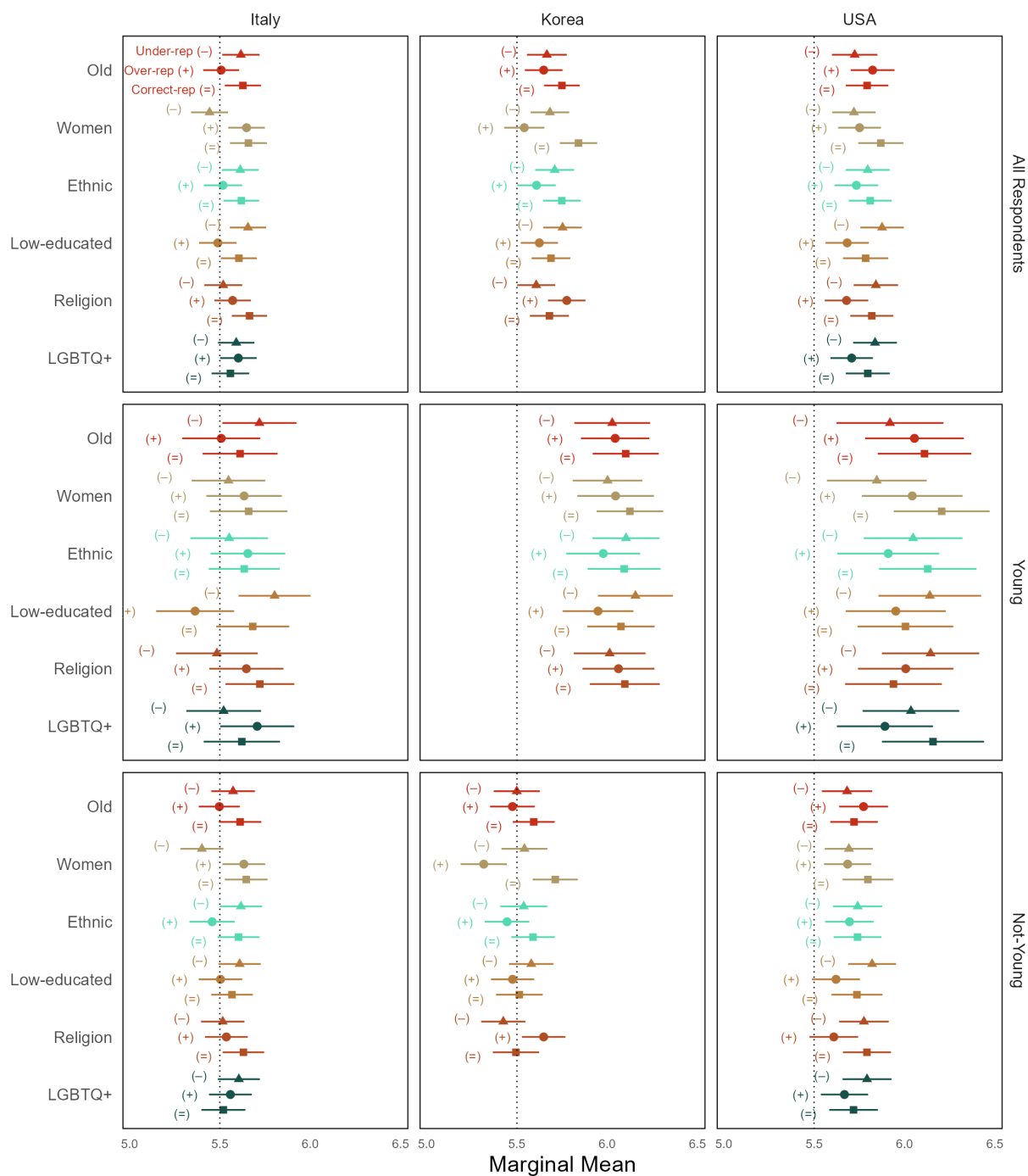
Figure H.16: Conjoint AMCES Means by Age: Forced Choice



Notes: AMCES estimates and 95% confidence interval. Reference group is always correct representation. The outcome is the forced choice of which one they prefer as elected body of the country's people. Young is 18-34 years old.

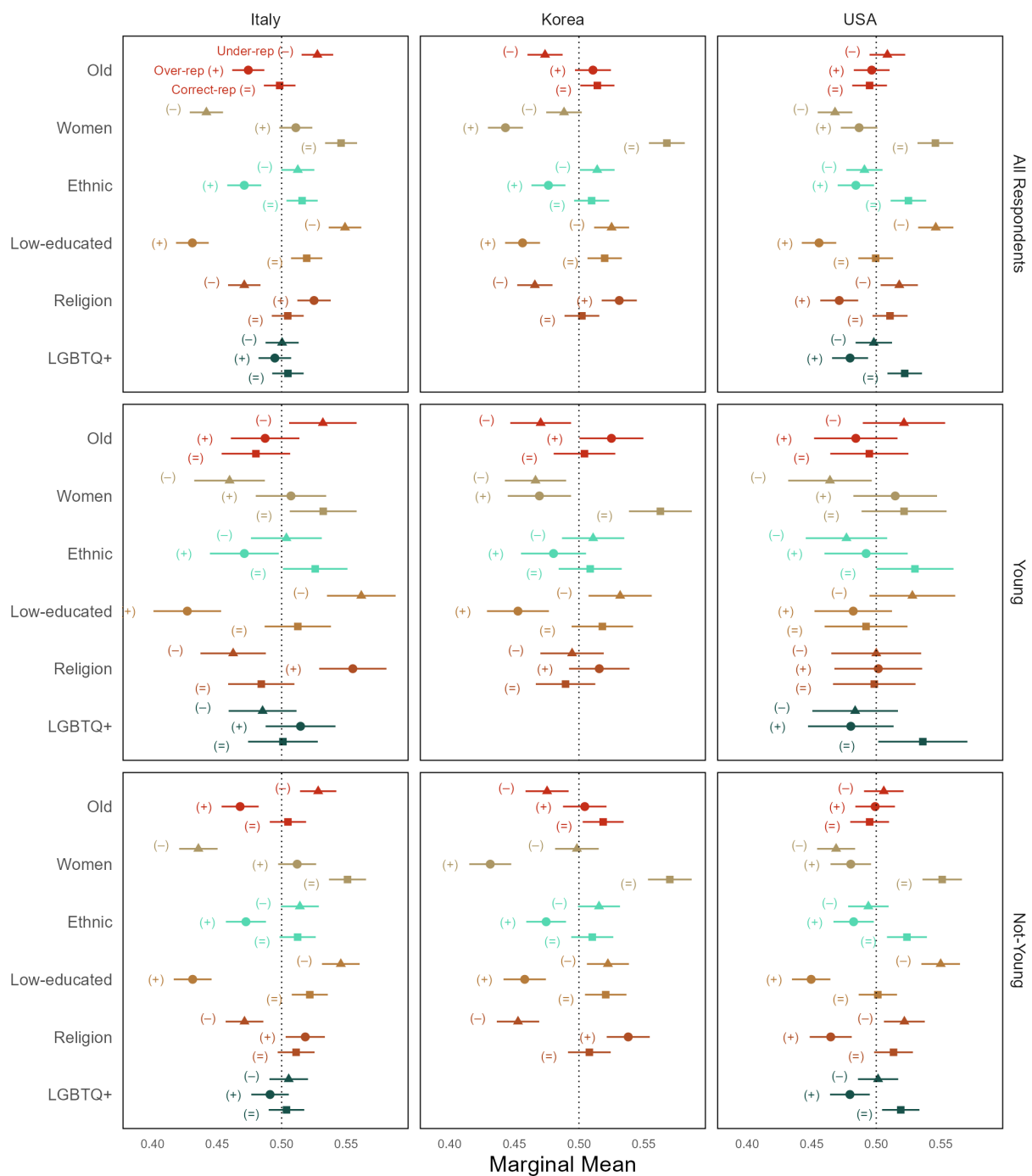
H6 Marginal Means

Figure H.17: Conjoint Marginal Means by Age: Ratings



Notes: Marginal Means estimates and 95% confidence interval. The outcome is a 10-point rating of how well the legislature would represent the public. Young is 18-34 years old. Young is 18-34 years old.

Figure H.18: Conjoint Marginal Means by Age: Forced Choice.



Notes: Marginal Means estimates and 95% confidence interval. The outcome is the forced choice of which one they prefer as elected body of the country's people. Young is 18-34 years old.

I Pre-Registration

Here we report our hypotheses and analyses from our pre-registration plan for the two experiments. The registered research designs are reported prior in this appendix.

I1 Hypotheses for Age Gap Information Experiment

Main Effects

We expect that respondents exposed to an age gap between political leaders and the adult public in their country will be less satisfied with how democracy functions in their country.

We expect that reduced satisfaction with democracy in their country will be linked to perceptions that the age gap makes democracy less representative of the public's preferences and less effective at addressing major issues in their country.

We also investigate whether exposure to their country's age gap reduces support for democracy generally (i.e. reduces their principled support for democracy rather than their satisfaction with democracy in their own country) and reduces perceptions of how representative and effective democracy is generally. However, we have only weak expectations that these general effects will apply, since the age gaps are specific to their own countries and may not be perceived as inherent to democratic political systems.

Variation by Country

We expect that negative effects of the age gap treatment on support for democracy will be stronger in countries with larger age gaps. As a result, we would expect to see larger treatment effects in the United States and South Korea compared to Italy, since the age gaps are much more pronounced in the former compared to the latter. However, this expectation may also not hold if other country-specific factors influence reactions to the treatment.

Subgroup Effects

We expect the negative effects on support for democracy to be stronger among younger respondents.

We expect the negative effects on support for democracy to be stronger among respondents who express more concern about the implications of the age gap.

We expect the negative effects on support for democracy to be stronger among respondents who hold more ageist attitudes toward older people.

We will also explore whether treatment effects vary by education, gender, and political affiliation.

I2 Analysis for Age Gap Information Experiment

We will analyze the results with data aggregated across the three countries and with data from each country separately.

For main effects, we will analyze the difference in means between the treatment and control groups using OLS regression. We will also implement models controlling for age, education, gender, region of residence, employment, religion, ethnicity (US only), income, and political ideology. For subgroup effects, we will interact an indicator for the treatment with the relevant variables, using OLS with and without the control variables.

I3 Hypotheses for Conjoint Experiment

The experiment is designed to assess whether gaps between political leaders and the public for some demographic characteristics are seen as more problematic for representation than others. Specifically, we randomize whether and to what extent political leaders differ from the public in terms of their age, gender, educational background, sexual orientation, ethnicity, and religious affiliation. We do not have strong expectations about which gaps may be perceived more negatively by respondents from the United States, Italy, and South Korea; rather, we are interested in understanding how age gaps compare to gaps on these other characteristics, and whether the effect is weaker, stronger, or similar in magnitude.

I4 Analysis for Conjoint Experiment

We will use standard methods for analyzing conjoint experiments, reporting the AMCE and the marginal means.

Internal Review Board Approval

The study was approved by the Bocconi Research Ethics Committee (application EA000694) on November 15, 2023. All participants provided informed consent prior to participating in the study.

Chapter 3

Far-Right Electoral Victories and Hate Crimes: Evidence from
U.S. Legislative Elections

Far-Right Electoral Victories and Hate Crimes: Evidence from U.S. Legislative Elections

Alonso Román Amarales * Dirck de Kleer *

May 14, 2026

Abstract

Do far-right electoral victories increase hate crimes? We examine this question in U.S. House of Representatives elections from 2006 to 2022, using a sharp regression discontinuity design around close races involving far-right candidates. We argue that legislative victories can increase hate crimes through two symbolic and communicative channels: the visibility of exclusionary rhetoric and the institutional legitimization of exclusionary views. Unlike studies of executive officeholders, our setting isolates the consequences of electoral victory in a context where winners have limited local policy authority. The results show a selective rather than general effect. Hate crimes against LGBTQ+ individuals increase consistently and substantially after far-right victories, especially during and after the election of Donald Trump. At the same time, we do not find a robust aggregate increase across all minority groups. Exploratory evidence from media data and surveys is consistent with the proposed visibility and legitimization channels, but not with rapid broad changes in attitudes toward minorities. Far-right electoral success can therefore reshape the local conditions under which minority groups exercise their rights, even in the absence of formal policy change.

1 Introduction

Liberal democracy rests on two foundations that can stand in tension: competitive elections and the protection of minority rights ([Mudde & Kaltwasser, 2012](#); [Mukand & Rodrik, 2020](#); [Powell, 2000](#)). As far-right politicians have entered legislative and executive offices in increasing numbers across Western democracies, this tension has become increasingly acute. Far-right parties have achieved historic electoral gains across European legislatures, and in the United States, the Make America Great Again movement now dominates the Republican Party in Congress ([Biebricher, 2024](#); [Pirro, 2023](#); [Valentim, 2024](#)). Central to their politics is the construction of ethnic, religious, and sexual minorities as threats

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to the national community, fuelling prejudice and hostility through rhetoric that frames these groups as outsiders to be marginalized or excluded (Mudde, 2007, 2019; Valentim, 2024). Yet how far-right electoral success translates into behavioral hostility against the very minorities these politicians target remains less well understood.

The stakes are high. Hate crimes are crimes targeting individuals because of their perceived membership in a social group, and they are a pervasive and growing problem across liberal democracies. In the United States, reported hate crimes have doubled between 2015 and 2025 (USAFacts, 2025). Their consequences extend well beyond their direct victims. Individual targets experience higher rates of depression, anxiety, anger, and post-traumatic stress than victims of otherwise similar offenses (Herek, Gillis, & Cogan, 1999). More broadly, hate crimes function as message crimes (Perry & Alvi, 2012): the victimization of one individual communicates threat to an entire minority community, producing an *in terrorem* effect that conditions where minorities live, work, and participate in public life (Perry & Alvi, 2012; Weinstein, 1992). The breadth of these effects is well documented: exposure to hate crimes reduces minority business ownership (Geisler, Enomoto, & Djaba, 2019), displaces minority families from their communities (Gabriel, Haws, Bailey, & Price, 2023), and causes affected groups to withdraw from public spaces and civic life (Benier, 2017; Hobbs & Lajevardi, 2019). Understanding the political conditions that drive hate crimes is therefore not only a question about criminal behavior. It is a question about the conditions under which minority groups can exercise their full rights in a democratic society.

Despite growing observational evidence linking far-right electoral gains to hate crime rates (L. Jacobs & van Spanje, 2021; Koopmans, 1996; Whittington & Piatkowska, 2025), the causal mechanisms through which electoral success generates behavioral change remain poorly understood. Existing causal work has linked increases in hate crimes to exclusionary rhetoric on social media (Müller & Schwarz, 2023), at campaign rallies (Feinberg, Branton, & Martinez-Ebers, 2022), and in the context of far-right mayoral victories in Italy (Romarri, 2020). Historical evidence further confirms that electoral outcomes can generate racial violence through political signaling, even absent direct policy effects (Testa & Williams, 2026). Yet an important question remains open: do the electoral victories of far-right candidates in legislative institutions increase hate crimes in the districts they represent?

We argue that far-right electoral success can increase hate crimes through two related channels: visibility and institutional legitimation. Far-right candidates do not simply express hostile views toward minority groups. When they win office, those views become attached to the authority and status of an elected representative. Victory keeps these candidates and their rhetoric present in the local political environment, through news coverage, public appearances, town hall meetings, social media, and other forms of representative communication (Gibson & King, 2024; Mayhew, 1974). It also signals that

exclusionary views have achieved democratic recognition. This does not require far-right success to persuade large numbers of residents to adopt new anti-minority attitudes. Rather, it may matter most for individuals who already hold such attitudes, by making them feel more visible, legitimate, and socially permissible to express (Bischof & Wagner, 2019; Valentim, 2021, 2024).

This argument implies that the effect of far-right success should be local, conditional, and uneven across groups. It should be local because electoral victory gives a specific candidate institutional authority over a specific constituency. It should be conditional because far-right victories may carry stronger signals when they occur in periods of broader far-right normalization, when exclusionary rhetoric is more visible and more openly defended in national politics (R. Dancygier, 2023; Valentim, 2024). It should be uneven across groups because far-right rhetoric does not target all minorities with equal intensity. Groups that are more directly targeted in far-right discourse should face the strongest behavioral consequences of far-right victory.

We test this argument in U.S. House of Representatives elections from 2006 to 2022, using a sharp regression discontinuity design around close races involving far-right candidates. House elections provide a useful setting because representatives maintain close links to their districts (Mayhew, 1974), while lacking the direct local policy instruments available to mayors or other executive officeholders. This is central to our design. We are not estimating the effect of simply living in a conservative or far-right-leaning district. Nor are we estimating the effect of far-right control over local policing, municipal spending, or public service provision. Instead, we compare districts where far-right candidates narrowly win to districts where they narrowly lose. In both sets of districts, the far-right was electorally competitive, and residents were exposed to a similar campaign environment. What changes at the threshold is whether the far-right candidate becomes the district’s elected representative. This allows us to focus on the symbolic and communicative consequences of far-right victory: visibility, institutional legitimacy, and representation.

Unlike European party systems, where far-right candidates can be identified based on party labels, the United States context requires a different approach to classify far-right candidates *within* the Republican Party. We use campaign finance scores from the Database on Ideology, Money in Politics, and Elections (DIME) (Bonica, 2024). These scores place candidates on a liberal-conservative ideological scale based on campaign donation patterns and have been extensively validated in previous work (Bonica, 2013). We construct an absolute threshold of far-right ideology using the ideological distribution of members of the Tea Party Caucus and the Freedom Caucus. We then apply this threshold to all Republican candidates, regardless of whether they formally belonged to either caucus. This allows us to identify far-right candidates within the Republican Party across election cycles.

To evaluate the consequences of these victories, we link electoral outcomes to hate crime data from the FBI’s Uniform Crime Reporting Program. Our outcome is the one-year change in reported hate crimes per 100,000 inhabitants before and after each election. This measure captures whether hate crimes increase after far-right electoral victory, while using a full annual window that allows post-election visibility and legitimacy to unfold and accounts for seasonality in hate crime reporting. Because reported hate crimes may be affected by underreporting and police classification practices, we also distinguish violent and non-violent offenses, which could differ in their sensitivity to reporting. We then analyze hate crimes against minorities as a whole and against specific targeted groups.

The results show a selective rather than a general effect of far-right electoral success. We do not find a clear aggregate increase in hate crimes across all minority groups, though estimates become larger and statistically significant when restricting the comparison to genuinely close races. Instead, the strongest and most robust evidence emerges for hate crimes targeting LGBTQ+ individuals. During the Trump years, districts where far-right candidates narrowly win experience an increase of roughly 0.56 reported anti-LGBTQ+ hate crimes per 100,000 inhabitants relative to districts where they narrowly lose. This is a large change given the rarity of reported anti-LGBTQ+ hate crimes. Strikingly, this effect is not confined to the post-2016 period: a consistent increase in anti-LGBTQ+ hate crimes is already detectable across the full period from 2006 to 2022, suggesting that anti-LGBTQ+ rhetoric has been a persistent feature of far-right congressional politics in the last 20 years. This is consistent with the prominence of anti-LGBTQ+ framing in far-right politics more broadly, where LGBTQ+ rights are frequently portrayed as threats to traditional family, gender, and moral order ([Ayoub & Stoeckl, 2024](#); [Haas, Bogatyrev, Abou-Chadi, Klüver, & Stoetzer, 2025](#); [Murib, 2025](#); [Payne & de Souza Santos, 2020](#)).

We also examine whether the aggregate pattern differs across types of hate crimes. The results suggest that the overall increase is concentrated in non-violent offenses, including intimidation, harassment, property crimes, and other less severe incidents. These offenses are more sensitive to reporting practices and police misclassification, so the estimates should be interpreted with caution. At the same time, the pattern is consistent across specifications and aligns with our argument that far-right victories may make lower intensity expressions of hostility more socially permissible.

We provide two pieces of exploratory evidence on mechanisms. First, using Nexis Uni data, we show that far-right candidates who narrowly win and narrowly lose follow similar media trajectories before the election, but diverge sharply afterward. Winners sustain elevated media coverage, while losers fade from the news cycle. This supports the visibility channel. Second, we use district-level averages from the Cooperative Election Studies and Nationscape to examine whether far-right victories are associated with changes in attitudes. These analyses are limited by small sample sizes and timeline constraints, so they should be read as exploratory. Consistent with existing work on far-right politics

and public opinion (Riaz, Bischof, & Wagner, 2024; Valentim, 2024), they do not show broad changes in favorability toward minority groups. Instead, they suggest changes in awareness of discrimination and beliefs related to racial hierarchy. Taken together, these patterns are consistent with the argument that far-right victories are followed by a more visible and legitimate local presence of exclusionary politics.

Our paper makes three contributions. First, it provides causal evidence that far-right victories in legislative elections can trigger behavioral consequences against minorities, even when these victories do not grant direct control over local policy. Existing causal work has examined social media exposure, campaign rallies, and executive office (Feinberg et al., 2022; Müller & Schwarz, 2023; Romarri, 2020). Our setting is different because legislative victory increases visibility and institutional legitimacy without granting the same local policy instruments as executive office. This allows us to identify electoral victory itself as a mechanism linking rhetoric to hate crimes: winning office can extend the reach and legitimacy of exclusionary rhetoric, turning electoral success into a source of behavioral change. Second, the paper shows that these consequences are selective rather than general. Far-right victories do not increase hate crimes against all minorities equally. Instead, the clearest effects are concentrated against LGBTQ+ individuals, a group that is especially salient in contemporary far-right discourse and policy goals (Murib, 2025; Osborn, 2025; Payne & de Souza Santos, 2020).¹ Third, the paper contributes to the study of far-right politics in two-party systems by identifying far-right candidates within a mainstream conservative party. Rather than relying on party labels, or voting patterns, we use a caucus-based ideological benchmark to classify candidates (winners and losers) across electoral cycles. This approach allows us to study the consequences of far-right success in settings where the far-right operates as a faction within a major party rather than as a separate party family. Together, these contributions show that far-right legislative success can reshape the local conditions under which minority rights are exercised, even when it does not directly alter local policy.

2 Theory

2.1 Far-Right Success, Visibility, and Legitimacy

Across liberal democracies, far-right politicians have entered legislative and executive offices in increasing numbers. Their success over the past two decades has generated a rapidly developing literature in political science examining how far-right electoral victories affect mass behavior, public opinion, and democratic norms (Bischof & Wagner, 2019; Bolet & Foos, 2023; Dickson & Hobolt, 2024; Immerzeel & Pickup, 2015; Müller &

¹In the United States, this issue has become increasingly central within the MAGA movement and Trump campaigns. See, for example, Associated Press: [Trump and Vance make anti-transgender attacks central to their campaign's closing argument](#); Government Executive: [What you need to know about Project 2025](#).

Schwarz, 2023; Pulejo, 2023; Romarri, 2020; Valentim, 2021). The starting point in this literature is that far-right actors are deviant cases: they are not only located at the ideological extremes of party competition (Kitschelt & McGann, 1997), but also hold political positions that are largely at odds with established social and political norms (Mudde, 2004; Valentim, 2024).

We use the term 'far-right' as an umbrella term to refer to radical right and extreme right actors who share an exclusionary worldview (Pirro, 2023). This worldview is rooted in nativism and authoritarianism. Nativism is a radical, exclusion-focused version of nationalism that claims a country should be populated only by those considered part of the 'native' group, while authoritarianism emphasizes order, hierarchy, and punishment (Mudde, 2019; Pirro, 2023). In their positions and rhetoric, far-right politicians actively fuel prejudice against ethnic, religious, and sexual minorities by framing these groups as threats to the cultural or ethnic status quo that must be controlled or marginalized (Haas et al., 2025; Mudde, 2007; Pirro, 2023; Turnbull-Dugarte, Grahn, Noonan, & Knudsen, 2025; Valentim, 2024).

The political success of these actors matters because it changes the environment in which these exclusionary ideas circulate. Far-right candidates do not simply express hostile views toward minority groups. When they win office, those views become attached to the authority, visibility, and status of an elected representative. Importantly, this does not require far-right electoral success to generate direct changes in mass attitudes toward minorities. Existing work shows a mixed picture when it comes to immigration attitudes in the wake of far-right victories (Bohman & Hjerm, 2016; Dennison & Kustov, 2023; Dunn & Singh, 2011; Malet, 2024), and attitudes toward minority groups tend to be relatively stable over time (Valentim, 2024). Instead, far-right electoral success may matter most for individuals who already hold anti-minority attitudes, by changing whether those attitudes feel visible, legitimate, or socially permissible to express. We therefore theorize two related pathways that do not require broad attitude change: increased visibility and institutional legitimation.

The first pathway is visibility. Electoral success increases the salience of a candidate's political views and rhetoric (Zaller, 1992). When far-right candidates win legislative office, their exclusionary messaging reaches constituents not only in the national legislature but also within the district, through town hall meetings, local media appearances, community events, and other forms of representative communication (Gibson & King, 2024; Mayhew, 1974). Already existing prejudice toward minorities may remain relatively dormant in the absence of successful exclusionary political elites, but can become activated and expressed once such actors gain electoral prominence (Hopkins, 2021; Valentim, 2024). In the case of the far-right, it is precisely the increased reach of exclusionary rhetoric that can activate latent prejudice and motivate citizens to express or act on those preferences (Newman et al., 2021).

The second pathway is institutional legitimization. Entry into elected office gives institutional recognition to the candidate and, indirectly, to the positions associated with the campaign (Jardina & Piston, 2023). This can lead supporters and sympathizers to believe that their exclusionary views have been democratically endorsed (Bischof & Wagner, 2019; Valentim, 2021). Studies of far-right parliamentary entry show that voters become more open about their exclusionary preferences (Valentim, 2021) and may polarize further in their views (Bischof & Wagner, 2019). District elections with clear winners and losers are especially relevant in this respect: they confer institutional recognition on a specific representative, in a specific constituency, and can signal to potential perpetrators of bias-motivated behavior that the local political environment is more tolerant of their actions.

2.2 Hate Crimes as Political Consequences

Hate crimes are one domain in which the consequences of far-right success may become visible. Hate crimes are generally defined as crimes that involve bias against someone's religious or ethnic background, sexual orientation, gender identity, disability, or other protected identity (N. Hall, 2013; Sheppard, Lawshe, & McDevitt, 2021).² Hate crimes can involve offenders who display high levels of explicit prejudice, but also offenders who exhibit low or subconscious prejudice. In some situations, the link between the offender's prejudice and the crime is clear; in others it is less evident (J. B. Jacobs & Potter, 2000). Although not all hate crimes are motivated by explicit hate, there is a common thread of some degree of prejudice across all of them (N. Hall, 2013). What unites these incidents is that the victim is targeted, at least in part, because of perceived membership in a social group, as defined by the UCR Program in the US (U.S. Department of Justice, 2024).

Hate crimes are a pervasive problem in liberal democracies. A recent report from the European Union Agency for Fundamental Rights (European Union Agency for Fundamental Rights, 2025) highlights that several minority groups in the EU are frequent targets of hateful acts, including hate crimes.³ In the United States, reported hate crimes have doubled between 2015 and 2025 (USAFacts, 2025). Although reported hate crimes capture only a fraction of actual victimization, their persistence and growth make them a particularly important outcome for understanding the social consequences of far-right politics.

The consequences of hate crimes are severe. For direct victims, hate crimes produce greater psychological harm than comparable non-biased crimes. Herek et al. (1999), for example, shows that hate crime victims report higher levels of depression, anger, anxiety, and post-traumatic stress than victims of otherwise similar offenses. The effects also

²See N. Hall (2013) for an extensive overview of different definitions.

³The groups mentioned in the report are Jews, Muslims, Black people, migrants, lesbian, gay, bisexual, transgender, intersex and queer people.

extend beyond the direct victim. Hate crimes are often understood as message crimes (Perry & Alvi, 2012). They communicate threat to a broader community, producing an *in terrorem* effect in which the victimization of one or more individuals intimidates members of the targeted group more broadly (Perry & Alvi, 2012; Weinstein, 1992). Research documents the scope of these downstream effects: hate crimes reduce minority business ownership (Geisler et al., 2019), weaken neighborhood attachment and community participation (Benier, 2017), and can cause minority group members to withdraw from public spaces and civic life (Gabriel et al., 2023; Hobbs & Lajevardi, 2019). Hate crimes therefore matter not only because they harm individuals, but also because they shape minority communities' sense of safety, belonging, and political inclusion.

The study of hate crimes has long emphasized the role of contextual conditions. R. M. Dancygier and Green (2010) distinguish between work on offender psychology, offender types, and contextual determinants such as local economic conditions, community dynamics, and political environments. Our argument fits in this third tradition. Earlier research on racially motivated crime in the United States shows that local community composition, perceived group threat, and neighborhood change are important predictors of anti-minority violence (D. P. Green, Strolovitch, & Wong, 1998; Rieder, 1985; Suttles, 1968). This work is part of a broader literature on prejudice, racial politics, and the political construction of outgroups in Western democracies (Herek, 2004; Kinder & Sears, 1981; Sniderman, Hagendoorn, & Prior, 2004). More recent studies show that hate crimes can also be triggered by events that make out-groups more salient. Riaz et al. (2024), for example, shows that immigrant-attributed crimes in Germany increase hate crimes against refugees, consistent with the activation of pre-existing prejudice after triggering events.

Taken together, this tradition establishes that political rhetoric and the broader political environment shape hate crime patterns. Historical and comparative scholarship confirms that this connection runs deep, documenting how political processes and elite rhetoric targeting racial or ethnic groups have created conditions for bias-motivated violence across a wide range of settings (Mann, 2005; Mansfield & Snyder, 1995; Smångs, 2016). Far-right parties, politicians, and social movements have long been associated with hostility toward minorities and with support for exclusionary or violent forms of politics (R. Dancygier, 2023; Miller-Idriss, 2022; Minkenberg, 2013; Mudde, 2019; Rees, Rees, Hellmann, & Zick, 2019; Wodak, 2018). Recent historical work by Testa and Williams (2026) speaks directly to this logic in the United States. Using a regression discontinuity design based on close presidential vote shares in Southern counties, they find that narrow Democratic electoral losses between 1880 and 1900 increased Black lynchings in the following four years. They document that Southern newspapers amplified narratives of Black criminality in the aftermath of electoral defeats, providing rhetorical justification for subsequent racial violence. These findings establish that electoral outcomes can shape

the local conditions for racial violence through political signaling rather than direct policy change. Our question is whether the same logic holds for the electoral victories of far-right candidates in contemporary legislative races.

Non-causal observational work has documented strong associations between far-right electoral gains and hate crime rates in contemporary democracies (Jäckle & König, 2018; L. Jacobs & van Spanje, 2021; Koopmans, 1996; Suárez Vergne, Cebolla Boado, Lund, & Serrano, 2026; Whittington & Piatkowska, 2025). This body of work establishes that the political success of the far-right and the incidence of hate crimes tend to move together, but it cannot speak to the causal direction of this relationship or isolate the specific mechanisms through which electoral success generates behavioral change.

A growing body of causal work has begun to address these limitations. Müller and Schwarz (2023) exploit variation in Twitter penetration across U.S. counties as an instrument to show that greater exposure to anti-Muslim rhetoric following the 2016 presidential primaries led to increases in anti-Muslim hate crimes, attributing the effect to the reach of exclusionary rhetoric rather than to any electoral outcome as such. Feinberg et al. (2022) find that counties hosting Trump campaign rallies experienced more hate crime and bias incidents than comparable counties, consistent with the idea that direct exposure to exclusionary rhetoric increases bias-motivated behavior. In Europe, Romarri (2020) uses a close-election regression discontinuity design to show that the election of far-right mayors in Italian municipalities increases the probability of hate crimes, providing direct causal evidence that far-right electoral success, rather than rhetorical exposure alone, can generate increases in bias-motivated violence.

At the same time, these studies leave open an important question. With the exception of Romarri (2020), existing causal work focuses on campaign events or social media exposure that precede entry into office, making it difficult to isolate the effect of electoral success from broader rhetorical exposure (Feinberg et al., 2022; Müller & Schwarz, 2023). Studies of executive officeholders bundle together the symbolic effect of electoral victory and the policy effects that follow (Romarri, 2020): far-right mayors in Italy also implement major cuts in spending for immigrants and lead to worsened economic performance (Bellodi, Morelli, & Vannoni, 2024; Pulejo, 2025), making it difficult to attribute hate crime increases to the symbolic victory alone. Our setting is different. By focusing on legislative elections, we study a context in which electoral victory increases visibility and institutional legitimacy, but does not directly grant the same local policy instruments as executive office. This allows us to focus more directly on the symbolic and communicative effects of far-right electoral success.

2.3 From Far-Right Victory to Hate Crimes

Building on this literature, we argue that the electoral victory of far-right candidates can increase hate crimes by changing the local political environment in which anti-minority attitudes are expressed. The central claim is not that far-right victories create prejudice where none existed before. Rather, the argument is that victory increases the visibility and legitimacy of exclusionary rhetoric, making already existing anti-minority attitudes more socially permissible to express. Because hate crimes are rare events committed by a small subset of individuals, even modest changes in perceived permission or tolerance can translate into observable changes in bias-motivated behavior (R. Dancygier, 2023; Riaz et al., 2024).

This logic is especially relevant in legislative elections. Far-right legislators do not control local policing, municipal budgets, or service provision in the way that mayors or other executive officeholders do. Their victory therefore does not primarily operate through direct local policy change. Instead, the relevant change is symbolic and communicative. A far-right candidate who wins becomes the district’s representative, remains visible in the local information environment, and signals that exclusionary views have achieved institutional recognition. In this sense, a far-right victory may lower the perceived social costs of acting on bias for those already predisposed to hostility toward minority groups.

This leads to our first hypothesis:

H1 The election of a far-right candidate leads to an increase in district-level hate crime rates.

The argument also implies that the effect of far-right victories should depend on the broader political environment. Far-right normalization is a multi-stage process in which exclusionary ideas become increasingly visible, legitimate, and integrated into mainstream politics (Valentim, 2021, 2024). A local far-right victory should carry a stronger signal when it occurs in a national environment where far-right rhetoric is already more prominent and more widely defended. In the United States, the period after 2016 provides such a context. The rise of Donald Trump and the MAGA movement increased the visibility of far-right rhetoric within the Republican Party and national politics (Biebricher, 2024; Gest, Reny, & Mayer, 2018). During this period, local far-right victories may therefore resonate more strongly than in earlier years.

We therefore hypothesize that:

H1a The effect of far-right electoral victory on hate crimes is stronger during periods of broader far-right normalization.

Finally, the same logic suggests that hate crime increases should not be evenly distributed across all minority groups. Far-right rhetoric does not target all groups with equal

intensity. Ethnic and religious minorities are often portrayed as outsiders to the national community, while LGBTQ+ individuals are frequently framed as threats to traditional family, gender, and moral order (Haas et al., 2025; Mudde, 2019). If far-right victories increase the visibility and legitimacy of exclusionary rhetoric, then the consequences should be strongest for groups that are most directly targeted in that rhetoric.

We therefore hypothesize that:

H1b The effect of far-right electoral victory on hate crimes is stronger for minority groups that are more directly targeted in far-right discourse.

3 Research Design

3.1 Case Selection: US House Races (2006-2022)

We focus on U.S. House of Representatives elections from 2006 to 2022 to study whether the electoral success of far-right candidates increases hate crimes. House elections are a suitable case for several reasons. First, House elections create clear local winners and losers. When a far-right candidate wins a congressional district, the candidate becomes the official representative of that district and gains institutional visibility as its elected member of Congress. This feature is central to our argument, since we are interested in the local consequences of far-right electoral success rather than in the effect of simply living in a conservative or far-right-leaning area.

Second, House members maintain close ties with their local constituencies (Mayhew, 1974). Constituents encounter their representative’s messaging not only in Washington but also within the district, for example through town hall meetings, local news coverage, and community events (Gibson & King, 2024), making the local consequences of electoral victory directly observable at the district level. If far-right candidates win, their exclusionary rhetoric and public profile may remain more present in the district after the election than if they narrowly lose.

Third, the period we study coincides with a notable supply of far-right candidates within the Republican Party (Biebricher, 2024). This trend began with the backlash to Barack Obama’s 2008 election and the emergence of the Tea Party movement (Arceneaux & Nicholson, 2012; A. B. Hall, 2015), and accelerated with Donald Trump’s 2016 victory and the growth of the MAGA movement (Gest et al., 2018). The institutional consolidation of this faction is reflected in the creation of the Tea Party Caucus (2010) and later the Freedom Caucus (2015) (Costa & Kane, 2015). In all, House of Representatives elections offer a case in which far-right candidates regularly compete, where electoral victories generate clear local representation, and where the consequences of far-right success can be observed locally.

3.2 Data and Variables

3.2.1 Candidate Ideology

We combine two sources to construct our data set of political candidates. First, we rely on the ‘Candidates in American General Elections’ data set by [Cha, Kuriwaki, and Snyder \(2021\)](#), which provides a comprehensive list of candidates who ran in House elections between 2006 and 2022. The main advantage of this data set is that, in contrast to other similar efforts, it provides standardized candidate names across years, candidate incumbency status, and other relevant variables combined into a single file. Importantly, it also contains variables essential to our empirical strategy, including electoral outcomes and the margin of victory.

We merge this data with candidate information from the Database on Ideology, Money in Politics, and Elections (DIME) ([Bonica, 2024](#)). In addition to candidate characteristics, this database includes data on campaign finances and contribution records, and contains multiple ideology estimates for candidates in House elections. Because DIME’s ideological scaling relies primarily on campaign contributions, some candidates were not included because they did not raise funds from the required number of contributors to be included in the scaling.⁴ We discuss the characteristics of missing candidates in subsection A.

We construct our main independent variable, candidate ideology, using campaign finance scores (CF scores) from the DIME database. These scores position each candidate along a liberal–conservative spectrum based on campaign contribution patterns. We use these scores to identify far-right candidates.⁵ Identifying far-right candidates is a challenge both empirically and conceptually. The conceptual challenge lies in the blurred distinction between radical right and extreme right actors ([Pirro, 2023](#)). The empirical challenge is deciding where to draw the line. Previous work in American politics has labelled candidates as extremist when they are more ideologically distant from the median primary opponent ([A. B. Hall, 2015](#); [Meisels, 2025](#)). However, this method identifies relative extremism and does not capture far-right ideology in absolute terms. Alternatively, one could use percentile-based cutoffs (e.g., top 10% or 25%), but these are arbitrary and lack theoretical justification.

To overcome these challenges, we draw on congressional caucuses as a benchmark of far-right ideology. Congressional caucuses are voluntarily formed associations of legislators with shared ideological or policy interests ([Hammond, 2001](#)), and are often associated with ideological outliers ([Ainsworth & Akins, 1997](#)). We focus on two caucuses that have historically been associated with the far-right: the Tea Party Caucus and its successor,

⁴For more information about the contents of the DIME files and the inclusion-exclusion rules applied in the ideological scaling (see also [Bonica \(2013\)](#)) we refer to the DIME [documentation](#).

⁵Dynamic CF scores are time-variant.

the Freedom Caucus (Arceneaux & Nicholson, 2012; M. Green, 2019). A full list of caucus members and their CF scores appears in Appendix B⁶.

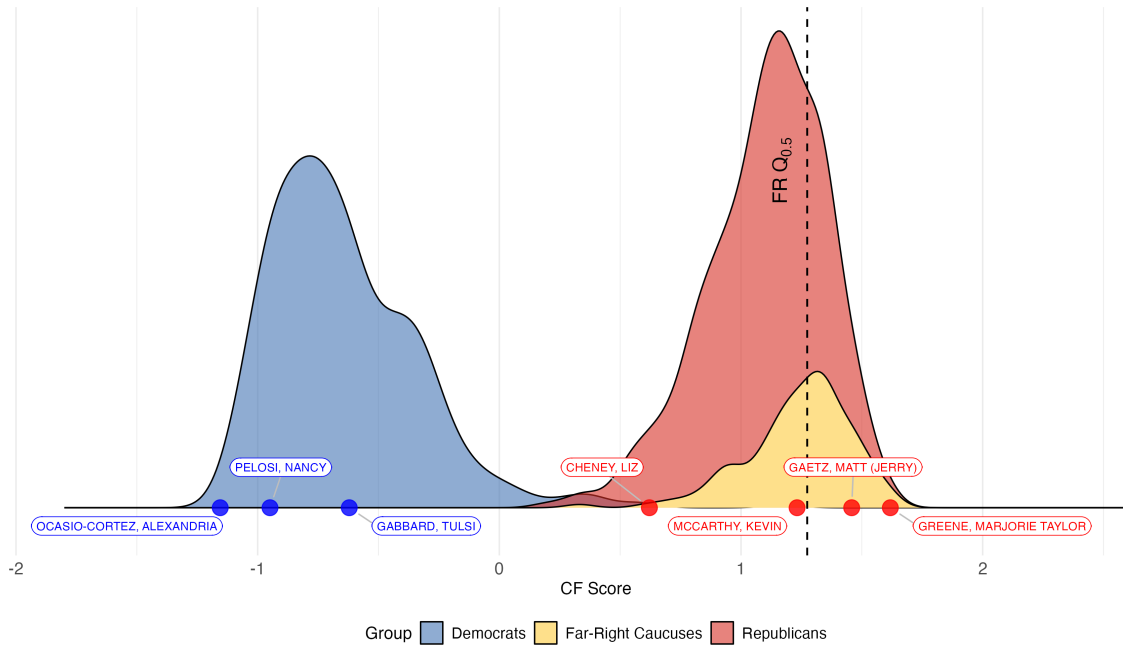
To create a threshold for far-right identification, we calculate the distribution of CF scores for all caucus members combined and use the median value as our cut-off:

$$FR_{\text{med}} = \text{median}(\{x_i \in \text{Far} - \text{RightCaucuses}\}) = 1.274$$

The caucuses serve here as an empirical anchor to derive a theoretically grounded threshold. We then apply this threshold to all Republican candidates in our sample, regardless of caucus membership, to obtain a consistent classification of far-right candidates across all election years.

Figure 1 visualizes the distribution of CF scores of House candidates. It includes all distinct House candidates who have won at least one election between 2006 and 2022. The plot shows a clear separation between Democrats and Republicans, with Tea Party and Freedom Caucus members concentrated at the extreme conservative end. The vertical dashed line marks the 1.274 threshold.

Figure 1: Distribution of CF Scores



Notes: The CF scores shown here are time-invariant. Distribution of Republicans and Democrats includes distinct candidates that have won at least one House election between 2006-2022. Densities are relatively proportional to sample sizes.

We argue that this is a conservative threshold in the sense that we may not capture

⁶Both caucuses do not keep official lists of their members in Congress. Appendix B also gives details of our sources.

all far-right candidates, especially some borderline cases. One example, plotted in Figure 1, is Kevin McCarthy who, despite close association with Donald Trump and Marjorie Taylor Greene, falls below our cut-off. While the CF scores of caucus members are approximately normally distributed, the left tail suggests some ideological heterogeneity within the group. Applying this threshold to our full candidate sample yields a consistent definition of far-right candidates across all election years.

We used the time-invariant CF score to compute our far-right cut-off, which means that the same cut-off is used across all years. One potential concern is that the ideological distribution of candidates may shift over time, which could affect how conservative or inclusive our threshold appears in different cycles. We address this issue in Appendix D, where we show that our selected cutoff remains substantively relevant across time and aligns with candidates commonly recognized as far-right.

3.2.2 Far-Right Races

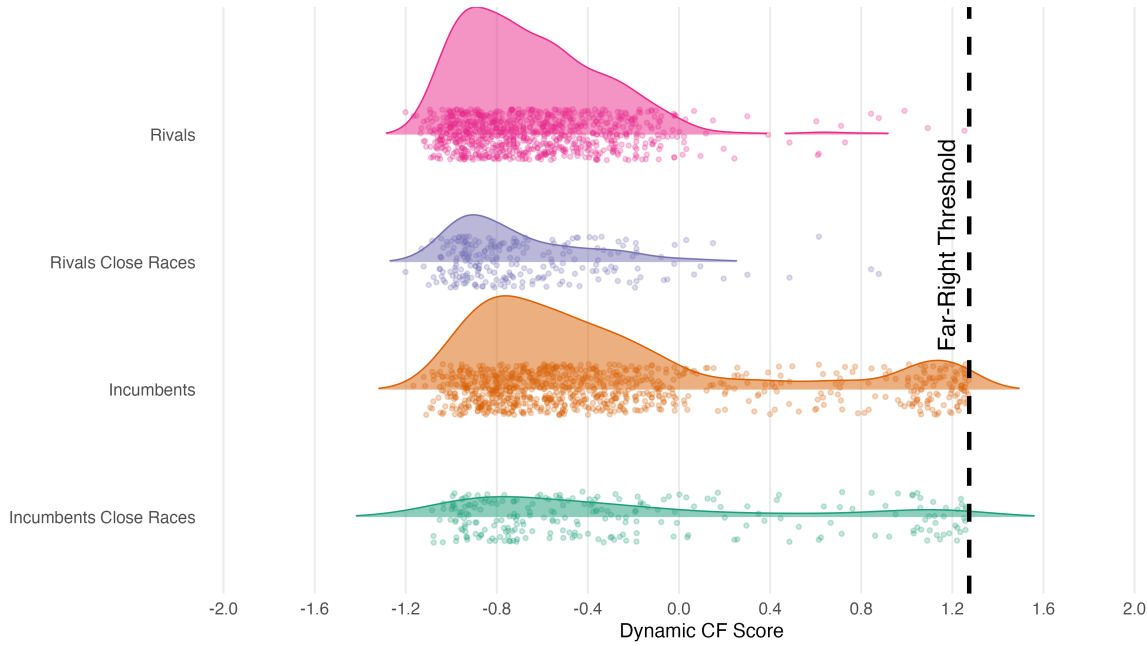
We define far-right races using the aforementioned threshold. Across the 2006–2022 period, we identify 1,119 congressional districts where at least one far-right candidate ran for office. Of those, 863 are races where the far-right candidate contested an election without an incumbent from the far-right, providing plausible treatment-control comparisons. Narrowing the sample to the Trump years, from 2016 onward, we observe 482 races with far-right candidates running in races without a far-right incumbent. Descriptive statistics for treatment and control groups are reported in Appendix C1.

We also report the ideological scores of the rivals who far-right candidates ran against, and of the previous incumbent for those districts and years. We do this for the 863 races that are at the core of our analysis⁷. The scores are summarized in Figure 2. The overall pattern is that most rivals and previous incumbents are moderate Democrats, both overall and in close races. However, for incumbents there is also a small bump on the right. Substantively, this means that we are analyzing races with significant *horizontal* ideological conflict, between the far-right candidate and the rival candidate, and, in many cases, *vertical* ideological conflict as well, between the far-right candidate at time t and the incumbent at $t - 1$.⁸ Our main analyses do not condition on the ideology of the rival candidate. In our design, the relevant comparison is between races in which a far-right candidate narrowly wins and races in which a far-right candidate narrowly loses. Conditioning on the rival’s ideology would therefore change the estimand, since the type of opponent is part of the electoral contest through which far-right success is realized.

⁷Namely, races where the far-right candidate contested an election without an incumbent from the far-right

⁸These categories are not mutually exclusive, since incumbents at $t - 1$ are often rivals at t .

Figure 2: Dynamic CF Scores for Rivals and Previous Incumbents



Notes: NA values were excluded from the analysis. Close races are defined as races where the margin of victory falls within 15 points below or above the threshold.

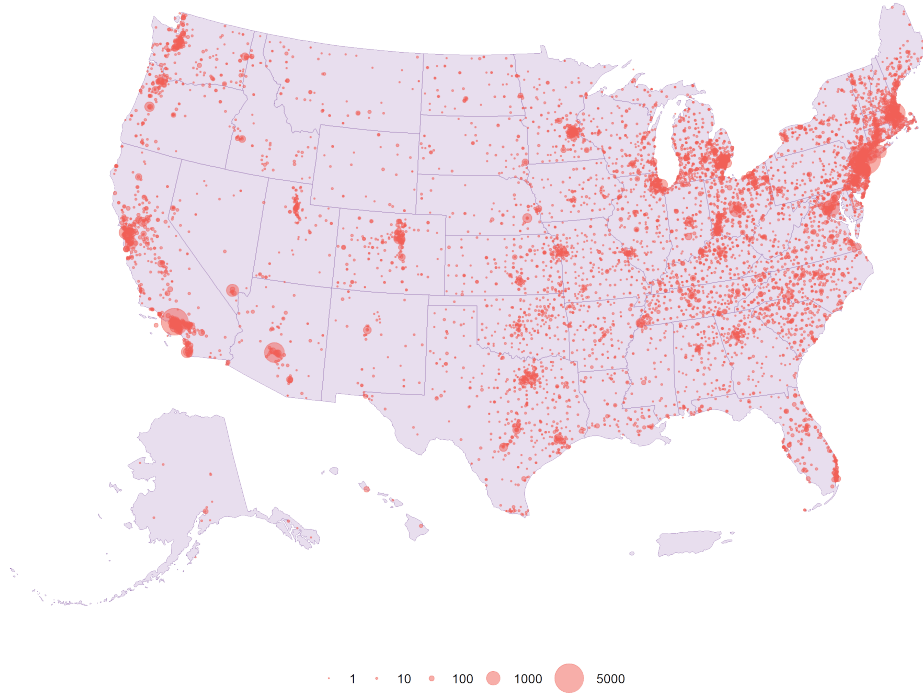
3.2.3 Outcome Variable: Hate Crime Rates

To measure hate crime occurrences, we rely on hate crime data reported through the FBI’s Uniform Crime Reporting (UCR) Program ([U.S. Department of Justice, 2024](#)). These incident-based data include detailed information on location, victim and offender characteristics, and the underlying bias motivation.⁹ We use the Hate Crime Statistics dataset, which tracks crimes “motivated in whole, or in part, by an offender’s bias against the victim’s perceived race, gender, gender identity, religion, disability, sexual orientation, or ethnicity” ([U.S. Department of Justice, 2024](#)). Figure 3 shows the geographical spread of hate crimes reported through the UCR program between 2005 and 2022.

These data present measurement challenges. We need to assume that hate crimes are severely underreported. In a study by [Pezzella, Fetzer, and Keller \(2019\)](#), the authors compare the incidents reported through UCR between 2004 and 2012 to those of the National Crime Victimization Survey (NCVS). While only 8,770 hate crimes were reported through the UCR, the NCVS reports an average of 269,000 victimizations. Reported incidents through UCR thus represent about 3% of the NCVS average. Underreporting stems from victims not coming forward and from police misclassification ([Pezzella et al.,](#)

⁹The UCR dataset does not provide the exact crime location. Instead, incidents are linked to the reporting law enforcement agency, which can be a municipal police department, county sheriff office, campus police office, or similar entity. We assign each hate crime to the ZIP code of the reporting agency. In the small number of cases where a ZIP code spans multiple congressional districts, we allocate the incident to all corresponding districts.

Figure 3: Map of Reported hate crimes in the United States (2005-2022)



Notes: Author's georeferencing of Hate Crimes reported to the UCR Program. We assign each hate crime to the ZIP code of the reporting agency. All hate crime incidents are reported and georeferenced in this map.

2019). Because the likelihood of reporting varies across offense types, we complement the main analysis by separating hate crimes into violent and nonviolent offenses. Violent offenses include incidents involving physical force or the threat of serious physical harm, such as aggravated assault, simple assault, homicide, robbery, and sexual assault. Non-violent offenses include intimidation, harassment, property damage, vandalism, and other minor offenses. A full list of UCR offense categories and their classification into these two groups is provided in Appendix C4. While we cannot fully correct for underreporting, this disaggregation allows us to assess whether the patterns we observe are concentrated in more severe offenses or in offenses that may be more sensitive to reporting practices. Our estimates should therefore be interpreted as effects on reported hate crimes and, given underreporting, as lower bounds of the underlying incidence of hate crimes.

Our main dependent variable is the change in hate crime rates in each congressional district around a given election. We first calculate the occurrence of hate crimes H as a fraction of the local population P . To do so, we use U.S. Census population data from 2006 to 2024 to calculate the total population of voting districts d for each election year y . We then calculate hate crime rates per 100,000 inhabitants using the following formula:

$$\text{Hate Crime Rate}_{d,y} = \left(\frac{H_{d,y}}{P_{d,y}} \right) \times 100,000$$

We calculate this rate for two periods around each election. The pre-election period covers the year before the election. The post-election period covers the year after the election. Because we analyze close races and some election results may not be known immediately, we exclude the first week after election day from the post-election period. Our outcome is then defined as the difference between the post-election hate crime rate and the pre-election hate crime rate:

$$\Delta \text{Hate Crime Rate}_{d,y} = \text{Hate Crime Rate}_{d,y}^{post} - \text{Hate Crime Rate}_{d,y}^{pre}$$

Our outcome is therefore the one year change in reported hate crime rates around the election. By using a full annual window, the measure accounts for seasonality in hate crime reporting and gives time for the potential consequences of far-right electoral success to materialize.

We pool together hate crimes against all minority groups. This includes religious minorities, such as Jewish and Muslim communities; racial and ethnic minorities, such as Black Americans, Latinos, and Arabs; LGBTQ+ individuals; and people with disabilities. We also report separate analyses for specific minority groups, since far-right rhetoric may not target all minorities in the same way. In Appendix C, we report descriptive statistics on hate crime rates for different minority groups. We also show trends in hate crimes among these groups and the composition of different hate crime types over time.

3.2.4 District and Candidate Characteristics

We include district-level socio-demographic and economic variables in our analyses that we draw from the American Community Survey (ACS). The ACS covers a broad set of topics including education, income, employment, housing, and population composition, which allows us to capture year-specific variation in population characteristics at the congressional district level. We use the following district-level socio-demographic variables: the percentage of minority residents, the percentage of Black residents, and the percentage of foreign-born residents. In addition, we include several socioeconomic indicators: median household income, the poverty rate, and the unemployment rate. These variables are used to describe baseline district characteristics and to adjust for potential selection into treatment in our empirical models. Summary statistics for all district-level control variables are presented in Appendix C. We also collect candidate characteristics such as age, gender, and ethnicity. We do not use these variables for covariate adjustment. Rather, we use candidate characteristics in one of our continuity tests, as described in more detail in Section 4.1.

4 Identification Strategy

Our identification strategy employs a sharp regression discontinuity design (RDD) to exploit the discontinuity created by closely contested elections between far-right and non-far-right candidates. We focus on elections where a far-right candidate is one of the top two contenders. We compute the vote margin as the difference between the far-right candidate’s vote share and that of their main competitor, and use this margin as the running variable. The cut-off is set to zero, so that we compare districts where a far-right candidate narrowly wins to districts where a far-right candidate narrowly loses. We estimate the effect of electing a far-right candidate on changes in district-level hate crime rates around the election. The key assumption behind this method is that districts where a far-right candidate narrowly wins are, in expectation, similar to those where a far-right candidate barely loses. Under this assumption, any discontinuous change in the outcome at the threshold can be interpreted as the local average treatment effect (LATE) of electing a far-right candidate.

4.1 PCRD Design and Estimand

Because our main interest is in *far-right* candidates, we are adopting a politician characteristic regression discontinuity (PCRD) design (Marshall, 2024). The key implication of this design is that we cannot fully isolate the causal effect of far-right *ideology*. In close races, far-right candidates who narrowly win may differ from their opponents not only ideologically, but also in terms of other observed and unobserved characteristics Z that may affect their competitiveness. As a result, our estimand captures the causal effect of the electoral victory of a far-right candidate, not the pure effect of far-right ideology itself.¹⁰ This distinction is important because far-right candidates who are competitive in close races may do so in part because of a bundle of compensating differentials Z that correlate with both ideology and electoral success.¹¹

The next question is how to deal with this bundle of characteristics. We choose not to adjust for candidate characteristics in our PCRD. Our main reason for not doing this is that we cannot adjust for all compensating differentials. Selecting some candidate characteristics but not others may create a subsequent problem in which we need to adjust for additional compensating differentials (Marshall, 2024). Instead, we show descriptive data and conduct a continuity test for candidate characteristics, which can be found in Appendix H3. In keeping with Marshall (2024), we use this information primarily as an aid for interpreting our results. These tests also allow us to assess whether observable candidate traits, such as gender, race, and age, change discontinuously at the threshold.

¹⁰We follow the wording of Buccianeri (2018) in defining our estimand.

¹¹In other words, our estimand takes on the following form: $\tau_X +$ (bias from compensating differentials Z)

4.2 District Characteristics and McCrary Density Test

We also take heed of potential concerns about covariate imbalance around the threshold of close-election RD designs (Caughey & Sekhon, 2011; De la Cuesta & Imai, 2016; Eggers, Fowler, Hainmueller, Hall, & Snyder Jr, 2015). We address these concerns in two tests. First, we conduct a continuity test for the district characteristics that we derived from the American Community Survey. None of the district characteristics show discontinuity around the threshold. The results can be found in Appendix H2. Second, we test for potential sorting around the threshold using the McCrary density test (McCrary, 2008). The results, reported in Appendix H1, do not show evidence of sorting around the threshold or manipulation of the running variable.

4.3 Empirical Specification

To estimate the treatment effect, we use the following specification:

$$Y_{d,t} = \alpha + \beta_1 FR_{d,t} + \beta_2 M_{d,t} + \beta_3 M_{d,t} FR_{d,t} + \zeta X_{d,t-1} + \gamma_t + \epsilon_{d,t} \quad (5)$$

Where $Y_{d,t}$ represents the change in reported hate crimes per 100,000 inhabitants in district d around election year t . $FR_{d,t}$ is a dummy for treated districts equal to 1 if a far-right candidate was elected in the most recent election year t . $M_{d,t}$ is the margin of victory, our running variable, in district d in most recent election year t . $X_{d,t-1}$ contains pre-treatment district-level controls such as employment rate, the share of minority residents, and median income. Finally, $\epsilon_{d,t}$ is an error term and γ_t are year fixed effects. The coefficient β_1 captures the discontinuous jump in the outcome at the threshold and is our main parameter of interest.

We implement this estimation using the robust local polynomial estimator developed by Calonico, Cattaneo, and Titiunik (2014), which selects optimal bandwidths to minimize mean squared error. Because these bandwidths are selected separately for each outcome, we also estimate all main models using a fixed bandwidth of 0.10 around the threshold. This robustness check restricts the analysis to closer elections and makes the estimates more directly comparable across outcomes. As an additional robustness check, we estimate models using second order polynomials for the RD running variable. Results for these models are shown in Appendix I and closely match our main estimates.

5 Empirical Results

We now turn to our results. Table 1 presents our main RD estimates for hate crimes against minorities. We report estimates without and with baseline covariates. All models include year fixed effects and are estimated using local linear regressions with robust

bias-corrected confidence intervals (Calonico et al., 2014). The outcome is the change in reported hate crimes per 100,000 inhabitants between the year before and the year after the election. Figure 4 presents the corresponding RD plots.

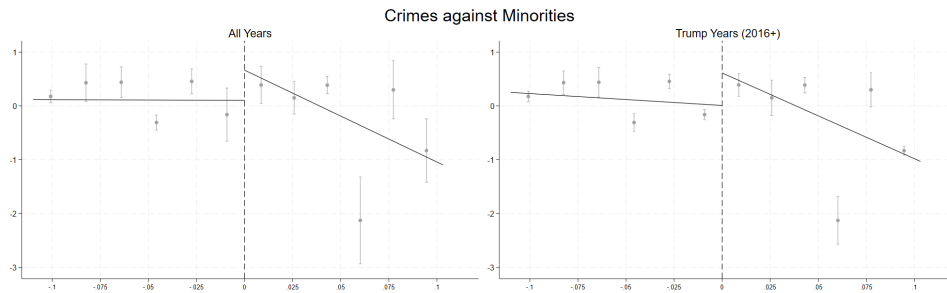
Table 1: Election of Far-right Candidates on Hate Crimes against Minorities

	Minority Hate Crimes			
	All Years		Trump Years	
	(1)	(2)	(3)	(4)
RD Estimate	0.223 (0.332)	0.323 (0.339)	0.225 (0.360)	0.280 (0.357)
Controls	No	Yes	No	Yes
Bandwidth	0.141	0.138	0.197	0.201
N (Left)	157	153	133	137
N (Right)	74	74	54	54
Order_polyn.	1	1	1	1

Notes: The Table reports RD estimates with robust standard errors in parentheses. Outcome variables are changes in crimes against minorities per 100,000 population in the year following the election. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).

Signif.: ***: 0.01, **: 0.05, *: 0.1.

Figure 4: Regression Discontinuity Plots for Crimes Against Minorities



Notes: Each point represents the bin sample average of hate crimes reported for the margin of victory, using the mimicking variance evenly-spaced method for bin selection. The straight line is a first-order polynomial in Margin of Victory fitted separately on each side of the margin of victory threshold at zero. 90% confidence intervals.

We hypothesized that the election of a far-right candidate would lead to subsequent increases in hate crimes in the districts they represent. Columns 1 and 2 in Table 1 pool together all election years from 2006 to 2022. We do not detect a clear effect of electing a far-right candidate on changes in district-level hate crime rates. Columns 3 and 4 restrict the sample to elections from 2016 onward, a period that coincides with a broader process of far-right normalization in national politics driven in part by the growth of the MAGA movement (Biebricher, 2024). Even in this subset, the estimates for hate crimes against minorities as a whole are positive but not estimated with enough precision to support a

clear aggregate effect. The RD plots in Figure 4 are consistent with this cautious reading. They suggest a small upward movement at the cutoff, especially in the Trump years, but the graphical evidence does not point to a strong discontinuity when all minority groups are pooled together.

These aggregate null results should be interpreted with caution for two reasons. First, hate crimes are rare events, and the minimum detectable effect analysis in Appendix I2 shows that our design has power to detect only relatively large effects.¹² Second, the optimal bandwidth selection procedure can in some cases extend the estimation window beyond the closest races. When we restrict the analysis to a fixed bandwidth of 0.10, limiting the sample to elections decided within ten percentage points, the estimates for hate crimes against minorities during the Trump years become larger and statistically significant (Appendix I2). This pattern is consistent with the idea that the effect of far-right victories on general hate crime rates is concentrated in genuinely close elections, where the outcome was uncertain and the electoral shock carries more local political signal. At wider bandwidths, districts where the far-right candidate won comfortably are included in the comparison, and in those districts the far-right victory was more anticipated and may therefore generate a weaker change in the local political environment.

We next examine whether these aggregate patterns differ by type of hate crime. This distinction is important because reported hate crimes vary in their sensitivity to reporting practices. Violent crimes are less likely to go unreported or misclassified by law enforcement, while non-violent crimes include intimidation, harassment, property crimes, and other less severe incidents that may be more sensitive to reporting and misclassification. Table 2 reports these results.

The estimates suggest that the overall pattern is concentrated in non-violent offenses. For violent hate crimes, the estimates remain close to zero across the full period and the Trump years. By contrast, non-violent hate crimes increase after far-right victories, especially during the Trump years. Substantively, this means that the aggregate pattern is not driven by the most severe forms of physical violence, but by offenses such as intimidation, harassment, property crimes, and other less severe incidents. In the Trump years, treated districts exhibit larger increases in non-violent hate crimes, with estimates ranging from roughly 0.6 to 0.8 incidents per 100,000 inhabitants depending on the specification. These offenses are more sensitive to variation in reporting practices, so these results warrant some caution¹³. That said, the direction and magnitude of the estimates

¹²The minimum detectable effect is the smallest true effect that the design would detect with a given probability under repeated sampling, not a threshold that determines whether a realized estimate can be statistically different from zero (see Bloom, 1995; Duflo, Glennerster, & Kremer, 2007). Estimates below the MDE can still be informative when the realized sampling variance is lower than expected. The main limitation arises when imprecise estimates are smaller than the MDE, since in those cases the design cannot rule out substantively meaningful effects.

¹³Far-right electoral victory may itself affect reporting behavior in opposing directions. Minority victims may become less willing to report incidents out of fear that the institutional environment is now

Table 2: Election of Far-right Candidates on Hate Crimes against Minorities: Violent vs. Non-Violent

	All Years				Trump Years			
	Violent		Non-Violent		Violent		Non-Violent	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.015 (0.109)	0.008 (0.109)	0.430 (0.296)	0.584* (0.311)	-0.070 (0.150)	-0.037 (0.168)	0.607* (0.350)	0.782** (0.371)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.233	0.216	0.128	0.121	0.143	0.129	0.139	0.129
N (Left)	263	243	139	130	96	87	95	87
N (Right)	101	95	71	65	45	41	44	41
Order_polyn.	1	1	1	1	1	1	1	1

Notes: The Table reports RD estimates with robust standard errors in parentheses. Outcome variables are changes in crimes against minorities per 100,000 population in the year following the election. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).

Signif.: ***: 0.01, **: 0.05, *: 0.1.

are consistent across specifications and visible in the RD plots (Figure I.10 in Appendix I1).

We then examine whether the effects vary across minority groups. This is central to our argument because far-right rhetoric does not target all groups in the same way. If the consequences of far-right victories operate through visibility and local legitimization of exclusionary messages, we should not necessarily expect all minority groups to be affected in the same way. Table 3 reports results for crimes against Jews, Muslims, Black Americans, Latinos, and LGBTQ+ people.

The results show that the burden of hate crimes is not evenly distributed across minority groups. We do not observe a clear increase in reported hate crimes against Jews, Muslims, Black Americans, or Latinos. By contrast, hate crimes against LGBTQ+ people display a consistent and substantively large increase after far-right victories. In the full period, districts where far-right candidates narrowly win experience a yearly increase of around 0.38 reported anti-LGBTQ+ hate crimes per 100,000 inhabitants relative to districts where far-right candidates narrowly lose. During the Trump years, this estimate rises to around 0.56 reported crimes per 100,000 inhabitants. These are large changes given the rarity of reported anti-LGBTQ+ hate crimes, and they align with the prominence of anti-LGBTQ+ rhetoric across different waves of the congressional far-right, from the Tea Party to the Freedom Caucus and the MAGA movement ([Arceneaux & Nicholson, 2012](#); [Osborn, 2025](#)). More broadly, they are consistent with our theoretical argument that

less sympathetic to their concerns, which would downward-bias the estimates. Conversely, victims or community organizations may be more motivated to report precisely in order to document what they perceive as emboldening of anti-minority behavior, which would upward-bias them. We cannot disentangle these forces from genuine changes in crime incidence, which is why we treat the non-violent estimates with particular caution.

Table 3: Election of Far-right Candidates on Hate Crimes against Targeted Minorities

Panel A: All years (2006 - 2022)

	Jews		Muslims		Blacks		Latino		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	-0.008 (0.064)	-0.001 (0.066)	0.002 (0.024)	-0.000 (0.025)	-0.170 (0.198)	-0.140 (0.207)	-0.076 (0.055)	-0.086 (0.053)	0.402*** (0.129)	0.378*** (0.127)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.273	0.254	0.165	0.163	0.126	0.123	0.152	0.113	0.122	0.130
N (Left)	328	294	185	181	136	135	167	122	135	141
N (Right)	111	109	81	81	71	71	78	63	69	71
Order_polyn.	1	1	1	1	1	1	1	1	1	1

Panel B: Trump years (2016 - 2022)

	Jews		Muslims		Blacks		Latino		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	-0.019 (0.109)	-0.036 (0.111)	-0.015 (0.036)	-0.019 (0.036)	-0.173 (0.180)	-0.203 (0.178)	0.002 (0.065)	0.020 (0.061)	0.647*** (0.219)	0.555*** (0.206)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.217	0.214	0.149	0.148	0.162	0.166	0.210	0.202	0.113	0.127
N (Left)	151	149	98	98	108	112	146	140	73	83
N (Right)	57	56	46	46	48	48	56	54	35	41
Order_polyn.	1	1	1	1	1	1	1	1	1	1

Notes: Each panel report RD estimates with robust standard errors in parentheses. Outcome variables are changes in crimes against minorities per 100,000 population in the year following the election. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

far-right electoral success should have stronger consequences for groups that are more directly targeted in far-right discourse.

Figure I.9 in Appendix I1 shows the RD plots for each minority group. For LGBTQ+ hate crimes, the discontinuity at the threshold is visible and consistent across both the full period and the Trump years. The slopes on either side of the threshold are also notable. Changes in reported LGBTQ+ hate crimes are larger in districts where the far-right is less electorally dominant, and smaller where the far-right wins by wider margins. This pattern may reflect broader differences in how LGBTQ+ visibility, local political conflict, and reporting behavior evolve across districts with different electoral profiles. In places where far-right candidates are already electorally dominant, the local political environment may leave less scope for a sharp post-election change. In places where far-right candidates lose by wider margins, changes in LGBTQ+ visibility, community mobilization, or reporting may generate increases in recorded incidents independently of the election result. We cannot adjudicate between these explanations with our data. The pattern therefore reinforces the local interpretation of our estimates, as effects concentrated around the threshold rather than extrapolated to districts where the electoral outcome was not in doubt.

We conduct several robustness checks to assess whether these patterns depend on specific modeling choices. First, the appendix reports all main models using a fixed bandwidth of 0.10 around the threshold (Appendix I2). As discussed above, this specification restricts the analysis to closer elections and makes the estimates more directly comparable across outcomes. The anti-LGBTQ+ results remain strong in this restricted sample, and the aggregate estimates for hate crimes against minorities during the Trump years also become statistically significant. Second, we estimate models using second-order polynomials in the running variable (Appendix I2). The results are substantively similar to the local linear estimates, especially for anti-LGBTQ+ hate crimes.

Finally, we examine whether the results depend on how we define far-right candidates. Our main specification uses the median CF score among Tea Party and Freedom Caucus members as the threshold for identifying far-right candidates. In Appendix I2, we re-estimate the main models using alternative thresholds¹⁴, including a Freedom Caucus benchmark and a threshold based on the 75th percentile of Republican CF scores. The anti-LGBTQ+ results remain substantively unchanged. This suggests that the main pattern is not driven by the specific cut-off used to classify candidates as far-right.

Taken together, these results point to a selective rather than uniform effect of far-right electoral success on hate crimes. We do not find a clear aggregate increase across all minority groups, but we do find a consistent, large, and robust increase in hate crimes against LGBTQ+ individuals. This pattern holds across the full period, across specifications, and across alternative definitions of far-right candidacy. It is also concentrated in the type of offenses most plausibly connected to the exclusionary rhetoric that far-right politicians direct at this group.

6 Mechanisms

The results in the previous section show that far-right electoral success increases hate crimes against LGBTQ+ individuals, while the aggregate effects across all minority groups are more limited. Our regression discontinuity design supports a causal interpretation of this association, but it does not allow us to directly identify the mechanism through which this effect operates. In this section, we present two complementary pieces of exploratory evidence consistent with our theoretical argument. We treat these as suggestive rather than definitive tests, and we discuss their limitations directly.

Our theoretical argument holds that when far-right candidates win, their rhetoric and public profile remain more present in the local political environment than when they

¹⁴Changing the far-right threshold does more than change the ideological score used to classify candidates. It also changes which candidates are coded as far-right, which races enter the analysis, and therefore which districts are used to estimate the RD. Similar estimates across these alternative definitions therefore provide a demanding robustness check.

narrowly lose, and that this shift may lower the social costs of bias-motivated behavior for a subset of residents. We explore two observable implications of this argument. First, we examine whether winning far-right candidates do in fact receive more sustained local media coverage after the election than losing candidates. Second, we examine whether far-right electoral victories are associated with shifts in community-level attitudes consistent with a more permissive local environment for hate crimes. Neither test identifies the mechanism directly. Together, they provide suggestive evidence that something changes in the local political environment when far-right candidates win that does not occur when they narrowly lose.

To examine the visibility of far-right candidates after the election, we use Nexis Uni, a comprehensive database of newspapers, press releases, and news wires, to track media coverage of far-right candidates in close races around the election. We compare the pre and post-election media trajectories of candidates who narrowly win and narrowly lose, using a kernel-weighted approach that assigns greater weight to candidates closer to the electoral threshold. Full details of this analysis are provided in Appendix J1. As shown in Figure J.12, winning and losing candidates follow near-identical trajectories of media attention before the election, which lends credibility to the local comparison. After the election, the two groups diverge sharply: winning candidates sustain long-term elevated media coverage, while losing candidates fade rapidly from the news cycle. This does not identify the content of the rhetoric that voters encounter, nor does it prove that visibility mediates the effect on hate crimes. Still, the pre-election parity and post-election divergence support a key assumption of the argument: winning office increases the public visibility of far-right candidates in the period when we observe changes in hate crimes.

To examine whether far-right victories are associated with attitudinal shifts, we draw on two large surveys: the Cooperative Election Studies (CES) and the Nationscape survey. Both surveys allow us to aggregate individual responses to the congressional district level and apply the same regression discontinuity design used in the main analysis. In Nationscape, we use changes in district-level attitudes before and after the 2020 election. In CES, we use post-election district averages across election cycles, since the relevant questions are not consistently available before and after each election. Full question wordings, coding decisions, and results are reported in Appendix I3. These analyses should be treated with particular caution. Neither survey is designed to be representative at the congressional district level, and the number of district-level observations close to the cutoff is limited, especially in Nationscape, which was fielded around a single electoral cycle.

With these caveats in mind, the results point to three suggestive patterns. First, consistent with existing work suggesting that short-run shifts in mass attitudes are unlikely ([Riaz et al., 2024](#); [Valentim, 2024](#)), we do not find meaningful changes in general favorability toward minority groups in treated districts (Table I.18). Second, districts where far-right candidates narrowly win show lower awareness of discrimination against

minorities, including Black Americans, Jews, and minorities as a whole. Third, the results suggest changes in beliefs related to racial hierarchy. Treated districts show stronger beliefs that dismiss structural barriers to Black advancement, and the CES results show higher agreement with the idea that racial problems in the United States are rare and isolated. At the same time, treated districts also show higher agreement with the idea that white people enjoy certain advantages because of their skin color.¹⁵ We therefore interpret the attitudinal evidence cautiously. The results do not show a broad increase in explicit hostility toward minorities, but they suggest that far-right victories may shift how residents perceive discrimination, racial hierarchy, and group disadvantage.

We stress that these attitudinal patterns do not establish that attitude shifts cause the hate crimes we document. Both the attitudinal changes and the behavioral patterns we observe may be downstream consequences of the same political shock rather than one mediating the other. What the evidence does suggest is that far-right electoral victories are followed by greater visibility for far-right candidates and by changes in how residents perceive minority disadvantage and racial hierarchy. These patterns are consistent with a local environment in which bias-motivated behavior may face lower social costs, but they should be read as exploratory rather than definitive evidence of the mechanism.

7 Discussion

Do far-right electoral victories increase hate crimes? This paper examines this question in the context of U.S. House of Representatives elections from 2006 to 2022, using a sharp regression discontinuity design to compare districts in which far-right candidates narrowly won to those in which they narrowly lost. Our focus on legislative elections is deliberate. Unlike executive officeholders, members of Congress have limited local policy authority. Any effect of electoral victory in this setting must therefore operate primarily through the symbolic and communicative consequences of winning office: increased visibility of exclusionary rhetoric and the institutional legitimation of exclusionary views in the districts these candidates represent.

The evidence points to a selective rather than general effect. We do not find a robust aggregate increase in hate crimes across all minority groups, although the estimates become larger and statistically significant when we restrict the comparison to genuinely close races. Instead, the clearest consequences are concentrated among LGBTQ+ individuals, a group repeatedly targeted across successive waves of far-right politics, both internationally and in the United States ([Arceneaux & Nicholson, 2012](#); [Haas et al., 2025](#); [Murib, 2025](#); [Osborn, 2025](#); [Payne & de Souza Santos, 2020](#)). This pattern suggests that far-right victories do not simply produce a generalized rise in hostility toward minorities.

¹⁵These findings are not necessarily contradictory. Both are consistent with a pattern in which residents in treated districts become more attentive to their own group position while becoming less attentive to the conditions facing minority groups.

Rather, their behavioral consequences are concentrated where electoral success amplifies rhetoric against groups that are already salient in far-right political conflict. The stronger effects during the Trump years further suggest that local far-right victories carry more weight when they occur in a national environment where this rhetoric is more visible, legitimate, and openly defended.

We also find that the overall pattern is concentrated in non-violent offenses, including intimidation, harassment, and property crimes, rather than in the most severe violent incidents. Non-violent offenses are precisely the types of incidents that may be more sensitive to victim reporting, police misclassification, and local recording bias, so these results should be interpreted with caution. At the same time, the pattern is consistent across specifications and aligns with our theoretical argument: if far-right victories make lower-intensity expressions of hostility more socially permissible, it is precisely these kinds of incidents that are most likely to respond to changes in the perceived local political environment.

We have argued that far-right electoral victories increase hate crimes through two pathways: increased visibility of exclusionary rhetoric and the institutional legitimization of exclusionary views. Two pieces of exploratory evidence are consistent with this argument. Media coverage data from Nexis Uni show that winning far-right candidates sustain substantially higher local media coverage than losing candidates after the election, while losing candidates fade rapidly from the local news cycle, consistent with the idea that electoral victory keeps far-right rhetoric more present in the local information environment (Gibson & King, 2024; Zaller, 1992). Attitudinal evidence from the Cooperative Election Studies and Nationscape further suggests that far-right victories are associated with reduced awareness of discrimination against minorities and with changes in beliefs related to racial hierarchy. Consistent with existing work showing that short-run shifts in mass attitudes are unlikely (Riaz et al., 2024; Valentim, 2024), we do not find broad changes in favorability toward minority groups. We stress that these analyses are exploratory, since they face limitations related to sample size, representativeness, and timing. Both the attitudinal patterns and the behavioral consequences may be downstream effects of the same political shock. What the evidence does suggest is that far-right victories change the perceived permissiveness of the local political environment for bias-motivated behavior, without necessarily transforming underlying attitudes toward minorities.

Our study offers three contributions. First, we provide causal evidence that far-right victories in legislative institutions can increase hate crimes in the districts they represent, even when these victories do not confer control over local policy. Existing causal work has examined social media exposure (Müller & Schwarz, 2023), campaign rallies (Feinberg et al., 2022), and executive officeholders (Romarri, 2020). Our setting is different because members of Congress have limited local policy authority, which allows us to isolate the visibility and legitimization effects of electoral victory from the policy channel. Second, the

paper shows that these effects are selective rather than general. The clearest consequences are concentrated among LGBTQ+ individuals and are stronger during periods of broader far-right normalization (Valentim, 2024). This demonstrates that both the identity of the targeted group and the broader political climate condition how far-right electoral success translates into behavioral change. Third, the paper contributes to the study of far-right politics in two-party systems. Rather than relying on party labels or relative ideological distance from a primary opponent, we develop a caucus-based ideological benchmark to classify far-right candidates within the Republican Party in absolute terms, consistently applied across election cycles. This approach may be useful for future research on the consequences of far-right politics in settings where the far-right operates as a faction within a mainstream party rather than as a separate party family.

Our analysis is not without limitations. The most important concerns the measurement of hate crimes through the UCR data. Reported incidents represent only a small fraction of actual victimization (Pezzella et al., 2019), so all our estimates should be interpreted as lower bounds of the true underlying rates. A specific concern for non-violent offenses is that reporting behavior itself may be affected by the treatment. In districts where a far-right candidate narrowly wins, minority victims may become less willing to report incidents to police out of fear that the institutional environment is now less sympathetic to their concerns. Conversely, others may be more motivated to report precisely because they want to document that the far-right victory is emboldening anti-minority behavior and to create a public record of it. These two forces work in opposite directions, and we cannot fully disentangle them from genuine changes in crime incidence. The non-violent estimates should therefore be interpreted with particular caution, as they may capture a mixture of true behavioral change and treatment-induced shifts in reporting propensity. Future research could address these limitations by drawing on National Crime Victimization Survey data, which captures both reported and unreported victimization independently of police recording practices, to construct correction factors that better approximate the underlying incidence of bias-motivated offenses.

A second limitation concerns the statistical power of our design and the breadth of our estimation window. The minimum detectable effect analysis in the appendix shows that our design can detect only relatively large effects in the pooled sample (see Bloom, 1995; Duflo et al., 2007). The null result on aggregate hate crimes should therefore not be taken as evidence that far-right victories have no general effect: it may reflect the limits of our design rather than the absence of an effect. A related concern is that the optimal bandwidth selection procedure (Calonico et al., 2014) sometimes extends the estimation window well beyond what one would strictly consider a close race. Our fixed-bandwidth estimates, which restrict the comparison to elections decided within ten percentage points, yield larger and more precise estimates in the Trump years, suggesting that effects on aggregate hate crimes may be present but detectable only under more

restrictive sampling conditions.

A third limitation concerns our operationalization of far-right candidates. We rely on a one-dimensional measure of ideology based on campaign finance data (Bonica, 2024). Far-right ideology is multidimensional (Pirro, 2023), and our measure cannot fully capture this variation. Nonetheless, the robustness of our findings across alternative definitions of far-right candidacy, including different CF score thresholds and caucus-based classifications, suggests that the main results are not driven by the specific operationalization we use. As the far-right continues to grow across democracies, developing richer and more multidimensional measures of far-right ideology within mainstream parties will be an important priority for future research.

Even with these limitations, the findings carry broader implications. The consequences of far-right electoral success are not limited to the policies these politicians pursue. When far-right candidates win legislative races, they change the local conditions under which minority groups exercise their rights, even without altering local law, spending, service provision, or any of the formal anti-minority measures that have characterized far-right governments elsewhere (Ayoub & Stoeckl, 2024; Haas et al., 2025). Hate crimes are a particularly consequential indicator of these changes: they harm not only their direct victims but entire minority communities, shaping where people live, how they participate in civic life, and whether they feel safe to do so (Perry & Alvi, 2012; Weinstein, 1992). Understanding the symbolic and communicative consequences of democratic elections is therefore essential for assessing the full scope of what far-right success means for the minorities these politicians target.

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Appendix

A Missing Candidates

Not all candidates appear in the DIME database. This is because not all candidates raised funds from the required number of contributors to be included in the scaling (Bonica, 2024). For some candidates that appear in year t and year $t + 1$ we retrieve CF scores by carrying forward CF scores from year t . In this appendix section, we inspect the descriptive statistics of the residual missing group of candidates relative to our sample ($n=587$). They make up 8% of our data. We compare their margins of victory in Figure A.1, their party affiliation in Figure A.2, and their incumbency status in Figure A.3. The overall picture that emerges is that, compared to our sample, missing candidates are always challengers, often electorally unsuccessful, and have an over-representation of independent candidates. In all, we are not concerned that their absence from our sample will meaningfully alter our estimates.

Figure A.1: Margin of Victory Scores

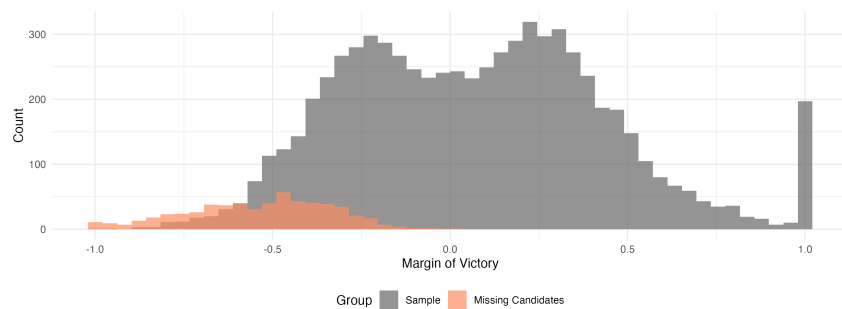


Figure A.2: Party Affiliation

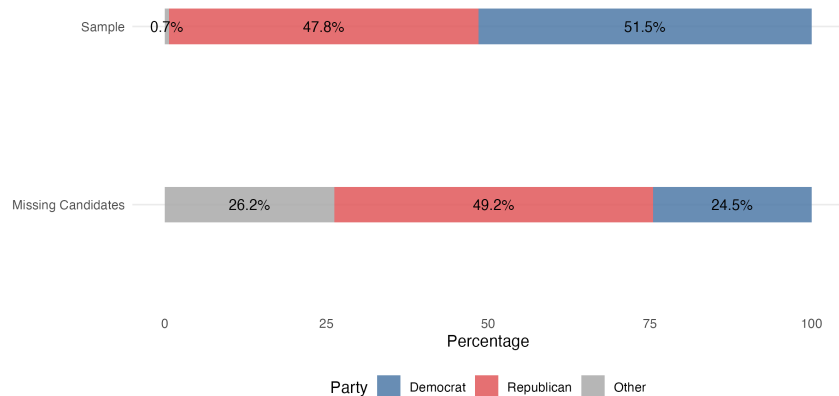
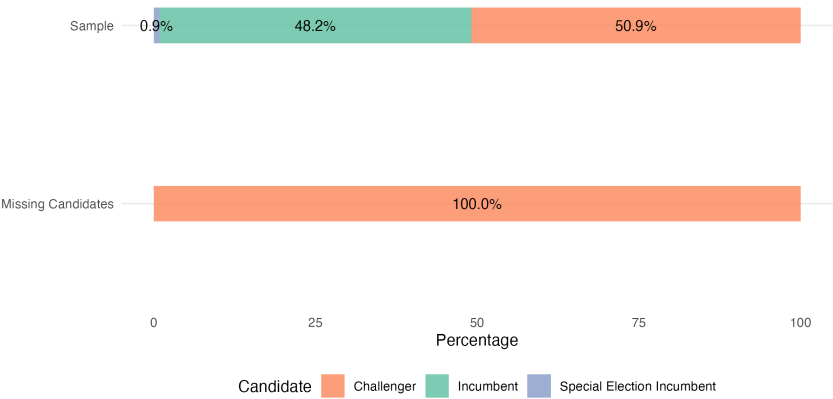


Figure A.3: Incumbency Status



B Tea Party and Freedom Caucus Members

Below, we include summary tables of the Tea Party and Freedom Caucus members that serve as our benchmark for calculating a far-right threshold. Tea Party Caucus members in Table B.1 were retrieved from the [archived member list](#). Freedom Caucus members in Table B.2 were retrieved from [Ballotpedia](#).

Table B.1: Tea Party Caucus Members and Their CF Scores

name_snyder	recipient.cfscore
ADAMS, SANDRA (SANDY)	0.95
ADERHOLT, ROBERT BROWN	1.09
AKIN, WILLIAM TODD	1.34
ALEXANDER, RODNEY M.	0.93
BACHMANN, MICHELE	1.48
BARTLETT, ROSCOE G.	0.93
BARTON, JOE LINUS	1.20
BILIRAKIS, GUS MICHAEL	0.73
BISHOP, ROBERT WILLIAM (ROB)	0.91
BLACK, DIANE L.	1.15
BROUN, PAUL COLLINS	1.42
BURGESS, MICHAEL C.	1.23
BURTON, DANNY L. (DAN)	0.83
CARTER, JOHN R.	1.17
CASSIDY, WILLIAM (BILL)	0.98
COBLE, JOHN HOWARD	0.81
CRENSHAW, ANDER M.	0.95
CULBERSON, JOHN ABNEY	1.23
DUNCAN, JEFFREY D. (JEFF)	1.33
FARENTHOLD, RANDOLPH BLAKE	1.25
FINCHER, STEPHEN LEE	1.21
FLEMING, JOHN C., JR.	1.34
FRANKS, TRENT	1.24
GINGREY, PHILLIP J. (PHIL)	1.35
GOHMERT, LOUIS (LOUIE)	1.47
HARTZLER, VICKY J.	1.36
HERGER, WALTER WILLIAM (WALLY), JR.	0.94
HUELSKAMP, TIMOTHY (TIM)	1.47
JENKINS, LYNN M.	1.18
KING, STEVEN A. (STEVE)	1.45
LAMBORN, DOUGLAS L. (DOUG)	0.99
LANDRY, JEFFREY M. (JEFF)	1.19
LUETKEMEYER, W. BLAINE	1.21
MARCHANT, KENNY E. (KEN)	1.08
MCCLINTOCK, THOMAS MILLER (TOM), II	1.27
MCKINLEY, DAVID B.	0.61
MILLER, GARY G.	0.71
MULVANEY, JOHN MICHAEL (MICK)	1.30
NEUGEBAUER, RANDY	1.34
NUGENT, RICHARD B. (RICH)	0.91
PALAZZO, STEVEN MCCARTY	1.20
PEARCE, STEVAN E. (STEVE)	1.21
PENCE, MICHAEL RICHARD (MIKE)	1.33
POE, LLOYD (TED)	1.08
PRICE, THOMAS EDMUNDS (TOM)	1.16
REHBERG, DENNIS R. (DENNY)	1.25
ROE, DAVID PHILIP (PHIL)	1.16
ROSS, DENNIS ALAN	0.93
ROYCE, EDWARD R. (ED)	0.90
SCALISE, STEPHEN J. (STEVE)	1.27
SESSIONS, PETER A. (PETE)	1.14
SMITH, ADRIAN M.	1.17
SMITH, LAMAR S.	1.23
STEARNS, CLIFFORD B. (CLIFF)	0.85
WALBERG, TIMOTHY L. (TIM)	1.32
WALSH, JOE	0.33
WEST, ALLEN B.	1.35
WESTMORELAND, LYNN A.	1.25
WILSON, JOE	1.11

Table B.2: Freedom Caucus Members and Their CF Scores

name_snyder	recipient.cfscore
AMASH, JUSTIN	1.07
BABIN, BRIAN	1.21
BIGGS, ANDY	1.45
BISHOP, DAN	1.41
BLUM, RODNEY LELAND (ROD)	1.47
BOEBERT, LAUREN	1.58
BRAT, DAVID A. (DAVE)	1.42
BRECHEN, JOSH	1.33
BRIDENSTINE, JAMES FREDERICK (JIM)	1.30
BROOKS, MORRIS J. (MO), JR.	1.36
BUCK, KENNETH (KEN)	1.36
BUDD, THEODORE PAUL (TED)	1.46
BURLISON, ERIC	1.32
CAWTHORN, MADISON	1.59
CLAWSON, CURT	1.11
CLINE, BENJAMIN LEE (BEN)	1.23
CLOUD, MICHAEL	1.34
CLYDE, ANDREW	1.41
COLLINS, M.A. (MIKE)	1.33
CRANE, ELI	1.61
DAVIDSON, WARREN	1.33
DESANTIS, RONALD D. (RON)	1.35
DESJARLAIS, SCOTT EUGENE	1.32
DONALDS, BYRON	1.46
DUNCAN, JEFFREY D. (JEFF)	1.33
FLEMING, JOHN C., JR.	1.34
FRANKS, TRENT	1.24
FULCHER, RUSSELL M. (RUSS)	1.39
GAETZ, MATT (JERRY)	1.46
GARRETT, E. SCOTT	1.03
GARRETT, THOMAS A. (TOM), JR.	1.23
GOHMERT, LOUIS (LOUIE)	1.47
GOOD, ROBERT G. (BOB)	1.49
GOSAR, PAUL ANTHONY	1.27
GREEN, MARK E.	1.32
GREENE, MARJORIE TAYLOR	1.62
GRIFFITH, H. MORGAN	1.11
HAGEMAN, HARRIET M.	1.55
HARRIS, ANDREW P. (ANDY)	0.96
HARSHBARGER, DIANA LYNN	1.32
HERRELL, YVETTE	1.43
HICE, JODY B.	1.52
HIGGINS, CLAY	1.30
HUELSKAMP, TIMOTHY (TIM)	1.47
JACKSON, RONNY	1.57
JOHNSON, MIKE	1.41
JORDAN, JAMES D. (JIM)	1.52
LABRADOR, RAUL RAFAEL	1.31
LESKO, DEBBIE	1.36
LOUDERMILK, BARRY D.	1.28
LUMMIS, CYNTHIA MARIE	1.13
LUNA, ANNA PAULINA	1.54
MEADOWS, MARK R.	1.26
MILLER, MARY E.	1.51
MILLER, MAX	1.15
MOONEY, ALEXANDER X. (ALEX)	1.26
MOORE, BARRY	1.35
MULVANEY, JOHN MICHAEL (MICK)	1.30
MURPHY, GREGORY F. (GREG)	1.15
NEHLS, TROY E.	1.29
NORMAN, RALPH W.	1.32
OGLES, ANDY	1.40
PALMER, GARY J.	1.32
PEARCE, STEVAN E. (STEVE)	1.21
PERRY, SCOTT GORDON	1.31
POE, LLOYD (TED)	1.08
POSEY, WILLIAM (BILL)	1.00
ROSENDALE, MATTHEW M. (MATT)	1.47
ROTHFUS, KEITH JAMES	1.17
ROY, CHIP	1.51
SALMON, MATTHEW JAMES (MATT)	1.11
SANFORD, MARSHALL CLEMENT (MARK), JR.	1.02
SCHWEIKERT, DAVID	1.23
SELF, KEITH A.	1.09
STEUBE, GREG	1.20
STUTZMAN, MARLIN A.	1.39
TIFFANY, THOMAS P. (TOM)	1.44
WEBER, RANDY K., SR.	1.08
WRIGHT, RON	1.38
YOHIO, THEODORE SCOTT (TED)	1.20

C Descriptive Statistics

C1 Descriptive Statistics for Treatment and Control Groups

Table C.3: Descriptive Statistics for Treatment and Control Groups

	Sample	Margin of Victory			
	(N)	(Mean)	(SD)	(Min)	(Max)
<i>Far-right running</i>					
Control Group	725	-0.299	0.181	-0.832	-0.002
Treatment Group	394	0.295	0.237	0.000	1.000
<i>Far-right running & Non-incumbency</i>					
Control Group	712	-0.304	0.180	-0.832	-0.003
Treatment Group	151	0.191	0.179	0.000	1.000
<i>Far-right running (Trump years)</i>					
Control Group	391	-0.288	0.184	-0.832	-0.003
Treatment Group	237	0.275	0.216	0.000	1.000
<i>Far-right running & Non-incumbency (Trump years)</i>					
Control Group	385	-0.292	0.182	-0.832	-0.006
Treatment Group	97	0.195	0.162	0.000	0.694

C2 Descriptive Statistics for Outcome and Control Variables

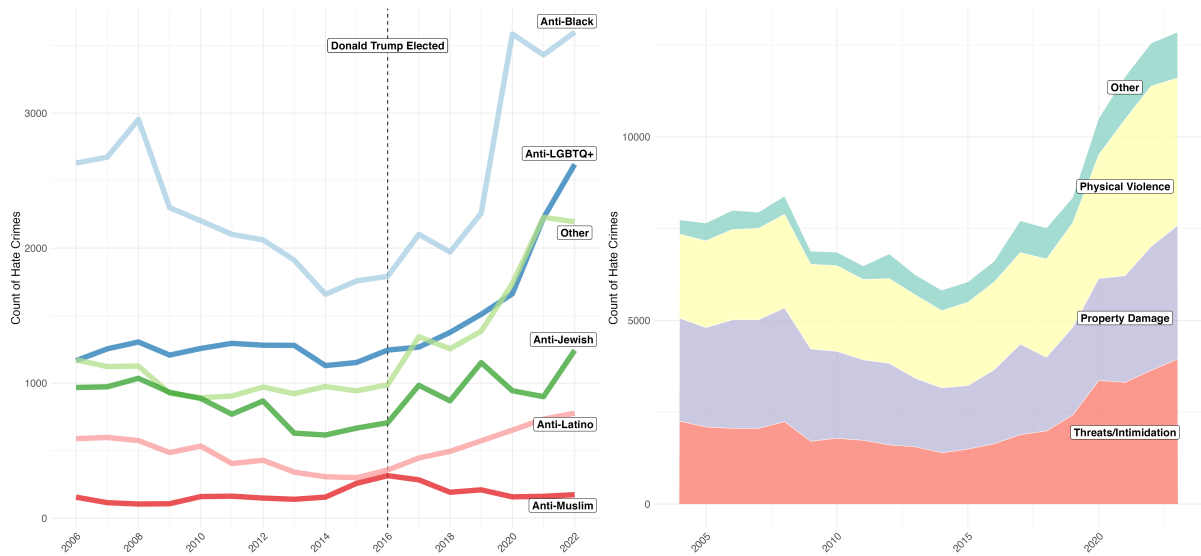
Table C.4: Descriptive Statistics for Outcome and Control Variables

Hate crimes Variables (Δ)					
	N	Mean	SD	Min	Max
Hate Crimes Against Minorities	861	0.15	2.14	-20.08	26.80
Hate Crimes Against Jews	861	0.07	0.55	-3.11	5.92
Hate Crimes Against Muslims	861	0.01	0.16	-1.22	1.06
Hate Crimes Against Blacks	861	-0.03	1.10	-16.99	5.33
Hate Crimes Against Latinos	861	0.02	0.36	-2.13	3.60
Hate Crimes Against LGBTQ+ People	861	0.11	0.64	-3.16	7.13
Attitudes Variables Nationscape (Δ)					
	N	Mean	SD	Min	Max
Discrimination Awareness Minorities	172	3.36	0.15	2.92	3.68
Discrimination Awareness Jews	172	3.08	0.15	2.67	3.42
Discrimination Awareness Muslims	172	3.61	0.17	3.18	3.96
Discrimination Awareness Blacks	172	3.78	0.20	3.21	4.24
Black's Work Ethic Prejudice	172	3.29	0.21	2.72	3.76
Black's Structural Barrier Prejudice	172	2.65	0.26	2.05	3.63
Favorability towards Minorities	172	3.03	0.10	2.71	3.29
Favorability towards Jews	172	3.24	0.12	2.83	3.53
Favorability towards Muslims	172	2.86	0.16	2.29	3.27
Favorability towards Blacks	172	3.21	0.12	2.75	3.52
Favorability towards LGBTQ+	172	3.00	0.17	2.50	3.47
Attitudes Variables CES					
	N	Mean	SD	Min	Max
White Privilege Awareness	482	2.52	0.36	1.55	3.59
Racial Denial Attitude	482	2.33	0.27	1.57	3.29
Black's Work Ethic Prejudice	664	3.36	0.43	2.18	4.54
Black's Structural Barrier Prejudice	664	2.93	0.41	1.75	4.18
Control Variables					
	N	Mean	SD	Min	Max
District % Minority	863	0.42	0.24	0.03	0.91
District % Black	863	0.14	0.16	0.00	0.68
District % Migrant	863	0.15	0.11	0.01	0.52
District Median Income	863	62847.56	21001.34	27955.00	152783.00
District % Poverty	863	0.14	0.05	0.04	0.30
District % Unemployment	863	0.07	0.03	0.02	0.19

Notes: Count of observations and descriptive statistics for our outcome variables and control variables by district between 2006 and 2022.

C3 Trends in Hate Crime Targets and Types

Figure C.4: Trends in Hate Crime Targets and Crime Types



C4 Classification of Hate Crime Offence Types

We group UCR hate crime incidents into three mutually exclusive categories: violent, non-violent, and property-related offenses. The classification is based on the primary offense code reported in the Hate Crime Statistics data. Violent hate crimes include offenses that involve physical force or the threat of serious physical harm, such as aggravated and simple assault, homicide and manslaughter, kidnapping, robbery, rape and other sexual assaults, and related offenses. Non-violent hate crimes include intimidation, weapon law violations, drug and fraud offenses, and other non-physical offenses. Property-related hate crimes include arson, burglary, motor vehicle theft, and destruction, damage, or vandalism of property.

Table C.5 reports the full list of UCR offense categories used in our analysis, along with their classification and the number and share of incidents in each group. The vast majority of non-violent incidents are coded as intimidation, most violent incidents are cases of aggravated or simple assault, and most property incidents involve destruction, damage, or vandalism of property.

Table C.5: Classification of Hate Crime Offense Types

Minor hate crime offenses	N	Percent
All Other Larceny	2,868	5.42
Assisting or Promoting Prostitution	9	0.02
Betting/Wagering	1	0.00
Bribery	6	0.01
Counterfeiting/Forgery	220	0.42
Credit Card/Automated Teller Machine Fraud	182	0.34
Drug Equipment Violations	476	0.90
Drug/Narcotic Violations	1,279	2.42
Embezzlement	55	0.10
Extortion/Blackmail	70	0.13
False Pretenses/Swindle/Confidence Game	349	0.66
Federal Liquor Offenses	1	0.00
Hacking/Computer Invasion	21	0.04
Identity Theft	116	0.22
Impersonation	157	0.30
Intimidation	43,092	81.51
Not Specified	620	1.17
Pocket-picking	32	0.06
Pornography/Obscene Material	93	0.18
Prostitution	14	0.03
Purchasing Prostitution	2	0.00
Purse-snatching	32	0.06
Shoplifting	749	1.42
Stolen Property Offenses	140	0.26
Theft From Building	610	1.15
Theft From Coin-Operated Machine or Device	13	0.02
Theft From Motor Vehicle	801	1.52
Theft of Motor Vehicle Parts or Accessories	203	0.38
Weapon Law Violations	625	1.18
Welfare Fraud	9	0.02
Wire Fraud	25	0.05
Total non-violent incidents	52,870	100.00
Property-related hate crime offenses	N	Percent
Arson	893	1.68
Burglary/Breaking & Entering	2,911	5.47
Destruction/Damage/Vandalism of Property	48,858	91.78
Motor Vehicle Theft	569	1.07
Total property incidents	53,231	100.00
Violent hate crime offenses	N	Percent
Aggravated Assault	16,886	31.31
Animal Cruelty	20	0.04
Criminal Sexual Contact	248	0.46
Human Trafficking, Commercial Sex Acts	3	0.01
Human Trafficking, Involuntary Servitude	1	0.00
Incest	7	0.01
Kidnapping/Abduction	170	0.32
Murder and Nonnegligent Manslaughter	153	0.28
Negligent Manslaughter	7	0.01
Rape	232	0.43
Robbery	2,809	5.21
Sexual Assault With An Object	55	0.10
Simple Assault	33,241	61.64
Sodomy	77	0.14
Statutory Rape	22	0.04
Total violent incidents	53,931	100.00

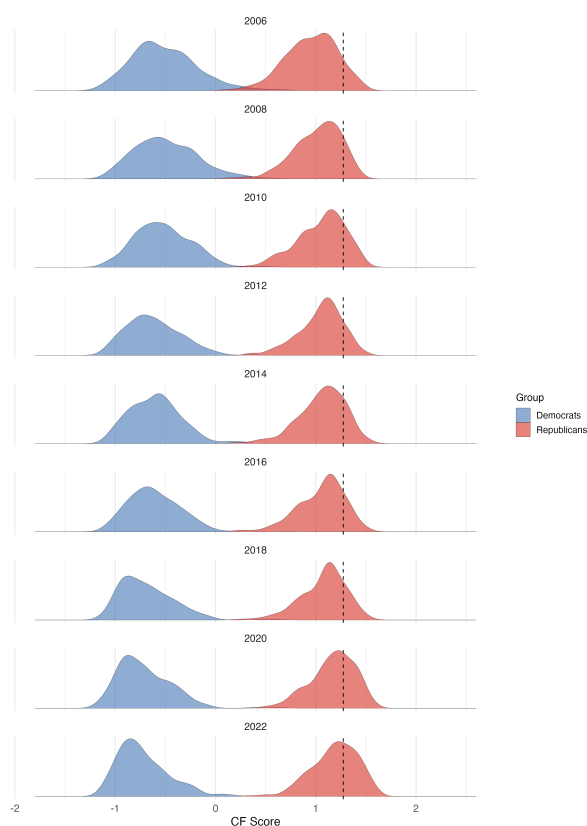
Notes: Count of observations and descriptive statistics of the types of Crimes reported to UCR. Classification made by authors.

D Far-Right Threshold Robustness Checks

D1 Spatial Relevance of Threshold with Dynamic CF Scores

In the main text, we contrast our far-right threshold to the distribution of time-invariant CF scores among Republicans and Democrats. In Figure D.5 we also show how our threshold holds up compared to dynamic CF scores over the years. Overall, Figure D.5 shows a process of ideological polarization where ideology scores for Democrats and Republicans are moving further apart from each other. Despite this our threshold remains spatially relevant to distinguish far-right candidates.

Figure D.5: Threshold Positions with Dynamic CF Scores



E Data Imputation

We rely on CF scores from the DIME data set ([Bonica, 2024](#)) to define far-right candidates. Some scores are missing. We therefore impute scores in two scenarios. First, the time-invariant CF score is sometimes missing for candidate-year observations where a time-variant (dynamic) CF score is missing, despite the candidate having been assigned a time-invariant score in other years. Since the time-invariant should be the same for each candidate across years, we impute time-invariant CF scores for candidate-year gaps if the candidate has at least one other time-invariant CF score in another of the other years. We do this for 227 out of 7645 cases, which amounts to 3% of our data set.

Second, when the time-variant (dynamic) CF score is missing but a time-invariant CF score is available for that candidate-year, we impute the dynamic score by substituting the available time-invariant score. We do this for 237 out of 7645 cases, which amounts to 3.1% of our data set.

F Redistricting Adjustments

F1 Weighted CF Scores

Between 2006–2022, nationwide redistricting took place in 2012 and 2022, and other instances of redistricting took place following Supreme Court rulings in other years (2016 in FL, NC, VA; 2018 in PA, and 2020 in NC). The redrawing of electoral boundaries matters for our identification of previous winners in each district d , since incumbents of a district at time $t - 2$ may run in a roughly similar or entirely different district at t . There might also be cases where newly drawn districts are composed of two or multiple former districts.

We address this challenge by comparing the territorial overlap between pre- and post-redrawn districts in 2012 and 2022 (all districts), 2016 (FL, NC, VA), 2018 (PA), and 2020 (NC). For each Congressional District at time t , we calculate the percentage of overlap with former Congressional Districts at time $t - 2$ using Census Congressional Districts shapefiles.

Rather than choosing one previous winner for each district, we plug in the territorial overlap percentages to calculate a weighted CF score for these districts by weighting previous incumbents’ CF scores.

Example: The 4th Congressional District in Alabama was redrawn in 2012 after the 2010 Census. The new district covers some parts of the 6th, 3th, 4th, and 5th Congressional Districts territories as they were drawn in 2010 and before. See Table F.6.

Table F.6: Overlap and CF-scores for last winners by GEOID

District PRE	District POST	Overlap	Last Winner Name	Last Winner CF Score (Time-Invariant)
AL04	AL06	9.10%	BACHUS, SPENCER THOMAS, III	0.986
AL04	AL03	0.83%	ROGERS, MICHAEL DENNIS (MIKE)	1.044
AL04	AL04	75.24%	ADERHOLT, ROBERT BROWN	1.086
AL04	AL05	14.83%	BROOKS, MORRIS J. (MO), JR.	1.356

We then compute the weighted CF-score for the new AL04 district as the overlap-weighted average of the static CF-scores:

$$\begin{aligned}\widehat{CF}_{AL04,2012} &= \sum_{d' \in \{03,04,05,06\}} w_{d'} CF_{d'}^{\text{static}} \quad \text{where} \quad w_{d'} = \frac{\text{Overlap}_{AL04,d'}}{100} \\ &= 0.0910 \cdot 0.986 + 0.0083 \cdot 1.044 + 0.7524 \cdot 1.086 + 0.1483 \cdot 1.356 \approx 1.12\end{aligned}$$

Thus the imputed (weighted) CF-score for Alabama’s 4th district in 2012 is 1.1166.

G Survey Data

We draw on two large-scale surveys to test our causal mechanism: the Cooperative Election Studies and Nationscape. Below we report the question wording of variables we use in our analysis.

G1 Cooperative Election Studies

White Privilege Statement: “White people in the U.S. have certain advantages because of the color of their skin.”

Answer options: “Strongly agree” (1), “Somewhat agree” (2), “Neither agree nor disagree” (3), “Somewhat disagree” (4), “Strongly disagree” (5).

Racial Denial Statement: “Racial problems in the U.S. are rare, isolated situations.”

Answer options: “Strongly agree” (1), “Somewhat agree” (2), “Neither agree nor disagree” (3), “Somewhat disagree” (4), “Strongly disagree” (5).

Black’s Work Ethic (same in Nationscape) Question Prompt: “Please tell us how much you agree or disagree with the following statements. - Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors.”

Re-scaled answer options: “Strongly agree” (5), “Somewhat agree” (4), “Neither agree nor disagree” (3), “Somewhat disagree” (2), “Strongly disagree” (1).

Black’s Structural Barrier (same in Nationscape) Question Prompt: “Please tell us how much you agree or disagree with the following statements. - Generations of slavery and discrimination have created conditions that make it difficult for blacks to work their way out of the lower class”

Answer options: “Strongly agree” (1), “Somewhat agree” (2), “Neither agree nor disagree” (3), “Somewhat disagree” (4), “Strongly disagree” (5).

G2 Nationscape

Group Favorability Question Prompt: “Here are the names of some groups that are in the news from time to time. How favorable is your impression of each group, or haven’t you heard enough to say?”

Re-scaled answer options: “Very favorable” (4), “Somewhat favorable” (3), “Somewhat unfavorable” (2), “Very unfavorable” (1)

Groups we include in our analysis: Blacks, Latinos, Asians, Muslims, Jews, Migrants, LGBTQ+ persons.

Discrimination Awareness **Question Prompt:** “How much discrimination is there in the United States today against each of the following groups?”

Re-scaled answer options: “A great deal” (5), “A lot” (4), “A moderate amount” (3), “A little” (2), “None at all” (1)

Groups we include in our analysis: Blacks, Asians, Muslims, Jews.

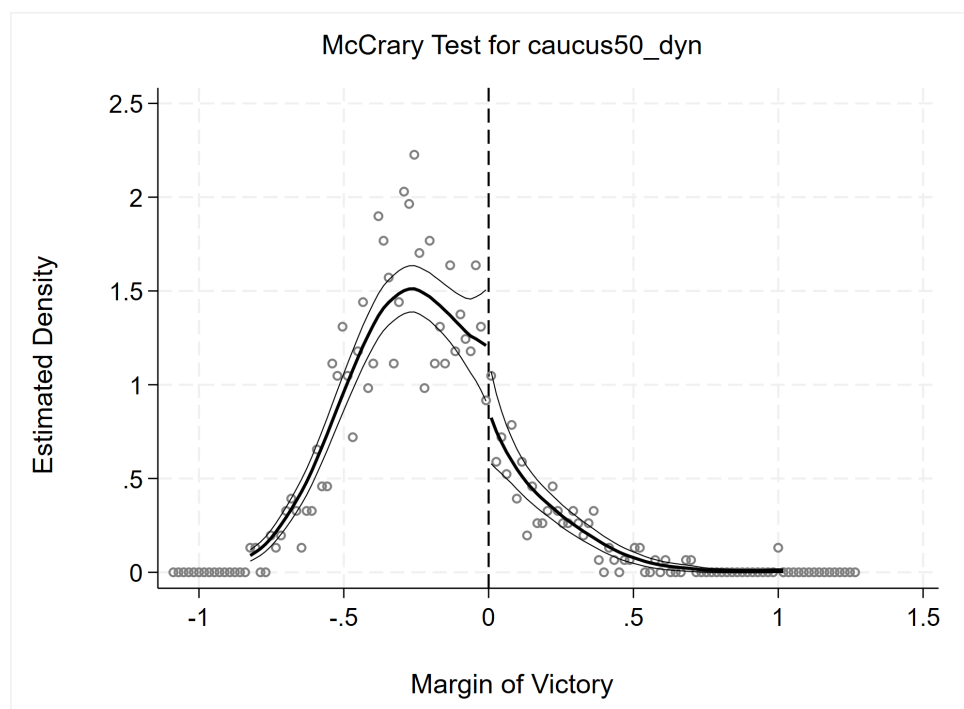
H RD Validity

H1 Validity of the RD Design: McCrary Density Test

The regression discontinuity (RD) design yields unbiased estimates of the treatment effect when certain identification conditions are met. In this section, we provide evidence that these assumptions hold in our empirical setting.

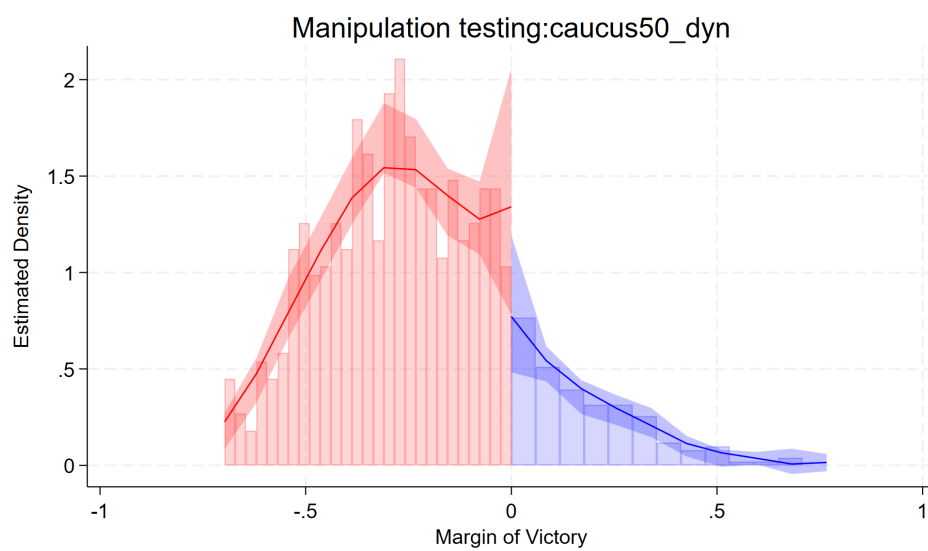
A key threat to the validity of the RD design arises if individuals can precisely manipulate the assignment variable. In our context, a discontinuity at the cutoff could suggest that candidates are able to influence their margin of victory. To assess this concern, we conduct a McCrary density test ([McCrary, 2008](#)), the results of which are shown in Figure H.6. Reassuringly, the test indicates no significant discontinuity in the distribution of far-right candidates' margins around the threshold. To further rule out non-random sorting into treatment and control groups, we implement a data-driven manipulation test based on a local polynomial density estimation approach developed by [Cattaneo, Jansson, and Ma \(2018\)](#). This method avoids pre-binning, thereby improving size accuracy, and permits the imposition of additional model restrictions, enhancing statistical power. As depicted in Figure H.7, this test also finds no evidence of manipulation around the cutoff.

Figure H.6: McCrary test: Far-right margin of victory (2006-2022)



Notes: Graph produced by the authors using DCdensity command in Stata created by ([McCrary, 2008](#))

Figure H.7: McCrary test: Far-right margin of victory (2006-2022)



Notes: Graph produced by the authors using rddensity command in Stata created by [Cattaneo et al. \(2018\)](#).

H2 Validity of the RD Design: Balance for Control variables

Another central assumption of the RD design is that, near the threshold, the assignment to treatment should be as good as random for any of the observable characteristics of the district. In our case, this means that whether a far-right representative is elected or not should not depend on district variables around the cutoff. Table H.7 provides evidence supporting this assumption, showing that covariates remain balanced around the threshold. Figure H.8 shows the Regression Discontinuity plots for these models around the threshold.

Continuity of Control Variables

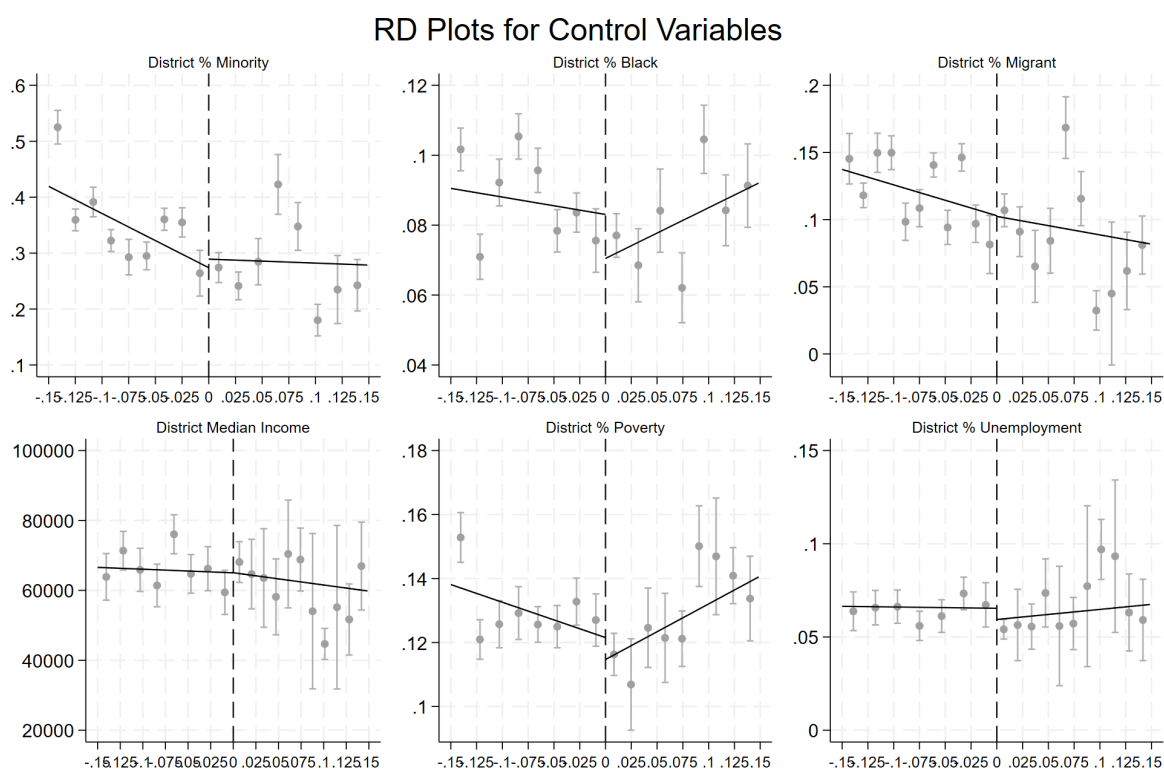
Table H.7: Election of Far-right Candidates on Control Variables

	% Minorities	% Black	% Migrant	Median Income	% Poverty	% Unemployment
	(1)	(2)	(3)	(4)	(5)	(6)
Bias-corrected	-0.069 (0.044)	-0.020 (0.023)	-0.007 (0.015)	474.122 (2959.743)	-0.014 (0.010)	-0.007 (0.005)
Bandwidth	0.145	0.185	0.253	0.145	0.132	0.195
N (Left)	161	201	291	162	148	214
N (Right)	76	88	109	76	71	89
Order_polyn.	1	1	1	1	1	1

Notes: Each column reports RD estimates with robust standard errors in parentheses. All regressions include Year Fixed Effects.

Signif.: ***: 0.01, **: 0.05, *: 0.1.

Figure H.8: Regression Discontinuity Plots for covariate variables



Notes: Graph produced by the authors using `rdplot` command in Stata created by [Calonico et al. \(2014\)](#). Each point represents the bin sample average of hate crimes reported for the margin of victory, using the mimicking variance evenly-spaced method for bin selection. The straight line is a first-order polynomial in Margin of Victory fitted separately on each side of the margin of victory threshold at zero. 90% confidence intervals.

H3 PCRD: Continuity of Candidates Characteristics

An important implication of our design is that we cannot fully isolate the causal effect of far-right ideology, because far-right candidates who win close races may differ systematically from their opponents on other characteristics. To assess this, we use candidate age, race, and gender. For winning candidates, we obtain this information from the United States House of Representatives Archives. For all candidates in the race, we use the candidate characteristics dataset created by [Bellodi, Román Amarales, and De Vries \(2025\)](#). Our assesment is limited to the subset of candidates that appeared in races between 2012 and 2020, which are the years for which we have candidate-level demographic data.

Using this data, we first compare how far-right candidates near the threshold differ from out entire sample of far-right candidates. These comparisons are summarized in Table H.8. We do not observe any major changes between the entire sample and the 15-point threshold besides the ratio of men to women. We further explore this gender difference in a second step.

Table H.8: Characteristics of Far-Right Candidates: All vs. 15-Point Threshold

	Group	N	Female (%)	Male (%)	Mean age	White (%)	Black (%)	Hispanic (%)	Other (%)
1	15-Point Threshold	267	31.40	68.60	51.80	87.70	5.90	5.90	0.50
2	All	1051	21.40	78.10	52.50	86.40	8.20	5.00	0.10

Second, we explore whether candidate characteristics display discontinuities at the cutoff. Table H.9 presents the results. While most characteristics appear continuous, we find a significant discontinuity for gender: far-right women candidates are more likely to win close races compared to their male counterparts. This reinforces our interpretation that the PCRD captures the effect of electing far-right candidates as a bundle of ideology and other characteristics, rather than the effect of ideology alone.

Table H.9: Election of Far-right Candidates on Candidates Characteristics

	Candidate White	Candidate Female	Candidate Under 40
	(1)	(2)	(3)
Bias-corrected	0.001 (0.080)	0.532*** (0.151)	-0.081 (0.120)
Bandwidth	0.283	0.145	0.215
N (Left)	198	83	38
N (Right)	60	41	49
Order_polyn.	1	1	1

Notes: Each column reports RD estimates with robust standard errors in parentheses. All regressions include Year Fixed Effects.

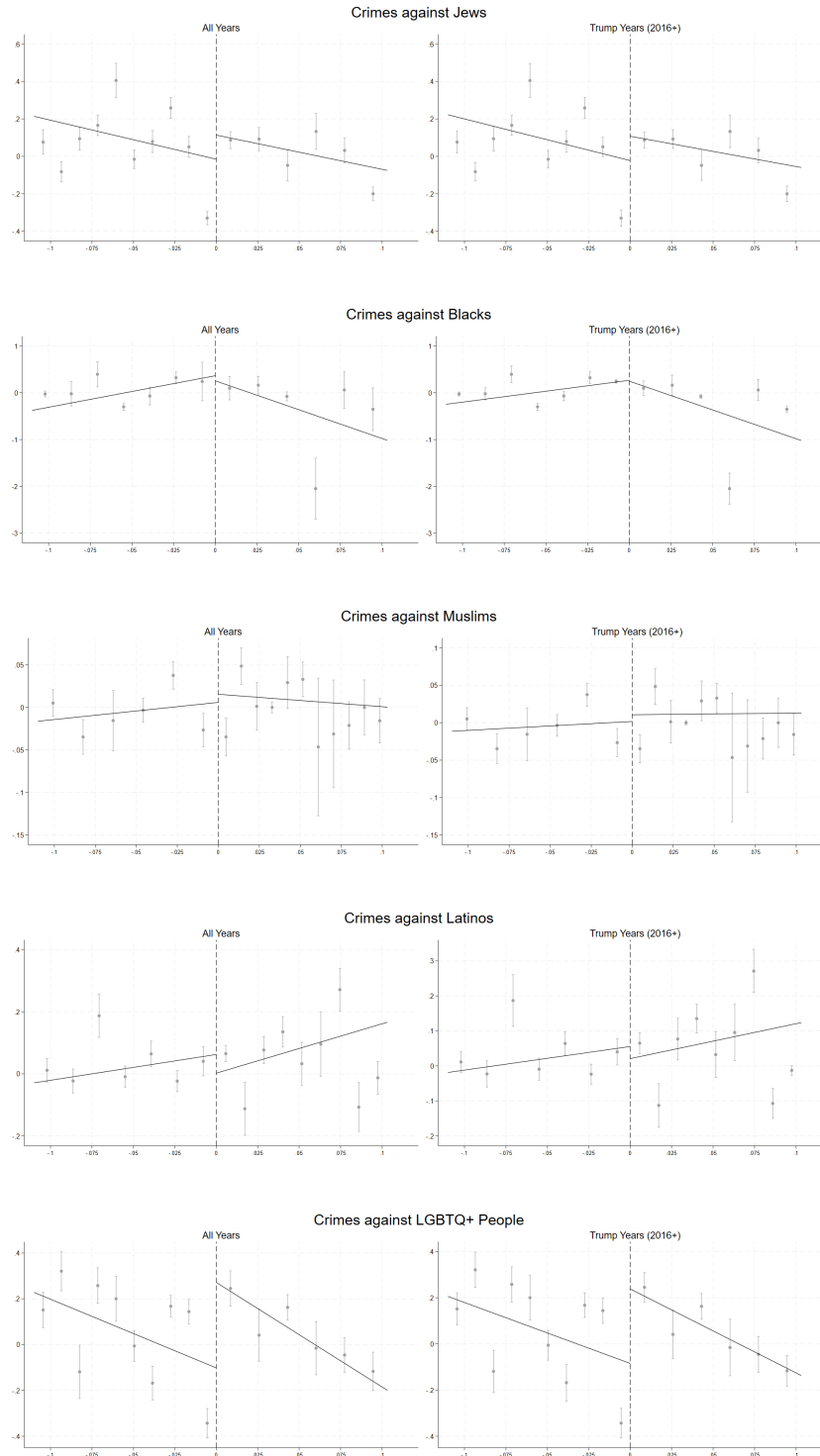
Signif.: ***: 0.01, **: 0.05, *: 0.1.

I Supplementary Empirical Results

I1 Hate Crimes Main Results

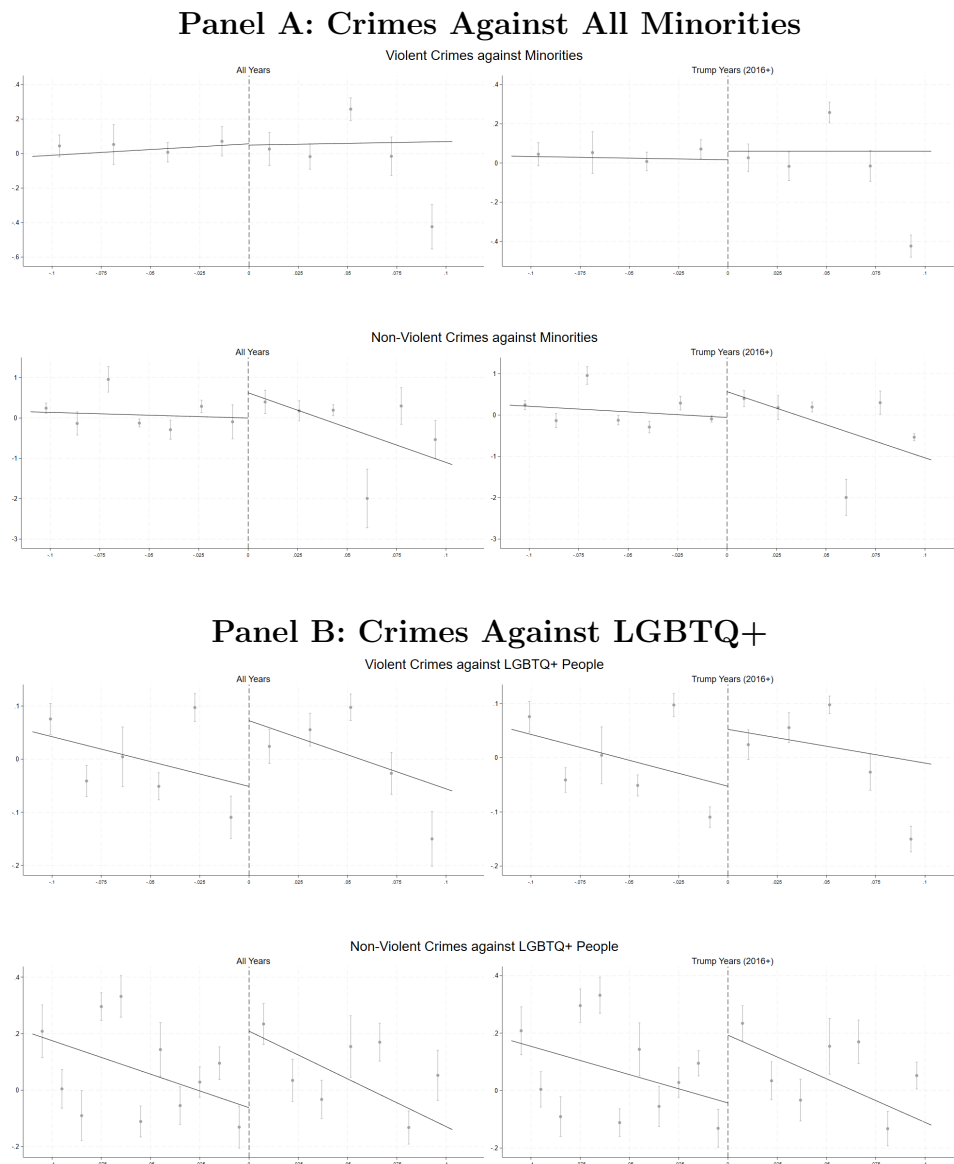
Regression Discontinuity Plots

Figure I.9: Regression Discontinuity Plots for Crimes Against Minority Groups



Notes: Each point represents the bin sample average of hate crimes reported for the margin of victory, using the mimicking variance evenly-spaced method for bin selection. The straight line is a first-order polynomial in Margin of Victory fitted separately on each side of the margin of victory threshold at zero. 90% confidence intervals.

Figure I.10: Regression Discontinuity Plots by Hate Crime: Violent & Non-Violent



Notes: Each point represents the bin sample average of hate crimes reported for the margin of victory, using the mimicking variance evenly-spaced method for bin selection. The straight line is a first-order polynomial in Margin of Victory fitted separately on each side of the margin of victory threshold at zero. 90% confidence intervals.

Regression Discontinuity Tables: Minority Subgroups & Type of Crime

Table I.10: Election of Far-right Candidates on Hate Crimes: Specific Minorities & Type of Crime

Panel A: All years (2006 - 2022)

	Jews		Muslims		Blacks		Latino		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.019 (0.031)	-0.013 (0.057)	0.004 (0.012)	0.003 (0.022)	-0.073 (0.065)	0.055 (0.209)	-0.041 (0.033)	-0.008 (0.042)	0.074 (0.059)	0.308*** (0.086)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.191	0.179	0.196	0.190	0.203	0.118	0.214	0.122	0.224	0.112
N (Left)	208	197	217	207	227	127	241	134	252	121
N (Right)	88	85	90	88	92	65	94	67	98	63
Order_polyn.	1	1	1	1	1	1	1	1	1	1

Panel B: Trump years (2016 - 2022)

	Jews		Muslims		Blacks		Latino		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	0.065 (0.062)	-0.077 (0.086)	-0.002 (0.018)	-0.020 (0.032)	-0.274*** (0.088)	0.114 (0.163)	-0.057 (0.046)	0.049 (0.041)	0.183 (0.111)	0.472*** (0.137)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.160	0.197	0.169	0.168	0.149	0.162	0.162	0.179	0.143	0.097
N (Left)	106	134	112	112	98	109	109	119	96	62
N (Right)	48	54	49	49	46	48	48	51	44	35
Order_polyn.	1	1	1	1	1	1	1	1	1	1

Notes: Each panel report RD estimates with robust standard errors in parentheses. Outcome variables are changes in crimes against minorities per 100,000 population in the year following the election. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).

Signif.: ***: 0.01, **: 0.05, *: 0.1.

I2 Hate Crimes Robustness Checks

Regression Discontinuity: Minimum Detectable Effect (MDE)

The minimum detectable effect (MDE) values reported below are ex-post calculations that summarize the smallest true effects our design would detect with a given probability under repeated sampling, given the realized sample size and variance (Bloom, 1995; Duflo et al., 2007). The MDE is therefore an ex-ante benchmark applied ex-post, used to assess the informativeness of null results rather than to constrain which realized effects may be statistically significant. Significant estimates smaller than the MDE can occur when the realized sampling variance is lower than expected. The substantive limitation arises only for non-significant estimates below the MDE, since in those cases the design lacks sufficient power to rule out effects of meaningful size.

Table I.11: Power Analysis - Main Models

	All Years	Trump Years
	(1)	(2)
RD Estimate	0.323 (0.339)	0.280 (0.357)
MDE	1.124	1.142
N (Left)	153	137
N (Right)	74	54
Bandwidth	.138	.201

Notes: Each model reports our main RD estimates with robust standard errors in parentheses. Outcome variables are crimes against minorities over a 100,000 population. All regressions include Year Fixed Effects and controls. These are District characteristics (share of minorities, migrants, unemployment and poverty; and district median income). Minimum Detectable Effects (MDE) are calculated using stata command `rdmde` by Cattaneo, Titiunik, and Vazquez-Bare (2019) using a significance level α equal to 5% and a desired power β of 0.80
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Regression Discontinuity Tables: Fixed Bandwidth - Close Elections

Table I.12: Election of Far-right Candidates on Hate Crimes: Fixed Bandwidth

Panel A: All years (2006 - 2022)

	Minorities		Jews		Muslims		Blacks		Latino		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.322 (0.422)	0.369 (0.424)	0.287*** (0.103)	0.301*** (0.103)	0.011 (0.029)	0.016 (0.029)	-0.522** (0.252)	-0.508** (0.254)	-0.098* (0.055)	-0.066 (0.052)	0.464*** (0.140)	0.443*** (0.140)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100
N (Left)	106	106	106	106	106	106	106	106	106	106	106	106
N (Right)	60	60	60	60	60	60	60	60	60	60	60	60
Order.polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Panel B: Trump years (2016 - 2022)

	Minorities		Jews		Muslims		Blacks		Latino		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.986** (0.500)	1.264*** (0.483)	0.537*** (0.185)	0.540*** (0.182)	-0.016 (0.043)	0.006 (0.042)	-0.456* (0.265)	-0.435* (0.253)	-0.132 (0.085)	-0.034 (0.073)	0.976*** (0.232)	1.016*** (0.228)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100
N (Left)	63	63	63	63	63	63	63	63	63	63	63	63
N (Right)	35	35	35	35	35	35	35	35	35	35	35	35
Order.polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Notes: Each panel report RD estimates with robust standard errors in parentheses. Outcome variables are changes in crimes against minorities per 100,000 population in the year following the election. All regressions include Year Fixed Effects and are fixed to a bandwidth of 10%. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income). Signif.: ***: 0.01, **: 0.05, *: 0.1.

Table I.13: Election of Far-right Candidates on Hate Crimes: Fixed Bandwidth & Violent Crimes

	Minorities		Jews		Muslims		Blacks		Latino		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	-0.011 (0.157)	0.443 (0.357)	0.163*** (0.048)	0.139* (0.071)	0.021 (0.015)	0.004 (0.029)	-0.243*** (0.084)	-0.245 (0.245)	-0.080** (0.041)	0.023 (0.044)	0.143 (0.088)	0.322*** (0.088)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100
N (Left)	106	106	106	106	106	106	106	106	106	106	106	106
N (Right)	60	60	60	60	60	60	60	60	60	60	60	60
Order.polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Notes: The table reports RD estimates with robust standard errors in parentheses. Outcome variables are changes in crimes against minorities per 100,000 population in the year following the election. All regressions include Year Fixed Effects and are fixed to a bandwidth of 10%. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income). Signif.: ***: 0.01, **: 0.05, *: 0.1.

Regression Discontinuity Tables: Polynomial order 2

Table I.14: Election of Far-right Candidates on Hate Crimes: Polynomial order 2

Panel A: All years (2006 - 2022)

	Minorities		Jews		Muslims		Blacks		Latino		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.523 (0.475)	0.500 (0.458)	0.065 (0.095)	0.075 (0.096)	-0.003 (0.032)	-0.010 (0.033)	-0.149 (0.316)	-0.139 (0.313)	-0.101 (0.061)	-0.093 (0.057)	0.436*** (0.152)	0.422*** (0.152)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.180	0.191	0.236	0.234	0.210	0.206	0.166	0.171	0.206	0.194	0.199	0.204
N (Left)	197	208	267	265	234	229	185	189	229	214	219	228
N (Right)	85	88	101	101	94	93	81	84	93	89	90	93
Order_polyn.	2	2	2	2	2	2	2	2	2	2	2	2

Panel B: Trump years (2016 - 2022)

	Minorities		Jews		Muslims		Blacks		Latino		LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.767 (0.516)	0.759 (0.499)	0.155 (0.166)	0.142 (0.164)	-0.023 (0.048)	-0.027 (0.047)	-0.152 (0.249)	-0.208 (0.233)	-0.043 (0.090)	-0.035 (0.084)	0.723*** (0.259)	0.703*** (0.257)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.199	0.211	0.224	0.230	0.205	0.209	0.209	0.225	0.235	0.217	0.194	0.197
N (Left)	136	147	155	158	142	145	146	155	165	150	130	134
N (Right)	54	56	58	61	55	56	56	58	61	57	53	54
Order_polyn.	2	2	2	2	2	2	2	2	2	2	2	2

Notes: Each panel report RD estimates with robust standard errors in parentheses. Outcome variables are changes in crimes against minorities per 100,000 population in the year following the election. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Table I.15: Election of Far-right Candidates on Hate Crimes: Polynomial order 2 & Violent Crimes

	Minorities		Jews		Muslims		Blacks		Latino		LGBTQ+	
	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent	Violent	Non-Violent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	-0.083 (0.148)	0.700* (0.414)	0.071 (0.050)	0.027 (0.071)	-0.011 (0.017)	-0.000 (0.029)	-0.113 (0.086)	0.073 (0.306)	-0.101** (0.044)	0.014 (0.045)	0.110 (0.083)	0.334*** (0.102)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	0.215	0.177	0.206	0.223	0.180	0.252	0.227	0.168	0.174	0.242	0.235	0.189
N (Left)	241	197	229	250	197	291	254	186	196	274	265	206
N (Right)	94	85	93	98	85	109	100	82	85	103	101	88
Order_polyn.	2	2	2	2	2	2	2	2	2	2	2	2

Notes: The table reports RD estimates with robust standard errors in parentheses. Outcome variables are changes in crimes against minorities per 100,000 population in the year following the election. All regressions include Year Fixed Effects. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Regression Discontinuity Tables: Other Far-right Definitions

To assess whether our findings are sensitive to the way we classify far-right candidates, we re-estimate the main RD models using three alternative thresholds. The first defines far-right candidates as those above the median CF score among all members of the Freedom and Tea Party Caucuses. The second uses only the median CF score of the Freedom Caucus. The third defines far-right candidates as those above the seventy-fifth percentile of CF scores among all Republican candidates. Table I.16 reports the results for these alternative definitions in minority groups as a whole, and also specifically for LGBTQ+ minorities. The estimates closely resemble those obtained with our main definition of far-right candidacies, particularly for LGBTQ+ individuals. These results indicate that our conclusions are not driven by the specific choice of CF score threshold.

Table I.16: Election of Far-right Candidates on Hate Crimes: Main outcome and other Far-right Definitions

Panel A: Crimes Against All Minorities

	Minority - F.R. All Caucus				Minority - F.R. Freedom Caucus				Minority - F.R. Republicans (75p)			
	All Years		Trump Years		All Years		Trump Years		All Years		Trump Years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.542 (0.332)	0.581* (0.336)	0.908** (0.399)	1.029** (0.403)	-0.054 (0.367)	-0.044 (0.368)	0.317 (0.381)	0.318 (0.357)	0.238 (0.295)	0.273 (0.290)	0.334 (0.365)	0.341 (0.341)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.141	0.141	0.141	0.141	0.228	0.228	0.228	0.228	0.266	0.266	0.266	0.266
N (Left)	157	157	95	95	218	218	138	138	269	269	165	165
N (Right)	74	74	44	44	80	80	53	53	83	83	55	55
Order.polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Panel B: Crimes Against LGBTQ+

	LGBTQ+ - F.R. All Caucus				LGBTQ+ - F.R. Freedom Caucus				LGBTQ+ - F.R. Republicans (75p)			
	All Years		Trump Years		All Years		Trump Years		All Years		Trump Years	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	0.440*** (0.129)	0.426*** (0.130)	0.889*** (0.212)	0.892*** (0.211)	0.311** (0.141)	0.282** (0.143)	0.643*** (0.210)	0.623*** (0.204)	0.352** (0.142)	0.325** (0.144)	0.723*** (0.211)	0.685*** (0.206)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.122	0.122	0.122	0.122	0.159	0.159	0.159	0.159	0.166	0.166	0.166	0.166
N (Left)	135	135	81	81	150	150	94	94	153	153	96	96
N (Right)	69	69	40	40	67	67	44	44	65	65	43	43
Order.polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are changes in crimes against minorities per 100,000 population in the year following the election. All regressions include Year Fixed Effects and use the All-Caucus optimal bandwidth for comparability. Controls include District characteristics (share of minorities, migrants, unemployment and poverty; and district median income). F.R. All Caucus defines far-right as those above the median CF Score from the Freedom and Tea-Party Caucuses. F.R. Freedom Caucus defines far-right as those above the median CF Score from the Freedom Caucus alone. F.R. Republicans (75p) defines far-right as those above the 75th percentile CF Score of all Republicans in Congress.

Signif.: ***: 0.01, **: 0.05, *: 0.1.

I3 Attitudinal Outcomes

Far-right Victories and Attitudinal Change

Table I.17: Election of Far-right Candidates on Attitudes: Nationscape & CES

Panel A: Nationscape

	Disc. Minorities		Disc. Jews		Disc. Muslims		Disc. Blacks		Black's Work Ethic		Black's Structural Barrier	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	-0.282*** (0.092)	-0.216*** (0.082)	-0.132* (0.072)	-0.158** (0.069)	-0.115 (0.079)	-0.151* (0.084)	-0.272*** (0.096)	-0.206** (0.085)	-0.014 (0.102)	0.023 (0.106)	0.156* (0.087)	0.218** (0.088)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.133	0.197	0.172	0.206	0.193	0.158	0.137	0.196	0.198	0.164	0.186	0.147
N (Left)	39	53	44	56	51	40	40	52	53	42	48	40
N (Right)	17	20	20	21	20	20	18	20	20	20	20	20
Order_polyn.	1	1	1	1	1	1	1	1	1	1	1	1

Panel B: CES

	White Privilege		Racial Denial		Black's Work Ethic		Black's Structural Barrier	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.127** (0.049)	-0.127*** (0.045)	-0.097* (0.053)	-0.086* (0.048)	-0.016 (0.042)	-0.019 (0.041)	-0.004 (0.036)	-0.015 (0.035)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.157	0.144	0.168	0.195	0.185	0.161	0.189	0.167
N (Left)	105	96	112	130	172	155	176	158
N (Right)	48	45	49	53	76	72	76	72
Order_polyn.	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are standardized. Controls include District characteristics (share of minorities, migrants, unemployment, poverty, and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

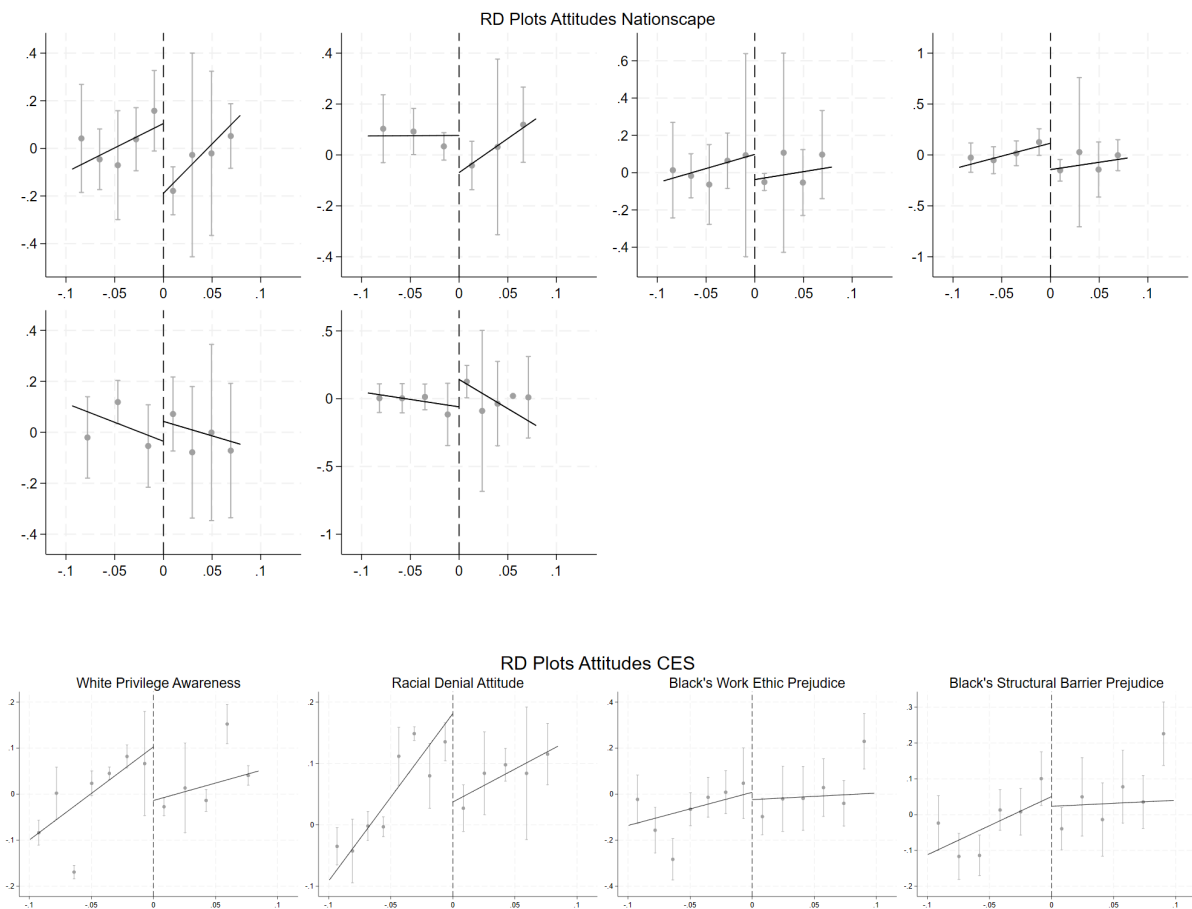
Table I.18: Election of Far-right Candidates on Attitudes: Other outcomes Nation-scape

	Att. Minorities		Att. Jews		Att. Muslims		Att. Black		Att. LGBTQ+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD Estimate	-0.100 (0.077)	-0.069 (0.076)	-0.044 (0.095)	-0.003 (0.081)	-0.108 (0.088)	-0.111 (0.088)	-0.130 (0.082)	-0.155* (0.085)	0.054 (0.099)	0.036 (0.104)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.183	0.204	0.232	0.221	0.142	0.139	0.215	0.186	0.232	0.186
N (Left)	48	55	63	61	40	40	59	48	63	48
N (Right)	20	21	22	21	18	18	21	20	22	20
Order_polyn.	1	1	1	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are standardized and correspond to respondents' favorability towards these minority groups. Outcome are in differences, before and after the 2020 Election. Controls include District characteristics (share of minorities, migrants, unemployment, poverty, and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Regression Discontinuity Plots

Figure I.11: Regression Discontinuity Plots for attitude variables



Notes: Each point represents the bin sample average of hate crimes reported for margin of victory. The straight line is a first-order polynomial in Margin of Victory fitted separately on each side of the margin of victory threshold at zero. 95% confidence intervals.

Regression Discontinuity Tables (Attitudes): Fixed Bandwidth

Table I.19: Election of Far-right Candidates on Attitudes with Fixed Bandwidth: Nationscape & CES

Panel A: Nationscape

Disc. Minorities		Disc. Jews		Disc. Muslims		Disc. Blacks		Black's Work Ethic		Black's Structural Barrier	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	-0.478*** (0.099)	-0.167** (0.079)	-0.098 (0.074)	-0.242** (0.110)	-0.259*** (0.096)	-0.445*** (0.112)	-0.401*** (0.104)	0.362** (0.143)	0.152 (0.130)	0.438*** (0.118)	0.238** (0.104)
Controls	No	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100	0.100
N (Left)	28	28	28	28	28	28	28	28	28	28	28
N (Right)	15	15	15	15	15	15	15	15	15	15	15
Order_polyn.	1	1	1	1	1	1	1	1	1	1	1

Panel B: CES

White Privilege		Racial Denial		Black's Work Ethic		Black's Structural Barrier	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.001 (0.058)	-0.039 (0.049)	-0.119* (0.065)	-0.160*** (0.059)	0.029 (0.054)	0.001 (0.050)	0.019 (0.047)
Controls	No	Yes	No	Yes	No	Yes	No
Bandwidth	0.100	0.100	0.100	0.100	0.100	0.100	0.100
N (Left)	63	63	63	63	94	94	94
N (Right)	35	35	35	35	54	54	54
Order_polyn.	1	1	1	1	1	1	1

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are standardized. Controls include District characteristics (share of minorities, migrants, unemployment, poverty, and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

Regression Discontinuity Tables (Attitudes): Polynomial order 2

Table I.20: Election of Far-right Candidates on Attitudes p(2): Nationscape & CES

Panel A: Nationscape

Disc. Minorities		Disc. Jews		Disc. Muslims		Disc. Blacks		Black's Work Ethic		Black's Structural Barrier	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
RD Estimate	-0.341*** (0.123)	-0.083 (0.104)	-0.054 (0.096)	-0.167 (0.112)	-0.150 (0.107)	-0.308*** (0.115)	-0.290*** (0.114)	0.096 (0.156)	0.131 (0.167)	0.274** (0.133)	0.283** (0.119)
Controls	No	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bandwidth	0.171	0.167	0.180	0.232	0.245	0.224	0.235	0.204	0.175	0.186	0.194
N (Left)	42	42	47	63	66	62	65	55	47	48	52
N (Right)	20	20	20	22	22	21	22	21	20	20	20
Order_polyn.	2	2	2	2	2	2	2	2	2	2	2

Panel B: CES

White Privilege		Racial Denial		Black's Work Ethic		Black's Structural Barrier	
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	-0.148** (0.062)	-0.159*** (0.056)	-0.175** (0.069)	-0.200*** (0.069)	-0.003 (0.052)	0.002 (0.047)	-0.009 (0.045)
Controls	No	Yes	No	Yes	No	No	Yes
Bandwidth	0.237	0.216	0.205	0.183	0.255	0.250	0.244
N (Left)	166	150	141	121	246	240	234
N (Right)	61	57	55	51	93	91	89
Order_polyn.	2	2	2	2	2	2	2

Notes: Each panel reports RD estimates with robust standard errors in parentheses. Outcome variables are standardized. Controls include District characteristics (share of minorities, migrants, unemployment, poverty, and district median income).
Signif.: ***: 0.01, **: 0.05, *: 0.1.

J Mechanism Tests

J1 Nexis Uni Data

We use Nexis Uni data to provide descriptive evidence of differential media attention to winning and losing far-right candidates. Nexis Uni is a comprehensive database of newspapers, press releases, and news wires, offering a valuable measure of candidate-level media exposure. We expect that far-right candidates in close races receive similar levels of media attention prior to the election — reflecting their comparable electoral standing — but that this pattern diverges sharply post-election. Specifically, we expect winning candidates to sustain media attention after taking office, while losing candidates fade from coverage.

In our setup, we conduct Nexis queries for all far-right candidates ($n = 140$) between 2006–2022 that fall within a ± 0.10 point margin of victory. We group these candidates by their electoral outcome, where

$$\text{Outcome}_i = \begin{cases} \text{Winner} & \text{if } \text{MOV}_i > 0 \\ \text{Loser} & \text{if } \text{MOV}_i < 0 \end{cases} \quad (6)$$

To ensure comparability between winners and losers, we apply a triangular kernel weighting scheme that assigns greater weight to candidates whose margin of victory falls closer to the electoral threshold:

$$w_i = 1 - \frac{|\text{MOV}_i|}{h} \quad (7)$$

where $h = 0.10$ is the bandwidth. This approach mirrors the identifying assumption underlying regression discontinuity designs, in which candidates just above and just below the threshold are treated as near-identical in expectation.

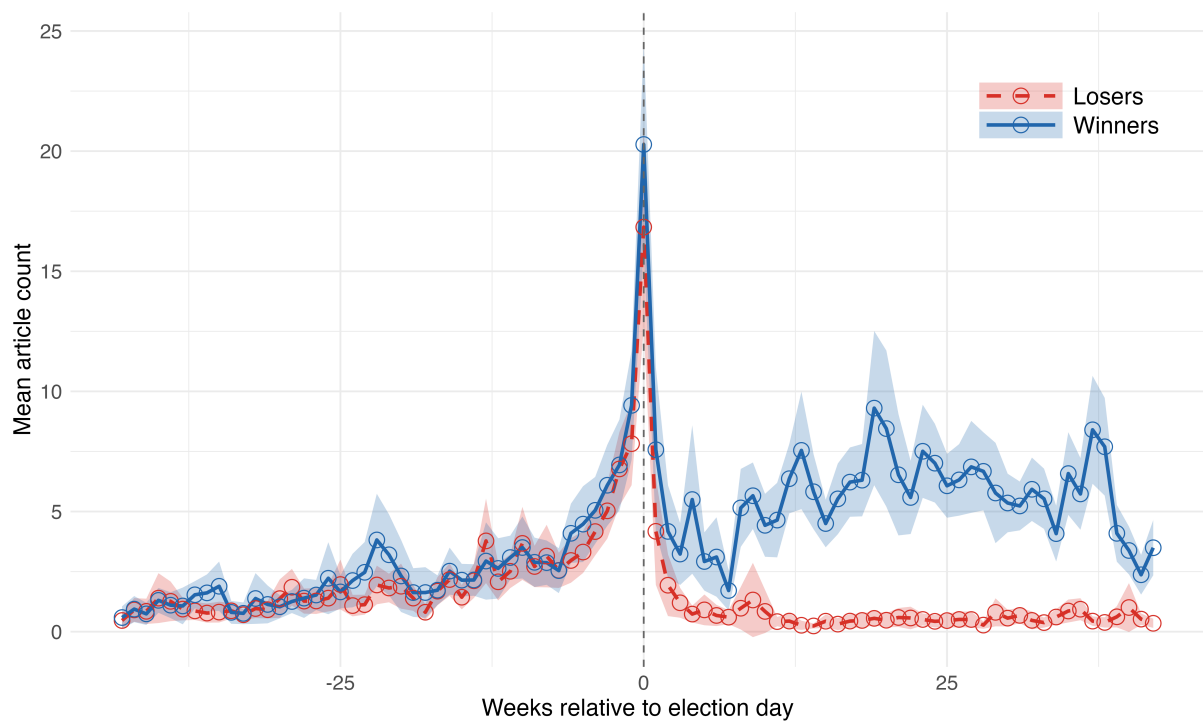
For each candidate, we construct a search string of the following form:

"[CANDIDATE NAME]" AND ([CANDIDATE STATE] AND 'DISTRICT')

where the candidate's state and the word 'district' serve to disambiguate the query. While this reduces total article counts, it prevents common names from introducing irrelevant news articles into the sample. We compute weekly kernel-weighted mean article counts for winning and losing candidates within a 300-day window around election day (± 43 weeks), removing duplicates and excluding one outlier candidate (George Santos).

The results are shown in Figure J.12. As expected, winning and losing candidates follow near-identical trajectories of media attention pre-election, but diverge sharply thereafter. Winning candidates sustain long-term elevated media coverage following their elec-

Figure J.12: Weighted Weekly Mean of Nexis Uni Hits for Far-Right Candidates



Note: The figure plots the kernel-weighted weekly mean article count for winning and losing far-right candidates within a 300-day window around election day. Shaded areas denote 95% confidence intervals. Candidates are weighted by their proximity to the electoral threshold ($h = 0.10$).

tion, while losing candidates fade rapidly from the news cycle. Though the analysis is purely descriptive, the pre-election parity lends credibility to the comparison.