Constituent-Elite Relationships and Their Implications for Modern Democracies

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Abstract

Political elites interact with their constituents in various ways. Our understanding of elite communication, elite-provided policy options, and elite-shaped national democracies can be constrained by commonly used methodological tools. Using new methodological approaches or newly available data, this dissertation presents three new perspectives on the relationship between political elites and how they influence the opinions of their constituents.

First, in the context of the European refugee crisis, I examine how the announcement of the Balkan route closure affected public perceptions of the crisis's severity. Using an Unexpected Event During Survey Design, I depart from the typically used survey experimental approach to assess the effect of this political communication and identify the causal effect of the announcement in a real-world setting. I find that political communication has a short-lived positive effect on citizens' perceptions of the crisis. Furthermore, my study serves as a template for future research aiming to post-factually identify the effect of political communication in real-world settings.

Second, I propose a novel conceptualisation of polarisation that associates the available policy alternatives within a political system with the preferences of individual citizens. This enables an examination of the impact of polarisation at the individual level. Utilising the same sample of respondents, I demonstrate that polarisation can simultaneously diminish citizen support for democratic governance and augment their level of engagement. These findings indicate that polarisation possesses a dual nature, which contradicts the recently proposed negative implications of polarisation for democracy.

Third, I use newly available data that captures perceptions of the European Union at the macro level to test how the national democratic standards shaped by political elites affect attitudes towards the EU. My findings show that the benchmark process by which EU citizens compare their national conditions with those of the EU only partially applies to democratic standards. People in new member countries compare the EU to their national standards, and higher national standards mean less EU support. However, the EU's growing reputation as a democratic policing body balances out the negative differences that member nations draw between their national and EU democracies the longer they are members. Ultimately, long-term members are more supportive of the EU the higher their level of democratic standards.

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In progress

Declaration

I declare that, except where explicit reference is made to the contribution of others, that this dissertation is the result of my own work and has not been submitted for any other degree at the University of Glasgow or any other institution

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Abbreviations

- ANES - American National Elections Study
- BES - British Election Study
- CARDS - The programme of Community Assistance for Reconstruction, Development and Stabilisation
- CHES - Chapel Hill Expert Survey
- CSES - Comparative Study of Electoral Systems
- EB - Eurobarometer
- EP - European Parliament
- EU - European Union
- GDP - Gross domestic product
- GIP - German Internet Panel
- IDEA - International Institute for Democracy and Electoral Assistance
- IRT - Item Response Theory
- OECD - Organisation for Economic Co-operation and Development
- PSU - Primary sampling units
- PP - Partido Popular / Spanish People's Party
- UESD - Unexpected Event during Surveys Design
- V-Dem - Varieties of Democracy

Chapter 1

Introduction

In an era of rapidly changing political landscapes, the role of public opinion in shaping the health and legitimacy of democracies has become a central concern for scholars and policymakers alike. The intricate relationship between public opinion and the ways in which political elites are polarised and communicate their stances, as well as the relationship of the governance system they form at the national level with supranational structures, in particular, the EU, is also of interest.

Whilst polarisation is often viewed as detrimental to democratic norms and governance, leading to potential gridlock or democratic backsliding, it's also an essential element in fostering democratic progress. Polarisation aligns societal differences along a single dimension and intensifies the perception of politics as an "Us" versus "Them" contest. It reflects diverse views within a society and stimulates healthy competition between different political groups. Furthermore, it mobilises marginalised or disunited sectors, underscoring societal issues and giving voice to the unheard, often facilitated by political elites who use divisive rhetoric for such mobilisation. Even though countries like Hungary, Turkey, Venezuela, and the United States illustrate the risks of polarisation, such as power consolidation and majoritarian politics, they also demonstrate how groups that felt marginalised could rise to power and effect change. As a persistent feature of modern democratic societies, polarisation, despite its potential negative effects, plays a key role in driving social change and fostering democratic competition. However, it needs to be managed effectively to avert democratic norm violations and prevent it from leading to negative outcomes, striking a balance that promotes its beneficial effects whilst mitigating the negative ones (McCoy, Rahman and Somer, 2018).

Political communication and the management of information are essential to the functioning of democratic governance. These processes involve strategies and techniques utilised by governmental institutions to deliver information about their operations to the public (Steinberg, 1958). Such activities, while aimed at keeping the public informed about the operations of various government departments, can also serve as a power resource, potentially having a significant influence on public opinion and shaping political landscapes (Denton and Woodward, 1990). The control of information can range from providing open access to official data to introducing measures that limit or direct the flow of information, serving specific governmental interests (McNair, 2017). Such strategies could aim to engage citizens in the democratic process or serve to insulate the government from public scrutiny. The choice between these strategies can have significant impacts on the democratic landscape of a country. For instance, the level of openness or secrecy maintained by a government regarding its operations can either foster an inclusive political culture or create a closed system that limits public participation (Ponting, 1989). These practices underscore the importance of political communication in shaping democracy, either facilitating transparency and accountability or fostering control and limitation (Cockerell, Hennessy and Walker, 1984). Understanding the critical role of political communication in democratic governance, and how it shapes public opinion in real-world scenarios, is of paramount importance for the robust functioning of our democracy.

Understanding public opinion towards the European Union (EU) is equally important for the health of democracy. In the past, European integration was seen as irrelevant to public sentiment and domestic political competition, but this has shifted over time (Hobolt, 2009; Hobolt and De Vries, 2016; Hooghe and Marks, 2009). Public resistance now influences the integration process, such as in referendums that introduced public participation, compelling national and European elites to strategise to gain public endorsement for the integration project. Examples of this influence include the election of the Euro-critical Syriza-led government in Greece in 2015, demonstrating how public attitudes can change towards EU policies and how Eurosceptic voices in government can complicate the EU policy-making process (Hobolt and Spoon, 2012; Hobolt, Spoon and Tilley, 2009). Public sentiment towards the EU also plays a significant role in national and European elections and referendums, particularly as the issue of European integration becomes more

politicised, resulting in a widening gap between the pro-European positions of mainstream parties and the Eurosceptical attitudes of the electorate, contributing to the rise of parties with Eurosceptic positions in European Parliament elections (Clark and Rohrschneider, 2009; de Vries et al., 2011). Furthermore, the increasing importance of direct democracy has led to over 50 referendums on various aspects of European integration (Hobolt, 2009; Mendez, Mendez and Triga, 2014), where public opinion has had consequential impacts on EU policy, such as when domestic electorates rejected further integration proposals despite consensus among national elites (Franklin, Marsh and McLaren, 1994; Franklin, Eijk and Marsh, 1995). Additionally, national governments represented in the Council of the European Union, the EU's most powerful decision-making body, are increasingly influenced by public sentiment towards the EU (Van der Eijk and Franklin, 2004; Franklin and Eijk, 2007; de Vries, 2007; de Vries, 2010; Tillman, 2004), showing how public opinion on European integration has influenced legislative output (Toshkov, 2011; Bølstad, 2015). Given these developments, further research is needed to better understand the dynamic responsiveness of governments to their domestic public opinion on European integration and how it influences policy outcomes, emphasising the importance of public opinion in EU policy making as a consideration for democratic integrity.

The thesis at hand presents three distinct studies that address fundamental aspects of how elites shape public opinion in relation to democratic governance. First, by the polarisation of elites; second, by communication about political positions; and third, by how national political elites who are part of a multi-level governance system shape attitudes towards supranational levels of governance like the EU. As a result, each distinct study seeks to advance understanding of the respective relationship by challenging existing methodological norms.

The Impact of Polarisation on Democratic Satisfaction and Political Engagement

The first analysis in this series of studies introduces a fresh perspective on the impact of the spread of party choices, i.e., polarisation, on democratic health. The relationship between public opinion and behaviour aimed at democratic governance and the political choices elites provide can be contradictory at times as demonstrated by several studies. On the one hand, findings suggest that citizen satisfaction increases when policy options offered by the

party system align closely with the preferences of the median voter. Ezrow and Xezonakis (2011) provides evidence for this, showing that the ideological proximity of party choices to the mean voter's position led to increased overall citizen satisfaction. Building upon this, Stecker and Tausendpfund (2016) demonstrated that policy congruence beyond the general left-right ideological axis influences citizen satisfaction. When there is a deviation from citizens' views on policy dimensions such as European integration and redistribution, satisfaction decreases.

The first study further identified political interest as a central moderating factor, with citizens with stronger political interest experiencing a greater decrease in satisfaction when the government is politically distant. In a complementary perspective, Ferland (2021) emphasised the impact of congruence between citizen preferences and actual policies on democratic satisfaction. Their research showed that policy congruence significantly affects citizens' satisfaction with democracy, potentially more so than previous measures of ideological congruence. This implies that citizens are more satisfied when their government and the implemented policies closely represent their preferences, which, however, becomes less likely as political elites become increasingly polarised. On the other hand, Wilford (2017) illustrated a contradictory dynamic concerning voter turnout. The study found that highly polarised systems with a few parties tend to stimulate higher voter turnout, suggesting that polarisation, while possibly undermining satisfaction with the democratic regime, may also galvanise individuals to participate in the democratic process.

Earlier analyses, such as those presented by Ezrow and Xezonakis (2011), Stecker and Tausendpfund (2016), Ferland (2021), and Wilford (2017), primarily employed macro-level measurements of party positioning and resultant polarization. However, this approach overlooks the variance in voter positions in relation to the choices offered by political elites. My new approach offers a more granular, individual-centred measure of polarization, termed "relative polarization". This measure factors in the relationship between citizens' preferred party choices and the alternatives available to them, providing a more nuanced view of the polarization effect. This perspective enables a more accurate capture of individual-level variations, which are driven by voter preferences vis-à-vis their available party options.

My findings from this comprehensive analysis, spanning multiple democracies and time periods, echo the seemingly contradictory results observed in earlier works. As relative polarization increases, satisfaction with democracy decreases. In contrast, relative polarization boosts voter turnout. Intriguingly, I found that the government status of a respondent's preferred party moderates the impact of relative polarization on democratic satisfaction. Respondents who see their preferred party in power appear less affected by relative polarization or only to a minor degree. By introducing this individual-centred measure of polarization, I provide a deeper understanding of how polarization shapes political engagement and satisfaction with democracy. This approach addresses the limitations of previous macro-level analyses, further illuminating the multifaceted relationship between polarization and its impacts on democratic systems. It confirms the intriguing contradictions seen in previous literature but underlines them with a more robust methodology, thereby providing a significant contribution to the discourse on polarization in modern democracies.

The Dynamics of Political Communication Effects in Real-World Contexts

Beyond the specific positioning of political elites, the manner in which these positions are communicated in real-world contexts is integral to our understanding of public opinion. However, the longevity and impact of such communications remain topics of extensive debate across various disciplines. Communication sciences research suggests that news framing can have enduring effects on citizens' understanding of politics (Lecheler and De Vreese, 2011, Tewksbury and Scheufele, 2009). Lecheler and De Vreese (2011) provide empirical evidence of the persistent nature of framing effects across various time points after the initial exposure. They argue that the persistence of these effects depends on the level of political knowledge of individuals, with moderately knowledgeable individuals exhibiting the most lasting framing effects.

In contrast, research in political sciences implies that the effects of political messages are often short-lived. Chong and Druckman (2010) argue that the impact of a political message can vary depending on its timing within a competitive context and how it is evaluated by the audience. They found that when competing messages are separated by time, individuals tend to place disproportionate weight on the most recent communication, as

previous effects decay. Hill et al. (2013) corroborate these findings by demonstrating the rapid decay of the persuasive impact of advertising in political campaigns, suggesting that communication effects may not have lasting impacts on public opinion. Moreover, social psychology research acknowledges the transient nature of communication effects. For example, Baesler and Burgoon (1994) studied the persuasion effects of different types of evidence over various time intervals, and found statistical evidence to be more persuasive than story evidence, with the former remaining persuasive for up to a week. Cook and Flay (1978) further discuss the persistence of experimentally induced change, noting that the theories of persistence may differ from theories of initial attitude change. Their review of persistence literature indicates that while initial attitude changes can be achieved experimentally, their persistence over time is less certain.

One of the reasons for the varied findings related to the lasting impact of communication effects may lie in the traditional research methodologies employed in political communication studies. The reliance on survey experiments, despite their value in providing controlled environments, might not accurately reflect the complexity of real-world communication dynamics. The inherent controlled nature of these experiments and their sensitivity to design choices could potentially restrict the comprehensiveness of their conclusions, thereby leading to divergent views on the persistence of political communication effects.

To corroborate these shortcomings the second study of this thesis presents a unique empirical design that examines political communication in the real world, using the 2016 EU-Turkey statement as a case study. This study provides a valuable contribution to our understanding of how political communication shapes public opinion by investigating the dynamics of communication effects in a real-world context. By utilising a quasi-experimental research design that leverages the timing of the EU-Turkey statement and subsequent events, this study captures the short-term and long-term impact of political communication on public sentiment. The research examines how the statement influenced people's attitudes toward refugees and asylum seekers. By examining these effects in real-world settings, the analysis provides a more comprehensive and context-sensitive understanding of how political communication can influence public sentiment.

The results of the study demonstrate that the EU-Turkey statement influenced respondents' perceptions of the crisis, their attitudes towards crisis management, and their policy preferences. This influence was most apparent among respondents surveyed one and two days following the statement, but gradually diminished thereafter. Despite the prominence of the European refugee crisis, the impact of the EU-Turkey statement was transitory, reinforcing the view that the effects of communication tend to dissipate swiftly in real-world scenarios. Yet, during the brief period when they are effective, these communication strategies can wield considerable influence on public sentiment. As a result, the analysis makes an important contribution to the study of public opinion by emphasising the significance of investigating political communication in real-world scenarios and demonstrating the need for more detailed exploration of communication effects on public opinion over time. To that end, the methodology used here can serve as a template for future research aimed at studying political communication in a real-world setting.

Political elites and the governance systems they operate within do not exist in isolation; rather, they are integral parts of multi-level governance systems. This is especially crucial to understand in the context of entities like the European Union (EU), where supranational structures are progressively assuming greater policy-making responsibilities. Hence, to fully grasp public attitudes towards democratic governance, it is necessary to discern how national democratic norms inform expectations at supranational levels, such as the EU.

De Vries' benchmark theory serves as a useful concept for understanding the relational nature of multi-level governance structures with a particular focus on the EU. It proposes the idea of an "EU differential," a process where individuals evaluate the benefits of the EU's current state against a potential alternative state outside of the EU. When the perceived benefits of the EU outweigh those of this alternative state, individuals are more likely to support the status quo, resisting any suggestions of leaving the EU. However, if the perceived benefits of an alternative state are greater, they show a tendency to favour changing the status quo.

The Interplay Between National Democratic Standards and Attitudes Towards the EU

In the third study of this thesis, I adapt benchmark theory to test how democratic practices at the national level shape expectation in EU democracy. As such this analysis attempt to provide a more nuanced perspective on the debate over a democratic deficit in the EU where critics have argued that as the EU takes on more responsibilities, it must meet the same democratic standards as nation-states. For example, Follesdal and Hix (2006) present a critical perspective on the state of democracy within the EU. The authors reject arguments suggesting that the EU is as democratic as it can or should be, but point out that key democratic elements are conspicuously absent. Particularly, the lack of contestation for political leadership and robust debates over the direction of policy is highlighted. Contestation for political leadership implies a democratic system in which diverse political groups compete openly for power and influence. Similarly, arguments over policy direction reflect a democratic practice in which diverse ideologies and strategies are proposed, debated, and refined. The noticeable absence of these essential democratic practices within the EU suggests a significant democratic deficit, underlining a negative differential between democracy at the national level and the EU level.

Meanwhile, Hix (2013) extends the argument that the EU suffers not from excessive politics but rather a lack thereof. It asserts an urgent need for greater democratic participation and transparency, particularly in key decision-making areas. Drawing a connection between rising distrust in the EU and national politics and the growing societal divide, this perspective suggests that the EU's democratic structures may not align with those at the national level, leading to a potential democratic deficit. Schmidt (2020) takes a different approach, delving into the democratic and legitimacy deficits in the EU by exploring concepts of policy-making and decision processes. It highlights a dichotomy of "policy without politics" at the EU level and "politics without policy" at the national level. Through its examination of institutional processes and practices, it underscores a shift from rigid economic orthodoxy to more discretionary decision-making within EU institutions. However, her analysis also identifies an escalating legitimacy crisis driven by increasing politicization and tensions at both levels, suggesting a democratic deficit at the EU level.

Furthermore, the study presented here introduces the idea that familiarity with an institution like the EU can moderate the comparison between national democratic standards and standards at the EU level. This moderation occurs due to the individuals' natural inclination to favour the known over the unknown (Hirshleifer, Jack and Riley, 1992). Based on this idea, I argue that over time, as citizens gain a more comprehensive understanding of EU democratic structures and processes, their perception of national and EU democratic practices undergoes a shift. I expect that this shift is marked by a growing preference for the EU's status quo and a receding certainty about the democratic practices outside the EU. This shift is not solely a function of time, but also individual engagement with EU affairs. Hence, a citizen's level of familiarity with the EU influences their perception of the democratic standards comparison, leading them to possibly prefer the EU's practices, and it also makes them less likely to support a departure from the EU.

Finally, to examine how differences in national democratic standards affect EU support and how this effect is moderated by familiarity with the EU and its institutions, the study employs two distinct research designs. First, at the macro-level, it assesses the relationship between national democratic qualities, membership time (as a proxy for familiarity), and aggregate EU support. This perspective is made possible by modern Bayesian IRT methodology. In particular, I use the data provided by Scotto di Vettimo (2022) who proposes a Bayesian IRT model as a superior method for estimating public EU support using aggregate-level data. The advantages of this technique are manifold. Firstly, the technique is grounded in theory, which helps handle neutral responses and better capture ambivalent attitudes. Secondly, it produces measures of public preferences that are comparable both over time and between countries. Finally, Bayesian IRT models can generate more precise estimates that align with established conceptualizations of EU support and are available and comparable across all EU member countries over an extended period of time. Second, I examine the extent to which national standards have an impact on individuals and the degree to which respondents' discussions of EU politics moderate this effect.

My findings suggest that people's familiarity with the EU plays a crucial role in the relationship between national democratic qualities and EU support. The effect of higher national-level democratic standards on EU support is found to be either insignificant or negative for people living in a recently joined EU country or those who do not discuss the EU, and thus are unfamiliar with it. However, when people are familiar with the EU because they occasionally or frequently discuss it or live in a country that has been a member for a long time, the effect is positive. This indicates that the EU has been quite successful in establishing a narrative of being a defender of democratic values among EU citizens. The analysis also shows that in most cases, higher democratic quality at the national level results in stronger EU support, even if there are higher differences from EU democracy. In a supplementary analysis, it is found that the higher a country's level of accountability, the less prevalent anti-EU sentiments are. Consequently, the detrimental role attributed to the EU's democratic deficits in recent literature may be exaggerated. People governed by highly functioning democracies appear to prefer a status quo within the EU, where the EU exercises some control over the democratic standards of its members over no control at all outside the EU.

Summary

In summary, this dissertation sheds light on critical aspects of the public-elite relationship in modern democracies. It introduces the concept of 'relative polarization,' which emphasizes the importance of individual political preferences in relation to available party options. This perspective adds to our understanding of how polarization affects democratic satisfaction and voter turnout. Furthermore, the analysis demonstrates the temporary but substantial effect of political communication on public sentiment. It provides an alternative to traditional survey experimental designs by demonstrating how to test this relationship in a real-world setting. Finally, the thesis investigates how elite-shaped national democratic standards form expectations in supranational organizations, and how people's familiarity with these supranational organizations is critical to understanding this relationship. It accomplishes this by employing a cutting-edge aggregate measure of attitudes toward the EU, which enables perspectives with previously unattainable range and comparability.

Chapter 2

Polarisation - The Boon and Bane of Democracy

Democracy's survival hinges on both public support (Claassen, 2020a) and electoral engagement, but in increasingly polarised societies bolstering one might imperil the other. Political elites who attempt to respond to their increasingly polarised populace may find themselves in an almost impossible position. Existing research, indicates that elite polarisation can, on the one hand, lead to citizens dissatisfaction with their democratic regime when available policy options are incongruous with the median voter position (Ezrow and Xezonakis, 2011). On the other hand, citizen are more engaged, i.e. more likely to vote, in highly polarised systems (Wilford, 2017).

These potentially divergent effects of elite polarisation pose a severe challenge to democratic governance. For a clearer comprehension of this significant obstacle to democratic governance, the study at hand introduces the concept of *relative polarisation*. This individual-level polarisation metric allows for the assessment of the impact that individual perceptions of polarisation may have on individual attitudes and behaviour.

The introduction of relative polarisation addresses key limitations of the currently available evidence on the diverging effects of polarisation. Firstly, existing evidence has examined the effects of elite polarisation on turnout or democratic satisfaction in distinct studies using different data resources and methodological approaches, which do not allow for a direct comparison of these results. Secondly, in efforts to relate citizens' prefer-

ences and elite polarisation, many scholars have relied on party positions from manifestos or expert surveys. This method does not adequately reflect polarisation as perceived by citizens. Furthermore, such approaches necessitate aggregation, which eliminates the insightful within-country variation in individual perceptions of polarisation.

Relative polarisation, by contrast, combines citizens preferences and perceived elite positions at the individual level. I measure relative polarisation using respondents' left-right self-placement as well as their placement of political parties on the same scale. This approach captures polarisation perceptions without requiring respondents to grasp the concept of polarisation. Furthermore, as opposed to approaches that use manifesto or elite evaluations of party positions, we know that there is no difference in the perception or understanding of the left-right scale because we only compare respondents' self-placement on the left-right scale with their personal evaluation of their available party choices using this approach. This method of measuring relative polarisation combines the frequently distinct perspectives of mass and elite polarisation, providing a new perspective on how citizens perceive polarisation.

The study of relative polarisation presented here is made possible by data from the Comparative Study of Electoral Systems (CSES). The available data allows to calculate relative polarisation and test its effect on turnout and democratic satisfaction across 34 democracies and 25 years. For further robustness, I also test these effects using a first-differences models and panel data from the British Election Study.

My results show that even when tested on the same sample, the effects of voter participation and democratic satisfaction diverge. The greater relative polarisation, the less satisfied respondents are with democracy. In contrast, but consistent with previous findings, polarisation has a positive effect on voter turnout. However, the effect of relative polarisation on respondents' satisfaction with democracy is moderated by the government status of their closest party. People whose preferred party is in power are either not affected by relative polarisation or are only affected to a small degree. There is no comparable moderating effect for relative polarisation and turnout; as polarisation increases,

all citizens are equally incentivised to vote. In summary, the dual effects of political polarisation become apparent. On one hand, it emerges as a boon, engaging the electorate and heightening voter participation. On the other hand, it proves a bane, by instigating a decline in overall satisfaction with democratic governance.

2.1 The Interplay of Masses and Elites: Polarisation and Congruence

Polarisation is one of the central challenges facing modern politics. In the literature, polarisation is frequently conceptualized and measured at both mass and elite levels, which represent two distinct yet highly interdependent dimensions (Enders, 2021; Hill and Tausanovitch, 2015; Jennings, 1992).

While elite polarisation is almost unanimously understood as a situation or process in which political elites have become more ideologically distant from one another (DiMaggio, Evans and Bryson, 1996), the scholarly debate surrounding ideological mass polarisation is polarised in itself (Lelkes, 2016). Some scholars allege that the United States, in particular, is embroiled in a cultural war (Abramowitz, 2010; Abramowitz and Saunders, 2005; Abramowitz and Saunders, 2008), while others counter that these assertions are overstated (**fiorina2008polarisation**; Fiorina and Levendusky, 2007; Levendusky, 2009; Fiorina, Abrams and Pope, 2011). These differences are to some extent due to differences in conceptualisation. For the former group, polarisation can be defined as consistency, which refers to the degree to which party identity increasingly matches party ideology and the degree to which attitudes become more internally consistent. In contrast, polarisation can also be defined as divergence or the degree to which the distribution of ideology has moved apart by those who reject the notion of a cultural war.

The divergence of ideology is one perspective scholars use to contrast elite and mass polarisation. For example, Zaller (1992) made one of the earlier theoretical arguments on the interplay of mass and elite polarisation. He proposes that higher elite polarisation will lead citizens, particularly more politically sophisticated citizens, to gravitate towards increasingly distant political poles, causing political masses to be equally polarised. Later

contributions provided empirical evidence on mass responses to elite polarisation. A key contributor is Druckman, Peterson and Slothuus (2013), who uses two survey experiments in the United States. His findings demonstrate how elite polarisation increases the impact of party endorsements on mass sentiment. It's also shown that this polarisation decreases the impact of substantive information. Additionally, it's established that elite polarisation fosters greater confidence in less substantively grounded opinions. Likewise, in Latin America, even after adjusting for other party system characteristics, such as the age of the party system or electoral fragmentation, the correlation between voters' self-placement on the left-right scale and their electoral choice is greater in divided party systems (Singer, 2016).

However, it is unclear whether, over time, this influence of elite polarisation on the masses is sufficient to polarise the public in a way that could compromise democratic governance, as some scholars have argued (Levitsky and Ziblatt, 2019). For example, using data from the American National Election Study, Levendusky (2009) shows that increases in mass polarisation are small and only observable over longer time periods. The network analysis by Della Posta (2020) shows that attitudes have shifted in a way that may exacerbate conflict, but a substantial number of cross-partisan issues have remained stable. In accordance with Zaller's original argument, voter sophistication can also play a role as citizens do not mindlessly follow party elites. Depending on elite positions, the degree of political polarisation, and the personal significance of issues, the public may be attentive to information and avoid party elites' influence (Mullinix, 2016).

The Diverging Effects of Polarisation

The evidence presented in the previous section demonstrates that the masses are not completely under the thumb of the elites and are quite capable of resisting elite persuasion. Can we, therefore, simply dismiss the threat that polarisation poses to democracy?

To answer this question, we must understand the direct impact of citizens' perceptions of polarisation on key pillars of democratic governance, such as participation, and overall satisfaction with democracy as a political system overall, rather than merely considering how much elite polarisation drives mass polarisation. Empirical evidence on the relationship between elite polarisation and these indicators of democratic health, however, diverges significantly.

On the one hand, it has been argued that polarisation among elites reflects polarisation among the engaged public, and encourages involvement among members of the mass public, who react with increased engagement and more clearly defined political preferences (Abramowitz, 2010). Scholars provide extensive macro-level evidence that elite polarisation increases interest and participation (Abramowitz and Stone, 2006; Dalton, 2008; Abramowitz and Saunders, 2008; Aldrich, 1993). For example, Wilford (2017) shows across 26 OECD countries that highly polarised systems with few parties encourage individuals to vote, whereas low levels of elite polarisation and a large number of parties reduce voting incentives. Furthermore, the perceived polarisation of voters within the political landscape increases a citizen's likelihood of voting (Muñoz and Meguid, 2021). Siaroff and Merer emphasise the role of political polarisation in their study on European parliamentary elections since 1990. The study reveals that countries with strictly polarised two-party systems tend to experience higher voter turnout, especially in regions like East-Central Europe where low turnout has been a persistent challenge. Hobolt and Hoerner emphasize congruence as a pivotal condition in understanding the relationship between polarization, party choice, and voter turnout. Their unique approach combines cross-national analysis of individual turnout in 80 legislative elections across 27 countries with a case study on the AfD's mobilisation in Germany. Their findings underscore that higher voter turnout is contingent upon not only parties providing more choices but also voters aligning themselves with a party's positions, highlighting the novel and crucial role of congruence in influencing voter participation (Hobolt and Hoerner, 2020).

In the literature on democratic satisfaction, the prominence of congruence is well-established, with scholars frequently finding that the greater the difference between voter preferences and party positions, the lower the level of satisfaction among voters with democracy (Ezrow et al., 2011; Reher, 2015; Ferland, 2021; Kim and Fording, 2002; Dahlberg and Holmberg, 2014). For example, Stecker and Tausendpfund (2016) demonstrate that voters are less satisfied with democracy if their government's policy position is distant from them. Additionally, Ferland (2021) differentiates between different kinds of policy congruence and finds positive effects of government, party, and enacted policy congruence. Finally, Ezrow and Xezonakis, 2011 considers the role of polarisation more directly and finds that citizens are more satisfied with democracy as a whole when policy options in the political system are organised around the median voter position.

However, polarisation may increase the available choices and, depending on the relative position of each citizen, may improve the congruence between citizens and elites. This tendency exemplified in research on vote-seeking parties shows that, while ideological convergence of parties may increase congruence between governments and the median voter, it can also reduce congruence between the party system and the electorate as a whole Laver and Sergenti, 2011; Laver, 2011; Brandenburg and Johns, 2014. Especially concerning the use of the median voter as a point of reference, when assessing the impact of mass-elite congruence on democratic satisfaction, it can be problematic. For instance, research has shown that citizens tend to be indifferent towards sociotropic congruence, which describes the alignment between the general populace and elected officials in terms of ideology. In contrast, citizen display increased satisfaction in cases of egocentric congruence, i.e the alignment of an individual citizen with party choices Mayne and Hakhverdian, 2017. These egocentric tendencies could also explain the puzzling finding that at macro-level the composition of party choices has no effect on citizens satisfaction with democracy (Dassonneville and McAllister, 2020).

Another limitation of using a macro approach to measuring elite polarisation and its consequences for citizen satisfaction is that it omits information on who the electoral process's winners and losers are. This omission can be highly consequential (see for example Blais and Indridason, 2007; Blais, Morin-Chassé and Singh, 2017; Hobolt, Hoerner and

Rodon, 2021; Singh, Lago and Blais, 2011). As Blais, Morin-Chassé and Singh shows, voting for parties gaining more votes and seats overall boosts democratic satisfaction. Using a combination of multi-level cross-national data and an experimental design, Hobolt, Hoerner and Rodon demonstrate the importance of considering not only the extent to which parties reflect citizens' preferences but also their capability to influence policies. Their findings revealed that citizens' satisfaction with democracy is influenced by the congruence of their chosen party and its potential to enact policies. Specifically, citizens who vote for a party that aligns with their ideology and has a promising likelihood of policy implementation express higher satisfaction with democracy. However, just having an ideologically congruent choice, without the potential for policy execution, does not yield the same satisfaction levels.

In light of the literature discussed, it becomes evident that we require a measure of polarisation, that test polarisation at the individual level and takes relative position of citizen into account to account for the intricate dynamics of individual voters and their nuanced relationships with the available party choices.

Based on the literature discussed, it is clear that we need a measure that assesses polarisation at the individual level, considering citizens' perceptions and relative positions. Such a metric would allow for a more accurate accounting of citizens' nuanced relationships with available party options, as well as individual level factors such as winner-loser dynamics and citizen egocentric tendencies.

2.2 Relative Polarisation

Existing studies have recognised the need for a more nuanced individual-level measure of perceived polarisation, especially in research on the United States. For example to measure perceptions of polarisation, the American National Elections Study (ANES) has used a series of questions asking respondents to rate each party on a 7-point scale on

a wide range of issues since 1970. These questions are used by Westfall et al. (2015) to evaluate the country's overall perceived polarisation. shows that the perceived distance between the two parties has grown from slightly more than 1 point in 1968 to slightly less than 2 points in 2008.

However, this method of measuring perceived polarisation only captures perceptions of elite polarisation but does not link those perceptions to respondents' preferences. Muñoz and Meguid (2021) present an individual-level measure of perceived polarisation that relates to voter preferences and perceived party positions they call relative polarisation. They measure relative polarisation by calculating the difference between a voter's preferred candidate and the opposing candidate in the 2012 and 2017 French presidential elections. Using their novel approach, the authors show that putting the positions of the masses and elites into perspective is critical for understanding responses to polarisation. The positive effect of higher polarisation on turnout is shaped not only by elite polarisation but also by the voter's position relative to their policy options.

The idea of relative polarisation is similar to what Downs (1957) calls "party differentials". Voters will select their best party option by comparing the utility they believe they would receive if each political party were in power. In a two-party case like the example presented by Muñoz and Meguid (2021), the difference between the utilities of the incumbent and the opposition is the citizen's expected party differential. If the differential is positive, they vote for the incumbent; if it is negative, they vote for the opposition; and the greater the resulting party differential between the two party options, the greater the relative polarisation. Thus, in a two-party system, given that we have data on voter perceptions of party position, party differentials or relative polarisation can serve as an effective individual-level measure of perceived polarisation that contrasts voters' preferences with the options provided by political elites.

However, measuring party differentials or *relative polarisation* is more complex in multi-party systems, and neither Downs nor Muñoz and Meguid account for these more complex cases. Voters will no longer be able to draw a simple differential between the two available parties. Instead, I expect them to view the party system as a set of options. Assuming that a voter's utility from a party is a function of the distance between the voter's preference and the party's position, I expect voters to perceive their party preferences as a set of party distances D_{ijt} which can be described as follows:

$$D_{ijt} = |P_{jt} - v_{ijt}|; \tag{2.1}$$

where P_{jt} describes the set of available party positions in a country j at a point in time t, and v_{ijt} the position of a voter i.

From this follows that there must exist a closest party $c_{ijt} \in P_{jt}$ such that party distance $d \in D_{ijt}$ is minimal. As a result, the closest party is defined as the one with the smallest ideological distance. Furthermore, there must exist a set of alternative party options A_{ijt} for which $A_{ijt} \subset P_{jt}$ and $c_{ijt} \notin A_{ijt}$ holds. Consequently, *relative polarisation* in a multi-party system can be described as follows:

$$rp_{ijt} = \sum_{a \in A_{ijt}} (|a - v_{ijt}| - |c_{ijt} - v_{ijt}|)w^a, \tag{2.2}$$

where $w \in W^A_{jt}$, and W^A_{jt} is a set of weights that correspond to the party alternatives, such that each party position a has a corresponding weight w.[1] Ultimately, *relative polarisation* captures perceived polarisation as the weighted average spread that each voter experiences between their closest party and the available party alternatives.

1. In this case parties are weighted by their vote share in the most recent elections.

2.3 Hypotheses and Mechanism

I have discussed the interplay of mass and elite polarisation, the divergent effects of elite polarisation on democratic health, and the necessity for a measure of individual-level perceived polarisation in prior sections. Now, I will describe and demonstrate the mechanism and my hypotheses regarding the underlying relationship between perceived levels of polarisation and two crucial components of democratic health: turnout and democratic satisfaction.

Downs (1957) already considered the relationship between polarisation and turnout and concluded that as polarisation increases, individuals' stakes in the election outcome rise, leading to increased interest and participation. I anticipate that this notion of stakes in the political process likewise applies to citizens' overall assessment of the political system based on the observation that there are significant differences between electoral winners and losers, as those who voted for the winning party are frequently more satisfied with the functioning of democracy than those who did not (Blais, Morin-Chassé and Singh, 2017; Singh, Lago and Blais, 2011; Blais and Indridason, 2007). However, turning out to vote as a citizen's decision to exert effort to influence the political status quo, and satisfaction with democracy as a citizen's overall assessment of the status quo, are two fundamentally different cognitive processes. Therefore, we must consider two distinct mechanisms by which polarisation and the increased stakes it produces affect individuals' likelihood to vote and overall satisfaction with their democratic regime.

Both mechanisms are based on the assumption that voters are rational actors. For turn out, in line with, Downs (1957) original conception this means that voters will weigh the benefits and costs of voting. If the benefits of voting (i.e., the satisfaction or expected policy change from voting for a preferred candidate) outweigh the costs of voting (i.e., time, effort, opportunity costs), then a person will choose to vote. As a citizen's level of *relative polarisation* increases, the voters' number of low utility party options increases, raising the stakes in the electoral process. As a consequence, their utility of voting rises

because their vote helps to reduce the possibility that candidates from low utility parties will win office. On the contrary, when perceived polarisation is low and all options in the party system have more similar utility, the cost of voting outweighs the benefits of voting, and people are more likely not to vote. Consequently, I conclude with the first hypothesis:

Hypothesis 1: The greater respondents levels of relative polarisation, the more likely they are to vote.

In contrast, I argue citizens' satisfaction with democracy is a function of the utility that all available party options provide. This utility is given by the benefits a voter derives from being represented by the best party option and the disutility of the party alternatives, with the disutility of the alternatives moderated by whether the best choice in government or not. Consequently, when *relative polarisation* is high, the respective political system is populated with more low-utility party options, resulting in a lower overall utility of the political system. From this follows:

Hypothesis 2: The greater an individual's level of relative polarisation, the lower her satisfaction with democratic governance.

Several scholars have, however, demonstrated that it is not solely congruence that influences voters' satisfaction with democracy but also the parties' ability to shape policy (Blais and Indridason, 2007; Blais, Morin-Chassé and Singh, 2017; Hobolt, Hoerner and Rodon, 2021; Singh, Lago and Blais, 2011). Therefore, we can anticipate that when a citizen's preferred party is in power, the negative impact of relative polarisation is negated because the less favoured party options exert significantly less influence over policy outcomes. From this observations follows:

Hypothesis 3: For respondents whose preferred party is in government, the effect of relative polarisation is insignificant.

Figure 2.1 depicts a fictional example to illustrate the presented mechanism. In both cases, the citizen has a single party that perfectly represents her, and her position remains consistent throughout both examples. However, in scenario A, the citizens' party alternatives are significantly further away from the voter's position than in scenario B, where they are more closely organised around her preference. When comparing the two scenarios, we

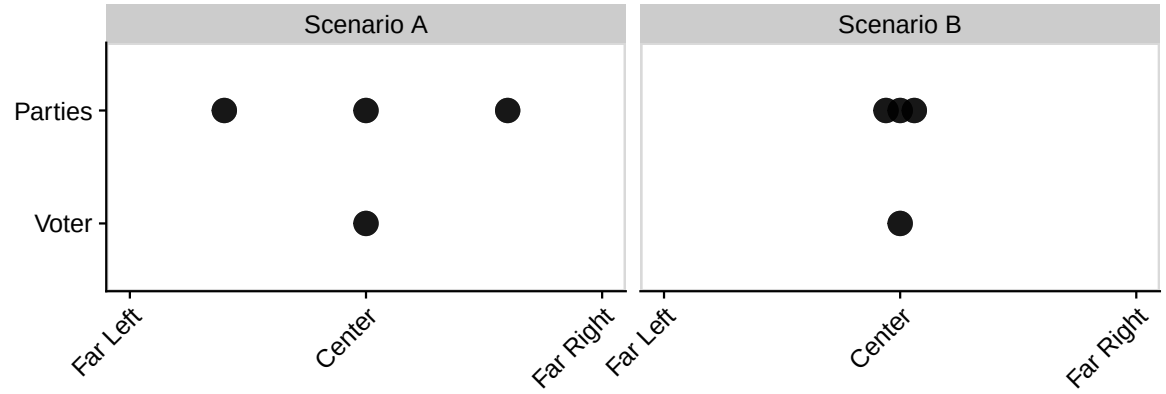

Figure 2.1: Fictional Example

would expect the citizen to be more motivated to vote but less satisfied with democracy in scenario A, where the greater spread of the alternative increases the likelihood that a more distant party option will be elected. In scenario B, on the other hand, we would expect the stakes for the citizen to be low because the party alternatives also capture her preferences well, and she will thus have little incentive to vote but will be extremely satisfied with the regime overall.

A case more similar to scenario B occurred in Spain in 2000, where the People's Party (PP) won a majority of seats after having governed a minority government. The PP minority government's economic and social policies were credited for legislative stability. Spain unexpectedly qualified for the Economic and Monetary Union under Rodrigo Rato, Minister of Economy and Finance, after demonstrating annual GDP growth of nearly 3.5 per cent between 1996 and 1999, while inflation fell from 5.3 per cent to 2.6 per cent (Chari, 2000). As a result, the stakes were low in this election, and there was little contestation over policy during this period, resulting in the lowest average level of relative polarisation recorded in the country in 2000 (see Figure 2.3). As shown in Figure 2.2, it is not surprising that the 2000 elections in Spain had the lowest voter turnout ever recorded, while satisfaction with the democratic regime was exceptionally high.

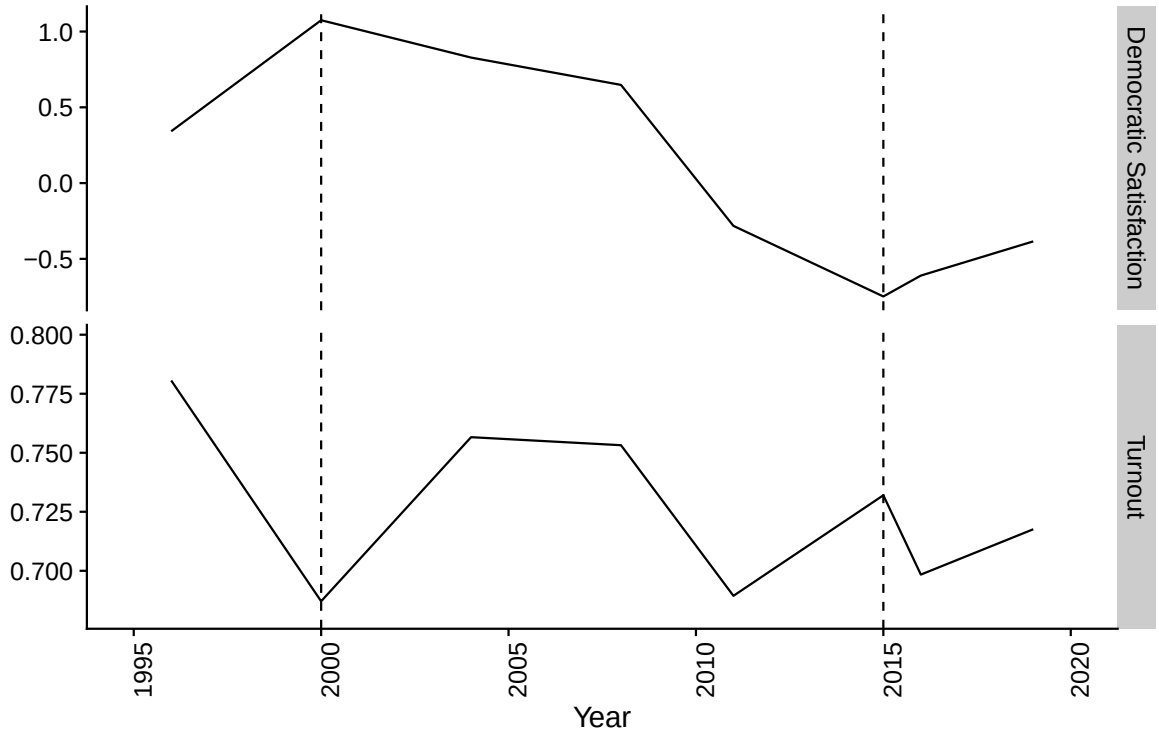

Figure 2.2: Real World Example: Spain

I use the supplementary data provided by Claassen (2020b) as a measure for satisfaction with democracy and turnout data provided by International IDEA (2022)

In contrast, during the 2015 general elections in Spain, a scenario comparable to scenario A (in Figure 2.1) occurred. This election represents a significant increase in elite polarisation as a result of the dissolved bipartisan structure following the severe economic recession between 2008 and 2013, as well as the transformation of once-nationalist parties into secessionist parties (Alvarez, 2019). As a result, Spain had the lowest level of democratic satisfaction in 2015, while turnout peaked.

2.4 Data and Methodology

For the analysis at hand, I utilise two survey data sources. First, I use data from the Comparative Study of Electoral Systems (CSES) and harmonise their integrated module (Center For Political Studies, University Of Michigan, Ann Arbor, 2019, 2020) and module 5. Data is available for 34 countries [2] and a time period between 1996 and 2020. Second, I use panel data from the British Election Study (BES) (Fieldhouse et al., 2021), which allows me to run a first-difference model across 21 waves from February 2014 to May 2021. Testing my hypotheses across these two data resources provides exceptional robustness as the CSES provides an extended time period and cross-country comparison, resulting in high external validity while the BES's panel data

Dependent Variables: Turnout and Democratic Satisfaction

At the heart of this analysis is the comparison of two dependent variables: Voter turnout and satisfaction with democracy. For both variables, I use existing survey items. Satisfaction with democracy is captured by both the BES[3] and the CSES[4] across all of their waves. The outcome variable satisfaction with democracy was recorded to a scale of 0 to 100. Thus, one scale point difference represents one percentage point of the variable's theoretical range.

The CSES collects whether respondents will or did vote in the upcoming or most recent election, depending on the time of data collection. This variable is a composite of survey items that capture turnout for the most recent national-level election, which could be a parliamentary or presidential election. The BES captures turnout as the respondents' self-declared likelihood to vote,[5] which was surveyed across all panel waves.

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2. The final sample used includes the following countries: Australia, Austria, Belgium, Bulgaria, Canada, Croatia, Czechia, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, Latvia, Lithuania, Netherlands, New Zealand, Norway, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, Switzerland, Turkey and the United Kingdom
3. Question text: *On the whole, how satisfied or dissatisfied are you with the way that democracy works in: The UK as a whole*
4. Question text: *On the whole, are you very satisfied, fairly satisfied, not very satisfied, or not at all satisfied with the way democracy works in COUNTRY?*
5. Question text: *Many people don't vote in elections these days. If there were a UK General Election tomorrow, how likely is it that you would vote?*

Independent Variable

For the calculation of *relative polarisation*, I use ideological left-right positions. While relative polarisation can be applied to any policy dimension, we need a data source that captures voter self-placement on a policy scale as well as party perceptions on that scale. To my knowledge, such data is only available for the left-right dimension over long time periods and across multiple countries. The left-right political dimension is also a common way to communicate party stances that even less politically sophisticated voters will be familiar with. The fact that left-right positions capture multiple policy dimensions and are widely available across numerous waves in both the BES and CSES makes them ideal for the analysis at hand.

Scholars are correct to criticise the wide variation in respondents' associations with the abstract concepts left and right (see for example Bauer et al., 2017), but we are interested in *perceived* polarisation, of which this variation between voters is a key component. The presented method for measuring relative polarisation contrasts self-positioning with the voter's own perception of party position, which is comparable because they are subject to the same individual variation and biases.

To create the final individual-level left-right position in the BES, I create an index from questions on attitudes towards redistribution, big business, wealth distribution, and employment. Likewise, respondents are asked to locate political parties on an eleven-point left-right scale. The CSES captures left-right voter positions as well as voter perceptions of party positions on a ten-point scale.

The perceived party positions as collected by the CSES and BES have the significant advantage of ensuring that there is no mismatch between voter perception and an external evaluation of party positions, such as the Chapel Hill Expert Survey data or the Manifesto Project's *Standard Right-left Scale*. While these measures are likely to capture party positions more accurately, it is more important for an individual measure of polarisation

that the individual's perception of the parties' positions, rather than their actual position, is reflected in the measure. Furthermore, measuring the position on the same scale eliminates the need to match the scales or make any other adjustments that compensate for different data generation processes.

The final measure of ideological left-right positions of parties and voters for both data sets are recorded on a scale of zero to ten. If all available party options are indistinguishable, the resulting *relative polarisation* measure is zero. It rises in proportion to how distant the closest party choice to the voter is on average from the other party options available.

In addition, I use a dummy variable to capture if a respondent's preferred party is currently in government to test whether relative polarisation affects individual citizens' satisfaction with democracy and turnout differently depending on their preferred party's government status. Similarly, I also test a dummy that captures whether the current head of government is of the preferred party. I will interact these dummy variable with the main effect of relative polarisation to test if the effect of relative polarisation differs for respondents whose preference party is in government or the party of the current head of government.

I expect that the perceptions of polarisation captured by relative polarisation differ across individuals with different socio-demographic characteristics, and because the CSES only provides repeated cross-sectional data, I control in the model for the respondent's age, education, gender, and income.

Furthermore, the purpose of this study is to capture responses in attitudes toward and engagement in democracy to changes in polarisation; however, citizens' predispositions regarding the role their government plays in their lives will bias these indicators of democratic health. Consequently, to control for respondents' perceptions of the importance of governance, I additionally control for government efficacy, which the CSES measures by asking respondents whether who is in power matters.[6] It is possible to make the case that democratic health indicators like democratic satisfaction and turnout are highly correlated with government efficacy, which would cause a multicollinearity problem. However, while

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6. Full question text: *Some people say that no matter who people vote for, it won't make any difference to what happens. Others say that who people vote for can make a big difference to what happens. Using the scale on this card, (where ONE means that voting won't make any difference to what happens and FIVE means that voting can make a big difference), where would you place yourself?*

I do expect a potential bias from perceptions of government efficacy, I argue that it is a distinct concept, as evidenced by the correlation between government efficacy and satisfaction with democracy, which is 0.09, and the correlation between government efficacy and turnout, which is 0.12 (based on the CSES data).

Modeling

Both the CSES and BES have hierarchical data structures that need to be accounted for. While CSES provides data for a period of 25 years, they only collect data on a sporadic basis across different country contexts. Treating the country and year as random effects allows us to account for the variability in the country effect caused by the CSES data collection process. To also account for the different scales of the outcome variables, I use a mixed-effect linear model for the impact on satisfaction with democracy and a mixed-effect logit model for the impact on turnout when analysing the effects of relative polarisation.

In the BES panel data, there is the possibility of serial correlation, as turnout and satisfaction with democracy may be affected by their previous levels. I run a Breusch-Godfrey test on the BES panel data to test for potential serial correlation, and both turnout and satisfaction with democracy show significant results.[7] Consequently, I run first-difference models that account for serial correlation, instead of an alternative fixed effects model.

2.5 Descriptive Statistics: Relative versus Elite Polarisation

In this section, I discuss the connection between elite polarisation and relative polarisation before moving on to the core analysis of this contribution, which tests the relationship between relative polarisation and democratic health.

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7. Satisfaction with Democracy: $\text{chi}^2 : 330.55$, $p < 0.001$; Turnout: $\text{chi}^2 : 636.51$, $p < 0.001$

Figure 2.3 depicts aggregate relative polarisation along elite polarisation in 11 different countries based on ideological left-right positions. Using data from the Chapel Hill Expert Survey (Jolly et al., 2022), I calculate the variance weighted by party vote share for each country and year to get a better estimate of actual elite polarisation. Relative polarisation is represented by the mean value of relative polarisation surrounded by a one standard deviation difference interval. Figure 6 in the appendix contains aggregates for relative polarisation for all countries in the CSES sample.

While elite polarisation varies significantly over time, changes in aggregate relative polarisation over time are moderate. Furthermore, relative polarisation only responds inconsistently to elite polarisation. For example, in the United Kingdom, there has been a significant increase in relative polarisation since 2015, which is most likely due to Jeremy Corbyn taking over as Labour leader and steering the party to the left. Relative polarisation rises in response to this increase, beginning in 2015. A similar trend can be seen in Sweden, where elite polarisation increased in 2015, albeit not to the same extent, likely owing to the Sweden Democrats' – a radical-right party – increasing success. Here, we can also see a corresponding rise in relative polarisation.

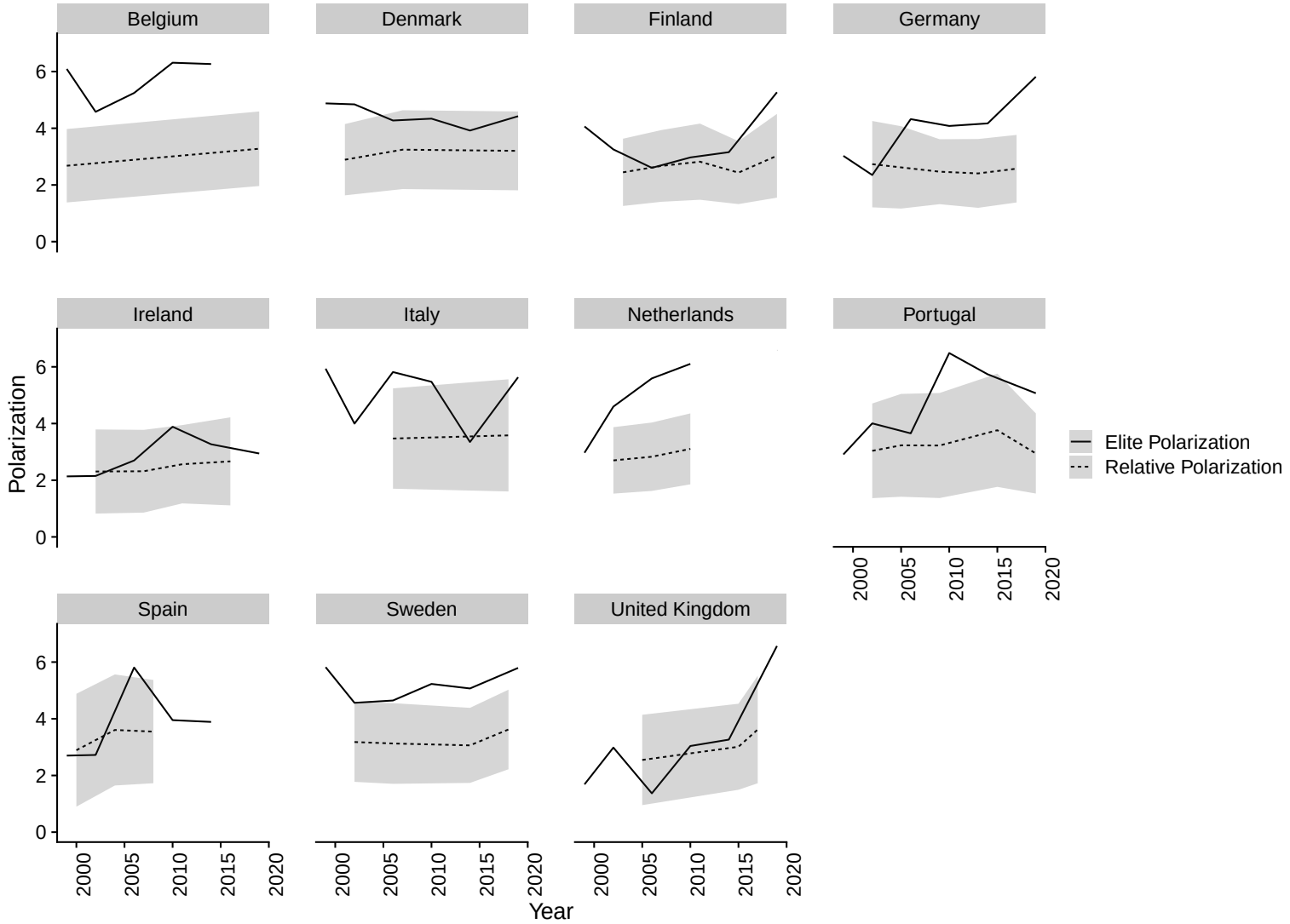


Figure 2.3: Country-Year variation of Relative Polarization versus Elite Polarization

Both variables ranges from 0 (no polarization) to 10 (high polarization). The interval of the relative polarization line shows a one standard deviation difference from the country mean.

In contrast, we can see significant fluctuations in elite polarisation in Italy over the entire period, whereas average relative polarisation only varies marginally. Similarly, we can see a peak of elite polarisation in Ireland around 2010, which was most likely a reaction to the European debt crisis, but there are no responses in the level of relative polarisation, which captures the individual-level perception of polarisation. Furthermore, there is the previously discussed case of Spain, where we can find, as expected, a steep increase in elite polarisation since 2000, most likely as a result of the country's slowly weakening bipartisan structure. While there is a slight increase in relative polarisation until 2005, there is almost no change in relative polarisation when elite polarisation peaks around 2008.

These varying descriptive findings suggest that while the perceptions of polarisation captured by relative polarisation are partly driven by elite polarisation, there must be other factors that drive them. Consequently, I examine this dynamic at the individual level using the CSES data described previously. I regress relative polarisation on elite polarisation in addition to a set of demographic and attitudinal controls. For this purpose, I employ a linear fixed-effects regression model with country and year fixed-effects and country-clustered standard errors.

According to the regression results in Table 2.1, the level of elite polarisation has a positive effect on relative polarisation. Considering that both variables are on a ten-point scale, however, the effect size of elite on relative polarisation is rather small, with only a 0.09 point change in relative polarisation for every point change in elite polarisation.

In contrast, the respondent's demographic characteristics show comparable and even larger effect sizes. For instance, age has a positive effect on perceptions of polarisation, as for every year a respondent is older, his level of relative polarisation will increase by 0.04 points on average. Similarly, women will have a 0.08 scale higher perception of polarisation, whereas income has a negative effect on polarisation, with each higher income bracket the respondents will have a 0.05 scale point lower level of relative polarisation.

Dependent Variable:	Relative Polarization
Model:	(1)
Variables	
Elite Polarization	0.0863**
	(0.0396)
Gender	0.0836***
	(0.0273)
Income	-0.0529***
	(0.0097)
Age	0.0400***
	(0.0119)
Education	-0.0099
	(0.0140)
Government Efficacy	0.1488***
	(0.0195)
Fixed-effects	
Country	Yes
Year	Yes
Fit statistics	
Observations	54,218
R^2	0.07270
Within R^2	0.02108

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 2.1: Fixed-Effects Model: The effect of Elite on Relative Polarisation

In addition to the countries depicted in figure 2.3, this model also incorporates data for Austria, France, and Greece, for which data was only available for one or two time periods.

Lastly, I examine the effect of the perception of government efficacy on relative polarisation. This metric will show if respondents' perspectives on the government's ability to make a difference in their lives affect their perception of democracy. We can see that perceived government efficacy has an effect on relative polarisation, with each scale point increase in perceived government efficacy increasing relative polarisation by 0.15 points.

These findings demonstrate that perceptions of polarisation are complex and influenced by a variety of factors that extend beyond the country's actual level of elite polarisation. Furthermore, the average citizen is not a party expert and perceives changes in polarisation more moderately than they are, according to expert judgement. If we want to understand public reactions to polarisation, we must use a measure like relative polarisation that captures the perception rather than the reality of polarisation.

2.6 Results: Satisfaction with Democracy

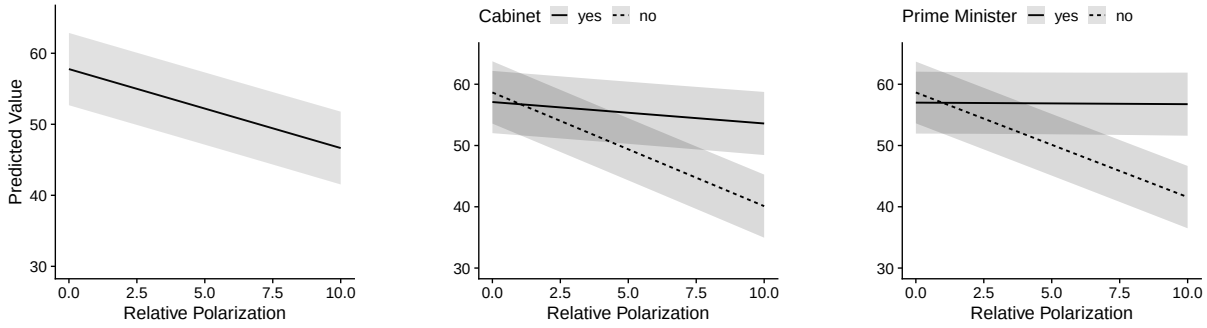
In this section, I investigate the impact of relative or perceived polarisation on satisfaction with democracy. Using first the CSES data, Table 2.2 reports the regression results of the linear mixed-effects model regressing relative polarisation on satisfaction with democracy while controlling for a set of demographic confounders. The first model presented depicts the effect of relative polarisation, while the other two depict the interaction of relative polarisation and dummies that capture whether the respondents' closest vote choice is in the cabinet or the party of the head of government.

Figure 2.4 depicts the predicted values for relative polarisation across the three presented models for ease of interpretation. We can observe that relative polarisation has a negative effect in the base model. These findings support the expectation that when relative polarisation is low, respondents are more satisfied with the way democracy works; however, the effect is small. Figure 2.4a shows that the difference in satisfaction with democracy between the highest and lowest possible levels of relative polarisation is only ten scale points.

Table 2.2: Mixed-Effects Model (CSES): Outcome - Satisfaction with Democracy

	(1)	(2)	(3)
Relative Polarization	-0.980^{***}	-0.214^{***}	0.164^{**}
	(0.047)	(0.064)	(0.079)
(not) Cabinet		1.502^{***}	
		(0.322)	
Relative Polarization * (not) Cabinet		-1.503^{***}	
		(0.092)	
(not) Prime Minister			1.584^{***}
			(0.340)
Relative Polarization * (not) Prime Minister			-1.719^{***}
			(0.096)
Gender	0.052	0.073	0.079
	(0.146)	(0.146)	(0.146)
Income	1.809^{***}	1.782^{***}	1.784^{***}
	(0.056)	(0.056)	(0.056)
Age	-0.037	-0.056	-0.061
	(0.045)	(0.045)	(0.045)
Education	0.306^{***}	0.343^{***}	0.341^{***}
	(0.054)	(0.054)	(0.054)
Government Efficacy	0.927^{***}	0.919^{***}	0.889^{***}
	(0.053)	(0.053)	(0.053)
Constant	47.244^{***}	46.636^{***}	46.594^{***}
	(2.567)	(2.566)	(2.549)
Random-Effects			
Countries	34	34	34
Years	25	25	25
Observations	136,700	136,700	136,700
Log Likelihood	$-646{,}706$	$-646{,}353$	$-646{,}318$
Akaike Inf. Crit.	1,293,432	1,292,730	1,292,661
Bayesian Inf. Crit.	1,293,531	1,292,848	1,292,779
Note:	*p<0.1; **p<0.05; ***p<0.01		

The results of the subsequent two models suggest that the effect of relative polarisation is moderated by the status of a respondent's closest party choice. In both cases, the interaction term with relative polarisation and the dummy indicating whether a respondent's closest party is in the cabinet or providing the prime minister are significant. The predicted values show that for those respondents for whom their closest party choice is part of the cabinet or providing the prime minister, the effect of relative polarisation is very small. For the former, we observe a two-scale point decrease, while the latter even shows a four-scale point increase in satisfaction with democracy between the lowest and highest level of relative polarisation.



(a) No Interaction (b) Cabinet (c) Prime Minister

Figure 2.4: Predicted Values Mixed-Effects Model: Outcome: Satisfaction with Democracy

In contrast, we observe a larger effect for respondents whose closest party is not in the government. Figure 2.4b shows that those respondents have a 25-point difference between the highest and lowest level of relative polarisation, and figure 2.4c shows that if the prime minister is not a member of the respondents' closest party, these respondents are 20 points less satisfied with democracy if they experience the highest level of relative polarisation. Table B.1 in the appendix shows an alternative more conservative operationalisation of the model using country and year fixed effects as well as country clustered standard errors, which shows the same directionality of the effects with an overall negative effect of clustered standard errors on satisfaction with democracy.

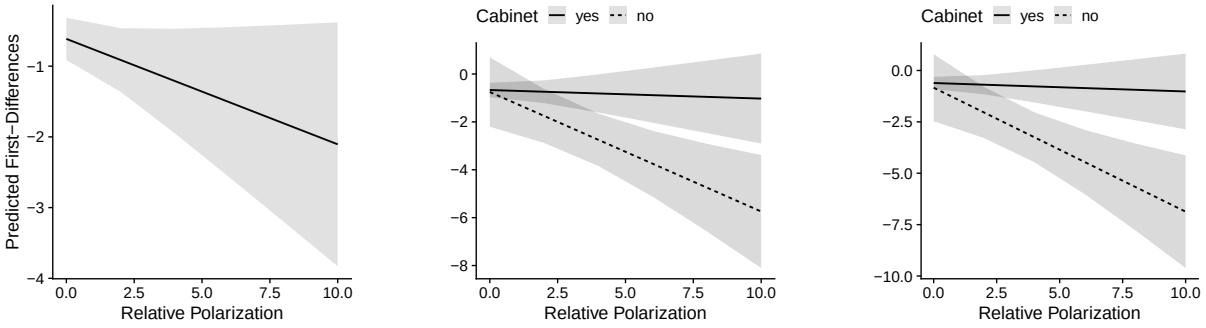
While the presented repeated-cross-sectional results span 34 countries and 25 years, providing a high level of external validity, attitudinal variables such as satisfaction with democracy may suffer from a high level of serial correlation, potentially biasing the results. I, therefore, replicate the analysis using panel data from the British Election Study and a first-difference model that controls for potential serial correlation. Table 2.3 presents the results. As presented above, the outcome variable satisfaction with democracy was rescaled to range from 0 to 100. In addition, Figure 2.5 illustrates the model's predicted values.

Table 2.3: First-Difference Model (BES):
Outcome - Satisfaction with Democracy

	(1)	(2)	(3)
Relative Polarization	−0.149*	−0.036	−0.041
	(0.087)	(0.095)	(0.093)
(not) Cabinet		−0.083	
		(0.715)	
Relative Polarization * (not) Cabinet		−0.463***	
		(0.169)	
(not) Prime Minister			−0.230
			(0.817)
Relative Polarization * (not) Prime Minister			−0.562***
			(0.195)
Constant	−0.614***	−0.669***	−0.609***
	(0.152)	(0.153)	(0.152)
Observations	28,610	28,610	28,610
R^2	0.0001	0.001	0.001
Adjusted R^2	0.0001	0.001	0.001
F Statistic	2.946* (df = 1)	9.427*** (df = 3)	9.386*** (df = 3)
Note:			*p<0.1; **p<0.05; ***p<0.01

These results demonstrate, consistent with previously presented models using the CSES data, that the status of the closest party choice moderates the effect of relative polarisation on satisfaction with democracy. We find no significant effect of relative polarisation in the model excluding the interaction terms. The models with interactions, on the other hand, show significant interactions with the dummies for cabinet membership and chancellorship. Respondents whose closest party choice is not in the cabinet or supplying the prime minister have a 7 and 9 scale point lower satisfaction with democracy.

Overall, there is only weak support for a general negative effect of polarisation on democratic satisfaction. In the repeated cross-sectional CSES data, we find a significant effect for relative polarisation on satisfaction with democracy, but the effect size is small. In addition, when using a first-difference model with panel data in the UK, the same effect cannot be found. Instead, the findings strongly suggest that whether a respondent's closest party is in government or not has a moderating effect that is critical for understanding the relationship between polarisation and satisfaction with democracy.



(a) No Interaction (b) Cabinet (c) Prime Minister

Figure 2.5: Predicted Values First-Differences
Outcome - Satisfaction with Democracy

These findings are consistent with the theoretical arguments presented above. When a citizens' most congruent party is in government and thus in a position to implement policy, citizens' democratic satisfaction is unaffected because the threat that the political system will produce disliked policy outcomes is small, regardless of the level of polarisation. However, if a citizen's preferred party is not in power, the likelihood of disliked policy outcomes increases with the level of polarisation.

2.7 Results: Voter Participation

I present a set of logistic mixed-effects models with demographic confounders to examine the relationship between relative polarisation and the decision to vote. Table 2.4 shows the results of this regression. The first model in the table depicts the impact of relative polarisation without taking into account any interaction terms, whereas the other two models investigate the interaction between relative polarisation and a dummy variable indicating whether the respondent's preferred political party is represented in the cabinet or the party to which the head of government belongs. For ease of interpretation, Figure 2.6 shows the predicted values for relative polarisation based on the three models.

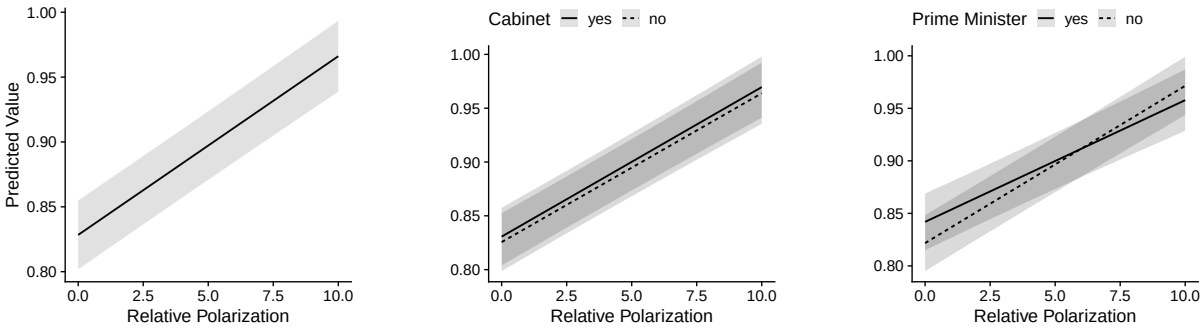
Across all three models, we find a significant positive effect of relative polarisation on turnout. The predicted values for the base model show that respondents who experience the highest possible level of relative polarisation are on average 17% more likely to turn out to vote, which confirms findings presented in previous research.

Table 2.4: Mixed-Effects Model (CSES): Outcome - Turnout

	(1)	(2)	(3)
Relative Polarization	0.014***	0.014***	0.012***
	(0.001)	(0.001)	(0.001)
(not) Cabinet		−0.005	
		(0.004)	
Relative Polarization * (not) Cabinet		−0.0001	
		(0.001)	
(not) Prime Minister			−0.020***
			(0.004)
Relative Polarization * (not) Prime Minister			0.003***
			(0.001)
Gender	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)
Income	0.020***	0.020***	0.020***
	(0.001)	(0.001)	(0.001)
Age	0.024***	0.024***	0.024***
	(0.001)	(0.001)	(0.001)
Education	0.012***	0.012***	0.012***
	(0.001)	(0.001)	(0.001)
Government Efficacy	0.015***	0.015***	0.015***
	(0.001)	(0.001)	(0.001)
Constant	0.546***	0.549***	0.560***
	(0.014)	(0.014)	(0.014)
Random-Effects			
Countries	34	34	34
Years	25	25	25
Observations	136,700	136,700	136,700
Log Likelihood	−38,114.670	−38,121.420	−38,108.950
Akaike Inf. Crit.	76,249.330	76,266.850	76,241.900
Bayesian Inf. Crit.	76,347.590	76,384.750	76,359.810

Note: *p<0.1; **p<0.05; ***p<0.01

In addition, there is no statistically significant interaction between relative polarisation and the dummy, indicating whether the respondent's closest preferred party is in government. The interaction between relative polarisation and the prime minister's party affiliation is statistically significant, but when we examine the predicted values in Figure 2.6c, we find that the difference in the effects of relative polarisation is not statistically significant, even when comparing no relative polarisation to the maximum possible level of relative polarisation. In Table B.2 of the appendix, a more conservative operationalisation



(a) No Interaction (b) Cabinet (c) Prime Minister

Figure 2.6: Predicted Values Mixed-Effects Model:
Outcome - Turnout

of the model using a binomial generalised linear model with country and year fixed effects and country clustered standard errors reveals the same directionality of the effects with a positive effect of relative polarisation on turnout but no significant interaction with the government and prime minister dummies.

As in the previous section, I replicate the repeated cross-sectional results using BES panel data and the first-difference model, which again yields results that are consistent with the repeated cross-sectional findings. The first-difference model also reveals a significant positive effect of relative polarisation on voter participation, whereas the interaction effects with the cabinet and prime minister dummies are insignificant.

Table 2.5: First-Difference Model (BES):
Outcome - Turnout

	(1)	(2)	(3)
Relative Polarization	0.253***	0.243***	0.260***
	(0.051)	(0.055)	(0.054)
(not) Cabinet		−1.452***	
		(0.418)	
Relative Polarization * (not) Cabinet		0.075	
		(0.099)	
(not) Prime Minister			−1.457***
			(0.478)
Relative Polarization * (not) Prime Minister			0.014
			(0.114)
Constant	0.030	−0.013	0.031
	(0.089)	(0.089)	(0.089)
Observations	28,610	28,610	28,610
R^2	0.001	0.002	0.002
Adjusted R^2	0.001	0.002	0.002
F Statistic	24.831*** (df = 1)	17.039*** (df = 3)	15.887*** (df = 3)
Note:			*p<0.1; **p<0.05; ***p<0.01

The findings further demonstrate the engaging effect polarisation can have. Furthermore, we observe that the extent to which polarisation mobilises turnout is not moderated by the status of voters' best party choice. Voters whose closest party is part of the government or is providing the prime minister are at most only marginally more mobilised by higher polarisation. This finding further supports the notion that polarisation affects individuals by raising the stakes. In an election, all parties have at least a theoretical chance of winning office. Whether their preferred choice has been successful in previous elections or not the stakes in an election rise when polarisation is high as a voter will face more distant party options competing for office.

2.8 Conclusion

This paper revisited the question of how citizens' relationships with democracy respond to polarisation by using a new measure of relative polarisation that combines the perspectives of mass and elite polarisation with data from individuals across 34 countries and 25 years.

There are several reasons to believe that if citizens experience higher polarisation it poses a threat to the survival of democracies. By design, elite polarisation must result in some parties adopting more extreme positions which may cause animosities among voters (Banda and Cluverius, 2018; Harteveld, 2021 and overall more polarised party choices may contribute to lower satisfaction with democratic regimes (Ezrow et al., 2011; Reher, 2015; Ferland, 2021; Kim and Fording, 2002; Dahlberg and Holmberg, 2014).

An extensive body of literature, however, contradicts the narrative that polarisation threatens democracy, finding that citizens in more polarised countries are more engaged in the democratic process (Abramowitz and Stone, 2006; Dalton, 2008; Abramowitz and Saunders, 2008; Aldrich, 1993; Muñoz and Meguid, 2021; Wilford, 2017). Indeed, the results presented in this paper show that polarisation is not purely the doom of democracy, as the same sample of citizens can both become more dissatisfied with and more engaged in democracy as a result of polarisation.

This contribution ties these diverging perspectives together using a novel measure of relative polarisation that combines mass and elite polarisation to capture how citizens *perceive* polarisation at the individual level. My findings suggest that when citizens perceive greater relative polarisation, they have a higher stake in the political process. This increase in individual stakes has a dual effect. On the one hand, my findings show that polarisation has a negative impact on citizen democratic satisfaction. This effect is small to insignificant for those whose closest party option is in government. The stakes are significantly lower for those whose preferred party is in government or even supplies the prime minister, as opposition parties have systematically less capacity to implement undesirable policies.

On the other hand, I find that polarisation engages all citizens, regardless of their preferred party's government status. In an election, the status quo is reset, and higher relative polarisation increases the likelihood of a disliked policymaker winning office, raising the stakes regardless of who is currently in office. Consequently, polarisation can be both boon and bane for democracy, as higher polarisation produces an electorate that is more likely to vote but more dissatisfied with the performance of their democratic system overall.

My results shed light on the challenges polarisation presents to democratic governance, but they do not offer solutions. Given that earlier studies, like Claassen, 2020b, show that strong public support helps democracy survive, and that voter turnout is crucial for democratic governance, further research is needed. Future research that assists in understanding a state of polarisation in which citizens in a democracy are actively engaged by voting and satisfied with democratic governance could significantly contribute to the stability, if not survival, of democracies.

Chapter 3

Political Communication in the Real World

Political communication can provoke intense, immediate public reactions. Beyond extreme cases research has demonstrated the ability of political communication to frame issues (de Vreese, 2003), set the agenda (Feezell, 2018; Shaw, 1979) and induce processes of learning (Bode, 2016).

These contributions frequently rely on survey experiments, which are typically carried out using designs with text-based treatments in which the effect is evaluated immediately after treatment exposure. Because these designs are unable to distinguish between short- and long-term effects, it is difficult to infer whether communication effects persist in the real world.

Scholars across different disciplines compensate for this limitation by presenting repeated stimuli, but their findings are inconclusive. While research in communication sciences argues that the effects of framing can be quite enduring (Lecheler and De Vreese, 2011; Tewksbury and Scheufele, 2009) other demonstrate that political messages are short lived (Chong and Druckman, 2010; Hill et al., 2013). Especially in social psychology research, the transient nature of communication effects has long been acknowledged (Baesler and Burgoon, 1994; Cook and Flay, 1978).

Furthermore, translating these results to real world cases is difficult because experimental designs guarantee that a desired communication effect reaches their target audiences rather than being deflected by recipients who are exposed to a plethora of competing messages in the real world. Consequently, we might overestimate both the significance and persistence of communication effects (Kinder, 2007). In response several scholars argue that the literature on political communication should be expanded with studies based on real rather than constructed stimuli (Lecheler and De Vreese, 2016; Kinder, 2007).

The goal of this contribution is to address this gap and better understand how communication effects persist in the real world by employing a causal design. I use time variation in existing survey data to capture communication effects and the period during which they affect public sentiment.

Specifically, I examine public reactions to the EU-Turkey statement given by the European Commission on March 18, 2016, during the 2015/16 European refugee crisis, widely recognized as the formal closure of the Balkan route. The statement reframed the issue and the focus of the debate around the European refugee crisis from a normative to a more pragmatic perspective focused on solutions to reduce inflows (Gürkan and Coman, 2021). Although the statement announced a policy to reduce the inflow of refugees to Europe, it took six months to produce the promised outcome. This scenario enables the isolation of the statement's framing effect from the impact of the actual policy outcome. Using survey data available through the German Internet Panel (Blom et al., 2017), I analyze the effect of the EU-Turkey Statement on a day-by-day basis.

My results show that the EU-Turkey statement affected respondents' perceptions and attitudes towards the handling of the crisis as well as policy preferences. Respondents were more likely to view refugee inflows as manageable and less supportive of security policies. The effects were largest for respondents surveyed one and two days after the statement, subsequently shrank and gradually disappeared.

Despite the high salience of the European refugee crisis and the potential solution it offers, the effect of the EU-Turkey statement we observe is only temporary. These findings strengthen the perspective that communication effects fade quickly in the real world. However, in the brief period when communication affects public sentiment, the consequences can be far-reaching. The findings presented here should be understood as a clear encouragement to better understand and further investigate the timeframes within which communication affects public opinion using real world cases.

3.1 Communication and the Time Dimension

Research has provided robust evidence for the profound effect political communication can have on public sentiment (Bode, 2016; de Vreese, 2003; Feezell, 2018; Shaw, 1979). Scholars have begun to investigate the persistence of communication effects (Lecheler and De Vreese, 2011; Chong and Druckman, 2010; Hill et al., 2013). However, given the significant implications that a possible decay of communication effects has for previous findings, we still need to better understand this dimension of communication effects, particularly how it manifests in real world scenarios (Lecheler and De Vreese, 2016; Kinder, 2007).

Contributions that discuss the decay of political communication more generally find quickly decaying effects. Hill et al. (2013) examine the time dimension of political communication using data from the effects of advertising in the 2000 presidential election and 2006 subnational elections in the United States and conclude that Communication is unlikely to have long-term consequences unless people pay close attention to it. Chong and Druckman (2010) examine the endurance of competing messages and find that if messages are not received simultaneously, the effects of older messages decay quickly. However, in both cases, it is unclear whether the repetition of the stimuli presented by the authors replicates their participant's media consumption behavior.

Lecheler and De Vreese (2011) examine framing effects in a survey experiment over one day, one week, and two weeks. They demonstrate that framing effects persist and that the duration of framing effects is affected by a person's level of political knowledge.

Tracing the influence of media messages over time is not a new concept, and scholars, particularly scholars in social psychology, have argued that communication and farming effects are transient (Baesler and Burgoon, 1994; Cook and Flay, 1978). Here, two core concepts are used to understand the endurance of communication effects.

The memory-based perspective assumes that individuals develop their opinions cumulatively based on previous judgments and experiences which they store in memory (Hastie and Park, 1986). Constructed attitudes, on the other hand, are understood as on-the-spot opinions based on information available at the time. In this more recent perspective, opinions are potentially so context-dependent that there is no such thing as a real attitude. Instead, attitudes are the present state of a connected system of experiences (Wilson, Lindsey and Schooler, 2000).

While these approaches have different perspectives on attitude formation, they both differentiate between more and less effortful processing to understand the longevity of communication effects. Scholars who take a memory-based perspective argue that individuals anticipate making a judgment of a specific item after getting a message. They weigh each piece of evidence as they receive it, changing an online tally up or down and store the resulting judgment in memory. If confronted with the necessity to make an unanticipated judgment, people generate opinions based on whatever information they can recollect (Hastie and Park, 1986).

In the perspective of constructed attitudes, persuasive communication, which invokes greater effortful processing, causes cognition to become more firmly established and thus remain accessible to affect attitudes for a longer period. New cognitions that have been exposed to less effortful processing may not become as firmly rooted and are therefore less likely to be remembered after a lengthy period. They may, however, still drive behavior during the brief period in which they are active.

Ultimately, both major perspectives on attitude formation suggest that citizens who engage in effortful processing while receiving a communicated message are more likely to be affected for a longer time. In contrast, those who process the message intuitively are more likely to be affected briefly (Hill et al., 2013).

Consequently, discerning which circumstances encourage people to engage in effortful information processing is critical for understanding the longevity of political communication. Kahneman's (2011) work on dual-processing theory describes the *need for cognition*, the tendency to think intuitively or effortfully about a presented problem or piece of information as an intrinsic human feature. However, individuals with an inherent tendency for effortful information processing are too few to explain broader behavioral patterns. Besides, the general *need for cognition* research has shown that issue salience can influence tendencies to engage in effortful thinking for a larger proportion of society. Citizens are more likely to engage in information processing if an issue is highly salient (Ciuk and Yost, 2016), especially in cases where compelling policy information is communicated (Boudreau and MacKenzie, 2014).

Salient issues occupy a central position in citizens' belief systems and are tied to deeply held beliefs and identities. Consequently, citizen develop clear demands when it comes to salient issues (Esses, Haddock and Zanna, 1993; Sears, 1993; Zaller, 2012). Messages that successfully relate to these demands affect perceptions by altering voters' perceptions of the circumstances surrounding their policy demands (Arnold, 1990; Chen and Luttig, 2019). This link between the message and the proposed outcome would be the strongest immediately following the announcement. As the novelty of the proposed policy wears off, the link between the announcement and the proposed outcomes weakens.

An additional theoretical approach that complements this narrative is online processing. Findings on online processing suggests that individuals form and adjust their evaluations of political figures or issues in real-time, based on immediate reactions to new information (Lodge, Steenbergen and Brau, 1995). In the context of salient issues, as citizens are presented with messages regarding their deeply held beliefs, they instantaneously integrate these messages into their existing evaluations. These real-time adjustments, guided by online processing, mean that while the specifics of a message or campaign information might fade from memory over time, the overall affective reaction to the message remains. The immediate nature of this processing further underscores the idea that the strongest impact of a policy announcement on perceived outcomes is felt right after its introduction. Over time, while the specifics of the message may fade, the sentiment, whether positive

or negative, becomes a lasting component of the individual's evaluation, influencing subsequent choices and decisions. Therefore, even when communicating on important issues, elite communication will only cause a brief shift in public opinion. In the long run, regardless of how much effort voters put into information processing, a policy change that meets citizens' demands will be required for lasting opinion change.

3.2 Communication Effects in a Real World Setting

Despite their great utility, survey experimental designs have limitations when it comes to understanding how communication affects the public in the real world. Artificial treatments do not compete with other influences and guarantee that the respondent receives the desired message (Kinder, 2007). In response, researchers suggest that communication effects should be tested using real rather than artificial treatments (Lecheler and De Vreese, 2016).

An alternative approach to traditional experimental designs is to observe communication effects in a natural experiment post-factually. A natural experiment allows us to exploit an issue that is salient in the real world and observe the impact of a related communication across time. Designs such as regression-discontinuity or difference-in-difference allow establishing causality post-factually with existing data sources. Natural experiments have the advantage of eliminating the need to artificially reconstruct people's communication channels. Instead, they enable us to observe how communication affects people from the moment a statement or message is delivered.

A requirement for this design is a case in which we can identify the precise moment at which communication about a subject began. For this purpose, the scenario that I exploit is the 2015/16 European Refugee Crisis and EU-Turkey statement announcing the closure of the Balkan route. In 2015, an unprecedented wave of refugees arrived in Europe. Over 1.2 million first-time asylum applications were submitted in Europe, with Germany being one of the most popular destinations. Such large-scale human movement resulted from

the "Arab Spring," a succession of uprisings and civil conflicts in North Africa and the Middle East. In Germany, the crisis reached a climax in late 2015. At that time, it was a major challenge for the German government to deal with the high influx of asylum seekers (Singleton, 2016).

On March 18, 2016, EU Heads of State and Turkey agreed to end irregular migration from Turkey to the EU. The so-called *EU-Turkey Statement* vouched to replace disorganised migratory flows. It proposed safe and legal pathways to Europe for those entitled to international protection in line with EU and international law (European Council, 2016). Despite many critical voices, the EU-Turkey Agreement was mostly perceived as the official closure of the Balkan route (e.g.: Koelner Stadt-Anzeiger, 2016; Mitteldeutsche Zeitung, 2016; Tagesspiegel, 2016; Die Welt, 2016).

The case of the European refugee crisis and the EU-Turkey statement is ideal for a post-factual investigation of the time dimension of communication effects on public opinion. The European refugee crisis has been highly salient for a long time. Therefore, according to previous research, citizens can be expected to engage in effortful information processing when elites communicate on the issue (Boudreau and MacKenzie, 2014; Ciuk and Yost, 2016).

This tendency should be especially true of the EU-Turkey Statement because it reframes the issue with a focus on a potential solution to the crisis and thus likely addresses citizens' demands to some extent (Gürkan and Coman, 2021). Therefore, if we only find a temporary effect of this communicated message, it presents first evidence that communication effects, in the real world, are time-constrained.

3.3 Data and Methodology

Empirical Strategy

I apply the *Unexpected Event during Surveys Design* (UESD) formalised by Muñoz Mendoza, Falcó Gimeno and Hernández (2019) to capture the effect of the EU-Turkey statement on public sentiments. Similar to regression-discontinuity designs, UESD enables the identification of causal effects by exploiting the occurrence of an unexpected event during the

fieldwork of a survey to estimate its causal effect on the relevant outcome by comparing the responses of the individuals interviewed before and after the event. Similar designs have been used, for example, to identify the effects of Covid-19-related lock-downs (Bol et al., 2021; Schraff, 2020) or terror attacks (Nussio, Bove and Steele, 2019; Boydstun, Feezell and Glazier, 2018).

The event I exploit in this paper, the EU-Turkey statement was released by the European commission on the evening of March 18th (European Council, 2016). According to UESD, the treatment group comprises respondents exposed to the event, while the control group includes those not exposed to the event. The exposure to the EU-Turkey statement can be formally described as follows:

$$X_i = \begin{cases} 1 \text{ if } d_i \geq 19 \\ 0 \text{ if } d_i < 19 \end{cases} ; \tag{3.1}$$

where X_i describes the news exposure and equates to one if respondents were surveyed on March 19 or after. i describes respondents in the GIP sample and d_i the day of survey completion. Identifying a causal effect based on a sample of respondents questioned before and after an event relies on multiple assumptions. The design used in this paper involves two main assumptions, as described by Muñoz Mendoza, Falcó Gimeno and Hernández (2019). The first is *Excludability*. Every discrepancy between respondents questioned before and after the incident is the sole product of the event. The interview time will only impact the dependent variable by an event at the defined cutoff point. Therefore, factors that render the pre-and post-event conditions different violate the assumption of *Excludability*.

The second key principle is *Ignorability*. The outcome for each respondent must be independent of the interview moment. For time to be a valid event instrument, the assignment to separate time values should be independent of the dependent variable. Respondents' assignment to interview times should be as good as random.

To examine the deterioration of communication effects over time, I test my treatment variable across different sub-samples. These sub-samples are selected based on the date the respondent completed the survey. The first sub-sample includes respondents surveyed one day before or after the cutoff, while the second to twelfth sub-sample includes respondents surveyed up to twelve days before or after the cutoff. As a result, the control and treatment groups are always equally sized.

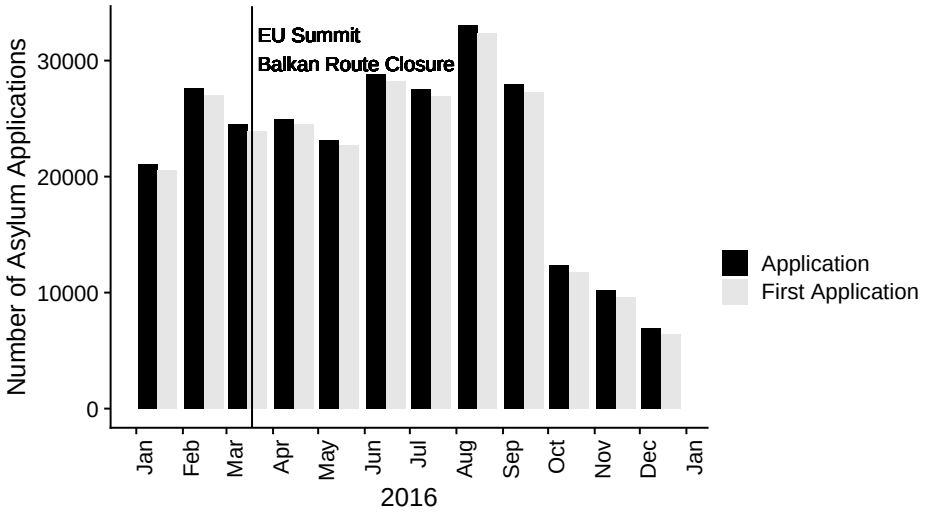
For each of these sub-samples, I will run, depending on the outcome variables scaling, a simple cross-sectional logistic or multinomial regression.

Inspired by King, Tomz and Wittenberg (2000), this paper presents simulated predicted values to provide more reader-friendly measures and account for fundamental uncertainty. More precisely, I will calculate and present the differences in simulated predicted values between the treatment and control groups.

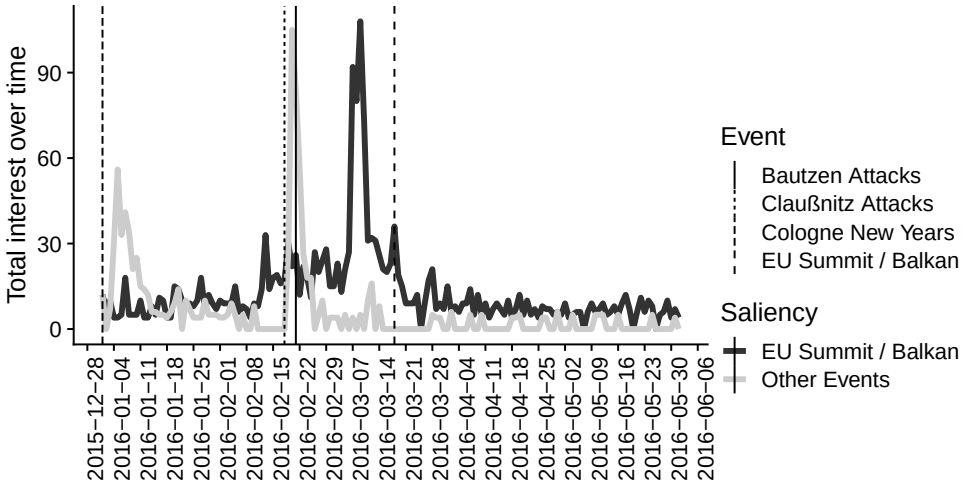
I use these models and predicted values to analyze the effect of the EU-Turkey statement on respondents' perception of the crisis, their policy preferences, and voting intentions. I will discuss the operationalisation of the different outcome variables more in the subsequent section.

Data

Survey data that provides a dense sample of respondents around the cutoff is needed to apply UESD and to measure the effects' longevity. The German Internet Panel (GIP) provides survey data for March 2016, enabling an analysis of the announcement's effect on public opinion across time. The sample was collected throughout March and comprised 3,141 observations. Respondents were able to join the survey at any time during the month due to a comprehensive sampling strategy detailed in the following section. This 22nd wave of the GIP includes a question asking respondents to chose between pro-refugee policy and security policy with the question:



(a) Refugee Numbers 2016



(b) Issue Salience (Google Trends)

Figure 3.1: Exogenous trends

In your opinion, how should politicians deal with this possible dilemma? Either one can fulfill the moral obligation to help refugees from war zones to help. Or you can guarantee security in German society.[1]

The GIP also surveys opinions about Germany's ability to cope with refugees inflows using the following statement on a likert scale of agreement. From here on, the question will be referred to as *ability to cope*:

Germany can cope with the challenges posed by the influx of refugees.[2]

Additionally, the GIP includes a question surveying the general support for asylum on the same scale. Hereafter specified as *asylum support*:

Germany should maintain its policy of accepting refugees from war zones.[3]

I create dummy variables from these questions to evaluate whether the treatment variable affects the respondents' perception of the refugee influx. The dummies evaluate to one if a respondent agrees or strongly agrees to the statement and zero otherwise. For evaluating policy preferences, I create a three-level nominal variable that specifies if respondents favor a focus on refugee support, security policy, or both policies equally.

3.4 Threats to Identification

Subsequently, I discuss various threats to the presented identification strategy, starting with threats to *Excludability*. The first potential origin of bias includes fluctuations in refugee inflows. They could be expected to change as the reduction of refugee inflow is the proposed aim of the announced agreement with Turkey.

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1. German: *Wie sollten Politiker Ihrer Meinung nach mit diesem möglichen Dilemma umgehen: Entweder kann man die moralische Verpflichtung erfüllen, Flüchtlingen aus Kriegsgebieten zu helfen. Oder man kann die Sicherheit in der deutschen Gesellschaft gewährleisten.*
2. German: *Deutschland kann die Herausforderungen bewältigen, die durch den Zuzug von Flüchtlingen entstehen.*
3. German: *Deutschland sollte seine Politik, Flüchtlinge aus Kriegsgebieten aufzunehmen, beibehalten.*

Figure 3.1 (a) shows that the number of refugees in March is only marginally lower than in February and on a par with numbers in April. We can only find a significant decrease in asylum applications from October 2016 after a slight increase in August and September. While monthly data does not exclude the possibility of variations throughout March, these findings strongly suggest that fluctuations in refugee inflows do not bias the presented measure.

Other events occurring right before the announcement represent a further potential source of bias. These events include the Bautzen[4] and Claußnitz Attacks[5] or the riots during new years celebrations in Cologne[6]. Figure 3.1 (b) provides a rough estimate of issue salience based on Google trend data (see for testings of Google trend data as a salience measure: (Mellon, 2013; Mellon, 2014). The figure shows that while other issues were highly salient in mid to end of February, the *Total Interest Over Time*[7] fell to a low level in March. I conclude that these events are unlikely to have influenced my outcome variables during the period studied.

Another challenge to *Excludability* is the endogenous timing of the speech. The German government and the EU were under severe pressure to announce convincing measures to handle the inflow of refugees. A potential deal had the intention to satisfy public opinion and discourage support for far-right parties. However, if we understand the negotiations between the EU and Turkey as a bargaining process (Krumm, 2015), either side could not solely control the conclusion of the negations. The *Total interest over time* presented in figure 3.1 also shows that the attention towards the event had peaked before the start of the summit. At the time of the summit attention towards the event is relatively lower and citizen are unlikely to have anticipated the exact timing of the announcement. Additionally, I will present a placebo test in the robustness section to further support the compliance with the assumption of *Excludability*.

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4. In Bautzen, there have been campaigns against asylum seekers and their accommodations since 2014. The national media mainly reported on the anti-refugee riots in 2016.
5. Riots erupted in Claußnitz, Germany on February 18, 2016, when a bus carrying refugees to a local refugee shelter was blocked by about a hundred demonstrators chanting xenophobic slogans.
6. During the night of December 31, 2015, to January 1, 2016, there were numerous sexual assaults on women in Cologne in the area of the main train station and the cathedral by groups of young men primarily reported as being of North African descent.
7. The *Total Interest Over Time* (G) is derived as follows: $G = \sum_{i \in I} (\frac{S_i}{S})_N$; where I specifies a set of search terms, S_i the number of searches for search term i in I and S the total number of searches.

The next major threat to identification is *Ignorability*. *Ignorability* is usually challenged due to insufficiently rigorous sampling strategies.

The German Internet Panel is mostly conducted as an online survey. Still, while online surveys are often associated with insufficient sampling and self-selection bias, the GIP uses a three-stage probability sample to counteract these issues. Primary sampling units (PSUs) are sampled stratified by state, government district, and urban level; for each PSU, interviewers list households along a random route with a random starting point. Each household along the predefined route was listed to prevent errors and interviewer cheating. Different interviewers conducted the listing and interview at all sample points. The listing yielded the third stage sampling frame. A fixed set of addresses per PSU was drawn at random start intervals to minimise clustering (Blom, Gathmann and Krieger, 2015). Considering this elaborate methodology, I assume sampling to be as good as random and *Ignorability* assumption to be fulfilled.

3.5 Descriptive Statistics

Table 3.1: Treatment Control Comparison

Bandwidth	T-Test (p-values)						N		
	East	University Degree	Unemployed	65+	Abitur	Female	Treatment	Control	All
1	0.454	0.102	0.817	0.513	0.045	0.306	64	149	213
2	0.627	0.336	0.265	0.949	0.437	0.607	105	190	295
3	0.838	0.621	0.305	0.465	0.656	0.096	151	275	426
4	0.781	0.705	0.476	0.132	0.942	0.355	197	331	528
5	0.432	0.433	0.476	0.380	0.928	0.073	278	359	637
6	0.825	0.118	0.555	0.279	0.946	0.300	398	401	799
7	0.460	0.046	0.419	0.308	0.843	0.724	500	425	925
8	0.895	0.094	0.263	0.668	0.667	0.504	628	450	1078
9	0.759	0.113	0.400	0.550	0.708	0.383	670	506	1176
10	0.919	0.083	0.592	0.255	0.658	0.318	742	567	1309
11	0.966	0.134	0.944	0.051	0.746	0.200	835	667	1502
12	0.878	0.321	0.704	<0.001	0.645	0.130	955	867	1822

Before the analysis, I descriptively examine the treatment and control groups in terms of their differences. Table 3.1 in the appendix shows the p-values resulting from a two-sided t-test evaluating the significant difference of means between treatment and control groups across different demographic features and different bandwidths used in the analysis. Equally, it presents the size of the bandwidth sub-samples and the treatment and control group's respective sizes. I test several demographic characteristics that could

shape the respondent evaluation of the *ability to cope* and their policy preference. The table shows that treatment and control only differ marginally. People aged 65 and older differ significantly in bandwidth twelve, respondents with a higher education entrance qualification differ significantly between treatment and control in bandwidth one and respondents with a university degree differ significantly in bandwidth seven. Otherwise, respondents surveyed before and after the announcement are highly comparable in terms of their demographic features.

Now that I have established the treatment and control group's comparability, I will examine the dependent variables around the cutoff point. Figure 3.2 shows the change in means for the output variables in March 2016.

From March 19 onwards, we can see a significant change in averages in all three outcome variables. The *ability to cope* and *asylum support* outcome variables, in particular, follow a similar trajectory throughout the month. Following the EU-Turkey Statement, the proportion of respondents who believe the refugee influx is manageable increased. On March 18, 35% of respondents thought the refugee influx was manageable, but 51% did after the announcement on March 19. Following this initial increase, we see a downward trend, with the lowest proportion on March 24, when only 17% of respondents saw the refugee flows as manageable.

Right after the European Commission announced the EU-Turkey statement, there is an increase in respondents who consider refugee support and security policy equally important in terms of policy preferences. On March 18, only 42% of respondents thought both policies were equally important, whereas 65% did on March 19. Simultaneously, the proportion of respondents who thought security policy was more important has dropped dramatically. On March 18, 36% of respondents thought security policy was the most important, while only 13% thought so one day later.

Overall, the descriptive analysis of the dependent variables indicates that the announcement has only a short-term effect on attitudes, which fades after a few days.

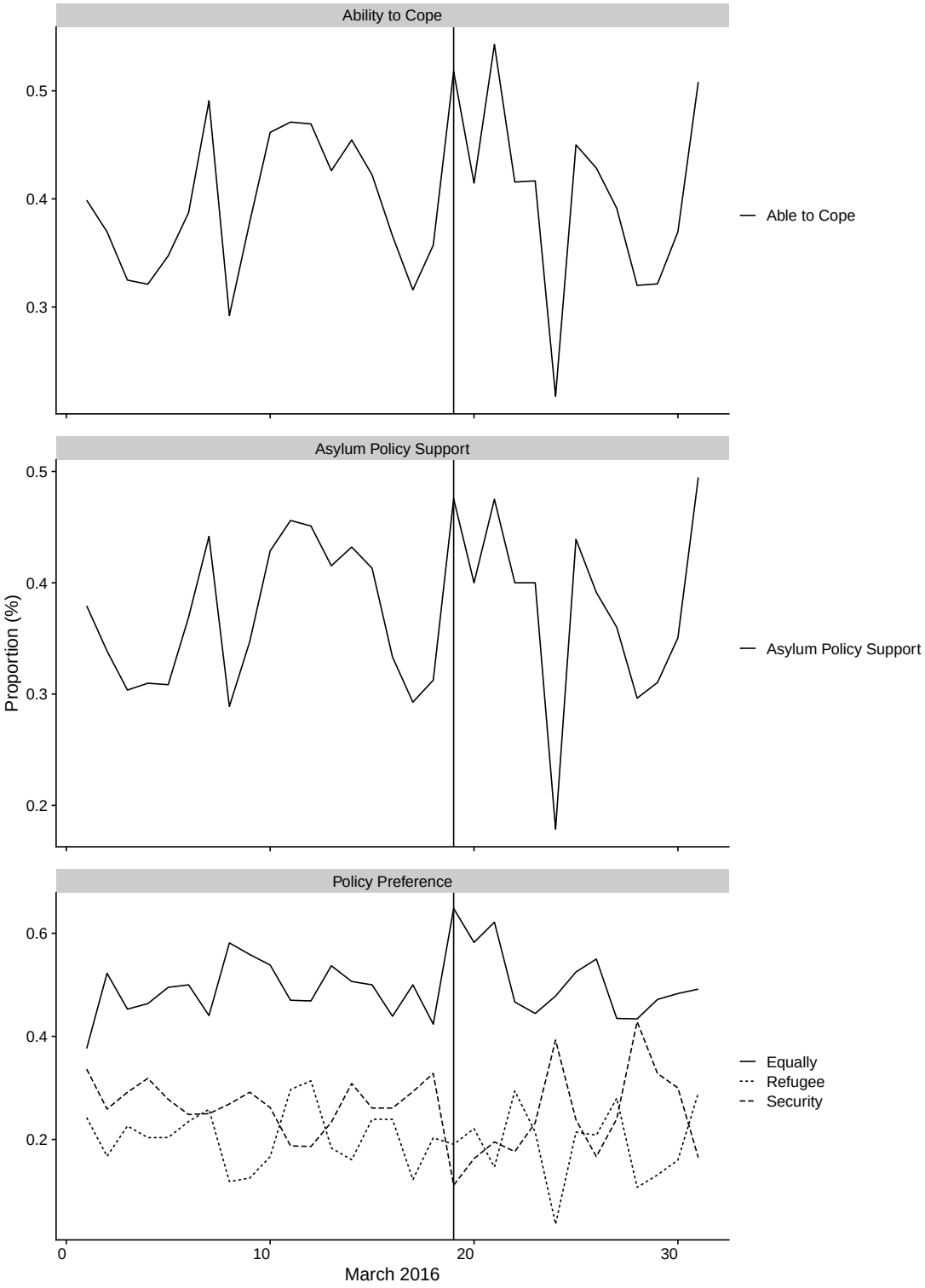

Figure 3.2: Daily average for outcome variables

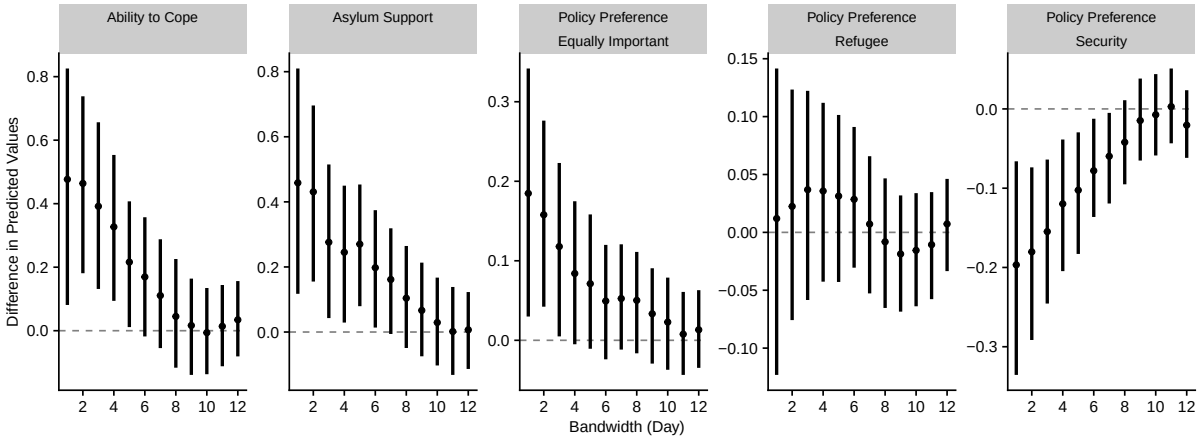

Figure 3.3: Difference in predicted probability. Bars show 95% confidence intervals. Regression tables can be found in the online appendix.

3.6 Results

Subsequently, I discuss various simulated predicted values of the effect of the treatment on the *ability to cope*, *asylum support*, and policy preferences. Figure 3.3 shows differences between treatment and control across the different bandwidths for all three outcome variables.

The results for the outcome variables *ability to cope* and *asylum support* are derived from two sets of linear regressions conducted across different bandwidths. Only for the narrower bandwidths do respondents in the treatment and control groups have significantly different attitudes toward the corresponding outcome variable.

For the *ability to cope* the difference in predicted values is largest in the one-day bandwidth where people on average evaluate Germany's ability to handle the inflow of refugees 0.48 scale points higher. The value difference falls to 0.22 scale points in the five-day bandwidth until it is no longer significant from the sixth day on. For *asylum support* the results show that in the one-day bandwidth, respondents are on average 0.45 scale points more supportive of asylum policy. which falls to 0.2 scale points in the six-day bandwidth until there are no longer any significant results from the seventh day.

Consequently, the EU-Turkey statement affected the perception of the refugee influx to Germany, and the results support the idea of communication effect's short-lived nature.

I use multinomial models regressing the respondent's policy preference on the treatment variable to estimate the effect of the EU-Turkey announcement on policy preferences. The results provide evidence that the Balkan route closure's announcement affects policy preferences. Respondents surveyed one day after the information became public are on average 19% more likely to think support for refugees and security are equally important and 23% less likely to respond that security should be a priority. For wider bandwidths, the difference in predicted probability declines until it is no longer significant. Eventually, the treatment and control groups' probability to think that both policies are equally important is no longer significantly different from the four-day bandwidth. Likewise, the treatment-control difference for security policy preferences is no longer significant from the eight-day bandwidth.

The announcement of the Balkan route's closure does not affect the likelihood of respondents favoring refugee policy. In combination with the negative effect on preferences for security policy, the Balkan route closure's announcement may mitigate the fears related to a higher level of migration rather than mobilising support for refugees. Again, both previously defined expectations are met. The announcement of the EU-Turkey statement affected policy preferences but only temporarily. These results provide further evidence for the importance of time when trying to understand the effect of communication on public opinion.

3.7 Robustness Checks

Figure 3.4 depicts a series of placebo tests designed to address potential sources of bias within the control group. I shift the treatment cutoff date from the 12th to the 17th, using a one-, two-, and three-day bandwidth.

When the cutoff is set to the 13th, the placebo treatments show significant results. On that date, state elections were held in the German states of Rhineland-Palatinate, Saxony-Anhalt, and Baden-Württemberg. The *Regional Elections* pane in figure 3.5 replicates figure 3.3 while omitting respondents surveyed on the 13th and 14th to ensure that respondents polled briefly after these elections do not skew the presented results. The figure

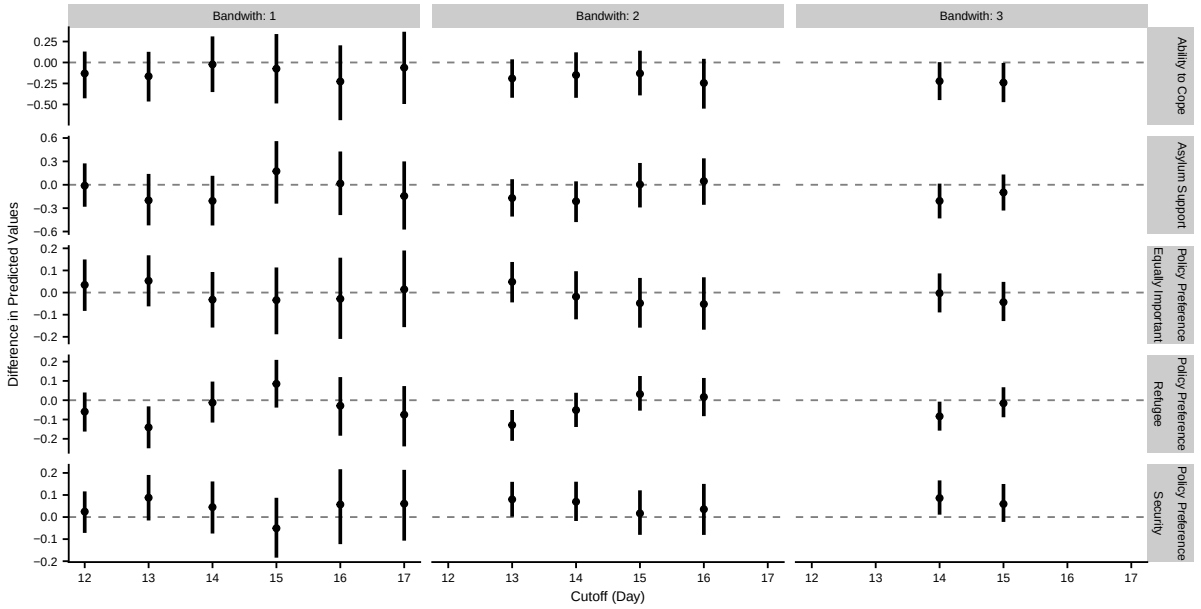

Figure 3.4: Placebo tests with difference in simulated predicted differences. Bars show 95% confidence intervals. Regression tables can be found in the online appendix.

only includes the results from the five-day bandwidth as only these bandwidths include those respondents sampled directly after the state elections. The resulting predicted values differ only marginally from the original results, implying that the state elections on March 13th did not bias our results.

In addition, in the *Asymmetrical Design* section of figure 3.5, I present an alternative design for bandwidth selection to the approach presented in figure 3.3 to further demonstrate that the presented treatment effect is not biased by state-level elections. In this alternative approach, the control group is limited to a two-day bandwidth before the treatment, avoiding the inclusion of respondents polled in close proximity to state-level elections. The bandwidth for the treatment group has also been reduced to two days to ensure that treatment and control remain comparable. Subsequently, the bandwidth for the treatment group shifts from the 19th/20th to the 29th/30th while maintaining its two-day width. The results of this alternative approach are in line with the previous results and show significant effects in the first bandwidth for the *ability to cope*, *Asylum Support* and policy preferences. They also demonstrate the same temporary nature of the effect, as the treatment no longer has a significant effect after the third bandwidth.

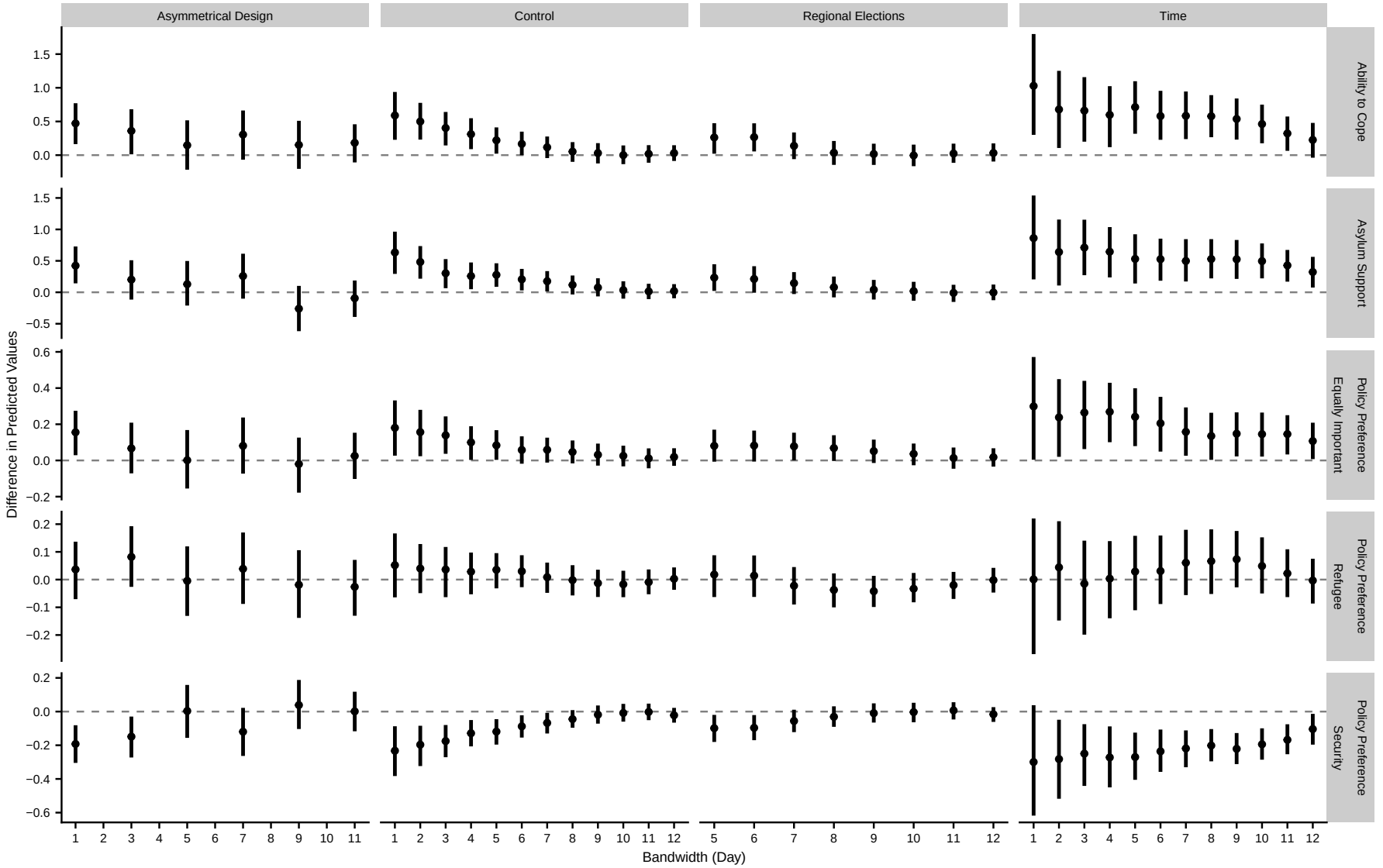

Figure 3.5: Robustness tests. Bars show 95% confidence intervals. Regression tables can be found in the online appendix.

Underlying time trends are another potential challenge for UESD. The *Time* section of figure 3.5 reproduces the results shown in figure 3.3 while adding a time control variable. The findings indicate that the treatment and control groups are still significantly different. Furthermore, we can see that the effects are still diminishing over time, and the original argument remains valid.

In addition to the key outcome variables, I apply my design to an external outcome variable to see if the treatment effect is subject to random variation. The alternative outcome assesses attitudes toward imposing an environmental toll in German city centers, a policy preference that I do not believe the treatment will affect. The results are shown in figure 3.6, and the treatment variable has no effect on the alternate outcome variable. These findings suggest that the previously discovered treatment effects are not subject to random variation.

I also run the model with demographic control to eliminate the possibility that the results are biased by respondent features. Included are dummies for respondents who live in East Germany, have higher education entrance qualification (Abitur), have a university degree, are unemployed, are over 65 years old, and respondent's gender.

In summary, the robustness checks presented show that the time dummy gauges exposure to the EU-Turkey statement. The potential bias I identified on March 13th as a result of state elections held in several German states have no significant impact on the results. The identified treatment effect is neither the sole expression of an underlying time trend nor the results of random variation.

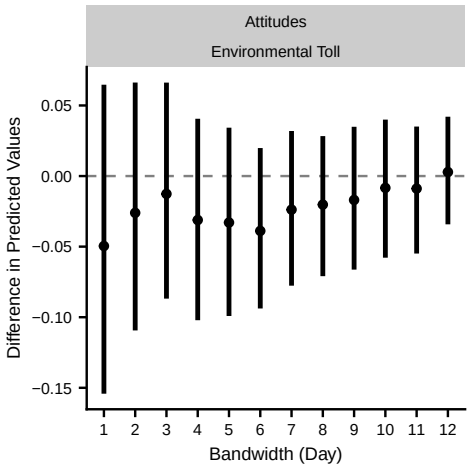

Figure 3.6: Alternative outcome: support for an environmental toll. Bars show 95% confidence intervals. Regression tables can be found in the online appendix.

3.8 Conclusion

This article investigates the persistence of communication effects in the real world by using a natural experiment approach to analyze the announcement of the Balkan route closure. Experiment-based contributions have provided strong evidence on the significance of communication effects on public sentiment (Bode, 2016; de Vreese, 2003; Feezell, 2018; Shaw, 1979) and shed some light on how these effects play out over time. However, we heretofore knew very little about whether communication effects persist in real world scenarios.

By utilising time variation in survey sampling, the study at hand was able to provide an alternative to traditional survey experimental designs. A natural experiment allowed to analyze changes in public opinion in response to the EU-Turkey statement announcing the closure of the Balkan route. The announcement affected how people perceived the issue, their policy preferences, and their voting intentions. All three effects peaked right after the announcement and faded quickly over the following week.

Social psychology research suggests that the longevity of communication effects is driven by the level of cognitive engagement with the communicated message. Given the importance of the European refugee crisis, previous literature suggests that citizens are likely to engage in effortful information processing in the case of the EU-Turkey statement.

Therefore, my findings suggest that even when people process a communicated frame very consciously, communication effects only cause temporary disruptions in public opinion. I expect this limitation of communication effects originates from the real political demands that need to be satisfied to achieve attitude change.

I concur with previous suggestions. Scholars should avoid presenting composite or average findings on communication effects that fail to discuss the degree of deterioration of the effect they present (Hill et al., 2013). To this end, natural experiments can be a valuable addition to survey experimental designs to collect more information about the endurance of communication effects.

Unlike experimental design, the methodology used here does not require its own data collection and thus has the exciting prospect of being easily and inexpensively applicable in a variety of contexts. As a result, the research presented here can be viewed as the first in a series of potential post-factual studies that can aid in our understanding of the real world persistence of communication effects.

Chapter 4

Democratic Deficits and Support for the EU

For many scholars the European Union (EU) lacks accountability (Schmidt, 2020), transparency (Crum and Curtin, 2015) and public engagement in elections (Hix and Marsh, 2011; Hobolt and Wittrock, 2011; Schmitt, 2005). How is it possible that the members of the EU – some of the oldest and most consolidated democracies in the world –tolerate such democratic deficiencies? This question exposes one of the most glaring contradictions in EU governance. On the one hand, there has been an increasingly adverse debate over whether the EU is democratically legitimate or suffers from considerable democratic deficits. On the other hand, the EU and its predecessor organisations over time have built an image as a guarantor of peace and democracy in Europe (de Vries, 2023), a narrative that was validated by the EU receiving the Nobel Peace Prize in 2012.

Existing literature addressing the EU democratic deficit and the ensuing crisis of legitimacy overlooks the legitimacy that may develop over time through reputation and familiarity (see for example Gibson, Lodge and Woodson, 2014; Hirshleifer, Jack and Riley, 1992). Instead, they typically focus on particular deficiencies in EU governance, such as the council's almost dictatorial role as a result of highly unequal power distribution among members (Schmidt, 2020); officials in the Commission who are not directly elected and thus cannot be easily held accountable; and directly elected officials in the EU Parliament who do not contest EU issues but serve as proxies for voters to express dissatisfaction with national politics (Hix and Marsh, 2011; Hobolt and Wittrock, 2011).

However, EU member states display diverse democratic norms, which are not without shortcomings (Ferrín and Kriesi 2016). The democratic characteristics, including accountability, transparency, and civil society involvement, can differ significantly among these countries. For example, while according to the data provided by the Varieties of Democracy (V-Dem) project some European countries like Denmark or Sweden have impeccable democratic qualities other consolidated democracies in the EU like Italy or Spain have substantially lower standards of transparency and civil society involvement (Michael Coppedge et al., 2023).

We can expect that these disparities in democratic quality among EU members will result in divergent perspectives on how democracy should function at the EU level and, as a result, in divergent attitudes of EU citizen toward the EU as a whole. Suppose we want to understand the complex relationship between perceptions of EU democratic deficits and EU support. In that case, we need to account for the differences across national democracies and relate these to how democracy works at the EU level.

Building on de Vries' (2018) Benchmark Theory, this study proposes an examination of the way in which democratic standards experienced within a country shape individuals' expectations regarding the functioning of democracy at the EU level. From this point forward, I will use the term "Democratic Gap" to describe the discrepancy between these expectations and the reality of EU governance. Given the wealth of literature on the EU's democratic deficit, we can conclude that the EU falls short of the democratic standards present in its member nations. As a result, people living in countries with high democratic standards likely perceive a considerable Democratic Gap, which could lead to reduced support for the EU as a whole.

Despite widespread scholarly consensus on the EU's democratic deficit, the quality of democratic governance is rarely contested in public, implying that there may be some factor that mitigates the negative impact the EU democratic Gap has on public sentiment. One explanation for the current lack of opposition could be citizens' general preference for familiar institutions and their preference for the known status quo over uncertain altern-

atives (Hirshleifer, Jack and Riley, 1992). Particularly considering the positive portrayal of the EU as a defender of democracy, the extent to which individuals are familiar with the status quo of EU governance may help to lessen the impact of Democratic Gaps on EU support.

In this study, I test how EU support is affected by Democratic Gaps and how this relationship is moderated by familiarity with the EU. My analysis is based on the notion that the characteristics of EU democracy vary only over time and not between member states. Therefore, accounting for time variation with fixed-effects enables us to link differences in national democracy with variations in the Democratic Gap. I apply this logic to two distinct research designs. First, utilising a new, highly sophisticated measure of aggregate EU support (Scotto di Vettimo, 2022), I examine how national democratic qualities affect EU support at the macro-level in a period between 1972 and 2021. Based on the assumption that the longer a country is a member of the EU, the more familiar its people are with the Union, I use membership time to assess how higher familiarity moderates the impact of national democratic qualities on EU support. Second, at the individual level, utilising a large repeated cross-sectional sample of Eurobarometer data, I test to what extent individuals are affected by national standards and how this effect is moderated by the frequency with which respondents discuss EU politics. For both analyses, I investigate the effect of the democratic qualities that the EU has been accused of lacking: national accountability, civil society involvement, and transparency.

If Democratic Gaps had a negative impact on EU support, people in countries with higher democratic standards would be less supportive of the EU. My findings contradict this expectation and suggest that people's familiarity with the EU plays a crucial role in the relationship between national democratic qualities and EU support. I find that national democratic qualities have a significant effect on EU support only when they interact with one of the measures of familiarity with the EU. This moderating effect occurs both at the micro and macro levels of analysis. On the one hand, people who live in a newly joined EU country or who do not discuss the EU and thus are unfamiliar with it are either unaffected by higher democratic standards at the national level, or they are less supportive of the

EU as democratic standards in their home country rise. People who are familiar with the EU, either because they discuss it occasionally or frequently or because they live in a country that has been a member for a long time, tend to be more supportive of the EU the higher their national democratic standards are.

The findings of this empirical study provide compelling evidence that familiarity with the EU plays a significant role in shaping perceptions of the EU. The data suggest that a higher level of familiarity not only counteracts the negative impact of high national democratic standards and perceived deficits at the EU level but also moderates this effect such that individuals in countries with high democratic quality on average express greater support for the EU when they are familiar with it. While a direct measure of exposure to the narrative portraying the EU as a defender of democratic values wasn't possible in this study, the outcomes are consistent with the notion that the EU has been notably successful in establishing this narrative among its citizens.

4.1 A Crisis of Legitimacy in the EU

The debate over the legitimacy of the EU, or, put differently, the question of a democratic deficit in the EU, has accompanied the union throughout most of its integration process. The debate around the democratic deficit in the EU hinges on whether it is a legitimate concern that the EU's decision-making processes, which are often criticised for lacking sufficient transparency and engagement with civil society, could lead to a crisis of legitimacy as citizens begin to question the authenticity of its democratic principles and its right to exercise authority.

Scholars on one side of the debate argue that the EU does not have a deficit or that democratic standards that are required from nation-states do not apply to the EU (Grimm, 1995; Kielmansegg, 2003; Moravcsik, 2008). On the other hand, scholars contend that because the EU assumes an increasing number of responsibilities from its members, it must be held to the same standards as nation-states, which it frequently fails to do (Follesdal and Hix, 2006; Schmidt, 2013; Schmidt and Wood, 2019). These scholars typically allude to a liberal definition of democracy in this debate, emphasising the need of defending indi-

vidual and minority rights against state and majority tyranny. The quality of democracy is judged by the constraints put on government accomplished through constitutionally protected civil freedoms, a strong rule of law, an independent judiciary, and effective checks and balances that, when combined, limit the exercise of executive authority.

The debate arose in the context of the Maastricht Treaty, with proponents of the No-Demos Thesis arguing that the lack of a unified European Populus precludes the need for an EU constitution and for the same level of legitimacy as nation-states. From the perspective of its proponents, the No-Demos Thesis rendered the debate over the EU's democratic deficit null and void (Grimm, 1995; Kielmansegg, 2003; Moravcsik, 2008). However, as integration has progressed, the No-Demos Thesis has been increasingly criticised as being too limited. Scholars have instead argued for a vision of the European community where citizens can belong to and identify with both European and national demos (Weiler, 1995; Nicolaïdis, 2013). Given the EU's increasingly important role in people's lives, more scholars have argued that the EU has a democratic deficit. The critique of EU democratic legitimacy was spearheaded by Follesdal and Hix (2006), who argued that it is critical for supranational institutions, such as the EU, that produce policies that affect people's lives, to have dependable processes ensuring policies are systematically responsive to citizen demands, rather than matching by happy coincidence. Initially, there was opposition to this line of argument, asserting that the EU could by design never achieve the procedural standards of a common nation due to its exceptional supranational structure (Moravcsik, 2008). Yet, the EU's struggles with the European debt crisis (Cramme and Hobolt, 2014), the refugee crisis, and the United Kingdom's departure from the Union (Hobolt, 2016) have quieted these defenders of EU legitimacy, leading to widespread agreement that the EU is in fact facing a legitimacy crisis.

In the ongoing debate over the EU's democratic deficits, the European Commission and Council are the primary institutions facing criticism and concern. In theory, the Council should exemplify the intergovernmental nature of the EU, as opposed to taking a monolithic stance. However, the institutional constraints of the unanimity rule, the different economic weights of member states, and bottom-up political pressures on EU leaders all combined to create an intergovernmental mode of decision-making in which the leaders of

a few member states hold unprecedented power. This accumulation of power results in a "one fits all" approach to governance practised both by the European Council and Commission. Consequently, the EU is dominated by the austere ideas of northern European members, particularly Germany (Newman, 2015).

The Commission while technically only a bureaucratic body faces considerable cross-tension. If we take the case of the European debt crisis: On the one hand, we have member states that seek to create budgetary space for economic development in order to meet citizens' demands while being financially monitored as a consequence of their financial bailouts. On the other hand, some members have advocated for stricter and more stringent enforcement of financial regulation, in response to public concern about having to pay the debts of others (Schmidt, 2020).

The European debt and refugee crises have also led to a shift in public perceptions of the EU and its deficits, growing awareness, and increased relevance of European politics at the national level, a process often referred to as the waking of the sleeping giant (de Vries, 2007; Van der Eijk and Franklin, 2004). Since this awakening, public support has become critical to understanding the EU and its ongoing integration process (Hooghe and Marks, 2009), as the European public shifts from the early stage of integration's "permissive consensus" (Lindberg and Scheingold, 1970) to a "constraining dissensus" as a result of the politicisation of EU issues in national elections and the rise of Euroskeptic parties (Hobolt and De Vries, 2016).

This more aware public will also be more attentive toward the democratic shortcomings of EU governance. For example, EU citizens have a limited ability to hold EU policymakers accountable because Council ministers and Commission officials cannot be punished directly, but only indirectly through national legislatures. Furthermore, insufficient civil society engagement exacerbates these accountability issues. The European Parliament (EP), which is intended to serve as an accountability forum for the EU, should outweigh the shortcomings of the Council and Commission. However, while the European Parliament is the EU institution that formally provides the most accountability because its members are directly elected, in practice, it holds little relevance in the lives of its consistency. EU citizens rarely use EP elections to respond to and punish EU policymakers; instead, EP

elections have a second-order nature and are often used as a signal for national issues (Hix and Marsh, 2011; Hobolt and Wittrock, 2011; Schmitt, 2005). Consequently, the EP largely fails to engage EU citizens and counterbalance the accountability deficiencies of the Council and Commission. Finally, scholars argue that the Commission lacks transparency because it frequently implements policies at its own discretion while relying on informal agreements with individual member states (Crum and Curtin, 2015).

The increasing public awareness was noted by EU policymakers, and during the so-called "reflection period," (June 2005 to December 2006), three options were discussed: continuing with the European Treaties as amended by the Nice Treaty; beginning new negotiations to adopt some changes deemed technically necessary; or attempting to have the previously failed Constitutional Treaty of 2004 approved as a new treaty. Most member states and the EU institutions were in support of the third alternative which led to the passage of the Lisbon Treaty in December 2007. The treaty aimed to alleviate some of the criticism levelled at the EU's legitimacy by establishing a system for citizens' initiatives in which a certain number of EU citizens from various member states might petition the Commission to submit a legislative proposal. It also established a stronger role for national parliaments and the Committee of the Regions in overseeing conformity with the subsidiarity concept (Ziller, 2019). Additionally, the treaty was anticipated to have an effect on the significance of EP elections, particularly as a result of the Spitzenkandidaten system. However, these changes did little to improve the perception of EU legitimacy or the second-order nature of EU parliamentary elections (Maier et al., 2018), and the legitimacy of the EU continues to be the subject of scholarly debate. The debate on EU democracy has largely concluded that the EU does, in fact, have a democratic deficit. However, due to the diversity of EU members, it has been difficult to test how these deficits directly affect EU support.

4.2 Relative Democratic Deficits in the EU

As outlined in the previous section, when compared to national states the EU has undeniable democratic deficits. However, the EU assumes policy capacity across a diverse range of national communities that have developed distinct democratic practices and expectations over several decades. Whether it is the strong position of the president in France or proportional systems like Germany without popularly elected executives, democracies in the EU differ dramatically. Nicolaïdis (2013) elegantly describes the diverse structure of the EU as *Demoicracy*, "a Union of peoples who govern together, but not as one" (also see Bellamy, 2019). Approaching an ideal state of democracy in this political landscape while maintaining the ability to govern effectively is significantly more difficult than it is for nation-states.

Scholars have shown expansively that national democratic institutions influence public opinion on the EU. Anderson (1998) underscored this by illustrating that citizens tend to use their viewpoints on domestic political entities as a reference when articulating their stance toward the European integration process, signifying that attitudes toward the national political system and establishment parties significantly reduce their support for EU membership. This is complemented by an assertion that support for the European Union and its integration processes is often shaped and mediated by domestic political attitudes rather than purely by economic considerations Anderson, 1998.

Expanding on the relationship between supranational and national politics, Sánchez-Cuenca (2000) postulates posits that the citizens' approval of the EU is conditioned by their perception of national and supranational institutions alike. The author argues that the lower the view of national institutions and the brighter the perspective on supranational institutions, the higher the support for European integration, particularly stemming from the reduced perceived cost of transferring sovereignty at times when confidence in national institutions is low. The empirical analysis provided reveals that nations plagued by higher corruption and possessing underdeveloped welfare states tend to exhibit higher support for integration, thereby highlighting the instrumental role of the evaluation of national political conditions in shaping attitudes toward European integration Sánchez-Cuenca, 2000.

Similarly, Rohrschneider (2002) indicates a pivotal relationship between the perceived democratic deficit of the EU and its impact on the public's support for political integration, especially in contexts where national institutions are functioning proficiently. The study implies that, particularly in nations with effective institutional frameworks, when citizens feel unrepresented at the EU level, their support tends to wane, regardless of their economic perceptions. This conveys the criticality of citizens' perception of representation at the supranational level, particularly in the context of well-established national institutions Rohrschneider, 2002.

In divergent vein, Hobolt (2012) explores the determinants of satisfaction with EU democracy from the citizens' perspective and introduces a novel viewpoint regarding the spillover effect from national to supranational institutions. Contrary to prior research, the authors introduces the notion of a positive spillover effect, where confidence in national institutions reciprocally influences and enhances confidence at the European level. Tested across 27 EU Member States, the findings reveal that both procedural and performance factors at the national level play an essential role, with the confidence in EU institutions proving particularly salient among citizens who possess a substantial knowledge about the EU Hobolt, 2012.

In synthesis, the literature cohesively implies that public opinion toward the EU is profoundly anchored in perceptions and evaluations of domestic political conditions, institutions, and experiences.

However, the preceding literature on Europe's democratic deficit, discusses the EU's democratic deficits and resulting legitimacy crisis in terms of a single, more or less clearly defined ideal of liberal democracy. This approach to studying EU legitimacy does not fully account for the complexity of democratic legitimacy in a supranational setting and might therefore lead to the mixed results the different authors present. Different communities in the EU are sure to have varied expectations of democracy due to the different norms they have formed within their national settings (Ferrín and Kriesi, 2016).

Therefore, how citizens perceive EU democracy, or citizens' acceptance of the rules and requirements of EU institutions (Easton, 1975), cannot be reduced to a single democratic norm. Instead, we require a relational concept that puts the EU's and national democratic standards in perspective. de Vries (2018) pioneered the idea that perceptions of the EU are best understood as a relational concept. According to her benchmark theory of support and scepticism, attitudes towards the current state of EU governance hinge on people's evaluation of the perceived benefits of an or counterfactual where the country is not a member of the EU. She refers to these comparisons of status quo as its counterfactual as "EU differentials."

Following the concept of reference point dependent preferences, which was popularised in economics and psychology by Daniel Kahneman and Amos Tversky's work on prospect theory (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992), the EU differential emphasises the potential value of losses and gains when making a decision rather than the final outcome. These changes are measured using a reference point, or benchmark. People weigh the benefits of the status quo, against the benefits of the counterfactual when deciding whether to change or maintain the status quo, for example, to support their country to remain or leave the EU. In general, people consider outcomes that are worse than the reference point to be losses, while those that are better are considered wins. When the benefits of the status quo appear to be less than those of the counterfactual, people will prefer changing the status quo. When people perceive the benefits of the status quo to be greater than those of the counterfactual, they are in a gain frame and will resist any attempt to change the status quo, which in this case would mean leaving the EU.

Building on this foundation, I propose an alternative application and empirical test of the Benchmark Theory and apply it to the analysis of the democratic deficit in the EU. Whereas Benchmark Theory has so far found limited empirical application due to a lack of adequate data, in this study I will present an empirical strategy for applying a relational measure of democratic deficits an extended time frame using existing macro and micro data. Here, I introduce the term "Democratic Gap." This term, akin to de Vries' EU differentials, refers to the perceived disparity between democratic practices at the national

level and those within the EU. People use their national democracy as a reference point against the democratic quality at the EU level to estimate a counterfactual to democratic governance within the EU's multi-level system, namely what democratic governance would be like outside the EU.

Based on these findings I expect citizens will perceive differences in the extent to which civil society is engaged in political processes, their ability to hold officeholders accountable, and the transparency of rules in line with this comparison, and these differences will factor into the perceived Democratic Gap. When national democratic standards are high, they set a high benchmark for the EU to meet. Given the frequent criticism of EU democracy, the fact that the EU is unlikely to meet these high standards will result in a larger Democratic Gap and that several scholars argue (Follesdal and Hix, 2006; Hix, 2013) the EU's democratic deficits are detrimental to EU support I conclude my initial hypothesis:

Hypothesis 1: The greater national democratic standards the lower the level of support for the EU

To understand the role Democratic Gaps play in driving Eurosceptic sentiments we must consider the pivotal role uncertainty plays when people evaluate the counterfactual outside the EU. As identified by Hirshleifer, Jack and Riley (1992), people naturally favour the known over the unknown - the tangible benefits of the present compared to uncertain possibilities of the future. This preference becomes critical in considering the Democratic Gap. The counterfactual here involves envisaging a political scenario outside of the EU, a realm where democratic governance may take a different shape, hence creating uncertainty.

Therefore, it is essential to note that familiarity with the EU's democratic structures and processes is not a constant; it varies across time and between individuals. When a country first joins the EU, people hold fresher memories of governance outside the EU and have less certainty about the democratic status quo within the EU. But as time elapses, this balance of familiarity tilts. The longer a country stays within the EU, the more familiar people become with its democratic operations and, in turn, the less certain they are about

the alternative democratic scenario outside the EU. I expect this is not just a matter of time, individual engagement also matters. Individuals who actively pay attention to the EU and frequently discuss its affairs are more familiar with its democratic processes than those who engage less.

We can expect that this familiarity, as well as the varying certainty about the status quo inside the EU and the counterfactual outside the EU that results from it, will have a critical moderating effect on how national democratic standards affect EU support. Because of the uncertainty associated with its approximation, people will only be able to perceive a range of possible values for the counterfactual. In order for people to perceive a benefit from switching to the counterfactual, in this case leaving the EU, the entire range would have to be greater than the status quo. If the status quo is within the range, people prefer the status quo because both gains and losses are possible. The more we pivot from a situation in which people are unfamiliar with the EU, where the status quo is uncertain and the counterfactual is certain, to a situation in which people are familiar with the EU, where the status quo is certain. The counterfactual is uncertain, the wider this range will become. I therefore I conclude:

Hypothesis 2: The higher citizen's familiarity with the more positive the effect of higher national level democratic standards.

In other words, the greater peoples' familiarity with the EU, the less likely they are that perceived losses resulting from a comparison of the status quo and the approximated counterfactual will lead them to favour the counterfactual outside the EU.

The transformation of the European Union into an expansive political and economic union sparked debates around national identity, as highlighted by scholars (Carey, 2002; Hooghe and Marks, 2005; Hooghe and Marks, 2009; McLaren, 2006). However, the aftermath of the Eurozone crisis presented a narrative that utility, more than identity, shaped citizens' perceptions of the EU (Hobolt and Wratil, 2017). This sentiment is evident from the steady euro support in the northern Eurozone, juxtaposed against a declining support elsewhere. Economic motivations, especially the perceived risks associated with leaving the euro for member states, seem pivotal here (Hobolt et al., 2014). Moreover, the prominence of economic cost-benefit evaluations, especially in economically challenged nations,

underscores a trend where financial considerations overshadow identity factors (Hobolt and Wratil, 2017; Kuhn and Stoeckel, 2014). Validating our hypothesis would, however, suggest that national democratic values persist as a central influence on EU attitudes, challenging the findings that utility predominantly shapes current EU sentiments.

4.3 Data and Methodology

For a comprehensive understanding of how national democratic qualities and the ensuing Democratic Gap affect EU support, and how familiarity with the EU moderates this relationship, we need to examine it from both micro and macro viewpoints. This dual approach enables us to scrutinise this relationship over the long term at the macro level, and in a more time-constrained yet granular manner at the micro level.

The macro dataset used in the analysis combines data from Scotto di Vettimo (2022) and the Varieties of Democracy Project (V-Dem, 2023) and covers a period between 1973 and 2020. The micro-level dataset used for the analysis uses data from 21 Eurobarometer waves which covers a period between 2009 and 2021 and 28 countries[1].

At both levels, the analysis includes a sample of EU members at any given time, encompassing only responses from countries that are current members. For instance, the United Kingdom is considered solely for the period between 1972 and 2020.

Measuring EU Support

For the analysis of macro-level EU Support, I use the measure provided by Scotto di Vettimo (2022). His highly sophisticated measure of aggregate EU support is available and consistent across all EU members. Scotto di Vettimo employs a Bayesian item response theory (IRT) model based on de Vries' (2018) two-dimensional conceptualisation of EU support. I anticipate that the author's measurement model which uses individual-level data to estimate aggregate EU support, will produce a measure that maps onto the micro

1. Austria, Belgium, Bulgaria, Croatia, Cyprus, Czechia, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, United Kingdom

level. The measure accounts for both opinions toward the EU as a polity and support for European integration. The theoretical scale of the measure ranges from 0 to 100, with 0 indicating maximal EU support and 100 maximal Euroscepticism. In the present sample, the range is from 21 to 76, with a median of 45.

At the micro level, I use attitudes toward EU membership.[2] The membership attitude variable captures three levels of attitudes toward the EU: positive, negative, and neutral. I recode these levels to negative one for negative, zero for neutral and one for positive membership attitudes. It has to be noted that the distribution of the variable is clearly skewed towards positive membership attitudes with only 18% of the used sample holding negative attitudes and 54% holding positive attitudes towards the EU.

Quantifying National Democratic Quality

I use data from the Varieties of Democracy Project (V-Dem) to assess the quality of national democracy at both the macro and micro levels, where I combine the country-level measure with individual respondents. Unfortunately, no comparable micro or macro data exist to assess the quality of EU democracy; however, because the EU is a supranational structure that stands above national institutions, its changes vary only in time and not within each year for each country in the sample. Therefore, differences in EU democracy can be accounted for by using time-fixed effects. These fixed effects account for all of the variance that EU democratic standards would introduce to a measure of democratic differentials. Therefore, the overall direction of the effects we observe when using only the country-level V-Dem data should correspond to the true differential, allowing us to approximate the relationship between EU differentials and EU support. I use three specific characteristics of national democracies that are frequently argued to be deficient at the EU level: the national level of accountability, the engagement of civil society, and transparency.

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2. Question Text: *Generally speaking, do you think that (OUR COUNTRY'S) membership of the EU is ...*

In my analysis, I use the Varieties of Democracy (V-Dem) accountability index as a comprehensive empirical measure to evaluate national-level accountability, which measures the extent to which governments need to justify their actions and face potential consequences (Lührmann, Marquardt and Mechkova, 2020). This index incorporates vertical, horizontal, and diagonal subtypes of accountability, providing a broad perspective on political accountability that includes all components that may contrast with EU structures. For example, EU citizens can only indirectly sanction Council ministers and Commission officials through national legislatures, which limits their vertical accountability, or their capacity to hold their government responsible. The Commission's ability to both initiate and implement policy undermines the EU's horizontal accountability, or institutional checks and balances. Diagonal accountability, or oversight by civil society, also is limited due to insufficient engagement with the European Parliament (EP), whose elections are frequently used to address national issues rather than to hold EU policymakers accountable.

This tendency of the EP to reflect national issues more than EU issues points to a larger issue, namely the EU's lack of a unified and engaged public. An engaged civil society plays a critical role in democratic processes, often complementing formal democratic structures like the EP. Therefore, I utilise the V-Dem project's Civil Society Participation index in my analysis to assess civil society's inclusion in governance processes within the EU. This index measures the extent to which prominent civil society organisations engage with policymakers, the degree of individual involvement in civil society activities, and the nature of legislative candidate nomination within party organisations.

Furthermore, scholars have highlighted transparency issues within the EU, particularly in relation to the Commission's operations, which often rely on informal agreements with individual member states for policy implementation (Crum and Curtin, 2015). This lack of transparency can influence public attitudes towards the EU, shaping perceptions of its democratic legitimacy. To examine this further, I utilise the V-Dem transparency index in my analysis. This index serves as an empirical measure of a country's legal transparency, taking into account whether laws are clear, well-publicised, consistent, and enforced steadily over time.

All three of these measures are quantified using survey excerpts and merged using a Bayesian IRT model, allowing me to compare and contrast national democratic qualities with those of the EU in such a way that higher values of national standards, as captured by the indices, corresponding to a greater contrast with EU governance.

Assessing the Role of Familiarity

To assess the role of familiarity at the macro level, I use the length of a country's EU membership to assess the collective level of familiarity with the EU. This design is based on the assumption that people in countries that have been members for a longer period of time are more familiar with the EU. I expect time dependency to have a nonlinear mediating effect. The longer a country is a member, the weaker this mediating effect becomes, with much uncertainty about the EU being cleared up in the early years of membership. I take this into account using the log transformed membership length and give shorter membership changes more weight. In a separate model for each democratic quality, I interact this time variable with the V-Dem measures of national democratic quality and test its impact on aggregate EU support.

At the micro level, I investigate how individual variations in certainty about the status quo of democratic governance in the EU mediates the impact of democratic differentials on EU support. I use a question that asks respondents how frequently they discuss the EU for this purpose.[3] The focus of the analysis lies in discerning the disparity between respondents who never engage in discussions about the EU and those who do so at least sporadically. Consequently, the original three-level scale has been recoded to facilitate this investigation. A value of one is assigned if respondents engage in any EU discourse, while a value of zero is attributed to those who abstain entirely. I anticipate that respondents who discuss the EU will exchange more information and, as a result, will be more certain about EU structures, including how EU democracy works. In a separate model for each democratic quality, I interact this variable with the different V-Dem measures.

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3. Question Text: *When you get together with friends or relatives, would you say you discuss frequently, occasionally, or never about European political matters?*

Modelling

For the macro level analysis, I use a linear fixed-effects model with country and year fixed effects and cluster robust standard errors at the national level. I include country-fixed effects to account for all time-invariant country characteristics. I also cluster errors at the country level to avoid overestimating the significance of the results, given that the data points are not independent but rather repeated measurements of 28 country units.

Moreover, I control for several potential confounds. Some measures of democratic quality may correlate with the general satisfaction with democracy in the respective member countries, so I consider this in the analysis. The economic performance of a country, which could relate to its length of EU membership given the EU's promotion of its members' economic progress, is another potential bias. To adjust for this, I control for the year-on-year growth in GDP per capita.

Finally, I include dummies for two key EU policies - the adoption of the Euro and the Schengen regulation - that were signed at the same time but adopted by members at different times. Including these dummies helps identify the period before and after the adoption of these policies and control for potential bias in the results.

At the micro level, I apply a linear regression model with country and time-fixed effects, clustering robust standard errors at the country level. The model includes demographic controls such as occupation, education, age, and gender. Additionally, I consider respondents' general life satisfaction, which could potentially bias their political attitudes at the time.

4.4 Macro Results

The results of the fixed-effects linear regression model testing the effect of national democratic quality indicators such as accountability, civil society engagement, and transparency on EU support are presented in separate tables 4.1, 4.2, and 4.3 for each democratic quality indicator. Each table shows two models with EU support as the outcome variable, with the first baseline model only including the respective measure of democratic quality

Dependent Variable:	EU Support	
Model:	(1)	(2)
Variables		
Accountability	0.108	-3.246***
	(0.178)	(1.080)
Accountability × log(Membership Length)		1.106***
		(0.340)
log(Membership Length)		-91.324***
		(30.398)
Fixed-effects		
Year	Yes	Yes
Country	Yes	Yes
Fit statistics		
Observations	847	847
R^2	0.67288	0.72416
Within R^2	0.03538	0.18658
Controls	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.1: Macro-Level Results: Accountability

and the second model, the main model of interest, including an interaction term between the respective measure of democratic quality and the log-transformed membership length as a measure of country-level familiarity with the EU. The baseline models that include only the respective democratic quality indicators show that none of the presented indicators on their own has a significant effect on the level of EU support. Significant results are found solely in the respective main model when incorporating the interaction with log-transformed membership length as an indicator of familiarity.

The main model for the national-level accountability measure (Model 2 in table 4.1) the regression coefficient for national-level democratic accountability shows a significant negative effect on EU support. That is, for every unit increase in national-level accountability, we can expect a corresponding decrease in EU support by approximately 3.25 units at the time of joining the EU (membership length = 0). Familiarity with the EU, measured as the log-transformed membership length, also displays a significant negative correlation with EU support. Specifically, each unit increase in the log-transformed duration of EU membership significantly results in a 91.32 unit fall in EU support. However, the

Dependent Variable:	EU Support	
Model:	(1)	(2)
Variables		
Civil Society	0.255	-0.907
	(0.169)	(0.567)
Civil Society × log(Membership Length)		0.366**
		(0.171)
log(Membership Length)		-20.462
		(14.527)
Fixed-effects		
Year	Yes	Yes
Country	Yes	Yes
Fit statistics		
Observations	847	847
R^2	0.68064	0.72501
Within R^2	0.05826	0.18910
Controls	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.2: Macro-Level Results: Civil Society Engagement

negative effects of membership length and log-transformed EU membership length can be mitigated by the statistically significant positive interaction effect between national-level democratic accountability and log-transformed duration of EU membership on EU support. This interaction term indicates that as EU familiarity increases, the effect of national-level accountability on EU support increases by approximately 1.11 units.

The first pane in Figure 4.2 visualises predicted values for this interaction term for easier interpretation. The data showcases the change in the level of EU support across increasing membership lengths, for countries with the lowest and highest observed levels of accountability, respectively. The log-transformed membership values were expounded to get the easier-to-interpret membership length values. For new members, countries with the highest level of accountability starts with EU support of 41, increasing to 73 at a membership length of 57 years. Conversely, for countries with the lowest accountability, predicted EU support begins at 61 and descends to 32 over the same period. Around a membership length of ten years, the EU support for countries with the highest and lowest observed levels of accountability intersect. This point marks a shift in the relationship

Dependent Variable:	EU Support	
Model:	(1)	(2)
Variables		
Transparancy	0.120	-0.929**
	(0.076)	(0.394)
Transparancy × log(Membership Length)		0.328**
		(0.120)
log(Membership Length)		-16.550*
		(9.053)
Fixed-effects		
Year	Yes	Yes
Country	Yes	Yes
Fit statistics		
Observations	847	847
R^2	0.67475	0.72959
Within R^2	0.04087	0.20259
Controls	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.3: Macro-Level Results: Transparency

between accountability and EU support. From this intersection point forward, the trajectories of the two groups diverge. Countries with the highest level of accountability see a gradual increase in EU support, whereas those with the lowest accountability experience a decrease.

In terms of significance, the confidence intervals overlap for membership lengths less than 20 years, signifying that differences in predicted EU support between countries with the highest and lowest observed accountability levels are not statistically significant at the early stages of membership. In higher membership lengths, the predicted EU support in countries with the lowest level of accountability continues its downward trend, while the countries with the highest level of accountability experience a contrary upward tendency. This divergence is important as it illustrates the crucial moderating role of familiarity that not only mitigates the negative effect of democratic standards at the national level and the democratic gap that results from it, as originally hypothesised but eventually shifts the effect of national democratic standards to have a positive impact on EU support exceeding the original hypothesis.

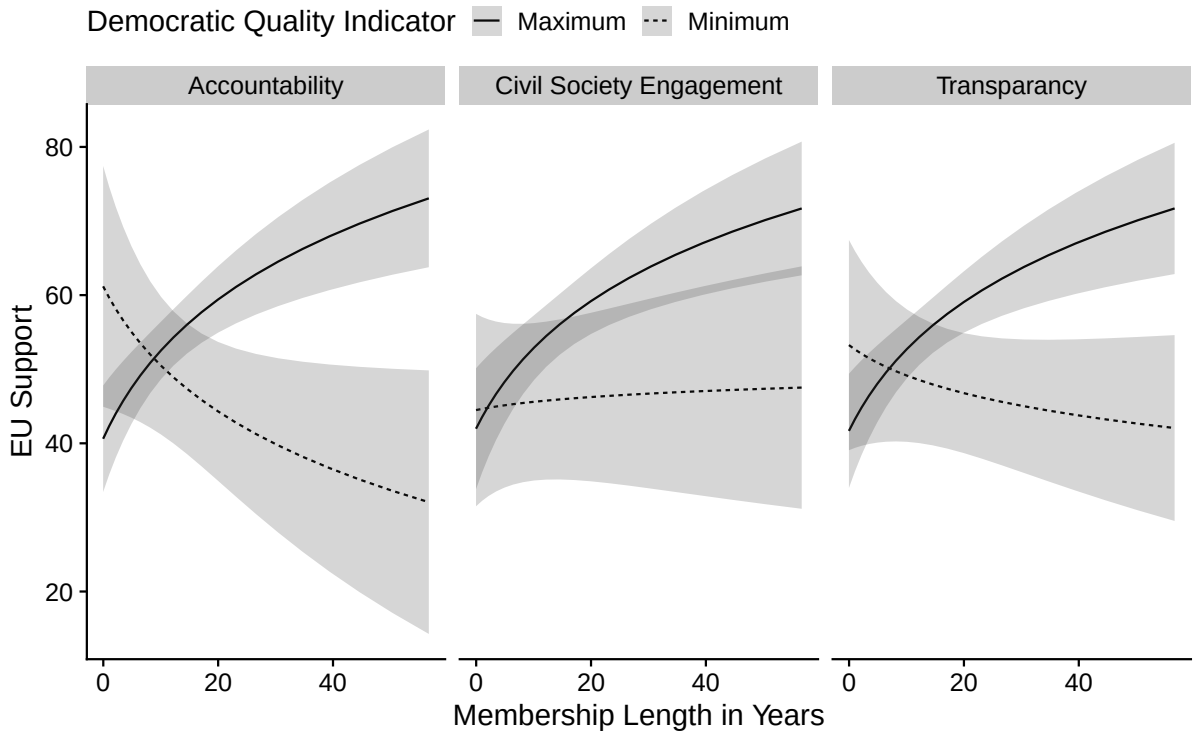

Figure 4.1: Macro-Level Results - Predicted Values

The regression coefficients in the main model for national-level civil society engagement (model 2 in table 4.2) exhibit a negative but insignificant relationship between civil society engagement and EU support. Similarly, the log-transformed duration of EU membership, interpreted as an indicator of EU familiarity, displays a negative, but not statistically significant, effect on EU support. Nevertheless, the interaction term between national-level civil society engagement and the log-transformed duration of EU membership presents a significant positive relationship with EU support. The predicted values for this interaction term are visualised in the second pane of Figure 4.2 for ease of interpretation. The figure suggests that the level of civil society engagement, whether at the lowest or highest observed level, has no effect on EU support across different lengths of EU membership. The consistent overlap of the confidence intervals exemplifies this.

In the main model for national-level transparency, the regression coefficient for national-level transparency illustrates a significant negative relationship with EU support. In other words, for each unit increase in national-level transparency, there is a decrease in EU support by roughly 0.93 units. The log-transformed duration of EU membership, interpreted as a measure of EU familiarity, shows a negative yet insignificant relationship

with EU support. The interaction term between national-level transparency and the log-transformed duration of EU membership presents a significant positive correlation with EU support. This indicates that as EU familiarity increases (as represented by a longer duration of EU membership), the negative effect of national-level transparency on EU support significantly decreases by about 0.33 scale points.

For ease of interpretation, the third pane of Figure 4.2 visualises the predicted values for this interaction term. The figure describes the progression in the level of EU support along countries' membership length in the European Union, differentiated by the minimum and maximum observed levels of transparency. For countries with the highest level of transparency, predicted EU support starts at a level of 42 for new members and rises to 72 by membership length of 57 years. For countries with the lowest transparency, predicted EU support starts at 53 and descends to 42 over the same membership length. The data reveals a point of intersection at a membership length of six years, where predicted EU support aligns for countries with both minimum and maximum observed transparency. Beyond this membership duration, the two groups start to diverge. For instance, at a membership length of 30, the difference in predicted EU support expands significantly to 19 points, with countries demonstrating maximum transparency at 64 and those with minimum transparency at 45.

In terms of significance, for membership lengths below 20, the confidence intervals overlap, indicating that differences in predicted EU support between countries with the minimum and maximum observed transparency are not statistically significant. Although not as pronounced as the observed divergence in levels of accountability, this disparity becomes even more evident with increasing membership length. As the duration of membership extends, nations with maximal observed levels of transparency exhibit a consistent upward trend in predicted EU support, while those with minimum transparency experience a slight decline. These results further highlight the influential role of membership length in moderating the relationship between transparency levels and EU support.

Overall these macro-level regression results illustrate the relationship between measures of democratic quality and EU support within EU countries. Both national accountability and transparency exhibit a negative relationship with EU support when an interaction term with log-transformed membership length, a measure of familiarity, is introduced to the model. The effects of these measures of democratic quality on EU support are moderated by the length of EU membership. This moderating effect implies that as nations become more familiar with the EU, the impact of their democratic quality and the resulting Democratic Gaps in EU support changes. In contrast to the study's initial expectations, this moderating effect goes beyond mitigating the effect of Democratic Gaps and eventually leads to a increase in EU Support among nations with higher democratic standards. Given that the newest member, Croatia, has been a member for ten years, these findings suggest that EU democratic deficits resulting from differences between the EU and high democratic standards at the national level are unlikely to be detrimental to EU support. On the contrary, countries with high standards, whose people have had the most differing experiences from EU democracy, are more supportive of the EU. These findings imply that the value of EU democracy must extend beyond formal democratic structures.

To validate the robustness of my findings, I conducted a series of tests with alternative operationalisations. The outcomes of these tests are compiled in Table B.3 for accountability, Table B.4 for civil society engagement, and Table B.5 for the transparency indicator in the appendix. Initially, I introduced an interaction term for membership length, not subjected to a log transformation which does not yield statistically significant results across the three indicators, as can be seen in the first model of each table. Next, I included an interaction term with a general time trend, ranging from 1972 (coded as 1) to 2020 (coded as 47). Here again, no statistically significant interaction was detected across the three indicators, as displayed in the second model of each table. Subsequently, I devised a model that cross-examines the democracy indicators with dummy variables capturing the timing of various EU treaties' ratification. The aim was to investigate whether the main model's interaction effects are confounded by policy changes triggered by different EU treaties (as shown in model three in each table).

Interestingly, only when interacting with the accountability measure do we observe a statistically significant effect. Among all included terms, only the Treaties of Nice and Lisbon demonstrate a significant negative moderating effect. This is not surprising given these treaties were aimed at addressing the democratic deficits of the EU, with a particular focus on Council reforms (Nice Treaty) and increasing parliamentary power (Lisbon Treaty). On estimating these interaction terms alongside the interaction terms with the log-transformed membership length, we find a persisting significance in the latter interaction and a considerable improvement in model fit, reflected in the within R^2 value. This implies that while EU treaties may somewhat reduce the positive effect on EU scepticism, this effect is supplementary to the moderating effect of familiarity expressed by membership length, which accounts for a larger part of the variance in EU support. Finally, I examined the potential confounding influence of the implementation timing of the Schengen Agreement and the Euro, which differs across member states. However, neither the timing of the Euro nor Schengen implementation reveal a significant main effect on EU scepticism or interaction effect on how different democratic standards influence EU scepticism (as seen in models four and five in each table).

Another possibility is that the effect we observe is one of politicisation as the time trend may capture increasing politicisation. To account for this potential alternative explanation I test the model with an alternative proxy for politicisation, the timing of EU referenda as referenda usually cause highly politicised public debates. I calculate a dummy that evaluates to one when a referendum relating to the EU was held in a specific year and country. The resulting regression table can be found in the appendix table B.6. The results show that there is no interaction between the referendum timing and the effect of national democratic standards. These results strongly suggest that is not politicisation that moderates the effect of national democratic standards but indeed increasing familiarity.

In conclusion, these robustness tests confirm that the log-transformed membership length is not confounded by any time-dependent variables and an effective proxy fir familiarity with the EU. Moreover, the presence of statistically significant findings exclusively in the log-transformed version of the variable indicates that it's primarily the variance in the early years of membership that moderate the effect of democratic standards on EU support.

4.5 Micro Results

The results of the linear model, which includes country and Eurobarometer wave fixed-effects, that assess the role of accountability, civil society engagement, and transparency on EU membership attitudes, are detailed in three separate tables. Table 4.4 includes the accountability measure, table 4.5 the civil society engagement measure, and table 4.6 the transparency measure. Each table has a baseline model that only incorporates country-level democracy indicators and controls. Further, they include a main model that takes into account an interaction term between the democracy indicator and a dummy variable, which represents whether people talk about the EU, a marker of EU familiarity.

The baseline models (1) of each indicator that includes only the level of democratic quality measures and the control variables show, similar to the results at the macro level, that a country's democratic standards have no direct effect on Membership attitudes. Again, we find a significant effect only when the democracy indicators interact with a measure of familiarity, in this case, a dummy variable that identifies respondents who at least occasionally discuss the EU.

The main model investigating national-level accountability (4.4 model 2) reveals that democratic accountability does not significantly affect EU membership attitudes in cases where people do no discuss the EU. In the theoretical case where the level of accountability is at zero EU familiarity bears a significant negative association with EU membership attitudes. The interaction between democratic accountability and EU familiarity demonstrates a significant positive association with EU membership attitudes. This result implies that among those who discuss the EU occasionally, higher levels of democratic

Dependent Variable:	Membership Attitudes	
Model:	(1)	(2)
Variables		
Accountability	-0.006	-0.014
	(0.010)	(0.009)
Accountability × Discussing EU		0.009***
		(0.002)
Discussing EU	0.065***	-0.808***
	(0.016)	(0.219)
Fixed-effects		
Country	Yes	Yes
Wave	Yes	Yes
Fit statistics		
Observations	92,242	92,242
R^2	0.10552	0.12265
Within R^2	0.05303	0.07117
Controls	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.4: Micro-Level Results: Accountability

accountability correlate with an increased likelihood of positive EU membership attitudes. This interaction term mitigates the negative main effect of familiarity and suggests that the level of national democratic accountability's influence on EU membership attitudes reduces positive EU attitudes among those who are familiar with the EU.

For easier interpretation, the first pane in Figure 4.2 shows the predicted values based on the main model for membership attitudes across different levels of accountability for respondents that do at least occasionally or do not discuss the EU. Overall, as we progress to higher national accountability levels, there's a marked decrease in pro-EU membership attitudes within both groups, with this decline being steeper among those unfamiliar with the EU. Across the majority of the scale, however, the two confidence intervals of the predicted values for both groups overlap until they diverge from a level of national accountability of 97, at which point the confidence intervals for the two groups no longer overlap. This indicates that from this level of accountability onwards, the difference in attitudes towards EU membership between the two groups is statistically significant. Following this divergence, the group familiar with the EU, despite the overall downward

Dependent Variable:	Membership Attitudes	
Model:	(1)	(2)
Variables		
Civil Society	0.002	-0.002
	(0.005)	(0.005)
Civil Society × Discussing EU		0.004**
		(0.002)
Discussing EU	0.066***	-0.274*
	(0.016)	(0.136)
Fixed-effects		
Country	Yes	Yes
Wave	Yes	Yes
Fit statistics		
Observations	92,242	92,242
R^2	0.10536	0.10554
Within R^2	0.05287	0.05306
Controls	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.5: Micro-Level Results: Civil Society Engagement

trend, consistently exhibits slightly higher pro-EU attitudes. For example, at the highest possible level of national accountability, the familiar group that at least occasionally discusses the EU has a roughly 0.1 scale point higher level of EU membership attitudes than those respondents who never discuss the EU. Overall, these findings show that similar to what we discovered at the macro level, familiarity with the EU mitigates the negative impact of national accountability on individual membership attitudes, but, unlike the macro findings, this does not eventually result in a positive effect of national accountability on EU attitudes.

Moving to the model examining national-level civil society engagement, we find again that civil society engagement, as a measure of democratic quality, does not significantly affect EU membership attitudes when people do not discuss the EU. However, just as with accountability, the EU familiarity dummy variable has a significant negative effect on EU membership attitudes in the theoretical case when civil society engagement is zero. The interaction between civil society engagement and EU familiarity, though, yields a significant positive effect on EU membership attitudes. This result suggests that among

Dependent Variable:	Membership Attitudes	
Model:	(1)	(2)
Variables		
Transparancy	-0.005	-0.008
	(0.007)	(0.007)
Transparancy × Discussing EU		0.004***
		(0.001)
Discussing EU	0.063***	-0.258**
	(0.017)	(0.094)
Fixed-effects		
Country	Yes	Yes
Wave	Yes	Yes
Fit statistics		
Observations	92,242	92,242
R^2	0.10574	0.10638
Within R^2	0.05327	0.05395
Controls	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 4.6: Micro-Level Results: Transparency

individuals who discuss the EU at least occasionally, increased levels of civil society engagement correspond with increased EU membership attitudes. This interaction term mitigates the negative influence of familiarity on its own, implying that national civil society engagement primarily reduces positive perceptions of EU membership among those familiar with the EU.

For easier interpretation, the second pane in Figure 4.2 illustrates the predicted values of pro-EU attitudes across varying levels of civil society engagement for two groups: respondents that do at least occasionally discuss the EU and those that do not. As we traverse higher levels of civil society engagement, we observe a decrease in pro-EU membership attitudes within both groups, with the descent being sharper among those not discussing the EU. For most of the civil society engagement scale, there is an overlap in the confidence intervals of the predicted values for both groups. This suggests that there is no statistically significant difference between the groups across these engagement levels. However, this overlap ceases at a level of civil society engagement of 96. This indicates that, from this point of engagement onwards, the difference in EU membership attitudes

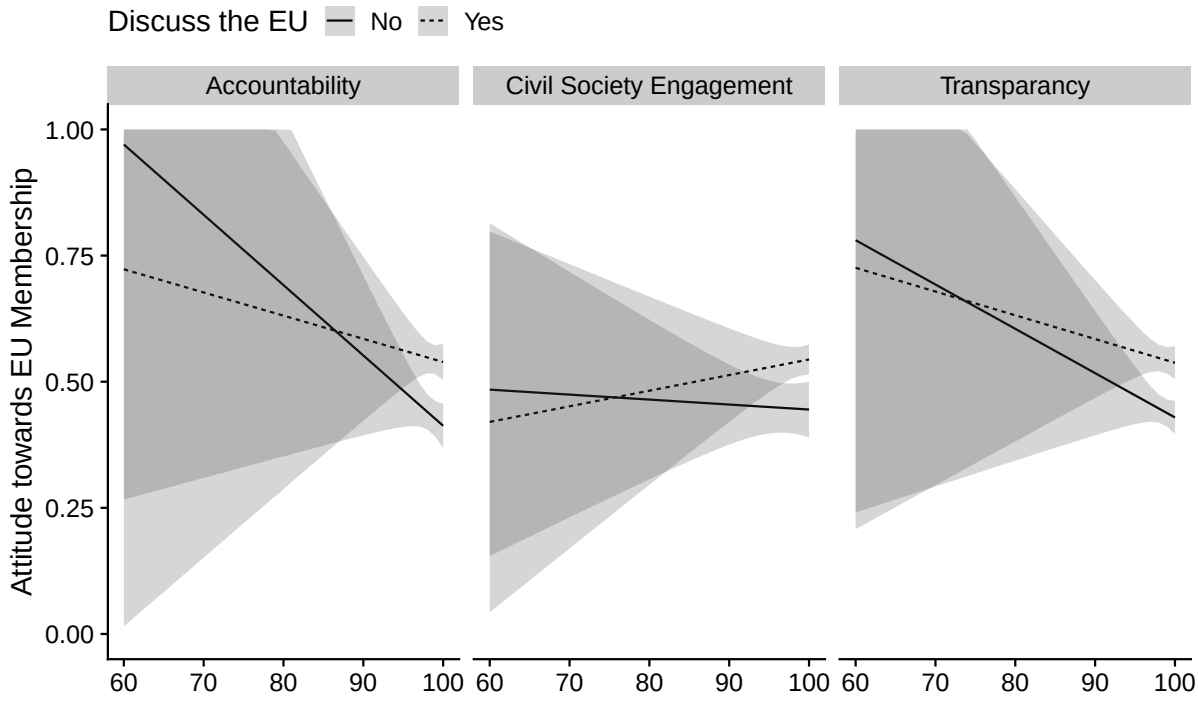

Figure 4.2: Micro-Level Results - Predicted Values

between those who at least occasionally discuss the EU and those who never do becomes statistically significant. Following this divergence, the group engaging in occasional or more frequent EU discussions, while following an overall downward trend, consistently displays slightly higher pro-EU attitudes compared to the group not discussing the EU. For instance, at the maximum civil society engagement level of 100, the group discussing the EU exhibits a roughly 0.099 scale point higher level of EU membership attitudes than their non-discussing counterparts.

Our findings mirror the macro-level patterns to some extent, with familiarity of the EU diminishing the negative repercussions of national accountability on individual membership attitudes. However, it's critical to point out the divergence at the micro level. Contrary to macro-level studies where increased familiarity enhances support, especially when the home state is democratic, here the familiarisation appears to mitigate the adverse effects arising from the home state's democratic nature. Still, the effect remains overall negative and does not culminate in a positive impact on EU attitudes stemming from national civil society engagement.

Lastly, the main model assessing national-level transparency shows a similar pattern. Transparency does not significantly influence attitudes towards EU membership in cases where respondents do not discuss the EU. However, the EU familiarity dummy variable bears a significant negative association with EU membership attitudes when national-level transparency is at zero. The interaction term between transparency and EU familiarity yields a significant positive effect on EU membership attitudes. Thus, among those individuals discussing the EU at least occasionally, higher transparency levels correspond with a higher EU membership views. This interaction term confirms its moderating role in mitigating the main positive impact of familiarity. It suggests that the national level of transparency primarily influences the attitudes towards EU membership among those familiar with the EU.

As for the previous indicators, the third pane in Figure 4.2 illustrates the predicted values of pro-EU attitudes across varying levels of national transparency for two groups: respondents that do at least occasionally discuss the EU and those that do not. For higher levels of national transparency, we observe a decrease in pro-EU membership attitudes within both groups, with the descent being sharper among those not discussing the EU.

For most of the national transparency scale, there's an overlap in the confidence intervals of the predicted values for both groups. This suggests that there is no statistically significant difference between the groups across these transparency levels. However, this overlap ceases at a level of national transparency of 74. This indicates that, from this point of transparency onwards, the difference in EU membership attitudes between those who at least occasionally discuss the EU and those who never do becomes statistically significant.

Following this divergence, the group engaging in occasional or more frequent EU discussions, while following an overall downward trend, consistently displays slightly higher pro-EU attitudes compared to the group not discussing the EU. For instance, at the maximum national transparency level of 100, the group discussing the EU exhibits a roughly 0.109 scale point higher level of EU membership attitudes than their non-discussing counterparts. Furthermore, this further confirms the patterns observed for the other two demo-

cracy indicators, similar to what we found at the macro level. Familiarity with the EU also mitigates the negative impact of national transparency on individual membership attitudes. However, in contrast to the macro findings, this does not ultimately lead to a positive effect of national transparency on EU attitudes.

In summary, the micro-level data generally align with the macro-level trends, confirming that familiarity acts as a significant moderator in the relationship between national democratic standards and attitudes toward the EU. It is important to note, however, that the interaction is weaker at the micro-level; whereas familiarity increases support under higher national democratic standards at the macro-level, it only mitigate the negative relationship between the democratic nature of the home state and attitudes toward the EU at the micro-level. As a result, the micro-level findings are more consistent with the original hypothesis that people prefer a familiar status quo. These findings do not necessarily contradict the macro level results, but they do suggest that the interaction discovered at the macro level is a collective rather than individual mechanism in which people in countries with high democratic standards increasingly identify a benefit of the EU that goes beyond mere democratic qualities the longer they are members.

4.6 Re-Evaluating EU Democracy

The original hypothesis of the paper was that familiarity with the EU would mitigate the negative effects of the EU's democratic deficits. The results presented at the macro-level, however, show that people in countries with higher national democratic standards are even more supportive of the EU if they are familiar with it. Consequently, the presented results exceed the original study's expectations, necessitating a reevaluation of the theoretical argument.

The fact that higher national democratic standards only result in greater support when people are familiar with the EU suggests that people in member countries with highly functional democracies perceive a value of the EU that extends beyond its formal democratic qualities, which require at least some familiarity with the EU to be recognised.

Beyond the formal democratic structures of the EU, the EU is frequently portrayed as a guarantor of democratic values and peace in Europe. This portrayal has long been one of the union's foundational narratives, as a union that binds members in peaceful cooperation and as an economic union that fosters political change in the EU's internal and external relationships through deeper economic interdependence (de Vries, 2023). This image of the union was affirmed when the EU received the 2012 Novel Peace Prize, which is awarded to those who "shall have done the most or the best work for fraternity between nations" (Martin, 1999). Regardless of how limited this control is and how unsuccessful sanctioning attempts directed at countries such as Poland and Hungary have been, the EU still represents a status quo with some control compared to an alternative with no control. The value of this status quo would be especially great for highly functioning democracies because, on the one hand, sanctions are extremely unlikely to be imposed on them. On the other hand, it has been demonstrated that political regimes prefer to cooperate with other regimes that share similar political values (Lai and Reiter, 2000). The EU can act as a sort of insurance policy, preserving the democratic quality of the surrounding regimes. Furthermore, the EU funds a number of long-term democracy assistance projects, which help to strengthen the global network of democratic regimes. The CARDS program, which ran from 2000 to 2006, the Instrument of Pre-Accession Assistance, which ran from 2007 to 2013, and the European Neighbourhood Instrument, which ran from 2013 to 2020, are all successful examples (Gafuri, 2022).

Finally, those who are used to strong democratic standards in their home country but are unfamiliar with the EU will only observe how the EU restricts their home country's power, how laws are less transparent, and elections seem to be insignificant. Those who are familiar with the EU will be more inclined to appreciate the EU's contribution to global democratic governance and perceive the EU as an organisation that makes it easier to retain democratic ideals in the world arena beyond national control.

4.7 Conclusion

This paper has revisited the debate over the EU's democratic deficits by drawing upon de Vries' Benchmark theory and adopting her logic of using the national level as a reference point in the analysis of the EU's democratic deficit. The analysis presented in this study was motivated by the premise that countries with higher national levels of accountability, civil society engagement, and transparency, identified as three major weaknesses of EU democracy, differ more significantly from EU democracy. Consequently, if the EU's democratic deficits indeed have negative implications for support, higher levels of these national democratic standards should negatively impact attitudes towards the EU—a dynamic that may be mitigated by increased familiarity with the EU. In contrast to the paper's original expectations, the presented findings question the negative implications for support for the EU that many researchers associate with the EU's democratic deficit.

Instead, the presented findings show that higher democratic standards at the national level lead to increased support for the EU among citizens who are familiar with the EU, either due to living in a long-standing member country or discussing the EU at least occasionally. The macro-level results indicate that high democratic standards can negatively affect EU support in countries that have recently joined the EU, while the micro-level analysis reveals that attitudes towards EU membership among individuals who never discuss the EU are not impacted by their national democratic standards.

As the newest member, Croatia has been a member for ten years, and only about 8% of respondents in the sample used for the micro-analysis never discuss the EU, the positive effect of democratic standards on EU attitudes will apply to a majority of EU citizens. This suggests that democratic deficits resulting from differences between the EU and national democratic standards may not be as detrimental to EU support as previously thought. The perceived democratic qualities of the EU appear to extend beyond its formal structures.

Since its inception, the EU has been associated with the narrative that it is a peacekeeper in Europe (de Vries, 2023). Despite the oversimplification of this narrative, the EU's perception as a promoter of peaceful cooperation and its factual support for political change through economic interdependence (Gafuri, 2022) can contribute to the perception of its value as a defender of democracy among EU citizens. This perceived value may ultimately mitigate or even outweighing the impact of its democratic deficits on support.

Future research should investigate more explicitly people's perceived benefits of the EU for democratic governance that go beyond formal structure, specifically to what extent people see the EU as a defender of democracy and how this image affects EU support and tolerance for democratic deficits. It is also important to examine how the EU's limited success in sanctioning countries sliding into autocracy, such as Poland or Hungary, influences this perception. Furthermore, it would be beneficial to develop a direct measure of the quality of EU democracy that is comparable to measures provided by the Varieties of Democracies project, allowing for a more accurate assessment of the relationship between EU and national democratic standards.

In summary, these findings should not be taken to imply that the EU's democratic structures are irrelevant to EU support. Instead, they suggest that the EU has little to gain in terms of democratic legitimacy by improving its formal structures, but much to lose if it allows member states like Hungary and Poland to further deconstruct their democratic institutions. The present study suggests that it may be crucial for the EU to take more decisive action against member states that violate basic liberal democratic principles in order to maintain its own democratic legitimacy. By doing so, the EU maintains the democratic quality of its members, which serves as the foundation of its own legitimacy, as well as its image as a defender of democracy and ensures that its perceived value for democratic governance remains intact.

Chapter 5

Conclusion

In this dissertation, I have thoroughly examined the multifaceted relationships between political elites, public opinion, and democratic governance. The three distinct studies presented shed light on political polarisation, the impacts of elite communication on public attitudes towards asylum seekers, and the perception of the European Union's democratic deficits. Each study pushes the boundaries of traditional methodologies, providing fresh insights into the ways political elites shape public opinion and affect the functioning of democratic systems.

The first study uses a new measure of relative polarisation to explore the relationship between polarisation and citizens' dissatisfaction as well as engagement in democracy. The study provides robust evidence on the divergent effects of polarisation, which can both engage and dissatisfy people with democratic governance. These findings point to the possibility of a balanced state of polarisation in which both satisfaction and engagement in democracy are maximised. The second study uses a natural experiment approach to study the lasting effects of communication about the closure of the Balkan route. The results show that the impact of such communication is temporary, suggesting that the current survey experimental methods used to assess these effects may overestimate the endurance of communication effects in the real world. The final study investigates the democratic deficits of the EU using newly available macro data on EU support. Contrary to initial expectations, it provides evidence that the democratic deficits of the EU do not have a significant negative impact on support for the EU. Instead, the perception of the EU as a defender of democracy might outweigh these deficits. The findings indicate that maintaining democratic standards among member states is crucial for the EU's legitim-

acy. Each study in this dissertation challenges current methodologies and contributes to a deeper understanding of how elites shape public opinion and democratic governance. Together, they reveal the complex interplay between elite positioning, communication, and governance expectations in democratic systems.

The findings from the conducted research studies offer both meaningful insights and new perspectives in the domain of communication, democracy, and political science, making noteworthy contributions to existing theories and applications. In the first study, a new metric of relative polarisation was introduced, capturing individuals' perceived levels of polarisation and their preferences in relation to available party choices. This presents an innovative lens to view political preferences. Further, this study added robust proof supporting the view that polarisation could serve dual roles - acting as a catalyst for democratic engagement while concurrently causing a decrease in satisfaction with democracy. This demonstrates the existence of an equilibrium, an optimal representation state, in which political engagement and democratic satisfaction are balanced to produce an engaged and satisfied electorate.

The second study applied an original method, a natural experiment approach, to probe the endurance of communication effects. This strategy offers an alternative to standard survey experimental designs by identifying the causal effect of the announced Balkan route closure using existing data and data collection timing. Therefore, it holds the potential for easy and cost-effective application in multiple scenarios. In line with previous suggestions that scholars should not present composite or average findings on communication effects without discussing the degree of effect deterioration, this study emphasises the importance of considering the endurance of communication effects (Hill et al., 2013).

Lastly, the third study posed a challenge to the existing belief about the EU's democratic deficit negatively affects public support. The findings show the importance of the perception of the EU as a protector of democracy, which could possibly counterbalance the democratic deficits. This finding emphasises the importance of looking at perceived

democratic qualities and benefits, beyond formal structures. These studies have resulted in numerous valuable outputs such as articles and publications, adding to the existing scholarly discussion on these topics. Moreover, they provide valuable insights for practitioners in the field, offering actionable guidance based on the research findings.

In reflecting upon the limitations of the studies presented herein, the first study warrants particular attention. Despite contributing a pioneering step towards comprehending the dual nature of polarisation, this study doesn't resolve the concerns regarding rising polarisation. Ideally, a democratic society should be both actively involved and content with democratic functioning. The study, thus, should be seen as an initial exploration, with room for further investigation into feasible solutions for polarisation. The second study also encounters a few limitations. Its natural experimental design somewhat restricts the precise discernment of specific communication effects to which people were exposed. Although it incorporates the time dimension effectively, a longer observation period could have provided valuable insights into long-term communication effects. Future research could potentially ameliorate this limitation through innovative natural experimental causal designs that can isolate distinct communication effects, contributing to a more comprehensive understanding of real-world communication dynamics. Lastly, the third study lacks a measure that captures the EU's democratic standards in a way that is comparable to national democratic standards. As a result, it assumes the Democratic Gap between national and EU democratic standards using fixed effects, which captures all of the variations that a true measure of the Democratic Gap would capture but is still only an approximation. The inclusion of a measure that quantifies the quality of EU democracy, possibly on a scale that's comparable to the V-Dem project's data, could be an essential next step to enhance the understanding of the EU Democratic Gap. Additionally, the third study utilises proxy variables to gauge familiarity with the EU. Parameters such as a country's membership duration and the frequency of EU discussions among its citizens only approximate the true level of familiarity with the EU. Future research could benefit from the development of more accurate measures to determine citizens' actual familiarity with the EU, thus providing a more nuanced understanding of this relationship.

Drawing from the analyses performed, several suggestions arise for future research. To begin, the first analysis underscores the necessity for a more granular exploration of the ideal state of representation where political participation and democratic satisfaction harmonise. This calls for a meticulous discussion of personal-level mechanisms, individual perceptions of stakes, and their impact on voter turnout and contentment. Future efforts that assist in pinpointing this equilibrium could be pivotal for the survival of liberal democracies. A key proposition of the second study is the application of natural experiments within communication sciences. Natural experiments would allow us to garner a more nuanced understanding of how communication impacts function outside of survey experiment settings. Additionally, this study provides an exemplary methodology to test the longevity or decrease of communication effects over time. In the context of the third analysis, there is a distinct call for extended research into the role of the EU as a defender of democracy. It could be notably enlightening to further investigate to what degree the democratic legitimacy of the EU hinges more on its actions against member states that infringe on basic principles rather than its formal democratic structures. Finally, a recurring theme across all studies is the endorsement of a shift from traditional methodologies. They collectively urge the testing of fresh methodological pathways in order to yield new insights into relationships that might otherwise be unachievable using standard methods. In essence, this collection of studies emphasises the importance of innovation in research methodology, with the promise of further enriching the body of knowledge in their respective fields.

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Appendices

A Political Communication in the Real World

A.1 Regression Tables

Table A.1: Regression Tables - Main Analysis: Ability to Cope

	Dependent variable:					
	Ability to Cope					
	1 Days	2 Days	3 Days	4 Days	5 Days	6 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.477**	0.459***	0.385***	0.320***	0.216**	0.168*
	(0.191)	(0.150)	(0.130)	(0.117)	(0.105)	(0.092)
Constant	1.661***	1.691***	1.711***	1.778***	1.825***	1.874***
	(0.161)	(0.121)	(0.105)	(0.092)	(0.078)	(0.065)
Observations	194	267	385	478	578	733
R^2	0.031	0.034	0.022	0.015	0.007	0.005
Adjusted R^2	0.026	0.030	0.020	0.013	0.006	0.003
Residual Std. Error	1.205 (df = 192)	1.172 (df = 265)	1.219 (df = 383)	1.238 (df = 476)	1.258 (df = 576)	1.250 (df = 731)
F Statistic	6.238** (df = 1; 192)	9.330*** (df = 1; 265)	8.735*** (df = 1; 383)	7.478*** (df = 1; 476)	4.192** (df = 1; 576)	3.307* (df = 1; 731)
Note:						*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:					
	Ability to Cope					
	7 Days	8 Days	9 Days	10 Days	11 Days	12 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.112	0.047	0.020	−0.005	0.014	0.034
	(0.087)	(0.081)	(0.078)	(0.073)	(0.068)	(0.061)
Constant	1.928***	1.980***	1.971***	1.966***	1.931***	1.961***
	(0.058)	(0.052)	(0.050)	(0.048)	(0.045)	(0.042)
Observations	852	996	1,085	1,207	1,388	1,679
R^2	0.002	0.0003	0.0001	0.00000	0.00003	0.0002
Adjusted R^2	0.001	−0.001	−0.001	−0.001	−0.001	−0.0004
Residual Std. Error	1.258 (df = 850)	1.256 (df = 994)	1.261 (df = 1083)	1.258 (df = 1205)	1.248 (df = 1386)	1.246 (df = 1677)
F Statistic	1.658 (df = 1; 850)	0.343 (df = 1; 994)	0.065 (df = 1; 1083)	0.004 (df = 1; 1205)	0.043 (df = 1; 1386)	0.313 (df = 1; 1677)
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A.2: Regression Tables - Main Analysis: Asylum Attitudes

	Dependent variable:					
	Asylum Attitudes					
	19th + 20th	21th + 22th	23th + 24th	25th + 26th	27th + 28th	29th + 30th
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.425^{***}	0.199	0.129	0.252	−0.264	−0.098
	(0.149)	(0.159)	(0.184)	(0.187)	(0.189)	(0.152)
Constant	2.182^{***}	2.182^{***}	2.182^{***}	2.182^{***}	2.182^{***}	2.182^{***}
	(0.114)	(0.116)	(0.120)	(0.115)	(0.123)	(0.117)
Observations	239	212	173	159	172	242
R^2	0.033	0.007	0.003	0.011	0.011	0.002
Adjusted R^2	0.029	0.003	−0.003	0.005	0.005	−0.002
Residual Std. Error	1.133 (df = 237)	1.153 (df = 210)	1.196 (df = 171)	1.144 (df = 157)	1.228 (df = 170)	1.161 (df = 240)
F Statistic	8.175^{***} (df = 1; 237)	1.566 (df = 1; 210)	0.493 (df = 1; 171)	1.806 (df = 1; 157)	1.943 (df = 1; 170)	0.416 (df = 1; 240)

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.3: Regression Tables - Main Analysis: Policy Preference

	Dependent variable:											
	Refugee 1 Days	Security 1 Days	Refugee 2 Days	Security 2 Days	Refugee 3 Days	Security 3 Days	Refugee 4 Days	Security 4 Days	Refugee 5 Days	Security 5 Days	Refugee 6 Days	Security 6 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU-Turkey statement	−0.307	−1.176***	−0.187	−1.036***	−0.080	−0.863***	−0.035	−0.650***	0.015	−0.519**	0.054	−0.403**
	(0.402)	(0.384)	(0.341)	(0.313)	(0.273)	(0.262)	(0.238)	(0.231)	(0.215)	(0.202)	(0.190)	(0.181)
Constant	−0.654*	−0.174	−0.847***	−0.241	−0.727***	−0.288	−0.730***	−0.376**	−0.834***	−0.397***	−0.875***	−0.492***
	(0.342)	(0.296)	(0.282)	(0.233)	(0.226)	(0.197)	(0.192)	(0.172)	(0.165)	(0.143)	(0.137)	(0.121)
Akaike Inf. Crit.	381.802	381.802	519.903	519.903	770.456	770.456	966.629	966.629	1,173.967	1,173.967	1,475.066	1,475.066
Note:												*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:											
	Refugee 7 Days	Security 7 Days	Refugee 8 Days	Security 8 Days	Refugee 9 Days	Security 9 Days	Refugee 10 Days	Security 10 Days	Refugee 11 Days	Security 11 Days	Refugee 12 Days	Security 12 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU Summit	−0.076	−0.345**	−0.134	−0.275*	−0.151	−0.134	−0.116	−0.072	−0.061	−0.010	0.013	−0.096
	(0.173)	(0.171)	(0.160)	(0.162)	(0.155)	(0.150)	(0.149)	(0.141)	(0.140)	(0.130)	(0.123)	(0.119)
Constant	−0.743***	−0.556***	−0.658***	−0.604***	−0.683***	−0.607***	−0.745***	−0.606***	−0.816***	−0.616***	−0.767***	−0.599***
	(0.117)	(0.110)	(0.102)	(0.101)	(0.100)	(0.097)	(0.096)	(0.091)	(0.092)	(0.086)	(0.085)	(0.080)
Akaike Inf. Crit.	1,723.209	1,723.209	2,023.340	2,023.340	2,218.255	2,218.255	2,464.287	2,464.287	2,816.208	2,816.208	3,431.279	3,431.279
Note:												*p<0.1; **p<0.05; ***p<0.01

(a) 1-Day Bandwidth

	Dependent variable:					
	Ability to Cope					
	March 12th	March 13th	March 14th	March 15th	March 16th	March 17th
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	−0.130	−0.169	−0.032	−0.063	−0.231	−0.065
	(0.143)	(0.160)	(0.165)	(0.204)	(0.228)	(0.229)
Constant	2.182***	2.133***	1.983***	1.935***	1.978***	1.756***
	(0.114)	(0.130)	(0.118)	(0.148)	(0.182)	(0.191)
Observations	334	290	237	163	124	135
Log Likelihood	−550.272	−485.565	−392.505	−274.100	−200.710	−218.957
Akaike Inf. Crit.	1,104.543	975.129	789.010	552.199	405.420	441.913
Note:						*p<0.1; **p<0.05; ***p<0.01

(b) 2-Day Bandwidth

	Dependent variable:			
	Ability to Cope			
	March 13th	March 14th	March 15th	March 16th
	(1)	(2)	(3)	(4)
EU-Turkey statement	−0.194	−0.150	−0.133	−0.240
	(0.118)	(0.134)	(0.145)	(0.156)
Constant	2.160***	2.052***	1.964***	1.951***
	(0.085)	(0.088)	(0.091)	(0.113)
Observations	456	376	316	257
Log Likelihood	−753.168	−628.200	−520.676	−422.334
Akaike Inf. Crit.	1,510.336	1,260.400	1,045.353	848.667
Note:				*p<0.1; **p<0.05; ***p<0.01

(c) 3-Day Bandwidth

	Dependent variable:	
	Ability to Cope	
	March 14th	March 15th
	(1)	(2)
EU-Turkey statement	−0.228**	−0.243**
	(0.112)	(0.120)
Constant	2.099***	2.021***
	(0.069)	(0.074)
Observations	535	470
Log Likelihood	−882.201	−777.409
Akaike Inf. Crit.	1,768.402	1,558.817
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table A.4: Regression Tables - Placebo Tests: Ability to Cope

(a) 1-Day Bandwidth

	Dependent variable:					
	Asylum Attitudes					
	March 12th	March 13th	March 14th	March 15th	March 16th	March 17th
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	−0.008	−0.187	−0.210	0.177	0.018	−0.143
	(0.143)	(0.159)	(0.166)	(0.206)	(0.219)	(0.225)
Constant	2.423***	2.455***	2.378***	2.114***	2.261***	2.325***
	(0.114)	(0.129)	(0.121)	(0.148)	(0.174)	(0.190)
Observations	335	291	236	165	125	139
R^2	0.00001	0.005	0.007	0.005	0.0001	0.003
Adjusted R^2	−0.003	0.001	0.003	−0.002	−0.008	−0.004
Residual Std. Error	1.261 (df = 333)	1.295 (df = 289)	1.274 (df = 234)	1.319 (df = 163)	1.178 (df = 123)	1.201 (df = 137)
F Statistic	0.003 (df = 1; 333)	1.376 (df = 1; 289)	1.604 (df = 1; 234)	0.739 (df = 1; 163)	0.006 (df = 1; 123)	0.405 (df = 1; 137)
Note:						*p<0.1; **p<0.05; ***p<0.01

(b) 2-Day Bandwidth

	Dependent variable:			
	Asylum Attitudes			
	March 13th	March 14th	March 15th	March 16th
	(1)	(2)	(3)	(4)
EU-Turkey statement	−0.171	−0.209	0.004	0.055
	(0.119)	(0.133)	(0.144)	(0.154)
Constant	2.437***	2.415***	2.268***	2.168***
	(0.085)	(0.088)	(0.091)	(0.112)
Observations	460	377	315	264
R^2	0.004	0.007	0.00000	0.0005
Adjusted R^2	0.002	0.004	−0.003	−0.003
Residual Std. Error	1.276 (df = 458)	1.280 (df = 375)	1.254 (df = 313)	1.253 (df = 262)
F Statistic	2.054 (df = 1; 458)	2.474 (df = 1; 375)	0.001 (df = 1; 313)	0.127 (df = 1; 262)
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A.5: Asylum Attitudes

(a) 3-Day Bandwidth

	Dependent variable:	
	Asylum Attitudes	
	March 14th	March 15th
	(1)	(2)
EU-Turkey statement	−0.207*	−0.101
	(0.112)	(0.118)
Constant	2.418***	2.333***
	(0.069)	(0.074)
Observations	539	476
R^2	0.006	0.002
Adjusted R^2	0.004	−0.001
Residual Std. Error	1.261 (df = 537)	1.254 (df = 474)
F Statistic	3.423* (df = 1; 537)	0.732 (df = 1; 474)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A.6: Regression Tables - Placebo Tests: Asylum Attitudes

(a) 1-Day Bandwidth

	Dependent variable:											
	Refugee March 12th	Security March 12th	Refugee March 13th	Security March 13th	Refugee March 14th	Security March 14th	Refugee March 15th	Security March 15th	Refugee March 16th	Security March 16th	Refugee March 17th	Security March 17th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU-Turkey statement	−0.276	0.045	−0.678**	0.258	0.020	0.212	0.476	−0.091	−0.045	0.273	−0.355	0.164
	(0.270)	(0.301)	(0.304)	(0.323)	(0.347)	(0.310)	(0.415)	(0.363)	(0.475)	(0.444)	(0.475)	(0.439)
Constant	−0.370*	−0.829***	−0.341	−0.862***	−0.969***	−0.728***	−1.099***	−0.445*	−0.738**	−0.651*	−0.493	−0.406
	(0.211)	(0.245)	(0.231)	(0.274)	(0.250)	(0.230)	(0.320)	(0.256)	(0.367)	(0.356)	(0.383)	(0.373)
Akaike Inf. Crit.	674.854	674.854	582.372	582.372	477.998	477.998	347.558	347.558	261.090	261.090	290.344	290.344

Note: *p<0.1; **p<0.05; ***p<0.01

(b) 2-Day Bandwidth

	Dependent variable:							
	Refugee March 13th	Security March 13th	Refugee March 14th	Security March 14th	Refugee March 15th	Security March 15th	Refugee March 16th	Security March 16th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EU-Turkey statement	−0.602***	0.231	−0.181	0.294	0.255	0.127	0.222	0.229
	(0.233)	(0.239)	(0.263)	(0.253)	(0.305)	(0.272)	(0.330)	(0.286)
Constant	−0.357**	−0.844***	−0.646***	−0.785***	−1.019***	−0.604***	−0.949***	−0.516**
	(0.156)	(0.182)	(0.168)	(0.176)	(0.197)	(0.171)	(0.240)	(0.208)
Akaike Inf. Crit.	923.117	923.117	771.674	771.674	635.894	635.894	543.675	543.675

Note: *p<0.1; **p<0.05; ***p<0.01

(c) 3-Day Bandwidth

	Dependent variable:			
	Refugee March 14th	Security March 14th	Refugee March 15th	Security March 15th
	(1)	(2)	(3)	(4)
EU-Turkey statement	−0.345	0.336	0.021	0.303
	(0.229)	(0.217)	(0.243)	(0.225)
Constant	−0.541***	−0.800***	−0.751***	−0.679***
	(0.131)	(0.143)	(0.148)	(0.145)
Akaike Inf. Crit.	1,086.198	1,086.198	969.950	969.950

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.7: Regression Tables - Placebo Tests: Policy Preferences

Table A.8: Regression Tables - Omitted Respondents March 13th and 14th: Asylum Attitudes

	Dependent variable:							
	Asylum Attitudes							
	5 Days	6 Days	7 Days	8 Days	9 Days	10 Days	11 Days	12 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EU-Turkey statement	0.229**	0.216**	0.146	0.080	0.045	0.017	−0.009	−0.005
	(0.109)	(0.106)	(0.093)	(0.085)	(0.082)	(0.077)	(0.070)	(0.063)
Constant	2.232***	2.232***	2.311***	2.345***	2.327***	2.308***	2.313***	2.323***
	(0.087)	(0.087)	(0.071)	(0.060)	(0.058)	(0.055)	(0.050)	(0.045)
Observations	512	551	673	819	907	1,027	1,201	1,488
R^2	0.009	0.007	0.004	0.001	0.0003	0.00005	0.00001	0.00000
Adjusted R^2	0.007	0.006	0.002	−0.0001	−0.001	−0.001	−0.001	−0.001
Residual Std. Error	1.182 (df = 510)	1.179 (df = 549)	1.194 (df = 671)	1.216 (df = 817)	1.234 (df = 905)	1.234 (df = 1025)	1.220 (df = 1199)	1.208 (df = 1486)
F Statistic	4.450** (df = 1; 510)	4.109** (df = 1; 549)	2.463 (df = 1; 671)	0.878 (df = 1; 817)	0.303 (df = 1; 905)	0.048 (df = 1; 1025)	0.017 (df = 1; 1199)	0.005 (df = 1; 1486)
Note:								*p<0.1; **p<0.05; ***p<0.01

Table A.9: Regression Tables - Omitted Respondents March 13th and 14th: Ability to Cope

	Dependent variable:							
	Asylum Attitudes							
	5 Days	6 Days	7 Days	8 Days	9 Days	10 Days	11 Days	12 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
EU-Turkey statement	0.229**	0.216**	0.146	0.080	0.045	0.017	−0.009	−0.005
	(0.109)	(0.106)	(0.093)	(0.085)	(0.082)	(0.077)	(0.070)	(0.063)
Constant	2.232***	2.232***	2.311***	2.345***	2.327***	2.308***	2.313***	2.323***
	(0.087)	(0.087)	(0.071)	(0.060)	(0.058)	(0.055)	(0.050)	(0.045)
Observations	512	551	673	819	907	1,027	1,201	1,488
R^2	0.009	0.007	0.004	0.001	0.0003	0.00005	0.00001	0.00000
Adjusted R^2	0.007	0.006	0.002	−0.0001	−0.001	−0.001	−0.001	−0.001
Residual Std. Error	1.182 (df = 510)	1.179 (df = 549)	1.194 (df = 671)	1.216 (df = 817)	1.234 (df = 905)	1.234 (df = 1025)	1.220 (df = 1199)	1.208 (df = 1486)
F Statistic	4.450** (df = 1; 510)	4.109** (df = 1; 549)	2.463 (df = 1; 671)	0.878 (df = 1; 817)	0.303 (df = 1; 905)	0.048 (df = 1; 1025)	0.017 (df = 1; 1199)	0.005 (df = 1; 1486)
Note:								*p<0.1; **p<0.05; ***p<0.01

Table A.10: Regression Tables - Omitted Respondents March 13th and 14th: Policy Preference

	Dependent variable:															
	Refugee 5 Days	Security 5 Days	Refugee 6 Days	Security 6 Days	Refugee 7 Days	Security 7 Days	Refugee 8 Days	Security 8 Days	Refugee 9 Days	Security 9 Days	Refugee 10 Days	Security 10 Days	Refugee 11 Days	Security 11 Days	Refugee 12 Days	Security 12 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
EU-Turkey statement	0.015	−0.519**	0.054	−0.403**	−0.076	−0.345**	−0.134	−0.275*	−0.151	−0.134	−0.116	−0.072	−0.061	−0.010	0.013	−0.096
	(0.215)	(0.202)	(0.190)	(0.181)	(0.173)	(0.171)	(0.160)	(0.162)	(0.155)	(0.150)	(0.149)	(0.141)	(0.140)	(0.130)	(0.123)	(0.119)
Constant	−0.834***	−0.397***	−0.875***	−0.492***	−0.743***	−0.556***	−0.658***	−0.604***	−0.683***	−0.607***	−0.745***	−0.606***	−0.816***	−0.616***	−0.767***	−0.599***
	(0.165)	(0.143)	(0.137)	(0.121)	(0.117)	(0.110)	(0.102)	(0.101)	(0.100)	(0.097)	(0.096)	(0.091)	(0.092)	(0.086)	(0.085)	(0.080)
Akaike Inf. Crit.	1,173.967	1,173.967	1,475.066	1,475.066	1,723.209	1,723.209	2,023.340	2,023.340	2,218.255	2,218.255	2,464.287	2,464.287	2,816.208	2,816.208	3,431.279	3,431.279

Note: *p<0.1; **p<0.05; ***p<0.01

Table A.11: Regression Tables - Time Control: Ability to Cope

	Dependent variable:					
	Ability to Cope					
	1 Days	2 Days	3 Days	4 Days	5 Days	6 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	1.018***	0.676**	0.655***	0.593***	0.701***	0.585***
	(0.382)	(0.295)	(0.241)	(0.221)	(0.208)	(0.188)
Time Control	−0.339	−0.095	−0.079	−0.063	−0.091***	−0.063**
	(0.208)	(0.111)	(0.059)	(0.043)	(0.034)	(0.025)
Constant	7.768**	3.365*	3.062***	2.823***	3.262***	2.813***
	(3.748)	(1.962)	(1.018)	(0.722)	(0.538)	(0.375)
Observations	194	267	385	478	578	733
R^2	0.045	0.037	0.027	0.020	0.020	0.013
Adjusted R^2	0.035	0.029	0.022	0.016	0.016	0.011
Residual Std. Error	1.200 (df = 191)	1.173 (df = 264)	1.218 (df = 382)	1.236 (df = 475)	1.251 (df = 575)	1.245 (df = 730)
F Statistic	4.476** (df = 2; 191)	5.026*** (df = 2; 264)	5.266*** (df = 2; 382)	4.812*** (df = 2; 475)	5.764*** (df = 2; 575)	4.901*** (df = 2; 730)
Note:						*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:					
	Ability to Cope					
	7 Days	8 Days	9 Days	10 Days	11 Days	12 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.575***	0.576***	0.528***	0.463***	0.318**	0.224*
	(0.179)	(0.167)	(0.157)	(0.148)	(0.137)	(0.128)
Time Control	−0.062***	−0.062***	−0.054***	−0.045***	−0.025**	−0.013*
	(0.021)	(0.017)	(0.015)	(0.012)	(0.010)	(0.008)
Constant	2.813***	2.832***	2.701***	2.548***	2.246***	2.118***
	(0.305)	(0.241)	(0.204)	(0.167)	(0.131)	(0.102)
Observations	852	996	1,085	1,207	1,388	1,679
R^2	0.012	0.013	0.013	0.011	0.005	0.002
Adjusted R^2	0.010	0.011	0.011	0.009	0.003	0.001
Residual Std. Error	1.253 (df = 849)	1.249 (df = 993)	1.253 (df = 1082)	1.251 (df = 1204)	1.246 (df = 1385)	1.245 (df = 1676)
F Statistic	5.212*** (df = 2; 849)	6.736*** (df = 2; 993)	6.876*** (df = 2; 1082)	6.656*** (df = 2; 1204)	3.270** (df = 2; 1385)	1.569 (df = 2; 1676)
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A.12: Regression Tables - Time Control: Asylum Attitudes

	Dependent variable:											
	Asylum Attitudes											
	1 Days	2 Days	3 Days	4 Days	5 Days	6 Days						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU-Turkey statement	0.867**	0.634**	0.717***	0.643***	0.532***	0.528***	0.494***	0.531***	0.533***	0.498***	0.429***	0.319**
	(0.361)	(0.272)	(0.225)	(0.207)	(0.197)	(0.180)	(0.171)	(0.162)	(0.154)	(0.145)	(0.134)	(0.124)
Time Control	−0.259	−0.092	−0.128**	−0.093**	−0.050	−0.050**	−0.044**	−0.051***	−0.051***	−0.045***	−0.036***	−0.022***
	(0.198)	(0.103)	(0.056)	(0.041)	(0.032)	(0.024)	(0.020)	(0.017)	(0.014)	(0.012)	(0.010)	(0.008)
Constant	6.805*	3.796**	4.424***	3.780***	2.992***	3.004***	2.931***	3.012***	2.990***	2.888***	2.751***	2.573***
	(3.570)	(1.817)	(0.965)	(0.681)	(0.511)	(0.362)	(0.293)	(0.235)	(0.200)	(0.164)	(0.130)	(0.100)
Observations	200	275	392	487	591	741	863	1,009	1,097	1,217	1,391	1,678
Log Likelihood	−312.084	−416.988	−614.197	−770.788	−950.923	−1,191.474	−1,392.812	−1,638.386	−1,791.543	−1,986.638	−2,256.337	−2,708.981
Akaike Inf. Crit.	630.168	839.976	1,234.393	1,547.575	1,907.846	2,388.947	2,791.625	3,282.773	3,589.086	3,979.275	4,518.674	5,423.962
Note:												*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:											
	Asylum Attitudes											
	7 Days	8 Days	9 Days	10 Days	11 Days	12 Days						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU-Turkey statement	0.867**	0.634**	0.717***	0.643***	0.532***	0.528***	0.494***	0.531***	0.533***	0.498***	0.429***	0.319**
	(0.361)	(0.272)	(0.225)	(0.207)	(0.197)	(0.180)	(0.171)	(0.162)	(0.154)	(0.145)	(0.134)	(0.124)
Time Control	−0.259	−0.092	−0.128**	−0.093**	−0.050	−0.050**	−0.044**	−0.051***	−0.051***	−0.045***	−0.036***	−0.022***
	(0.198)	(0.103)	(0.056)	(0.041)	(0.032)	(0.024)	(0.020)	(0.017)	(0.014)	(0.012)	(0.010)	(0.008)
Constant	6.805*	3.796**	4.424***	3.780***	2.992***	3.004***	2.931***	3.012***	2.990***	2.888***	2.751***	2.573***
	(3.570)	(1.817)	(0.965)	(0.681)	(0.511)	(0.362)	(0.293)	(0.235)	(0.200)	(0.164)	(0.130)	(0.100)
Observations	200	275	392	487	591	741	863	1,009	1,097	1,217	1,391	1,678
Log Likelihood	−312.084	−416.988	−614.197	−770.788	−950.923	−1,191.474	−1,392.812	−1,638.386	−1,791.543	−1,986.638	−2,256.337	−2,708.981
Akaike Inf. Crit.	630.168	839.976	1,234.393	1,547.575	1,907.846	2,388.947	2,791.625	3,282.773	3,589.086	3,979.275	4,518.674	5,423.962
Note:												*p<0.1; **p<0.05; ***p<0.01

Table A.13: Regression Tables - Time Control: Policy Preference

	Dependent variable:											
	Refugee 1 Days	Security 1 Days	Refugee 2 Days	Security 2 Days	Refugee 3 Days	Security 3 Days	Refugee 4 Days	Security 4 Days	Refugee 5 Days	Security 5 Days	Refugee 6 Days	Security 6 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU-Turkey statement	−0.603	−1.863**	−0.234	−1.568**	−0.651	−1.548***	−0.571	−1.629***	−0.363	−1.523***	−0.266	−1.334***
	(0.799)	(0.932)	(0.643)	(0.656)	(0.496)	(0.516)	(0.449)	(0.462)	(0.429)	(0.411)	(0.390)	(0.370)
Time Control	0.187	0.426	0.021	0.232	0.166	0.199	0.123	0.223**	0.070	0.187***	0.048	0.140***
	(0.432)	(0.515)	(0.241)	(0.248)	(0.120)	(0.127)	(0.087)	(0.090)	(0.069)	(0.066)	(0.051)	(0.048)
Constant	−4.022	−7.839	−1.219	−4.326	−3.575*	−3.704*	−2.763*	−4.083***	−1.940*	−3.357***	−1.594**	−2.599***
	(7.776)	(9.269)	(4.253)	(4.382)	(2.071)	(2.190)	(1.454)	(1.505)	(1.099)	(1.057)	(0.777)	(0.738)
Akaike Inf. Crit.	385.058	385.058	523.002	523.002	771.051	771.051	963.918	963.918	1,169.828	1,169.828	1,470.597	1,470.597
Note:												*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:											
	Refugee 7 Days	Security 7 Days	Refugee 8 Days	Security 8 Days	Refugee 9 Days	Security 9 Days	Refugee 10 Days	Security 10 Days	Refugee 11 Days	Security 11 Days	Refugee 12 Days	Security 12 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU-Turkey statement	−0.050	−1.249***	0.002	−1.183***	0.024	−1.221***	−0.076	−1.084***	−0.177	−0.935***	−0.237	−0.618**
	(0.364)	(0.352)	(0.337)	(0.330)	(0.320)	(0.309)	(0.302)	(0.288)	(0.284)	(0.268)	(0.259)	(0.253)
Time Control	−0.003	0.120***	−0.016	0.107***	−0.019	0.115***	−0.004	0.096***	0.010	0.076***	0.018	0.037**
	(0.043)	(0.041)	(0.035)	(0.034)	(0.030)	(0.028)	(0.025)	(0.024)	(0.021)	(0.019)	(0.016)	(0.016)
Constant	−0.694	−2.298***	−0.440	−2.085***	−0.433	−2.170***	−0.696**	−1.865***	−0.935***	−1.578***	−0.973***	−1.031***
	(0.619)	(0.601)	(0.484)	(0.480)	(0.412)	(0.398)	(0.340)	(0.326)	(0.272)	(0.258)	(0.207)	(0.202)
Akaike Inf. Crit.	1,717.191	1,717.191	2,014.692	2,014.692	2,201.142	2,201.142	2,449.174	2,449.174	2,803.427	2,803.427	3,429.646	3,429.646
Note:												*p<0.1; **p<0.05; ***p<0.01

Table A.14: Regression Tables - Alternative Outcome: Support for Environmental Tolls

	Dependent variable:					
	Support for Environmental Tolls					
	1 Days	2 Days	3 Days	4 Days	5 Days	6 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	−0.051	−0.029	−0.015	−0.032	−0.033	−0.038
	(0.059)	(0.049)	(0.042)	(0.037)	(0.033)	(0.029)
Constant	0.844***	0.819***	0.800***	0.804***	0.804***	0.805***
	(0.049)	(0.039)	(0.033)	(0.030)	(0.025)	(0.021)
Observations	209	291	420	519	621	779
Log Likelihood	−102.204	−146.721	−219.302	−276.065	−328.356	−411.704
Akaike Inf. Crit.	208.408	297.442	442.605	556.130	660.712	827.409
Note:						*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:					
	Support for Environmental Tolls					
	7 Days	8 Days	9 Days	10 Days	11 Days	12 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	−0.023	−0.018	−0.016	−0.008	−0.009	0.003
	(0.028)	(0.026)	(0.025)	(0.024)	(0.022)	(0.020)
Constant	0.781***	0.780***	0.777***	0.778***	0.781***	0.775***
	(0.019)	(0.017)	(0.016)	(0.016)	(0.015)	(0.014)
Observations	902	1,053	1,147	1,274	1,462	1,770
Log Likelihood	−499.136	−578.601	−634.336	−696.664	−795.581	−961.656
Akaike Inf. Crit.	1,002.273	1,161.203	1,272.672	1,397.329	1,595.162	1,927.311
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A.15: Regression Tables - Analysis with Control: Ability to Cope

	Dependent variable:					
	Ability to Cope					
	1 Days	2 Days	3 Days	4 Days	5 Days	6 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.598***	0.496***	0.397***	0.315***	0.218**	0.165*
	(0.182)	(0.145)	(0.127)	(0.113)	(0.102)	(0.090)
East Germany	−0.601***	−0.357*	−0.302*	−0.290**	−0.280**	−0.315***
	(0.214)	(0.184)	(0.160)	(0.141)	(0.134)	(0.116)
University Degree	0.108	0.111	0.090	0.127	0.078	0.092
	(0.234)	(0.217)	(0.195)	(0.179)	(0.163)	(0.142)
Age > 65	−0.006	−0.045	−0.088	−0.074	0.010	0.069
	(0.194)	(0.167)	(0.148)	(0.134)	(0.126)	(0.110)
Abitur	0.754***	0.605***	0.616***	0.615***	0.627***	0.573***
	(0.188)	(0.166)	(0.141)	(0.127)	(0.118)	(0.105)
Gender	0.190	0.061	0.135	0.105	0.068	0.077
	(0.164)	(0.140)	(0.122)	(0.110)	(0.103)	(0.091)
Constant	1.236***	1.455***	1.449***	1.533***	1.579***	1.640***
	(0.202)	(0.162)	(0.144)	(0.126)	(0.113)	(0.098)
Observations	194	266	384	477	576	730
R^2	0.172	0.120	0.100	0.094	0.081	0.073
Adjusted R^2	0.146	0.100	0.085	0.083	0.072	0.066
Residual Std. Error	1.129 (df = 187)	1.130 (df = 259)	1.178 (df = 377)	1.194 (df = 470)	1.214 (df = 569)	1.209 (df = 723)
F Statistic	6.479*** (df = 6; 187)	5.901*** (df = 6; 259)	6.950*** (df = 6; 377)	8.151*** (df = 6; 470)	8.387*** (df = 6; 569)	9.521*** (df = 6; 723)
Note:						*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:					
	Ability to Cope					
	7 Days	8 Days	9 Days	10 Days	11 Days	12 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.117	0.053	0.028	0.001	0.020	0.032
	(0.084)	(0.078)	(0.075)	(0.071)	(0.065)	(0.059)
East Germany	−0.262**	−0.248**	−0.247***	−0.236***	−0.240***	−0.258***
	(0.109)	(0.098)	(0.094)	(0.089)	(0.080)	(0.073)
University Degree	0.117	0.169	0.093	0.088	0.183*	0.224**
	(0.134)	(0.121)	(0.116)	(0.112)	(0.104)	(0.095)
Age > 65	0.113	0.170*	0.195**	0.183**	0.166**	0.158**
	(0.103)	(0.095)	(0.092)	(0.087)	(0.078)	(0.073)
Abitur	0.615***	0.632***	0.653***	0.679***	0.674***	0.640***
	(0.097)	(0.089)	(0.086)	(0.082)	(0.076)	(0.069)
Gender	0.078	0.026	0.002	−0.002	0.019	0.005
	(0.084)	(0.077)	(0.074)	(0.070)	(0.064)	(0.059)
Constant	1.653***	1.703***	1.708***	1.700***	1.651***	1.706***
	(0.087)	(0.077)	(0.074)	(0.070)	(0.065)	(0.060)
Observations	849	993	1,082	1,202	1,383	1,673
R^2	0.075	0.083	0.080	0.083	0.091	0.088
Adjusted R^2	0.069	0.077	0.074	0.078	0.087	0.085
Residual Std. Error	1.214 (df = 842)	1.206 (df = 986)	1.212 (df = 1075)	1.207 (df = 1195)	1.192 (df = 1376)	1.191 (df = 1666)
F Statistic	11.447*** (df = 6; 842)	14.824*** (df = 6; 986)	15.502*** (df = 6; 1075)	17.951*** (df = 6; 1195)	22.924*** (df = 6; 1376)	26.903*** (df = 6; 1666)
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A.16: Regression Tables - Analysis with Control: Asylum Attitudes

	Dependent variable:					
	Asylum Attitudes					
	1 Days	2 Days	3 Days	4 Days	5 Days	6 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.631***	0.481***	0.311**	0.258**	0.283***	0.204**
	(0.170)	(0.133)	(0.121)	(0.107)	(0.097)	(0.086)
East Germany	−0.507**	−0.392**	−0.291*	−0.320**	−0.325**	−0.346***
	(0.204)	(0.171)	(0.154)	(0.135)	(0.127)	(0.111)
University Degree	0.018	−0.028	0.024	0.027	−0.011	0.012
	(0.218)	(0.198)	(0.185)	(0.169)	(0.154)	(0.137)
Age > 65	0.122	−0.033	0.063	0.092	0.211*	0.227**
	(0.185)	(0.156)	(0.143)	(0.129)	(0.120)	(0.106)
Abitur	0.819***	0.665***	0.566***	0.599***	0.621***	0.569***
	(0.177)	(0.151)	(0.135)	(0.121)	(0.112)	(0.101)
Gender	0.004	0.060	0.072	0.081	0.020	0.049
	(0.154)	(0.129)	(0.116)	(0.105)	(0.098)	(0.087)
Constant	1.717***	1.932***	1.973***	1.972***	1.944***	2.011***
	(0.193)	(0.151)	(0.139)	(0.121)	(0.108)	(0.094)
Observations	200	274	391	486	589	738
R^2	0.179	0.135	0.080	0.086	0.090	0.079
Adjusted R^2	0.154	0.115	0.065	0.074	0.081	0.071
Residual Std. Error	1.079 (df = 193)	1.056 (df = 267)	1.136 (df = 384)	1.145 (df = 479)	1.168 (df = 582)	1.171 (df = 731)
F Statistic	7.030*** (df = 6; 193)	6.920*** (df = 6; 267)	5.534*** (df = 6; 384)	7.488*** (df = 6; 479)	9.622*** (df = 6; 582)	10.416*** (df = 6; 731)
Note:						*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:					
	Asylum Attitudes					
	7 Days	8 Days	9 Days	10 Days	11 Days	12 Days
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.173**	0.114	0.075	0.036	0.013	0.016
	(0.081)	(0.076)	(0.073)	(0.070)	(0.064)	(0.057)
East Germany	−0.318***	−0.338***	−0.336***	−0.322***	−0.301***	−0.291***
	(0.104)	(0.095)	(0.092)	(0.088)	(0.079)	(0.072)
University Degree	0.002	0.085	0.009	0.001	0.053	0.164*
	(0.128)	(0.117)	(0.114)	(0.110)	(0.102)	(0.092)
Age > 65	0.227**	0.279***	0.311***	0.276***	0.276***	0.289***
	(0.099)	(0.092)	(0.090)	(0.085)	(0.077)	(0.071)
Abitur	0.618***	0.639***	0.657***	0.668***	0.681***	0.595***
	(0.094)	(0.087)	(0.085)	(0.081)	(0.075)	(0.068)
Gender	0.054	0.029	0.009	0.017	0.028	0.048
	(0.081)	(0.075)	(0.073)	(0.069)	(0.063)	(0.057)
Constant	2.029***	2.043***	2.043***	2.033***	2.018***	2.031***
	(0.084)	(0.074)	(0.073)	(0.069)	(0.064)	(0.058)
Observations	860	1,006	1,094	1,212	1,386	1,672
R^2	0.080	0.090	0.086	0.083	0.090	0.084
Adjusted R^2	0.074	0.085	0.081	0.079	0.086	0.080
Residual Std. Error	1.175 (df = 853)	1.180 (df = 999)	1.195 (df = 1087)	1.195 (df = 1205)	1.177 (df = 1379)	1.168 (df = 1665)
F Statistic	12.431*** (df = 6; 853)	16.551*** (df = 6; 999)	17.004*** (df = 6; 1087)	18.207*** (df = 6; 1205)	22.786*** (df = 6; 1379)	25.289*** (df = 6; 1665)
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A.17: Regression Tables - Analysis with Control: Policy Preferences

	Dependent variable:											
	Refugee 1 Days	Security 1 Days	Refugee 2 Days	Security 2 Days	Refugee 3 Days	Security 3 Days	Refugee 4 Days	Security 4 Days	Refugee 5 Days	Security 5 Days	Refugee 6 Days	Security 6 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU-Turkey statement	−0.009	−1.282***	−0.040	−1.059***	−0.087	−0.952***	−0.060	−0.715***	0.002	−0.612***	0.049	−0.448**
	(0.438)	(0.402)	(0.364)	(0.317)	(0.285)	(0.269)	(0.247)	(0.237)	(0.224)	(0.208)	(0.197)	(0.185)
East Germany	−1.658**	0.223	−1.395**	0.382	−1.052**	0.286	−0.689*	0.504*	−0.720**	0.506**	−0.929***	0.564***
	(0.784)	(0.477)	(0.646)	(0.382)	(0.442)	(0.328)	(0.365)	(0.280)	(0.359)	(0.254)	(0.335)	(0.219)
University Degree	0.401	−0.766	0.491	−0.226	0.177	−0.247	0.480	−0.039	0.472	0.061	0.307	0.187
	(0.485)	(0.674)	(0.445)	(0.556)	(0.379)	(0.476)	(0.344)	(0.420)	(0.313)	(0.362)	(0.276)	(0.315)
Age > 65	−0.761	−0.667	−0.561	−0.315	−0.503	−0.431	−0.687**	−0.382	−0.790***	−0.525**	−0.732***	−0.631***
	(0.507)	(0.484)	(0.430)	(0.383)	(0.332)	(0.325)	(0.307)	(0.281)	(0.295)	(0.256)	(0.257)	(0.227)
Abitur	1.295***	−0.142	1.263***	−0.053	0.874***	−0.186	0.676**	−0.163	0.759***	−0.191	0.740***	−0.404*
	(0.452)	(0.451)	(0.388)	(0.384)	(0.299)	(0.317)	(0.265)	(0.277)	(0.247)	(0.245)	(0.220)	(0.223)
Gender	−0.275	−0.403	0.065	−0.301	−0.362	−0.649**	−0.423*	−0.709***	−0.413*	−0.700***	−0.406**	−0.594***
	(0.388)	(0.385)	(0.339)	(0.314)	(0.264)	(0.268)	(0.236)	(0.236)	(0.223)	(0.210)	(0.199)	(0.186)
Constant	−1.218**	0.362	−1.451***	−0.031	−0.732**	0.244	−0.656**	0.068	−0.797***	0.099	−0.794***	−0.016
	(0.531)	(0.435)	(0.436)	(0.339)	(0.336)	(0.301)	(0.283)	(0.257)	(0.258)	(0.220)	(0.223)	(0.191)
Akaike Inf. Crit.	367.176	367.176	504.263	504.263	753.526	753.526	941.491	941.491	1,134.282	1,134.282	1,414.523	1,414.523
Note:												*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:											
	Refugee 7 Days	Security 7 Days	Refugee 8 Days	Security 8 Days	Refugee 9 Days	Security 9 Days	Refugee 10 Days	Security 10 Days	Refugee 11 Days	Security 11 Days	Refugee 12 Days	Security 12 Days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU Summit	−0.069	−0.381**	−0.103	−0.275*	−0.130	−0.141	−0.124	−0.087	−0.069	−0.029	−0.022	−0.123
	(0.180)	(0.174)	(0.166)	(0.164)	(0.160)	(0.153)	(0.154)	(0.143)	(0.145)	(0.132)	(0.128)	(0.121)
East Germany	−0.683**	0.458**	−0.712***	0.379**	−0.729***	0.375**	−0.764***	0.268	−0.532***	0.318**	−0.500***	0.297**
	(0.279)	(0.206)	(0.246)	(0.190)	(0.239)	(0.180)	(0.229)	(0.171)	(0.201)	(0.156)	(0.176)	(0.143)
University Degree	0.299	−0.017	0.368	−0.015	0.251	−0.003	0.279	0.066	0.334*	0.023	0.270	−0.149
	(0.253)	(0.302)	(0.230)	(0.280)	(0.221)	(0.263)	(0.215)	(0.252)	(0.202)	(0.239)	(0.182)	(0.221)
Age > 65	−0.678***	−0.525**	−0.643***	−0.511***	−0.584***	−0.648***	−0.619***	−0.742***	−0.599***	−0.682***	−0.563***	−0.573***
	(0.235)	(0.211)	(0.214)	(0.198)	(0.204)	(0.191)	(0.197)	(0.181)	(0.182)	(0.163)	(0.165)	(0.152)
Abitur	0.912***	−0.308	0.861***	−0.336*	0.826***	−0.419**	0.859***	−0.470***	0.856***	−0.505***	0.834***	−0.345**
	(0.201)	(0.208)	(0.183)	(0.193)	(0.177)	(0.184)	(0.171)	(0.175)	(0.162)	(0.164)	(0.144)	(0.148)
Gender	−0.401**	−0.502***	−0.441***	−0.482***	−0.373**	−0.419***	−0.401***	−0.478***	−0.364**	−0.457***	−0.409***	−0.382***
	(0.181)	(0.173)	(0.165)	(0.161)	(0.158)	(0.151)	(0.153)	(0.142)	(0.144)	(0.132)	(0.128)	(0.121)
Constant	−0.794***	−0.146	−0.691***	−0.211	−0.712***	−0.193	−0.750***	−0.113	−0.870***	−0.138	−0.774***	−0.208*
	(0.195)	(0.172)	(0.168)	(0.154)	(0.162)	(0.146)	(0.156)	(0.137)	(0.150)	(0.128)	(0.133)	(0.119)
Akaike Inf. Crit.	1,652.355	1,652.355	1,938.572	1,938.572	2,130.577	2,130.577	2,351.959	2,351.959	2,690.903	2,690.903	3,297.520	3,297.520
Note:												*p<0.1; **p<0.05; ***p<0.01

Table A.18: Regression Tables - Asymmetrical Design: Ability to Cope

	Dependent variable:					
	Ability to Cope					
	19th + 20th	21th + 22th	23th + 24th	25th + 26th	27th + 28th	29th + 30th
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.446***	0.353**	0.154	0.341*	0.048	0.113
	(0.159)	(0.170)	(0.200)	(0.193)	(0.192)	(0.160)
Constant	1.691***	1.691***	1.691***	1.691***	1.691***	1.691***
	(0.123)	(0.125)	(0.131)	(0.121)	(0.127)	(0.125)
Observations	232	206	165	155	167	242
R^2	0.033	0.021	0.004	0.020	0.0004	0.002
Adjusted R^2	0.029	0.016	−0.003	0.014	−0.006	−0.002
Residual Std. Error	1.188 (df = 230)	1.214 (df = 204)	1.271 (df = 163)	1.172 (df = 153)	1.231 (df = 165)	1.213 (df = 240)
F Statistic	7.891*** (df = 1; 230)	4.322** (df = 1; 204)	0.591 (df = 1; 163)	3.140* (df = 1; 153)	0.063 (df = 1; 165)	0.495 (df = 1; 240)
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A.19: Regression Tables - Asymmetrical Design: Asylum Attitudes

	Dependent variable:					
	Asylum Attitudes					
	19th + 20th	21th + 22th	23th + 24th	25th + 26th	27th + 28th	29th + 30th
	(1)	(2)	(3)	(4)	(5)	(6)
EU-Turkey statement	0.425***	0.199	0.129	0.252	−0.264	−0.098
	(0.149)	(0.159)	(0.184)	(0.187)	(0.189)	(0.152)
Constant	2.182***	2.182***	2.182***	2.182***	2.182***	2.182***
	(0.114)	(0.116)	(0.120)	(0.115)	(0.123)	(0.117)
Observations	239	212	173	159	172	242
R^2	0.033	0.007	0.003	0.011	0.011	0.002
Adjusted R^2	0.029	0.003	−0.003	0.005	0.005	−0.002
Residual Std. Error	1.133 (df = 237)	1.153 (df = 210)	1.196 (df = 171)	1.144 (df = 157)	1.228 (df = 170)	1.161 (df = 240)
F Statistic	8.175*** (df = 1; 237)	1.566 (df = 1; 210)	0.493 (df = 1; 171)	1.806 (df = 1; 157)	1.943 (df = 1; 170)	0.416 (df = 1; 240)
Note:						*p<0.1; **p<0.05; ***p<0.01

Table A.20: Regression Tables - Asymmetrical Design: Policy Preference

	Dependent variable:											
	Refugee 19 th + 20 th	Security 19 th + 20 th	Refugee 21 th + 22 th	Security 21 th + 22 th	Refugee 23 th + 24 th	Security 23 th + 24 th	Refugee 25 th + 26 th	Security 25 th + 26 th	Refugee 27 th + 28 th	Security 27 th + 28 th	Refugee 29 th + 30 th	Security 29 th + 30 th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
EU-Turkey statement	−0.113	−1.109***	0.221	−0.684**	−0.022	−0.015	0.021	−0.586	−0.084	0.146	−0.194	−0.066
	(0.352)	(0.338)	(0.359)	(0.339)	(0.434)	(0.358)	(0.427)	(0.396)	(0.432)	(0.343)	(0.368)	(0.298)
Constant	−0.847***	−0.241	−0.847***	−0.241	−0.847***	−0.241	−0.847***	−0.241	−0.847***	−0.241	−0.847***	−0.241
	(0.282)	(0.233)	(0.282)	(0.233)	(0.282)	(0.233)	(0.282)	(0.233)	(0.282)	(0.233)	(0.282)	(0.233)
Akaike Inf. Crit.	450.431	450.431	431.069	431.069	343.987	343.987	324.003	324.003	359.018	359.018	492.129	492.129

Note: *p<0.1; **p<0.05; ***p<0.01

A.2 R Code

Data Management

```
# load required packages
library(haven)
library(tidyverse)
library(naniar)

#set work directory to file location
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))
data_raw <- read_dta("data/ZA6754_v2-0-0_stata14.dta")

#data management
data <- data_raw %>%
  replace_with_na(
    replace = list(
      AA22039 = c(-99, -98, -97, -91, -90, -80),
      educ_school_15 = c(-99, -98, -97, -91, -90, -80),
      educ_job_15 = c(-99, -98, -97, -91, -90, -80),
      gender_15 = c(-99, -98, -97, -91, -90, -80),
      ZJ22011 = c(-99, -98, -97, -91, -90, -80),
      ZJ22013 = c(-99, -98, -97, -91, -90, -80),
      AA22039 = c(-99, -98, -97, -91, -90, -80),
      year_of_birth_cat = c(-99, -98, -97, -91, -90, -80),
      occupation_15 = c(-99, -98, -97, -91, -90, -80),
      AI22001 = c(-99, -98, -97, -91, -90, -80)) ) %>%
  transmute(
    date = dDatum,
    state = state,
    vote = recode_factor(as.numeric(AA22039), `1` = "Will not Vote",
                                    `3` = "CDU", `4` = "SPD",
```

```
                                `7` = "Die Linke", `6` = "Green",
                                `5` = "FDP", `11` = "AfD"),
  vote = relevel(vote, ref = "Will not Vote"),
  abitur = ifelse(educ_school_15 == 6,1,0),
  uni = if_else(educ_job_15 == 10, 1,0),
  female = if_else(gender_15 == 2,1,0),
  handle =  if_else(is.na(ZJ22011), NaN, if_else(ZJ22011 %in% c(1,2) ,1,0)) ,
  asylum =  if_else(is.na(ZJ22010), NaN, if_else(ZJ22011 %in% c(1,2) ,1,0)) ,
  handle_c = recode(as.numeric(ZJ22011),
                    `1` = 2, `2` = 1,
                    `3` = 0, `4` = -1,
                    `5` = -2, .default = NaN),
handle_c = factor(handle_c, ordered = TRUE,
                    levels = -2:2),
          ref_par = recode_factor(as.numeric(ZJ22013),
                    `1` = "Refugee",
                    `2` = "Refugee",
                    `3` = "Refugee",
                    `4` = "Both",
                    `5` = "Security",
                    `6` = "Security"),
          ref_par = relevel(ref_par, ref = "Both"),
          ref = if_else(ZJ22013 %in% 1:3,1,0),
          eql = if_else(ZJ22013 == 4,1,0),
          sec = if_else(ZJ22013 %in% 5:6,1,0),
          old = if_else(year_of_birth_cat < 5 & year_of_birth_cat > 0,1,0 ),
          east = ifelse(state %in% 11:16,1,0),
          elec = ifelse(state %in% c(7,8,15),1,0),
          unemployed = if_else(occupation_15 %in% c(5, 6, 15),1,0),
          day = lubridate::day(date),
          event = if_else(day >= 19,1,0),
```

```
            event = event,
            green = if_else(is.na(AA22039), NaN,if_else(AA22039 == 6,1,0)),
            linke = if_else(is.na(AA22039), NaN,if_else(AA22039 == 7,1,0)),
            fdp = if_else(is.na(AA22039), NaN,if_else(AA22039 == 5,1,0)),
            cdu = if_else(is.na(AA22039), NaN,if_else(AA22039 == 3,1,0)),
            spd = if_else(is.na(AA22039), NaN,if_else(AA22039 == 4,1,0)),
            afd = if_else(is.na(AA22039), NaN,if_else(AA22039 == 11,1,0)),
            e_toll = if_else(is.na(AI22001), NaN,if_else(AI22001 %in% 1:3,1,0)),
            c13 = if_else(day %in% c(13,14),1, 0),
            intr = AI22014
            )
```

Descriptive Statistics

```
library(haven)


library(tidyverse)
library(cowplot)
library(ggpubr)
library(readxl)
library(gridExtra)
library(compareGroups)
theme_set(theme_cowplot())
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))


# Descriptive Table
tab_list <- list()
demog <- c("East", "University Degree",
           "Unemployed", "65+",
           "Abitur", "Female")
p_tab <- data.frame(matrix(NA, nrow = 6, ncol = 0))
```

```
n_tab <- data.frame(matrix(NA, nrow = 3, ncol = 0))


for (a in 1:12){
  b <- data %>% filter(between(day, 19-a,19+a))
  n_tab <-  as.data.frame(cbind(n_tab, c(table(b$event), nrow(b))))
  b <- compareGroups(event ~ east + uni +
                       unemployed + old + abitur +
                       female, data = b)
  b <- createTable(b)
  p_tab <-  as.data.frame(cbind(p_tab, b$descr[,3]))
}
rownames(n_tab) <- c("Treatment", "Control", "All")
rownames(p_tab) <- demog
colnames(p_tab) <- 1:12
colnames(n_tab) <- 1:12
tab <- as.data.frame(t(rbind(p_tab, n_tab)))
xtable(tab)




#function to extract legend
g_legend <- function(a.gplot){
  tmp <- ggplot_gtable(ggplot_build(a.gplot))
  leg <- which(sapply(tmp$grobs, function(x) x$name) == "guide-box")
  legend <- tmp$grobs[[leg]]
  return(legend)
}


# Refugee statistics
## first time asylum applicants
r.data <- read_sav("data/migr_asyappctzm.sav") %>%
```

```
  filter(GEO == "DE", CITIZEN == "EXT_EU28") %>%
  transmute(date = as.Date(paste0(TIME, "15"), format = "%YM%m%d"),
            appl = ifelse(ASYL_APP == "NASY_APP",
                          "First Application","Application"),
            value = as.numeric(as.character(value)))

mymonths <- c( "Dec","Jan","Feb","Mar",
               "Apr","May","Jun",
               "Jul", "Aug", "Sep",
               "Oct", "Nov", "Dec", "Jan")

p.asyl <- ggplot(r.data, aes(x=date, y=value, fill=appl)) +
  geom_bar(position="dodge", stat="identity") +
  scale_x_date(date_breaks = "month", date_labels = mymonths ) +
  theme(axis.text.x = element_text(angle = 90, hjust = 1)) +
  geom_vline(xintercept = as.numeric(as.Date("2016-03-18"))) +
  scale_fill_grey(start = 0, end = .9, name = "") +
  ylab("Number of Asylum Applications") + xlab("2016") +
  geom_text(x = as.Date("2016-03-18"), y= 31000,
            label = " EU Summit \n Balkan Route Closure", hjust = "left")

asyl_legend <- cowplot::get_legend(p.asyl)
as_ggplot(asyl_legend)
p.asyl + theme(legend.position = "none")

## Google Trend data
e_names <- c("EU Summit / Balkan Route Closure",
             "Cologne New Years Riots",
             "Clau??nitz Attacks",
             "Bautzen Attacks")
e_dates <- as.Date(c("2016-03-18", "2016-01-01", "2016-02-18", "2016-02-21"))
```

```
events <- data.frame(Event = e_names , dates = e_dates, ymin = 0, ymax = 100)


### 01 -06
g.plot1 <- read_excel("data/google_t_2016_01-06.xls") %>%
  transmute(`Other Events` = `k??ln silvester fl??chtlinge: (Germany)`+
  `clau??nitz fl??chtlinge: (Germany)`+
  `bautzen fl??chtlinge: (Germany)`,
  `EU Summit / Balkan Route Closure` = `eu gipfel fl??chtlinge: (Germany)` +
              `balkanroute: (Germany)`,
            date = as.Date(Day)) %>%
  gather(key = "Saliency", value = "value",-date) %>%
  ggplot(aes(x=date, y=value, color= Saliency)) +
  geom_line(size = 1.5) +
  scale_x_date(date_breaks = "week") +
  theme(axis.text.x = element_text(angle = 90, hjust = 1)) +
  geom_vline(data = events,
             mapping = aes(xintercept = dates, linetype = Event),
             show_guide = TRUE) +
  scale_fill_discrete(name = "Saliency") +
  ylab("Total interest over time") +
  xlab("") +
  scale_colour_grey()

#seperate legend from plot
g_legend <- cowplot::get_legend(g.plot1)
as_ggplot(g_legend)
g.plot1 + theme(legend.position = "none")



#dependent variables Full
#dependent variables descriptive
```

```
df.descrip <- data %>%
  select(handle,asylum, ref, eql, sec, day) %>%
  transmute(`Able to Cope` = handle,
            `Asylum Policy Support` = asylum,
            `Refugee` = ref,
            `Equally` = eql, `Security` = sec, day = day) %>%
  gather("var", "value", -day) %>%
  mutate(cat =
          ifelse(var == "Asylum Policy Support",
                 "Asylum Policy Support",
          ifelse(var %in% c("Refugee", "Equally", "Security"),
                  "Policy Preference",
          ifelse(var == "Able to Cope", "Ability to Cope", "Error")))) %>%
  na.omit(.) %>%
  group_by(cat,var, day) %>%
  summarise(prop = sum(value=="1")/n())

plotdata <- split(df.descrip, f= df.descrip$cat)



p1 <- ggplot(plotdata$`Ability to Cope`, aes(x=day, y=prop, linetype= var))+
  geom_line() +
  geom_vline(xintercept = 19) +
  scale_color_grey(start = 0, end = .8, name = "Variable") +
  ylab("") + xlab("") + facet_wrap(~ cat, ncol = 1) +
  theme(axis.title.x=element_blank(), axis.text.x=element_blank(),
        legend.title = element_blank()) + facet_wrap(~ cat)



p2 <- p1 %+% plotdata$`Asylum Policy Support` + ylab("Proportion (%)")
p3 <- p1 %+% plotdata$`Policy Preference`   + ylab("") +
```

```
  theme(axis.text.x = element_text(hjust = 1),
        axis.title.x = element_text()) +
  xlab("March 2016")



#seperate legend from plot
l1 <- as_ggplot(cowplot::get_legend(p1))
l2 <- as_ggplot(cowplot::get_legend(p2))
l3 <- as_ggplot(cowplot::get_legend(p3))

p1 <- p1 + theme(legend.position = "none")
p2 <- p2 + theme(legend.position = "none")
p3 <- p3 + theme(legend.position = "none")



descrip <- plot_grid(p1,l1,p2,l2,p3,l3, rel_widths = c(4,1), ncol = 2)
ggsave(descrip, filename = "dep_var.pdf", width = 10, height = 14)
```

Analysis

```
############################
##Zelig - Simulations######
############################
library(Zelig)
library(cowplot)
library(tidyverse)
library(ZeligChoice)
set.seed(24021995) # no seed hacking
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))
theme_set(theme_cowplot())
cutoff <- 19
```

```
#handle
handle <- data.frame(event0=numeric(), event1 = numeric(),
                        fd = numeric(), bw = numeric(),
                        lower = numeric(), upper = numeric())


for (a in 1:12){
  m <- data %>% filter(between(day, 19-a,19+a)) %>%
    zelig(handle ~ event + east +
            uni +  old + abitur +
            female,  data = ., model = "logit")
  s0 <- setx(m, event = 0)
  s1 <- setx(m, event = 1)
  sim <- Zelig::sim(m, x = s0, x1 = s1)
  ev_event0 <- sim$get_qi(qi='ev', xvalue = 'x')
  ev_event1 <- sim$get_qi(qi='ev', xvalue = 'x1')
  x <- as.data.frame(cbind(ev_event0, ev_event1, (ev_event1-ev_event0)))
  colnames(x) <- c("event0", "event1", "fd")
  y <-  as.data.frame(apply(x, 2, quantile, probs = c(0.025,0.975))) %>%
    dplyr::select(fd) %>% t(.) %>% as.data.frame(.) %>%
    transmute(lower = `2.5%`,
              upper = `97.5%`,
              fd = mean(x$fd),
              bw = a)

  handle <- as.data.frame(rbind(handle, y))
}
handle$level <- ""
handle$var <- "Ability to Cope"
```

```
#asylum
asylum <- data.frame(event0=numeric(), event1 = numeric(),
                     fd = numeric(), bw = numeric(),
                     lower = numeric(), upper = numeric())

for (a in 1:12){
  m <- data %>% filter(between(day, 19-a,19+a)) %>%
    zelig(asylum ~ event + east + uni +
            old + abitur + female,
          data = ., model = "logit")
  s0 <- setx(m, event = 0)
  s1 <- setx(m, event = 1)
  sim <- Zelig::sim(m, x = s0, x1 = s1)
  ev_event0 <- sim$get_qi(qi='ev', xvalue = 'x')
  ev_event1 <- sim$get_qi(qi='ev', xvalue = 'x1')
  x <- as.data.frame(cbind(ev_event0, ev_event1, (ev_event1-ev_event0)))
  colnames(x) <- c("event0", "event1", "fd")
  y <-  as.data.frame(apply(x, 2, quantile, probs = c(0.025,0.975))) %>%
    dplyr::select(fd) %>% t(.) %>% as.data.frame(.) %>%
    transmute(lower = `2.5%`,
              upper = `97.5%`,
              fd = mean(x$fd),
              bw = a)

  asylum <- as.data.frame(rbind(asylum, y))
}

asylum$level <- ""
asylum$var <- "Asylum Support"
```

```
#Policy
policy <- data.frame(event0=numeric(), event1 = numeric(),
                         fd = numeric(), bw = numeric(),
                         lower = numeric(), upper = numeric(),
                         levels = numeric())



for (a in 1:12){
  m <- data %>% filter(between(day, 19-a,19+a)) %>%
    zelig(ref_par ~ event + east + uni +
            old + abitur + female,
          data = ., model = "mlogit")
  s0 <- setx(m, event = 0)
  s1 <- setx(m, event = 1)
  sim <- Zelig::sim(m, x = s0, x1 = s1)
  ev_event0 <- sim$get_qi(qi='ev', xvalue = 'x')
  ev_event1 <- sim$get_qi(qi='ev', xvalue = 'x1')
  x <- as.data.frame(cbind((ev_event1-ev_event0)))
  y <-  as.data.frame(apply(x, 2, quantile, probs = c(0.025,0.975)))%>%
    t(.) %>% as.data.frame(.) %>%
    transmute(lower = `2.5%`,
              upper = `97.5%`,
              bw = a)
  y$fd <- c(mean(x$`Pr(Y=Both)`),
            mean(x$`Pr(Y=Refugee)`),
            mean(x$`Pr(Y=Security)`))
  y$level <- c("Equally Important", "Refugee", "Security")
  policy <- as.data.frame(rbind(policy, y))
}
policy$var <- "Policy Preference"
```

```
#plotting
plotdata <- rbind(policy, handle, asylum)
ggplot(plotdata, aes(x = bw, y = fd)) +
  geom_hline(yintercept = 0, colour = gray(1/2), lty = 2) +
  geom_point(aes(x = bw,
                 y = fd)) +
  geom_linerange(aes(x = bw,
                     ymin = lower,
                     ymax = upper),
                 lwd = 1) +
  facet_wrap(var + level ~ ., scales = "free_y", ncol = 5)+
  xlab("Bandwidth (Day)") +
  ylab("Difference in Predicted Probability") +
  scale_x_continuous(breaks = 1:12) +
  theme(text = element_text(size = 8))+
  theme(axis.text = element_text(size = 7))

ggsave("results.pdf", width = 10.7, height = 4)



# alternative outcome variable
toll <- data.frame(event0=numeric(), event1 = numeric(),
                     fd = numeric(), bw = numeric(),
                     lower = numeric(), upper = numeric())

for (a in 1:12){
  m <- data %>% filter(between(day, 19-a,19+a)) %>%
    zelig(e_toll ~ event + east +
            uni +  old + abitur +
            female,  data = ., model = "logit")
```

```
  s0 <- setx(m, event = 0)
  s1 <- setx(m, event = 1)
  sim <- Zelig::sim(m, x = s0, x1 = s1)
  ev_event0 <- sim$get_qi(qi='ev', xvalue = 'x')
  ev_event1 <- sim$get_qi(qi='ev', xvalue = 'x1')
  x <- as.data.frame(cbind(ev_event0, ev_event1, (ev_event1-ev_event0)))
  colnames(x) <- c("event0", "event1", "fd")
  y <-  as.data.frame(apply(x, 2, quantile, probs = c(0.025,0.975))) %>%
    dplyr::select(fd) %>% t(.) %>% as.data.frame(.) %>%
    transmute(lower = `2.5%`,
              upper = `97.5%`,
              fd = mean(x$fd),
              bw = a)

  toll <- as.data.frame(rbind(toll, y))
}
toll$level <- "Environmental Toll"
toll$var <- "Attitudes"

#plotting
ggplot(toll, aes(x = bw, y = fd)) +
  geom_hline(yintercept = 0, colour = gray(1/2), lty = 2) +
  geom_point(aes(x = bw,
                 y = fd)) +
  geom_linerange(aes(x = bw,
                     ymin = lower,
                     ymax = upper),
                 lwd = 1) +
  facet_wrap(var + level ~ .,
             scales = "free_y", ncol = 5)+
  xlab("Bandwidth (Day)") +
```

```
  ylab("Difference in Predicted Probability") +
  scale_x_continuous(breaks = 1:12) +
  theme(text = element_text(size = 8))+
  theme(axis.text = element_text(size = 7))

ggsave("toll.pdf", width = 3, height = 3)
```

B Polarization - The Boon and Bane of Democracy

B.1 Regression Tables

Table B.1: Fixed-Effects Model (CSES):
Outcome - Satisfaction with Democracy

Dependent Variable:	Satisfaction with Democracy		
Model:	(1)	(2)	(3)
Variables			
Relative Polarization	-1.127***	-0.3489	-0.0233
	(0.1293)	(0.2332)	(0.2428)
Gender	0.0306	0.0615	0.0712
	(0.2898)	(0.2888)	(0.2877)
Income	1.667***	1.643***	1.640***
	(0.1175)	(0.1185)	(0.1191)
Age	-0.0500	-0.0702	-0.0727
	(0.2134)	(0.2094)	(0.2101)
Education	0.4409***	0.4766***	0.4722***
	(0.1522)	(0.1479)	(0.1521)
Government Efficacy	2.236***	2.194***	2.180***
	(0.1952)	(0.1948)	(0.1954)
(not) Cabinet		1.559***	
		(0.5618)	
Relative Polarization × (not) Cabinet		-1.505***	
		(0.3468)	
(not) Prime Minister			1.665***
			(0.5921)
Relative Polarization × (not) Prime Minister			-1.683***
			(0.3246)
Fixed-effects			
country	Yes	Yes	Yes
year	Yes	Yes	Yes
Fit statistics			
Observations	124,756	124,756	124,756
R^2	0.19762	0.20171	0.20194
Within R^2	0.02137	0.02635	0.02663

Clustered (country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.2: Fixed-Effects Model (CSES):
Outcome - Turnout

Dependent Variable:	Turnout		
Model:	(1)	(2)	(3)
Variables			
Relative Polarization	0.0117***	0.0115***	0.0099***
	(0.0021)	(0.0023)	(0.0025)
Gender	0.0033	0.0033	0.0033
	(0.0035)	(0.0035)	(0.0035)
Income	0.0179***	0.0179***	0.0179***
	(0.0021)	(0.0021)	(0.0021)
Age	0.0230***	0.0230***	0.0230***
	(0.0034)	(0.0034)	(0.0034)
Education	0.0146***	0.0147***	0.0147***
	(0.0029)	(0.0029)	(0.0029)
Government Efficacy	0.0318***	0.0318***	0.0317***
	(0.0045)	(0.0045)	(0.0045)
(not) Cabinet		-0.0030	
		(0.0064)	
Relative Polarization × (not) Cabinet		0.0004	
		(0.0015)	
(not) Prime Minister			-0.0162**
			(0.0074)
Relative Polarization × (not) Prime Minister			0.0027
			(0.0024)
Fixed-effects			
Country	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	124,756	124,756	124,756
Squared Correlation	0.09734	0.09735	0.09751
Pseudo R^2	0.16477	0.16479	0.16504
BIC	65,688.2	65,710.4	65,690.6

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

B.2 Figures

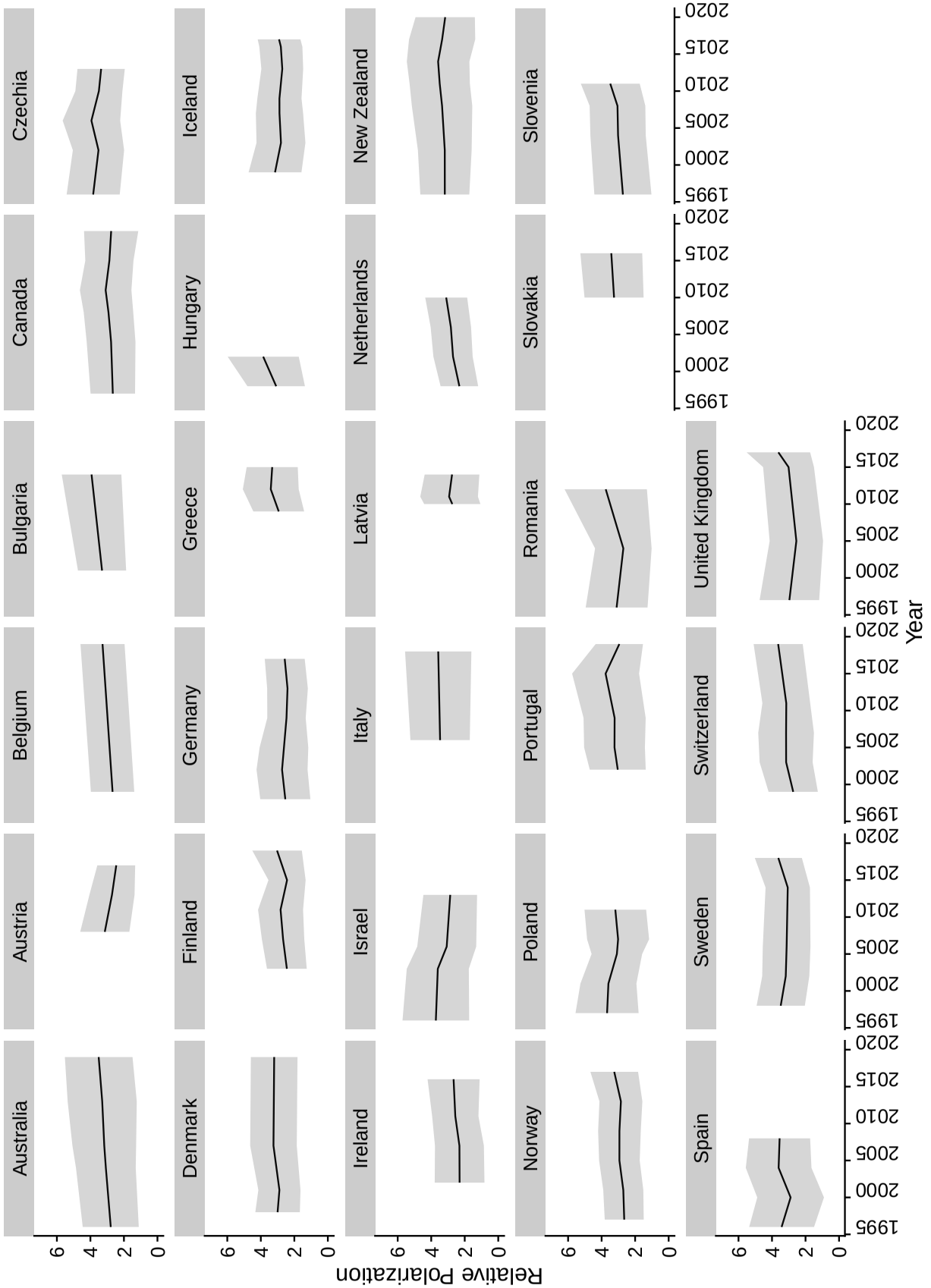

Figure 6: Country-Year variation of relative polarization for all available countries

B.3 R Code

Data Management for CSES Data

```
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))
library(tidyverse)
library(cowplot)
library(countrycode)
library(naniar)
library(stargazer)
library(scales)
load("cses_imd.Rdata")
load("cses5.Rdata")

cabinet <- read.csv("parlgov/view_cabinet.csv") %>%
  transmute(year = lubridate::year(election_date),
            cabinet = cabinet_party,
            country = countrycode(country_name,
                                  origin = "country.name",
                                  destination = "iso2c"),
            party_id = party_id,
            prime_minister = prime_minister) %>%
  filter(year > 1995)



party1 <- cses_imd %>%
  dplyr::select(contains("IMD5101"), IMD1006_UNAlpha2, IMD1008_YEAR) %>%
  rename(country = IMD1006_UNAlpha2, year = IMD1008_YEAR) %>%
  pivot_longer(cols = !c(country, year),
               names_to = "near_party",
               values_to = "party_id") %>%
  mutate(near_party = str_replace(near_party,"IMD5101_", "" ))
```

```
party <- cses5 %>%
  dplyr::select(contains("E5201"), E1006_UNALPHA2, E1008) %>%
  rename(country = E1006_UNALPHA2, year = E1008) %>%
  pivot_longer(cols = !c(country, year),
               names_to = "near_party",
               values_to = "party_id") %>%
  mutate(near_party = str_replace(near_party,"E5201_", "" )) %>%
  bind_rows(party1) %>%
  left_join(cabinet) %>% unique()
rm(party1)
```

```
vdem <- readRDS("vdem/V-Dem-CY-Core-v12.rds") %>%
  transmute(country = countrycode(country_text_id,
                                  origin = "iso3c",
                                  destination = "iso2c"),
            year = year,
            dem = v2x_polyarchy,
            dem_dummy = ifelse(dem <= .3 ,0,1))

vote1 <- cses_imd %>%
  dplyr::select(contains("IMD5001"), ) %>%
  mutate(id = row_number()) %>%
  pivot_longer(cols = !id, names_to = "party", values_to = "vote") %>%
  mutate(vote = ifelse(vote > 100, 0, vote/100),
         party = str_replace(party,"IMD5001_", "" ))
```

```
vote <- cses5 %>%
  dplyr::select(contains("E4004")) %>%
  mutate(id = max(vote1$id) + row_number()) %>%
  pivot_longer(cols = !id, names_to = "party", values_to = "vote") %>%
  mutate(vote = ifelse(vote > 100, 0, vote/100),
         party = str_replace(party,"E4004_", "" )) %>%
  bind_rows(vote1)
rm(vote1)



prep1 <- cses_imd %>%
  dplyr::select(contains("IMD3007"),IMD3013_1,  IMD1010_2, IMD3010,
                IMD3006, IMD1006_UNAlpha2, IMD1008_YEAR, IMD2001_1,
                IMD2002 , IMD2003, IMD2006, IMD1010_1, IMD2014, IMD3001,
                IMD1006_REG, IMD3011, IMD3014, IMD2018, IMD3005_1, IMD3012) %>%
  rename(econ = IMD3013_1, age = IMD2001_1, country = IMD1006_UNAlpha2,
         year = IMD1008_YEAR, satdem = IMD3010, income = IMD2006,
         region = IMD1006_REG, government = IMD3014, close = IMD3005_1,
         lr = IMD3006, weight = IMD1010_1, demw = IMD1010_2,
         female = IMD2002 , education = IMD2003, employ = IMD2014,
         turnout = IMD3001, efficacy = IMD3011,
         industry = IMD2018, vmatter = IMD3012 ) %>%
  mutate(id = row_number()) %>%
  pivot_longer(cols = contains("IMD3007"),
               names_to = "party",
               values_to = "party_lr") %>%
  mutate(party = str_replace(party,"IMD3007_", "" ),
         age = ifelse(age < 0 | age > 200, NA, age),
         interest = NA,
         trust = NA)
```

```
prep <- cses5 %>%
  dplyr::select(contains("E3019"), E3011,  E1010_2, E3020, E3023,
                E1006_UNALPHA2, E1008, E2001_Y, E2002, E2003,
                E2006, E2008,E1012_1, E2010, E3012, E1006_REG,
                E3016_1, E3009, E2022, E3024_1, E3016_2, E3001, E3004_3) %>%
  rename(econ = E3011, country = E1006_UNALPHA2, year = E1008,
         satdem = E3023, social = E2008, income = E2010,
         region = E1006_REG, government = E3009,
         close = E3024_1, trust = E3004_3,
         lr = E3020, weight = E1012_1, demw = E1010_2,
         female = E2002, education = E2003, employ = E2006,
         turnout = E3012, efficacy = E3016_1, urban = E2022,
         vmatter = E3016_2, interest = E3001) %>%
  mutate(id = max(prep1$id) + row_number()) %>%
  pivot_longer(cols = contains("E3019"),
               names_to = "party",
               values_to = "party_lr") %>%
  mutate(
    party = str_replace(party,"E3019_", "" ),
    age = year-E2001_Y,
    age = ifelse(age < 0, NA, age),
         ) %>%
  bind_rows(prep1) %>%
  left_join(vote) %>%
  mutate(
    age = age/10,
    interest = ifelse(interest > 4,NA,-(interest-4)),
    trust = ifelse(trust > 5, NA, -(trust-5)),
```

```
    close = ifelse(close == 1,1,0),
    lr = ifelse(lr > 10, NA, lr),
    party_lr = ifelse(party_lr > 10, NA, party_lr),
    lr.dist = abs(party_lr - lr),
    turnout = ifelse(turnout == 1, 1,0),
    temp_id = paste0(id, party),
    education = ifelse(education > 9, NA, education),
    employ = case_when(employ == 1 ~ "Full Time",
                       employ == 2 ~ "Part Time",
                       employ == 3 ~ "Part Time",
                       employ == 5 ~ "Unemployed",
                       employ %in% c(4, 6:12) ~ "Not in the workforce"),
    female = ifelse(female > 2, NA, female-1),
    income = ifelse(income > 6, NA, income),,
    urban = ifelse(urban > 4, NA, urban),
    satdem = case_when(satdem == 5 ~ 0,
                           satdem == 4 ~ 1,
                           satdem == 6 ~ 2,
                           satdem == 2 ~ 3,
                           satdem == 1 ~ 4),
         government = case_when(government == 5 ~ 0,
                                government == 4 ~ 1,
                                government == 6 ~ 2,
                                government == 2 ~ 3,
                                government == 1 ~ 4))



#prepare data for LR
near_lr <- prep %>%
  group_by(id) %>%
```

```
  mutate(lr.dist.w = lr.dist * (1- vote)) %>%
  arrange(id,  lr.dist) %>%
  summarise(near_party = first(party),
            temp_id = first(temp_id),
            lr.alien = first(lr.dist)) %>%
  mutate(lr.alien = ifelse(lr.alien > 10, NaN, lr.alien))



analysis <- prep %>%
  filter(!(temp_id %in% unique(near_lr$temp_id)))%>%
  left_join(near_lr, by = 'id') %>%
  mutate(lr.con = abs(party_lr-lr),
         lr.indiff = lr.con - lr.alien,
         lr.indiff = ifelse(lr.indiff> 10, NA, lr.indiff)) %>%
  group_by(id) %>%
  summarise(party.n = sum(!is.na(lr.indiff)),
            lr.indiff = weighted.mean(lr.indiff, vote, na.rm = T),
            party_lr = weighted.var(party_lr, vote, na.rm = T),
            efficacy = first(efficacy),
            near_party = first(near_party),
            satdem = first(satdem),
            country = first(country),
            year = first(year),
            trust = first(trust),
            female = first(female),
            income = first(income),
            social = first(social),
            turnout = first(turnout),
            region = first(region),
            education = first(education),
            employ = first(employ),
```

```
            urban = first(urban),
            interest = first(interest),
            vmatter = first(vmatter),
            close = first(close),
            age = first(age),
            econ = first(econ),
            government = first(government),
            lr = first(lr),
            weight = first(weight),
            demw = first(demw)) %>%
  mutate(
    region_c = region,
    region = case_when(region_c == 14 ~ "Eastern Africa",
                            region_c == 15 ~ "Northern Africa",
                            region_c == 18 ~ "Southern Africa",
                            region_c == 5 ~ "South America",
                            region_c == 13 ~ "Central America",
                            region_c == 21 ~ "Northern America",
                            region_c == 30 ~ "Eastern Asia",
                            region_c == 35 ~ "South Eastern Asia",
                            region_c == 143 ~ "Central Asia",
                            region_c == 145 ~ "Western Asia",
                            region_c == 39 ~ "Southern Europe",
                            region_c == 151 ~ "Eastern Europe",
                            region_c == 154 ~ "Northern Europe",
                            region_c == 155 ~ "Western Europe",
                            region_c == 9 ~ "Oceania"),
           region = relevel(as.factor(region), ref = "Western Europe")) %>%
  left_join(vdem) %>%
  filter(party.n > 1 & lr.indiff > 0 & dem > .4) %>%
  left_join(party) %>%
```

```
  mutate(cabinet = ifelse(cabinet == 1, "yes", "no"),
         prime_minister = ifelse(prime_minister == 1, "yes", "no"),
         efficacy = ifelse(efficacy > 5, NA, efficacy),
         econ = ifelse(econ > 5, NA, -(econ-5)))



saveRDS(analysis, file = "analysis.RDS")
```

Analysis for CSES Data

```
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))
library(tidyverse)
library(fixest)
library(cowplot)
library(countrycode)
library(lme4)
library(stargazer)
library(sjPlot)
library(ggeffects)
library(scales)
theme_set(theme_cowplot())

analysis <- readRDS("analysis.rds") %>%
  mutate(cabinet = relevel(as.factor(cabinet), ref = "yes"),
         prime_minister = relevel(as.factor(prime_minister), ref = "yes"),
         satdem = rescale(satdem, from = c(0,4), to = c(0,100)))



# descriptive statistics
descrip <- analysis %>%
  filter(!(region %in% c("Central Asia", "Northern Africa",
```

```
                        "Southern Africa", "Eastern Africa",
                        "Oceania"))) %>%
  mutate(country = countrycode(country,
                               origin = "iso2c",
                               destination = "country.name"),
         period = case_when(year <= 2000 ~ "95-00",
                            year > 2000 & year <= 2005 ~ "01-05",
                            year > 2005 & year <= 2010 ~ "06-10",
                            year > 2010 & year <= 2015 ~ "11-15",
                            year > 2015 & year <= 2020 ~ "16-20"),
         region = factor(region, levels = c("Eastern Europe",
                                            "Northern Europe",
                                            "Southern Europe",
                                            "Western Europe",
                                            "Central America",
                                            "Northern America",
                                            "South America",
                                            "Central Asia",
                                            "Eastern Asia",
                                            "South Eastern Asia",
                                            "Western Asia")),
         period = factor(period, levels = c("95-00", "01-05",
                                            "06-10", "11-15",
                                            "16-20"))) %>%
  ggplot(aes(x=lr.indiff)) + geom_histogram() +
  facet_grid(period~region, scales = "free",
             labeller = label_wrap_gen(width=10)) +
  theme(axis.text.x = element_text(angle = 90)) +
  panel_border() +
  xlab("") + ylab("")
ggsave(descrip, width = 11.69, height = 8.27, filename = "descrip.pdf")
```

```
d1 <- descrip %+% filter(analysis, region == "Western Europe")



#Satisfaction with Democracy
analysis.o <- analysis %>%
  dplyr::select(satdem, turnout, lr.indiff,
                cabinet, prime_minister, female,
                income, age, education, efficacy,
                country, year, weight) %>%
  na.omit()

#get list of countries
paste(sort(countrycode(unique(analysis.o$country),
                       origin = "iso2c",
                       destination = "country.name")),
      collapse = ", ")
paste(sort(unique(analysis.o$year)), collapse = ", ")



#mixed effects
a0 <- lmer(satdem ~ lr.indiff + female +
             income + age +
             education + efficacy +
             (1|country) + (1|year), data = analysis.o)



ac0 <- lmer(satdem ~ lr.indiff*cabinet + female +
              income + age + education + efficacy +
              (1|country) + (1|year), data = analysis.o,
```

```
            weights = analysis.o$weight)

ap0 <- lmer(satdem ~ lr.indiff*prime_minister + female +
              income + age + education + efficacy +
              (1|country) + (1|year), data = analysis.o,
            weights = analysis.o$weight)

#fixed-effects
a1 <- feols(satdem ~ lr.indiff + female + income + age +
              education + efficacy | country + year,
            data = analysis.o, weights = analysis.o$weight)
ac1 <- feols(satdem ~ lr.indiff*cabinet + female +
               income + age + education + efficacy | country + year,
             data = analysis.o, weights = analysis.o$weight)
ap1 <- feols(satdem ~ lr.indiff*prime_minister + female +
               income + age + education + efficacy | country + year,
             data = analysis.o, weights = analysis.o$weight)

dict <- c( satdem = "Satisfaction with Democracy",
           lr.indiff = "Relative Polarization",
           female = "Gender",
           income = "Income",
           age = "Age",
           education = "Education",
           cabinetno = "(not) Cabinet",
           prime_ministerno = "(not) Prime Minister",
           efficacy = "Government Efficacy",
           country = "Country",
           year = "Year")

etable(a1, ac1, ap1, dict = dict, file = "tab1_fe.tex")
```

```
#regression tables
stargazer(a0, ac0, ap0, type = "text")


stargazer(a0,ac0,ap0,
          no.space = T,
          model.names = F,
          table.layout ="-d-t-a-s=n",
          add.lines = list("Random-Effects",
                           c("Countries",
                             length(unique(a0@flist[["country"]])),
                             length(unique(ac0@flist[["country"]])),
                             length(unique(ap0@flist[["country"]]))),
                           c("Years",
                             length(unique(a0@flist[["year"]])),
                             length(unique(ac0@flist[["year"]])),
                             length(unique(ap0@flist[["year"]])))),
          dep.var.labels = c("Satisfaction with Democracy"),
          covariate.labels = c("Relative Polarization",
                               "(not) Cabinet", "(not) Prime Minister",
                               "Gender", "Income", "Age", "Education",
                               "Government Efficacy",
                               "Relative Polarization * (not) Cabinet",
                               "Relative Polarization * (not) Prime Minister"))



#turnout
#mixed-effects model
t0 <- lmer(turnout ~ lr.indiff + female + income + age +
             education + efficacy + (1|country) + (1|year),
           data = analysis.o, weights = analysis.o$weight)
```

```
tc0 <- lmer(turnout ~ lr.indiff*cabinet + female + income + age +
              education + efficacy + (1|country) + (1|year),
            data = analysis.o, weights = analysis.o$weight)
tp0 <- lmer(turnout ~ lr.indiff*prime_minister + female + income +
              age + education + efficacy + (1|country) + (1|year),
            data = analysis.o, weights = analysis.o$weight)
stargazer(t0, tc0, tp0, type = "text")

#fixed-effects model
t1 <- feglm(turnout ~ lr.indiff + female + income + age + education +
              efficacy | country + year,   data = analysis.o,
            weights = analysis.o$weight)
tc1 <- feglm(turnout ~ lr.indiff*cabinet + female +
               income + age + education +
               efficacy | country + year,
             data = analysis.o, weights = analysis.o$weight)
tp1 <- feglm(turnout ~ lr.indiff*prime_minister + female + income +
               age + education + efficacy | country + year, data = analysis.o,
             weights = analysis.o$weight)

dict <- c( turnout = "Turnout",
           lr.indiff = "Relative Polarization",
           female = "Gender",
           income = "Income",
           age = "Age",
           education = "Education",
           cabinetno = "(not) Cabinet",
           prime_ministerno = "(not) Prime Minister",
           efficacy = "Government Efficacy",
           country = "Country",
           year = "Year")
```

```
etable(t1, tc1, tp1, dict = dict, file = "tab2_fe.tex")



stargazer(t0,tc0,tp0,
          no.space = T,
          model.names = F,
          table.layout ="-d-t-a-s=n",
          add.lines = list("Random-Effects",
                           c("Countries",
                             length(unique(t0@flist[["country"]])),
                             length(unique(tc0@flist[["country"]])),
                             length(unique(tp0@flist[["country"]]))),
                           c("Years", length(unique(t0@flist[["year"]])),
                             length(unique(tc0@flist[["year"]])),
                             length(unique(tp0@flist[["year"]])))),
          dep.var.labels = c("Turnout"),
          covariate.labels = c("Relative Polarization",
                               "(not) Cabinet",
                               "(not) Prime Minister", "Gender",
                               "Income", "Age", "Education",
                               "Government Efficacy",
                               "Relative Polarization * (not) Cabinet",
                               "Relative Polarization * (not) Prime Minister"))


#turnout
t0 <- plot_model(t0, type = "pred",
                 terms =  c("lr.indiff"), colors = "bw") +
  ylab("Predicted Value") +
  xlab("Relative Polarization") + ggtitle("")
t1 <- plot_model(tc0, type = "pred",
```

```
                terms =  c("lr.indiff", "cabinet"), colors = "bw") +
  ylab("Predicted Value") +
  xlab("Relative Polarization") + labs(linetype = "Cabinet") +
  ggtitle("") + theme(legend.position="top") + ylab("")
t2 <- plot_model(tp0, type = "pred",
                terms =  c("lr.indiff", "prime_minister"), colors = "bw") +
  ylab("Predicted Value") +
  xlab("Relative Polarization") + labs(linetype = "Prime Minister") +
  ggtitle("") + theme(legend.position="top") + ylab("")

ggsave(t0, width = 100, height = 100, filename = "turnout0.pdf", units = "mm")
ggsave(t1, width = 100, height = 100, filename = "turnout1.pdf", units = "mm")
ggsave(t2, width = 100, height = 100, filename = "turnout2.pdf", units = "mm")

#satdem
p0 <- plot_model(a0, type = "pred",
                terms =  c("lr.indiff"), colors = "bw") +
  ylab("Predicted Value") + xlab("Relative Polarization") +
  ggtitle("") + ylim(30,65)
p1 <- plot_model(ac0, type = "pred",
                terms =  c("lr.indiff", "cabinet"), colors = "bw") +
  ylab("Predicted Value") +
  xlab("Relative Polarization") + labs(linetype = "Cabinet") +
  ggtitle("") + theme(legend.position="top") +
  ylab("") + ylim(30,65)
p2 <- plot_model(ap0, type = "pred",
                terms =  c("lr.indiff", "prime_minister"), colors = "bw") +
  ylab("Predicted Value") + xlab("Relative Polarization") +
  labs(linetype = "Prime Minister") +
  ggtitle("") + theme(legend.position="top") + ylab("") + ylim(30,65)
```

```
ggsave(p0, width = 100, height = 100, filename = "satdem0.pdf", units = "mm")
ggsave(p1, width = 100, height = 100, filename = "satdem1.pdf", units = "mm")
ggsave(p2, width = 100, height = 100, filename = "satdem2.pdf", units = "mm")
```

```
ac1 <- lmer(government ~ lr.indiff*cabinet + female +
              income + age + education + efficacy + (1|country) + (1|year),
            data = analysis, weights = analysis$weight)
ac2 <- lmer(interest ~ lr.indiff*cabinet + female +
              income + age + education + efficacy + (1|country) + (1|year),
            data = analysis, weights = analysis$weight)
ac3 <- lmer(trust ~ lr.indiff*cabinet + female + income + age +
              education + efficacy + (1|country) + (1|year),
            data = analysis, weights = analysis$weight)
ac4 <- glmer(turnout ~ lr.indiff*cabinet + female +
               income + age + education + efficacy + (1|country) + (1|year),
             data = analysis, weights = analysis$weight, family = binomial)
ac5 <- glmer(close ~ lr.indiff*cabinet + female +
               income + age + education + efficacy + (1|country) + (1|year),
             data = analysis, weights = analysis$weight, family = binomial)
```

```
#regression tables
# political engagement
stargazer(a2,a5,a4,
          no.space = T,
          model.names = F,
          file = "tab1.tex",
          table.layout ="-d-t-a-s=n",
```

```
          add.lines = list("Random-Effects",
                           c("Countries",
                             length(unique(a2@flist[["country"]])),
                             length(unique(a5@flist[["country"]])),
                             length(unique(a4@flist[["country"]]))),
                           c("Years", length(unique(a2@flist[["year"]])),
                             length(unique(a5@flist[["year"]])),
                             length(unique(a4@flist[["year"]])))),
          dep.var.labels = c("Political Interest", "Feeling Close", "Turnout"),
          covariate.labels = c("Relative Polarization", "Gender",
                               "Income", "Age", "Education",
                               "Government Efficacy"))


# political satisfaction
stargazer(a0,a1,a3,
          no.space = T,
          model.names = F,
          file = "tab1.tex",
          table.layout ="-d-t-a-s=n",
          add.lines = list("Random-Effects",
                           c("Countries",
                             length(unique(a0@flist[["country"]])),
                             length(unique(a1@flist[["country"]])),
                             length(unique(a3@flist[["country"]]))),
                           c("Years", length(unique(a0@flist[["year"]])),
                             length(unique(a1@flist[["year"]])),
                             length(unique(a3@flist[["year"]])))),
          dep.var.labels = c("Satisfaction with Democracy",
                             "Perceived Government Performance",
                             "Elite Trustworthiness"),
```

```
          covariate.labels = c("Relative Polarization", "Gender",
                               "Income", "Age", "Education",
                               "Government Efficacy"))
```

```
sim1 <- ggpredict(a0, "lr.indiff") %>%
  mutate(var = "Satisfaction with Democracy")
sim2 <- ggpredict(a1, "lr.indiff")%>%
  mutate(var = "Government Performance")
sim3 <- ggpredict(a2, "lr.indiff")%>%
  mutate(var = "Political Interest")
sim4 <- ggpredict(a3, "lr.indiff")%>%
  mutate(var = "Elite Trustworthiness")
sim5 <- ggpredict(a4, "lr.indiff")%>%
  mutate(var = "Turnout")
sim6 <- ggpredict(a5, "lr.indiff")%>%
  mutate(var = "Feels Close to a Party")

plotdata <- sim1 %>%
  bind_rows(sim2) %>%
  bind_rows(sim3) %>%
  bind_rows(sim4) %>%
  bind_rows(sim5) %>%
  bind_rows(sim6) %>%
  mutate(var = factor(var, levels = c("Satisfaction with Democracy",
                                      "Government Performance",
                                      "Elite Trustworthiness",
                                      "Political Interest",
                                      "Feels Close to a Party",
                                      "Turnout")))
```

```
p <-ggplot(plotdata, aes(x = x, y = predicted)) +
  geom_line() +
  geom_ribbon(aes(ymin = conf.low, ymax = conf.high), alpha = .1) +
  panel_border() +
  facet_wrap(  ~ var, scales="free") +
  xlab("Relative Polarization") +
  theme(legend.position="top") +
  ylab("Predicted Values")

p1 <- p %+% filter(plotdata, var %in% c("Political Interest",
                                        "Feels Close to a Party",
                                        "Turnout"))
p2 <- p %+% filter(plotdata, var %in% c("Satisfaction with Democracy",
                                        "Government Performance",
                                        "Elite Trustworthiness"))



ggsave(p1, width = 8, height = 3, filename = "pred1.pdf")
ggsave(p2, width = 8, height = 3, filename = "pred2.pdf")
```

Data Management for BES Data

```
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))
library(haven)
library(tidyverse)
library(naniar)
library(parallel)
library(multidplyr)
library(zoo)
library(scales)
```

```
library(purrr)
library(multidplyr)
library(labelled)
library(sjlabelled)
#survey data
bes_raw <- read_dta("BES2019_W21_Panel_v21.0.dta")

#electoral data
elect <- read_dta("BES-2019-General-Election-results-file-v1.1.dta") %>%
  dplyr::select(matches(c("Lab1[0-9]",
                  "UKIP1[0-9]",
                  "SNP1[0-9]",
                  "Green1[0-9]",
                  "LD1[0-9]",
                  "Con1[0-9]")),
         ConstituencyName) %>%
  pivot_longer(cols = !ConstituencyName, names_to =c('party', 'year'),
               values_to = 'vote',
               names_pattern = '(UKIP|Lab|Con|SNP|LD|Green)(.*)') %>%
  filter(as.numeric(year) %in% 10:19) %>%
  replace_na(list(vote = 0)) %>%
  arrange(ConstituencyName, party, year)%>%
  transmute(year_match = as.numeric(paste0(20, year)),
            pcon = ConstituencyName,
            party = as.character(party),
            vote = as.numeric(vote)/100)
```

```
bes <- bes_raw %>%
  dplyr::select(id,
         contains('satdemUK'),
         contains('partyIdW'),
         contains('generalElectionVoteW'),
         contains('turnoutUKGeneralW'),
         contains('trustMPsW'),
         wt_full_W1W2W3W4W5W6W7W8W9
  ) %>%
  pivot_longer(cols = !c(id, wt_full_W1W2W3W4W5W6W7W8W9),
               names_to =c('var', 'wave'), values_to = 'val',
               names_pattern = '(.*)(W.*)') %>%
  pivot_wider(names_from = var, values_from = val) %>%
  replace_with_na(list(satDemUK = 9999,
                       partyId = 9999,
                       generalElectionVote = 9999,
                       turnoutUKGeneral = 9999)) %>%
  rename(weight = wt_full_W1W2W3W4W5W6W7W8W9) %>%
  filter(!is.na(weight)) %>%
  group_by(id, wave) %>%
  mutate(satdem = na.approx(satDemUK, na.rm = F, maxgap = 3),
         wave = as.numeric(gsub("W","",wave)))



cluster <- new_cluster(8)
cluster_library(cluster, "dplyr")
cluster_library(cluster, "zoo")
cluster_library(cluster, "scales")

cluster_copy(cluster, c("zoo"))
```

```
#prepare and interpolate in data
bes.att <- bes_raw %>%
  dplyr::select(id,
         contains("polAttentionW")) %>%
  mutate_all(~ na_if(., 9999)) %>%
  pivot_longer(cols = !c(id), names_to =c('var', 'wave'),
               values_to = 'val', names_pattern = '(polAttention)(W.*)') %>%
  pivot_wider(names_from = var, values_from = val) %>%
  mutate(polAttention = as.numeric(polAttention),
         wave = as.numeric(str_replace(wave, 'W', ''))) %>%
  right_join(expand_grid(id = unique(.$id), wave= 1:21)) %>%
  group_by(id) %>%
  arrange(id, wave) %>%
  partition(cluster) %>%
  mutate(polAttention = na.approx(polAttention, na.rm = F)) %>%
  collect() %>%
  ungroup() %>%
  dplyr::select(id,wave, polAttention) %>%
  na.omit()

#constituency
const <- bes_raw %>%
  dplyr::select(id,
         contains("pconW", ignore.case = F)) %>%
  mutate_all(~ na_if(., 0)) %>%
  pivot_longer(cols = !c(id), names_to =c('var', 'wave'), values_to = 'pcon',
               names_pattern = '(pcon)(W.*)') %>%
  mutate(pcon = to_character(pcon),
         wave = as.numeric(str_replace(wave, 'W', ''))) %>%
```

```
  dplyr::select(id, pcon) %>% na.omit() %>% unique()


#preapre and interpolate LR data
bes.lr <- bes_raw %>%
  dplyr::select(id,
         contains(paste0('lr', 1:5))) %>%
  mutate_all(~ na_if(., 9999)) %>%
  pivot_longer(cols = !c(id), names_to =c('var', 'wave'), values_to = 'val',
               names_pattern = '(lr[1-9])(W.*)') %>%
  pivot_wider(names_from = var, values_from = val) %>%
  mutate(wave = case_when(wave == 'W1_W5' ~ 'W1',
                          wave == 'W10_W12' ~ 'W10',
                          wave == 'W14W15' ~ 'W14',
                          wave == 'W7_W9' ~ 'W7',
                          TRUE ~ wave),
         wave = as.numeric(str_replace(wave, 'W', '')),
         lr1 = as.numeric(lr1),
         lr2 = as.numeric(lr2),
         lr3 = as.numeric(lr3),
         lr4 = as.numeric(lr4),
         lr5 = as.numeric(lr5)) %>%
  right_join(expand_grid(id = unique(.$id), wave= 1:21)) %>%
  group_by(id) %>%
  arrange(id, wave) %>%
  partition(cluster) %>%
  mutate(lr1 = na.approx(lr1, na.rm = F),
         lr2 = na.approx(lr2, na.rm = F),
         lr3 = na.approx(lr3, na.rm = F),
         lr4 = na.approx(lr4, na.rm = F),
         lr5 = na.approx(lr5, na.rm = F),
         lr = -((lr1+lr2+lr3+lr4+lr5)/5)-5) %>%
```

```
  collect() %>%
  ungroup() %>%
  dplyr::select(id,wave, lr) %>%
  mutate(lr = rescale(lr, c(0,10))) %>%
  na.omit()



parties <- data.frame(party = c('UKIP','Lab', 'Con', 'SNP', 'Green', 'LD'),
                      partyfacts_id = c(601, 1516, 1567, 986, 1794, 1388))

# Function and setup to pick latest che date
ch  <- elect %>%
  dplyr::select(year_match) %>%
  mutate(country = 'GBR') %>%
  unique() %>%
  na.omit()

# function to create merge variable - select previous election

#before
cls_before <- function(x, c, d){
  e <- d %>% filter(country == c)
  f <-  as.numeric(unlist(e$year_match))
  maxless <- max(f[f < x])
  # find out which value that is
  e$year_match[which(f == maxless)]
}
cls_before <- Vectorize(cls_before, vectorize.args = c("x", "c"))

cluster <- new_cluster(detectCores())
```

```
cluster_library(cluster, c("dplyr", "lubridate", "naniar"))
cluster_copy(cluster, c("cls_before", "ch"))



bes.party <- bes_raw %>%
  dplyr::select(id,
    contains(paste0('immig', c('EconW','CulturalW','UKIPW',
                               'LabW', 'ConW', 'LDW',
                               'SNPW', '|GreenW')), ignore.case = F),
    contains(paste0('brexit', c('SelfW','UKIPW',
                                'LabW', 'ConW', 'LDW', "GreenW")),
             ignore.case = F),
    contains(paste0('lr', c('UKIPW','LabW',
                            'ConW', 'LDW', 'Green')),
             ignore.case = F),
    contains('starttime'),
    wt_full_W1W2W3W4W5W6W7W8W9
         ) %>%
  mutate(across(contains('starttime'),~ lubridate::year(.))) %>%
  mutate_all(~ na_if(., 9999)) %>%
  pivot_longer(cols = !c(id, wt_full_W1W2W3W4W5W6W7W8W9),
               names_to =c('var', 'wave'), values_to = 'val',
               names_pattern = '(.*)(W.*)') %>%
  pivot_wider(names_from = var, values_from = val) %>%
  rename(weight = wt_full_W1W2W3W4W5W6W7W8W9) %>%
  filter(!is.na(weight)) %>%
  mutate(wave = as.numeric(gsub("W","",wave))) %>%
  pivot_longer(cols = matches('(UKIP|Lab|Con|SNP|LD|Green)',
                              ignore.case = F),
               names_to =c('var', 'party'),
               values_to = 'val',
```

```
              names_pattern = '(.*)(UKIP|Lab|Con|SNP|LD|Green)') %>%
  mutate(var = paste(var, 'party', sep = '_')) %>%
  pivot_wider(names_from = var, values_from = val) %>%
  left_join(bes.lr) %>%
  mutate(immigCultural = as.numeric(immigCultural),
         immigEcon = as.numeric(immigEcon),
         immig_party = as.numeric(immig_party),
         lr_party = as.numeric(lr_party),
         year = as.numeric(starttime)) %>%
  group_by(id,party, wave) %>%
  mutate(diff.immig = abs(((immigCultural+immigEcon)/2) - immig_party),
         diff.immig = ifelse(diff.immig > 10, NA, diff.immig),
         diff.lr = abs(lr_party-lr),
         temp_id = paste0(id,party),
         country = 'GBR') %>%
  ungroup() %>%
  partition(cluster) %>%
  mutate(year_match = cls_before(year, country, ch)) %>%
  collect() %>%
  filter(!is.na(year)) %>%
  mutate(year_match = as.numeric(year_match))


saveRDS(bes.party, file = 'bes-party.rds')

bes.party <- readRDS('bes-party.rds')


# lab = 1556
# con 773
# libdem = 659
```

```
# SNP = 1284
# ukip = 1272
# green = 467


parties_parlgov <- tibble(near_lr = c("UKIP", "Lab", "Con","LD" , "Green"),
                          parlgov_id = c(1272, 1556, 773, 659, 467 ))

cabinet <- read_csv("parlgov/view_cabinet.csv") %>%
  transmute(year_match = lubridate::year(election_date),
            parlgov_id = as.numeric(party_id),
            cabinet = cabinet_party,
            prime_minister = prime_minister) %>%
  left_join(parties_parlgov) %>% na.omit()



#LR poliy
near_lr <- bes.party %>%
  dplyr::select(id, wave, diff.lr, party, temp_id, year_match) %>%
  left_join(const) %>%
  left_join(elect) %>%
    mutate(diff.w = diff.lr * (1 - vote)) %>% # weighted party distance sorting
  group_by(id, wave) %>%
  arrange(id, wave, diff.lr, .by_group = T) %>%
  summarise(lr.alien = first(diff.lr),
            near_lr = first(party),
            temp_id = first(temp_id),
            year_match = first(year_match))
```

```
analysis.lr <- bes.party %>%
  filter(!(temp_id %in% unique(near_lr$temp_id)))%>%
  left_join(parties) %>%
  left_join(const)%>%
  left_join(elect) %>%
  left_join(near_lr, by = c("id", "wave")) %>%
  dplyr::select(!c(temp_id.x, temp_id.y)) %>%
  mutate(lr.relpol = diff.lr - lr.alien)%>%
  group_by(id, wave) %>%
  summarise(lr.relpol = weighted.mean(lr.relpol, vote, na.rm = T),
            near_lr = first(near_lr),
            year_match = year_match.x) %>%
  na.omit() %>%
  left_join(cabinet)




lr <- analysis.lr %>%
  left_join(bes) %>%
  left_join(bes.att) %>%
  mutate(cabinet = as.factor(ifelse(cabinet == 1, "no", "yes")),
         relevel(cabinet, ref = "no"),
         prime_minister = as.factor(ifelse(prime_minister == 1, "no", "yes")),
         prime_minister = relevel(prime_minister, ref = "no")) %>%
  dplyr::select('lr.relpol', 'id', 'wave', 'cabinet',
                'prime_minister', 'satdem',
                'turnoutUKGeneral', 'weight') %>%
  rename(turnout = turnoutUKGeneral ) %>%
  na.omit() %>%
```

```
  unique()



saveRDS(lr, file = "analysis.rds")
```

Analysis for BES Data

```
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))


library(ecm)
library(fixest)
library(pdynmc)
library(clubSandwich)
library(tidyverse)
library(plm)
library(stargazer)
library(sjPlot)
library(ggeffects)
library(fixest)
library(cowplot)
library(scales)
library(sjlabelled)
library(lmtest)
theme_set(theme_cowplot())



lr <- readRDS("analysis.rds") %>%
  mutate(satdem = scales::rescale(as.numeric(satdem),
                                  to = c(0,100), from = c(1,4)),
         turnout = as.numeric(remove_all_labels(turnout)),
         turnout = scales::rescale(turnout, to = c(0,100), from = c(1,5))) %>%
```

```
  mutate(cabinet = relevel(cabinet, ref = "yes"),
         prime_minister = relevel(prime_minister, ref = "yes"),
         relpol_stag =  (lag(lr.relpol, 1L) + lag(lr.relpol, 2L) +
                           lag(lr.relpol, 3L))/3,
         satdem_stag =  (lag(satdem, 1L) + lag(satdem, 2L) +
                           lag(satdem, 3L))/3)

pdata <- pdata.frame(lr, index = c("id","wave"), drop.unused.levels = T)



#serial coorelation test

pbgtest(satdem ~ lr.relpol, data=pdata, model="random")
pbgtest(turnout ~ lr.relpol, data=pdata, model="random")




pdwtest(turnout ~ lr.relpol, data=pdata, model="random")



#satdem
m1 <- plm(satdem ~ lr.relpol, data = pdata, model = "fd")
m2 <- plm(satdem ~ lr.relpol*cabinet , data = pdata, model = "fd")
m3 <- plm(satdem ~ lr.relpol*prime_minister, data = pdata, model = "fd",
          cluster = id)

#endogeneity testing
e0 <- plm(satdem ~ lr.relpol , data = pdata, model = "fd")

e1 <- plm(lr.relpol ~ satdem + lag(lr.relpol), data = pdata, model = "fd")
stargazer(e0, type = "text")
```

```
stargazer(e1, type = "text")



#turnout
t1 <- plm(turnout ~ lr.relpol, data = pdata, model = "fd")
t2 <- plm(turnout ~ lr.relpol*cabinet , data = pdata, model = "fd")
t3 <- plm(turnout ~ lr.relpol*prime_minister, data = pdata, model = "fd")



#tab satdem
stargazer(m1, m2, m3,
          no.space = T,
          covariate.labels = c("Relative Polarization", "(not) Cabinet",
                               "Relative Polarization * (not) Cabinet",
                               "(not) Prime Minister",
                               "Relative Polarization * (not) Prime Minister"),
          dep.var.labels = "Satisfaction with Democracy")



#table turnout
stargazer(t1, t2, t3,
          no.space = T,
          covariate.labels = c("Relative Polarization", "(not) Cabinet",
                               "Relative Polarization * (not) Cabinet",
                               "(not) Prime Minister",
                               "Relative Polarization * (not) Prime Minister"),
          dep.var.labels = "Turnout")



#plots  satisfaction with democracy
```

```
t0 <- plot_model(m1, type = "pred", terms = c("lr.relpol"), colors = "bw") +
  ylab("Predicted First-Differences") + xlab("Relative Polarization") +
  labs(linetype = "Cabinet") +
  ggtitle("") + theme(legend.position="top")
t1 <- plot_model(m2, type = "pred", terms = c("lr.relpol", "cabinet"),
                 colors = "bw") +
  ylab("") + xlab("Relative Polarization") +
  labs(linetype = "Cabinet") +
  ggtitle("") + theme(legend.position="top")
t2 <- plot_model(m3, type = "pred",
                 terms = c("lr.relpol", "prime_minister"),
                 colors = "bw") +
  ylab("") + xlab("Relative Polarization") + labs(linetype = "Cabinet") +
  ggtitle("") + theme(legend.position="top")

ggsave(t0, width = 100, height = 100,
       filename = "satdem0_plm.pdf", units = "mm")
ggsave(t1, width = 100, height = 100,
       filename = "satdem1_plm.pdf", units = "mm")
ggsave(t2, width = 100, height = 100,
       filename = "satdem2_plm.pdf", units = "mm")
```

```
t1 <- plm(satdem ~ lr.relpol*prime_minister ,
          data = pdata, model = "fd", cluster = "id")
t2 <- plm(satdem ~ lr.relpol*prime_minister ,
          data = pdata, model = "within", cluster = "id")
```

```
coeftest(t2, vcovHC(t2, type = 'HC0', cluster = 'group'))


t3 <- feols(satdem ~ lr.relpol*prime_minister| wave+ id ,
            data = lr, cluster = "id")
```

C Democratic Deficits and Support for the EU

C.1 Regression Tables

Dependent Variable:	EU Support					
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Accountability	-0.2312	0.7230	1.920***	-0.1264	0.3936	4.490***
	(0.5040)	(1.243)	(0.6927)	(0.1823)	(0.6410)	(1.120)
Accountability × Membership Length	0.0093					
	(0.0324)					
Accountability × Time		-0.0204				
		(0.0270)				
Accountability × Amsterdam Treaty			-0.0437			0.0417
			(0.3799)			(0.4215)
Accountability × Brussels Treaty			-1.455			-1.196*
			(1.119)			(0.6002)
Accountability × Lisbon Treaty			-2.165***			-1.836***
			(0.6846)			(0.4722)
Accountability × Nice Treaty			-2.156***			-1.698***
			(0.6781)			(0.4359)
Accountability × Single European Act			-0.4095			-0.1362
			(0.6484)			(0.4394)
Euro Implimentation				-72.97*		
				(42.30)		
Accountability × Euro Implimentation				0.8026*		
				(0.4694)		
Schengen Implimentation					60.19	
					(59.80)	
Accountability × Schengen Implimentation					-0.6335	
					(0.6284)	
log(Membership Length)						77.45**
						(36.16)
Accountability × log(Membership Length)						-0.9552**
						(0.3864)
Fixed-effects						
Year	Yes	Yes	Yes	Yes	Yes	Yes
Country	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	847	847	847	847	847	847
R^2	0.67333	0.67690	0.68977	0.68394	0.67809	0.73718
Within R^2	0.03669	0.04722	0.08518	0.06798	0.05072	0.22498
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.3: Robustness Checks: Accountability

Note that the main effect for *Time* and *Membership Length* are omitted from the model as they are cliniar with the time fixed-effects

Dependent Variable:	EU Support				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Civil Society	-0.2239	-0.1473	-0.0176	-0.2198	-0.3200**
	(0.2255)	(0.4320)	(0.2390)	(0.2601)	(0.1458)
Civil Society × Membership Length	-0.0028				
	(0.0103)				
Civil Society × Time		-0.0038			
		(0.0100)			
Civil Society × Amsterdam Treaty			0.1339		
			(0.1909)		
Civil Society × Brussels Treaty			-0.1962		
			(0.3512)		
Civil Society × Lisbon Treaty			-0.2761		
			(0.2680)		
Civil Society × Nice Treaty			-0.2108		
			(0.2716)		
Civil Society × Single European Act			-0.2654		
			(0.2646)		
Schengen Implimentation				8.016	
				(20.89)	
Civil Society × Schengen Implimentation				-0.0988	
				(0.2365)	
Euro Implimentation					-20.09
					(16.56)
Civil Society × Euro Implimentation					0.2709
					(0.2037)
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
Country	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	847	847	847	847	847
R^2	0.68110	0.68177	0.68386	0.68229	0.69603
Within R^2	0.05962	0.06158	0.06774	0.06311	0.10363
Controls	Yes	Yes	Yes	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.4: Robustness Checks: Civil Society Engagement

Note that the main effect for *Time* and *Membership Length* are omitted from the model as they are cliniar with the time fixed-effects

Dependent Variable:	EU Support				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Transparancy	0.0453	0.1785	0.2366	0.0288	-0.1499
	(0.1877)	(0.4276)	(0.2507)	(0.1953)	(0.1058)
Transparancy × Membership Length	-0.0091				
	(0.0086)				
Transparancy × Time		-0.0072			
		(0.0094)			
Transparancy × Amsterdam Treaty			0.0109		
			(0.1072)		
Transparancy × Brussels Treaty			-0.2306		
			(0.2631)		
Transparancy × Lisbon Treaty			-0.3800		
			(0.2409)		
Transparancy × Nice Treaty			-0.3007		
			(0.2252)		
Transparancy × Single European Act			-0.2159		
			(0.1961)		
Schengen Implimentation				17.36	
				(17.71)	
Transparancy × Schengen Implimentation				-0.2033	
				(0.2050)	
Euro Implimentation					-1.119
					(18.36)
Transparancy × Euro Implimentation					0.0506
					(0.2134)
Fixed-effects					
Year	Yes	Yes	Yes	Yes	Yes
Country	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	847	847	847	847	847
R^2	0.68328	0.68012	0.68437	0.68188	0.68308
Within R^2	0.06603	0.05673	0.06924	0.06190	0.06545
Controls	Yes	Yes	Yes	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table B.5: Robustness Checks: Transparency

Note that the main effect for *Time* and *Membership Length* are omitted from the model as they are cliniar with the time fixed-effects

Table B.6: Macro results using Referendum timing

Dependent Variable:	EU Support		
Model:	(1)	(2)	(3)
Variables			
Accountability	6.473		
	(13.033)		
Referendum	-4.425	3.777	0.748
	(8.624)	(6.308)	(9.013)
Accountability × Referendum	3.708		
	(9.260)		
Transparancy		11.848	
		(7.058)	
Transparancy × Referendum		-5.093	
		(6.945)	
Civil Society			21.340*
			(11.645)
Civil Society × Referendum			-1.650
			(9.434)
Fixed-effects			
Year	Yes	Yes	Yes
Country	Yes	Yes	Yes
Fit statistics			
Observations	762	762	762
R^2	0.67719	0.68008	0.68378
Within R^2	0.04236	0.05093	0.06192
Controls	Yes	Yes	Yes

Clustered (Country) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

C.2 R Code

Data Management

```
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))
library(haven)
library(tidyverse)
library(countrycode)
library(fixest)
library(stargazer)
library(scales)
library(BBmisc)
library(readxl)
library(zoo)

#EUSANCT
eusanct <- read_dta("~/data/euscant/EUSANCT_Dataset_Case-level.dta") %>%
  filter(EU == 1) %>%
  transmute(start = startyearEU,
            end = ifelse(is.na(endyearEU), 2023, endyearEU),
            caseid = caseid)

eusanct <- left_join(expand_grid(caseid = unique(eusanct$caseid),
                                 time = 1970:2023), eusanct) %>%
  mutate(sanct = ifelse(time >= start & time <= end,1,0)) %>%
  group_by(time) %>%
  summarise(sact = sum(sanct))



#ERT
ert <- read_csv('ert.csv')
```

```
#rule of law data
wjp <- read_xlsx("FINAL_2022_wjp_rule_of_law_index_HISTORICAL_DATA_FILE.xlsx",
                 sheet = "Historical data") %>%
  transmute(wjp = as.numeric(`WJP Rule of Law Index: Overall Score`),
            country = countrycode(Country,
                                  origin = "country.name",
                                  destination = "iso2c"),
            time = Year) %>%
  separate_rows(3,sep = "-") %>%
  mutate(time = as.numeric(time))

#GDP
gdp <- read_xls('API_NY.GDP.PCAP.CD_DS2_en_excel_v2_4845611.xls',
                sheet = 'Data', skip = 3) %>%
  select(`Country Code`,contains(as.character(1960:2021) )) %>%
  pivot_longer(cols = !`Country Code`,
               names_to = 'year',
               values_to = 'gdp') %>%
  transmute(country = countrycode(`Country Code`,
                                  origin = 'iso3c',
                                  destination = 'iso2c') ,
            time = as.numeric(year),
            gdp =gdp) %>%
  group_by(country) %>%
  arrange(time, .by_group = T) %>%
  na.omit()

gdp <- gdp %>%
  right_join(expand_grid(country = unique(gdp$country), time = 1960:2021)) %>%
  group_by(country) %>%
```

```
  arrange(time, .by_group = T) %>%
  mutate(gdp = na.approx(gdp, na.rm = F)) %>%
  mutate(growth = (gdp - dplyr::lag(gdp))/gdp)


spend <- read_csv('API_NE.CON.GOVT.ZS_DS2_en_csv_v2_5192569.csv', skip = 4) %>%
  select(`Country Code`,contains(as.character(1960:2021) )) %>%
  pivot_longer(cols = !`Country Code`,
               names_to = 'year',
               values_to = 'spend') %>%
  transmute(spend = as.numeric(spend),
            time = as.numeric(year),
            country = countrycode(`Country Code`,
                                  origin = 'iso3c',
                                  destination = 'iso2c')
         )


growth <- read_csv('API_NY.GDP.MKTP.KD.ZG_DS2_en_csv_v2_5225938.csv',
                   skip = 4) %>%
  select(`Country Code`,contains(as.character(1960:2021) )) %>%
  pivot_longer(cols = !`Country Code`,
               names_to = 'year',
               values_to = 'growth') %>%
  transmute(growth = as.numeric(growth),
            time = as.numeric(year),
            country = countrycode(`Country Code`,
                                  origin = 'iso3c',
                                  destination = 'iso2c')
  )
```

```
# government efficeny
gov <- read_xlsx('~/data/worldbank/gov.xlsx', sheet = 'Data') %>%
  select(`Country Code`,contains(as.character(1996:2021) )) %>%
  pivot_longer(cols = !`Country Code`,
               names_to = 'year',
               values_to = 'gov') %>%
  transmute(country = countrycode(`Country Code`,
                                  origin = 'iso3c',
                                  destination = 'iso2c') ,
            time =  as.numeric(substr(year, 1, 4)),
            gov = rescale(as.numeric(gov), from = c(-2.5,2.5), to = c(0,1)))


gov <- gov %>%
  na.omit() %>%
  right_join(expand_grid(country = unique(gov$country), time = 1996:2021)) %>%
  group_by(country) %>%
  arrange(time, .by_group = T) %>%
  mutate(gov = na.approx(gov, na.rm = F))


# Chris democratic Mood data
dem_mood <- read_csv("~/data/dem_mood_v4.csv") %>%
  left_join(read_csv("~/data/satis_est_v2.csv")) %>%
  transmute(country = countrycode(ISO3c,
                                  origin = "iso3c",
                                  destination = "iso2c"),
```

```
            mood =  SupDem,
            satis = Satis,
            time = Year)

euop_base <- read_dta("~/data/Scotto/irt_estimates.dta") %>%
  transmute(time = t,
            country = country_iso2,
            euop = opinion,
            policy = policy,
            lisbon = ifelse(date > as.Date("2009-12-01"), 1,0)) %>%
  group_by(country)

# EU countries in the v-dem sample
vparty_eu <- readRDS("~/data/v-dem/V-Dem-CY-Core-v12.rds") %>%
  select(country_name) %>% unique()

#V-dem data
vdem <- readRDS("~/data/v-dem/V-Dem-CY-Full+Others-v12.rds") %>%
  transmute(country = countrycode(country_text_id,
                                  origin = "iso3c",
                                  destination = "iso2c"),
            time = year,
            egal = v2x_egal,
            exec = v2xlg_legcon,
            regional = v2xel_regelec,
            elect = v2x_elecoff,
            electoral = v2x_polyarchy,
            enga = rescale(v2dlengage_osp, from = c(0,5), to = c(0,1)) ,
            ratio = rescale(v2exl_legitratio_osp, from = c(0,5), to = c(0,1)),
            perf =  rescale(v2exl_legitperf_osp, from = c(0,5), to = c(0,1)),
            account = v2x_accountability_osp*100,
```

```
transp =  rescale(v2cltrnslw_osp, from = c(0,4), to = c(0,100)) ,
eqlacess = v2xeg_eqaccess,
libdem = v2x_libdem,
liberal = v2x_liberal,
legit_p = v2exl_legitperf,
partipdem = v2x_partipdem,
partip_comp = v2x_partip,
cspart = v2x_cspart*100,
cspart_c5 = (cspart - lag(cspart, n = 5)),
dd = v2xdd_dd,
partip = (v2x_cspart+v2xdd_dd)/2,
partip_l1 = lag(v2x_partip, n = 1),
partip_l2 = lag(v2x_partip, n = 2),
partip_stag = (lag(v2x_partip)+
                lag(v2x_partip, n = 2)+
                lag(v2x_partip, n =3)+
                lag(v2x_partip, n=4)+
                lag(v2x_partip, n= 5))/5,
partip_ch_s = ((v2x_partip-lag(v2x_partip))+
                (lag(v2x_partip)-lag(v2x_partip, n = 2))+
                (lag(v2x_partip, n =2)-lag(v2x_partip, n =3))+
                (lag(v2x_partip, n =3)-lag(v2x_partip, n=4))+
                (lag(v2x_partip, n =3)-lag(v2x_partip, n= 5)))/5,
partip_ch5 = v2x_partip - lag(v2x_partip, n= 5),
partip_ch3 = v2x_partip - lag(v2x_partip, n= 3),
partip_ch1 = v2x_partip - lag(v2x_partip, n= 1),
delibdem = v2x_delibdem,
delib = v2xdl_delib,
egaldem = v2x_egaldem,
minority = v2x_liberal,
elected = v2x_elecoff,
```

```
            leg_control = v2xlg_legcon,
            civil = v2x_cspart,
            direct = v2xdd_dd,
            freefair = v2xel_frefair,
            jucon = v2x_jucon,
            suff = v2x_suffr)

#V-Party data
vparty <- readRDS("~/data/v-dem/V-Dem-CPD-Party-V2.rds") %>%
  transmute(vote = v2pavote,
            illib = v2xpa_antiplural,
            country = countrycode(country_text_id,
                                  origin = "iso3c",
                                  destination = "iso2c"),
            time = year) %>%
  na.omit() %>%
  group_by(country, time) %>%
  summarise(illib = weighted.mean(illib, vote))



#later membership countries\
late.members <- c('Cyprus', 'Czech Republic',
                  'Estonia', 'Hungary', 'Latvia',
                  'Lithuania', 'Malta', 'Poland',
                  'Slovakia', 'Slovenia', 'Bulgaria',
                  'Romania', 'Croatia') %>%
  countrycode(origin = 'country.name', destination = 'iso2c')

substrRight <- function(x, n){
  substr(x, nchar(x)-n+1, nchar(x))
}
```

```
members <- read_csv('~/data/members.csv') %>%
  filter(Name != '???') %>%
  transmute(country = countrycode(Name, origin = 'country.name',
                                  destination = 'iso2c'),
            join = ifelse(Accession == "Founder", 1952,
                          as.numeric(substrRight(Accession, 4))),
            today = 2023 - join
            ) %>%
  bind_rows(tibble(country = "GB", join = 1973, today = 2023 - join))

eurozone <- read_csv('~/data/eurozone.csv') %>%
  transmute(country = `ISO code`,
            euro = as.numeric(`adopted on 1 January of`))

schengen <- read_csv('~/data/schengen.csv') %>%
  transmute(schengen = as.numeric(substrRight(implementation, 4)),
            country = str_extract(State,  pattern = '^[^\\s]+'),
            country = ifelse(country == 'Czech', 'Czechia', country),
            country = countrycode(country,
                                  origin = 'country.name',
                                  destination = 'iso2c'))



#chapel hill
link <- readRDS("link.rds")

ches_raw <- read_csv("1999-2019_CHES_dataset_means(v3).csv")
```

```
ches <- ches_raw %>%
  transmute(eu_position = rescale( -(eu_position)-7, to = c(0,1)),
         eu_salience = rescale(eu_salience, to = c(0,1), from = c(0,10)),
         eu_dissent = rescale(eu_dissent, to = c(0,1), from = c(0,10)),
         year = year,
         vote = vote,
         country_id = country,
         party_id = party_id
         ) %>%
  right_join(expand_grid(year = 1999:2019,
                         country_id = unique(ches_raw$country),
                         party_id = unique(ches_raw$party_id))) %>%
  group_by(country_id, party_id) %>%
  arrange(year, .by_group = T) %>%
  mutate(eu_position = zoo::na.approx(eu_position, na.rm = F),
         eu_salience = zoo::na.approx(eu_salience, na.rm = F),
         eu_dissent = zoo::na.approx(eu_dissent, na.rm = F),
         vote = zoo::na.locf(vote, na.rm = F),
         eu_p = (eu_position+(eu_salience+eu_dissent)/2)/2) %>%
  na.omit() %>%
  group_by(country_id, year) %>%
  summarise(eu_p = weighted.mean(eu_p, vote, na.rm = T)) %>%
  na.omit() %>% ungroup %>% left_join(link) %>% select(!country_id) %>%
  mutate(country = countrycode(country,
                               origin = "country.name",
                               destination = "iso2c")) %>%
  rename(time = year)
```

```
euop <-  euop_base %>%
  filter(policy == "jags") %>%
  left_join(vdem)%>%
  left_join(members) %>%
  left_join(dem_mood) %>%
  left_join(growth) %>%
  left_join(spend) %>%
  left_join(eurozone) %>%
  left_join(schengen) %>%
  left_join(eusanct) %>%
  left_join(ches) %>%
  ungroup() %>%
  mutate(throug = (exec+account+transp+cspart),
         year = as.numeric(time),
         time = time - min(time),
        age = year - 1952,
        post_com = ifelse(country %in%
        c('BG', 'HR', 'CZ', 'EE', 'HU',
        'LV', 'LT', 'PL', 'RO', 'SK', 'SI'),1,0),
        age_c = cut(age, breaks = c(min(age),seq(50,60, by = 5), max(age)),
                    labels = c("50>", "50-55", "55-60", "60<") ),
        memb = ifelse(country %in% late.members,1,0),
         treaty = case_when(year < 1986 ~ "Brussels",
                            year >= 1986 & year < 1992 ~ "Single European Act",
                            year >= 1992 & year < 1999 ~ "Maastrichts",
                            year >= 1999 & year < 2001 ~ "Amsterdam",
                            year >= 2001 & year < 2009 ~ "Nice",
                            year >= 2009 ~ "Lisbon"),
        treaty = relevel(as.factor(treaty), ref = "Brussels" ),
         maastricht = ifelse(year >= 1992,1,0),
        amsterdam = ifelse(year >= 1999,1,0),
```

```
       nice = ifelse(year >= 2001,1,0),
        SEA = ifelse(year >= 1986,1,0),
       eu_time = year - join,
       eu_time.l = log(eu_time+10),
       member = ifelse(eu_time < 0, 0,1),
       lisbon = ifelse(year < 2010 | member == 0 ,0,1),
       euro = ifelse(euro > year | is.na(euro), 0,1),
       schengen = ifelse(schengen > year | is.na(schengen), 0,1),
       memb_l = case_when(
         eu_time < 0  ~ 'Not Member',
         eu_time > 0 & eu_time <= 1 ~ 'New Member',
         eu_time > 1 & eu_time <= 10 ~ '2-10',
         eu_time > 10 & eu_time <= 20 ~ '10-20',
         eu_time > 20 & eu_time <= 30 ~ '20-30',
         eu_time > 30  ~ '30+'),
       memb_l = factor(memb_l,
       levels = c('Not Member',
       'New Member', '2-10', '10-20',
       '20-30', '30+' )),
       memb_l = relevel(as.factor(memb_l), ref = "30+"),
       memb_d = ifelse(eu_time < 10,1,0),
       nobel = ifelse(year > 2012,1,0),
       year = as.factor(year),
       eu_p = -(eu_p-1) ) %>% # rescaling
 filter(country != "EU") %>%
 group_by(year) %>%
 mutate(account.r = account - mean(account, na.rm =T),
        account.d = ifelse(account.r >= 0,1,0),
        transp.r = transp - mean(transp, na.rm =T),
        transp.d = ifelse(transp.r >= 0,1,0),
        cspart.r = cspart - mean(cspart, na.rm =T),
```

```
         cspart.d = ifelse(cspart.r >= 0,1,0)
         )



saveRDS(euop, file = "euop.rds")
```

Macro Level Analysis

```
# requires 01
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))
euop <- readRDS('euop.rds')


library(stargazer)
library(miceadds)
library(countrycode)
library(fixest)
library(ggeffects)
library(cowplot)
library(interactions)
library(sjPlot)
library(tidyverse)
library(lubridate)
library(margins)
library(lmtest)
library(gridExtra)
library(estimatr)
theme_set(theme_cowplot())
# descriptive of the key independent variables
descript <- euop %>%
  select(country, join, year, throug, euop) %>%
```

```
  mutate(throug = throug*100,
         join = paste("Joined", join)) %>%
  rename(`EU Support` = euop, `Throughput Legitimacy` = throug) %>%
  pivot_longer(cols = !c(country, year, join),
               names_to = 'vars', values_to = 'val') %>%
  mutate(year =  year,
         country = countrycode(country, origin = 'iso2c',
                               destination = 'country.name')) %>%
  ggplot(aes(x = year, y = val, linetype = vars)) +
  geom_line() +
  geom_text(
    size    = 2.5,
    mapping = aes(x = Inf, y = Inf, label = join),
    hjust   = 1.00,
    vjust   = 10.5,
    check_overlap = TRUE
  ) +
  facet_wrap(. ~ country, ) +
  theme(panel.spacing = unit(2, "lines"), legend.position="top") +
  xlab('') + ylab('') +
  theme(axis.text.x = element_text(angle = 45, vjust = 0.5, hjust=1)) +
    labs(linetype = "")

ggsave(descript, height = 8,
       width = 11, units = 'in',
       filename = 'descript.pdf')



# histogram
hist <-ggplot(euop, aes(x=throug)) +
  geom_histogram(color="black", fill="white") +
```

```
  facet_wrap(. ~ year) +
  theme(panel.spacing = unit(2, "lines")) +
  ylab('Frequency') + xlab("Throughput Legitimacy")
ggsave(hist, height = 8, width = 11,
       units = 'in',
       filename = 'hist.pdf')



#library(Matrix)
#correlation matrix
#through_corr <- euop %>%
#  select(account,eqlacess,gov,transp,cspart) %>%
#  rename(`Equal Access` = eqlacess,
#         `Accountability` = account,
#         `Government Effectiveness`= gov,
#         `Transparancy` = transp,
#         `Civil Society` = cspart) %>%
#  cor(.) %>% round(2)

#through_corr[lower.tri(through_corr, diag=TRUE)]<-""
#stargazer(as.matrix(through_corr), float = F)




#tables
dict <- c(euop = "EU Support",
          satis = 'Satisfaction with Democracy',
          eu_time.l = 'log(Membership Length)',
          growth = "GDPPC Growth",
```

```
          input = "Input Legitimacy",
          throug = "Throughput Legitimacy",
          output = "Output Legitimacy",
          year = "Year",
          country = "Country",
          ratio = "Throughput Emphasis",
          schengen = "Schengen Implimented",
          euro = "Euro Implimented",
          schengen = "Schengen Implimented",
          lisbon = "Lisbon Implimented",
          `memb_lNot Member` = "Not Member",
          account = 'Accountability',
          transp = 'Transparancy',
          cspart = 'Civil Society',
          eu_p = "Party EU Support"
          )

#fixest

#assimilation process
a <- feols(abs(account.r) ~ eu_time | country,
           data = euop, cluster = "country")
t <- feols(abs(transp.r) ~ eu_time | country,
           data = euop, cluster = "country")
c <- feols(abs(cspart.r) ~ eu_time | country,
           data = euop, cluster = "country")

# accountability
a0 <- feols(euop ~ account | year+country,
            data = euop, cluster = "country")
a1 <- feols(euop ~ account  + growth  | year + country,
```

```
            data = euop, cluster = "country")
a2 <- feols(euop ~ account * eu_time.l + growth   | year+country,
            data = euop, cluster = "country")


# Transparancy
t0 <- feols(euop ~ transp | year+country,
            data = euop, cluster = "country")
t1 <- feols(euop ~ transp   + growth | year+country,
            data = euop, cluster = "country")
t2 <- feols(euop ~ transp * eu_time.l  + growth  | year+country,
            data = euop, cluster = "country")


# Civil Society Engament
c0 <- feols(euop ~ cspart |year+country,
            data = euop, cluster = "country")
c1 <- feols(euop ~ cspart +  growth  | year+country,
            data = euop, cluster = "country")
c2 <- feols(euop ~ cspart * eu_time.l  + growth  | year+country,
            data = euop, cluster = "country")


#tables
etable(a1,a2,
       dict = dict, replace = T,
       digits = "r3",
       order = c("Accountability", "Civil Society", "Transparancy"),
       group = list("_Controls" = "GDPPC Growth"),
       file = "macro_tab1.tex"
)
```

```
etable(c1,c2,
       dict = dict, replace = T,
       digits = "r3",
       order = c("Accountability", "Civil Society", "Transparancy"),
       group = list("_Controls" = "GDPPC Growth"),
       file = "macro_tab2.tex"
)
etable(t1,t2,
       dict = dict, replace = T,
       digits = "r3",
       order = c("Accountability", "Civil Society", "Transparancy"),
       group = list("_Controls" = "GDPPC Growth"),
       file = "macro_tab3.tex"
)


###################
### Robustness#####
###################
dict <- c(euop = "EU Support",
          time = "Time",
          satis = 'Satisfaction with Democracy',
          eu_time.l = 'log(Membership Length)',
          eu_time = 'Membership Length',
          growth = "GDPPC Growth",
          input = "Input Legitimacy",
          throug = "Throughput Legitimacy",
          output = "Output Legitimacy",
          year = "Year",
          country = "Country",
          ratio = "Throughput Emphasis",
          schengen = "Schengen Implimented",
```

```
          euro = "Euro Implimented",
          schengen = "Schengen Implimented",
          lisbon = "Lisbon Implimented",
          `memb_lNot Member` = "Not Member",
          account = 'Accountability',
          transp = 'Transparancy',
          cspart = 'Civil Society',
          eu_p = "Party EU Support",
          treatyMaastrichts = "Maastrichts Treaty",
          treatyBrussels = "Brussels Treaty",
          treatyLisbon = "Lisbon Treaty",
          treatyNice = "Nice Treaty",
          treatyAmsterdam = "Amsterdam Treaty",
          `treatySingle European Act` = "Single European Act",
          schengen = "Schengen Implimentation",
          euro = "Euro Implimentation")



#EU sceptic parties
e1 <- feols(eu_p ~ account  + growth   | year+country,
            data = euop, cluster = "country")
e2 <- feols(eu_p ~ transp  + growth  | year+country,
            data = euop, cluster = "country")
e3 <- feols(eu_p ~ cspart  + growth  | year+country,
            data = euop, cluster = "country")

etable(e1,e2,e3,
       dict = dict, replace = T,
       digits = "r3",
       group = list("_Controls" = "GDPPC Growth"))
```

```
#time modeling
# accountability
a1 <- feols(euop ~ account * eu_time + growth   | year+country,
            data = euop, cluster = "country")
a2 <- feols(euop ~ account * time + growth   | year+country,
            data = euop, cluster = "country")
a3 <- feols(euop ~ account * treaty + growth   | year+country,
            data = euop, cluster = "country")
a4 <- feols(euop ~ account * euro + growth   | year+country,
            data = euop, cluster = "country")
a5 <- feols(euop ~ account * schengen + growth   | year+country,
            data = euop, cluster = "country")
a6 <- feols(euop ~ account * eu_time.l + account * treaty  + growth |
              year+country, data = euop, cluster = "country")
etable(a1,a2,a3,a4,a5,a6, dict = dict, file = "macro_rob1.tex", replace = T,
       group = list("_Controls" = "GDPPC Growth"))

# Civil Society Engament
c1 <- feols(euop ~ cspart * eu_time + growth   | year+country,
            data = euop, cluster = "country")
c2 <- feols(euop ~ cspart * time + growth   | year+country,
            data = euop, cluster = "country")
c3 <- feols(euop ~ cspart * treaty + growth   | year+country,
            data = euop, cluster = "country")
c4 <- feols(euop ~ cspart * schengen + growth   | year+country,
            data = euop, cluster = "country")
c5 <- feols(euop ~ cspart * euro + growth   | year+country,
            data = euop, cluster = "country")
```

```
etable(c1,c2,c3,c4,c5, dict = dict, file = "macro_rob2.tex", replace = T,
       group = list("_Controls" = "GDPPC Growth"))



# Transparancy
t1 <- feols(euop ~ transp * eu_time + growth   | year+country,
            data = euop, cluster = "country")
t2 <- feols(euop ~ transp * time + growth   | year+country,
            data = euop, cluster = "country")
t3 <- feols(euop ~ transp * treaty + growth   | year+country,
            data = euop, cluster = "country")
t4 <- feols(euop ~ transp * schengen + growth   | year+country,
            data = euop, cluster = "country")
t5 <- feols(euop ~ transp * euro + growth   | year+country,
            data = euop, cluster = "country")


etable(t1,t2,t3,t4,t5, dict = dict, file = "macro_rob3.tex", replace = T,
       group = list("_Controls" = "GDPPC Growth"))







#################
##for plotting###
#################


ma1 <- lm_robust(euop ~ account * eu_time.l  + growth + year + country,
                 clusters = country, data = euop)
```

```
ma2 <- lm_robust(euop ~ transp * eu_time.l  + growth + year + country,
                 clusters = country,data = euop)
ma3 <- lm_robust(euop ~ cspart * eu_time.l  + growth + year + country,
                 clusters = country, data = euop)


#two way interaction effect
d1 <- ggpredict(ma1,
                terms = c("eu_time.l[2.3:4.2 by = .1]",
                          "account[minmax]")) %>%
  mutate(var = 'Accountability',
         group = case_when(group == 68.7 ~ "Minimum",
                           group == 98 ~ "Maximum"),
         x = exp(x)-10,
         conf.high = ifelse(conf.high > 100, 100, conf.high))


d2 <- ggpredict(ma2,
                terms = c("eu_time.l[2.3:4.2 by = .1]",
                              "transp[minmax]")) %>%
  mutate(var = 'Transparancy',
         group = case_when(group == 32.5 ~ "Minimum",
                           group == 98.65 ~ "Maximum"),
         x = exp(x)-10,
         conf.high = ifelse(conf.high > 100, 100, conf.high))



d3 <- ggpredict(ma3,
                terms = c("eu_time.l[2.3:4.2 by = .1]",
                          "cspart[minmax]")) %>%
  mutate(var = 'Civil Society Engagement',
         group = case_when(group == 60.3 ~ "Minimum",
                           group == 98.7 ~ "Maximum"),
```

```
         x = exp(x)-10,
         conf.high = ifelse(conf.high > 100, 100, conf.high))


p1 <- bind_rows(d1,d2,d3) %>%
  ggplot(aes(x= x, y = predicted, linetype = group)) +
  geom_line() +
  geom_ribbon(aes(ymin = conf.low, ymax = conf.high),alpha = .2) +
  labs(linetype = "Democratic Quality Indicator") + ylab("EU Support") +
  xlab("Membership Length in Years") +
  theme(legend.position="top")   +
  facet_wrap(. ~ var)
ggsave(p1, units = "in", height = 5, width = 8, file = "macro_margin.pdf")
```

Micro Level Analysis

```
setwd(dirname(rstudioapi::getActiveDocumentContext()$path))
library(haven)
library(tidyverse)


library(countrycode)
library(fixest)
library(lme4)
library(stargazer)
library(scales)
library(BBmisc)
library(readxl)
library(sjlabelled)
library(robustbase)
library(ggeffects)
library(cowplot)
library(miceadds)
library(marginaleffects)
```

```
library(estimatr)
library(stringi)
theme_set(theme_cowplot())

substrRight <- function(x, n){
  substr(x, nchar(x)-n+1, nchar(x))
}



#V-dem data
vdem <- readRDS("~/data/v-dem/V-Dem-CY-Core-v12.rds") %>%
  transmute(country = countrycode(country_text_id,
                                  origin = "iso3c",
                                  destination = "iso2c"),
            year = year,
            cspart = v2x_cspart*100,
            account = v2x_accountability_osp*100,
            transp =  rescale(v2cltrnslw_osp, from = c(0,4), to = c(0,100)) ,
            suff = v2x_suffr)

#mebership timing
members <- read_csv('~/data/members.csv') %>%
  filter(Name != '???') %>%
  transmute(country = countrycode(Name,
                                  origin = 'country.name',
                                  destination = 'iso2c'),
            join = ifelse(Accession == "Founder", 1952,
                          as.numeric(substrRight(Accession, 4))),
            today = 2023 - join
  ) %>% bind_rows(tibble(country = "GB", join = 1973, today = 2023 - join))
```

```
eb <- read_dta("harmonised_EB_2004-2021_v3-0-0.dta")

eb_raw <- readRDS("eb.rds")


eb <- eb_raw %>%
  select(wsample, wpol, wnation, age, gender, educ, occup, weuro,
         satislife, satisdms, satisdeu, benefit, year, country,
         studid, trms_ngov, poldisc_nat, poldisc_eu, euspeed, mem, ebid) %>%
  mutate(country = sjlabelled::as_label(country),
         country =  countrycode(country,
                                origin = "country.name",
                                destination = "iso2c"),
         country = as.factor(country),
         ebid = as.factor(ebid),
         benefit = na_if(benefit, -99991),
         benefit = ifelse(benefit == 1,1,0),
         age = ifelse(age < 15, NaN, age),
         birth = year - age,
         female = ifelse(gender == 2,1,0),
         poldisc_eu.f = case_when(poldisc_eu == 1 ~ "Frequently",
                   poldisc_eu == 2 ~ "Occasionally",
                   poldisc_eu == 3 ~ "Never"),
         poldisc_eu.d = ifelse(poldisc_eu == 3,0,1),
         poldisc_nat.d = ifelse(poldisc_nat == 3,0,1),
         poldisc_eu.f = relevel(as.factor(poldisc_eu.f), ref = "Never"),
         poldisc_nat = -(na_if(poldisc_nat, 4)-3)+1,
         poldisc_eu = -(na_if(poldisc_eu, 4)-3)+1,
         satislife = na_if(satislife, -99991),
```

```
        educ = na_if(educ, -99951),
        occup = occup/100,
        mem.d = ifelse(mem == 3,1,0),
        mem.d1 = ifelse(mem == 1,1,0),
        mem.m = case_when(mem == 3 ~ "Bad Thing",
                          mem == 2 ~ "Neither",
                          mem == 1 ~ "Good Thing"),
        relevel(as.factor(mem.m), ref = "Neither" ),
        mem = -(na_if(mem, -99991)-2),
        euspeed = na_if(satislife, -99991),
        euspeed = rescale(as.numeric(euspeed), to = c(0,100)),
        satisdms = na_if(satisdms, -99991),
        satisdeu = -(4-na_if(satisdeu, -99991)))  %>%
  left_join(members) %>%
  mutate(
    today = ifelse(join > year,0, year-join ),
    eu_t = ifelse(age-today <= 0, 1, (abs((age-15)-today)) ),
    eu_r = ifelse(age-today <= 0, 1, 1-(abs(age-today)/age) ),
    eu_d = ifelse(age-today  <= 0, 1,0),
    ww2 = ifelse(birth < 1940,1,0)) %>%
  filter(today != 0)



# attention based
analysis <- eb %>%
  as_tibble() %>%
  select(female, age, satislife, weuro, country, year,
        mem,  ebid, occup, satislife, educ,  poldisc_eu.d,
        poldisc_nat) %>%
  left_join(vdem) %>% filter(weuro > 0) %>%
  na.omit
```

```
test <- eb_raw %>% select(mem, year, country) %>% filter(year == 2012) %>%
  mutate(country = sjlabelled::as_label(country)) %>% na.omit()


# print the included countries
paste(sort((countrycode(unique(analysis$country),
                        origin = "iso2c",
                        destination = "country.name"))), collapse = ", ")


# scaling 1 = membership is a bad thing
ma0 <- feols(mem ~ account + occup + educ + age + female |
               country + ebid, data = analysis, weights = analysis$weuro)
ma1 <- feols(mem ~ account + poldisc_eu.d + poldisc_nat + occup + educ + age +
               female | country + ebid,
             data = analysis,  weights = analysis$weuro)
ma2 <- feols(mem ~ account * poldisc_eu.d  +  occup + educ + age + female +
               satislife | country + ebid,
             data = analysis,  weights = analysis$weuro)

mc0 <- feols(mem ~ cspart + occup + educ + age + female |
               country + ebid, data = analysis, weights = analysis$weuro)
mc1 <- feols(mem ~ cspart + poldisc_eu.d  + poldisc_nat +
               occup + educ + age + female | country + ebid,
             data = analysis,  weights = analysis$weuro)
mc2 <- feols(mem ~ cspart * poldisc_eu.d +  poldisc_nat +
               occup + educ + age + female | country + ebid,
             data = analysis,  weights = analysis$weuro)

mt0 <- feols(mem ~ transp + occup + educ + age + female |
```

```
                country + ebid, data = analysis, weights = analysis$weuro)
mt1 <- feols(mem ~ transp + poldisc_eu.d  + poldisc_nat +
                occup + educ + age + female | country + ebid,
              data = analysis, weights = analysis$weuro)
mt2 <- feols(mem ~ transp * poldisc_eu.d  + poldisc_nat +
                occup + educ + age + female | country + ebid,
              data = analysis, weights = analysis$weuro)



# Graphs and tables
dict <- c(mem = "Membership Attitudes",
          account = "Accountability",
          cspart = "Civil Society",
          transp = "Transparancy",
          occup = "Occupation",
          educ = "Education",
          age = "Age",
          female = "Gender",
          satislife = "Life Satisfaction",
          poldisc_eu.d = "Discussing EU",
          poldisc_nat = "Discussing National",
          ebid = "Wave", country = "Country")


etable(ma1,ma2,
       dict = dict, replace = T,
       digits = "r3",
       order = c("Accountability",
                 "Civil Society",
                 "Transparancy"),
```

```
       group = list("_Controls" = c("Occupation", "Education", "Age",
                                    "Gender", "Life Satisfaction",
                                    "Discussing National")),
       file = "micro_tab1.tex"
       )
etable(mc1,mc2,
       dict = dict, replace = T,
       digits = "r3",
       order = c("Accountability", "Civil Society", "Transparancy"),
       group = list("_Controls" = c("Occupation", "Education",
                                    "Age", "Gender",
                                    "Life Satisfaction",
                                    "Discussing National")),
       file = "micro_tab2.tex"
)
etable(mt1,mt2,
       dict = dict, replace = T,
       digits = "r3",
       order = c("Accountability", "Civil Society", "Transparancy"),
       group = list("_Controls" = c("Occupation", "Education", "Age",
                                    "Gender", "Life Satisfaction",
                                    "Discussing National")),
       file = "micro_tab3.tex"
)



#for plotting
#for scaling 1 = Eu membership a bad thing
r1 <- lm_robust(mem ~ account * poldisc_eu.d + occup + educ + age + female +
                  satislife + ebid + country,
                data = analysis,clusters = analysis$country,
```

```
                se_type = "CR0", weights = analysis$weuro)
r2 <- lm_robust(mem ~ cspart * poldisc_eu.d + poldisc_nat + occup +
                  educ + age + female + satislife + ebid + country,
                data = analysis, clusters = analysis$country,
                se_type = "CR0", weights = analysis$weuro)
r3 <- lm_robust(mem ~ transp * poldisc_eu.d + poldisc_nat + occup + educ +
                  age + female + satislife + ebid + country,
                data = analysis, clusters = analysis$country,
                se_type = "CR0", weights = analysis$weuro)



d1 <- ggpredict(r1, terms = c("account [60:100]", "poldisc_eu.d")) %>%
  mutate(var = 'Accountability',
         group = ifelse(group == 1, "Yes", "No"),
         conf.high = ifelse(conf.high > 1, 1, conf.high)) %>%
  filter(group != "Frequently")
d2 <- ggpredict(r2, terms = c("cspart [60:100]", "poldisc_eu.d")) %>%
  mutate(var = 'Civil Society Engagement',
         group = ifelse(group == 1, "Yes", "No"),
         conf.high = ifelse(conf.high > 1, 1, conf.high)) %>%
  filter(group != "Frequently")
d3 <- ggpredict(r3, terms = c("transp [60:100]", "poldisc_eu.d")) %>%
  mutate(var = 'Transparancy',
         group = ifelse(group == 1, "Yes", "No"),
         conf.high = ifelse(conf.high > 1, 1, conf.high)) %>%
  filter(group != "Frequently")

p1 <- bind_rows(d1,d2,d3) %>%
  ggplot(aes(x= x, y = predicted, linetype = group)) +
  geom_line() +
```

```
  geom_ribbon(aes(ymin = conf.low, ymax = conf.high),alpha = .2) +
  labs(linetype = "") + ylab("") + xlab("") + ylim(0,1) +
  theme(legend.position="top")   +
  facet_wrap(. ~ var) + labs(linetype = "Discuss the EU") +
  ylab("Attitude towards EU Membership")


ggsave(p1, units = "in", height = 5, width = 8, file = "micro_margin.pdf")
```