ESSAYS ON THE POLITICAL ECONOMY OF POPULISM: ORIGIN, CONSEQUENCES, AND POTENTIAL REMEDIES

Felix Hagemeister

Dissertation

September 2020

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Inaugural Dissertation
zur Erlangung des Grades Doctor oeconomiae publicae (Dr. oec. publ.)
an der Ludwig-Maximilians-Universität München

2020

vorgelegt von

Felix Hagemeister

Referent: Prof. Davide Cantoni, PhD
Korreferent: Prof. Fabian Waldinger, PhD
Promotionsabschlussberatung: 3. Februar 2021

For my family

Acknowledgments

Just as it takes a village to raise a child, it takes many supporters to complete a PhD. I am immensely grateful for all the support that I received over the last four years at the Munich Graduate School of Economics (MGSE). Although it is hopeless to try to name everyone, I would like to individually thank certain individuals who have provided me with indefatigable support.

My PhD was made possible by the incredibly generous support of Davide Cantoni. I have the honour to be a coauthor on a joint project and learnt a lot from him - not just economics. Davide provided me with everything I needed to grow as a researcher, be it countless emails on the intricacies of Stata code, comments on draft papers and presentations, travel grants for conferences, or career advice. Davide, thank you for believing in me and your outstanding supervision.

I also had the pleasure of writing parts of my thesis with my coauthors Mark Westcott and Daniela Miehling. I admire Mark for his coding abilities and Daniela for her attention to detail. Thank you for giving me the opportunity to learn from you.

Special thanks go to Ruben Durante for his kind welcome in Barcelona and for accepting me as his student during my research stay at UPF. My single-authored paper benefited a lot from his incredible ingenuity, passion for economics, and skilful advice. Thank you also for agreeing to serve as third examiner at my PhD defence.

I am very grateful for the research community provided by the MGSE. Many fellow PhD Candidates have become dear friends over the past few years. I am particularly grateful for the time I spent together with Michael, Marc, Franziska, Brendan, and Corinna – not only in seminars and conferences, but also at lunch breaks or in the evening. I am also indebted to my colleagues at the chair of economic history. Running with Leo and Mathias through the English Garden proved both to be a refreshing break from coding and sometimes even solved econometric issues. Thank you also, Britta, for your kind help with all administrative issues, and you, Edyta, for your help with geographic mapping.

During my PhD I was affiliated with the GRK programme which enabled me to participate in interesting courses. Serving as a PhD representative, I discovered how much work it is to maintain an intellectually stimulating research environment. Thank you to everyone who helped make the PhD programme in Munich so enriching and enjoyable. I would like to thank Davide Cantoni (again), Derya Uysal, and Fabian Waldinger for giving me the freedom to design and carry out my

own teaching. Thanks also to Fabian for agreeing to be my second examiner.

Finally, I would like to thank my family for always being at my side and for the unconditional love they share with me.

Felix Hagemeister
Munich, August 2020

Contents

Introduction

"Those who tell the stories rule society."

– Plato, *Politeia*

Populism is on the rise and offers a different narrative than liberal democracy: unmitigated majority rule without liberal protection for minorities (Mudde, 2007a). Its rapid rise across most of the developed world in recent years is remarkable. Donald Trump has been elected President of the United States, people in Great Britain have voted to leave the European Union, Victor Orbán has suspended democratic rule in Hungary, and a right-wing nativist party has become the third largest force in the German parliament. Liberal democracy, once seen as set in stone in western societies, is now starting to appear fragile (Mounk, 2018). This baffling political transformation poses several questions at many levels. The objective of the chapters in this thesis is to contribute to a better understanding of populism. In particular, the aim is to address the following three issues. First, what explains the success of populist parties in recent years? Second, what are the consequences of the success of populist parties, in particular regarding far-right extremism? Third, if populism is cause for concern, what are potential remedies?

A rapidly growing literature on populism, on which Guriev and Papaioannou (2020) provide a thorough literature overview, proposes several answers to these questions. On the one hand, secular factors such as shocks from globalisation, automation, and trade as well as austerity measures and economic consequences of the financial crisis (Fetzer, 2019; Dal Bó et al., 2018; Dehdari, 2018), have been found to increase populists' vote shares (Rodrik, 2018; Autor et al., 2016). On the other hand, a second strand of literature emphasizes the role of non-economic factors has played in the rise of populism. As argued by Norris and Inglehart (2019), a cultural backlash to the politics of ethnic, racial, and gender-based non-discrimination might explain why right-wing populists have gained a larger vote share in the recent past. Gennaioli and Tabellini (2019) show that, indeed, salience of identity rather than economic concerns can have wide-ranging impacts. However, since cultural traits typically remain stable over long periods of time (Voigtländer and Voth, 2012; Becker et al., 2016; Guiso, Sapienza, and Zingales, 2016), a valid question is why and how these traits suddenly become activated. The first chapter of this thesis, joint work with Davide Cantoni and Mark

Westcott, investigates how persistent right-wing attitudes in Germany, dormant for many decades, have both been activated and led to the rise of the first successful right-wing populist party in Germany since the end of World War II.

Given the growing presence of populism, important questions revolve around its implications. Experimental research by Bursztyn, Egorov, and Fiorin (2019) was able to document a link between populist success and xenophobic behaviour. The second chapter of this thesis aims to shed light on the mechanism behind this link in a non-experimental context. Adding to studies on the link between populist success and hate crimes (Schilter, 2018; Müller and Schwarz, 2018; 2019; Kuipers, Nellis, and Weaver, 2019), I examine how information shocks from state elections can propagate far-right extremist protest marches in Germany. In particular, I provide new insights on an element of surprise which the propagation effect depends upon.

The third chapter, joint work with Daniela Miehling, has the most indirect, but maybe the most important link to the political economy of populism. The chapter starts from the observation that the rise of populism goes hand in hand with eroding levels of trust in democratic institutions (Dustmann et al., 2017; Algan et al., 2017). We document that – unexpectedly – one major reform to schools in Germany is associated with higher levels of trust in the EU institutions. We argue that the most likely explanation is that high school graduates earn international experience during the "extra year" they gained from the reform. Going abroad after high school graduation might thus be seen as a potential remedy to the phenomenon of populism.

In what follows I provide a brief summary of all three chapters. Each chapter is self-contained and can be read independently. A consolidated bibliography is presented at the end of the thesis.

In chapter 1, joint work with Davide Cantoni and Mark Westcott, we argue that alongside other determinants, the persistence of right-wing ideology can explain the recent rise of populism. Our explanation addresses the puzzle of why populist parties were able to gain substantial support so quickly despite no major shift in underlying attitudes. Our answer is that changes in the supply of political party options can enable populist parties to tap into existing demand for right-wing policies, and thus "activate" dormant cultural traits.

Focusing on Germany, we document how the recently established AfD party turned from a monothematic anti-Euro party into an openly xenophobic right-wing populist party. This turn enabled the party to sidestep social problems of social acceptability which usually deter Germans from voting for the extreme right. We find that municipalities that expressed strong support for the Nazi party (the NSDAP) in 1933 have a stronger vote base for the AfD after the right-wing shift of the AfD. In our baseline specification, a one standard deviation increase in Nazi party support is associated with 0.06 standard deviations more of support for the AfD in the 2017 federal election. This association should not be seen as causal, but remains robust to controlling for factors usually associated with the rise of right-wing populist parties. We interpret this finding as an activation of a deep-seated cultural trait which is distinct from antisemitism. Consistent with theories of vertical transmission of cultural traits, we find that the influx of ethnic German refugees after

WWII breaks the historical persistence and considerably reduces the correlation. We can rule out that our findings are driven by a concurrent shift in attitudes using survey data.

In chapter 2, I study the nexus of populism and right-wing extremism. Previous literature has shown that key to understanding the link between populism and right-wing actions are changes in social acceptability (Bursztyn, Egorov, and Fiorin, 2019). These changes can be induced by information shocks, for example a surprising outcome of an election, and can give rise to an empowerment effect: when social norms surrounding far-right actions change, xenophobes might be more likely to take to the streets. I contribute to the understanding of this empowerment effect in two ways. First, I document that unexpectedly high populist vote shares at state elections in Germany increase the number of far-right protests in more liberal areas. Second, I show how the empowerment effect depends on an element of surprise. When success of the populist party is severely underestimated, a municipality with a populist vote share 10 percentage points below the state average faces a roughly 30 percent increase of the mean likelihood of an additional far-right protest. The effect materializes only after the rightward shift of the AfD and vanishes when polling institutions correctly estimate the populist party's success. Bottom-up far-right protests initiated by non-formal organizers respond more heavily to the information shock, suggesting that the effect is not due to strategic targeting. The results are robust to demographic controls, unemployment or refugee influx, as well as a placebo test.

In chapter 3, which is joint work with Daniela Miehling, we examine an unintended side-effect of a major educational reform in Germany on trust in the EU institutions. The so-called G8 reform reduced the total years of schooling from nine to eight years for academic-track high school students. We find that, on average, individuals who thus had eight years of schooling display higher levels of trust in EU institutions than those who had nine years of schooling. We argue that the increase in trust is connected to treated individuals using their "extra" year after graduation to take a gap-year, for example, to volunteer abroad. Although the aim of the policy was to allow for earlier job market entry, we show that the reform significantly increased the probability of an individual taking time off after graduation and before continuing either with university, an apprenticeship, or employment. We show that the increase in trust in EU institutions is not due to an increase in general trust or specifically trust in politics amongst treated individuals. As low levels of trust seem to engender the rise of populist parties (Algan et al., 2017), international experience might point to a potential counterweight to increasing populist success.

Chapter 1

Persistence and Activation of Right-Wing Political Ideology

1.1 Introduction

Throughout Western democracies, the recent rise of right-wing populism has been swift and remarkable — from Orbán to Salvini, from Le Pen to Wilders, from Trump to Bolsonaro. Social scientists have been grappling with its causes since. Several explanatory factors have been brought forward and tested in different settings: from the rise in unemployment following the great recession, to "import competition" from China and increasing insecurity among manufacturing workers, to immigration and especially the refugee crisis of 2015.[1] And yet, each one of these factors can only account for some of the variation in success of right-wing populists across time, countries, and regions. We propose cultural persistence of right-wing political ideology as a further determinant of electoral outcomes. If such a persistent demand for right-wing ideology is combined with a shift in the supply of political platforms, sharp changes in electoral support may result even in the absence of underlying sharp changes in the demand for right-wing policy.

In this paper, we study the rise of a new right-wing party, the "Alternative for Germany" (*Alternative für Deutschland*, henceforth AfD). Its recent emergence in the German political landscape has offered a new political platform on the far right: conservative, nationalistic, and at times outright xenophobic. We show that municipalities that expressed strong support for the Nazi party (the NSDAP) in 1933 now have a stronger vote base for the AfD. In our baseline specification, a one standard deviation increase in Nazi support is associated with 0.06 standard deviations more support for the AfD in the 2017 federal election. This result is not confounded by other factors often associated with the rise of right-wing populist parties, such as unemployment, exposure to

1. Studying the rise of right-wing populism has given rise to a burgeoning literature: e.g., on unemployment, see Dehdari (2019). On the loss of manufacturing jobs, see Anelli, Colantone, and Stanig (2019). On the consequences of trade exposure, see Autor et al. (2016), Dippel, Gold, and Heblich (2016), Malgouyres (2017), Colantone and Stanig (2018). On immigration, see Halla, Wagner, and Zweimüller (2016) Dustmann, Vasiljeva, and Piil Damm (2016).

trade shocks, or the presence of refugees.

Importantly, we do not suggest that this finding should be seen as a *causal* effect of Nazi voting on contemporary electoral outcomes. Rather, we see Nazi vote share in 1933 as a proxy for a deep-seated, persistent right-wing ideology. In accordance with this interpretation, we show that municipalities that voted for the NSDAP in 1933 consistently express more right-wing attitudes in opinion surveys, from the 1990s until today. Moreover, surveys show that voters in these municipalities do not turn more right-wing before the 2017 election, ruling out a demand shift as a potential explanation.

Instead, these attitudes translate into electoral results only once the AfD is available on the ballot: for the first time in postwar German history, as a party that identifies as firmly right-wing, but is more socially acceptable than other, more extreme parties existing before. These other parties — strongly right-wing or even neo-Nazi — exhibit only a much lower correlation with Nazi vote share throughout the period considered. Our interpretation also helps understanding the apparent disconnect between the increasing electoral success of newly founded, populist right-wing movements across Europe and the world, while overall attitudes in the population remain broadly constant. The successful establishment of new parties with low social stigma, which tap on an existing demand for right-wing policies, can explain these sudden shifts in electoral outcomes.

We interpret our findings in the context of the literature on cultural persistence, which has shown how norms and values often have roots in the distant past and are transmitted across generations. Such norms and values — e.g., trust toward strangers, gender roles, or antisemitism — can have a first-order impact on a wide set of social and economic outcomes.[2] However, there is also a growing understanding that not all historical shocks that shape culture and values manifest themselves up to the present: cultural persistence may be mediated or dampened by intervening factors.[3]

Our research proposes an alternative interpretation to the presence, or lack, of cultural persistence: we distinguish between the persistence of cultural traits, such as xenophobia or antisemitism, and their activation, as they are turned into manifest actions. Cultural traits may be present but dormant, because they do not result in actions: antisemitism may be persistent but not result in pogroms; xenophobia may be persistent but not result in votes for extreme right-wing parties. These traits would only be visible to the researcher, if at all, through opinion surveys, although the power of such instruments may be limited by factors such as stigma and social desirability. The persistent, but latent demand for the expression of cultural attitudes will only result in actions once its manifestation becomes less costly.

Sharp shifts in the party landscape, such as the creation of a new political party or the rise of a

2. The recent literature in economics on deep roots and persistence of cultural values is large; see, e.g., Alesina and Fuchs-Schündeln (2007), Nunn and Wantchekon (2011), Jha (2013), Spolaore and Wacziarg (2013), Alesina, Giuliano, and Nunn (2013), Guiso, Sapienza, and Zingales (2016), Becker et al. (2016), Becker and Pascali (2016). On economic and social effects of cultural norms, see Tabellini (2010).

3. Voigtländer and Voth (2012) show that in some German cities, e.g. those with a tradition of commerce, the transmission of antisemitism is lower. Giuliano and Nunn (2017) provide a broader framework to understand cultural transmission.

new, charismatic leader, are examples in which the relative costs of manifesting existing attitudes change. The specific setting of Germany allows us to observe a case in which a change in the supply of political platforms is key in making a long-run persistence of ideological traits reemerge. After the catastrophic experience of Nazism and World War II, the postwar legal setting severely constrained the expression of right-wing ideology and put obstacles to the creation of parties on the extreme right fringe. The AfD bypassed these constraints: it was founded in 2013 as a mono-thematic platform to promote fiscally conservative principles and oppose the Greek bailout. Two years later, in 2015, the initial leadership was ousted by a narrow margin and the party veered strongly to the right, focusing on immigration and nationalism as main themes. As a consequence of this sudden shift, the party could avoid the intense legal scrutiny, and public stigma, that newly founded right-wing parties are usually subject to in Germany.

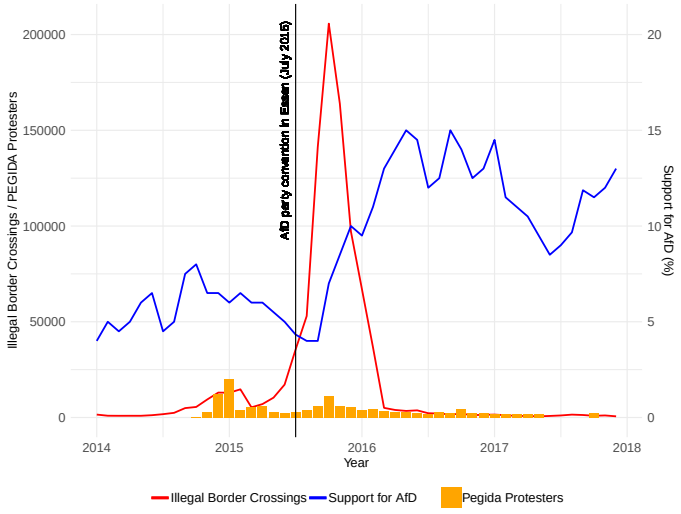
This shift was a fundamental change to the German party landscape. Arguably for the first time in post-war German history, in 2017 electors had the viable option to vote for a party to the right of the Christian Democrats (the mainstream conservative party). The AfD was, compared to previously existing far-right parties, a relatively "cheap" option in terms of social image costs: it carefully eschewed the neo-Nazi associations that characterize other parties on the right fringe, cultivating instead a respectable, bourgeois image. Voting, mobilizing, canvassing for the AfD is much less associated with social stigma than for other far-right parties. Moreover, voting for the AfD in 2017 represented a viable option (not a pure protest vote), since all polls put the party comfortably above the 5% threshold required to obtain seats in parliament.

Consistent with our hypothesis, we find only a small and insignificant correlation between the AfD's electoral fortunes and Nazi support in 2013, when the AfD espoused merely economic conservatism. The correlation is strong and significant in 2017, when the party had veered to the right. We show that, in contrast to the findings by Voigtländer and Voth (2012, 2015), who document persistent patterns of antisemitism across space and time, antisemitism is not the reason for this persistence of far-right voting. Measures of antisemitism in the 1930s and before are not correlated with today's electoral success of the AfD, while proxies for conservative attitudes are. The same holds for the share of Protestants in a community, which is widely recognized as an accurate predictor for conservative attitudes and Nazi vote share in the 1920s and 30s.

We also consider other major determinants of the rise of right-wing populism proposed by the literature. While unemployment levels, changes in unemployment rates, exposure to import competition, and educational attainments are also, to some extent, determinants of the AfD's electoral success, their inclusion in the regressions does not affect the estimated historical persistence of Nazi voting. Moreover, these factors do not interact with historical Nazi voting, suggesting that they do not play an activating role. In line with theories of vertical transmission of values, we find that the influx of ethnic German refugees after WWII — in some communities, these "expellees" represented up to half of the post-war population – breaks the historical persistence and substantially reduces the correlation of voting between the 1930s and today.

Importantly, note that our argument does not rest on an assumption of the exogeneity of the *timing* of the emergence of the AfD. As shown in Figure 1.1, the AfD's rightward turn occurred in the spring of 2015, culminating in the party convention in July, and thus preceded the massive inflow of Syrian refugees that peaked in the following fall. Nevertheless, public sentiment against the perceived threat of immigration from Islamic countries might have been mounting even beforehand, or throughout the time period analyzed.[4] Yet such a general, overall rightward shift in attitudes would be accounted for by the comparison of elections in different years (2013 vs. 2017). For the single municipality, the new availability of the AfD as a "respectable" populist right-wing option in 2017 was exogenous. It represented an expansion of the political supply that affected all regions equally, as the AfD was on the ballot in all states.

Figure 1.1: Timeline of Events



Notes: The graph shows voting intentions for the AfD (source: Infratest dimap), illegal border crossing by refugees along the Western Balkan route (source: European Border and Coast Guard Agency — Frontex), and attendance of "Pegida" demonstrations (source: `https://durchgezaehlt.org`).

Our interpretation of the findings as a supply-side shift meeting an existing, persistent ideological demand would be spurious if, instead, a sharp rightward move in attitudes had occurred over the same time period, *only* in the municipalities that had a history of past Nazi voting. Such a localized shift in attitudes could result in the specific electoral patterns observed: in that case, they would be the result of both a supply and a demand side shift. To exclude this possibility, we study political attitudes through the German General Social Survey (ALLBUS). We find that respondents in municipalities with higher support for the Nazi party in the past expressed more right-wing attitudes along a wide range of questions throughout all waves studied (1996–2016),

4. Figure 1.1 also shows a time series of attendance of "Pegida" (Patriotic Europeans Against the Islamisation of the Occident) demonstrations: these are nationalistic, decidedly islamophobic, anti-immigration protests.

consistent with our view of a persistent right-wing ideology in these areas. Importantly, however, we do not find a rightward shift in these municipalities between 2014 and 2016, suggesting that there was no shift in demand that could explain the geography of electoral support of the AfD.

Our analysis speaks to several research agendas in economics and political science. First, we contribute to the literature cited above on the long-term persistence of cultural traits and attitudes. As in, e.g., the papers by Voigtländer and Voth (2012), Guiso, Sapienza, and Zingales (2016), Becker et al. (2016), we show that cultural traits — in our specific case, political attitudes — have deep origins that are correlated spatially with analogous traits in the past, and are transmitted across generations.

Additionally, the AfD's electoral success shows that the historical persistence of political attitudes is not always visible, and may need to be "activated" by changes in the institutional setting or the political marketplace. This activation of historical memories has also been evidenced by two recent papers. In Fisman, Hamao, and Wang (2014), anti-Japanese hatred is selectively stoked by Chinese leaders for domestic policy purposes, with consequences on stock market prices. Fouka and Voth (2016) show how sales of German cars declined, as the debt crisis of 2010–2015 mounted, in Greek localities that witnessed massacres perpetrated by German forces in WWII.

In these papers, incidental changes in the political background have economic consequences. Another literature has focused on the endogenous choice of politicians to selectively activate feelings in the electorate: Glaeser (2005) and, more recently, Guiso et al. (2018) discuss how the supply of political platforms, generated by politicians, interacts with voters' demand for policies such as hatred or populism. Enke (2019) studies the dynamics of supply and demand for moral values in voting in the context of the recent U.S. elections. Recent work by Ochsner and Roesel (2017) studies a context close to ours — the populist right-wing FPÖ party in Austria — showing that this party is successful in unearthing a resentment against Turkish immigrants that dates back to the Ottoman sieges of Vienna in the 16th and 17th century. While in Ochsner and Roesel (2017) memories of Turkish massacres are not present in the population any more and are strategically inculcated by politicians, in our setting we argue that right-wing leanings are present throughout, and emerge incidentally as a consequence of the change in political landscape.

Second, our work is a contribution to understanding the determinants of (radical) right-wing voting.[5] Economic insecurity, spurred by increasing globalization and the demise of traditional manufacturing, may explain part of this political shift; so do increasing levels of immigrant population in (Western European) countries.[6] Cultural factors are also discussed frequently: Inglehart and Norris (2016) argue that the recent rise of populism can best be understood as a reactionary response to a cultural change that is perceived as too fast and unsettling by some sectors of the

5. For an (admittedly less than comprehensive) literature review, cf. footnote 1. The literature in comparative political science is reviewed by Golder (2016); see also the earlier works by Norris (2005), Mudde (2007b), and Arzheimer (2008).

6. Although importantly for the context of this paper, Germany retains one of the strongest and most competitive industrial economies in Europe, and ha maintained very low levels of unemployment throughout the period considered.

population. To our knowledge, we are among the first to bring two new factors, and the interaction thereof, to the explanation of the electoral successes of right-wing parties. On the one hand, we shed light on the role of long-standing, deeply ingrained political beliefs — this is especially salient in Germany, a country that experienced a most destructive instance of fascism.[7] On the other hand, we emphasize the importance of political structures in facilitating the expression of right-wing ideology.

The paper proceeds as follows. Section 1.2 provides an introduction to the political context in Germany. We describe the entry of the AfD and explain why we interpret it as a consequential supply shock. Section 1.3 describes the data used. Section 1.4 presents the empirical analysis linking historical support for the NSDAP with the AfD's electoral results. In section 1.5, we study potential demand-side shifts through opinion surveys. Section 1.6 provides an interpretation of the combined results and of the underlying mechanisms. Section 1.7 concludes. Supplementary Appendices provide further results.

1.2 Historical Context

1.2.1 The Political Landscape in Germany

After the collapse of the Nazi regime and Germany's defeat in World War II, the reconstruction of the political party system in West Germany (the Federal Republic of Germany, founded in 1949) faced two major challenges. First, rebuilding a system that would supersede the structural weaknesses of the Weimar era and ensuring that the rise of an extremist party would be avoidable. Second, integrating large swaths of the population actively involved in the Nazi dictatorship (8.5 million former card-carrying NSDAP party members) into the new democratic system. These challenges were met both through the creation of new parties, and through special provisions in the post-war constitution.

On the right side of the political spectrum, the main actor was the Christian Democratic Union (CDU). It built on the previous experience of the Catholic "Zentrum" party, but explicitly tried to appeal also to Protestant voters, who before the war largely supported nationalist/conservative parties. The CDU (and its Bavarian sister party, the CSU) succeeded in the endeavor of becoming the main, "big tent" conservative party in Germany, channeling nationalists, economic liberals, and social conservatives into one party strongly supporting democratic values.

Political parties emerging to the right of the CDU in later years were unsuccessful, enjoying

7. Despite the availability of high-quality electoral data from the Weimar era, only few researchers have tried to correlate post-war political outcomes in the Federal Republic of Germany with early Nazi support: Liepelt (1967) showed that in 1966 there was a strong correlation between electoral successes of the NPD (a neo-Nazi party) and the NSDAP in 1932. See also the early contributions by Kaltefleiter (1966), Kühnl, Rilling, and Sager (1969), Sahner (1972), and Winkler (1994). Schwander and Manow (2017) point out how areas of AfD support voted for other, far-right parties in the years before 2017; we also document this correlation, but emphasize how the correlation has increased by an order of magnitude after the appearance of the AfD.

at best very temporary support.[8] The NPD (National Democratic Party) was founded in 1964 and enjoyed some temporary popularity in the late 1960, and then again in the late 2000s in East Germany; the *Republikaner* (Republicans) were notable for their successes in the late 1980s and early 1990s. However, no party ever managed to break through the 5% threshold of votes required to gain representation in the *Bundestag*, the federal parliament.[9]

Another factor constraining the emergence and success of far-right parties was a provision in the Basic Law (the post-war constitution of the Federal Republic of Germany) that enabled the Constitutional Court to disband extremist parties on the left and the right. Article 21.2 of the Basic Law states that *"[p]arties that, by reason of their aims or the behavior of their adherents, seek to undermine or abolish the free democratic basic order or to endanger the existence of the Federal Republic of Germany shall be unconstitutional."* This article was invoked twice with success: in 1952, the Constitutional Court outlawed the SRP (*Sozialistische Reichspartei*, Socialist Reich Party), a party that had an openly neo-Fascist agenda and recruited former Nazi functionaries, and in 1956 the communist party (KPD). This provision in the Basic Law was successful in disciplining the extremeness of right-wing political platform even when it did not result in an explicit party ban — the mere threat of disbandment sufficed.[10]

1.2.2 The "Alternative for Germany" (AfD)

In September 2012 three individuals — Bernd Lucke (an economics professor from Hamburg), a former CDU politician, and a journalist — launched a manifesto to oppose the policies pursued by the German government to fight the Euro crisis. The manifesto called for the foundation of a party, the "Alternative for Germany" (*Alternative für Deutschland*, or AfD), and explicitly ruled out that this party should take a stance on policy concerns other than the Euro crisis and the Greek bailout. Running on this platform, the AfD won 4.7% of the votes in the federal election of September 2013, only narrowly missing the 5% threshold to enter the *Bundestag*.

Following the federal election, the AfD gained further strength, obtaining 7.1% of the votes in the European Parliament election of May 2014. This expansion meant that the party increasingly attracted conservatives of all sorts. The tensions between the initial group of party members — eco-

8. Smaller parties on the right appealing to specific constituencies, such as the BHE (League of Expellees), targeting the expellees losing their ancestral homelands after WWII, and the DP (German Party), appealing especially to war veterans and northern German conservatives, quickly disappeared and were not represented in the federal parliament after 1957.

9. The ability of the CDU/CSU to squeeze out all margins on the right end of the political spectrum, all the while remaining solidly grounded in democratic and liberal principles, is well summarized by the long-time leader of the CSU, Franz Josef Strauss, who quipped in 1986 that there *"shall not be a democratically legitimate party to the right of the CSU."*

10. The NPD was twice brought to the Constitutional Court, once in the early 2000s, when the case was dismissed on formal grounds, and once in 2016-17, when the court ruled that, while the party's ideology is unconstitutional, its support is too small to undermine the democratic order and thus to justify its ban. See Conradt and Langenbacher (2013) and Collings (2015) for an introduction to the German political system, and especially the roles of the 5% threshold and the Constitutional court.

nomics professors and fiscal conservatives — and the newer, national-conservative, anti-immigration members became virulent in the spring of 2015 when two leading party functionaries published the "Erfurt Resolution," calling for a policy of opposition to the "social experiments of the past decades (gender mainstreaming, multiculturalism) [...]" and encouraged the party leadership to embrace the xenophobic, anti-immigrant PEGIDA ("Patriotic Europeans Against the Islamisation of the West") movement. At the following party congress in Essen, in July 2015, Frauke Petry, representing the conservative, anti-immigrant wing was unexpectedly elected party leader with 60% of the vote. The congress in Essen sanctioned the takeover of the party by its right-wing, nationalistic faction; the fiscal conservatives rallying around Bernd Lucke left the party.

The "new" AfD quickly adopted a very different rhetoric, moving away from the fiscally conservative topics centering around the Euro and the Greek bailout, and focusing instead on mainstay themes of the European populist right: immigration, nationalism, and islamophobia.[11] As a consequence, the AfD enjoyed considerable successes in the state elections held in 2016, obtaining over 20% of the votes in some states. The party leadership also moved further to the right. At the federal election of September 2017 the AfD scored 12.6%, thus becoming the third largest force in the German Parliament: the first time that a conservative party to the right of the CDU would gain representation in the *Bundestag*.

1.2.3 The AfD: Offering a Low-Stigma, Right-Wing Political Option

We view the turn of the AfD from a monothematic, anti-Euro and anti-Greek bailout party to a more traditional xenophobic, anti-immigrant right-wing party as a policy experiment in which an existing party changes its placement on the political spectrum, without changing the name, logo, or most of the party structures. For the first time in post-war German history, voters could choose a political party with a staunchly right-wing profile, without the strong social stigma attached to previously available fringe parties.

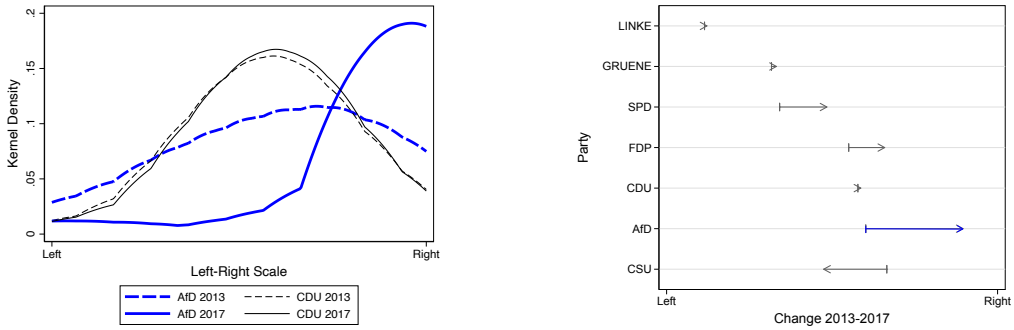
Clearly, this change was also perceived by the voters. In surveys conducted for the German Longitudinal Election Study (GLES), potential voters are asked to place parties on an 11-point left-right scale.[12] As shown in Figure 1.2, left panel, in 2013 voters were not sure where to place the AfD on a left-right spectrum; the modal answer is the score of 6, right in the middle of the spectrum, and the median is 7, just to the right of the center. Over the course of the following years, the public perception of the party shifted radically, and in 2017 most respondents placed the party to the far right (the rightmost answer, 11, is also the modal answer).[13]

11. subsection 1.8.3 documents this shift of the AfD, relative to all other German parties, through a semantic analysis of the language used in party manifestos, political speeches, tweets and Facebook posts. Appendix Figure C1 and Figure C2 show, anecdotally, how this change was reflected in party billboards.

12. We use component 8 of the GLES (Long-term online tracking), studies ZA5720, ZA5726, ZA5728, ZA5732. All studies are available through the GESIS website (`www.gesis.org`).

13. Appendix Figure F1 and Figure F2 provide the full distribution of answers to this survey, for all years and all parties.

Figure 1.2: Perception of Political Parties



Notes: The graphs show where GLES survey respondents placed different political parties on the left-right spectrum. The graph on the left plots the kernel density histogram for the CDU (the mainstream conservative party) and the AfD in both 2013 and 2017 (bandwidth=1.5). The graph on the right plots means and differences in means between 2013 and 2017 for all major German political parties. See Appendix Figure F2 for more detailed results.

A popular political interpretation is that the AfD filled the void left behind on the right side of the spectrum by the CDU, who, under the leadership of Angela Merkel, had adopted a more centrist stance. However, the survey evidence in Figure 1.2 shows that the CDU barely moved in the public's perception: the two kernel densities overlap almost perfectly. The right graph of Figure 1.2 shows, analogously, that the mean perception of the AfD shifted dramatically to the right between 2013 and 2017, while most other mainstream parties either stayed stable or moved only slightly.[14]

Importantly, much more than any other right-wing party to the right of the CDU, the AfD could claim a certain aura of respectability. This was true even after its more moderate, fiscally conservative founders had left in 2015. As described by Arzheimer (2015, p. 540), its *"success was only possible because the party was formed by 'moderates' with very high SES, considerable civic skills, and some political experience."* This distinguishes the AfD from other right-wing parties such as the NPD or the *Republikaner*, who never managed to dismiss their extremist, even neo-Nazi image. Such parties were strongly stigmatized in the political discourse of post-war Germany and thus, lacking endorsement from "respectable" people, had difficulties in mobilizing any existing voter potential (Güllner, 2016). In its first years, the AfD was very careful not to accept members that had previously been active in organizations of the extreme right, and tried to avoid controversial statements on Germany's Nazi past or the Holocaust.[15]

14. The only exception is the CSU, the Bavarian sister party of the CDU. Its leftward move seems an aberration of the 2017 survey; in all other years, the CSU is stable and slightly to the right of the CDU, consistent with its law-and-order appeal (cf. Appendix Figure F2).

15. Most newly-founded extreme right-wing parties in Germany are immediately scrutinized by the German domestic intelligence agency. By developing out of an existing, "bourgeois" party, the present-day AfD managed to avoid this fate.

The qualitative difference between the AfD and previously existing, more extreme right-wing parties is also visible from the divergence between expressed voting intention in polls and actual electoral results. For other right-wing parties, voters are reluctant to express their support even in anonymous political surveys: in the GLES, in most waves zero respondents (or low single-digit numbers) declare support for the NPD, much lower than the figures expected from projecting electoral results to the survey sample size. By contrast, while opinion polls often underestimate true support for the AfD, the relative scope of such social stigma in surveys is much smaller than for other right-wing parties such as the NPD.[16]

A final, important dimension of the AfD as a viable political option is that the party had — prior to the federal election of September 2017 — a realistic chance of passing the 5% threshold and entering parliament. In all state elections between 2016 and 2017 the AfD had passed the threshold and obtained up to 24.1% of the vote. Voters in 2017 thus had the plausible expectation that the AfD would pass the threshold at the national level and be represented in the *Bundestag*. A vote for the AfD was thus not merely an act of protest/expressive voting, but could have instrumental motives (Fiorina, 1976). This, again, distinguishes the AfD from other parties to the right of the CDU, which never polled close to 5% nationally.

1.3 Data Description

1.3.1 Electoral Data

Our electoral data are drawn from the official website of the Federal Returning Officer (*Bundeswahlleiter*) for the federal elections to the *Bundestag* in September 2013 and 2017. The data are provided at the municipality (*Gemeinde*) level. Data for the federal elections prior to 2013 are obtained from DESTATIS, the German federal statistical office. We purchased the municipality-level tabulations of all elections from 1998 until 2009. We harmonize all results to reflect the geography of municipalities in 2015; when municipalities are split and assigned to neighboring units, we assign the outcomes fractionally based on population weights.[17]

For the electoral results of right-wing parties during the Weimar Republic, we make use of the pathbreaking work of Jürgen Falter and Dirk Hänisch (Falter and Hänisch, 1990), who digitized the votes for the *Reichstag* elections from 1920 until 1933 as published in the series *Statistik des Deutschen Reiches*. In all years, except for the two elections of 1932 (July and November), electoral

16. In Appendix Figure F3, we calculate "stigma shares" as the ratio of actual electoral support relative to declared support in the GLES. A ratio of 0 would indicate a perfect correspondence between electoral results and polls. For the NPD, the "stigma shares" are often equal to, or close to, 1: (almost) all of the actual support declared in electoral results exceeds the support estimated from polls. For the AfD, the share is positive, indicating a certain amount of reluctance to express support in polls, but considerably lower than for the NPD. As a term of comparison, the ratio is negative for the Greens, suggesting that people are more willing to declare support for the party than actually to vote for it.

17. This algorithm is explained in Supplementary Appendix 1.8.1

results were published at the level of counties as a whole (*Kreis* or *Stadtkreis*), and then separately for all municipalities above 2,000 inhabitants contained in a county.[18]

We match present-day electoral outcomes to the Weimar era party support through a geocoding algorithm, in two steps: in the first step, we geocode the Weimar-era electoral entities (counties and municipalities) listed in the Falter and Hänisch (1990) dataset, using a combination of historical county shapefiles,[19] current geodata from OpenStreetMap, and a variety of other online sources. In the second step, we match modern electoral geographies to these geocoded entities. Supplementary Appendix 1.8.1 describes this algorithm in detail.[20]

1.3.2 Other Variables

We complement our analysis of electoral results with a range of historical and contemporary control variables. For the Weimar era, we rely on the same dataset by Falter and Hänisch (1990), which also contains statistics on, among others, population, unemployment, employment structure, and religious composition in 1925 and 1933. Population and religion data are available at the municipal level (municipalities above 2,000 inhabitants), all other statistics are measured at the county level.

We include a variety of contemporary control variables in our electoral data regressions. These comprise the total population of the municipality, unemployment rate, change of unemployment rates between 2007 and 2017, and a full set of indicators characterizing the degree of urbanization of a municipality.[21] These data are obtained from DESTATIS. Moreover, we use data on educational attainment at the county level (share of workforce with tertiary degrees); the source of these data is the INKAR database.

The most salient political event happening in this time frame is the "(Syrian) refugee crisis", which peaked in the fall of 2015 after Germany's decision to suspend the Dublin agreement and not to deport asylum seekers back to the first EU member state they entered. While most asylum seekers enter Germany through the German-Austrian border in the south-east of the country, they are supposed to be reallocated to the single federal states, and then again to counties, according to

18. From this disaggregation, we can easily reconstruct the aggregate votes for all municipalities contained in a county, but below the 2,000 inhabitants threshold (the "remainder of the county"). For the elections of 1932, no data at a level of disaggregation below the county were published. After 1933, the new regime did not consider the publication of disaggregated electoral results from past democratic elections a priority. We therefore cannot use the 1932 electoral results in our analysis.

19. Provided through the Census Mosaic project, `http://www.censusmosaic.org`.

20. Based on the geographic location, a current municipality is either matched to a city-county (*Stadtkreis*) of the Weimar era, or to one of the municipalities whose electoral data is known because it had more than 2,000 inhabitants. We call these municipalities "exact matches". The remaining municipalities are then assigned, based on their location, to the entity "remainder of the county", i.e. to the aggregate electoral results in a historical county, *outside* the municipalities with more than 2,000 inhabitants. Typically, for any Weimar-era observation relating to the "remainder of the county", there will be several present-day municipalities matched. We account for this by clustering our regression analysis at the level of observation in the Weimar era (*Stadtkreis*, municipality above 2,000 inhabitants, or "remainder of the county").

21. Following EUROSTAT guidelines, DESTATIS classifies municipalities according to its urbanization density as follows: "densely populated" if at least 50% of the population lives in high-density clusters, "thinly populated" if more than 50% of the population lives in rural grid cells, and "intermediate density" (all other municipalities).

a quota system which takes into account population and GDP. Within counties, asylum seekers are further assigned to municipalities according to a variety of criteria. From the Federal Employment Agency (*Bundesagentur für Arbeit*), we obtain the number of asylum seekers in each municipality, as of December 31, 2016.[22]

Another major shock often blamed for the rise of right-wing populism is the loss of qualified manufacturing jobs over the last decades, due to import competition from China or other low-wage countries. We capture these forces through the "trade exposure" variable (import competition minus export competition), measured at the county level, from Dauth, Findeisen, and Südekum (2014).

Finally, we analyze public opinion data to investigate whether a localized shift towards more right-wing viewpoints is responsible for the AfD's electoral success. We use the German General Social Survey (ALLBUS), an opinion survey conducted as a repeated cross-section every two years (we use the waves spanning the period 1996–2016). These data are described in more detail in section 1.5 below.

1.4 Electoral Results

1.4.1 Empirical Setup

How did the emergence of the AfD as a new, relatively cheap (in terms of social image costs) and viable political option to the right of the CDU result in a realignment of the electoral geography of Germany, reflecting older patterns of Nazi party support? In our first, baseline research design we compare electoral results for the AfD in the elections to the federal parliament in September 2013 and 2017: i.e., before and after 2015, the watershed year in which fiscal conservatives were replaced by right-wing populists in the party leadership. In 2013, running on a strict anti-Euro platform, the AfD barely missed passing the 5% threshold to enter the federal parliament; in 2017, the AfD became the third largest force in the German Parliament, scoring 12.6%.[23]

Our baseline regression specification is as follows:

$$\text{ShareAfD}_{it} = \theta_s + \beta \cdot \text{NSDAP}_i + x_{1i}{}'\gamma + \varepsilon_{it}, \tag{1.1}$$

where ShareAfD_{it} is the share of votes cast for the AfD in municipality i in year t. Note that, in our baseline setting, we calculate the share of votes relative to *all* eligible voters, not just relative to votes cast. We do this in order to incorporate two margins of voter mobilization towards the AfD:

22. To be precise, the data from the Federal Employment Agency refer to *Erwerbsfähige Leistungsberechtigte im Kontext von Fluchtmigration*, i.e. potential transfer recipients, able to work, in the context of escape migration. This includes, roughly, all asylum applicants who are above age 15, not disabled, excluding family members who join first emigrants at a later stage.

23. Note that we ignore the elections in the Saarland as the Saar region did not vote for the *Reichstag* in the Weimar era, being under French occupation).

switching from non-voting to the AfD, or from other parties to the AfD. The dependent variable is regressed on a full set of state fixed effects, θ_s, the (standardized) vote share of the NSDAP party in 1933, NSDAP_i, and in some specifications also a set of municipal-level covariates, x_{1i}, such as population or unemployment rates. To facilitate the interpretation of the coefficients, all variables, dependent and explanatory, are standardized.

To take care of municipal-level, time-invariant omitted factors that may determine a constant inclination to vote for the AfD, the following specification takes advantage of the fact that each municipality is observed twice and focuses on the *change* in vote share from 2013 to 2017:

$$\Delta(\text{ShareAfD}_{i,2017-2013}) = \theta_s + \beta \cdot \text{NSDAP}_i + x_{2i}{}'\gamma + \varepsilon_{it} \tag{1.2}$$

Even though the effect of time-invariant municipality characteristics are "differenced out" in such a first-differences specification, one may still want to allow for time-varying effects of covariates, or investigate *changes* in municipal-level covariates occurring between 2013 and 2017. For these reasons, we may also include a vector of covariates x_{2i}, potentially different from the covariates included in Equation 1.1.

Of the 10,963 municipalities in the sample, 2,466 are exactly matched to the same municipality in the Weimar era; the remaining municipalities are assigned one of 259 Weimar-era "remainders of a county".[24] To account for potential correlation between these multiple observations assigned to a single historical electoral result, we cluster all error terms ε_{it} at the Weimar-era unit of observation (either exactly matched municipality, or "remainder of the county").

1.4.2 Baseline Electoral Results and Spatial Correlations

Table 1.1 report our first results. The first column shows that the historical relationship between NSDAP votes and AfD support in 2013 is positive, but small and insignificant. However, looking at support for the AfD in the federal election of 2017, the results are very different. The correlation between past Nazi support and contemporary AfD support, in 2017, when the AfD represented a populist right, xenophobic platform, is strong and significant. In the baseline result of column 2, a one standard deviation increase in NSDAP votes in the Weimar era corresponds to a 0.056 standard deviations higher vote share for the AfD.

The effect is very similar when the dependent variable is defined as the 2013 to 2017 *change* vote share going to the AfD (column 3). To address further concerns about small municipalities today being matched across time to a large unit representing the "remainder of a county" in 1933, we show that results are also robust to aggregating municipal-level data to the (present-day) county level (column 4).

24. More precisely: 2,466 municipalities are either matched to a *Stadtkreis* (city-county) of the Weimar era, or to a municipality contained in a larger county, but which had more than 2,000 inhabitants in the Weimar era. The remaining present-day municipalities cover regions for which the Weimar-era records report only aggregates at the level of "remainder of a county".

Table 1.1: First Results

	(1) AfD 2013	(2) AfD 2017	(3) Δ 17-13	(4) Δ 17-13	(5) Δ 17-13
NSDAP 1933 [std.]	0.0036	0.0573***	0.0601***	0.0768***	-0.0084
	(0.0263)	(0.0195)	(0.0180)	(0.0266)	(0.0169)
NSDAP 1933 [std.] × Soviet Zone					0.1970***
					(0.0656)
NSDAP 1933 [std.] × UK Zone					0.0099
					(0.0254)
NSDAP 1933 [std.] × French Zone					0.1610***
					(0.0440)
Observations	10957	10957	10957	392	10957
R^2	0.164	0.679	0.656	0.780	0.661

Notes: The dependent variable is the standardized gross vote share of the AfD, i.e. the number of valid votes relative to eligible voters. The explanatory variable is the NSDAP vote share in March 1933. All variables (explanatory and dependent) are standardized. Columns 1-3 and 5 include all municipalities in all German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. Column 4 provides a regression on the county level. All regressions include state fixed effects. Standard errors are clustered at the level of historic municipalities/counties. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Finally, in column 5 we investigate whether the persistence effect is stronger in certain areas than in others. Level effects, such as the much higher electoral success of the AfD in former East Germany, are already captured by the full set of state (*Bundesland*) fixed effects included in all regressions, yet there may still be differences between states in the gradient of historical correlation between the 1930s and today. As a first take, we divide Germany into four regions, corresponding to the post-war Allied occupation zones. The regression in column 5 shows that persistence was essentially nil in the US occupation zone (the omitted category) and in the UK zone; that is, in the north and in the south of (former) West Germany. At the other extreme, persistence is highest in the former Soviet occupation zone ("East Germany", or the former German Democratic Republic), and is also high in the French occupation zone (in the south-west). These correlations are consistent with historians' take on the effectiveness of Denazification (Biddiscombe, 2007; Taylor, 2011).

How large, quantitatively, is the explanatory power of our proposed determinant of right-wing voting? The baseline regressions suggest that our proposed channel amounts to about 6% in terms

of standardized effect sizes. In the Appendix, we compare this result to the effect of other plausible determinants of populist right-wing voting that have been extensively discussed in the literature: unemployment levels, changes in unemployment levels from before the great recession until today, increase in trade exposure, and the allocation of refugees in municipalities (see Appendix Table D1). The beta coefficients for the correlations estimated there are in a similar range of magnitude (2–10% in absolute terms) as the beta coefficient relating to the NSDAP vote share. A comparison of the partial R^2 values of these explanatory factors leads to similar conclusions. This simple variance decomposition thus suggests that, while cultural persistence clearly is only one among many factors associated with the rise of populist right-wing parties, the magnitude of the correlation is comparable to other factors often mentioned in the literature.

Figure 1.3 provides an exemplary geographic depiction of the electoral patterns studies here.[25] In these maps, every hexagon corresponds to one municipality. Panel A shows the outcome (NSDAP vote shares) of the 1933 election; Panel B the outcome of the 2017 election (AfD vote shares). Some smooth, broad spatial gradients are evident, as well as apparently very idiosyncratic, highly localized patterns of variation in party support.

Panel C combines these two spatial distributions and displays areas of historical continuity in voting patterns. To provide a visual comparison, we divide municipalities into terciles of NSDAP and of AfD vote shares (lower, middle and upper terciles), resulting in 9 possible combinations. Municipalities that conform to our hypothesis were in the lower (middle, upper) tercile of NSDAP support in 1933, and are in the lower (middle, upper) tercile of AfD support today. We color these municipalities in shades of blue. The remaining municipalities are those in which NSDAP results in 1933 do not map into current outcomes for the AfD: e.g, municipalities with high rates of NSDAP support historically, but low support for the AfD today. They are colored in grey. As Panel C shows, a large number of municipalities are conforming to our hypothesis: at the high, middle, and low end of the range of right-wing support.[26]

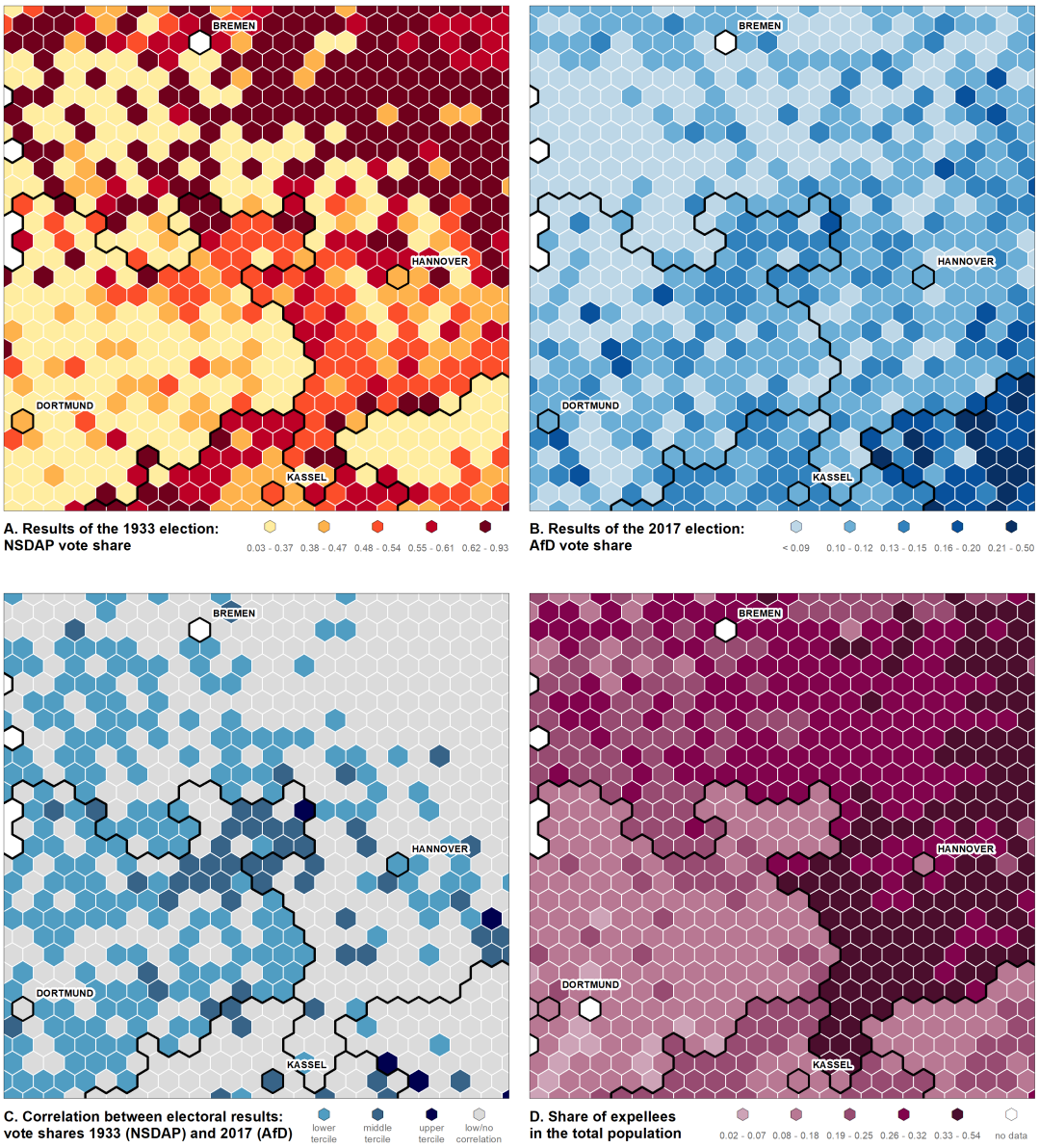
The electoral patterns visible in Figure 1.3 suggest that spatial correlation of the variables, while present, is not very high. In recent research, Kelly (2019) points out how a large part of the literature examining historical persistence relies on highly spatially clustered variables or "treatments". In fact, the spatial autocorrelation of residuals — an important diagnostic test suggested by Kelly (2019) — is very low in our setting: for the baseline estimate of Table 1.1, column 3, the value of Moran's I is 0.04. To further investigate the robustness of our findings to a varying degree of spatial autocorrelation, we follow the advice by Colella et al. (2019) and report Conley standard errors, allowing for varying cutoffs (from 1 to 200km). Figure 1.4 shows that the baseline point

25. Figure 1.3 focuses on a region in North/Central Germany, between Bremen, Hanover, Dortmund, and Kassel, at the intersection of four states (Lower Saxony, North Rhine-Westphalia, Hesse, and Thuringia). We provide full maps of Germany with the electoral results in 1933, 2017, and a comparison of the two — for all 10,963 German municipalities — in subsection 1.8.2.

26. There are also a few notable areas that do not support our hypothesis; notably, the northeastern quadrant, and the area south of Hanover. We will discuss those exceptions further below.

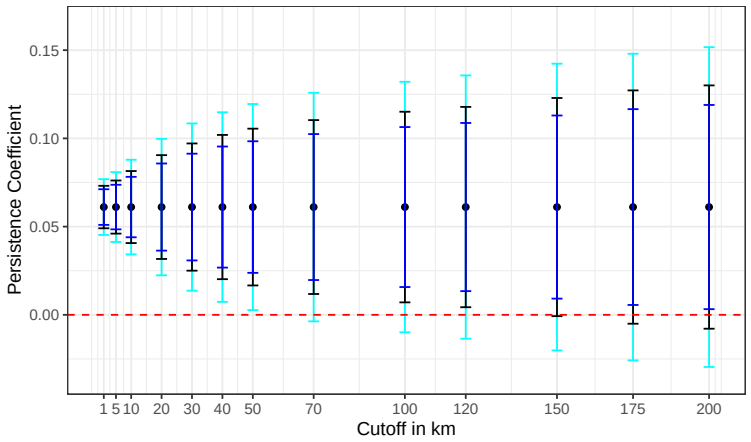
estimate of 0.611 remains significant even under very extreme assumptions about the degree of spatial autocorrelation.

Figure 1.3: Electoral Persistence



Notes: The map shows electoral results, at the municipal level, for a subset of Germany. The area covers a region in the North/Center. Major cities are indicated. Every hexagon corresponds to one municipality. Panel A shows vote shares obtained by the NSDAP in the 1933 election; Panel B shows vote shares obtained by the AfD in 2017. The color scale corresponds to quintiles. Panel C evidences areas of historical persistence. Blue hexagons (of different shades) indicate municipalities that were both in the upper (middle, lower) tercile of NSDAP votes in 1933, and in the upper (middle, lower) tercile of AfD votes in 2017. Grey hexagons are municipalities in which NSDAP support is not correlated with current AfD support. Panel D displays the spatial distribution of post-WWII expellees as a fraction of resident population in ca. 1950.

Figure 1.4: Varying Spatial Correlation Cutoff Distances



Notes: Figure shows coefficients and confidence intervals from regressions of the standardized difference in AfD gross vote share 2017-2013 on standardized historical vote share for the NSDAP in 1933. Blue, black, cyan confidence bands represent 90%, 95%, and 99% confidence interval bounds respectively. Regressions correct for spatial autocorellation at different cutoff distances (x-axis). A distance linear decay is imposed in the spatial correlation structure. Sample includes all municipalities in all German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. All regressions include state fixed effects. Code from Colella et al. (2019).

1.4.3 Persistence of Conservatism or Antisemitism?

The previous tables have shown how the pattern of historical Nazi vote shares is correlated with current AfD support. It is, however, crucial to better define this historical legacy. What exactly constitutes this ideological complexion that shaped the electoral geography of the 1930s, and that, in our interpretation, was manifested again in voting after 2015?

A defining feature of the NSDAP was its antisemitism, and the seminal work by Voigtländer and Voth (2012, 2015) has shown how antisemitism is a persistent feature of certain regions in Germany. In Table 1.2 we argue, however, that antisemitism is not what explains the success of the AfD in more recent years. In each one of the columns, we vary the definition of the explanatory variable, while the dependent variable remains the electoral success of the AfD (change in gross vote share, 2017–2013). Again, all coefficients can be interpreted as standardized beta coefficients.

We first compare the correlation between AfD results and the vote shares of the NSDAP in two other elections, 1930 and 1928.[27] In 1928 the NSDAP was still a fringe party, virulently antisemitic, obtaining only 2.6% of the votes at the national level; in 1930, after having toned down its antisemitic rhetoric and with messages trying to appeal to a broader public, it obtained 18.3% of the votes. As the results in Table 1.2, columns 1 and 2, show, electoral support for the

27. In 1924, the NSDAP did not present a separate list for the *Reichstag* election, but supported the *Deutschvölkische Freiheitspartei* (DVFP). The electoral results of 1932 were not published at the disaggregate level.

Table 1.2: Right-Wing Ideology vs. Antisemitism

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	0.0376*	0.0189	-0.0401**	0.0830***	0.0503***	0.0134	-0.0771**
	(0.0194)	(0.0125)	(0.0168)	(0.0229)	(0.0159)	(0.0169)	(0.0320)
Explanatory Variable	NSDAP 1930	NSDAP 1928	DVFP 1924 (Antisemitic)	DNVP 1924 (Conservative)	NSDAP 1933	1920s/30s Anti-semitism	Medieval Black Death Pogroms
Cities Sample					✓	✓	✓
Observations	10944	10906	10740	10740	767	767	767
R^2	0.654	0.654	0.655	0.657	0.53	0.522	0.526

Notes: The dependent variable is the standardized 2017-2013 difference in gross vote shares of the AfD, i.e. the number of valid votes relative to eligible voters. The column header indicates the respective explanatory variable used. All variables (explanatory and dependent) are standardized, except the indicator variable for Black Death Pogroms in column (7), which has a mean of 0.251. The explanatory variable in column (6) is the standardized first principal component of six measures of 1920s/30s antisemitism, as in Voigtländer and Voth (2012). Sample includes municipalities in all German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. Columns (5) to (7) restrict the sample to those towns used in Voigtländer and Voth (2012). All regressions include state fixed effects. Standard errors are clustered at the level of historic municipalities/counties. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

NSDAP in 1928 and in 1930 is also correlated with today's successes of the AfD. However, the largest correlation obtains with the results in 1933, when the NSDAP is less openly antisemitic than in 1928 and 1930, and is closer to a big-tent right-wing populist party appealing to varied constituencies, eager to take (absolute) power.

The *Reichstag* election of 1924 provides a convenient experiment to discriminate between persistent antisemitism and persistent right-wing ideology. Two right-wing parties were on the ballot: the *Deutschnationale Volkspartei* (DNVP) and the *Deutschvölkische Freiheitspartei* (DVFP). The DNVP was the main conservative party of the Weimar era, before the emergence of the NSDAP: nationalist, reactionary, monarchist. The DVFP was split off the DNVP, as some of its members thought it should be more explicitly antisemitic. As the results in columns 3 and 4 show, the electoral success of the AfD is highly correlated with the staunchly conservative party in the Weimar era (the DNVP), but not with its antisemitic spin-off (the DVFP).

Finally, in columns 5–7 of Table 1.2 we limit the analysis to the 796 cities that are featured both in our dataset and in the seminal work by Voigtländer and Voth (2012) on persistence of antisemitism. In column 5, we first confirm that our baseline estimate of Table 1.1 can be replicated, with broadly similar results, within those 796 cities. In column 6 we then regress the AfD's electoral fortunes on the composite measure created by Voigtländer and Voth (2012): a z-score index encompassing six measures of antisemitism in the 1920s and 30s.[28] The correlation is positive, but

28. This index included measures for: pogroms in the 1920s, the share of DVFP votes 1924, the share of NSDAP votes 1928, letters to the Stürmer (an antisemitic newspaper), deportations per 100 Jews in 1933, and an indicator variable for whether a synagogue was destroyed (or damaged).

smaller and only marginally significant. Finally, in column 7 we use the indicator variable for the occurrence of pogroms in the wake of the Black Death of 1348. The correlation between medieval hatred of Jews and AfD support is in fact *negative* and significant.

These findings suggest that what persisted between the Weimar era and today, and determines the AfD's electoral success, is not antisemitism but rather a broadly conservative, right-wing ideology.[29] Rather, the common ground between the NSDAP in 1933 and the AfD in its post-2015 incarnation is more likely to be found in nationalism, outgroup hatred, and xenophobia.

This hypothesis — a deeply-seated cultural conservatism that correlates with both the electoral success of the NSDAP and of the AfD — is corroborated by the results in Table 1.3. Here, we examine how the baseline correlations vary depending on the inclusion of controls. We consider both controls relating to more transient, arguably endogenous economic conditions, and controls relating to more fundamental cultural traits.

We start with our preferred specification of Table 1.1, column 3, using the change in AfD votes from 2013 to 2017 as the dependent variable. We then successively add covariates, measured both historically (variables that may explain the predominance of NSDAP voters in the 1920s and 30s) and in contemporary terms (present-day sociodemographics as correlates of electoral outcomes). Column 1 of Table 1.3 first presents the baseline estimate (without controls) as a benchmark. In the following columns, we add variables related to population, employment structures, and education. In Panel A, we only include the controls relating to the Weimar era. In Panel B, we only include the controls relating to the present day. Finally, in Panel C we repeat each regression including both historical and contemporary controls.

Column 2 considers the domain of "population": we control either for the (log) size of the municipality in the 1920s/30s, or for its size today, or for all of these variables together. In neither case is the baseline estimate modified substantially. Column 3 considers another major set of determinants of voting behavior: the occupational structure and economic conditions. We control for sectoral employment shares and unemployment rates (in 1933 and/or today). Across all panels, including these controls does not affect the baseline correlation between historical Nazi support and contemporary votes for the AfD (if anything, the correlation becomes stronger). The same conclusion holds true when one considers controls for contemporary or past educational levels (column 4).

As opposed to the variables presented so far, religion is arguably a deeply seated cultural trait: the denominational map of Germany has been set in the 16th century and barely changed since. Religion is also a well-known driver of electoral behavior (see, e.g., Spenkuch and Tillmann, 2017): Catholic regions were substantially less likely to vote for the NSDAP. Consequently, when in column 5 we control for contemporary and/or past shares of religious groups (Catholics, Jews,

29. In fact, the AfD is so far publicly bent on keeping antisemitism out of its official policy platforms and taking an explicitly pro-Israel stance; its religious animus is clearly more directed against Islam. At the same time, however, several elected officials of the AfD have expressed antisemitic attitudes.

others), the magnitude of estimated coefficients is reduced substantially, although not entirely.

Taken together, these results suggest that certain communities, often (but not exclusively) characterized by a Protestant religious majority, nurture a consistently conservative social environment. When a corresponding political option is on the electoral menu, as during the Weimar republic and then again after 2015, this right-wing ideology is also expressed in electoral results. Importantly, this phenomenon is ideologically and geographically distinct from the historical persistence of antisemitism documented elsewhere.

1.4.4 Contextual and Mediating Factors

In the following table, we consider more closely other control variables. A common characteristic of the factors examined here is that they can have two roles. The first concern is that some other shocks, such as the refugee crisis of 2015, might coincidentally and spuriously give rise to similar patterns of electoral support, confounding the interpretation of findings. A second concern is that certain variables may have the role of mediating factors: interacting with the historical legacy of right-wing leaning, either amplifying or dampening it.

In each of the columns of Table 1.4, we run a regression of this type

$$\Delta(\text{ShareAfD}_{i,2017-2013}) = \theta_s + \beta \cdot \text{NSDAP}_i + \gamma \cdot \text{C}_i + \delta \cdot \text{NSDAP}_i \cdot \text{C}_i + \varepsilon_{it} \quad , \tag{1.3}$$

where C_i is a control variable, measured at the municipal (or county) level. γ indicates whether this control variable has a direct effect (not necessarily with a causal interpretation) on the AfD's electoral success; δ measures to what extent this factor interacts with the electoral geography of 1933. All variables are converted to z-scores, so that the coefficients can be interpreted as standardized beta coefficients, and β thus measures the effect of NSDAP support at average levels of C_i.

Arguably the most important political event in Germany in 2015 was the sudden and dramatic influx of refugees, mostly fleeing the Syrian civil war. Hundreds of thousands of them reached Germany on foot starting in September 2015. Refugees were allocated to states and counties according to their size and GDP; however, within counties, the allocation of refugees to municipalities was idiosyncratic. The effect of the refugee inflow on votes for the far right is ambiguous. On the one hand, refugees are often perceived as a threat and a potential source of crime, moving voters to the right (Dustmann, Vasiljeva, and Piil Damm, 2016); on the other hand, in line with Allport's (1954) "contact hypothesis", direct acquaintance with refugees could actually increase empathy and support for moderate parties (Steinmayr, 2017).

After presenting the baseline result again in Table 1.4, column 1, in column 2 we control for the presence of refugees in each municipality (calculated as a share relative to total population, as of December 2016). The effect is slightly negative but insignificant, suggesting that more refugees led, if anything, to *fewer* votes for the AfD. The coefficient for the direct effect of Nazi vote

shares is, however, hardly affected, and the interaction term is very small and not significant. This is also consistent with the observation that the allocation of refugees across Germany was fairly orthogonal to patterns of NSDAP support: as reported at the bottom of the table, the partial correlation between the control variable and the NSDAP vote shares is negative and small (correlation coefficient, conditional on state fixed effects: -0.09).

To gauge the magnitude of the interaction term, we present (just below the coefficient) estimates of the effect of a one standard deviation increase in NSDAP support at the 25th and 75th percentile of the control variable (in this case, refugee presence). Corresponding to a small interaction term, this "interquartile range" of the effects is very narrow, from 6.0% to 6.5% in terms of standardized beta coefficients.

Globalization, the decline of manufacturing, and a decrease in job security are often cited as a cause of the far right's recent electoral fortunes. Overall, Germany had a comparatively strong economy in the time frame considered, and among developed countries it remains among those with the highest shares of employment in (skilled) manufacturing, and the lowest rates of unemployment, also among youths. In fact, across the municipalities in our dataset, between 2007 (before the great recession) and 2017 the unemployment rate decreased by 1.52 percentage points on average.

Levels of unemployment are positively correlated with votes going to the AfD (column 3). The corresponding interaction term is however nearly zero. In column 4, we control for the change in unemployment (between 2007 and 2017); in column 5, we control for a likely major determinant of changes in unemployment, the increase in trade exposure due to the opening of markets to products from Eastern Europe and the Far East.[30] In both cases, the results are somewhat surprising, as areas with *increases* in unemployment (or trade competition) are associated with higher AfD vote shares. Yet the interaction term is very small, and thus the effect of NSDAP voting largely unaffected.

Columns 6 and 7 consider possible mitigating factors that, over the decades since the end of the Nazi regime, might have reduced the vertical transmission of right-wing ideology. First, we look at population growth of municipalities, from the 1930s until today (column 6). Here, neither the direct effect, nor the interaction term are economically large or significant.

Finally, column 7 looks at one major historical event that had the potential of substantially altering the social structure of communities between the 1930s and today: the influx into post-war Germany of over 12 million ethnic German refugees (called *Heimatvertriebene*, or "expellees") from the areas ceded to other countries after 1945 (Poland, Soviet Union, Czechoslovakia). These people, having left their homes and fleeing without possessions, were distributed across Germany following idiosyncratic patterns, based on their ports of entry, but also on the availability of housing stock. They brought their values and traditions from their home regions — Silesia, Pomerania, East Prussia, Sudeten — into present-day Germany, partly maintaining their identities but also

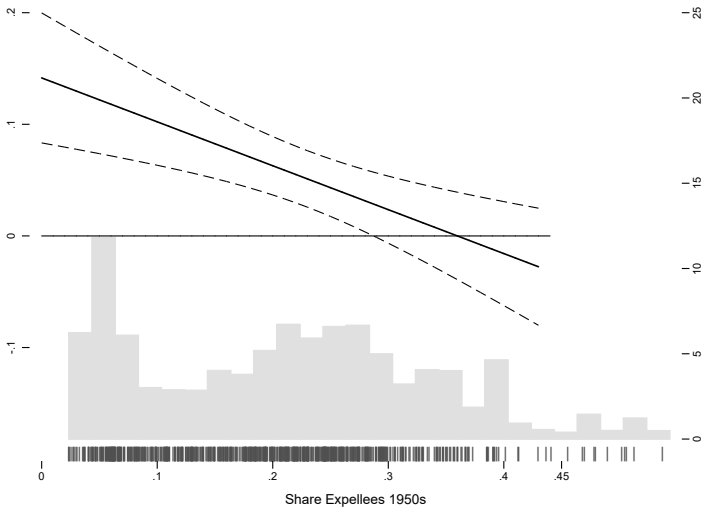
30. We follow the definition of our data source (Dauth, Findeisen, and Südekum, 2014) and capture trade exposure as the difference between import competition and export competition. As an example, regions of Germany specializing in the garment industry score very high on this measure, whereas regions with car factories score low.

integrating rapidly (Bauer, Braun, and Kvasnicka, 2013). As such, one would expect areas with a stronger influx of refugees to exhibit less historical persistence, as the vertical transmission of values will be affected by this massive reshuffling of populations. Moreover, the experience of having to integrate and give housing to expellees might have turned the local populations more sympathetic to the plight of refugees and more open to outsiders

In fact, the direct effect of the presence of expellees is positive and significant. This is consistent with the observation that there is a positive correlation (0.224) between areas of historical Nazi support and areas where expellees are allocated, but also that expellees have traditionally supported more conservative parties. However, we estimate also a large, negative and significant interaction term, suggesting that more expellees weaken the historical persistence of right-wing voting. This effect is also quantitatively important. Communities at the 75th percentile of the distribution of expellees presence witness almost no effect of NSDAP voting on AfD results (point estimate: -0.0293). Figure 1.5 displays the marginal effect of historical NSDAP support at different levels of expellees presence.

The presence of expellees also helps explaining the areas of the electoral map of Germany in which little or no persistence occurred. Areas with a very strong influx of expellees, especially the North (Schleswig-Holstein, Lower Saxony, Mecklenburg) and Bavaria, where many of the Sudeten Germans were relocated, display the lowest degree of persistence (see Appendix Figure B3). This is also visible in Figure 1.3, Panel D: the areas northeast and south of Hanover, where electoral results are not correlated across time (grey hexagons in Panel C), are also the ones with the highes shares of expellees among post-war population.

Figure 1.5: Persistence, Depending on 1950s Expellees Share



Notes: The graph shows a marginal effect plot of the (standardized) NSDAP 1933 vote share on the (standardized) 2017–13 difference in AfD gross vote shares for different shares of 1950s expellees. Code courtesy of Matt Golder.

Table 1.3: Results Including Controls

	(1) Baseline	(2) Population	(3) Employment	(4) Education	(5) Religion
Panel A: *With historical controls*					
NSDAP 1933 [std.]	0.0601***	0.0553***	0.0752***	0.0551**	0.0129
	(0.0180)	(0.0188)	(0.0261)	(0.0244)	(0.0176)
Observations	10957	10889	8080	9416	10263
Panel B: *With contemporary controls*					
NSDAP 1933 [std.]	0.0601***	0.0470**	0.0605***	0.0557***	0.0233
	(0.0180)	(0.0204)	(0.0175)	(0.0190)	(0.0176)
Observations	10957	10957	10847	10957	10143
Panel C: *With historical and contemporary controls*					
NSDAP 1933 [std.]	0.0601***	0.0457**	0.0666***	0.0541**	0.0046
	(0.0180)	(0.0206)	(0.0252)	(0.0249)	(0.0195)
Observations	10957	10889	7996	9416	9809

Notes: The dependent variable is the (standardized) change in vote share for the AfD (relative to eligible voters) from 2013 to 2017. Each column adds a different set of control variables. The explanatory variable across all columns is the 1933 NSDAP vote share (standardized). Population controls are: [historical] log population size (1933); [contemporary] log population size in 2015 and urbanization code dummies (3 categories). Employment controls are: [historical] shares of employed in industry and manufacturing, employed in trade and commerce, and employed in administration (agriculture and "other sectors" is the omitted category), all measured in 1925, as well as the unemployment share in 1933; [contemporary] the unemployment rate in 2015. Education controls are: [historical] share of white collar workers ("Angestellte und Beamte"); [contemporary] share of workers with university degree in 2015. Religion controls are: [historical] the share of Catholics and Jews; [contemporary] the share of Catholics and "Others" (i.e., Muslims, other religions, and no religion). Sample includes municipalities in all German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. All regressions include state fixed effects. Standard errors are clustered at the level of historic municipalities/counties. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table 1.4: Contemporary Shocks

	(1) Baseline	(2) Refugees 2016	(3) Unempl. Level	(4) Unempl. Change	(5) Trade Shocks	(6) Population Growth Rate	(7) Expellees 1940-50s
NSDAP 1933 [std.]	0.0601***	0.0610***	0.0602***	0.0520***	0.0632***	0.0492**	0.0303**
	(0.0180)	(0.0186)	(0.0172)	(0.0189)	(0.0178)	(0.0194)	(0.0149)
Control [std.]		-0.0087	0.0675***	-0.1459***	-0.0264**	-0.0729***	0.1426***
		(0.0072)	(0.0132)	(0.0166)	(0.0122)	(0.0138)	(0.0304)
NSDAP 1933 [std.]							
× Control [std.]		-0.0094	-0.0016	0.0107	-0.0075	-0.0212	-0.0914***
		(0.0087)	(0.0123)	(0.0240)	(0.0113)	(0.0157)	(0.0192)
NSDAP 1933 [std.]							
at 25th Percentile		0.0675	0.0612	0.0498	0.0666	0.0595	0.120
at 75th Percentile		0.0574	0.0596	0.0573	0.0606	0.0370	-0.0316
Partial Correlation							
conditional on FEs		-0.102	-0.000466	-0.0691	0.0830	-0.185	0.243
R^2	0.66	0.66	0.66	0.66	0.66	0.66	0.66
Observations	10957	10872	10847	10957	10930	10889	10931

Notes: The dependent variable is the (standardized) change in vote share for the AfD (relative to eligible voters) from 2013 to 2017. All reported variables are standardized. Sample includes municipalities in all German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. Unemployment Level in column 3 is calculated as the number of unemployed people in 2016 projected onto 2015 municipality borders and divided by the 2015 population. Unemployment Change in column 4 is calculated by projecting the number of unemployed people in 2007 and 2017 onto 2015 municipalities, subtracting and dividing by 2015 population. The Trade Shocks in column 5 are defined as in Dauth, Findeisen, and Südekum (2014) and are calculated by subtracting a measure of export competition from import competition, both over the periods 2008-1998. The population growth rate for column 6 is calculated by subtracting the log population in 1933 from the log population in 2015, whereby remainders of the county are assigned the mean population values of the total of the remainder of the county's respective population in 1933 or 2015. The average share of expellees in 1940-50s (column 7) is 21.2%, with minimum 2.36% and maximum 53.7%. All regressions include state fixed effects. Standard errors are clustered at the level of historic municipalities/counties. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

1.4.5 A Long-Term Perspective

Thus far, we have compared the electoral results of the AfD in 2013 vs. 2017. Our interpretation rested on the premise that the "reinvention" of the AfD as a populist right-wing party in 2015 represented a shift in the supply of political platforms in Germany: for the first time, a rather respectable and plausibly effective right-wing party was available in the electoral menu. Yet even before 2015, German voters had the option of choosing one of the other far-right parties on the ballot, such as the NPD or the *Republikaner*. These parties, however, had a more extreme image than the AfD, with clear neo-Nazi fringes. Moreover, none of these parties had a realistic chance of passing the 5% threshold required to enter the federal parliament, so that a vote for those parties was purely expressive, protest voting. Voting for these parties thus was very "costly:" in terms of social image, and in terms of an arguably lost vote.

We analyze the correlation between historical Nazi vote shares and the electoral results of these parties in all federal elections from 1998 until today. For every election, we calculate the share of votes going to all parties explicitly to the right of the CDU: NPD, DVU, *Republikaner*, and *Die Rechte*. In 2017, we add the vote share of the AfD as well. We then study the correlation of aggregate far-right vote share with the historical vote share of the NSDAP in 1933, following the baseline regression setup of equation (1.1), only using this more encompassing measure of right-wing support as the dependent variable.

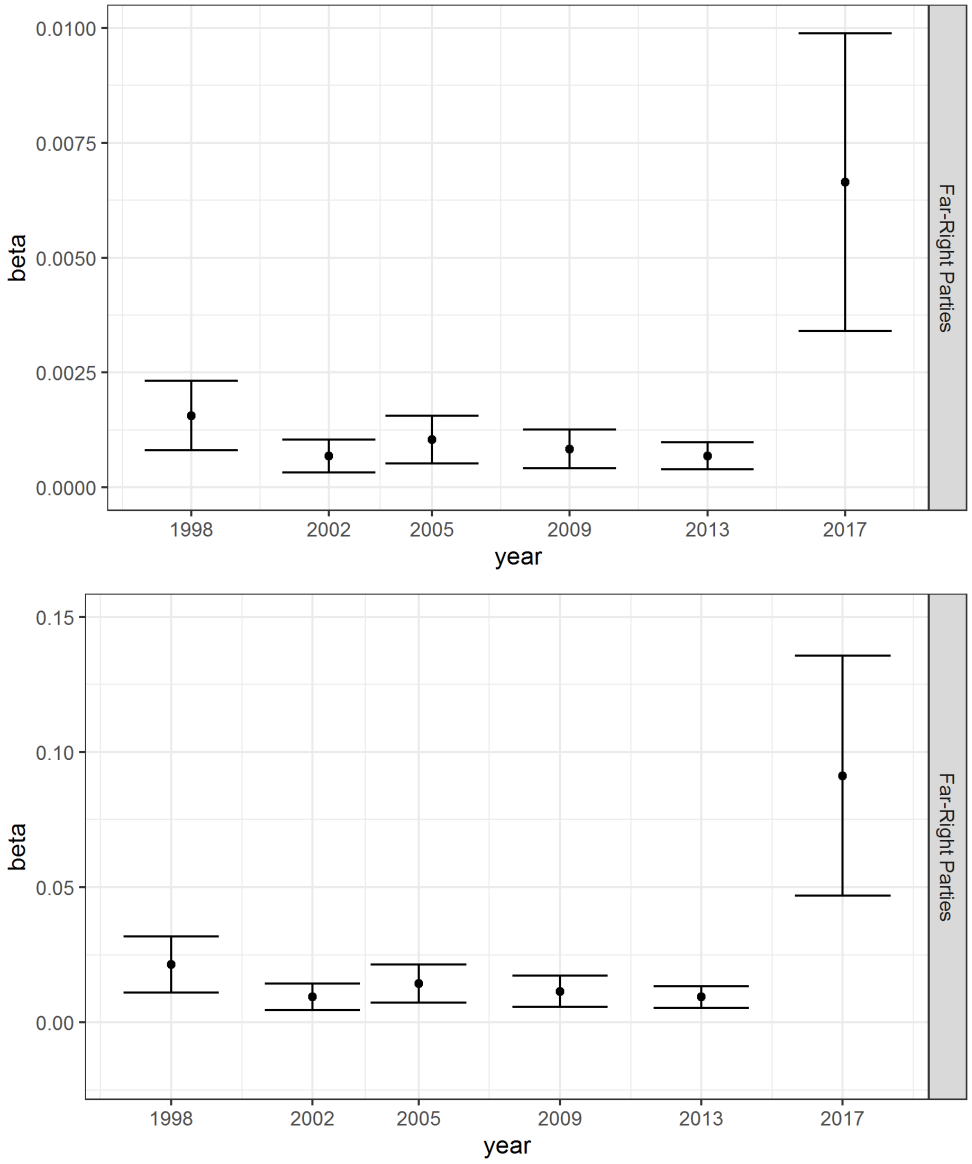
Figure 1.6 displays point estimates and 95% confidence intervals for these regressions, run separately for every election year. The upper graph uses absolute vote shares as the dependent variable. The correlation between NSDAP vote share and far-right vote shares is positive and significant in all years, but experiences a major jump in the election of 2017, when the AfD is included. In 2013 and prior years, one standard deviation higher NSDAP vote is associated with approximately 0.1 percentage points higher vote shares for far right parties, whereas this coefficient rises to 0.65 in 2017.

Part of this increased correlation may simply be mechanical, as the total number of votes going to far-right parties increases considerably once the AfD is added to the camp.[31] For this reason, the lower panel of Figure 1.6 uses standardized vote shares for every year. By doing this, we take care of the large level jump in absolute votes, and focus only on the spatial variation irrespective of levels. The results, however, are qualitatively identical. There is a small, positive, and significant correlation in all years, but the magnitude of the correlation increases fourfold once the AfD is in the choice set of voters.[32]

31. The highest vote shares obtained by far-right parties prior to 2017 were 1.8% for the *Republikaner* in 1998, and 1.6% for the NPD in 2005, much less than the 12.6% of votes going to the AfD in 2017.

32. Appendix Table D4 shows the results of running the analysis in a panel setting, pooling all electoral years and testing whether the effect of past NSDAP support changes in 2017. Figure D1 shows that this jump between 2017 and all previous years is unique to the far-right parties; no other major party experienced a similar discontinuity. Our analyses so far are limited to the elections from 1998 onwards. Electoral data for elections back to 1980 are available from DESTATIS, however no digital maps of municipal borders and electoral constituencies for the years before 1998 exist. We can however limit ourselves to the subset of municipalities that could be matched manually, through their

Figure 1.6: Persistence of (Generic) Far-Right Voting



Notes: The graphs show coefficients and confidence intervals of regressions of far right vote shares (NPD, DVU, Republikaner, Die Rechte, and AfD in 2017) on the standardized NSDAP vote share in March 1933. In the first graph, the dependent variable is the absolute vote share; in the second graph, the standardized vote share for far-right parties. The sample includes modern municipalities in all German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. Municipalities are projected to 2015 borders using population-weighted raster techniques (see 1.8.1 for more detail). Standard errors are clustered at the level of the historical vote shares.

numeric code, to the period 1980–1998. Figure D2 replicates the main results in this sample.

1.5 Survey Results

1.5.1 Data and Empirical Setup

So far, we have documented that municipalities with stronger support for the Nazi party in 1933 now have a stronger vote base for the AfD. Our interpretation of this finding is that the AfD, by expanding the electoral platform and representing a respectable and viable political option, has "activated" an existing demand for right-wing ideology and channeled it into manifest voting actions. An alternative to this supply-side interpretation would be that an overall rightward shift in public sentiment, i.e. a shift in attitudes, led to the persistence we find.

To generate the same patterns that we have observed so far — little or no correlation between Nazi voting and AfD/right-wing support in 2013 and before, and a positive correlation in 2017 — such a demand-side interpretation would require that municipalities with a past history of Nazi support have turned more righ-wing between 2013 and 2017. For example, it could be that these municipalities are "bellwether" regions, turning to the extreme earlier or more strongly than others when confronted with shocks such as the Great Depression in the 1930s, or the influx of refugees in 2015. We thus ask: did a simultaneous change in attitudes occur in areas with historically strong Nazi support between 2013 and 2017?

We analyze this question using the German General Social Survey (ALLBUS) from the Leibniz Institute for the Social Sciences (GESIS). The ALLBUS survey provides rich data on attitudes and political opinions in Germany, and is conducted every two years as a repeated cross-section. To link our historical electoral results with the ALLBUS data, we obtained access to the restriced-use ALLBUS with municipality indicators. We successfully matched 1,273 municipalities in the survey sample, with a total of 39,449 individual observations (across all waves from 1996 until 2016).

The ALLBUS is a very extensive survey with scores of different questions. At the same time, right-wing ideology might comprise a large spectrum of different attitudes. We therefore want to capture broad changes in attitudes that are only imperfectly measured by any single survey question, while addressing concerns about multiple hypothesis testing. For this purpose, we construct standardized indices for different categories of attitudes pertaining broadly to right-wing ideology.

We first identify all pertinent questions and categorize them into seven broad categories: *(i)* xenophobia, *(ii)* attitudes toward Islam, *(iii)* antisemitism, *(iv)* disenchantment with politicians, *(v)* gender attitudes, *(vi)* pride to be German, and *(vii)* left-right self-evaluation. As not every question in the ALLBUS is asked in every wave and to every participant, we split up the categories xenophobic attitudes and gender attitudes into two subcategories, based on the survey cycle in which the questions are asked.[33]

In a second step, we recode all questions into variables between 0 and 1, with higher values indicating more right-wing attitudes. Within every (sub)category, we create an index following

33. Appendix 1.8.5 provides a detailed overview of the questions used for the indices.

Anderson (2008): each component is standardized, and then all z-scores are added up to a summary index (which is, in turn, standardized), weighting each component by the inverse of the covariance matrix of the standardized components.[34] When a category consists of only one survey question (disenchantment with politicians, pride to be German, and left-right self-evaluation) we simply standardize the outcome. Finally, we also construct a summary index of all our outcome indices, again following the procedure by Anderson (2008).[35]

We analyze changes in attitudes through a simple regression setup:

$$\text{Attitude}_{ijt} = \theta_t + \varphi_{s(i)} + \beta \cdot \text{NSDAP}_j + \gamma \cdot \text{NSDAP}_j \cdot \mathbb{1}_{t=2016} + x'_{it}\xi + \varepsilon_{ijt}, \tag{1.4}$$

where Attitude_{ijt} are index values (or standardized responses) as described above, pertaining to individual i in municipality j, in wave t. NSDAP_j is the municipal-level Nazi vote share in 1933, θ_t are year fixed effects and $\mathbb{1}_{t=2016}$ is an indicator for the year 2016. To account for the differences between states, all regressions include a full set of state fixed effects $\varphi_{s(i)}$. Standard errors are clustered at the level of variation of NSDAP_j.[36]

If municipalities that supported the Nazi party in 1933 exhibit persistent right-wing ideology, this will be reflected by a positive point estimate on β.[37] Crucially for our hypothesis, we expect the interaction coefficient γ to be indistinguishable from zero, suggesting that no significant shift in attitudes occurred between 2014 and 2016 in those municipalities with a history of Nazi support.

1.5.2 Results: Right-Wing Attitudes

Table 1.5 summarizes our results. First, we focus on the estimates of β, i.e. the time-invariant effect of past NSDAP support. Throughout the (sub)categories, we find that a history of Nazi support is positively, sometimes significantly, correlated with more right-wing attitudes today, after more than half a century. The effect is particularly pronounced for attitudes towards immigration, towards islamic religious teaching in public schools, and disenchantment with politicians. Consistently with the evidence in Section 1.4.3, we find a positive but small and insignificant effect on antisemitic attitudes (column 5).[38] Higher levels of Nazi support are also only weakly correlated with attitudes towards women, national pride, or a generic left-right self-evaluation (columns 7–10).

34. Since the resulting index is standardized as well, it allows for easy comparison of the estimated magnitudes. This weighting maximizes the amount of information captured by the z-score index. We show that our results are robust to using an equally-weighted index in Appendix Table E3 and Table E4.

35. Appendix Table E5 shows that these indices are correlated with stated voting intentions for the AfD. In Table E6, we focus on the question about left-right self-evaluation (which is asked regularly in every wave), and show that a more right-wing self-evaluation in the ALLBUS is predictive of AfD voting intentions in 2016, but *not* in 2014.

36. Alternatively, one can aggregate responses to the municipal level, j. We show the corresponding analysis in Appendix Table E1.

37. To the extent that some sensitive questions, such as those about antisemitic or xenophobic attitudes, will be affected by social desirability bias, our estimates will be biased downward.

38. Note that our finding complements Voigtländer and Voth (2015), who find that anti-Semitic attitudes are particularly pronounced only for ALLBUS respondents who grew up under the Nazi regime.

Our summary index of the indices (column 11) captures the broad thrust of findings: people living in historically Nazi supporting areas have more right-wing attitudes today. In panel B, we introduce individual-level controls such as age, gender, education, income levels, or citizenship. The inclusion of controls reduces magnitude and significance of most estimates; yet all cases, β remains non-negative. The estimate on the global index of column 11 suggests that an increase in historical NSDAP support by one standard deviation is associated with approximately 5% of a standard deviation more right-wing attitudes.

We now consider the estimates of γ, i.e. the interaction term between NSDAP voting and a 2016 indicator, to investigate whether there is a *shift* in attitudes in 2016 for those municipalities with strong Nazi support in 1933. Our evidence in Table 1.5 shows that all interaction terms are insignificant and often negative. There is a positive, marginally significant effect in the case of disenchantment with politicians (column 6), which however is not robust to the inclusion of controls. The only consistent effect is a negative estimate in column 3, suggesting that individuals in former NSDAP-supporting municipalities became more open to foreigners as neighbors or relatives in 2016. The global index in column 11 suggests, too, that if anything those municipalities turned less right-wing in recent years, although the estimate is not significant.

To conclude, we find no evidence of a sharp and localized demand-side shift that could explain our findings on electoral outcomes. Historical Nazi support is positively associated with all categories of far-right attitudes which we capture in the ALLBUS data, yet not with a rightward shift in 2016.

Table 1.5: Survey Results: Attitudes

Dependent variable:	Xenophobia (Immigration)	Xenophobia (Equal Rights)	Xenophobia (Marriage/ Neighbour)	Islam at School	Antisemitism	Disenchantment with politicians	Gender Attitudes Index 1	Gender Attitudes Index 2	Left-Right Self-Evaluation	Pride to be German	Summary Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: *individual level, without controls*											
NSDAP 1933 [std.]	0.0635***	0.0589**	0.0263	0.0421***	0.0212	0.0110***	0.0065	0.0021	0.0016	0.013	0.0803***
	(0.0236)	(0.027)	(0.0282)	(0.0121)	(0.0341)	(0.0041)	(0.025)	(0.0159)	(0.002)	(0.0105)	(0.0264)
NSDAP × 2016	-0.0404	-0.0698	-0.0923*	-0.0136	0.0095	0.0202*	0.064	0.0451	0.0015	-0.0305**	-0.0277
	(0.0533)	(0.0435)	(0.0506)	(0.0169)	(0.052)	(0.0119)	(0.0389)	(0.0345)	(0.0062)	(0.0153)	(0.0559)
Observations	6632	8290	8305	10100	7305	28255	3411	16036	36957	9434	6227
Panel B: *with individual-level controls*											
NSDAP 1933 [std.]	0.0371*	0.032*	0.0048	0.0363***	0.0078	0.0047	0.0171	0.0006	0.0001	0.008	0.0531**
	(0.022)	(0.0256)	(0.0298)	(0.0123)	(0.0343)	(0.0037)	(0.0277)	(0.0149)	(0.002)	(0.0109)	(0.0219)
NSDAP × 2016	-0.0301	-0.0506	-0.0910*	-0.0159	0.0222	0.0194	0.0543	0.0399	0.0029	-0.0283*	-0.0141
	(0.0485)	(0.0449)	(0.0538)	(0.0169)	(0.0514)	(0.0119)	(0.0424)	(0.0329)	(0.0065)	(0.0162)	(0.0520)
Observations	5714	7149	7122	8701	6344	24873	3065	14086	32625	8152	5463

Notes: Data is individual survey data from Allbus. Indices in columns 1-3, 5, 7-8, and 11 are summary indices as described in Anderson (2008). All regressions contain state fixed effects. Controls include age, gender, marriage status, education, party membership, income, Germany citizenship, and whether the respondent lives in former Eastern Germany. Standard errors are clustered at the level of historical vote shares. See 1.8.5 for the wording of the questions used for the indices. See E1, E2, E3, and E4 for alternative specifications. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

1.6 Mechanisms and Interpretation

Our findings have important implications about the intergenerational transmission of political preferences, and how these preferences are translated into political outcomes. These results can be validated further.

First, our results speak to the existing literature on cultural persistence. To explain cultural persistence, social scientists have often relied on models of from evolutionary biology, studying the vertical transmission of traits from parents to children (Bisin and Verdier, 2001). Our results show an instance in which political inclinations are persistent across generations: places that voted for the Nazi party in 1933 tend to vote more for the AfD in 2017, and exhibit more right-wing attitudes in survey questions.

This raises the question whether this inclination was transmitted vertically from parents to children, over 80 years. Appendix Table F1 provides evidence in support of this hypothesis: using the German Longitudinal Election Study (GLES), we show that in Germany the left-right self-evaluation of children is highly correlated with that one of their parents. This correlation is even stronger in small communities, consistent with our results in Table 1.4. Related to this, an important literature in political science has studied the intergenerational transmission of political preferences (Beck and Jennings, 1991; Jennings, Stoker, and Bowers, 2009), also specifically for the context of right-wing ideology (Avdeenko and Siedler, 2017).[39]

Second, an important conclusion is that, even as underlying attitudes are stable or move only slowly, electoral results can change suddenly and sharply when the political landscape experiences idiosyncratic shocks, such as the creation of a new party or the emergence of a new, charismatic leader. Political systems and institutions — such as the impediments posed by the German constitution to the rise of extremist parties — play an important role in mediating and translating shifts in attitudes into electoral successes. Our finding rationalizes the observation by political scientists that it is hard to square the recent "wave" of right-wing populism with a concurrent shift in attitudes (Bartels, 2017; Bonikowski, 2017).[40]

We corroborate this observation by expanding our attention to five Western European countries that have witnessed the success of populist right-wing movements in recent years: Germany, France, the United Kingdom, Netherlands, and Sweden. For each of these countries, we calculate the vote shares going to far-right parties in national elections, from 1997 until today. From Eurobarometer surveys, we calculate the average left-right self-placement of individuals. We then add up results across five countries, weighting by population.

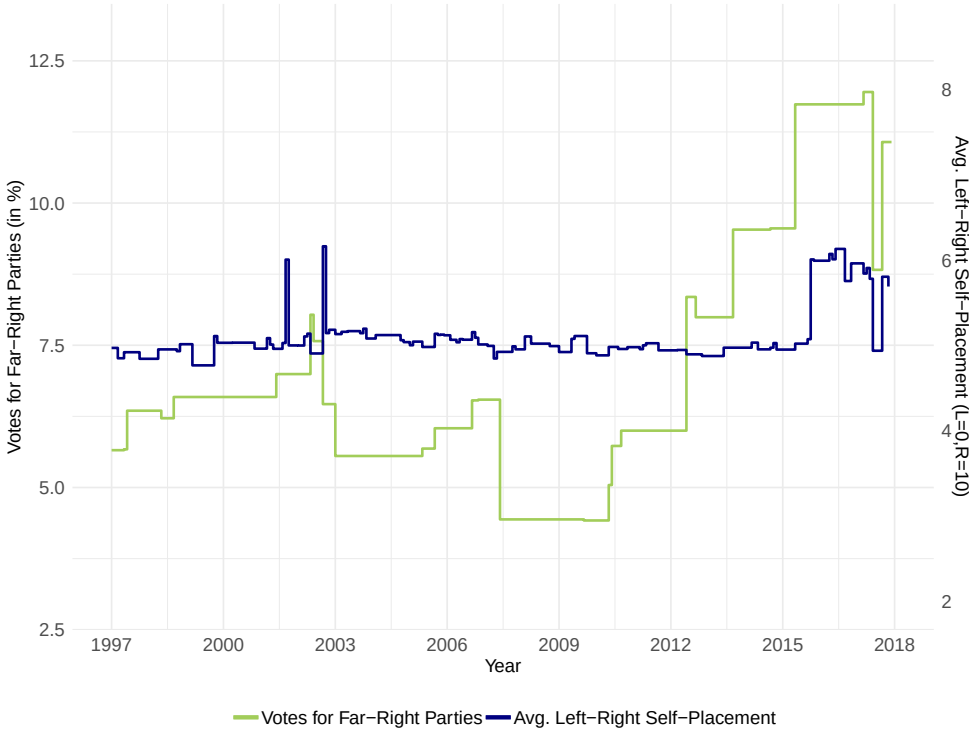
The results in Figure 1.7 show how the aggregate share of votes going to far-right parties picks up in 2010 and rises continuously, with every national election, until 2017. Major jumps in the

39. Another literature considers the genetic origins of political ideology: see Alford, Funk, and Hibbing (2005), Hatemi and McDermott (2012).

40. See also Hatton (2016). In emphasizing the interaction between demand and supply factors explaining the rise of right-wing populism, our research is in line with the theoretical framework by Mudde (2007b) and Arzheimer (2009).

aggregate share of votes for the far right are mostly caused either by entry of new parties (such as the AfD) or of new political leaders skilled in changing the terms of political communication (e.g., Geert Wilders in the Netherlands). In contrast to this steady increase, the time series of the average left-right self-placement of the population is remarkably stable, and jumps upwards only towards the end of the time period observed.

Figure 1.7: Left-Right Self-Evaluation and Votes for the Far Right



Notes: The graph shows average left-right self-evaluation and votes for far right parties, 1997–2018, for five European countries: Germany, France, the United Kingdom, Sweden, and the Netherlands (population-weighted average). Left-right self-evaluation is from Eurobarometer. Electoral data data are from the Parlgov database, `http://www.parlgov.org/`. We classified parties with a score of 7.8 or above as "far-right". This includes, among others, UKIP, AfD, Front National, Sweden Democrats, and the Dutch Party for Freedom (Geert Wilders), in the category.

1.7 Conclusion

We have argued that a hitherto unexplored historical persistence of right-wing ideology is a determinant of electoral outcomes in Germany. As an existing party, the Alternative for Germany, moved to the right end of the political spectrum and espoused a nationalist, xenophobic platform, a historical pattern emerged: municipalities that supported the NSDAP during the Weimar republic

voted proportionally more for the AfD. This historical correlation is positive, significant, and large: in our baseline specification, a one standard deviation increase in Nazi support during the Weimar era is associated with 0.06 standard deviations more support for the AfD in recent elections.

We show that this cultural persistence factor is not confounded by other factors associated with the rise of right-wing populist parties, such as unemployment, exposure to trade shocks, or the presence of refugees. Whereas cultural persistence in other domains has been extensively studied in the economics literature, ours is the first study, to the best of our knowledge, to link historical far-right voting from before 1945 to present-day electoral outcomes.

Cultural persistence of far-right voting in Germany, in our view, has been "activated" by an expansion of the supply of (respectable) political platforms. We rule out an alternative interpretation of our findings as stemming from a demand-side shift, i.e. a shift in attitudes, occurring at the same time in municipalities with higher support for the Nazis in 1933. The experience of Germany — we argue —is representative for many other European countries, where populist right-wing parties have emerged successfully in the last decades, while attitudes have remained broadly constant.

1.8 *Appendices*

1.8.1 *Data Description*

Table A1: Summary Statistics

	(1) Mean	(2) Std.Dev.	(3) Min	(4) Max	(5) Obs.
Panel A: *Voting outcomes*					
AfD vote share, 2017 (gross)	0.11	0.05	0.00	0.39	10957
AfD vote share, 2013 (gross)	0.04	0.02	0.00	0.19	10957
AfD vote share, Δ 2017-2013 (gross)	0.07	0.05	-0.10	0.33	10957
NSDAP vote share, March 1933	0.50	0.14	0.03	0.93	10957
NSDAP vote share, Sept. 1930	0.19	0.11	0.01	0.78	10944
NSDAP vote share, May 1928	0.03	0.04	0.00	0.63	10906
DVFP vote share, April 1925	0.07	0.07	0.00	0.59	10740
DNVP vote share, April 1925	0.19	0.18	0.00	0.81	10740
Panel B: *Control Variables*					
Log population, 1933	10.06	0.91	7.36	13.59	10889
Log population, 2015	7.52	1.51	2.83	14.19	10957
Share employed in industry and manufacturing, 1925	0.30	0.13	0.04	0.90	8960
Share employed in trade and commerce, 1925	0.10	0.05	0.02	0.52	8960
Share employed in administration, 1925	0.03	0.01	0.01	0.43	8960
Share unemployed, 1925	0.05	0.03	0.01	0.22	8536
Share unemployed, 2016 (gross)	0.02	0.01	0.00	0.14	10847
Share Catholic, 1925	0.41	0.41	0.00	1.00	10263
Share Jewish, 1925	0.00	0.01	0.00	0.14	10263
Share Catholic, 2011	0.35	0.31	0.00	1.00	10155
Share other/no religion, 2011	0.30	0.21	0.00	0.95	10143
Share refugees, 2016	0.00	0.00	0.00	0.05	10872
Share unemployed, Δ 2017-2007	-0.02	0.02	-0.17	0.07	10957
Trade exposure, Δ 2008-1998	-0.95	1.75	-16.11	12.94	10930
Population growth rate, 2015-1930 (logs difference)	0.25	0.68	-3.49	4.08	10889
Share expellees, 1940-50s	0.21	0.12	0.02	0.54	10931
Share workers with tertiary education, 2015	0.10	0.03	0.05	0.40	10957

Notes: Sample includes German municipalities in 2015, except city states and the Saarland. Gross vote shares are votes cast divided by the total of eligible voters. See 1.8.1 for more detail on sources and definitions of the variables.

Sources and definitions of Variables

Variable	Description and source
AfD vote share, 2017 (gross)	AfD vote share in 2017 federal election, relative to eligible voters. Municipalities of 2017 matched to borders of 2015. Source: DESTATIS (German Federal Statistical Office).
AfD vote share, 2013 (gross)	AfD vote share in 2013 federal election, relative to eligible voters. Municipalities of 2013 matched to borders of 2015. Source: DESTATIS.
AfD vote share, Δ 2017-2013 (gross)	Difference in AfD vote shares in 2013 and 2017 federal elections (matched to 2015 municipality borders), relative to eligible voters. Source: DESTATIS.
NSDAP vote share, March 1933	NSDAP vote share, March 1933. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
NSDAP vote share, Sept. 1930	NSDAP vote share, September 1930. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
NSDAP vote share, May 1928	NSDAP vote share, May 1928. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
DVFP vote share, April 1925	DVFP vote share, April 1925. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
DNVP vote share, April 1925	DNVP vote share, April 1925. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
Log population, 1933	Logarithm of population in 1933, if missing 1925. Municipalities in the "remainder of the county" are assigned the population count of that remainder divided by the number of modern municipalities assigned to it. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990) and DESTATIS.
Log population, 2015	Logarithm of the 2015 population count. Source: DESTATIS.
Share employed in industry and manufacturing, 1925	Share employed in industry and manufacturing, 1925. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)

Continued on next page

Variable	Description and source
Share employed in trade and commerce, 1925	Share employed in trade and commerce, 1925. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
Share employed in administration, 1925	Share employed in administration, 1925. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
Share unemployed, 1925	Share unemployed, 1925. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
Share unemployed, 2016 (gross)	Total of unemployed people in 2016 projected on 2015 municipalities, and divided by 2015 population. Data matched to municipality borders of 2015. Source: DESTATIS.
Share of white collar workers, 1933	Share of white collar workers ("Angestellte" and "Beamte") amongst working population, 1933. If missing, replaced by 1925 share of white collar workers among population. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
Share of workers with university degree, 2015	Share of workers with university degrees. Variable defined at *Kreis* (county) level. Data matched to municipality borders of 2015. Source: INKAR
Share Catholic, 1925	Share Catholic, 1925. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
Share Jewish, 1925	Share Jewish, 1925. Data matched to municipality borders of 2015. Source: Falter and Hänisch (1990)
Share Catholic, 2011	Share Catholic, 2011. Data matched to municipality borders of 2015. Source: DESTATIS.
Share other/no religion, 2011	Share other/no religion, 2011. Data matched to municipality borders of 2015. Source: DESTATIS.
Share refugees, 2016	Share of people eligible to benefits in the context of flight and migration (*"Erwerbsfähige Leistungsberechtigte im Kontext von Flucht und Migration"*), 2016. Source: Federal Employment Agency.
Share unemployed, Δ 2017-2007	Total of people unemployed in 2017 and 2007 matched on 2015 municipality borders, and divided by 2015 population. Source: Federal Employment Agency.

Continued on next page

Variable	Description and source
Trade exposure, Δ 2008-1998	Import Competition minus export competition. Variable defined at *Kreis* (county) level. Data matched to municipality borders of 2015. Source: Dauth, Findeisen, and Südekum (2014)
Population growth rate, 2015-1930	Growth rate of population 2015 - 1933 (1925 if missing), both periods matched to 2015 municipality borders. Source: Falter and Hänisch (1990) and DESTATIS.
Share expellees, 1940-50s	Share of expellees in 1940-50s. Variable defined at *Kreis* (county) level. Data matched to municipality borders of 2015. Source: Data for Eastern Germany stems from: Seraphim (1954); Western Germany: Braun and Kvasnicka (2014)

Matching electoral data to 2015 municipality borders

We generate transition matrices to trace population shares within changing municipalities' borders. Each matrix uses 2015 as reference year, and represents the population of a municipality in year 2015 as a sum of shares of populations of municpalities existing in year t.

Step 1: First, we approximate the spatial distribution of the population in municipalities based on a raster dataset (100m grid) of the 2011 Census[41], using shapefiles for German municipalities between 1998 and 2017 from the Service Center of the Federal Government for Geo-Information and Geodesy (*Dienstleistungszentrum des Bundes für Geoinformation und Geodäsie*).[42]

Step 2: In the next step, we overlay administrative borders for both timestamps and calculate population in areas which changed municipalities. Based on those values we generate a transition matrix, which indicates to what degree the population from a given municipality in the first comparison year contributes to the population residing in municipalities in 2015.

Step 3: Finally, we multiply the (transposed) transition matrix with the respective data for the comparison year to project values on the 2015 municipality borders.

41. `https://www.zensus2011.de/DE/Home/Aktuelles/DemografischeGrunddaten.html`
42. `http://www.geodatenzentrum.de/geodaten/gdz_rahmen.gdz_div`

Matching contemporary and historical election data

We match present-day electoral outcomes to the Weimar era party support in two steps:

Step 1: First, we identify the boundary of each county with electoral data in the Falter and Hänisch (1990) dataset, using the county name to match counties to polygons in the shapefile provided by the Census Mosaic project[43]. We then identify coordinates for each Weimar era municipality (to the best of our knowledge, no shapefiles of municipalities in the period are available): for each municipality, we first use OpenStreetMap's Nominatim API to search for modern administrative centers, villages, towns, cities or suburbs sharing a name with the historic municipality. We overlay the returned coordinates on the county map and discard any results which lie outside the boundary of the county to which the historic municipality belongs, according to the Falter and Hänisch (1990) data. In this way, we obtain valid latitude and longitude coordinates for around two thirds of the Weimar era municipalities. For municipalities which return no valid matches, for example because of name changes between the Weimar era and today, we manually search for coordinates. To do so, we use a combination of sources including gov.genealogy.net, a database of historic geographies, and Wikipedia. We check the manual lookups for validity by ensuring that the coordinates lie within the boundaries of the county to which the municipality belongs, again according to the Falter and Hänisch (1990) data.

Step 2: In this step, we match contemporary municipalities to a Weimar era geography for which the Falter and Hänisch (1990) dataset provides electoral data. If a modern municipality's coordinates (provided by DESTATIS) are within 2.5 kilometers of the coordinates of a municipality identified in Step 1, we match the contemporary district to the electoral data from that historical municipality. Otherwise, we overlay the coordinates of the modern municipality on top of the shapefile of counties and assign the electoral results for the "remainder of the county" to the modern municipality. Because electoral geography is not constant between 1924 and 1932, a modern municipality can be matched to different entities for different election years.

43. Electoral geography changes between the years 1924 and 1932, the result of counties being merged or split and other boundary changes. We thus match counties to boundaries separately for each of the 1924, 1928, 1930 and 1932 elections. In a very small number of cases, we make changes to the county shapefiles in order to better match the county/municipality hierarchy provided by the Falter and Hänisch (1990) dataset

1.8.2 *Electoral maps*

The following three maps show electoral results in 1933, in 2017, and a visual representation of persistence patterns for all 10,963 municipalities in our analysis. Cf. the description of Figure 1.3 for further detail. Note that the state of Rhineland-Palatinate (in the far West of Germany) is "exploded" in the maps and presented separately because of its large number of municipalities.

Figure B1: NSDAP 1933

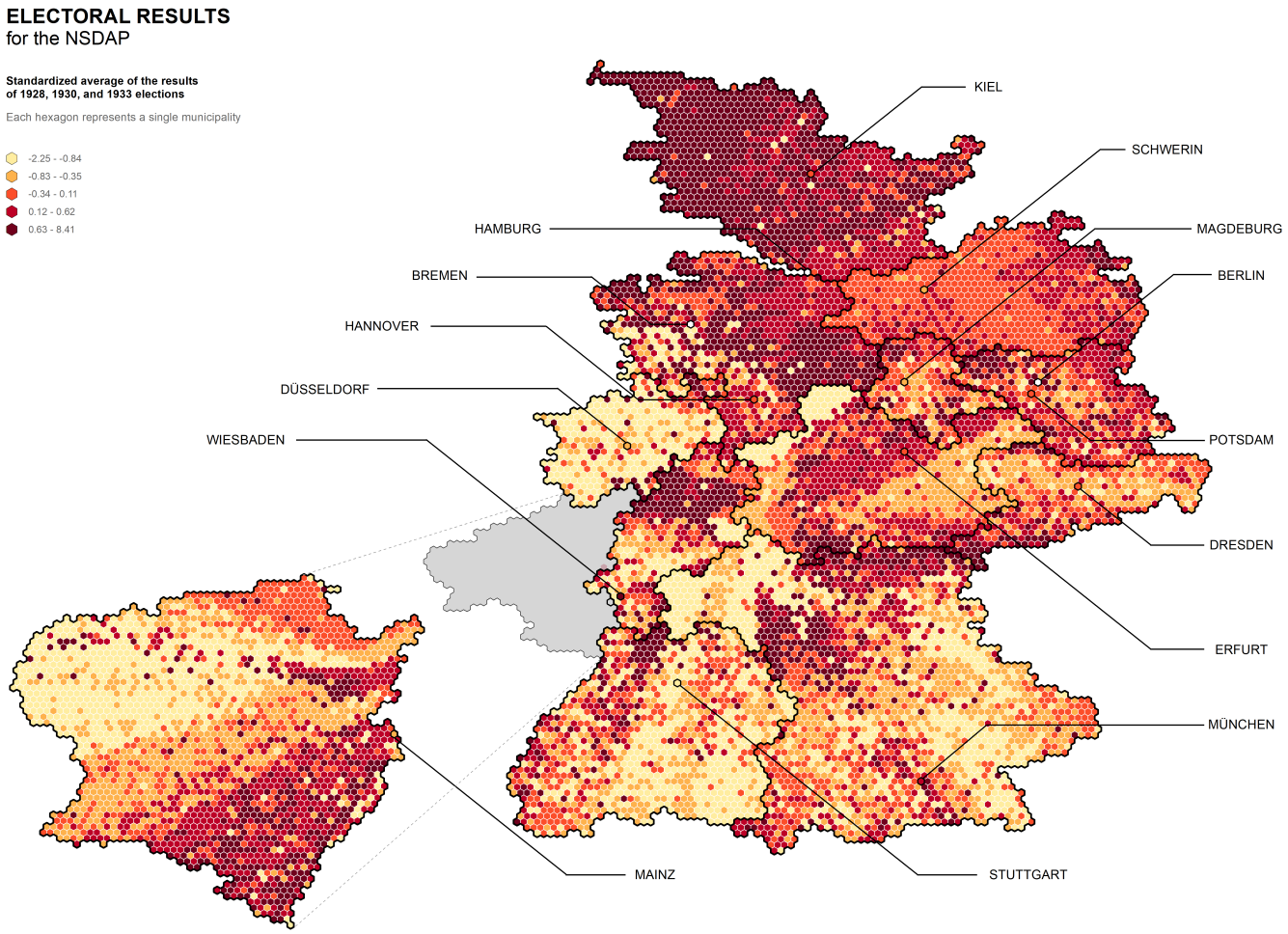


Figure B2: AfD 2017

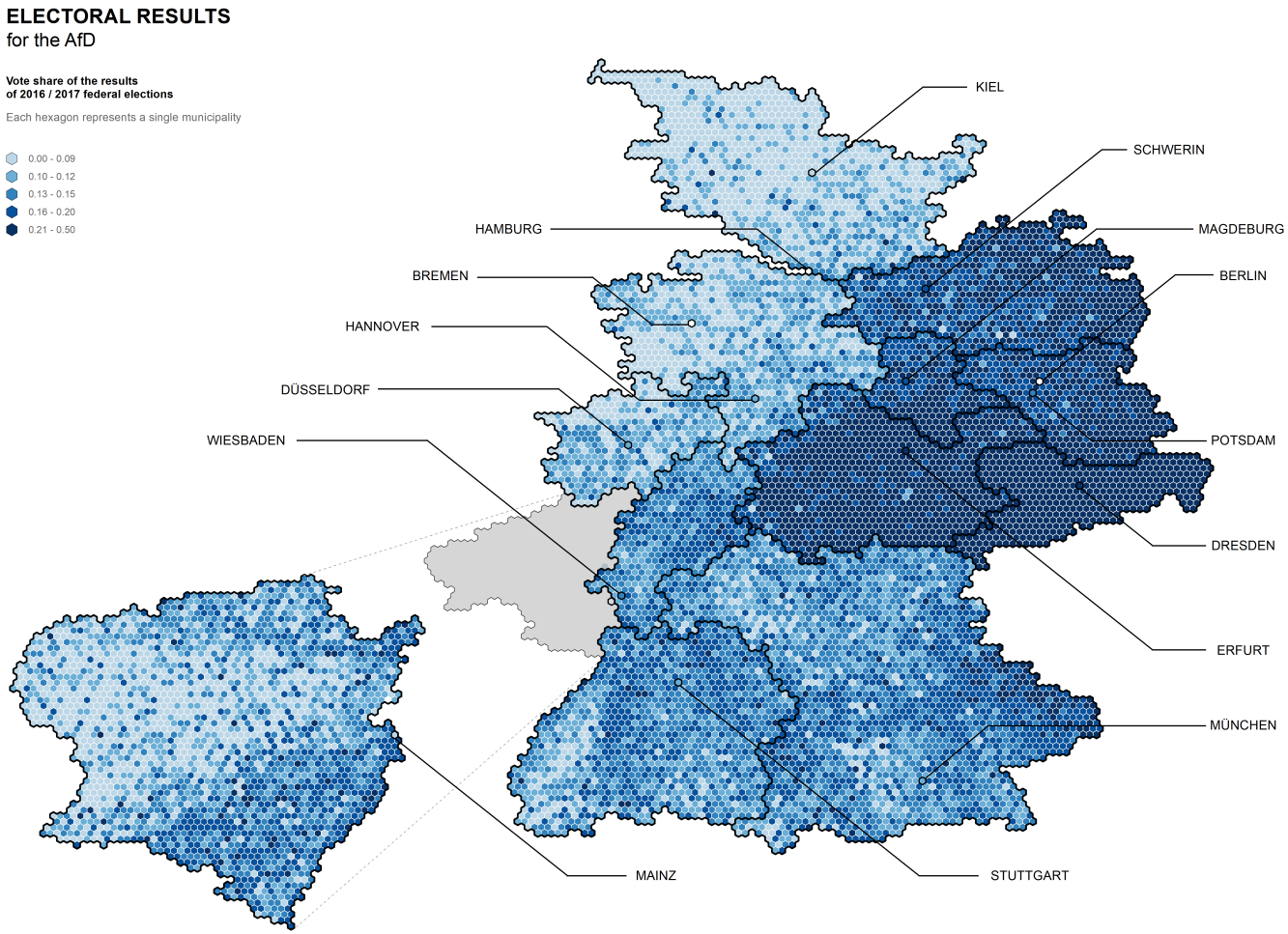
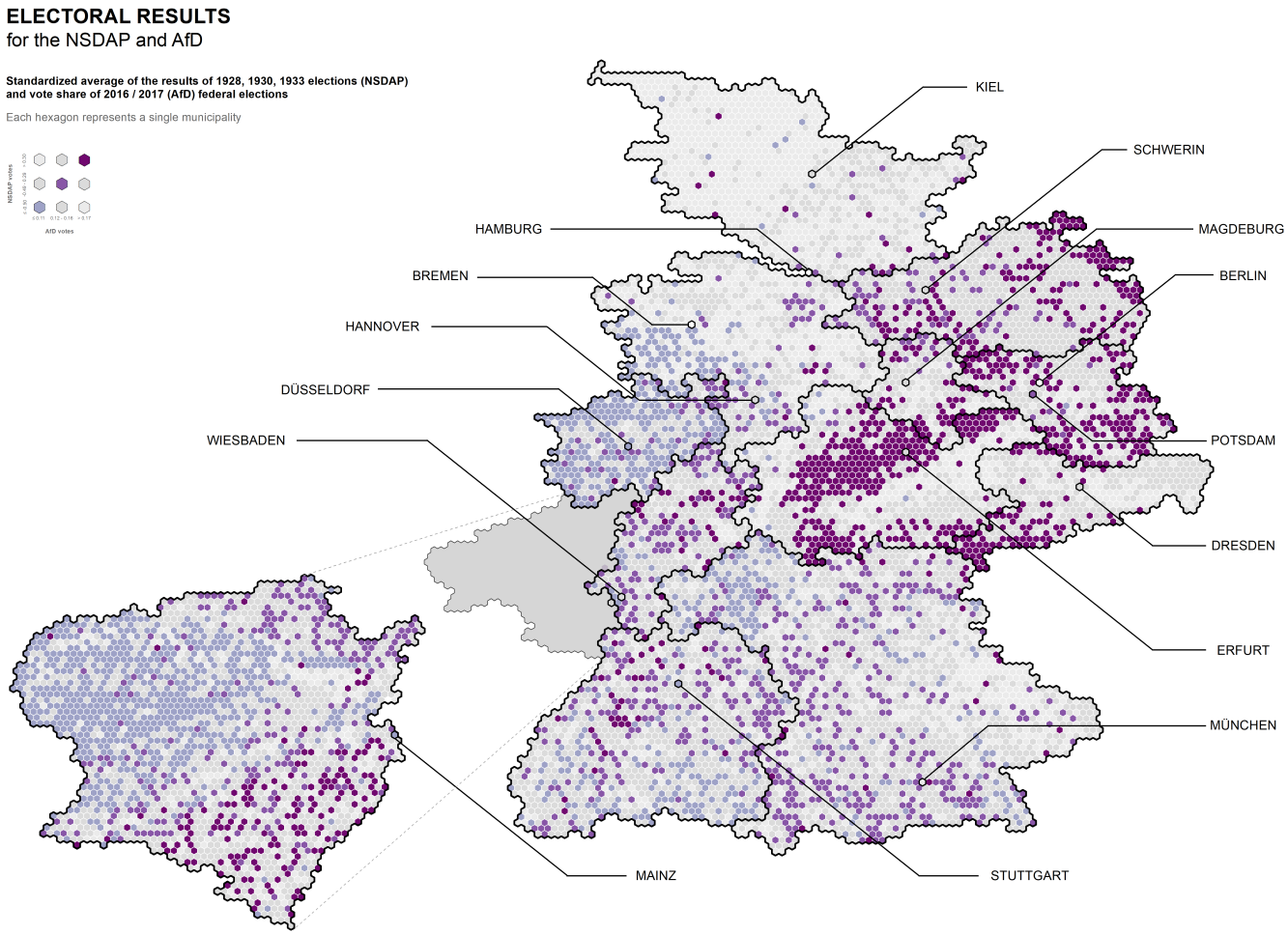


Figure B3: Persistence



1.8.3 *Evidence on Semantic Change*

Language data sources

We analyze the language used by the AfD and other major German parties by considering a variety of sources. Besides the AfD, we consider the following parties: CDU/CSU[44] (Christian democratic, moderately conservative), SPD (social democratic, moderately left-wing), Grüne (green party), FDP (free democrats, economic/socially liberal), and the NPD (nationalistic, starkly right-wing, only represented in a few state legislatures).

First, we look at all party manifestos, official documents setting out the parties political platforms in advance of major elections (federal, state, and European Parliament elections), published from 2013 onwards. These manifestos are usually published a few months ahead of the election, and contain variously detailed statements of political objectives and policy proposals. We obtained the full text (as PDF) of 74 manifestos from the respective party websites; the median manifesto is 56 pages long and encompasses approximately 19,500 words. Table C5 provides an overview of the manifestos used.

Second, we consider the content of major political speeches held at party congresses, at national-level party meetings (e.g., the traditional Epiphany meeting of the FDP on 6 January), or so-called "Ash Wednesday" speeches[45] by major political leaders (usually the party secretaries or the main candidates), from 2013 until today. If the speeches are not available in a transcribed version, we resort to online videos of these speeches and transcribe them with speech recognition software or manually. Our final dataset contains 112 speeches; the median length of a speech is 27 minutes.

Third, we analyze tweets posted from the official Twitter accounts of those six major parties (we restrict ourselves to the main/national account of the party, not of its regional branches and candidates). We scrape all tweets from April 2008 (when the first party, the CDU, opened a twitter account) until the end of June 2017, obtaining a total of 66,422 tweets (the most prolific party is the NPD, with 18,057 tweets, followed by the SPD, with 10,580 tweets; the AFD posted 4,119 tweets). Table C6 provides an overview of timing and quantity of tweets for each party.

Finally, we also scrape posts from the official Facebook pages of the major parties (again restricting ourselves to the federal-level party organization, not to its local branches). We obtain 36,089 posts from November 2008 until May 2017; 12,794 of these posts pertain to the NPD page, 2,881 to the AFD. Table C7 provides an overview of the Facebook posts included.

44. For speeches and party manifestos, we consider the CDU and the CSU as one party (among other reasons, because of the low number of observations). For tweets and Facebook posts, we look at the CDU and the CSU accounts separately.

45. On Ash Wednesday, all major political parties in Germany hold speeches, often in beer halls, which are typically more polemical and more directly targeted against opponents.

Analysis

Figure C3 gives a first quantitative impression of the nationalistic turn imparted on the AfD starting in mid-2015. We classify Facebook posts (looking at trimmed word stems) depending on whether they contain a word that is related to the Euro, to Greece (likely in the context of the bailout talks), to Islam/Muslims, or to Germany/the nation. Up until 2015, about 20% of posts refer, on average, to the Euro, and approximately the same amount refer to Germany/the nation. There is, however, already a slight downward trend in references to the Euro before 2015, which suggests that, as the base expanded, the party's outlook widened beyond its initial narrow focus on economic topics. 2015 witnesses two major changes. First, as the Greek crisis approached a new zenith (the infamous "bailout" referendum was held on July 5), Greece and the Euro reach a short-lived peak in frequencies. At the same time, after the party congress in Essen, the AfD turns rightward: posts referring to Germany or the nation steadily increase in frequency, and so do posts referring to Islam or the Muslim world. Note that the latter change only occurs in mid-2016, well after the peak of the refugee crisis in September 2015.

However, these suggestive trends may also be misleading, and merely capture an overall change in topics relevant to German politics. It is plausible that other parties in Germany, in the context of the dramatic political and economic crises of the past years, have readjusted their rhetoric and the focus of their policy proposals. For this purpose, in Table C1 we look at the overall text body that we collected in manifestos, speeches, tweets, and Facebook posts, for seven major parties in Germany: the AfD, as well as the CDU/CSU, SPD, Greens, FDP, Linke, and (as a benchmark of a more radical, right-wing party) the ultra-nationalist NPD. With this body of data we can estimate a full differences-in-differences specification as follows:

$$f(\text{stem} = s)_{ipt} = \gamma_p + \delta_t + \beta \cdot \mathbb{1}\{\text{party} = \text{AfD}\} \cdot \text{Post}_t + \varepsilon_{ipt}, \tag{1.5}$$

where the dependent variable $f(\text{stem} = s)$ is the frequency (mention per 100 words) of stem s in document i (party manifesto, speech), of party p at time t. For shorter pieces of text (tweets, Facebook post), we use the following variant specification:

$$\mathbb{1}\{(\text{stem} = s) \in i\}_{ipt} = \gamma_p + \delta_t + \beta \cdot \mathbb{1}\{\text{party} = \text{AfD}\} \cdot \text{Post}_t + \varepsilon_{ipt}, \tag{1.6}$$

where $\mathbb{1}\{(\text{stem} = s) \in i\}$ is a dummy indicating whether stem s is contained in document i (tweet, post) of party p at time t. In all specifications, we include a full set of party fixed effects (γ_p) and time fixed effects (δ_t): these are year fixed effects for speeches and manifestos, and month×year fixed effects for tweets and Facebook posts. Post_t is a dummy for all periods after the Erfurt Resolution (March 2015). Standard errors ε_{ipt} are clustered at the party×year cell level (for manifestos and speeches) or at the party×year×month level (for tweets and Facebook posts).

The crucial difference-in-differences parameter of interest is β, indicating the increase in fre-

quency (or mentions) of a given stem in documents of the AfD, after the Erfurt Resolution, conditional on state and time fixed effects. Table C1 reports the estimates of β across four media (manifestos, speeches, tweets, and Facebook posts, in Panels A through D, respectively), and for five outcome stems of interest: Greece, the Euro, Islam, migration, and nation.[46] Every cell in that table reports the estimate of the difference-in-differences parameter for one regression, defined by a dyad of medium and stem.

Across all text media, we see consistent results. Even when viewed in relation to the language used by the other political parties in Germany, the AfD notably reduces the mentions of Greece and the Euro in its rhetoric, and increases the usage of words related to Islam, to migration, and to Germany/the nation. For example, the estimate in panel B, column 2, suggests that after 2015, the reduction of mentions of stems relating to the Euro in speeches by AfD members is 0.546 per 100 words (significant at <1% level). This compares to a mean of the dependent variable of 0.703 (for AfD speeches, before 2015); it is thus a very sizable decrease.[47]

By converse, the estimate in panel D, column 4, suggests that after March 2015, the share of Facebook posts mentioning a stem related to the migration context increases by 11.2 percentage points (significant at <1% level). Again, this is sizable if compared to a pre-March 2015 mean of the outcome variable of 5.8 percent (for the AfD).

Arguably, the five word stems shown in Table C1 have been arbitrarily chosen, based on our priors regarding which words should witness the starkest changes following the rightward turn imparted on the AfD after the Erfurt Resolution. To avoid our subjective bias, and to validate the stems chosen in Table C1, in Figure C4 we follow a different approach. Here, we repeat the standard differences-in-differences estimations of Equation 1.5 above, applying this regression setup to each of the 645 most frequent word stems that we identified in our entire body of Facebook posts.[48]

Figure C4 presents the distribution of the β coefficients estimated from Equation 1.5, across 645 stems. First, it is noticeable that the distribution of point estimates is skewed to the right of zero: this indicates that the language used by the AfD, after March 2015, becomes more varied. Second, vertical dashed lines in the figure show the positioning of the point estimates relating to key words used so far. Confirming the results of Table C1, we see that "Euro" and "Greece" are to the left of zero, whereas the usage of words such as "Islam", "asylum", and especially "Germany" increases dramatically for the AfD after March 2015, relative to other parties. Third, it is also noticeable that a traditional mainstay of conservative political ideology, the "family", does not move into the

46. More precisely, the stem "Greece" encompasses all German words including **griech**; "Euro" all words that start with *euro**, but not *europ**, and also the acronym *EZB* (European Central Bank, in German); the stem "Islam" all words including **islam** and **muslim**; the stem "migration" all words including **migration**, **wander**, **flüchtling**, and **asyl**; the stem "nation" all words including **nation** and **deutsch**. Table C4 reports the 10 most frequent words identified by this algorithm for each stem.

47. Table C2 provides (conditional) means for all dependent variables.

48. To be more precise, we consider the universe of words in the body of Facebook posts we collected. We remove numbers, punctuation, and stopwords, and then stem the resulting words using the *tm* package for R. We keep all stems that are used at least 200 times. This results in 645 word stems.

focus of the AfD's rhetoric: the point estimate is very close to zero. We see this as suggestive of the fact that the post-March 2015 turn experienced by the AfD was explicitly nationalistic and xenophobic (anti-Muslim), not merely conservative.

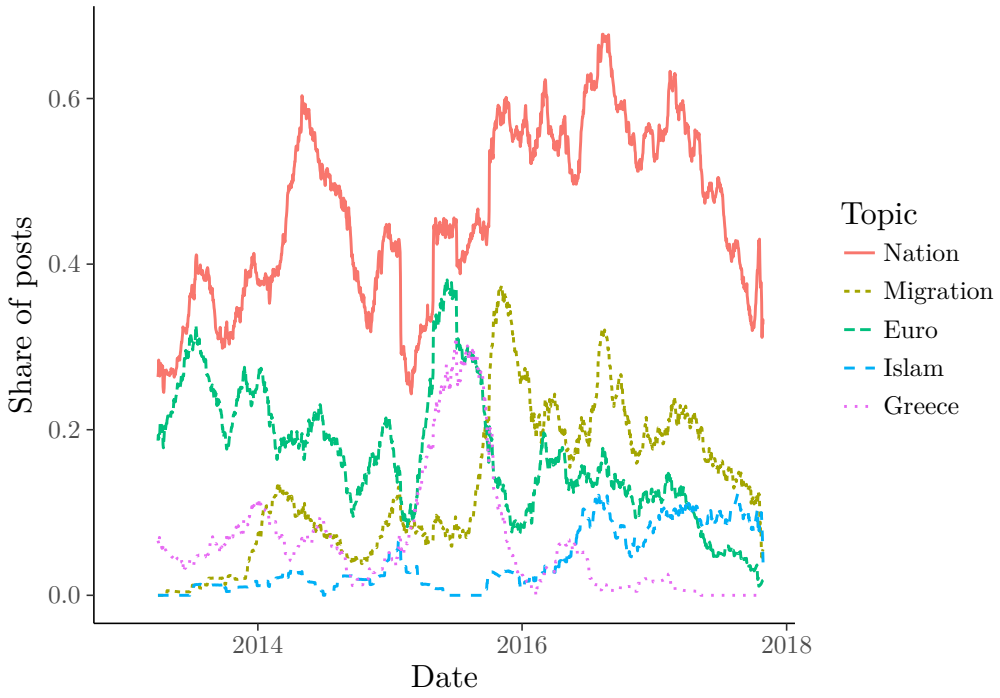
Figure C1: AfD Electoral Poster, 2013

Notes: Electoral poster for the federal election of September 2013. It reads: "Greeks are desperate. Germans are paying. Banks are cashing in. Stop this."

Figure C2: AfD Electoral Poster, 2016

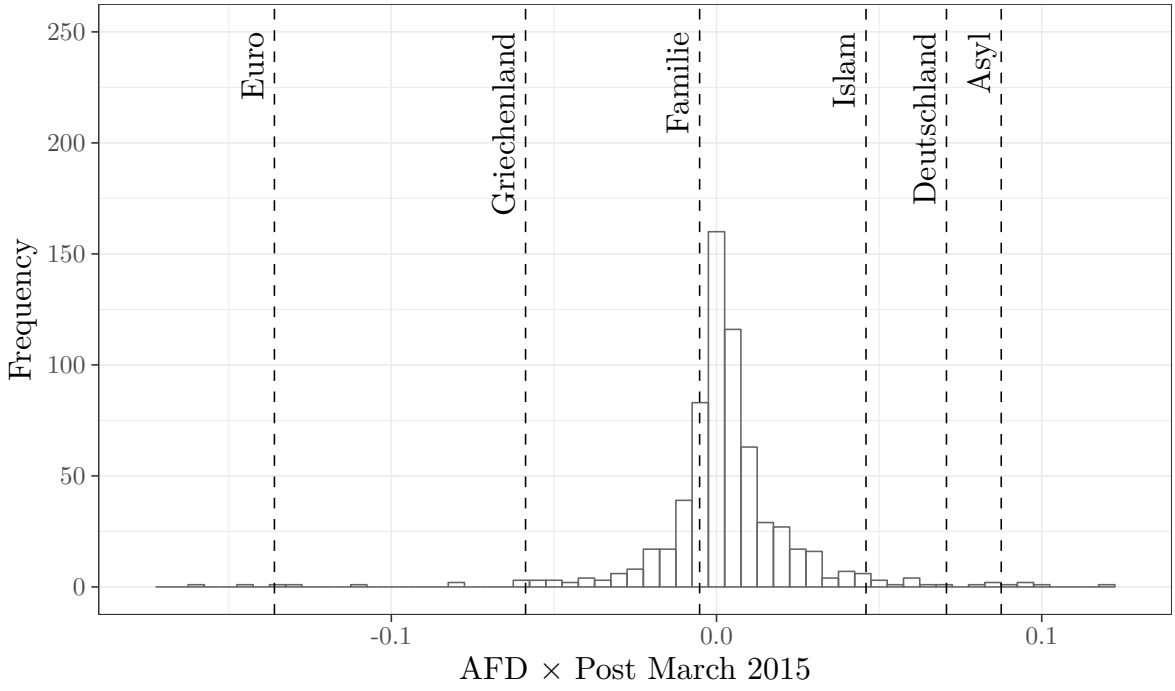
Notes: Electoral poster for the state election in Baden-Württemberg in March 2016. It reads: "For our state – for our values. Immigration needs clear rules."

Figure C3: Language Use on AfD's Facebook Page: Selected Stems



Notes: The graph shows the frequency of Facebook posts containing one of four, selected word stems/families. 90-day moving averages displayed.

Figure C4: Language Use on AfD's Facebook Page: All Stems



Notes: The graph shows the empirical distribution of estimated difference-in-difference coefficients, resulting from the empirical setup in equation (1.6), relating to 645 frequent word stems on Facebook, together with the location of six selected word stems.

Table C1: AfD's Language Change: Diff-in-diff Estimates

	(1) Greece	(2) Euro	(3) Islam	(4) Migration	(5) Nation
Panel A: *Mentions per 100 words in manifestos*					
AfD × After March 2015	-0.011	-0.780***	0.052***	0.269***	-0.041
	(0.021)	(0.193)	(0.013)	(0.050)	(0.237)
Panel B: *Mentions per 100 words in speeches*					
AfD × After March 2015	-0.183**	-0.546***	0.063*	-0.028	0.112
	(0.070)	(0.099)	(0.034)	(0.097)	(0.100)
Panel C: *Mentioned in Twitter posts*					
AfD × After March 2015	-0.059***	-0.157***	0.020**	0.023**	-0.098***
	(0.009)	(0.011)	(0.009)	(0.012)	(0.018)
Panel D: *Mentioned in Facebook posts*					
AfD × After March 2015	-0.017	-0.055***	0.042***	0.112***	0.209***
	(0.016)	(0.021)	(0.011)	(0.023)	(0.030)

Notes: Coefficients and standard errors (in brackets) from OLS regressions. In panel A the unit of observation is a manifesto, in panel B a speech, in panel C a Twitter post and in panel D a Facebook post. All regressions include party (AFD, CDU, CSU, FDP, Grüne, Die Linke, NPD, SPD) fixed effects. Panels A and B include year fixed effects, panels C and D month fixed effects. Number of observations: 70 (panel A), 113 (panel B), 66,422 (panel C) and 40,118 (panel D). One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table C2: AfD's Language Change: Means of Dependent Variables (Table C1)

	(1) Greece	(2) Euro	(3) Islam	(4) Migration	(5) Nation
Panel A: *Mentions per 100 words in manifestos*					
Mean (overall)	0.005	0.140	0.030	0.264	0.610
Mean (all parties, pre-March 2015)	0.013	0.292	0.013	0.170	1.028
Mean (AfD)	0.015	0.401	0.046	0.405	0.963
Mean (AfD, pre-March 2015)	0.031	1.043	0.000	0.199	1.406
Panel B: *Mentions per 100 words in speeches*					
Mean (overall)	0.055	0.145	0.033	0.126	0.556
Mean (all parties, pre-March 2015)	0.088	0.235	0.023	0.074	0.498
Mean (AfD)	0.102	0.367	0.028	0.128	0.789
Mean (AfD, pre-March 2015)	0.216	0.703	0.000	0.105	0.697
Panel C: *Mentioned in Twitter posts*					
Mean (overall)	0.011	0.021	0.010	0.043	0.086
Mean (all parties, pre-March 2015)	0.012	0.027	0.009	0.022	0.089
Mean (AfD)	0.053	0.133	0.009	0.027	0.113
Mean (AfD, pre-March 2015)	0.069	0.175	0.002	0.016	0.133
Panel D: *Mentioned in Facebook posts*					
Mean (overall)	0.019	0.055	0.024	0.088	0.230
Mean (all parties, pre-March 2015)	0.017	0.059	0.017	0.040	0.200
Mean (AfD)	0.064	0.184	0.044	0.166	0.371
Mean (AfD, pre-March 2015)	0.068	0.214	0.015	0.058	0.231

Notes: Table reports means for five groups of words. These are the dependent variables in the diff-in-diff regressions of Table C1. Overall means (first row in each panel) and conditional means reported.

Table C3: AfD's Language Change: Diff-in-diff Estimates
Negative Binomial Regression

	(1) Greece	(2) Euro	(3) Islam	(4) Migration	(5) Nation
Panel A: Mentions per 100 words in manifestos					
AfD × After March 2015	1.781	-0.882	17.266	0.850	0.429
	(8.309)	(1.393)	(10998.819)	(1.563)	(0.721)
Panel B: Mentions per 100 words in Speeches					
AfD × After March 2015	-16.738	-1.517	16.537	0.475	0.119
	(3485.886)	(1.544)	(3176.458)	(1.976)	(0.643)
Panel C: Mentions in Facebook Posts					
AfD × After March 2015	-0.712***	-0.305***	0.640***	0.556***	0.003
	(0.161)	(0.108)	(0.239)	(0.128)	(0.059)
Panel D: Mentions in Tweets					
AfD × After March 2015	-2.014***	-2.228***	2.571***	0.763***	-0.046
	(0.376)	(0.189)	(0.473)	(0.168)	(0.069)

Notes: Coefficients and standard errors (in brackets) from negative binomial regressions. In panel A the unit of observation is a manifesto, in panel B a speech, in panel C a Twitter post and in panel D a Facebook post. All regressions include party (AFD, CDU, CSU, FDP, Grüne, Die Linke, NPD, SPD) fixed effects. Panels A and B include year fixed effects, panels C and D month fixed effects. Number of observations: 70 (panel A), 113 (panel B), 66,422 (panel C) and 40,118 (panel D). One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table C4: Most Frequent Words (Stems in Table C1)

Greece	Euro	Islam	Migration	Nation
griechenland	euro	islamischen	zuwanderung	deutschlands
griechische	eurokrise	muslime	flüchtlingen	deutsche
griechischen	euroraum	islam	asyl	deutscher
griechen	ezb	islamistische	migration	deutsch
griechenlands	eurostaaten	islamistischen	einwanderung	nationale
grieche	eurozone	islamische	flüchtlingspolitik	national
griechisch	eurorettung	islamisten	asylverfahren	nationalen
griechenlandanleihen	euros	islamischer	zuwanderer	deutschen
griechischer	eurobonds	muslimen	asylsuchende	deutsches
griechenlandkrise	eurojust	muslimischen	einwanderer	nationaler

Table C5: List of Political Manifestos

Year	Manifesto type	Party	# of words	# of pages
2013	Federal election	AfD	3923	12
2013	Federal election	CDU/CSU	41367	81
2013	Federal election	FDP	38040	104
2013	Federal election	Grüne	86557	337
2013	Federal election	Linke	39011	100
2013	Federal election	NPD	3585	52
2013	Federal election	SPD	41003	120
2014	European Parliament election	AfD	8974	25
2014	European Parliament election	CDU/CSU	22020	84
2014	European Parliament election	FDP	10778	28
2014	European Parliament election	Grüne	22223	57
2014	European Parliament election	Linke	12971	76
2014	European Parliament election	SPD	6383	14
2014	Party platform	AfD	3143	14
2015	Resolution	FDP	6520	13
2015	Erfurter Resolution	AfD	630	3
2016	State election, Baden-Württemberg	AfD	19474	64
2016	State election, Baden-Württemberg	CDU/CSU	33658	156
2016	State election, Baden-Württemberg	FDP	20213	63
2016	State election, Baden-Württemberg	Grüne	50632	249
2016	State election, Baden-Württemberg	Linke	25084	44
2016	State election, Baden-Württemberg	NPD	6310	26
2016	State election, Baden-Württemberg	SPD	25232	41
2016	Guidelines	CDU/CSU	18117	47
2016	Guidelines	AfD	23846	96
2016	Guidelines	AfD	21892	78
2016	State election, Mecklenburg-A. Pomerania	AfD	6744	22
2016	State election, Mecklenburg-A. Pomerania	CDU/CSU	8464	27
2016	State election, Mecklenburg-A. Pomerania	FDP	21439	86
2016	State election, Mecklenburg-A. Pomerania	Grüne	21702	28
2016	State election, Mecklenburg-A. Pomerania	Linke	22124	54
2016	State election, Mecklenburg-A. Pomerania	NPD	2978	7
2016	State election, Mecklenburg-A. Pomerania	SPD	17426	48

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Year	Manifesto type	Party	# of words	# of pages
2016	State election, Rhineland-Palatinate	AfD	7784	17
2016	State election, Rhineland-Palatinate	CDU/CSU	1815	8
2016	State election, Rhineland-Palatinate	FDP	31497	83
2016	State election, Rhineland-Palatinate	Grüne	39389	57
2016	State election, Rhineland-Palatinate	Linke	18830	57
2016	State election, Rhineland-Palatinate	NPD	1506	3
2016	State election, Rhineland-Palatinate	SPD	16737	56
2016	State election, Saxony-Anhalt	AfD	4867	68
2016	State election, Saxony-Anhalt	CDU/CSU	19715	64
2016	State election, Saxony-Anhalt	FDP	2705	12
2016	State election, Saxony-Anhalt	Grüne	27260	76
2016	State election, Saxony-Anhalt	Linke	19222	44
2016	State election, Saxony-Anhalt	NPD	1750	6
2016	State election, Saxony-Anhalt	SPD	19070	53
2017	State election, North Rhine-Westfalia	AfD	12091	39
2017	State election, North Rhine-Westfalia	CDU/CSU	38115	120
2017	State election, North Rhine-Westfalia	FDP	24188	49
2017	State election, North Rhine-Westfalia	Grüne	82836	131
2017	State election, North Rhine-Westfalia	Linke	44709	132
2017	State election, North Rhine-Westfalia	NPD	8182	52
2017	State election, North Rhine-Westfalia	SPD	38163	116
2017	State election, Schleswig-Holstein	AfD	18053	56
2017	State election, Schleswig-Holstein	FDP	28952	117
2017	State election, Schleswig-Holstein	Grüne	34612	94
2017	State election, Schleswig-Holstein	CDU/CSU	23827	96
2017	State election, Schleswig-Holstein	Linke	24669	70
2017	State election, Schleswig-Holstein	SPD	21670	66
2017	State election, Saarland	AfD	9654	43
2017	State election, Saarland	CDU/CSU	25816	72
2017	State election, Saarland	FDP	6462	19
2017	State election, Saarland	Grüne	23263	70
2017	State election, Saarland	Linke	15468	34
2017	State election, Saarland	NPD	1895	8
2017	State election, Saarland	SPD	19303	52

Table C6: Number of Tweets

Month	AfD	CDU	CSU	FDP	Grüne	Linke	NPD	SPD
2008–04					3			
2008–05					28			
2008–06					29			
2008–07					12			
2008–08					77			
2008–09					52			
2008–10					47			
2008–11					102			
2008–12					27			
2009–01					139			
2009–02		14			51			
2009–03		38	30		48			22
2009–04		25	79		54		18	78
2009–05		53	74	1	150		86	105
2009–06		84	72		307	191	47	94
2009–07		30	88		129		28	77
2009–08		27	49		88		32	168
2009–09		118	69		329	373	34	201
2009–10		21	22		53		18	25
2009–11		23	17		36		21	60
2009–12		13	15		21	1	11	32
2010–01		25	17		27	1	20	47
2010–02		26	26		36	12	45	55
2010–03		27	23		50	10	63	46
2010–04		20	19		53	1	46	52
2010–05		25	23	35	56	156	66	38
2010–06		34	28	61	77	11	67	55
2010–07		30	19	45	27	10	56	44
2010–08		23	27	44	26	3	98	22
2010–09		38	17	71	56	7	110	130
2010–10		36	42	70	49	10	140	153
2010–11		63	22	54	75	21	153	173

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Month	AfD	CDU	CSU	FDP	Grüne	Linke	NPD	SPD
2010–12		25	17	43	29	4	136	151
2011–01		46	29	90	43	28	199	123
2011–02		35	42	111	70	16	179	146
2011–03		50	69	52	77	26	200	155
2011–04		25	28	39	34		151	73
2011–05		50	67	133	63	1	154	171
2011–06		36	42	26	78	3	163	118
2011–07		26	31	27	41		170	118
2011–08		31	30	24	27		208	128
2011–09		45	58	22	57	5	222	179
2011–10		43	46	22	58	271	222	133
2011–11		154	23	50	67	18	235	91
2011–12		27	26	17	21	16	309	205
2012–01		39	52	10	31	38	398	137
2012–02		28	77	3	49	37	461	103
2012–03		32	93	16	71	48	346	171
2012–04		24	27	18	54	29	268	101
2012–05		71	102	12	108	34	274	99
2012–06		40	93	8	118	286	232	81
2012–07		31	54	6	72	22	313	71
2012–08		33	33	12	33	39	167	78
2012–09		86	40	17	137	27	270	83
2012–10	137	37	150	24	61	13	184	171
2012–11	263	21	36	21	140	92	176	147
2012–12	159	108	32	8	18	18	243	125
2013–01	109	82	23	16	68	65	237	116
2013–02	105	21	43	14	60	139	184	45
2013–03	266	18	59	53	88	46	275	72
2013–04	133	15	29	28	444	19	273	93
2013–05	200	74	49	56	96	30	260	132
2013–06	73	115	30	15	148	281	222	123
2013–07	76	48	50	21	144	46	174	37
2013–08	172	42	30	43	158	92	311	173
2013–09	189	305	127	95	494	187	406	306

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Month	AfD	CDU	CSU	FDP	Grüne	Linke	NPD	SPD
2013–10	111	17	1	9	222	21	239	42
2013–11	126	20	10	19	58	2	212	73
2013–12	84	50	1	126	25		223	40
2014–01	86	37	2	98	90	2	263	55
2014–02	73	29	1	9	247	365	207	32
2014–03	76	70	32	4	53	47	283	84
2014–04	90	113	33	3	59	44	269	37
2014–05	94	206	36	127	193	234	273	184
2014–06	80	29	20	2	23	29	165	48
2014–07	67	44	33	10	14	24	235	45
2014–08	78	47	28	16	22	115	211	33
2014–09	66	140	83	4	30	100	194	108
2014–10	49	155	143	6	29	18	171	163
2014–11	29	139	121	10	129	9	220	117
2014–12	12	230	162	20	24	6	187	97
2015–01	14	67	131	26	19	47	246	107
2015–02	10	40	124	35	39	19	184	210
2015–03	23	36	148	38	23	54	174	105
2015–04	1	21	134	39	56	212	163	108
2015–05	4	44	103	249	37	79	138	109
2015–06	5	107	119	74	71	507	206	108
2015–07	3	91	177	30	35	156	151	41
2015–08	6	51	73	56	16	40	149	79
2015–09	12	125	119	53	42	52	206	107
2015–10	13	99	172	44	56	54	230	164
2015–11	5	53	184	62	135	56	263	72
2015–12	2	211	85	46	15	42	151	94
2016–01	3	48	121	55	22	86	175	63
2016–02	5	70	105	47	21	133	217	84
2016–03	4	109	89	83	38	263	227	131
2016–04	4	36	127	252	17	98	228	71
2016–05	2	56	128	63	24	331	164	125
2016–06	1	99	95	53	52	134	180	134
2016–07	1	65	161	32	33	83	198	92

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Month	AfD	CDU	CSU	FDP	Grüne	Linke	NPD	SPD
2016–08	14	63	50	20	12	51	191	83
2016–09	9	114	148	52	53	104	138	237
2016–10	7	60	174	99	87	46	129	126
2016–11	13	90	249	85	108	81	145	140
2016–12	11	99	99	84	17	107	160	142
2017–01	6	94	115	86	60	127	215	169
2017–02	13	86	104	66	34	77	142	124
2017–03	7	149	179	60	88	168	183	156
2017–04	120	115	94	144	64	53	144	84
2017–05	508	224	190	80	77	163	150	147
2017–06	295	108	88	66	162	394	133	173
2017–07	340	121	218	101	80	105	161	125
2017–08	329	382	210	162	174	184	139	276
2017–09	302	963	445	760	484	468	182	700

Table C7: Number of Facebook Posts

Month	AfD	CDU	CSU	FDP	Grüne	Linke	NPD	SPD
2008–11					2			
2008–12					1			
2009–01					1	26		
2009–02					4	14		4
2009–03				16		11		12
2009–04				14	15	12		24
2009–05			4	30	42	18		19
2009–06			1	12	33	14		15
2009–07			2	6	22	8		12
2009–08			4	22	23	9		55
2009–09		26	10	34	56	105		46
2009–10		21	1	22	13			8
2009–11		25	5	7	12	2		36
2009–12		20	2	3	9	5		31

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Month	AfD	CDU	CSU	FDP	Grüne	Linke	NPD	SPD
2010–01		21	2	10	13	5		48
2010–02		16	25	10	8	29	9	61
2010–03		12	12	14	26	18	50	69
2010–04		10	13	8	26	6	27	72
2010–05		11	12	13	27	164	51	53
2010–06		15	10	18	32	19	53	60
2010–07		15	11	15	11	10	47	49
2010–08		24	12	12	9	8	69	26
2010–09		35	9	13	31	23	92	44
2010–10		36	37	19	29	14	124	66
2010–11		65	10	16	32	23	145	61
2010–12		24	12	13	16	3	123	51
2011–01		55	12	19	25	25	177	48
2011–02		39	18	21	51	21	158	64
2011–03		57	28	17	77	39	174	102
2011–04		25	3	24	37	9	124	53
2011–05		52	16	42	63	18	112	121
2011–06		40	11	16	63	14	132	86
2011–07		29	6	18	42	35	146	82
2011–08		23	7	19	27	62	152	81
2011–09		53	28	22	54	14	163	98
2011–10		47	32	32	45	61	182	124
2011–11		64	41	42	59	26	164	69
2011–12		26	32	40	24	24	182	95
2012–01		30	23	46	33	34	263	103
2012–02		28	29	39	34	34	293	99
2012–03		35	55	47	54	46	197	124
2012–04		32	18	40	31	20	195	85
2012–05		40	19	29	51	32	231	95
2012–06		38	34	32	52	242	165	82
2012–07		27	43	31	44	30	202	68
2012–08		32	6	29	37	23	108	77
2012–09		39	12	33	55	19	177	83
2012–10		41	21	38	57	14	155	138

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Month	AfD	CDU	CSU	FDP	Grüne	Linke	NPD	SPD
2012–11		29	18	35	66	30	102	146
2012–12		42	18	24	26	19	170	77
2013–01		43	39	43	46	33	177	109
2013–02		18	35	30	47	28	139	63
2013–03	91	23	20	56	68	32	180	43
2013–04	89	19	22	32	70	29	197	37
2013–05	66	51	12	49	74	27	168	63
2013–06	83	50	33	36	81	168	139	68
2013–07	88	45	40	50	82	36	113	48
2013–08	74	49	34	60	95	49	222	85
2013–09	78	129	41	76	146	70	265	204
2013–10	60	16	2	39	39	15	154	27
2013–11	62	26	10	39	58	18	129	52
2013–12	57	38	2	59	30	14	132	45
2014–01	63	30	3	66	67	12	156	23
2014–02	67	29	6	49	35	80	133	48
2014–03	57	39	43	61	38	18	186	101
2014–04	59	52	29	46	22	22	169	80
2014–05	76	88	43	71	32	66	182	150
2014–06	62	27	12	44	14	13	131	31
2014–07	51	30	21	55	9	21	177	39
2014–08	61	36	18	65	14	23	202	52
2014–09	52	59	46	56	16	52	141	71
2014–10	46	55	45	56	14	26	131	76
2014–11	38	48	47	50	18	23	163	81
2014–12	36	65	55	50	14	18	153	68
2015–01	68	54	56	54	17	47	174	70
2015–02	43	31	61	52	16	19	130	93
2015–03	39	29	62	61	20	38	139	69
2015–04	43	23	60	70	19	34	132	71
2015–05	43	21	60	79	18	27	115	68
2015–06	53	46	74	54	20	59	153	63
2015–07	65	36	69	57	10	31	117	52
2015–08	37	21	56	59	7	20	113	58

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Month	AfD	CDU	CSU	FDP	Grüne	Linke	NPD	SPD
2015–09	39	33	75	68	13	38	179	70
2015–10	55	46	66	53	12	26	185	69
2015–11	48	29	66	57	20	23	220	41
2015–12	26	39	50	59	14	15	121	44
2016–01	43	28	59	67	16	25	145	39
2016–02	44	30	53	61	11	30	163	63
2016–03	57	34	55	68	21	39	165	77
2016–04	47	35	48	90	7	27	184	48
2016–05	45	28	59	59	14	40	109	59
2016–06	50	54	55	57	18	34	117	56
2016–07	57	27	72	68	10	28	127	55
2016–08	59	30	48	68	11	29	125	45
2016–09	67	38	64	66	26	31	88	54
2016–10	74	30	52	65	23	21	97	49
2016–11	65	27	74	69	28	23	101	46
2016–12	50	29	71	70	25	27	111	48
2017–01	48	43	60	64	32	27	188	64
2017–02	58	44	59	72	35	34	126	39
2017–03	73	55	73	79	44	41	155	54
2017–04	74	47	59	80	32	26	106	47
2017–05	87	53	78	79	37	32	109	53
2017–06	79	50	61	67	54	44	118	64
2017–06	1							
2017–07	4	2		1		3		2
2017–08	12	14	6	11	8	6	4	12
2017–09	21	18	9	16	18	13	3	21

Notes: After 2017–06, the number of facebook posts goes down because Facebook limited ability to scrape page histories.

1.8.4 *Additional Results, Electoral Analysis*

In this section we conduct additional analyses regarding our electoral results. In Table D1, we compare our finding to the correlations between AfD vote shares in 2017 and four other variables: unemployment levels, the change in unemployment level from before the great recession until today, the increase in trade exposure (the difference between import competition and export competition), and the allocation of refugees in municipalities. These correlations are presented in columns 1–4 of Table D1 in terms of standardized beta coefficients; it is important to emphasize that these coefficients should not be seen as *causal* estimates of the effects, as clearly there is no claim to the exogeneity of the spatial variation of these variables with respect to AfD support.

For three variables — unemployment change, trade exposure, and allocation of refugees — we find a negative point estimate: surprisingly, AfD support is higher in regions where unemployment or trade exposure *decreased* in the last decade, or where there is a lower share of refugees. In the case of unemployment levels (column 1), the correlation is positive, as one would have plausibly expected. The beta coefficients in Table D1 are in a similar range of magnitude (2–10% in absolute terms) as the beta coefficient relating to the NSDAP vote share (5.6%, reported in column 5 for reference). Our magnitudes are also comparable to the effect of import competition on far right voting in France in 2007–2012, estimated at 8.98% as a standardized beta coefficient (Malgouyres, 2017).

A comparison of the partial R^2 values of these explanatory factors (bottom of Table D1) leads to similar conclusions: the cultural persistence factor "explains" a comparable share of the variance of the outcome (about 0.7%) as the other factors (whose partial R^2 vary between 0.2% and 1.6%). This simple variance decomposition thus suggests that, while historical persistence clearly is only one among many factors associated with the rise of populist right-wing parties, the magnitude of the correlation is comparable to other factors often mentioned in the literature.

Table D2 shows robustness of these results to the use of the differenced dependent variable, AfD vote shares Δ 2017–2013, instead of absolute vote shares in 2017.

In Table D3 we show that results from including controls carry through when the number of observations is held constant throughout all specifications. In Table D4 we show that the patterns investigated through the simple cross-sectional regressions of Figure 1.6 also hold when aggregating the data to a municipality-level panel. We regress far-right electoral shares for all federal election years on the standardized measure of 1933 NSDAP votes, and on the 1933 NSDAP votes interacted with an indicator for the 2017 elections. Note that the inclusion of municipality fixed effects in even-numbered columns is irrelevant, as the interaction term of interest is orthogonal to municipalities.

Table D1: Economic vs. Historic Determinants of AfD 2017 Vote Share

	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment, 2016 [std.]	0.0384***					0.0250**
	(0.0094)					(0.0099)
Unemployment, Δ 2017-2007 [std.]		-0.0983***				-0.0859***
		(0.0153)				(0.0155)
Trade Exposure, Δ 2008-1998 [std.]			-0.0257***			-0.0325***
			(0.0046)			(0.0047)
Share Refugees, 2016 [std.]				-0.0077		-0.0057
				(0.0060)		(0.0064)
NSDAP 1933 [std.]					0.0567***	0.0550***
					(0.0072)	(0.0070)
Partial R^2	0.0026	0.0097	0.0018	0.00018	0.0075	
R^2	0.679	0.682	0.679	0.679	0.681	0.685
Observations	10817	10817	10817	10817	10817	10817

Notes: The dependent variable is the (standarized) vote share for the AfD in 2017, relative to all eligible voters. All reported explanatory variables are also standardized. All regressions include state fixed effects. Sample consists of German municipalities, excluding the Saarland and city states. Standard errors are clustered at the standardized 2017 AfD gross vote shares. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table D2: Economic vs. Historic Determinants of Δ 2017-13 AfD Vote Share

	(1)	(2)	(3)	(4)	(5)	(6)
Unemployment, 2016 [std.]	0.0658***					0.0484***
	(0.0093)					(0.0107)
Unemployment, Δ 2017-2007 [std.]		-0.1463***				-0.1251***
		(0.0137)				(0.0147)
Trade Exposure, Δ 2008-1998 [std.]			-0.0213***			-0.0295***
			(0.0051)			(0.0052)
Share Refugees, 2016 [std.]				-0.0141**		-0.0157**
				(0.0065)		(0.0070)
NSDAP 1933 [std.]					0.0605***	0.0556***
					(0.0063)	(0.0062)
Partial R^2	0.0071	0.020	0.0012	0.00054	0.0079	
R^2	0.657	0.662	0.655	0.655	0.658	0.666
Observations	10817	10817	10817	10817	10817	10817

Notes: The dependent variable is the (standardized) difference of vote share for the AfD between 2017 and 2013, relative to all eligible voters. All reported explanatory variables are also standardized. All regressions contain a full set of state fixed effects. Sample consists of German municipalities, excluding the Saarland and city states. Standard errors are clustered at the standardized 2017 AfD gross vote shares. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table D3: Results Including Controls - Identical Number of Observations

	(1) Baseline	(2) Population	(3) Employment	(4) Education	(5) Religion
Panel A: *With historical controls*					
NSDAP 1933 [std.]	0.0682***	0.0709***	0.0683***	0.0530*	0.0087
	(0.0261)	(0.0263)	(0.0260)	(0.0290)	(0.0243)
Observations	7652	7652	7652	7652	7652
Panel B: *With contemporary controls*					
NSDAP 1933 [std.]	0.0682***	0.0592**	0.0663***	0.0633**	0.0370
	(0.0261)	(0.0280)	(0.0253)	(0.0279)	(0.0254)
Observations	7652	7652	7652	7652	7652
Panel C: *With historical and contemporary controls*					
NSDAP 1933 [std.]	0.0682***	0.0619**	0.0619**	0.0524*	0.0078
	(0.0261)	(0.0281)	(0.0250)	(0.0299)	(0.0263)
Observations	7652	7652	7652	7652	7652

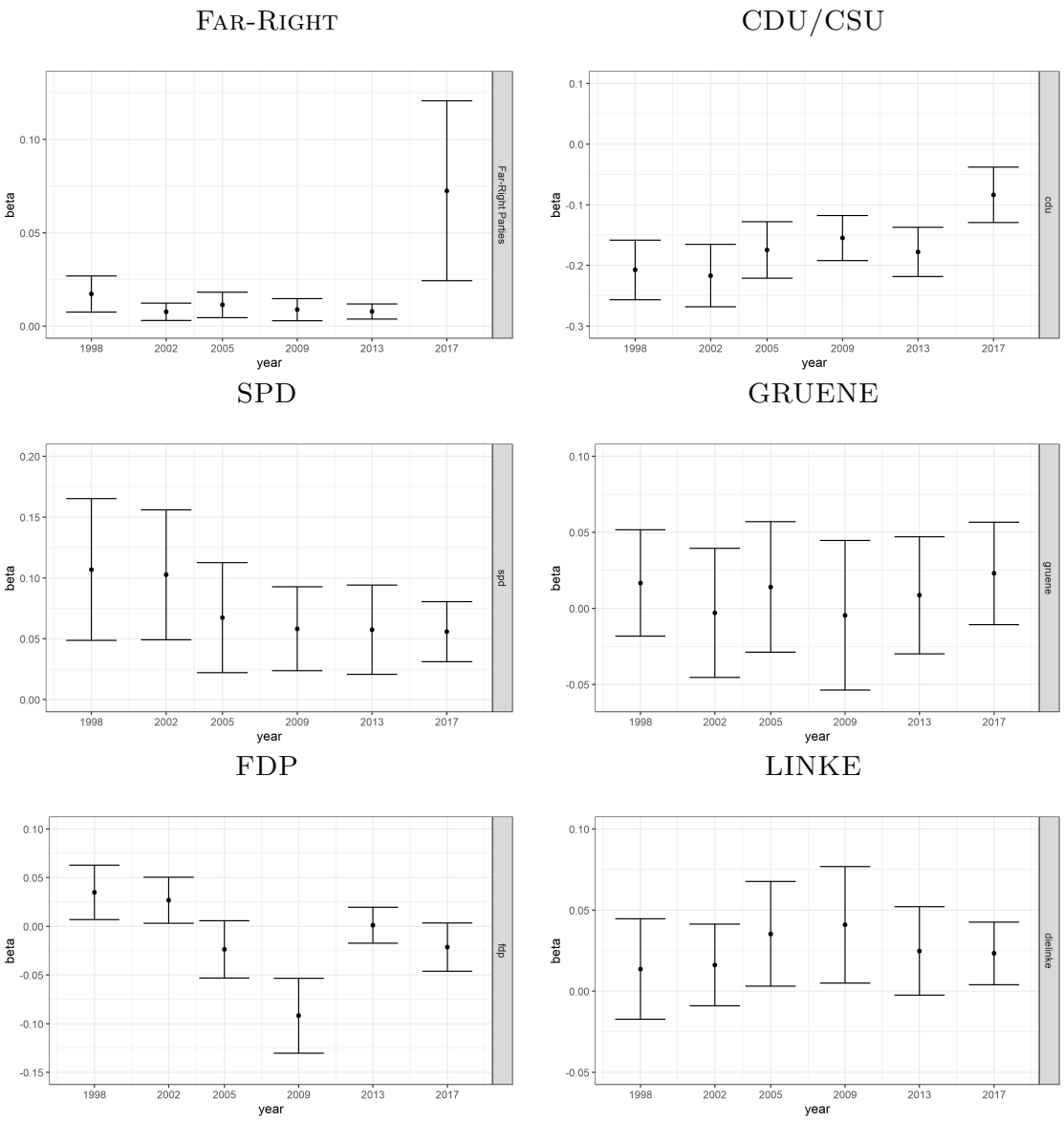
Notes: The dependent variable is the (standardized) change in vote share for the AfD (relative to eligible voters) from 2013 to 2017. Each column adds a different set of control variables. However, sample is restricted to observations where all control variables are non-missing. The explanatory variable across all columns is the 1933 NSDAP vote share (standardized). Population controls are: [historical] log population size (1933); [contemporary] log population size in 2015 and urbanization code dummies (3 categories). Employment controls are: [historical] shares of employed in industry and manufacturing, employed in trade and commerce, and employed in administration (agriculture and "other sectors" is the omitted category), all measured in 1925, as well as the unemployment share in 1933; [contemporary] the unemployment rate in 2015. Education controls are: [historical] share of white collar workers ("Angestellte und Beamte"); [contemporary] share of workers with university degree in 2015. Religion controls are: [historical] the share of Catholics and Jews; [contemporary] the share of Catholics and "Others" (i.e., Muslims, other religions, and no religion). Sample includes municipalities in all German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. All regressions contain a full set of state fixed effects. Standard errors are clustered at the level of historic municipalities/counties. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table D4: Persistence of Far Right Vote Share

	(1)	(2)	(3)	(4)
	Absolute far-right vote share		Standardised far-right vote share	
NSDAP 1933 [std.]	0.0862*		0.9861*	
	(0.0491)		(0.5622)	
NSDAP × 2017	0.0059***	0.0059***	0.0675***	0.0675***
	(0.0021)	(0.0023)	(0.0242)	(0.0259)
R^2	0.903	0.933	0.903	0.933
Observations	65843	65843	65843	65843
NSDAP × Year Trend	✓	✓	✓	✓
State-Year FEs	✓	✓	✓	✓
Municipal FEs		✓		✓

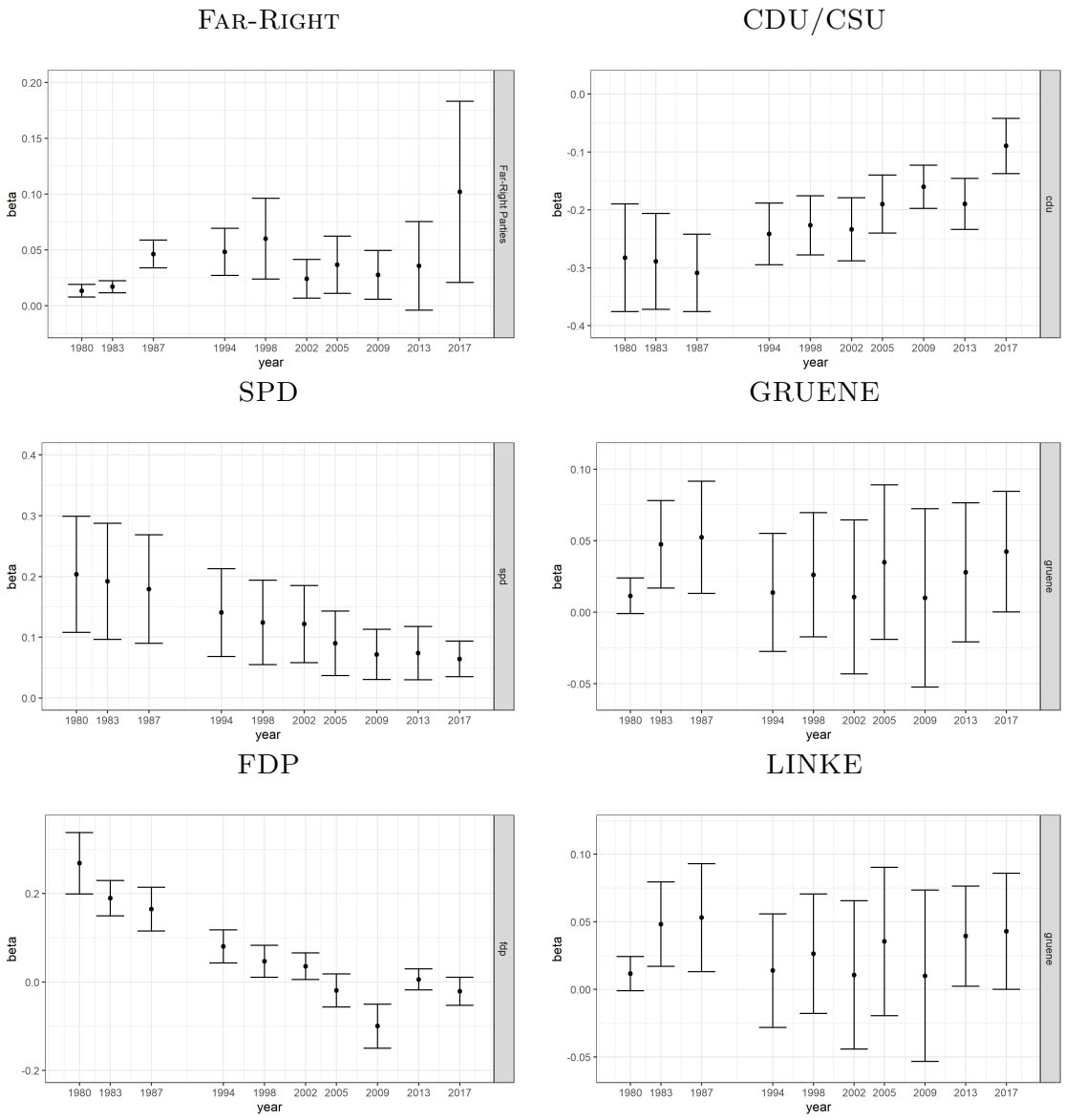
Notes: The dependent variables are vote shares for far-right parties (NPD, DVU, Republikaner, Die Rechte, and AfD in 2017). The sample includes modern municipalities in all German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. Municipalities are projected to 2015 borders using population-weighted raster techniques (cf. Appendix Section 1.8.1 for more detail). Standard errors are clustered at the level of the historical vote shares. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Figure D1: Persistence For Major Parties
- Single Year Regression Coefficients



Notes: The graph shows regressions coefficients and confidence intervals. Each coefficient stems from a single regression in a specific year. The dependent variable is the standardized gross vote share of the respective party (where the far-right includes NPD, DVU, Republikaner, Die Rechte, and AfD in 2017). All regressions include state fixed effects. Standard errors are clustered at the level of the historical vote shares. The sample includes modern municipalities in all German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. Municipalities are projected to 2015 borders using population-weighted raster techniques.

Figure D2: Persistence For Major Parties
- Sngle Year Regression Coefficients



Notes: The graph shows regressions coefficients and confidence intervals. Each coefficient stems from a single regression in a specific year. The dependent variable is the standardized gross vote share of the respective party (where the far-right includes NPD, DVU, Republikaner, Die Rechte, and AfD in 2017). All regressions include state fixed effects. Standard errors are clustered at the level of the historical vote shares. The sample includes modern municipalities in all Western German states apart from city states (Berlin, Hamburg, and Bremen), and the Saarland. Municipalities are matched on the basis of the eight digit municipal identity code. Only municipalities which could be tracked through 10 out of 11 federal election years are included (roughly matched panel).

1.8.5 *Additional Results, Survey Evidence from ALLBUS*

In this section we conduct additional analyses regarding our survey results from the Allbus data. In Table E1 we show that results are robust to collapsing the individual-level data at the municipality level and using residualized on individual-level controls. Table E2 adds the share of expellees to show robustness. Table E3 uses equal weights for the indices. Table E4 both uses municipalities as unit of observation and equal weights in indices. Table E5 shows the relationship between indices and voting intention for the AfD in 2016. In Table E6 we show that left-right self-evaluation predicts AfD voting intention positively and significantly in 2016, but negatively in 2014. This change does not occur for the NPD, a more extreme far-right party. Finally, Table 1.8.5 provides a detailed overview of the questions used for the indices.

Table E1: Survey Results: Attitudes Remain Constant

Dependent variable:	Xenophobia (Immigration)	Xenophobia (Equal Rights)	Xenophobia (Marriage/ Neighbour)	Islam at School	Antisemitism	Disenchantment with politicians	Gender Attitudes Index 1	Gender Attitudes Index 2	Left-Right Self-Evaluation	Pride to be German	Summary Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: *municipal level, without controls*											
NSDAP 1933 [std.]	0.0595**	0.0530*	0.0222	0.0459***	0.0007	0.0107**	0.0272	0.0057	0.00231	0.0124	0.0696**
	(0.0244)	(0.0285)	(0.0304)	(0.0127)	(0.0364)	(0.0044)	(0.0326)	(0.0159)	(0.00199)	(0.0115)	(0.0325)
NSDAP × 2016	-0.0398	-0.0654	-0.1164**	-0.0176	0.0450	0.0238*	0.047	0.0296	0.000537	-0.027*	0.0469
	(0.0586)	(0.0455)	(0.0557)	(0.0181)	(0.0544)	(0.0126)	(0.0441)	(0.0357)	(0.00563)	(0.0152)	(0.0573)
Observations	437	437	437	437	437	1315	293	878	2049	437	436
Panel B: *residualized on individual-level controls*											
NSDAP 1933 [std.]	0.0265	0.0232	-0.0003	0.0414***	-0.0322	0.0027	0.0384	0.0031	0.0013	0.0069	0.0403
	(0.0231)	(0.0260)	(0.0299)	(0.0121)	(0.0368)	(0.0041)	(0.0356)	(0.0162)	(0.0021)	(0.0116)	(0.0263)
NSDAP × 2016	-0.0111	-0.0359	-0.0989*	-0.0216	0.0652	0.0248**	0.0468	0.0257	0.0011	-0.0213	0.0670
	(0.0524)	(0.0480)	(0.0567)	(0.0174)	(0.0541)	(0.0119)	(0.0495)	(0.0396)	(0.0060)	(0.0155)	(0.0532)
Observations	436	436	437	437	436	1315	292	878	2040	437	435

Notes: Individual survey data from Allbus. Indices in columns 1-3, 5, 7-8, and 11 are weighted indices. All regressions contain a full set of state fixed effects. Standard errors are clustered at the level of historical vote shares. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table E2: Survey Results: Attitudes Remain Constant

Dependent variable:	Xenophobia (Immigration)	Xenophobia (Equal Rights)	Xenophobia (Marriage/ Neighbour)	Islam at School	Antisemitism	Disenchantment with politicians	Gender Attitudes Index 1	Gender Attitudes Index 2	Left-Right Self-Evaluation	Pride to be German	Summary Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: *individual-level, with controls and interaction with expellees*											
NSDAP 1933 [std.]	0.0424*	0.0236	-0.007	0.0349***	-0.0153	0.0051	0.0163	-0.0118	-0.0025	0.005	0.0428*
	(0.0245)	(0.0268)	(0.0296)	(0.0130)	(0.0353)	(0.0038)	(0.0311)	(0.015)	(0.002)	(0.0111)	(0.0238)
NSDAP × 2016	-0.0265	-0.048	-0.0806	-0.015	0.0286	0.0197	0.0596	0.0434	0.0014	-0.0218	-0.0079
	(0.0477)	(0.0467)	(0.0549)	(0.0173)	(0.0539)	(0.0122)	(0.0436)	(0.0329)	(0.0065)	(0.0151)	(0.052)
Share Expellees 1950s [std.]	-0.0253	0.0613	0.0581	0.0108	0.0643	-0.0029	-0.0053	0.0872***	0.024***	0.0076	0.07
	(0.0577)	(0.0479)	(0.0485)	(0.0199)	(0.0575)	(0.0067)	(0.0621)	(0.0261)	(0.004)	(0.0212)	(0.0473)
NSDAP × Expellees	-0.0390	-0.0049	-0.0271	-0.0002	-0.0223	-0.0017	-0.0139	-0.0103	-0.0009	0.0009	0.0083
	(0.0260)	(0.0257)	(0.0294)	(0.011)	(0.0283)	(0.0038)	(0.0288)	(0.0147)	(0.0023)	(0.0109)	(0.0227)
Observations	5446	6825	6798	8301	6043	23793	2913	13476	31182	7791	5201

Notes: Individual survey data from Allbus. Indices in columns 1-3, 5, 7-8, and 11 are weighted indices. All regressions contain a full set of state fixed effects. Standard errors are clustered at the level of historical vote shares. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table E3: Survey Results: Attitudes Remain Constant

Dependent variable:	Xenophobia (Immigration)	Xenophobia (Equal Rights)	Xenophobia (Marriage/ Neighbour)	Islam at School	Antisemitism	Disenchantment with politicians	Gender Attitudes Index 1	Gender Attitudes Index 2	Left-Right Self-Evaluation	Pride to be German	Summary Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: *individual level, without controls, equal weights in indices*											
NSDAP 1933 [std.]	0.0109***	0.0135 *	0.0045	0.0421***	0.0068	0.0110***	0.0004	0.0004	0.0016	0.013	0.0149***
	(0.0042)	(0.0075)	(0.0053)	(0.0121)	(0.0075)	(0.0041)	(0.0079)	(0.0049)	(0.002)	(0.0105)	(0.0048)
NSDAP × 2016	-0.0051	-0.0176	-0.0163*	-0.0136	0.0022	0.0202*	0.0209*	0.0126	0.0015	-0.0305**	-0.0074
	(0.0086)	(0.0117)	(0.0094)	(0.0169)	(0.0107)	(0.0119)	(0.0122)	(0.01)	(0.0062)	(0.0153)	(0.0097)
Observations	6632	8290	8305	10100	7305	28255	3411	16036	36957	9434	6227
Panel B: *with individual-level controls, equal weights in indices*											
NSDAP 1933 [std.]	0.0061	0.0064	0.0003	0.0363***	0.0002	0.0047	0.0043	-0.0003	0.0001	0.008	0.0102***
	(0.0038)	(0.0074)	(0.0055)	(0.0123)	(0.0074)	(0.0037)	(0.0087)	(0.0045)	(0.002)	(0.0109)	(0.0041)
NSDAP × 2016	-0.0029	-0.0129	-0.0142	-0.0159	0.0037	0.0194	0.0157	0.011	0.0029	-0.0283*	-0.0053
	(0.0078)	(0.0121)	(0.0099)	(0.0169)	(0.0106)	(0.0119)	(0.0133)	(0.0095)	(0.0065)	(0.0162)	(0.009)
Observations	5714	7149	7122	8701	6344	24873	3065	14086	32625	8152	5463

Notes: Individual survey data from Allbus. Indices in columns 1-3, 5, 7-8, and 11 are equally weighted. All regressions contain a full set of state fixed effects. Standard errors are clustered at the level of historical vote shares. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table E4: Survey Results: Attitudes Remain Constant

Dependent variable:	Xenophobia (Immigration)	Xenophobia (Equal Rights)	Xenophobia (Marriage/ Neighbour)	Islam at School	Antisemitism	Disenchantment with politicians	Gender Attitudes Index 1	Gender Attitudes Index 2	Left-Right Self-Evaluation	Pride to be German	Summary Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: *municipal level, without controls, equal weights in indices*											
NSDAP 1933 [std.]	0.0112***	0.0119	0.00394	0.0459***	0.00228	0.0107**	0.00733	0.00127	0.00231	0.0124	0.0133**
	(0.00418)	(0.00792)	(0.00568)	(0.0127)	(0.00774)	(0.0044)	(0.0101)	(0.00483)	(0.00199)	(0.0115)	(0.00582)
NSDAP × 2016	-0.00647	-0.0163	-0.0206**	-0.0176	0.00982	0.0238*	0.0155	0.00823	0.000537	-0.027*	0.00497
	(0.00969)	(0.012)	(0.0103)	(0.0181)	(0.011)	(0.0126)	(0.0138)	(0.0103)	(0.00563)	(0.0152)	(0.0988)
Observations	437	437	437	437	437	1315	293	878	2049	437	436
Panel B: *residualized on individual-level controls, equal weights in indices*											
NSDAP 1933 [std.]	0.0053	0.0044	-0.0004	0.0414***	-0.0055	0.0027	0.0115	0.0001	0.0013	0.0069	0.0081*
	(0.0037)	(0.0075)	(0.0056)	(0.0121)	(0.0078)	(0.0041)	(0.0111)	(0.0048)	(0.0021)	(0.0116)	(0.0048)
NSDAP × 2016	0.0014	-0.0091	-0.0176*	-0.0216	0.0121	0.0248**	0.0147	0.007	0.0011	-0.0213	0.0083
	(0.0082)	(0.0126)	(0.0105)	(0.0174)	(0.111)	(0.0119)	(0.0155)	(0.0113)	(0.006)	(0.0155)	(0.0092)
Observations	436	436	437	437	436	1315	292	878	2040	437	435

Notes: Individual survey data from Allbus. Indices in columns 1-3, 5, 7-8, and 11 are equally weighted indices. All regressions contain a full set of state fixed effects. Standard errors are clustered at the level of historical vote shares. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table E5: Indices and AfD Voting Intention, 2016

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Xenophobia (Immigration)	0.0571***							
	(0.0147)							
Xenophobia (Marriage/ Neighbour)		0.0399***						
		(0.0069)						
Islam at School			0.0697***					
			(0.0139)					
Antisemitism				0.0620***				
				(0.0099)				
Disenchantment with politicians					0.0771***			
					(0.0084)			
Gender Attitudes Index 1						-0.0010		
						(0.0044)		
Gender Attitudes Index 2							0.0012	
							(0.0028)	
Left-Right Self-Evaluation								0.0928***
								(0.0128)
Observations	755	1652	3410	1445	3360	3490	3490	3490

Notes: The dependent variable is an indicator of voting intention for the AfD in 2016. Individual survey data from Allbus (2016 cross-section). All regressions contain a full set of state fixed effects. Controls include a dummy for former East Germany, age, marriage status, gender, and income categories. Robust standard errors are used in all specifications. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table E6: Left-Right Self-Evaluation and AfD Voting Intention, 2014-2016

	(1) AfD	(2) NPD
Left-Right Self-Evaluation	-0.0094***	0.0228***
	(0.0028)	(0.0025)
Left-right self-evaluation × 2016	0.1516***	0.0021
	(0.0104)	(0.0039)
Observations	41445	41445

Notes: The dependent variable is an indicator of voting intention for the AfD (column 1) and the NPD (column 2). Individual survey data from Allbus (panel for 2014 and 2016). All regressions contain a full set of state fixed effects. Controls include a dummy for former East Germany, age, marriage status, gender, and income categories. Robust standard errors are used in all specifications. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Questions in Indices from Allbus Survey

A: XENOPHOBIA (IMMIGRATION)				
Question set 1: Opinions on Immigration		B[49]	N[50]	I[51]
Preamble:	The next question is about the immigration of various groups of people to Germany. What is your opinion about this?			
Possible Answers:	Immigration should be unrestricted (= 0); Immigration should be restricted (= 0); Immigration should be stopped completely (= 1)			
A.1.1	What about ethnic Germans from Eastern Europe?	✓		
A.1.2	Asylum Seekers?	✓		
A.1.3	People from EU countries coming to work here?	✓		
A.1.4	People from non-EU countries, e.g. Turkey, coming to work here?	✓		
Question set 2: German citizenship		B	N	I
Preamble:	I will tell you a few things which may play a role in the decision whether or not to grant German citizenship. Using the scale, please tell me how important these things should be in your opinion.			
Possible Answers:	Scale (1 to 7): not at all important – very important			
A.2.1	Whether the person was born in Germany		✓	
A.2.2	Whether the person is of German origin		✓	
A.2.3	Whether the person is fluent in German		✓	
A.2.4	Whether the person has lived in Germany a long time		✓	
A.2.5	Whether the person is prepared to adapt to the German way of life		✓	
A.2.6	Whether the person belongs to a Christian denomination		✓	
A.2.7	Whether the person has committed any crimes		✓	
A.2.8	Whether the person can support himself or herself		✓	
Question 3: Stranger in one's own country		B	N	I
Preamble:	To what extent do you agree with the following statements?			

Continued on next page

49. Binarized: answers have been binarized as indicated in the *Possible Answers* row.
50. Normalized: initial scale has been normalized to a 0-1 range.
51. Inverted: initial scale has been inverted.

Possible Answers:	Scale (1 to 7): completely disagree – completely agree			
A.3.1	With so many foreigners in Germany, one feels increasingly like a stranger in one's own country.		✓	

B: XENOPHOBIA (EQUAL RIGHTS)

Question set 1: Opinions on foreigners' rights		B	N	I
Preamble:	This question is about foreigners who live in Germany. Please tell me for each statement to what extent you agree with it.			
Possible Answers:	Scale (1 to 7): completely disagree – completely agree			
B.1.1	Foreigners living in Germany should be able to acquire German citizenship without having to give up their own citizenship, i.e. dual citizenship should be possible.		✓	✓
B.1.2	Foreigners living in Germany should be entitled to the same welfare benefits and other social entitlements as Germans.		✓	✓
B.1.3	Italians living in Germany should have the same rights as Germans in every respect.		✓	✓
B.1.4	Ethnic Germans from Eastern Europe living in Germany should have the same rights as Germans in every respect.		✓	✓
B.1.5	Asylum-seekers living in Germany should have the same rights as Germans in every respect.		✓	✓
B.1.6	Turkish people living in Germany should have the same rights as Germans in every respect.		✓	✓

C: XENOPHOBIA (MARRIAGE/ NEIGHBOUR)

Question set 1: Opinions on foreigners as neighbours		B	N	I
Preamble:	How pleasant or unpleasant would it be for you to have members of these groups as neighbours?			
Possible Answers:	Scale (-3 to +3): would be very unpleasant for me – would be very pleasant for me			
C.1.1	…an Italian person as a neighbour?		✓	✓
C.1.2	…an ethnic German from Eastern Europe as a neighbour?		✓	✓
C.1.3	…an asylum-seeker as a neighbour?		✓	✓
C.1.4	…a Turkish person as a neighbour?		✓	✓

Continued on next page

Question set 2: Opinions on foreigners marrying into family		B	N	I
Preamble:	And what if a member of one of these groups married into your family? To what extent would it be pleasant or unpleasant for you…			
Possible Answers:	Scale (-3 to +3): would be very unpleasant – would be very pleasant			
C.2.1	…if an Italian person married into your family?		✓	✓
C.2.2	…if an ethnic German from Eastern Europe married into your family?		✓	✓
C.2.3	…if an asylum-seeker married into your family?		✓	✓
C.2.4	…if a Turkish person married into your family?		✓	✓

D: ISLAM AT SCHOOL

Question set 1: Opinions on Islam at school		B	N	I
Preamble:	It is being debated whether there should be Islamic religious instruction for Muslim children in state schools.			
Possible Answers:	Islamic religious instruction too (= 0); Only Christian religious instruction (= 1); No religious instruction at all (= 0)			
D.1.1	What is your opinion about this: In state schools in Germany, there should be ...	✓		

E: ANTISEMITISM

Question set 1: Opinions on Jewish people		B	N	I
Preamble:	Every now and then, one hears different opinions about Jewish people. Would you please tell me to what extent you agree or disagree with these statements?			
Possible Answers:	Scale (1 to 7): completely disagree - completely agree			
E.1.1	As a result of their behaviour, Jewish people are not entirely without blame for being persecuted.		✓	
E.1.2	Jewish people have too much influence in the world.		✓	
E.1.3	Many Jewish people try to take personal advantage today of what happened during the Nazi era and make Germans pay for it.		✓	

Continued on next page

E.1.4	Jewish people living in Germany should have the same rights as other Germans in every respect.		✓	✓
E.1.5	I'm ashamed that Germans have committed so many crimes against Jewish people.		✓	✓
Question 2: Opinions on Jewish people as neighbours		B	N	I
Preamble:	How pleasant or unpleasant would it be for you to have members of these groups as neighbours?			
Possible Answers:	Scale (-3 to +3): would be very unpleasant for me – would be very pleasant for me			
E.2.1	A Jewish person as a neighbour?		✓	✓
F: DISENCHANTMENT WITH POLITICIANS				
Question set 1: Politicians' interest in ordinary people		B	N	I
Preamble:	I'm going to read you some statements now. Please tell me after each one whether you have the same or a different opinion.			
Possible Answers:	Have the same opinion (= 1); have a different opinion (= 0); don't know			
F.1.1	Most politicians are not really interested at all in the problems of ordinary people.	✓		
G: GENDER ATTITUDES INDEX 1				
Question set 1: Role of mothers and fathers		B	N	I
Preamble:	People have different opinions about the role of mothers and fathers. For each of the following statements please tell me whether you -			
Possible Answers:	completely agree (= 1); tend to agree (= 1); tend to disagree (= 0); completely disagree (= 0)			
G.1.1	Even if both parents work, it is still better if the mother has main responsibility for looking after the home and children.	✓		
G.1.2	The best way to organise family and work life is for both partners to work full-time and to look after the home and children equally.	✓		✓

Continued on next page

G.1.3	A full-time working mother can normally establish just as close a relationship with her small child as a mother who doesn't work.	✓		✓

H: GENDER ATTITUDES INDEX 2

Question set 1: Role of women in the family		B	N	I
Preamble:	People have different opinions about the role of women in the family and in bringing up children. For each of the statements on the card, please tell me whether you -			
Possible Answers:	completely agree (= 1); tend to agree (= 1); tend to disagree (= 0); completely disagree (= 0)			
H.1.1	A working mother can establish just as loving and secure a relationship with her children as a mother who doesn't work.	✓		✓
H.1.2	It's more important for a wife to help her husband with his career than to pursue her own career.	✓		
H.1.3	A married woman should not work if there are not enough jobs to go round and her husband is also in a position to support the family.	✓		

I: LEFT-RIGHT SELF-EVALUATION

Question set 1: Left-right self-evaluation		B	N	I
Preamble:	Many people use the terms ''left'' and ''right'' when they want to describe different political views. Here we have a scale which runs from left to right.			
Possible Answers:	Scale (1 to 10)			
I.1.1	Thinking of your own political views, where would you place these on this scale?		✓	

J: PRIDE TO BE GERMAN

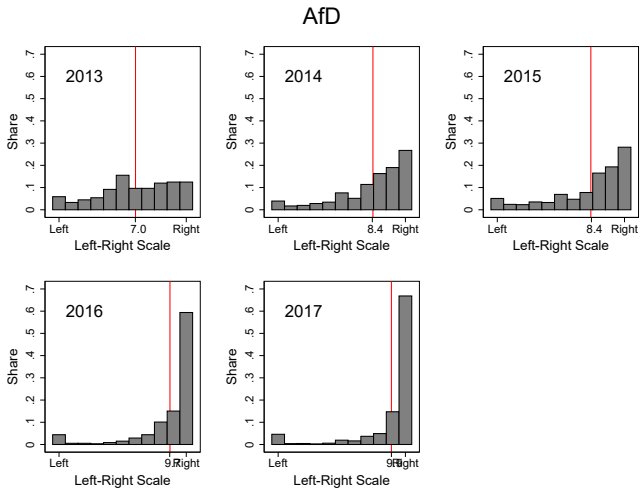
Question set 1: Pride to be German		B	N	I
Possible Answers:	very proud (= 1); fairly proud (= 1); not very proud (= 0); not at all proud (= 0)			
J.1.1	Would you say you are very proud, fairly proud, not very proud or not at all proud to be German?	✓		

Notes: We coded answers into variables ranging from 0 to 1, with "1" corresponding to more right-wing/conservative responses.

1.8.6 *Additional Results, Survey Evidence from GLES*

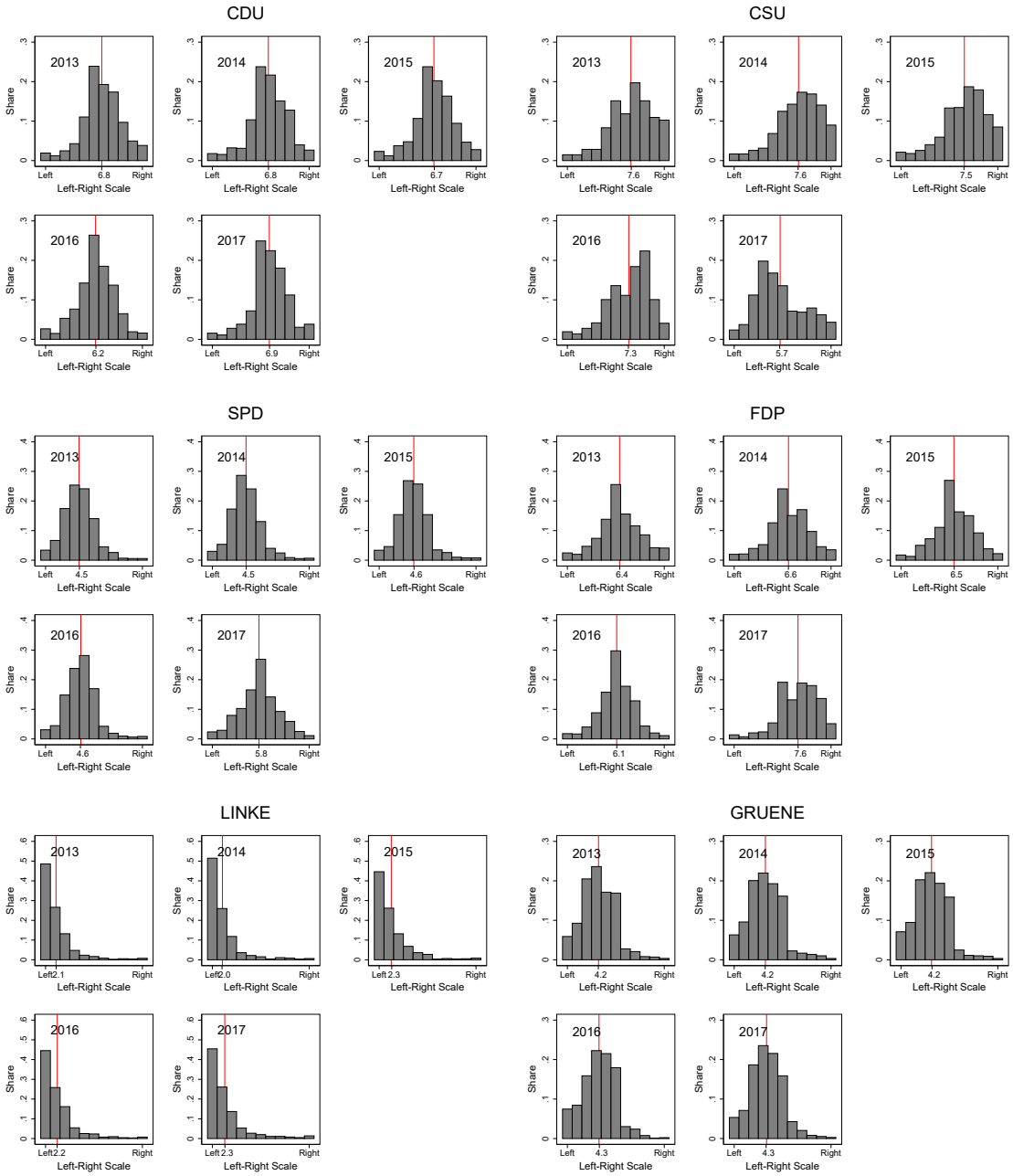
In this section we provide detail on our analyses based on survey data from GLES. Figure F1 shows histograms for the evaluation of the AfD on a left-right scale for all years between 2013-2017. Figure F2 shows similar histograms for all other main German parties for comparison. Figure F3 displays "relative stigma" levels for parties from 2009 until 2017: we define stigma as the (relative) difference between a party's actual electoral success and stated voted intentions. A value of 0 indicates exact correspondence between stated voted intentions in polls and actual electoral shares. A value of 1 indicates that no survey respondents declare support for the party, and that actual electoral support is entirely in excess of the expected share calcluated from polls. Table F1 shows that left-right self-evaluation of children is highly correlated to that of their parents. This correlation is even higher in small communities.

Figure F1: Survey Evidence on Party Orientation - AfD



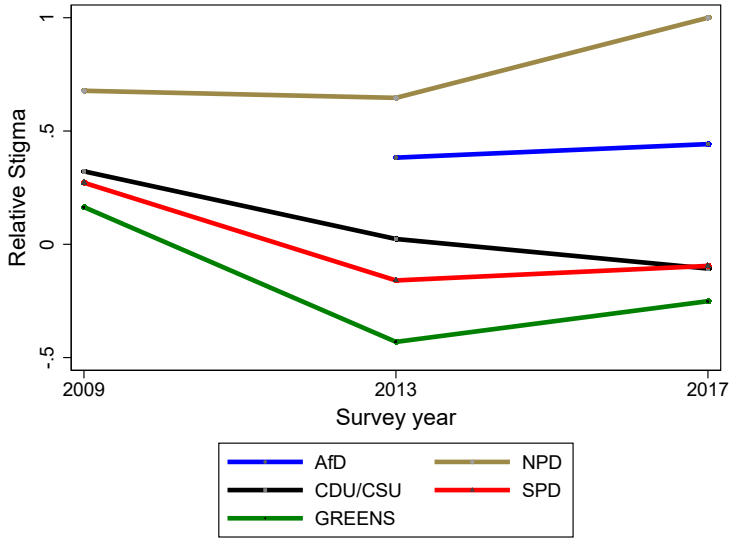
Notes: The graphs show where GLES survey respondents placed the AfD on the left-right spectrum. The graphs plot histograms for all years during 2013-2017. Red lines in the graph represent means.

Figure F2: Survey Evidence on Party Orientation - Other Parties



Notes: The graphs show where GLES survey respondents placed different political parties on the left-right spectrum. The graphs plot histograms for all main German political parties during 2013-2017. Red lines in the graph represent means.

Figure F3: Relative Stigma 2009-2017



Notes: Graph plots means of "relative stigma" values for nine German parties in German election districts across time. Relative stigma is defined as difference between the actual vote share and the mean of voting intentions, divided by the actual vote share, for a given party in a given year.

Table F1: Correlations Between the Political Attitudes of Children and Parents

	Dependent Variable: Imputed Left-Right Self-Placement					
	Both		Mother		Father	
	(1)	(2)	(3)	(4)	(5)	(6)
Parent(s)	0.612***	0.539***	0.556***	0.458***	0.507***	0.480***
	(0.020)	(0.054)	(0.018)	(0.051)	(0.019)	(0.049)
Parent(s)		0.124*		0.137**		0.039
× Small Town		(0.065)		(0.060)		(0.059)
Small Town		−0.389		−0.415		0.146
		(0.340)		(0.311)		(0.305)
Observations	2,893	1,351	3,270	1,514	3,410	1,613
Adjusted R^2	0.265	0.275	0.246	0.249	0.207	0.211

Notes: The table shows the results of regressing an individual's political attitude on the political attitude of her parent(s), interacted with an indicator for neighbourhood size. Political attitude is measured as imputed value on a left-right scale: based on the underlying party identification as indicated by the interviewee in the GLES, the party's corresponding left-right value (taken from Parlgov.org) is assigned. In columns (1) and (2), the independent variable is the mean value of the political attitudes of the mother and the father; in columns (3) and (4), the independent variable is the mother's political attitude; in columns (5) and (6), it is the father's political attitude. Small Town is an indicator variable for whether the individual lives in a rural area, village, or small or medium-sized town. All regressions include an intercept (not shown). Robust White standard errors are in parentheses. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Chapter 2

Populism and Propagation of Far-Right Extremism

2.1 Introduction

Recent work in economics has found that beliefs and information play a crucial role for individual decision-making. Building on early publications by Elster (1989) and Akerlof (1980), the notion of social norms as important drivers of individual behaviour has been developed further. Social norms derive from beliefs about other people's attitudes. Thus, social norms can change when new information about others becomes available, for example after elections. As a consequence, information shocks can have impacts on various dimensions. This paper examines how electoral information shocks about the success of a xenophobic populist party can propagate far-right protests.

When it comes to protest behaviour, beliefs about other people's attitudes can be pivotal for protests to take place, or not to take place at all (Aidt, Leon, and Satchell, 2017; Cantoni et al., 2019). Information shocks which lead to updates of such beliefs could impede or induce protest behaviour, for example by overcoming pluralistic ignorance (Kuran, 1991; Enikolopov, Makarin, and Petrova, 2019; González, 2020; Manacorda and Tesei, 2020). In an experiment, Bursztyn, Egorov, and Fiorin (2019) show that xenophobic individuals are empowered after a surprising populist electoral success and act more openly upon their beliefs and attitudes. Some studies have documented this empowerment effect in non-experimental settings (Schilter, 2018; Müller and Schwarz, 2018; 2019; Kuipers, Nellis, and Weaver, 2019). However, more work is needed to better understand under which conditions an empowerment effect manifests itself. This paper contributes to this young literature using novel data on far-right extremist protest marches ("far-right protests") from the German *Bundestag*. In particular, it extends the literature by showing how the empowerment effect depends on an element of surprise.

I find that populist electoral success leads to more far-right protests when the success is surprising. My interpretation is that if populists win unexpectedly, the resulting decrease in social stigma

attached to far-right protests will be higher. In this paper, I employ three strategies to capture surprise. First, populist success is more surprising if polling institutes severely underestimate the populist party's vote share. Vote shares of Germany's recently established right-wing populist AfD party ("Alternative für Deutschland") were underestimated by more than 20 percent in six state elections between 2014 and 2016. Second, surprise could be captured by the difference between populist support in a federal state and populist support in a single municipality. The underlying assumption is that individuals might have more precise knowledge about other people's attitudes in their municipality than about other people's attitudes in the broader society.

When success of the populist party is severely underestimated, a municipality with a populist vote share 10 percentage points below state average faces a roughly 30 percent increase of the mean likelihood of an additional far-right protest. The effect materializes only after the rightward shift of the AfD and vanishes when polling institutes correctly estimate the populist party's success. Including municipality, month, and election date-relative month fixed effects allows me to rule out that the effect is driven by unobserved heterogeneity with respect to each of these dimensions. My data also allows me to proxy for time-varying presence of refugees at the municipal level and to exploit the staggered timing of state elections to come closer to a causal estimate of the empowerment effect. A placebo regression before 2013, i.e. before the AfD was founded, suggests that I do not pick up general patterns of far-right protests around elections, but rather the effect of a populist party's surprising success.

A third way to capture an element of surprise is through the polarisation or fractionalisation of the political party landscape in a municipality. Individuals in municipalities with highly polarised vote shares and few successful marginal parties can be expected to be more surprised by the rise of a new xenophobic party. The reason is that more homogeneous communities probably do not reflect the more heterogeneous political landscape of the broader society well. To test this hypothesis, I construct indices of polarisation and fractionalisation following Montalvo and Reynal-Querol (2005). I find that, indeed, the empowerment effect is close to zero and insignificant if fractionalisation is high and polarisation low.

The association between surprising populist success and far-right protests would be spurious if organizers of far-right protests strategically target municipalities with below-average populist vote shares. Moreover, were organizers more inclined to do so when polling institutes underestimated the populist vote share, strategic targeting would be able to explain my results. To rule out this alternative explanation, I test whether the additional far-right protests after surprising elections tend to be initiated by formal or non-formal organizers. The intuition is that strategic targeting would be used by formal organizers, if at all. I find that bottom-up far-right protests initiated by non-formal organizers respond more heavily to the information shock, suggesting that the effect is not due to strategic targeting.

I leverage restricted-use survey data from the German Longitudinal Election Study (GLES) to explore potential mechanisms of the empowerment effect (GLES, 2019). I hypothesize that

the mechanism at work is a reduction in social stigma attached to far-right protests, and not a change in attitudes. A priori, both mechanisms could explain my results. On the one hand, several studies show that attitudes can change after certain information shocks (Sunstein et al., 2016; Dinas and Fouka, 2018; Giani and Méon, 2019; Gerling and Kellermann, 2019; Grigorieff, Roth, and Ubfal, 2020). On the other hand, many studies document that attitudes can remain stable for long periods (Voigtländer and Voth, 2012; Becker et al., 2016; Guiso, Sapienza, and Zingales, 2016). To distinguish between these competing mechanisms, I investigate whether attitudes changed after state elections in municipalities with different levels of information shocks. I find that attitudes towards cultural adaptation of immigrants – arguably a proxy for right-wing ideology – are not affected differentially in municipalities with more surprising information shocks after elections. If anything, surprisingly high populist success leads individuals in areas with lower AfD support to regard the AfD as more right-wing.

A valid question concerning the empowerment effect is about its reach. Can information shocks from elections in other states impact behaviour? Further, is the effect specific to far-right extremism, or does it also impact antisemitic incidents? I shed light on these questions by leveraging data on antisemitic incidents and attacks against refugees from the Amadeu Antonio Foundation[1]. Although there have been some individual cases of antisemitism within the AfD, the party's main political target does not seem to be the Jewish population, but rather Islam in general and asylum seekers in particular (Arzheimer and Berning, 2019). Consistent with this observation, I find no effect of surprisingly high populist vote shares on antisemitic incidents, but significant effects for attacks against refugees. I find no evidence that electoral information shocks translate into more far-right protests in other states.

To substantiate my interpretation that the empowerment effect works through reduced social stigma, I test whether patterns of heterogeneity associated with this interpretation hold. According to my interpretation, the information shock induces far-right protests because xenophobic individuals realise that more people share or at least tolerate their views than previously thought. If this is the case, one can expect that the following patterns of heterogeneity arise. First, the propagation effect should be more pronounced in areas where people care more about other people's opinions. Second, the effect should come from politically informed far-right extremists. Third, the effect should be strong in areas where attitudes associated with the support for the AfD are weak. I obtain restricted-use access to the German Socio-Economic Panel (GSOEP) and present evidence that these patterns of heterogeneity seem to hold.

A relevant question is the role of social media in propagation of far-right protests. An emerging literature documents how social media influences political outcomes (Müller and Schwarz, 2019; Enikolopov, Makarin, and Petrova, 2019; Bursztyn et al., 2019; Müller and Schwarz, 2018; Theocharis et al., 2015). I obtain the universe of tweets geolocalised in Germany between the 2013 and 2017 federal elections which contain the hashtag #AfD to address this question. First, I

1. www.amadeu-antonio-stiftung.de/chronik/

classify the tweets into anti- and pro-AfD using both manual and machine learning methods. Cross-validation with the universe of tweets from German political parties confirms that most tweets are directed against the AfD. Political tweets might be a mediating outcome. On the one hand, tweets might react to electoral information shocks. On the other hand, tweets itself could fuel or impede far-right protests. I examine both potential roles. It turns out that – as in the case of far-right protests – the element of surprise is again a key factor that influences twitter responses.

I find that in response to surprisingly high populist success, anti-AfD tweets tend to increase in areas with lower populist support. When polling institutes correctly estimate the populist party's success, the effect vanishes. Further, I provide suggestive evidence that municipalities where pro-AfD tweets originate from exhibit higher probabilities of far-right protests before and after the tweets. Anti-AfD tweets do not seem to be associated with higher or lower probabilities of far-right protests. Note that this evidence should not be seen as causal because tweets are likely to be endogenous to factors also determining far-right protests. However, the geolocalised nature of the tweets allow for the inclusion of municipality and month fixed effects, thus mitigating the bias from time-constant confounders or macroeconomic shocks.

One might argue that polling forecasts for the populist party could also be information shocks. If polls report higher support of the populist party, people might update beliefs about social acceptability and act accordingly. I compare the effect of polling releases with the effect of state election results. To this end, I collect polling data on the AfD from 21 different polling institutes covering the time between September 2013 and September 2017. Consistent with an interpretation of increased social acceptability I find that polling releases increase pro-AfD tweets in the same month as the release. Polling releases seem to impact the number of far-right protests only with a lag of two months, and with roughly half the effect size in terms of standard deviation as information shocks from state elections.

This paper contributes to the literature in several respects. While existing research into the origin of populism is abundant, less attention has been paid to the implications of populism (Guriev and Papaioannou, 2020). Several studies point to a simultaneous increase in populist success and occurrences of hate crime (Jäckle and König, 2017; Bursztyn et al., 2019; Rees et al., 2019). However, despite these studies, the impact of populist success on far-right protests remains unclear. Work on the consequences of populist success often has remained purely correlational and unable to address identification challenges of reverse causality and endogeneity. Protests could – in theory – be both cause and consequence of populist success. Staggered state election results in Germany between 2014 and 2017 allow me to exploit a unique quasi-natural experiment and address these challenges.

Previous literature evaluating the consequences of populist electoral success has almost exclusively focused on the nexus of populism and hate crime: Bursztyn, Egorov, and Fiorin (2019, p.5) note that, after Donald Trump's election, reports about hate crimes against immigrants increased.[2]

2. The authors postulate that populist electoral success might facilitate coordination for large-scale actions, such

In another paper, Bursztyn et al. (2019) establish a link between votes for a nationalistic party in Russia and hate crime in areas with higher social media usage. Schilter (2018) and Albornoz, Bradley, and Sonderegger (2020) study the relationship between the Brexit vote and hate crimes. Müller and Schwarz (2018) examine how tweets from Donald Trump spur increases in hate crime against Muslims. Romarri (2019) shows that the presence of extreme right-wing mayors is associated with more hate crimes in Italy. Finally, in the German context, Jäckle and König (2017) document a correlation between the AfD's vote share in German districts and the number of attacks against refugees. More relatedly, Müller and Schwarz (2019) provide empirical evidence of a relationship between anti-refugee Facebook posts from AfD and hate crime in Germany.

Previous literature has also found a link between unexpectedly high populist vote shares and changes in reported attitudes. Gerling and Kellermann (2019) show that high AfD vote shares reduced social desirability bias and increased individuals' likelihood to report intending to vote for the AfD. In a similar vein, Giani and Méon (2019) show that reported racist attitudes increased after Donald Trump's election.

Twitter data has been the object of study for many scholars both in economics and political science. However, most studies using digital trace data have stopped short of analysing causal links and seldomly go beyond illustration of usage patterns (Jungherr and Theocharis, 2017). In fact, the analysis of digital trace data such as twitter might have big potentials, but come with serious challenges such as contextualisation, data quality, methodology, and interdisciplinarity. As I use geolocalised tweets, I have the advantage that I can use both geographical and time variation to come much closer to causal analysis of digital trace data than previous studies did. A final concern might be that tweets in general do not reflect the political reality perfectly and are themselves influenced by political reality, user characteristics, and twitter technology (Jungherr, Schoen, and Jürgens, 2015). I mitigate these issues by restraining my analysis to inherently political tweets – those with the hashtag #AfD.

The paper is structured as follows: Section 2.2 provides a brief overview of the right-wing populist AfD party and far-right protests in Germany. section 2.3 outlines the data, and Section 2.4 the econometric setting. Section 2.5 presents and discusses the results. Finally, Section 2.6 concludes.

2.2 Background

2.2.1 Populism in Germany: The AfD Party

Founded only months before the 2013 federal election as a Eurosceptic and socially conservative party, the AfD ("Alternative für Deutschland") attracted various stripes of right-wingers as members from the beginning on (Arzheimer and Berning, 2019). With a remarkable success for a newly founded party, the party started to embrace xenophobic attitudes more openly after March 2015

as demonstrations and movements, but postpone empirical analysis of this question to future work.

- when the "Erfurter Resolution", a right-wing manifesto of the party base, was published. Only a few months later, the AfD's former leadership was ousted and exited the party. The party radicalised quickly in early July 2015, which can be seen as the definite right-wing shift of the AfD (Arzheimer and Berning, 2019). As it happened, another few months later hundreds of thousands of asylum seekers arrived in Germany, and the party gained substantial support in the following state elections (see Appendix Figure G1). At the same time, the number of far-right extremist protest marches increased in Germany. In the more recent past, scholars have argued that "the AfD are enablers of right-wing terror" (Funke, 2019).

Despite the AfD's success, there are reasons to believe that the stigma attached to voting for the new populist right-wing party in Germany was high: Bergmann and Diermeier (2017) argue that the social stigma and undesirability of the AfD party prevented forecasters to accurately predict the AfD's vote share. They show that two major forecasting institutions severely understated its vote shares in the first eight state elections after 2013. Further, the AfD is regarded as much more moderate by its supporters than by non-AfD voters (Bergmann and Diermeier, 2017).

2.2.2 Far-Right Protests

Articles five and eight of the German Basic Law stipulate the right to freedom of assembly and freedom of expression. Protest marches in general do not need legal approval and are allowed take place as long as the local authority is notified within 48 hours before the protest begins.[3] Thus, far-right protest marches could, in theory, take place in any municipality in Germany at any point in time. Appendix Figure G2 illustrates two examples of far-right extremist protest marches.

How are far-right protests organized? My data allow me to differentiate between formal and non-formal organizers of far-right protests. Appendix Figure G3 shows a breakdown of far-right protests by organization and month. Formal organizers include organizations such as "Der III. Weg" ("The Third Way", in reference to the so-called "Third Reich"), the far-right "NPD" party ("Nationale Partei Deutschlands") or "Die Rechte" ("The Right"). Non-formal organizers are not affiliated with a far-right group or movement.[4] Appendix Figure G4 shows a word cloud of slogans for far-right protests since 2005. Typical slogans under which far-right protest marches are reported at the local authority include the following: "Gegen Überfremdung und Asylmissbrauch" ("Against foreign infiltration and asylum misuse"), "Tag der deutschen Heimattreue" ("Day of German Loyalty"), and "Das Boot ist voll - Asylbetrüger abschieben" ("The boat is full - expel asylum cheaters"). Far-right protests are of high political concern as can be seen by regular parliamentary enquiries about their occurrences and even mass media coverage like in the case of the far-right protests in Chemnitz on 27th August 2018.

It is not exaggerated to see these far-right extremist protest marches as a serious threat to

3. The only exception is the case of immediate threat of public security, which enables local authorities to cancel the protest march as a last resort.

4. The data attributes the far-right protest organization in this case to "neo-Nazis" in general or "Other".

liberal democracy. As Madestam et al. (2013) argue, protests can build political movements that ultimately affect policymaking. They do so by influencing political views rather than solely through the revelation of existing political preferences. Further, Germany's Federal Constitutional Court pronounces the following judgement about the NPD – one of the main organizers of the far-right protest marches:

> *"The National Democratic Party of Germany (NPD) advocates a concept aimed at abolishing the existing free democratic basic order. The NPD intends to replace the existing constitutional system with an authoritarian national state that adheres to the idea of an ethnically defined 'people's community' (Volksgemeinschaft). Its political concept disrespects human dignity and is incompatible with the principle of democracy." (Federal Constitutional Court, 2017)*

2.3 Data

2.3.1 Main Outcome: Far-Right Protests

I digitize and geocode responses to parliamentary enquiries about 1,249 far-right extremist protest marches ("far-right protests") between 22nd September 2013 and 24th September 2017, the dates of federal elections and period in which the newly emerging right-wing populist party gained considerable success at various state elections.[5] For a placebo regression, I also use observations between 2009-2013, comprising an additional 513 far-right protests.

Figure 2.1 shows the geographic dispersion of the far-right protest marches across the 16 federal states. Far-right protests seem to occur more frequently in the East, but also in various areas in West Germany. For most of my analysis I will focus on observations within the shaded states that held a state election between the 2013-2017 federal elections. Appendix Figure G5 illustrates that far-right protests peak at the end of 2015 during the height of the so-called refugee crisis. In addition to the affiliations of organizers, I also digitize the slogans under which far-right protests are reported to the authorities. Appendix Figure G6 shows that the total of far-right-protest marches varies substantially by state.

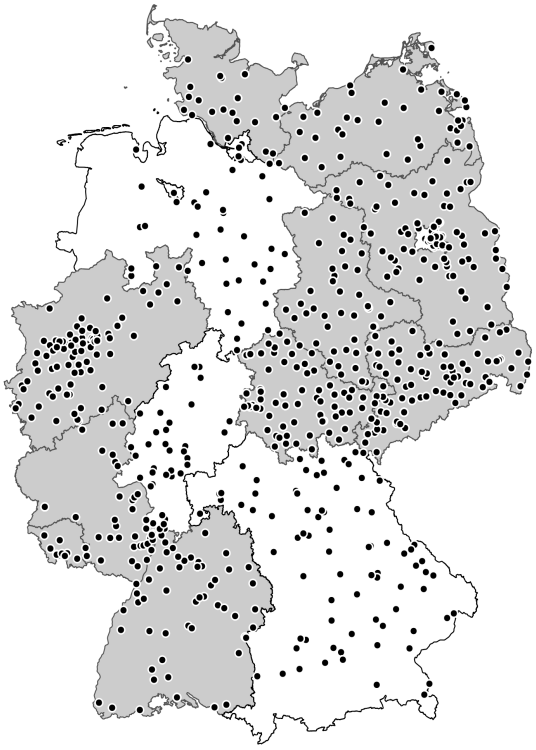
2.3.2 Further Outcomes: Attacks Against Refugees and Antisemitic Incidents

I use data on attacks against refugees from the Amadeu Antonio Foundation.[6] The data covers 3,335 incidents in the period between January 2015 and early 2017. All attacks against refugees

5. According to §104 of Germany's parliamentary rules of procedure, members of parliament can request information from the government using so-called "small enquiry". The answers to these requests are made public. A group of mostly left-wing members of parliament has posed the following question to the government every quarter since 2005: How many far-right demonstrations or other public events by right-wing extremist took place in the quarter?

6. The data is publicly available in the Harvard dataverse

Figure 2.1: Map: Far-Right Protest Marches between 2013 and 2017 federal elections



Notes: The shaded area indicates states with state elections between the 2013-2017 federal elections that are not city states. Black dots represent occurrences of far-right protests. Digitized data from the German Parliament.

feature a brief description. For example, it is noted that at 3rd January 2017, in a municipality called Alsbach-Hänlein in the state of Hesse,

> "unknown people threw a Molotov cocktail on an asylum seekers' hotel. The residents managed to extinguish the fire before the firefighters arrived. The building took damage from the fire, but nobody was injured."

In general, attacks against refugees comprise five categories of incidents: Property damage to refugee homes (2,226 incidents), assault (534), anti-refugee demonstrations (339), and arson (225). 11 incidents are classified as suspected cases still under investigation. I use the geolocations provided for each incident to geographically link the incidents to German municipalities. Figure G7 in the appendix provides a geographic overview of the data which covers anti-refugee incidents from 1st January 2015 until 13th February 2017. As in the case of far-right protests, the number of attacks against refugees culminates at the end of 2015 and declines afterwards (see Appendix Figure G5 and Figure G6).

Finally, I web-scrape and digitize entries in the chronicle of antisemitic incidents from the Amadeu Antonio foundation's website.[7] The chronicle reports 423 antisemitic incidents between the 2013 and 2017 federal elections. Incidents include violent assaults, verbal insults, and antisemitic scribbling that were reported in local newspapers. The peak around July 2014 coincides with demonstrations in Germany in the context of Israel's military offensive in the Gaza Strip (see Appendix Figure G5 and Figure G6).

2.3.3 Treatment: AfD Shock Variable

I exploit data from a quasi-natural experiment: between the federal elections in 2013 and 2017, 13 out of 16 German states held elections (see Appendix Figure G1). Starting from 4.7 percent in the federal election of 2013, the AfD gained up to 24.4 percent in Saxony-Anhalt in 2016 and became the third largest force in the German parliament in the federal election of 2017. I restrict my sample to monthly observations of those 7,620 municipalities in states with state elections, and omit the single-municipality city states of Hamburg, Bremen, and Berlin. There are two reasons for ignoring city states. One is that they are administratively only single municipalities. Another is that, even if one wanted to perform an analysis of far-right protests at the level of constituencies within city states, far-right protests would probably be held at certain locations within the city states for reasons of visibility. Easy access of these locations via public transport is likely to render any eventual association between vote shares at constituencies and far-right activism insignificant.

I use the difference of the state-level AfD result to a municipality's AfD result as a measure of the information shock of populist success. Note that I construct the variable to be time-constant

7. The chronicle is publicly available on www.amadeu-antonio-stiftung.de/chronik/

and defined at the point in time directly after a state election. As municipalities with below state-average AfD vote share receive the populist information shock positively, I call this variable "AfD Shock". It is defined as follows:

$$\text{AfD Shock}_i = \Delta(\text{AfD}_{s(i)} - \text{AfD}_i)$$

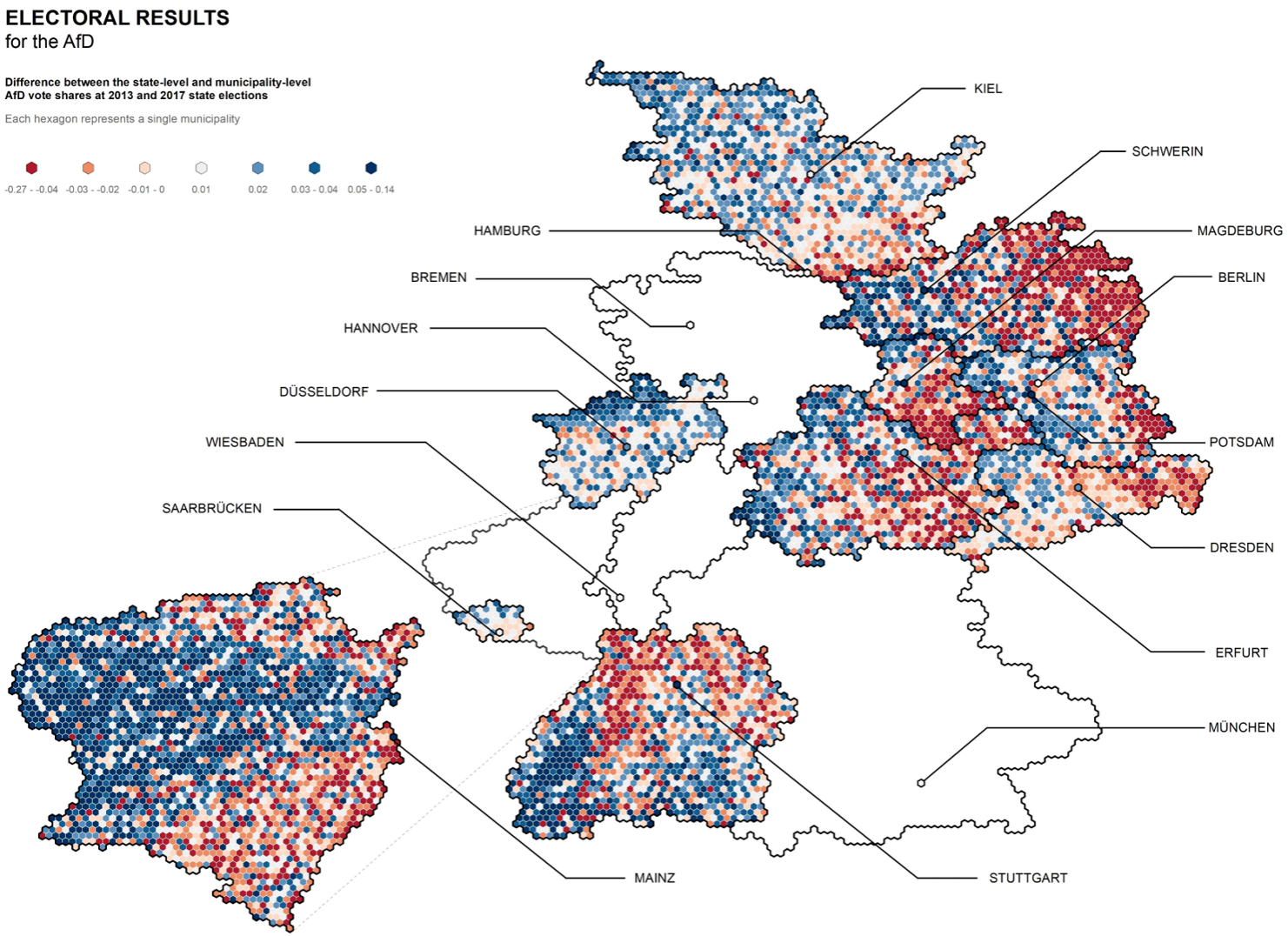
where AfD is the AfD vote share, i indexes a municipality, an $s(i)$ a state.

Table 2.1 presents summary statistics. The mean of the AfD Shock variable is -0.002 and not exactly zero because – apart from averaging over municipalities and not over the population – some municipalities have very high AfD support. As negative vote shares are impossible, there are no equivalent municipalities with equally low AfD vote shares to upset the impact on the mean of the state-municipality difference. Figure 2.2 maps the AfD shock variable for German municipalities. Each hexagon represents one municipality and blank states are states without state elections in the period between the 2013 and 2017 federal elections.

What are correlates of the AfD Shock variable? Table 2.2 presents means of various socioeconomic characteristics for municipalities with negative and positive AfD Shock. It turns out that municipalities above and below their state-average populist vote show some differences. More liberal places with below-average AfD support tend to have fewer inhabitants, less refugees, and slightly higher trade exposure. However, they do not differ significantly from the municipalities with above state-average populist support in these respects. Significant differences are found in employment and human capital characteristics. For example, 2.8 percent of people in the average municipality with populist support above state average were unemployed in 2016, compared to 2.3 percent in more liberal municipalities, amounting to roughly a fourth of a standard deviation in difference.[8] Further, there is some persistence in voting for the AfD despite the party's rightward shift after July 2015. Municipalities with below-average AfD support at a state election have already been voting on average 1.6 percentage points less for the AfD in the 2013 federal election. The persistence holds also with regard to relative AfD support of municipalities within states: municipalities with positive AfD Shock at state elections were on average already 1.5 percentage points below the state average AfD vote share in the 2013 federal election.

8. Note that I calculate unemployment as share of unemployed among total population.

Figure 2.2: Map: AfD Shock variable for German municipalities



Notes: Map shows the distribution of the AfD Shock variable. See subsection 2.3.3 for detailed explanation.

Table 2.1: Summary Statistics

	(1) Mean	(2) Std.Dev.	(3) Min	(4) Max	(5) Obs.
Panel A: *Voting Variables*					
Municipal AfD vote share, state election	0.126	0.066	0.000	0.482	373,380
State-Average AfD vote share, state election	0.124	0.045	0.059	0.243	373,380
AfD Shock: Δ State-Municipal AfD, state election	-0.002	0.041	-0.274	0.137	373,380
Panel B: *Outcome Variables*					
Far-right protests	0.003	0.067	0	8	373,380
Attacks against refugees	0.011	0.120	0	5	198,120
Antisemitic incidents	0.001	0.026	0	2	373,380
#AfD Tweets	0.020	0.525	0	121	373,380
Pro-AfD #AfD Tweets, manually classified	0.001	0.034	0	7	373,380
Anti-AfD #AfD Tweets, manually classified	0.002	0.066	0	10	373,380
Panel C: *Control Variables*					
Δ Unemployment, 2017-2007	-0.017	0.027	-0.167	0.069	373,380
Unemployment, 2016	0.026	0.015	0.000	0.138	371,910
Trade exposure	-1.011	1.409	-6.390	12.940	372,057
Share people with university degree	0.097	0.029	0.055	0.400	373,380
Municipal AfD vote share, 2013 federal election	0.051	0.022	0.000	0.278	373,331
Refugees	17	236	0	11,997	259,391
Population, 2015	6,461	28,089	9	1,060,582	373,380

Notes: Sample includes monthly observations for German municipalities in states with state elections between the 2013 and 2017 federal elections, where $T = 49$ (months) and $N = 7,620$ (municipalities in states with state election in 2013-2017, except city states). Voting outcomes for each municipality are time-constant and defined at the time of the state election. The AfD Shock variable is defined as time-constant difference of a state election AfD result with the municipal AfD result (positive values denoting higher state averages than municipal AfD vote shares at state elections).

2.3.4 Asymmetric Information Assumption

The above section has shown the construction of the AfD Shock variable as the difference between state average and municipal populist vote shares. This variable captures the information shock of populist electoral success on individual beliefs if the following assumption holds:

- **Assumption 1.1:** People generally hold quite accurate beliefs about the attitudes of other people in their immediate surroundings (i.e. in their municipality). In particular, they have a good feeling of how many people are extremist or xenophobe.
- **Assumption 1.2:** People generally hold quite imprecise or erroneous beliefs about the at-

Table 2.2: Correlates of AfD Shock

	(1) Negative Shock	(2) Positive Shock	(3) Diff(2)-(1)
Population, 2015	6679.3	6271.6	-407.7
	[26170.5]	[29652.6]	[645.4]
Refugees	30.1	26.8	-3.35
	[335.3]	[320.0]	[9.39]
Share people with university degree	0.10	0.094	-0.0063***
	[0.026]	[0.031]	[0.00067]
Δ Unemployment, 2017-2007	-0.021	-0.014	0.0066***
	[0.030]	[0.025]	[0.00063]
Unemployment, 2016	0.028	0.023	-0.0049***
	[0.016]	[0.013]	[0.00034]
Trade exposure	-1.04	-0.99	0.045
	[1.48]	[1.35]	[0.032]
Municipal AfD vote share, 2013 federal election	0.059	0.043	-0.016***
	[0.024]	[0.018]	[0.00048]
AfD Shock at 2013 federal election	-0.0068	0.0077	0.015***
	[0.022]	[0.017]	[0.00044]
Observations	3,532	4,088	7,620

Notes: Sample includes a cross-section of German municipalities at state elections between the 2013 and 2017 federal elections, except city states. The AfD Shock variable is defined as time-constant difference of a state election AfD result with the municipal AfD result (positive values denoting higher state averages than municipal AfD vote shares at state elections). Standard deviations in parentheses. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

titudes of other people in the broader society (i.e. in the federal state). In particular, they lack a good feeling of how many people in the broader society are extremist or xenophobe.

I argue that this assumption is credible. First, there is secondary literature that supports both parts of the assumption. In particular, several papers find a "neighbourhood effect" when it comes to attitudes at a very local level (Tolsma, Van der Meer, and Gesthuizen, 2009; Gundelach and Freitag, 2014). Gundelach and Freitag (2014) argue that "individuals generally spend a lot of time in their neighbourhood, so that trusting attitudes are thought to be most affected by this immediate social context and less so by larger regional or even national contexts." Further, several studies document effects which are present at the local level, but not at a broader level (Hooghe et al., 2009; Quillian, 1995).

Second, assumption 1.2 is supported by the fact that leading German polling institutes often failed to predict AfD outcomes (Bergmann and Diermeier, 2017). Figure 2.3 presents deviations between polling and actual AfD vote shares for state elections between the 2013 and 2017 federal elections, excluding city states. Until late 2016, the actual state-level AfD vote shares were severely underestimated in all state elections by roughly 20 percent. After the election in Mecklenburg-Western Pomerania, polling results tend to reflect the actual AfD vote shares well.

One might object that AfD vote shares of the 2013 federal election seem to have at least some explanatory power for the information shock. However, Table G1 in the appendix shows that – consistent with the AfD's shift to the right after July 2015 – correlations of 2013 AfD vote shares, as well as their deviations from state averages, are less correlated with both AfD vote shares and information shocks in later state elections. This is in line with my finding that the effect of populist success is most pronounced in the period after the AfD's turn into a right-wing populist party.[9]

2.3.5 Twitter Data

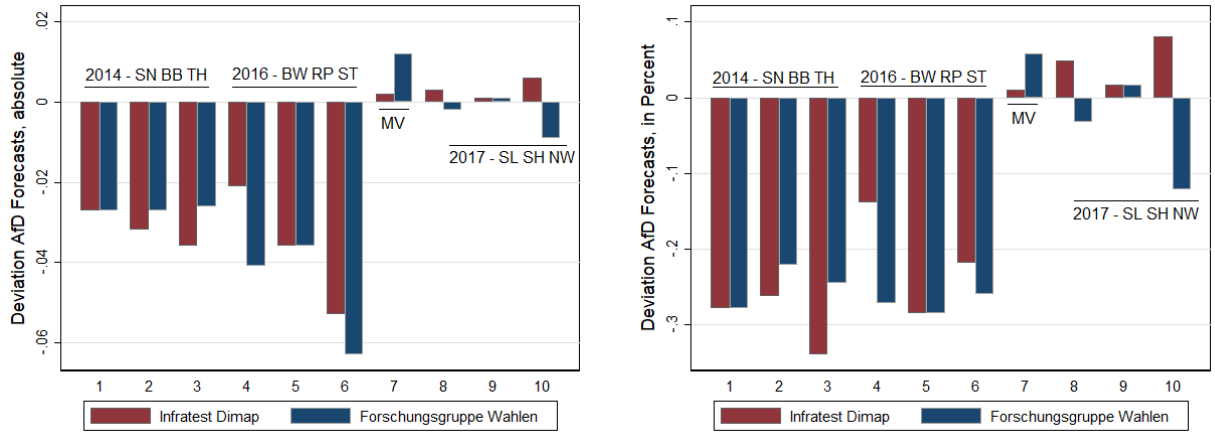
Purchasing a twitter premium API account and using python scraping allowed me to obtain geolocated tweets in German municipalities for the period between the 2013 and 2017 federal elections[10]. Similarly to how Müller and Schwarz (2019) handle Facebook posts, I restrict tweets to those containing the hashtag #AfD (about 22,000). I geographically match tweets to the AfD Shock variable of the respective municipality (see previous section).

As Jungherr and Theocharis (2017) point out, it is important to consider the context of digital trace data in order to ask meaningful questions because, as shown by Posegga and Jungherr

9. Fourth, even if one takes the "pure" municipal specification instead of a municipality's difference to the state average, the findings remain very similar. Appendix Table G2 shows regression results where the municipal AfD support is used instead of the AfD Shock variable.

10. Note that about 1 percent of all tweets in Germany are geolocalisable (Scheffler, 2014). Several studies show that geolocalisable tweets are good predictors of various kinds of social phenomena, ranging from park visitation (Hamstead et al., 2018) and unhealthy food consumption (Widener and Li, 2014) to crimes (Ristea, Andresen, and Leitner, 2018) and illnesses (Young et al., 2018). However, users of geoservices on Twitter have been found to exhibit different characteristics than Twitter users who do not enable geoservices (Sloan and Morgan, 2015).

Figure 2.3: Deviation of Polling from AfD Vote Shares – Absolute and in Percent



Notes: Graphs plot deviations (absolute and in percent) of two different polling institutes from actual AfD vote share for the 10 state elections between 2013 and 2017 federal elections, except city states. Own calculations based on data from wahlrecht.de. Polling forecasts are taken from the last polling result before the respective election.

(2019), tweets might not necessarily reflect political agendas. To cross-validate the content of the geolocalised #AfD tweets, I thus also obtain the (non-geolocalised) universe of tweets from German political parties.

2.3.6 Control Variables and Additional Data

The arguably most salient political event between the 2013 and 2017 federal elections is the so-called "Syrian refugee crisis". As most far-right protests seem to be linked to immigration topics according to their slogans, I purchase data on the number of refugees in each municipality for every month in my sample from the Federal Employment Agency (Bundesagentur für Arbeit). As refugees are not directly observable, I compose a proxy following the Federal Employment Agency's instructions in which asylum seekers should be classified as refugees if they come from one of the so-called "asylum countries" Afghanistan, Eritrea, Irak, Iran, Nigeria, Pakistan, Somalia, or Syria.

Two often debated factors behind the occurrence of far-right protest marches are unemployment and job losses. I obtain municipality-level data on levels of and 10-year changes in unemployment rates from the Federal Employment Agency (Bundesagentur für Arbeit). A municipality's population in 2015 is added to my set of controls from the Federal Bureau for Statistics (destatis). Additionally, to account for the losses in manufacturing jobs as studied by Dauth, Findeisen, and Suedekum (2014), I match a time-constant variable on trade exposure from the county level to the municipalities, defined as the difference between import and export exposure. Finally, I use the share of people with tertiary education in a given county as a proxy for human capital, obtained from destatis.

I complement my data with the restricted-use data from the German Longitudinal Election Study (GLES) to examine how attitudes might change as result of information shocks from populist electoral success.[11] I also obtain restricted-use access to the German Socio-Economic Panel (GSOEP) to study patterns of heterogeneity. Finally, I compile data on polling forecasts from wahlrecht.de.

2.4 Econometric Setting

To study the local impact of populist electoral results on far-right protests, I set up a simple difference-in-difference design as follows:

$$\begin{aligned} Y_{i,t} = \beta_0 + \delta \left(\text{AfD Shock}_i \times post_{s(i),t} \right) + \beta_1 \text{AfD Shock}_i + \beta_2 post_{s(i),t} \\ + \beta_3 \text{AfD 2013}_i + \gamma X_i + \psi Z_{i,t} + \zeta_{s(i)} + \xi_t + \eta_{r(t)} + \epsilon_{i,t} \end{aligned}$$

where $Y_{i,t}$ are far-right protests in municipality i in state $s(i)$ at month t, and at relative-to-election month $r(t)$. The variable $post_{s(i),t}$ is a dummy for after a state election, AfD Shock$_i$ is a measure of information shock from populist electoral success at the time of state election, AfD 2013$_i$ is the AfD vote share of the municipality at the 2013 federal election, X_i and $Z_{i,t}$ are time-constant and time-varying control variables. $\zeta_{s(i)}$, ξ_t, and $\eta_{r(t)}$ are fixed effects with respect to geography (state, county, or municipality), with respect to time (calendar month) and with respect to distance to state election (month before or after state election). The coefficient of interest judging the difference in outcomes due to information shocks about populist success after an election is δ. Note that time-constant variables will be omitted when municipality fixed effects are included. In this way, any idiosyncratic time-constant difference between municipalities is accounted for by municipality fixed effects. Moreover, macroeconomic shocks affecting all geographic entities – such as the influx of refugees around 2015 – are captured by the month fixed effects. The relative month fixed effects account for differences in outcomes specific to certain months before or after state elections. In addition to fixed effects specifications, I also use an event study design. Here, I normalize my data to include 2-year windows around state elections. I show that the result is robust to different choices of time windows, though. I employ OLS regressions to ease interpretation, but also show robustness to poisson estimation.[12]

11. The data is available without municipality identifiers on `www.gesis.org`.

12. Poisson estimation takes into account the fact that number of far-right protests is a count variable and exhibits many zero values, which leads to bias in OLS. Moreover, Table 2.1 shows that the mean and variance of far-right protests are not extremely different, making poisson regressions a natural choice. The sample size in Poisson estimations with high dimensional fixed effects is greatly reduced because observations are dropped which are either

2.5 Results

2.5.1 Main Result: Far-Right Protests

Table 2.3 presents the main result. Column 1 of Panel A reports a simple difference in difference estimate of the coefficient δ from the regression model in section 2.4. Here, the effect of the AfD Shock after state elections on the number of far-right protests is estimated without any fixed effect or control variables. In this first specification, the likelihood of an additional far-right protest incresases by 0.253 percentage points on average if a municipality has an AfD support 10 percentage points below state average. This is a sizeable effect and amounts to an 84 percent (0.00253/0.003) increase of the mean of far-right protests.

Including different combinations of state, month, relative month, and even county or municipality fixed effects reduces the coefficient size by about 50 percent. Notably, the coefficient of interest retains significance at the five percent level in all specifications - even in the most demanding specification using month, relative month, and municipality fixed effects. When control variables are added in panel B, the magnitude of the coefficient is reduced only slightly: an 0.1 increase in the AfD Shock is related to a roughly 0.1 percentage points higher likelihood of an additional far-right protest, i.e. increases the mean of far-right protests by roughly a third (0.001/0.003). With exception of column 6, all specifications with controls retain significance at the five percent level.[13] [14]

So far, the linear models might have failed to adequately deal with the presence of many zeros of the right-skewed count variable of far-right protests. Adding to the results from linear models, Appendix Table G4 implements poisson regression. The result again remains robust to various combinations of state, county, municipality, month, and relative month fixed effects (columns 1, 2, and 5). It is hence not driven by a specific location or timing of an election. To check whether the effect is due to outliers with a multitude of far-right protests in a given month, I perform the same regression as in column 2 with a binary dependent variable (column 3). The result remains unchanged, lending credence to a general pattern which is not due to outliers.[15]

Figure 2.4 illustrates the dynamics of this finding. In the left graph, the line represents monthly interaction coefficients of the AfD Shock variable before and after state elections. The omitted category is the month of the election as indicated by the vertical blue line. Before an election, the

singletons or separated by the fixed effects. However, even the poisson results with relatively small number of observations also show significance of the propagation effect, suggesting that the effect does not stem from singleton observations (Correia, 2015).

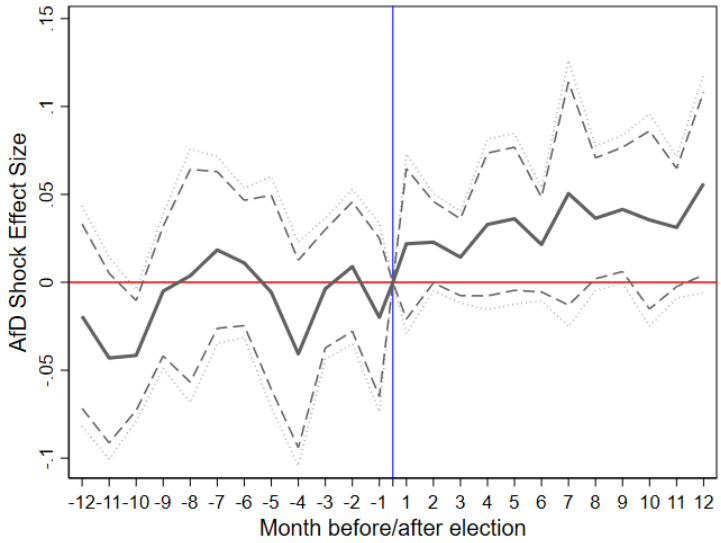
13. Appendix Table G3 shows that when restricting the sample to 2-year windows around state elections, the magnitude of the effect in the linear models remains qualitatively unaffected. Further, Appendix Figure G8 shows how the coefficient of interest changes when different time windows are employed.

14. Results remain qualitatively unchanged when interaction terms of control variables with the post election dummy are included. Results are available upon request.

15. Note that the coefficient size is not comparable across columns because of either different number of observations or binarization of the dependent variables. Further, magnitudes of Poisson coefficients are to be interpreted as expected increases in log counts, so are not directly comparable to OLS coefficients.

confidence intervals almost always include zero and no clear pre-treatment trend is observable.[16] After state elections, interaction coefficients increase markedly. Appendix Figure G10 shows long-run dynamics. Following Borusyak and Jaravel (2017), I only include interactions of quarterly dummies after elections with the AfD Shock variable. This long-run analysis should be interpreted with caution because bigger time windows around state elections will include other elections, thus leading to potential bias. However, the graph suggests that the treatment effect becomes smaller and insignificant after the first three quarters.

Figure 2.4: Event Study: Far-Right Protests



Notes: Graph plots regression coefficients of the interaction of AfD Shock with month dummies relative to state election. Regressions include state, month, and relative month fixed effects, as well as AfD Shock. Standard errors are clustered at the state-times-month level. Sample consists of 2-year monthly windows for German municipalities in states with state elections between the 2013-2017 federal elections, except city states. Omitted category is the election month.

16. Pre-trends seem to be informative as the number of far-right protests before state elections are not particularly small; see Appendix Figure G9 for an overview of far-right protest frequencies in treatment and control groups in the relevant time window.

Table 2.3: Main Results (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: No Controls											
AfD Shock × Post	0.0253***	0.0302***	0.0271***	0.0314***	0.0309***	0.0104***	0.0116***	0.0110***	0.0083**	0.0097**	0.0090**
	(0.0055)	(0.0058)	(0.0057)	(0.0060)	(0.0060)	(0.0036)	(0.0039)	(0.0039)	(0.0038)	(0.0042)	(0.0042)
Observations	373331	373331	373331	373331	373331	373331	373331	373331	373331	373331	373331
Panel B: With Controls											
AfD Shock × Post	0.0121***	0.0134***	0.0132***	0.0139***	0.0146***	0.0066*	0.0072**	0.0077**	0.0090**	0.0097**	0.0105**
	(0.0039)	(0.0039)	(0.0040)	(0.0040)	(0.0040)	(0.0034)	(0.0035)	(0.0035)	(0.0039)	(0.0041)	(0.0041)
State FEs		✓		✓	✓						
Month FEs			✓	✓	✓		✓	✓		✓	✓
Relative Month FEs					✓			✓			✓
County FEs						✓	✓	✓			
Municipality FEs									✓	✓	✓
Observations	256886	256886	256886	256886	256886	256886	256886	256886	256753	256753	256753

Notes: Table reports coefficients and standard errors from linear regressions. The dependent variable is the number of far-right protest marches. Each column adds a different combination of fixed effects. Panel A reports results without control variables, apart from 2013 AfD vote shares. Regressions in panel B include population, unemployment level, unemployment change, number of refugees, trade exposure and human capital controls, as well as 2013 AfD vote shares. The sample comprises $T = 49$ (months) and $N = 7,620$ (municipalities in states with state election in 2013-2017, except city states). Standard errors are clustered at the state-times-month level. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

2.5.2 The Effect of Polling Underestimation

My main result adds to the existing literature by documenting an empowerment effect in the context of far-right protests in Germany. In the following two sections I go beyond this contribution and show that the empowerment effect depends on an element of surprise.

Table 2.4 shows that the propagation effect is heterogeneous with respect to polling underestimation (see Figure 2.3 for details on polling underestimation). The idea is that if more severe underestimation is associated with a stronger information shock and positive updating of beliefs, the propagation effect should be stronger in state elections with higher underestimation of the AfD vote share. Column 1 of Table 2.4 repeats my main result. Columns 2-4 split the sample into three parts according to underestimation by polling institutes and before/after the rightward shift of the AfD in July 2015. The propagation effect is strong and significant in states with high underestimation (column 3), regardless of a sample restriction to 2-year windows around elections (Panel B), or demanding fixed effects specifications (Panel C). In the 2014 period – when the AfD was a Eurosceptic, but not openly right-wing populist party (Arzheimer and Berning, 2019) – the effect size is much smaller and insignificant once fixed effects or sample restrictions are employed (column 1). Column 4 reports results for state elections in which the AfD election was forecasted with only minor deviations. Here, the effect is negligible and very close to zero (column 4).

These results suggest that the empowerment effect from populist success depends on an element of surprise, as captured by the underestimation of polling institutes. They shed light on a condition for manifestation of an empowerment effect as studied in Bursztyn, Egorov, and Fiorin (2019). Social norms, so it seems, change only if information shocks about other people's attitudes are unexpected and lead to an update in beliefs.

2.5.3 Heterogeneity: Polarisation and Fractionalisation

I argue that the propagation effect of populism's success on far-right protests in more liberal areas depends on an element of surprise. Yet, in what sense are individuals surprised by election outcomes? So far, my analysis has tried to capture surprising information shocks in two ways. First, the AfD Shock variable conceptualizes the information shock as the asymmetry between uncertainty about attitudes at the local and federal state level (see subsection 2.3.4). Second, surprise is captured by underestimation of the AfD's results by official polling institutes. Another possibility is that individuals might be more surprised by the AfD's success if they live in more homogeneous communities which do not reflect the more heterogeneous political landscape of the broader society. Arguably, an unexpectedly high vote share of a marginal party at the state level could be more surprising in municipalities where marginal parties receive very few or almost no votes at all. For example, if the local political landscape of a community is dominated by two major parties (low fractionalisation and high polarisation), high support for a marginal party at the state level might represent a larger information shock. On the other hand, seeing a marginal party gain

Table 2.4: Asymmetric Information Effect:
Heterogeneity of Propagation Effect with respect to Underestimation

	(1)	(2)	(3)	(4)
	Baseline All states	2014 Under-estimation	2016 Under-estimation	2017 No U.-estimation
Panel A: State FEs				
AfD Shock × Post	0.0134***	0.0134**	0.0196***	0.0004
	(0.0039)	(0.0063)	(0.0059)	(0.0060)
Observations	256886	66567	116223	74096
Panel B: Event Study Sample				
AfD Shock × Post	0.0151**	0.0012	0.0299**	0.0027
	(0.0068)	(0.0061)	(0.0125)	(0.0113)
Observations	120808	33100	57868	29840
Panel C: Municipality, Month, and Relative Month FEs				
AfD Shock × Post	0.0147**	0.0013	0.0268**	0.0004
	(0.0067)	(0.0063)	(0.0133)	(0.0083)
Observations	120638	33085	57794	29759

Notes: Table shows coefficients and standard errors from OLS regressions. Dependent variable is the number of far-right protests. Columns 2-4 restrict the sample to state elections according to underestimation of AfD vote share by polling institutes (see Figure 2.3) and according to before and after the rightward shift of the AfD (July 2015): Column 2 includes Saxony, Brandenburg, Thueringia. Column 3 includes Baden-Württemberg, Rhineland-Palatine, and Saxony-Anhalt. Column 4 includes Mecklenburg-Western Pomerania, Saarland, Schleswig-Holstein, and North Rhine-Westphalia. All regressions include controls for refugees, population, unemployment, trade exposure, and human capital. Panel A regressions include state fixed effects. Panel B uses an event-study sample that only includes two-year time windows around state elections and includes state fixed effects. Panel C regressions include municipality fixed effects, as well as month and relative month fixed effects, and use the event-study sample of two-year windows around state elections. Standard errors are clustered at the state-times-month level. Mean of dependent variable in Panel C is 0.0016, 0.0052, 0.0011, and 0.0027 in columns 1 to 4 respectively. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

high vote shares in state elections might not be as surprising for communities with many successful marginal parties (high fractionalisation, low polarisation).

To assess this hypothesis, I construct indices of polarisation and fractionalisation of party vote shares at the municipality level following Montalvo and Reynal-Querol (2005):

$$\text{Fractionalisation}_i = 1 - \sum_{p=1}^{P} \pi_{i,p}^2 = \sum_{p=1}^{P} \pi_{i,p}(1 - \pi_{i,p})$$

$$\text{Polarisation}_i = 1 - \sum_{p=1}^{P} \left(\frac{1/2 - \pi_{i,p}}{1/2} \right)^2 \pi_{i,p} = 4 \sum_{p=1}^{P} \pi_{i,p}^2 (1 - \pi_{i,p})$$

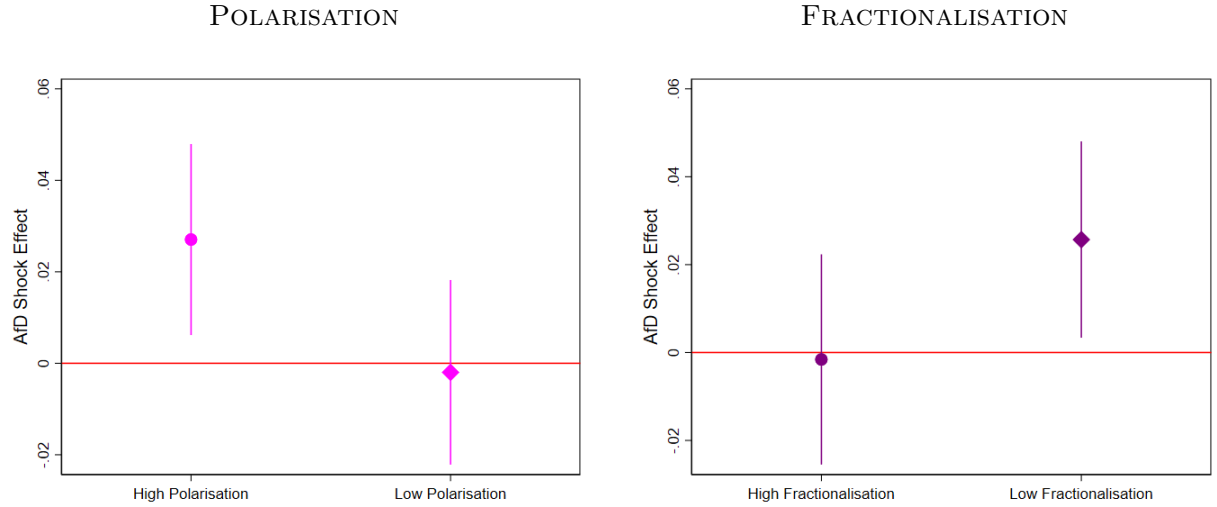
where $\pi_{i,p}$ are vote shares for party p in municipality i. The fractionalisation index in my context can be interpreted as the probability that two randomly selected votes within one municipality were not cast for the same party. A high value of the fractionalisation index thus indicates the presence of small minority groups, such as the AfD before September 2017. The polarisation index shows how far away the distribution of vote shares is from the highest level of polarisation – a bipolar 50-50 percent split. Higher levels in the polarisation index suggest the presence of large groups – such as the SPD and CDU – but also hint at the absence of small minority groups.

Appendix Figure G11 presents descriptive statistics of the indices. Note that the indices of fractionalisation and polarisation are negatively correlated (ρ = -0.4354, p<0.01), as shown in the two graphs at the bottom. This is intuitive as they capture the relative presence and absence of small minority parties. The top two graphs show their density distributions for the entire sample and within each state. Both means and distributions of indices vary substantially across federal states. Therefore, and because I use staggered elections of federal states, I create indicator variables which turn one if a municipality has a higher value of the fractionalisation or polarisation index than the median municipality in its state. I expect the coefficient of interest to be particularly high in municipalities with high polarisation and low fractionalisation. On the other hand, individuals living in municipalities with low polarisation and high fractionalisation of vote shares might not be surprised by the AfD's success.

Figure 2.5 shows that the propagation effect is heterogeneous with respect to polarisation and fractionalisation of vote shares. The main coefficient of interest – the interaction between AfD Shock and a dummy for after a state election – is particularly big in the sample of municipalities with high polarisation and low fractionalisation of vote shares. On the other hand, it is almost exactly zero for municipalities with low polarisation and high fractionalisation. Note that this result is not an artefact of unequal sample sizes as the split by median index values within states ensures almost identical number of observations for high/low polarisation or fractionalisation. Yet, one has to be cautious with a causal interpretation here as the sample split might be based on an

endogenous variable.

Figure 2.5: Effect Heterogeneity: Polarisation and Fractionalisation



Notes: Graph plots interaction coefficients of interest (AfD Shock times after state election) and 95% confidence intervals from OLS regressions for different samples of municipalities, split within states into above- and below median value of fractionalisation or polarisation indices. Monthly observations of municipalities in 2-year windows around state elections between the 2013 and 2017 federal elections. Municipalities are from states with state elections between the 2013 and 2017 federal elections, except city-states. All regressions include municipality, month, and relative month fixed effects, as well as time-varying controls for refugees. Standard errors are clustered at the state-times-month level. See Table 2.5 for a regression table with interaction effects.

Table 2.5 presents the results in a regression set-up. Here, the main information shock variable is interacted with indicators for above or below median values of the fractionalisation and polarisation index within a given state.[17] Column 1 repeats the main result with municipality, month, and relative month fixed effects using the event study sample. Here, a 0.1 increase in the AfD shock is related to a 0.147 percentage points higher likelihood of an additional far-right protest after a state election. Yet, the following columns show that this relationship is much stronger for municipalities which do not have many marginal parties. The marginal effect of the AfD shock on far-right protests after state elections is almost doubled in municipalities with low fractionalisation and high polarisation. Although the indices are highly correlated, column 4 suggests that polarisation is the key driver of this result. This may be due to the fact that in the polarisation index the size of the group also enters the weight of the probabilities of two votes being cast for different parties. The result is that votes for small parties – such as the AfD – contribute proportionally more to the index of polarisation than their relative size, but less than their relative size to the index of fractionalisation (Montalvo and Reynal-Querol, 2005).

This evidence sheds additional light on the importance of a surprise element for an empower-

17. Interactions of these polarisation and fractionalisation indicators with the post dummy and AfD Shock variable are also included.

Table 2.5: Polarisation and Fractionalisation

	(1)	(2)	(3)	(4)
AfD Shock × Post	0.0147**	0.0254**	-0.0016	0.0097
	(0.0067)	(0.0110)	(0.0100)	(0.0085)
AfD Shock × Post × High Fractionalisation		-0.0266*		-0.0186
		(0.0154)		(0.0120)
AfD Shock × Post × High Polarisation			0.0278**	0.0212**
			(0.0138)	(0.0099)
Observations	120597	120597	120597	120597

Notes:´ Table shows coefficients and standard errors from OLS regressions. Dependent variable is the number of far-right protests. All regressions include municipality, month, and relative month fixed effects, and control for time-varying number of refugees. Sample consists of German municipalities in states with state elections between the 2013 and 2017 federal elections, except city states. Monthly observations in 2-year windows around state elections. High Polarisation and High Fractionalisation variables are indicators for above-median values of polarisation and fractionalisation indices within a given state as explained in subsection 2.5.3. Standard errors are clustered at the state-times-month level. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

ment effect as described in Bursztyn, Egorov, and Fiorin (2019). However, the heterogeneity of effects should not be seen as causal because variation in polarisation or fractionalisation might be endogenous. The analysis is based on the intuition that if polarisation is low and fractionalisation is high, the success of a marginal party at the state level comes as a bigger surprise. The correlational evidence points to a much stronger empowerment effect in this case.

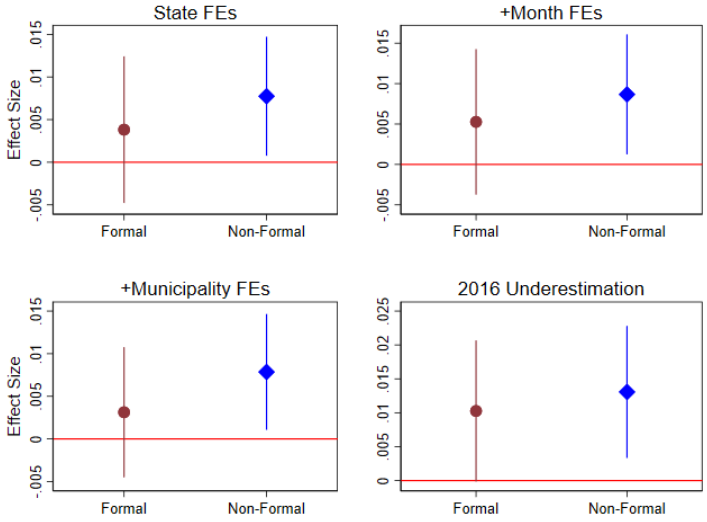
2.5.4 Political Targeting vs. Social Acceptability

An alternative interpretation of my finding could be a story of political targeting. For strategical reasons, formal right-wing extremist organizations might target municipalities with below-average AfD results more after state elections. The increase in far-right protests in below-average AfD municipalities after state elections might thus not be due to empowered xenophobes showing their "true colours", but due to some supra-regional organizations targeting these areas, maybe in an effort to gain more voters. Such top-down protests would probably not stem from a reduction in social stigma, but rather from strategic campaigning. On the other hand, the increase in far-right protests could be driven by bottom-up protests, organized not by supra-regional organizations, but in a bottom-up fashion by local far-right extremists.

I examine whether formal organizations tend to organize more far-right protests after elections at below-average AfD municipalities than people without affiliation to formal organizations. It is interesting that about half of all far-right protests are organized by people without affiliation

to a formal movement or party.[18] Figure 2.6 shows that propagation effect sizes are somewhat higher and significant for non-formal protests as outcome variable, even if month and municipality fixed effects are taken into account. The bottom right plot only considers the 2016 period where the information shock was biggest. Although far-right protests by formal organizers increase in more liberal areas after state elections as well, the effect is more pronounced for protests by non-formal organizers. In summary, an analysis of organizations behind the far-right protests makes an alternative explanation of political targeting seem less convincing.

Figure 2.6: Formal vs. Non-Formal Organizers of Far-Right Protests



Notes: Graph plots interaction coefficients of interest (AfD Shock times after state election) and 95% confidence intervals from OLS regressions. The dependent variable is a count of formally organized far-right protests ("formal"), and protests which are organized without attribution to a formal right-wing extremist group ("non-formal"). Formal right-wing extremist groups include "Der III. Weg", the far-right party NPD ("Nationale Partei Deutschlands"), "Die Rechte", "PEGIDA", and the"Pro-Movement". Sample consists of monthly observations of German municipalities in 2-year windows around state elections between the 2013 and 2017 federal elections, with exception of the bottom right graph that includes 2-year windows around states during the 2016 underestimation period (see Figure 2.3). Regression specifications are as indicated in graph headers. All regressions cluster standard errors at the state-times-month level.

2.5.5 Mechanism: No Change in Attitudes

Which mechanism is at work to generate the increase in far-right protests in municipalities with information shocks? I argue that the information shock leads to a decrease in social stigma in more liberal areas, which in turn induces xenophobes to organize far-right protests and take to the streets. A competing mechanism could be a change in attitudes as a result of the information shock. Several

18. See Appendix Figure G3 for a monthly overview of frequencies and organizers of far-right protests.

studies show that individuals might change their attitudes when provided with certain information (Sunstein et al., 2016; Dinas and Fouka, 2018; Giani and Méon, 2019; Gerling and Kellermann, 2019; Grigorieff, Roth, and Ubfal, 2020). However, other studies document that attitudes can remain stable for long periods (Voigtländer and Voth, 2012; Becker et al., 2016; Guiso, Sapienza, and Zingales, 2016). To distinguish between these competing mechanisms, I obtain access to the restricted-use German Longitudinal Election Study with postal code geographic indicators (GLES, 2019). I geographically match the municipalities' AfD Shock values to postal codes using territorial shares. My goal is to examine whether individuals change their attitudes when confronted with surprisingly high populist vote shares in the broader society.

Table 2.6 presents the results. I compare responses to three different questions between areas with higher and lower information shocks before and after state elections. The column headers indicate which states held elections in the respective period (city states in brackets are not represented in the sample). The evidence suggests that attitudes did not change as a result of the information shock, but the perceived position of the AfD party did to some extent. Panel A suggests that attitudes towards integration of immigrants are not systematically different in areas with stronger information shocks after state elections. Panel B indicates that the number of people leaning towards the AfD did not change with information shocks from state elections. Acknowledge, however, that measurement error is very likely to be present due to the geographic matching of municipalities to postal code areas. Yet, if anything, the coefficients in Panels A and B point towards even more liberal attitudes in more liberal areas after state elections. The only significant difference for areas with higher information shocks is in the left-right assessment of the AfD (Panel C). The dependent variable in Panel C is constructed from the question where one would place the AfD on a left-right spectrum on a scale from 1 (left) to 11 (right). Consistent with the rightward shift of the AfD after July 2015 (Arzheimer and Berning, 2019) and the high underestimation of AfD results in the first state elections in 2016 by polling institutes (see Figure 2.3), the AfD is seen as more right-wing after the first 2016 state elections, and particularly so in places with below average AfD support (Panel C, column 2).[19] This effect seems to be reversed in one state election in East Germany when the polls correctly estimated the AfD's success (column 3)

In summary, survey evidence does not seem to support a shift in attitudes as a result of electoral information shocks about populist success. This is consistent with Cantoni, Hagemeister, and Westcott (2019) who find that attitudes associated with the rise of the AfD in Germany do not change much between 2013 and 2017. If anything, the evidence points towards a shift in the perceived position of the AfD party on a left-right scale after state elections depending on underestimation by polling institutes.

19. Appendix Figure G12 shows where survey respondents placed the AfD on a left-right spectrum before and after state results.

Table 2.6: Mechanism: AfD positioning changes, but attitudes remain constant

	(1) 2014 Underestimation SN BB TH (HH HB)	(2) 2016 Under-estimation BW RP ST	(3) 2016 No Under-estimation MV (BE)	(4) 2017 No Under-estimation SL SH NW
Panel A: Immigrants must adopt German culture				
AfD Shock × Post	-3.75	-1.34	-1.37	-2.13
	(2.58)	(1.49)	(0.86)	(1.76)
Observations	1651	2004	2014	1335
Panel B: AfD identification				
AfD Shock × Post	-0.58	-0.64	-0.26	-0.22
	(0.46)	(0.85)	(0.25)	(0.35)
Observations	1605	1882	1915	1285
Panel C: Left-right positioning AfD				
AfD Shock × Post	-25.52	9.18***	-8.36**	6.90
	(23.46)	(2.40)	(3.84)	(6.39)
Observations	1310	1843	1903	1253

Notes: Table reports coefficients and standard errors from regressions using OLS. Dependent variables are as indicated in panel headers (see Appendix Table G6 for details). Data from GLES. Samples consists of PLZ areas before and after state elections (as indicated in column headers). All regressions use state fixed effects. The columns indicate time periods under consideration, including the abbreviations of states with state elections in these periods (see Appendix Figure G1). Survey periods in the respective columns include (1) 09/2013 - 12/2015, (2) 02/2016 - 06/2016, (3) 08(09)/2016 - 12/2016, and (4) 12/2016 - 09/2017. Control Variables include AfD Shock, post, age, age squared, gender, and a measure of political interest. Standard errors are clustered at the state level. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

2.5.6 Antisemitism and Attacks Against Refugees

A valid question concerning the empowerment effect is about its reach. Are other outcomes beyond far-right protest marches affected as well, or is the effect specific to far-right extremism? Given the debate about the AfD's position towards Israel and the Jewish Community in Germany (Grimm, 2019), an interesting outcome are antisemitic incidents. Although there have been some individual cases of antisemitism within the AfD, the party's main political target does not seem to be the Jewish population, but rather Islam in general and asylum seekers in particular (Arzheimer and Berning, 2019). One would thus suspect that antisemitic incidents do not experience the same increase in liberal areas after state elections. Attacks against refugees on the other hand might be expected to react in a similar pattern as far-right protests.

Table 2.7 shows evidence for this hypothesis. Panel A repeats the main result for far-right protests equivalent to the last panel in Table 2.4. Panel B uses antisemitic incidents as dependent variable. Here, the interaction effect is insignificant irrespective of underestimation and, if anything, points in the opposite direction.[20] Panel C in Table 2.7 presents results for attacks against refugees as dependent variable. As expected, the coefficient is positive and significant in the 2016 period with high underestimation. Thus, populist electoral success directed against a Muslim minority and asylum seekers does not seem to induce higher rates of antisemitic incidents. Attacks against refugees on the other hand show a similar pattern as far-right extremist protest marches.

2.5.7 Spill-over Effects

The results so far suggest that the empowerment effect from populist success depends on an element of surprise, as captured by the underestimation of polling institutes and polarisation of the political party options. In addition, one might wonder how the effect depends on many states holding elections at the same point in time. Indeed, three elections in 2016 in which the AfD's result was severely underestimated also happened to be held on the same day (Baden-Württemberg, Rhineland-Palatinate, and Saxony-Anhalt). Two elections in 2014, also with high underestimation, coincided as well (Brandenburg and Thuringia). Could it be that the strong propagation effects for these state elections are to some extent due to their simultaneity? One answer comes from the fact that also in the 2017 period without underestimation, two states held elections in the same month (Schleswig-Holstein and North Rhine-Westphalia). However, as is evident from Table 2.4, the propagation effect in this period is very close to zero and insignificant.

If simultaneity of elections increases the empowerment effect, one would expect to find strong spill-over effects of election information shocks to protest behaviour in other states. It could be that xenophobic individuals in other states perceive populist support in another state as surprisingly high and update beliefs about acceptability of xenophobic behaviour. Intuitively, the reaction to

20. Appendix Figure G13 illustrates that this average coefficient for the time after the election does not mask any dynamics related to the information shock.

Table 2.7: Far-Right Extremism vs. Antisemitism

	(1)	(2)	(3)	(4)
	Baseline All states	2014 Under-estimation	2016 Under-estimation	2017 No U.-estimation
Panel A: Far-Right Protests				
AfD Shock × Post	0.0147**	0.0013	0.0268**	0.0004
	(0.0067)	(0.0063)	(0.0133)	(0.0083)
Observations	120638	33085	57794	29759
Panel B: Antisemitic Incidents				
AfD Shock × Post	-0.0013	-0.0028	0.0004	-0.0021
	(0.0014)	(0.0026)	(0.0010)	(0.0034)
Observations	120638	33085	57794	29759
Panel C: Attacks against Refugees				
AfD Shock × Post	0.0243**	-	0.0246*	0.0259
	(0.0107)	-	(0.0128)	(0.0180)
Observations	87803	12616	56018	19169

Notes: Table shows coefficients and standard errors from OLS regressions. Dependent variables are number of far-right protests (Panel A), antisemitic incidents (Panel B), and attacks against refugees (Panel C). Columns 2-4 restrict the sample to state elections according to underestimation of AfD vote share by polling institutes (see Figure 2.3) and according to before and after the rightward shift of the AfD (July 2015): Column 2 includes Saxony, Brandenburg, Thuringia. Column 3 includes Baden-Württemberg, Rhineland-Palatine, and Saxony-Anhalt. Column 4 includes Mecklenburg-Western Pomerania, Saarland, Schleswig-Holstein, and North Rhine-Westphalia. All regressions include controls for refugees, as well as municipality, month, and relative month fixed effects, and use the event-study sample of two-year windows around state elections. Standard errors are clustered at the state-times-month level. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

another state's electoral information shock might depend on how much individuals regard other states' election results as relevant for social acceptability. For example, geographic proximity might lead to a stronger response. Also, the spill-over effect might again depend on how surprising the election result is in terms of polling underestimation and the rightward shift of the AfD.

Investigating possible spill-over effects comes with two difficulties. The first difficulty concerns the creation of an AfD Shock variable at the municipal level, the second the choice of the time window. In what follows I briefly describe these difficulties and how I aim to solve them.

First, I need to create an AfD Shock variable similar as in subsection 2.3.3 to exploit variation in information shocks between municipalities in other states without elections. However, as I would like to test whether other states without state election experience spill-over effects, I cannot rely on municipal vote shares of these states. The reason is that these states did not hold state elections between September 2013 and September 2017. Therefore, I impute support for the AfD party in states without state elections by fitting a linear prediction for each municipality using its 2013 and 2017 federal AfD vote shares. The top left graph of Appendix Figure G14 illustrates the imputation by the example of Lower Saxony. The solid blue line shows the imputed AfD support for an example municipality. For each month between the 2013 and 2017 federal elections, municipal support of the populist party is imputed by a simple linear fit. Vertical shaded lines indicate times of other states' elections. To construct an AfD Shock variable for spill-over effects at the municipal level, I take the difference between the mean of imputed AfD supports to a municipality's imputed AfD support in a given state at the point of state election.

I validate the imputed AfD Shock values in other states using the AfD vote shares of actual state election results. The bottom two graphs in Appendix Figure G14 show binned scatterplots for the imputed AfD support values and actual vote shares from state elections at the time of the election. It turns out that the imputed AfD support values can predict the actual AfD support at state elections very well. The R squared in a regression of actual AfD vote share on imputed AfD vote share is 59,7 percent without state fixed effects, and 86,1 percent with state fixed effects. The estimated beta coefficient in the latter case equals 1,05. Thus, the linear fit seems to capture the true AfD support reasonably well.

The second difficulty for the estimation of spill-over effects is that state elections are often close to each other in time. As explained in Gerling and Kellermann (2019), one needs to carefully choose a time window that avoids overlap of untreated and treated periods before and after elections. The top right graph in Appendix Figure G14 shows how differences in far-right protests between municipalities with high and low imputed AfD support behave over time. Vertical shaded lines indicate the timing of state elections. Clearly, it almost seems impossible to find an adequate time window around the first two state elections without overlap of control and treatment units. To maximise the information in time windows, I run the analysis on the week-municipality level. I disregard the first and last two state elections because including them would diminish the time window substantially. Further, I treat as one election week the week in between the elections of

Mecklenburg-Western Pomerania and Berlin (which are in fact two weeks apart from each other). This procedure allows me to use a time window of five weeks prior and five weeks after elections.

Table 2.8 presents results on spill-over effects. The coefficient of interest is an interaction of the imputed AfD Shock variable in other states at the time of a state election with a dummy for after the election. Panels A, B, and C use different definitions of "other states". A weak definition (Panel A) includes states which do not hold state elections in the time window around a state election. A strict definition (Panel B) includes states without state elections between the 2013 and 2017 federal elections (Lower Saxony, Bavaria, and Hessia). Panel C only includes adjacent states to test whether spill-over effects are particularly strong in states with closer proximity. As in previous tables, column 1 reports results of all periods and columns 2-4 split the sample according to polling underestimation and the rightward shift of the AfD. All regressions use municipality, week, and relative week fixed effects, to account for any time-constant unobserved heterogeneity across municipalities or macroeconomic conditions affecting all municipalities. A control for the presence of refugees at the municipality-month level is also included.

Overall, the evidence suggests that electoral information shocks about populist support seem not to impact far-right protests in other states. All reported coefficients are close to zero and insignificant. One has to be cautious with this conclusion, though, as it could also be due to the necessarily narrow time window or measurement error from the imputation of municipal AfD support. Still, an explanation could be that people care about social acceptability in the state they live in. Surprisingly high populist vote shares in other states do not seem to empower xenophobes, particularly so if they do not live in close proximity.

2.5.8 Treatment Effect Heterogeneity

To substantiate my interpretation of the propagation effect, I investigate further patterns of heterogeneity using data from the German Socio-Economic Panel (GSOEP). Using the restricted-use microdata allows me to aggregate responses to survey questions at the municipal level and merge survey responses to my data. The main argument of this paper is that with surprisingly high populist vote shares, more xenophobes take to the streets because the social stigma attached to far-right protests declines. If this is the case, one can expect that the following patterns of heterogeneity arise.

First, the effect can be expected to be more pronounced in areas where people care more about other people's opinions. A xenophobic individual who attaches high importance to social acceptability might be be more willing to participate in a far-right protest after seeing that many other people share or tolerate her views. On the other hand, a xenophobic individual who does not care about other people's opinions will probably not change her likelihood to protest when new information about other people's attitudes becomes available. I proxy for importance of social acceptability by using responses to the question whether recognition by other people was a priority for choosing one's job. The assumption is that if people care about other people's views when they

Table 2.8: Election Effect on Far-Right Protests in Other States

	(1)	(2)	(3)	(4)
	Baseline All periods	2014 Under-estimation	2016 Under-estimation	2017 No U.-estimation
Panel A: States without elections (weak)				
AfD Shock × Post	0.0009	0.0005	0.0028	0.0002
	(0.0020)	(0.0030)	(0.0076)	(0.0022)
Observations	347086	170833	51577	130908
Panel B: States without elections (strict)				
AfD Shock × Post	-0.0037	-0.0024	-0.0025	0.0043
	(0.0043)	(0.0033)	(0.0100)	(0.0034)
Observations	95795	108287	19977	81738
Panel C: Adjacent states without elections (weak)				
AfD Shock × Post	0.0033	-0.0028	0.0040	0.0035
	(0.0043)	(0.0034)	(0.0083)	(0.0028)
Observations	107795	91456	42836	84204

Notes: Dependent variable is the number of far-right protests. Coefficient of interest is the interaction between an indicator for after a state election and the imputed municipal AfD Shock. Imputation of AfD Shock in other states is based on linear fit between municipal AfD vote shares in 2013 and 2017 federal elections. The AfD Shock is the difference to the mean of the imputed AfD vote share in a given state at times of state election. Time window for effect on other states includes 5 weeks prior and 5 weeks after elections due to election proximity in time. Panel A uses a sample of municipalities in time windows in states that did not hold the respective state election. Panel B only includes municipalities in states without state elections between the 2013 and 2017 federal election. Panel C restricts the sample to municipalities in states neighbouring the election state. All regressions include municipality, week, and relative week fixed effects and cluster standard errors at the municipal times week level. Columns differentiate between periods of polling underestimation (2014 and 2016) and right-ward shift of the AfD (after July 2015). One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

choose their job, they will probably also care about other people's views when deciding whether to participate in a far-right protest.

Second, the effect should come from politically informed far-right extremists. Municipalities without far-right extremists or municipalities where people are not interested in the results of state elections can be expected to show a noticeably smaller effect, if at all. I classify as far-right extremists the 0.7 percent of 40,220 respondents in GSOEP waves 2013 until 2017 who state that they politically lean towards the far-right NPD party. Further, roughly 32 percent of respondents say that they are either very interested or moderately interested in politics. I use this information as a proxy for political informedness.

Third, the effect should be strong in areas where attitudes associated with support for the AfD are weak. The reason is that with asymmetric information, the information shock from state elections will be stronger in municipalities with lower AfD support (see subsection 2.3.4). As the AfD's central topics concern refugees and foreigners (Arzheimer and Berning, 2019), I examine how the propagation effect is different if people in municipalities show high support for refugees and if individuals mistrust foreigners. I hypothesize that the effect will be stronger in municipalities with high support for refugees and weaker in municipalities with low trust in foreigners.

I test these patterns of heterogeneity with the following econometric specification.

$$\begin{aligned} Y_{i,t} = \beta_0 + \delta_0 \left(\text{AfD Shock}_i \times post_{s(i),t} \times V_i\right) + \delta_1 \left(\text{AfD Shock}_i \times post_{s(i),t}\right) \\ + \beta_1 \left(post_{s(i),t} \times V_i\right) + \psi Z_{i,t} + \zeta_i + \xi_t + \eta_{r(t)} + \epsilon_{i,t} \end{aligned}$$

where V_i is a variable measuring the importance of social acceptability, presence of far-right extremists, or attitudes. $Y_{i,t}$ are far-right protests in municipality i in state $s(i)$ at month t, and at relative-to-election month $r(t)$. The variable $post_{s(i),t}$ is a dummy for after a state election, AfD Shock_i is a measure of information shock from populist electoral success at the time of state election, $Z_{i,t}$ are time-varying control variables. ζ_i, ξ_t, and $\eta_{r(t)}$ are municipality, month and relative month fixed effects. The coefficient δ_0 indicates how the propagation effect δ_1 changes with higher levels of V_i. All time-constant variables are omitted due to the municipality fixed effects.

Table 2.9 presents results.[21] The first column repeats my main finding. On average, a municipality with a 0.1 increase in the AfD Shock faces a roughly 0.146 percentage points higher likelihood of an additional far-right protest after state elections. This result uses all observations from 2-year windows around state elections which have not been omitted due to missing values of time-varying control variables, or municipality, month, and relative month fixed effects. Columns 2-6 also report coefficients δ_0 on interactions of the propagation effect with variables V_i generated from GSOEP.

21. Appendix Table G5 provides summary statistics of survey question used from GSOEP.

Although these coefficients offer a meaningful way to report patterns of heterogeneity, one needs to be cautious with their interpretation. One reason is that the sample size is reduced as respondents between 2013 and 2017 waves do not cover all municipalities. It is not clear whether the reduction in sample size preserves representativeness of all municipalities. Secondly, variation in variables V_i is probably not exogeneous and could be impacted by factors that also impact far-right protests and AfD Shock. Third, variables V_i are time-constant aggregated statistics from answers to surveys between 2013 and 2017 and thus might not reflect changes over time. As with all survey data, problems of measurement error from biased reporting might also be an issue. Therefore, coefficients δ_0 shall not be understood as causal effects, but as an empirically enhanced way to gauge patterns of heterogeneity.

Columns 2-7 of Table 2.9 suggest that the hypothesized patterns of heterogeneity hold. Given the greatly reduced number of municipalities and the additional interaction terms, the coefficients are not statistically significant. However, signs and relative magnitudes of coefficients confirm that social acceptability (column 2), as well as politically informed extremists (columns 3 and 4) appear to be driving factors for the empowerment effect. The last two columns suggest that, indeed, information shocks appear to be stronger in areas where attitudes associated with the AfD are weak. Absolute coefficient sizes vary substantially given the greatly reduced number of municipalities in the sample. However, the direction of heterogeneity patterns substantiates my interpretation of the propagation effect: when surprising populist success provides new information about other people's attitudes, social stigma attached to far-right protests is reduced and more far-right protests result.

Table 2.9: Heterogeneity of Propagation Effect – SOEP

	(1)	(2)	(3)	(4)	(5)	(6)
AfD Shock × Post	0.0146**	-0.0535	0.0128	-0.0061	0.0102	0.3740*
	(0.0060)	(0.1197)	(0.0293)	0.0426	0.0436	0.2049
AfD Shock × Post × Acceptability		0.1447				
		(0.0909)				
AfD Shock × Post × Political Interest			0.0370			
			0.0453			
AfD Shock × Post × Far-Right Extremist				0.4418		
				0.6232		
AfD Shock × Post × Pro-Refugees					0.1112	
					0.1622	
AfD Shock × Post × Mistrust Foreigners						-0.2001
						0.3058
Observations	121858	7608	24054	15971	16675	2487
Number of Municipalities	6572	569	1656	1120	1159	190

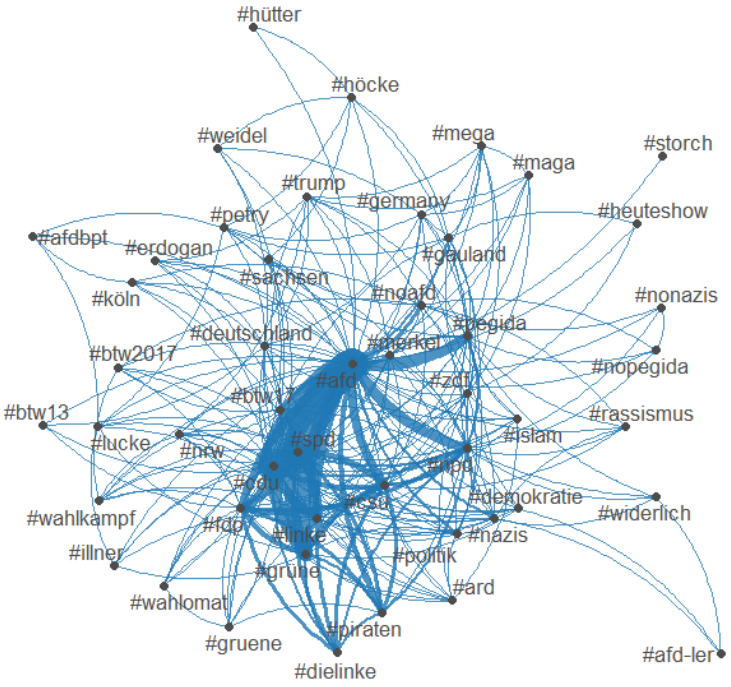
Notes: Dependent variable is the number of far-right protests. Table reports coefficients and standard errors from OLS regressions. All regressions include municipality, month, and relative month fixed effects. Standard errors are clustered at the month times state level. AfD Shock is the difference between state average and municipal AfD vote share at state elections between the 2013 and 2017 federal elections in Germany. Post is a dummy for after a state election. Sample includes monthly observations of municipalities with state elections between September 2013 and September 2017, except city states, in 2-year windows around elections. Columns 2 to 7 show heterogeneity of the effect with respect to variables aggregated from the German Socio-Economic Panel (GSOEP) waves 2013-2017. See Appendix Table G5 for summary statistics and Appendix Table G7 details on survey question used from GSOEP. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

2.5.9 # AfD Tweets

How does the information shock impact social media and what is its role in the propagation of extremism? I examine geolocated twitter data containing the hashtag #AfD as a mediating outcome which in turn could influence far-right protests.

An important observation is that #AfD tweets usually seem to express opposition to the AfD's agenda or anger about the AfD's success. The network plot in Figure 2.7 gives reason to believe that #AfD is heavily used for statements directed against the AfD. In the plot, a thicker connecting line indicates higher co-occurrence of the top 40 user names and top 50 hashtags in the #AfD tweets sample. As can be seen, #AfD tweets often clearly reference people, institutions or parties that can better be described as anti- rather than pro-AfD.[22]

Figure 2.7: Network Plot: Top-50 Other Hashtags in #AfD Tweets



Notes: Graph shows co-occurrences of the top-50 other hashtags in geolocalisable #AfD tweets between the 2013 and 2017 federal elections. Fatter lines indicate higher frequencies of co-occurrence within #AfD tweets.

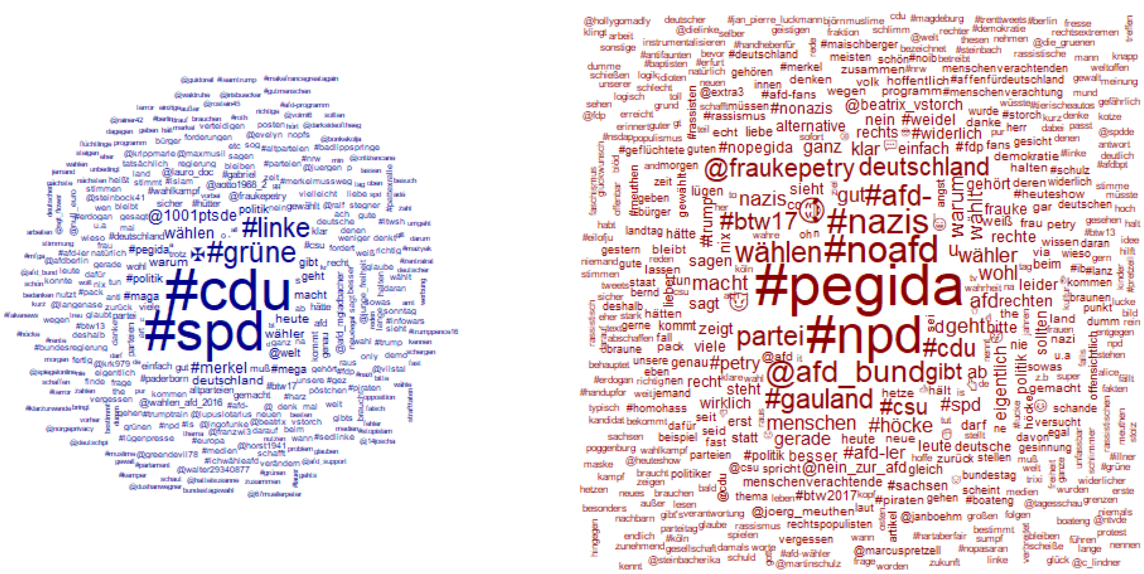
I employ several classification methods for #AfD tweets and cross-validate them with the universe of tweets from the official accounts of German parties. First, I limit the sample of #AfD tweets to those originating in states with state elections between the 2013 and 2017 federal elec-

22. Hashtags suggesting an anti-AfD content are for example "#nonazis" and "#widerlich". Appendix Figure H1 also documents that users mentioned in the tweets include both pro-AfD agents ("joerg_meuthen", "fraukepetry", and "afd_bund") and anti-AfD agents ("cdu", "spd", "dielinke").

tions. Then, I manually classify the #AfD tweets based on the content of their text and the self-description of the user. Due to possible irony and context dependence of tweets, this method was able to classify only 57 percent of #AfD tweets in states with state elections as either pro-AfD (15 percent) or anti-AfD (42 percent). Figure 2.8 shows word clouds for pro- and anti-AfD tweets. Frequent words mentioned in anti-AfD tweets include "#noafd", "#nazis", as well as angry and sarcastic emoticons, indicating stark opposition to the AfD. Pro-AfD tweets often mention hasthags of more established parties and also use a version of the "Iron Cross" - a symbol that was adopted by Hitler and is often displayed by neo-Nazi groups today. Appendix Table H1 lists the top 15 words in each category, after preprocessing and removal of stop-words[23]. The #AfD tweets classified as anti-AfD have higher usage of words that could associate the AfD with the far-right, such as "#pegida", "#npd". or "#nazis". Thus, the manual classification seems to work reasonably well, but provide a conservative classification in the sense that about half of the #AfD tweets remain unclassified. Figure H2 in the appendix presents monthly frequencies of thus classified tweets.

Figure 2.8: Word Cloud: #AfD Tweets

Pro-AfD   Anti-AfD



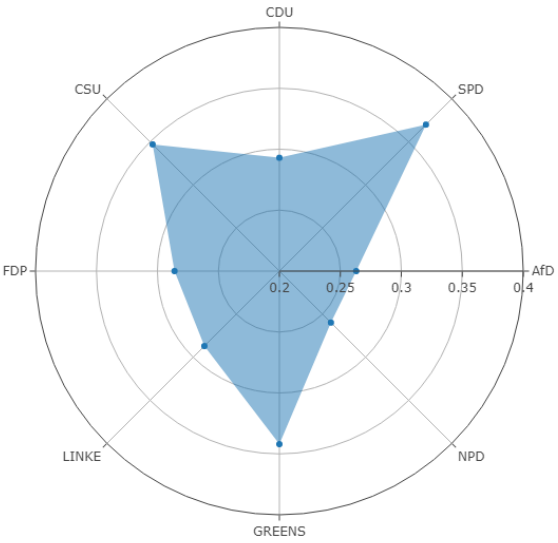
Notes: Word clouds for pro- and anti-AfD tweets, according to manual classification. Size of words indicates frequencies.

In order to further cross-validate my manual classification, I compute cosine similarity measures of #AfD tweets to the corpora of tweets from German parties. Appendix Figure H3 provides an overview of how much the AfD emits tweets in comparison to three other German parties.

23. Preprocessing includes conversion to lowercase, removing punctuation and numbers. For calculus of cosine similarity I also use stemming.

Starting in October 2012, official tweets from the AfD party see a substantial increase before the 2017 federal election, but generally remain below the frequency of tweets from the CDU, FDP, or Greens. Given that the rightward shift of the AfD in 2015 changed the content of the tweets as well (Cantoni, Hagemeister, and Westcott, 2019), I use cosine similarity to AfD party tweets after 2015 to cross-validate the #AfD tweet classification. Figure 2.9 shows the result. #AfD tweets have higher similarity to tweets from the more moderate parties SPD, CDU, GREENS, and CSU. The similarity of #AfD tweets to official AfD party tweets or even to extreme right-wing NPD party tweets is relatively small.

Figure 2.9: Cosine Similarity of #AfD Tweets and Party Tweets



Notes: The graph plots average cosine similarities for political parties' tweets to #AfD tweets using corpora aggregated at the month level, and AfD party tweets after 2015 only.

How do #AfD tweets react to information shocks from state elections? Figure 2.10 presents kernel density estimates of manually classified #AfD tweets for municipalities with different strengths of information shocks. The left graphs show densities for the 2016 period with underestimation, the graphs on the right for the 2017 period with no underestimation. The solid and dashed lines indicate densities before and after state elections respectively. The top graphs show results for anti-AfD tweets (red), the bottom graph for pro-AfD tweets (blue). The general pattern of left-skewed density distributions illustrates that Twitter is a platform more heavily used in liberal areas with lower populist vote shares. Interestingly, more liberal municipalities (with higher AfD Shock values) show more anti-AfD tweets after the 2016 state elections when the AfD's success was severely underestimated, and slightly fewer pro-AfD tweets. Without underestimation, the differences in densities before and after state elections is much smaller, and – if anything – point in the opposite

direction.[24] In summary, it seems that a surprisingly high populist electoral success did not only induce xenophobic individuals to participate in far-right protest marches, but did also encourage critics of the AfD to react with anti-AfD tweets.

It seems intuitive that pro-AfD tweets could lower the stigma attached to far-right protests, whereas anti-AfD tweets might increase it. Further, a pro-AfD tweet might have a lagged effect and influence the number of protests not in the given, but in the next month. Anti-AfD tweets could be a reaction to far-right protests, so they might be tweeted in the same month as the far-right protest is taking place. To make use of the universe of geolocalisable #AfD tweets, I use the manual classification as training data in a naive bayesian machine learning algorithm and classify the remaining geolocalisable #AfD tweets. The algorithm performs well with an 85 percent out-of-sample accuracy (95% CI: 83 and 88 percent). The left graph in Appendix Figure H4 shows the weekly frequency of the thus classified tweets. The graph on the right presents first suggestive evidence that the share of pro-AfD tweets is positively correlated with the number of far-right protests in a given week, and particularly so after July 2015.[25] To test a potential mediating mechanism further, I regress the number of far-right protests on the number of pro- and anti-AfD tweets, including their first lead and first two lags. I also add municipality and month fixed effects to control for general time patterns and any time-constant unobserved heterogeneity between municipalities.

The top left graph of Figure 2.11 shows that the first lead and lag of pro-AfD tweets are significantly associated with a higher number of far-right protests. In the same month, the association between pro-AfD tweets and far-right protests is almost exactly zero. However, the coefficient for anti-AfD tweets is positive and significant, albeit with a much smaller magnitude than the pro-AfD tweets. When the sample is split across underestimation periods and before/after the rightward shift of the AfD, the most notable difference is the changing association between pro-AfD tweets and far-right protests in the same month of the protest. In periods of underestimation – and arguably high social stigma – the association is negative, and even more so after the rightward shift of the AfD. Without underestimation and a somewhat higher social acceptance after many successful state elections (Gerling and Kellermann, 2019), pro-AfD tweets originating from the same municipality are also positively associated with far-right protests in the month of the far-right protest.

It might be surprising that I do not find a significant negative association between anti-AfD tweets and far-right protests, particularly not in the months before the protests. However, one has to keep in mind that the #AfD tweets are geolocalised in the municipality where they originate – they can be read everywhere else. Anti-AfD tweets could still influence protest behaviour in other places, but in a given municipality, they do not seem to curb far-right protests.

Note that although I employ a demanding specification with month and municipality fixed effects, I do not claim causality here. On the one hand, a causal effect from tweets to far-right

24. Appendix Table H2 shows the results more formally.
25. However, the correlation is not statistically significant.

protests would be conceptually difficult to grasp. At the other hand, I do not exploit exogenous variation in #AfD tweets to be able to make such a claim. However, my analysis of #AfD tweets shows that reactions on social media are different when the vote share of the populist party is surprising as to when it is not. Tweets seem to impact far-right protests depending on content and timing.

2.5.10 The Effect of Polling Releases

Before a state election takes place, various polling institutes make their forecasts public. The release of a surprisingly high polling forecast could be an information shock comparable to an election information shock itself. However, polling forecasts do not receive as much media attention as an election result and can be imperfect measures of party support (see Figure 2.3). Nevertheless, the question arises whether and how polling releases impact far-right protests and tweets. Previous literature has discussed the effects of polling releases in particular with respect to subsequent voting outcomes and turnout (Bursztyn et al., 2020; Dahlgaard et al., 2016; Sudman, 1986), as well as with respect to attitudinal changes and persuasion (Moy and Rinke, 2012; Boudreau and McCubbins, 2010). I contribute to this literature by studying the effect of polling releases for populist party vote shares on far-right protests and #AfD tweets.

I combine polling forecasts from 21 different polling institutes and calculate the mean forecast for the AfD in each week and state between September 2013 and September 2017. The left graph in Figure 2.12 depicts these absolute polling forecasts for the AfD. I construct the information shock from polling as the difference of an AfD forecast to the previous month's AfD forecast. The result is shown in the right graph of Figure 2.12.

I run the following regression specification to test for the impact of polling forecast about the AfD on far-right protests and #AfD tweets.

$$Y_{i,t} = \beta_0 + \beta_1 \Delta\text{AfD Polling}_{s(i),t} + \beta_2 \Delta\text{AfD Polling}_{s(i),t-1} + \beta_3 \Delta\text{AfD Polling}_{s(i),t-2} + \beta_4 election_{s(i),t} + \zeta_i + \xi_t + \epsilon_{i,t}$$

where $Y_{i,t}$ is the number of far-right protests, all #AfD tweets, anti-AfD tweets, or pro-AfD tweets. $\Delta\text{AfD Polling}_{s(i),t}$ is the difference of the AfD polling forecast to the AfD polling forecast of the previous month, and $election_{s(i),t}$ is an indicator variable for an election month of a state election. ζ_i and ξ_t are municipality and month fixed effects.

Table 2.10 presents the results. In the same month, higher AfD polling forecasts do not seem to impact the number of far-right protests or #AfD tweets in general. However, pro-AfD tweets appear to react to polls that report increased AfD support (column 4). If polling institutes put

the AfD forecast one percentage point higher than in the previous month, the likelihood of an additional geolocated pro-AfD tweet is increased by roughly 0.32 percentage points. This amounts to a 53 percent increase of the mean of pro-AfD tweets (0.0032/0.006). In comparison, the effect on anti-AfD tweets amounts to only 22 percent in terms of its mean (0.0061/0.028), and is statistically indistinguishable from zero. This finding is consistent with an interpretation of increased social acceptability of the AfD through higher polling forecasts. Tweets react much less to increased AfD polling results from the previous month, and do not seem to show any reaction two months after a polling release with higher AfD forecast. This is intuitive as twitter is a platform where opinions are shared and spread in direct response to events and where discussions of a single event usually do not last for several months.

The coefficient on the second lag of the delta in AfD polling is positive and significant for far-right protests. In terms of one standard deviation, the effect is about half the size of the propagation effect from state elections.[26] Surely, effects from polling releases and state elections are not directly comparable because the polling release effect uses the entire population of German municipalities, whereas the propagation effect is specific to municipalities in states with state elections, and excluding city states. Further, I am able to exploit the exogenously given timing of state elections to determine the latter effect, but I cannot rely on exogeneity of AfD polling forecasts. Although I employ month and municipality fixed effects, the variation in AfD polling forecasts is at the state level and could be driven by time-varying factors that also determine far-right protests.

2.5.11 Robustness: Placebo

Finally, I check my findings using a placebo regression. As the AfD was founded only in 2013, I examine whether far-right protests in 2009-2013 show similar patterns after state elections in 2009-2013 – before the AfD party existed. Note that I use the same time-constant municipal AfD Shock treatment as defined by differences to state average AfD results in 2013-2017 state elections. If my findings so far reflect a reaction to an information shock of a successful populist party, the same interaction term should not show a marked difference before and after state elections four years earlier. Figure 2.13 shows the result of this placebo exercise in an event study graph. Overall, the interaction coefficients are comparable in size and precision before and after the election month, confirming my results. It turns out that in 2009-2013, far-right protests before and after election months had a tendency to take place in municipalities which four years later show below average populist support. This tendency seems to decrease slightly over time. An exception is the month prior to the election. Here, far-right protests do not seem to be more frequent in municipalities with higher information shocks in the future. Nevertheless, the important aspect of the graph is

26. Two weeks after a polling institute increased the AfD's forecast by one percentage point, far-right protests tend to go up by 0.000725 on average, amounting to a 28 percent increase over the mean (0.01*0.0725/0.00259). However, a standard deviation in AfD Shock is 0.041 whereas one standard deviation in Δ AfD Polling is 0.00469. In terms of one standard deviation, the state election and polling release effects are 0.060 percentage points (0.041*0.0147) and 0.034 percentage points (0.00469*0.0725) respectively.

Table 2.10: Effect of Polling Release

	(1) Far-Right Protests	(2) #AfD tweets	(3) Anti-AfD tweets	(4) Pro-AfD tweets
Δ AfD Polling	-0.0118	0.9329	0.6145	0.3184**
	(0.0371)	(0.6627)	(0.5270)	(0.1594)
L.Δ AfD Polling	0.0554	0.6722	0.5945	0.0776
	(0.0507)	(0.7706)	(0.5886)	(0.2142)
L2.Δ AfD Polling	0.0725**	-0.0396	0.0028	-0.0424
	(0.0326)	(0.5370)	(0.4198)	(0.1370)
Observations	526,812	526,812	526,812	526,812
Mean	0.0026	0.0345	0.0278	0.0068
Std.Dev.	0.0645	1.22	1.004	0.2659

Notes: Dependent variables as indicated by column headers. Table reports coefficients and standard errors from OLS regressions. All regressions include municipality and month fixed effects. Standard errors are clustered at the month times state level. Delta AfD Polling is the difference in monthly AfD polling values (see Figure 2.12 for details). Sample includes monthly observations of German municipalities between September 2013 and September 2017. Mean and standard deviation of the Delta AfD Polling variable are -0.000135 and 0.00469 respectively. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

the absence of a marked difference between interaction coefficients before and after state elections in 2009-2013. This suggests that I do not pick up general patterns of far-right protests around elections, but rather the effect of a populist party's surprising success.

2.6 Conclusion

This study has focused on the nexus between populist electoral success and far-right extremism. I have argued that populist electoral success can act as an information shock for areas with below-average populist support. The result is a significant increase in bottom-up far-right protests in more liberal areas. My contribution to the literature is twofold. First, this study adds to the literature by documenting an empowerment effect in a new context – far-right protests in Germany. Second, I show that the empowerment effect manifests itself only under conditions of surprise. I capture surprise via three strategies, using polling forecasts and differences between vote shares and political party structures at the municipal and state level.

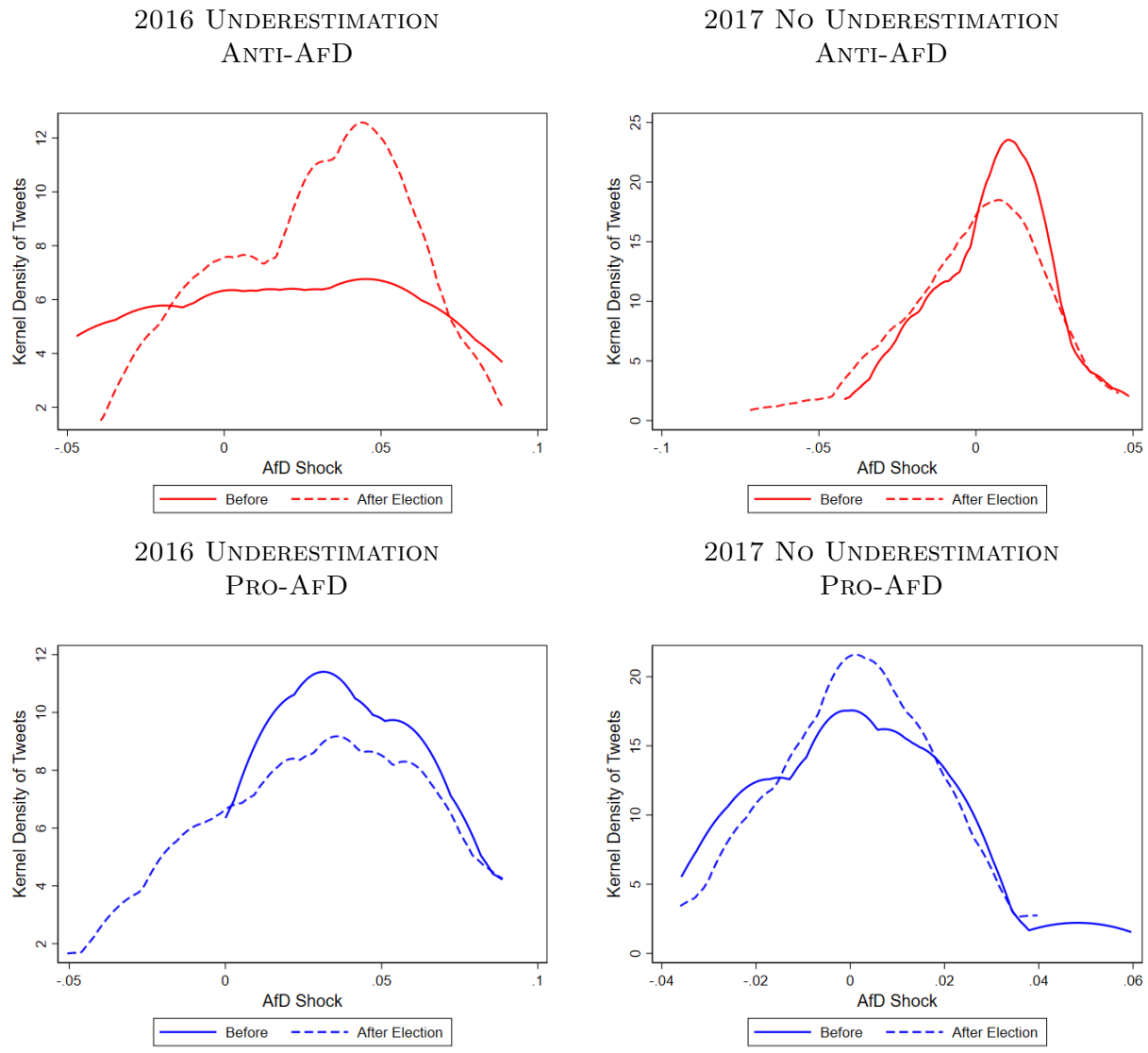
The empirical analysis validates that far-right protests seem to increase depending on surprise. If success of the populist party is severely underestimated, a municipality with a populist vote share 10 percentage points below state average faces a roughly 30 percent increase of the mean likelihood of an additional far-right protest. The effect vanishes when polling institutes correctly estimate the populist party's success. The effect is stronger in municipalities without successful marginal

parties, and negligible in municipalities with highly polarised vote distributions. Further, the effect materializes only after the rightward shift of the AfD, and is specific to far-right extremism, not antisemitism.

Consistent with Müller and Schwarz (2019), I find suggestive evidence that social media can impact the propagation of far-right extremism. In addition, I can show that the effect depends on content and timing, as well as a municipality's relative position in the distribution of AfD vote shares. The response in tweets is different in periods when the populist party is underestimated. My findings are consistent with a mechanism in which xenophobic individuals hide their attitudes if they believe that they are not shared by the broader public. Once the broader public's attitudes become apparent, xenophobic individuals in more liberal areas update their beliefs and take to the streets. Anti-AfD tweets increase as a response of unexpectedly high populist success, but do not seem to influence the number of far-right protests in the municipality they originate from.

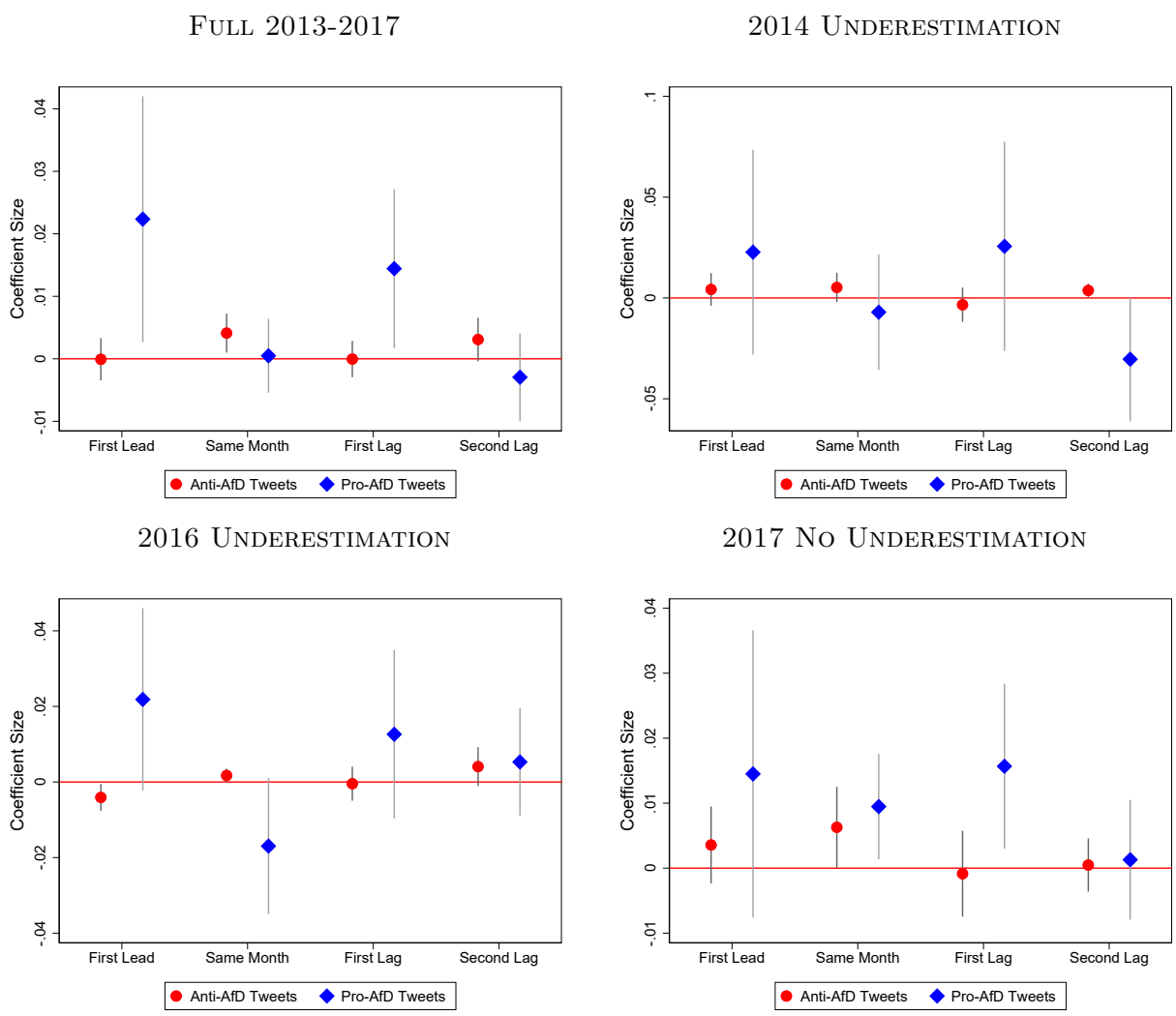
Organizers of the far-right protests in my analysis might support more extremist standpoints than most populist parties do. Still, my analysis suggests that one consequence of the surprising success of right-wing populist outfits across the world are more open manifestations of far-right extremist attitudes and behaviour based upon them – particularly in areas that had not seen them for a long time.

Figure 2.10: Tweets in Differently Shocked Municipalities



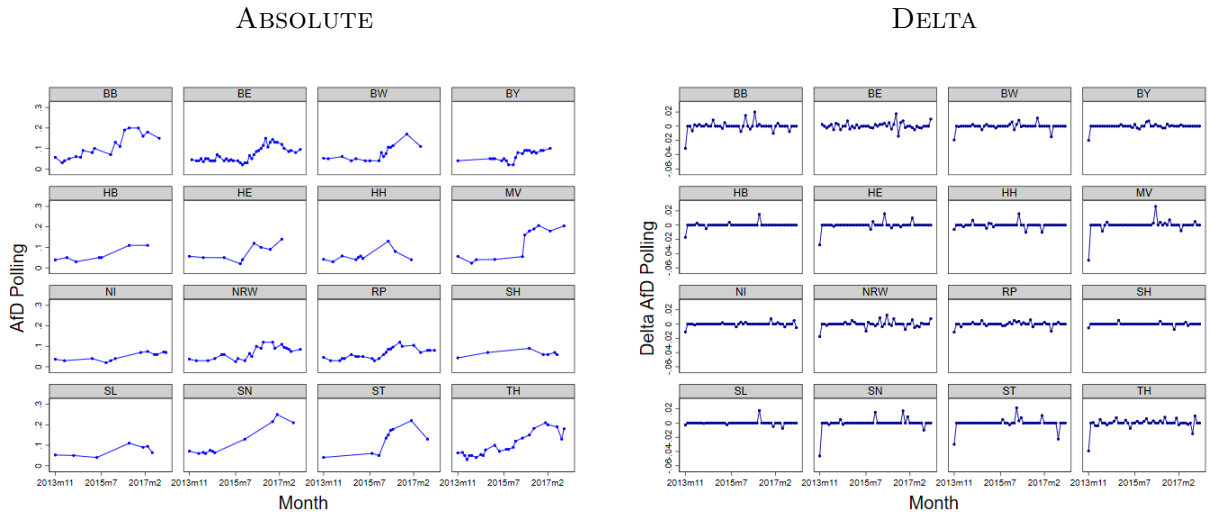
Notes: Graphs plots kernel density estimates of anti- and pro-AfD tweets for municipalities with different intensity of information shocks. Solid lines indicate the period before, dotted lines indicate the period after a state election. Time window is one year around state elections.

Figure 2.11: Tweets and Far-Right Protests



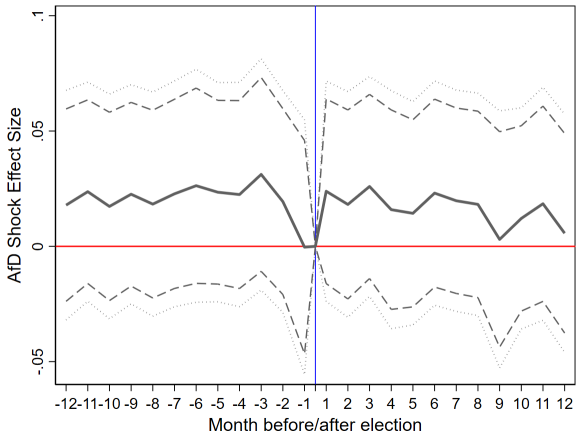
Notes: Graph plots regression coefficients from OLS regressions. Dependent variable is the number of far-right protests. Independent variables are the number of anti- and pro-AfD tweets, their first lead, and their first and second lags. Sample in the top left graph includes monthly observations in all German municipalities between September 2013 and September 2017. The remaining three graphs show coefficients split across underestimation periods (see Figure 2.3) and the right-ward shift of the AfD. The top right graph restricts the sample to observations between September 2013 and July 2015 (right-ward shift of the AfD). The bottom left graph restricts the sample to the July 2015- March 2016 period (severe underestimation of the AfD by polling institutes). The bottom right graph restricts the sample to the period between April 2016 and September 2017 (no underestimation). Regressions include municipality and month fixed effects. Standard errors are two-way clustered at the month and state level. See Appendix Table H3 for details.

Figure 2.12: AfD Polling Releases



Notes: Graph shows monthly AfD polling forecasts for the federal states in Germany between September 2013 and September 2017. The left graph shows absolute polling values, the right graph monthly changes in polling forecasts. Data from wahlrecht.de. Polling institutes include infratest dimap, Forsa, the Allensbach institute, Customer Research 42, Emnid, FGWTelefonfeld, Forschungsgruppe Wahlen, GESSPhone & Field, GMS, IM Filed, INSA, Dukath, Mentefactum, TNS Infratest, Trend Research Hamburg, Universtiät Hamburg, YouGov, aproxima, mafo.de, pollytix, and uniQma.

Figure 2.13: Placebo: Event Study Graph



Notes: Graphs plot OLS regression coefficients of the interaction of AfD Shock with month dummies relative to state election for far-right protests in 2009-2013 (before the AfD came into existence). Regressions include state, month, and relative month fixed effects, as well as AfD shock. Standard errors are clustered at the state-times-month level. Sample consists of 2-year monthly windows for German municipalities in states with state elections between the 2009-2013 federal elections, except city states. Omitted category is the election month.

2.7 *Appendices*

2.7.1 *Appendix Figures and Tables*

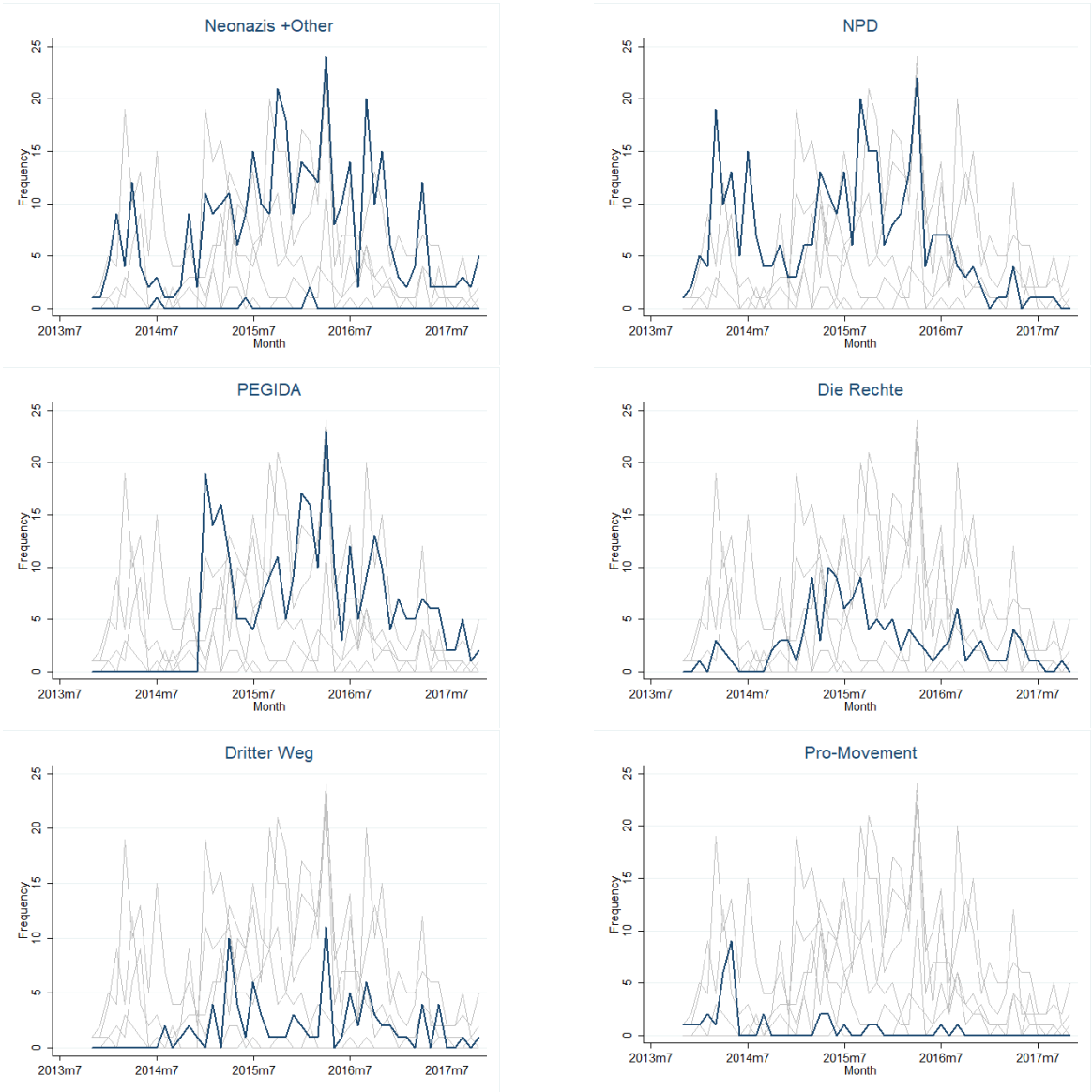
Figure G1: AfD State Elections since 2013

Date	Election	State	AfD Result
24.09.2017	Federal		12.6%
14.05.2017	State	North Rhine-Westphalia	7.4%
07.05.2017	State	Schleswig-Holstein	5.9%
26.03.2017	State	Saarland	6.2%
18.09.2016	City state	Berlin	14.2%
04.09.2016	State	Mecklenburg-Western Pomerania	20.8%
13.03.2016	State	Saxony-Anhalt	24.3%
13.03.2016	State	Rhineland-Palatinate	12.6%
13.03.2016	State	Baden-Württemberg	15.1%
10.05.2015	City state	Bremen	5.5%
15.02.2015	City state	Hamburg	6.1%
14.09.2014	State	Thuringia	10.6%
14.09.2014	State	Brandenburg	12.2%
31.08.2014	State	Saxony	9.7%
22.09.2013	Federal		4.7%

Figure G2: Far-Right Extremist Protest Marches in Germany

Notes: **Left picture:** Far-right extremist protest march in Berlin on 19th August 2017. Some 1000 participants affiliated with Neo-Nazi and extreme right groups marched through the street of Berlin's Spandau district in commemoration of 30 years to Rudolf Hess's (deputy "Führer" under Hitler) death. Link to source here.
Right picture: Far-right extremist protest march on 1st May 2016 in the small town of Plauen in Saxony. Link to source here.

Figure G3: Organizers of Far-Right Protests

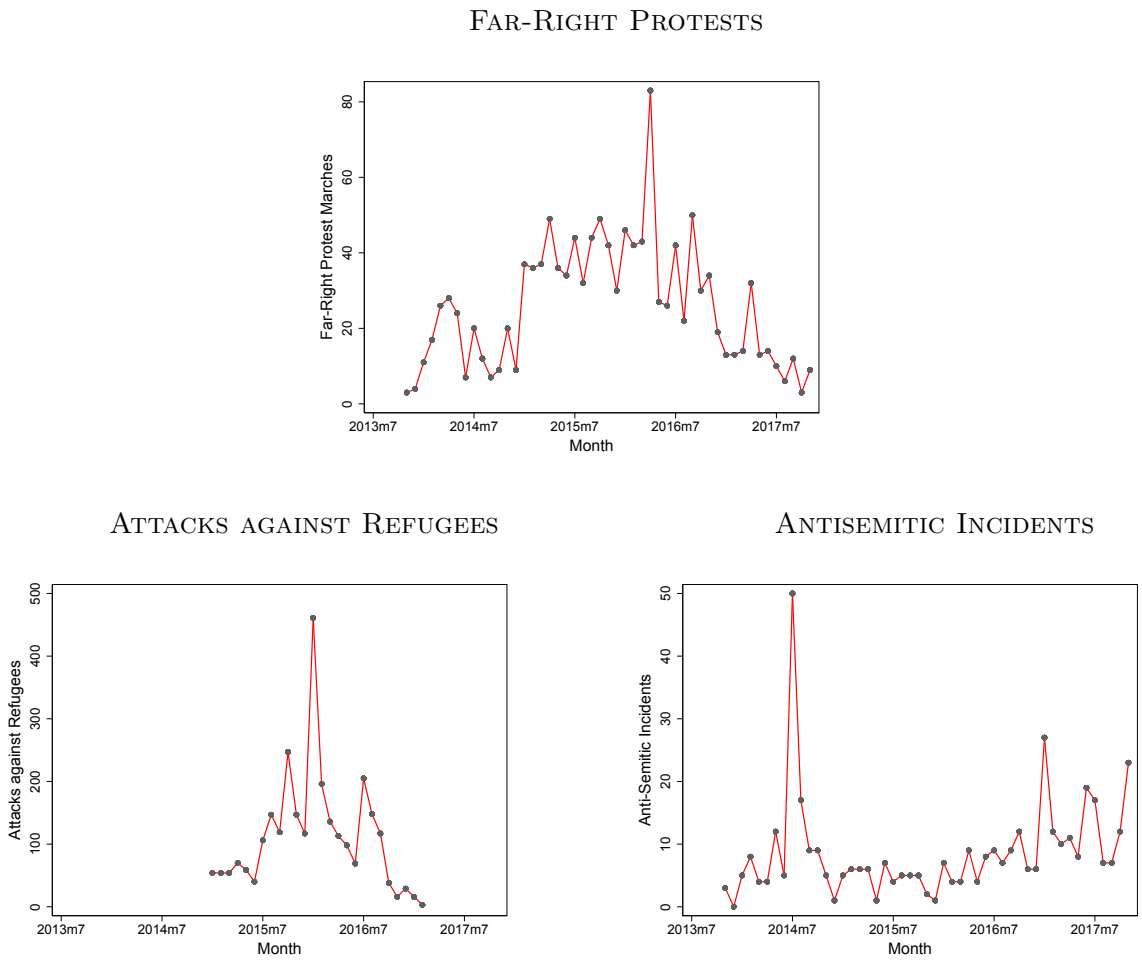


Notes: Graph shows monthly frequencies of far-right protests, split by organizers. Data from the German *Bundestag*. Non-formal organizers are "Neonazis" and "other" (top left graph, where other has lower frequencies). Formal organizers are the right-wing extremist "National Democratic Party of Germany" (NPD), the movement "Patriotic Europeans Against the Islamisation of the Occident" (PEGIDA), the right-wing extremist parties "DIE RECHTE", and "Dritter Weg", as well as the "Pro-Movement".

Figure G4: Word cloud: Slogans of Far-Right Protests since 2005

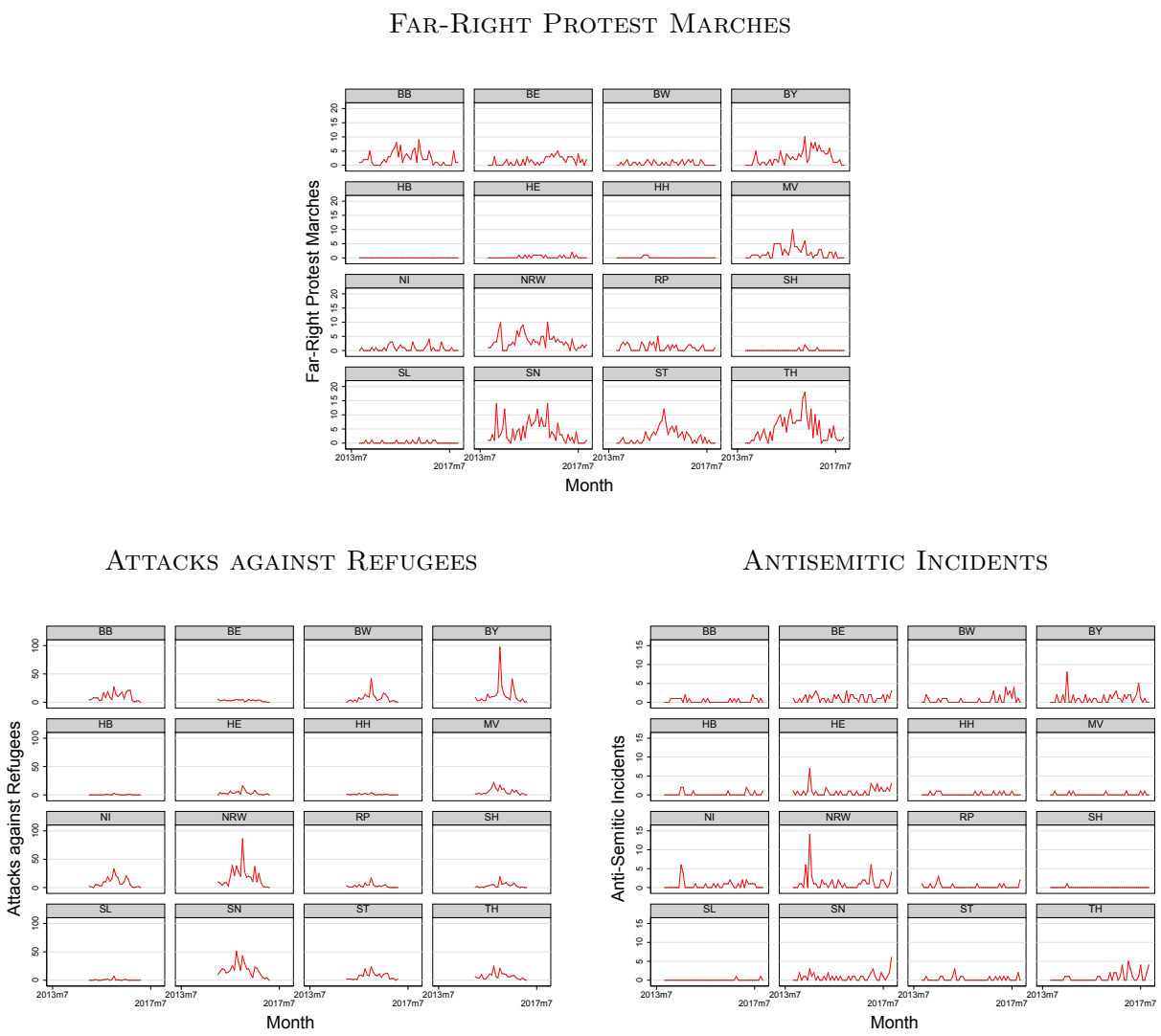
Notes: Graph shows word cloud from slogans of far-right protests since 2005. Data from the German *Bundestag*. Bigger size indicates higher frequencies of terms in slogans of far-right protests since 2005. Terms have been pre-processed with rendering to lower case, stemming, removing of numbers, punctuation, and special characters.

Figure G5: Monthly Frequencies of Far-Right Protests, Attacks against Refugees, and Antisemitic Incidents



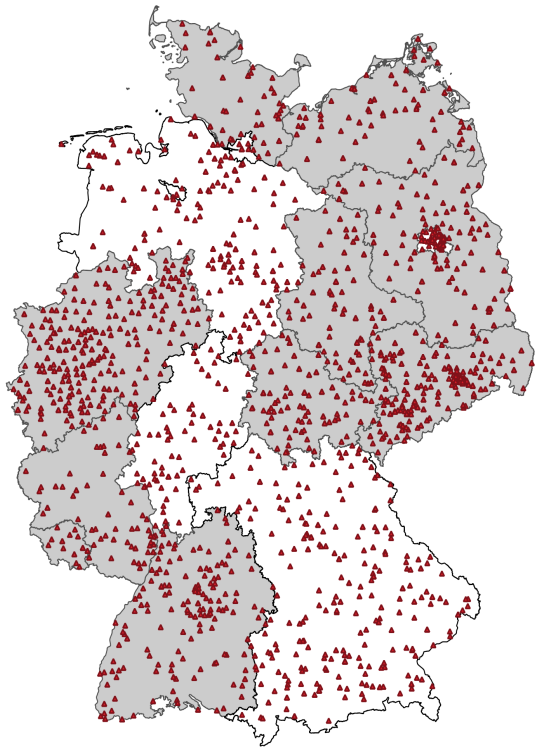
Notes: Graph shows monthly frequencies of far-right protests, attacks against refugees, and antisemitic incidents, between the 2013 and 2017 federal elections in Germany. Data from the German *Bundestag* and the Amadeu Antonio Foundation.

Figure G6: Monthly Frequencies per state



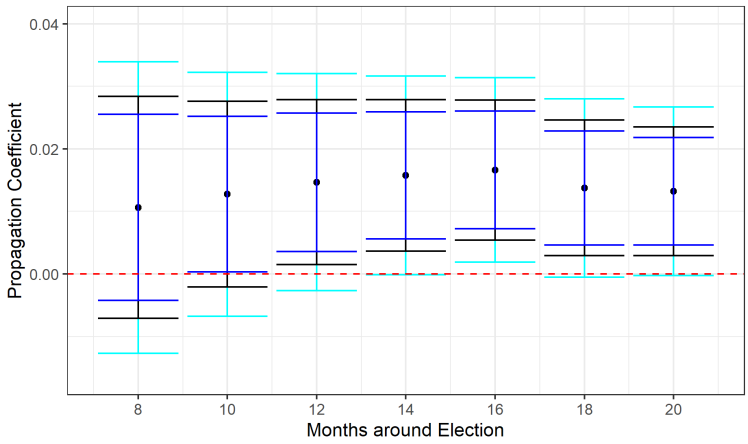
Notes: Graph shows monthly frequencies of far-right protests, attacks against refugees, and antisemitic incidents, between the 2013 and 2017 federal elections in Germany, split by federal states. Data from the German *Bundestag* and the Amadeu Antonio Foundation.

Figure G7: Map: Attacks against Refugees



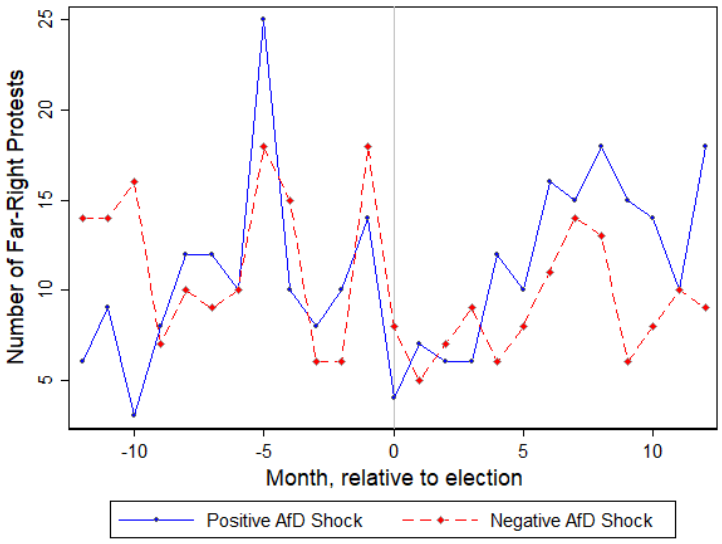
Notes: The shaded area indicates states with state elections between the 2013-2017 federal elections that are not city states. Red triangles indicate occurrences of attacks against refugees and their homes.

Figure G8: Choice of Different Time Windows



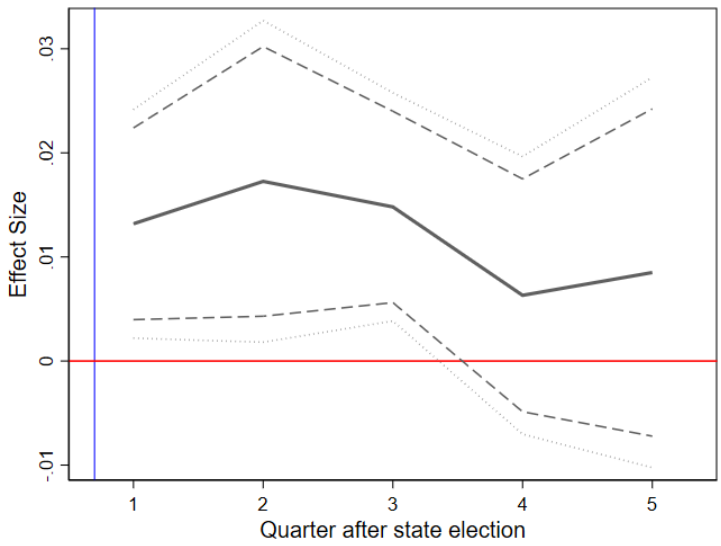
Notes: Graph plots coefficients and confidence intervals of OLS regressions with different time windows. The dependent variable is the number of far-right protests. Coefficients are on the interaction of AfD Shock and a dummy for after state elections. The AfD Shock is defined as the difference between the mean AfD vote share and a municipality's AfD vote share at the time of a state election. All regressions include municipality, month, and relative month fixed effects, as well as a time-varying control for refugees at the municipality level. Blue, dark, and cyan intervals are for the 10, 5, and 1 percent significance levels respectively. Standard errors are clustered at the state-times-month level.

Figure G9: Common Trend Assumption:
Number of Far-right Protests for low and high AfD Shock Municipalities



Notes: Graphs plots number of far-right protests in municipalities with above and below state-average AfD results relative to the election month.

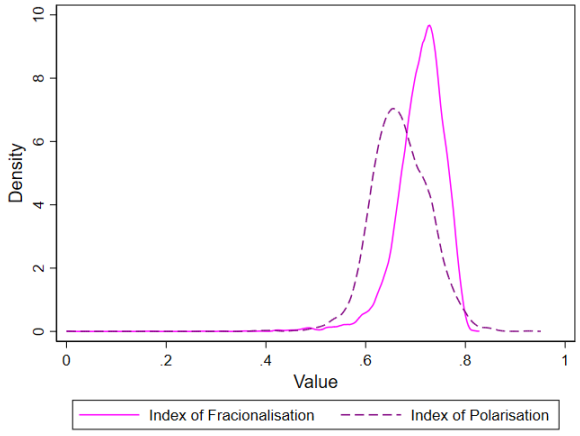
Figure G10: Long-Run Effect



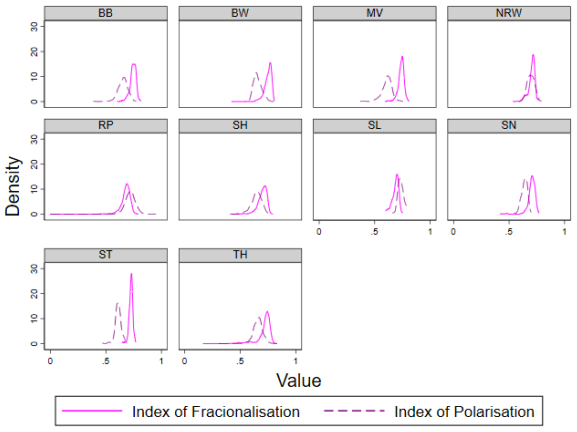
Notes: Graph plots regression coefficients of the interaction of AfD Shock with month quarterly dummies relative to state elections. Regressions include municipality, month, and relative month fixed effects, as well as a time-varying control for refugees at the municipal level. Standard errors are clustered at the state-times-month level. Sample consists of 4-year monthly windows for German municipalities in states with state elections between the 2013-2017 federal elections, except city states.

Figure G11: Fractionalisation and Polarisation Indices

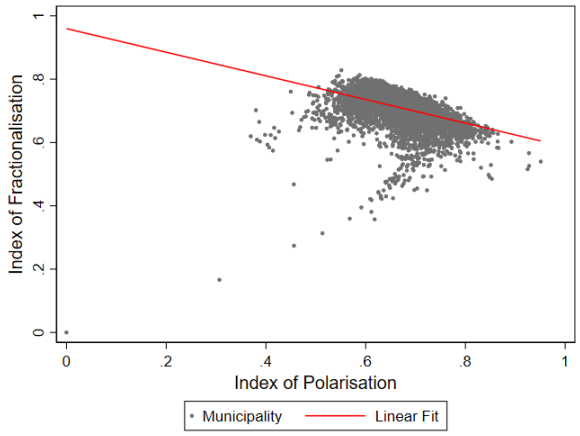
Overall



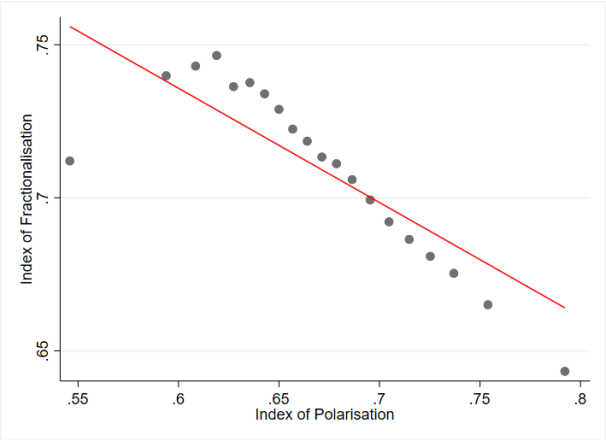
State-by-state



Bivariate Scatterplot

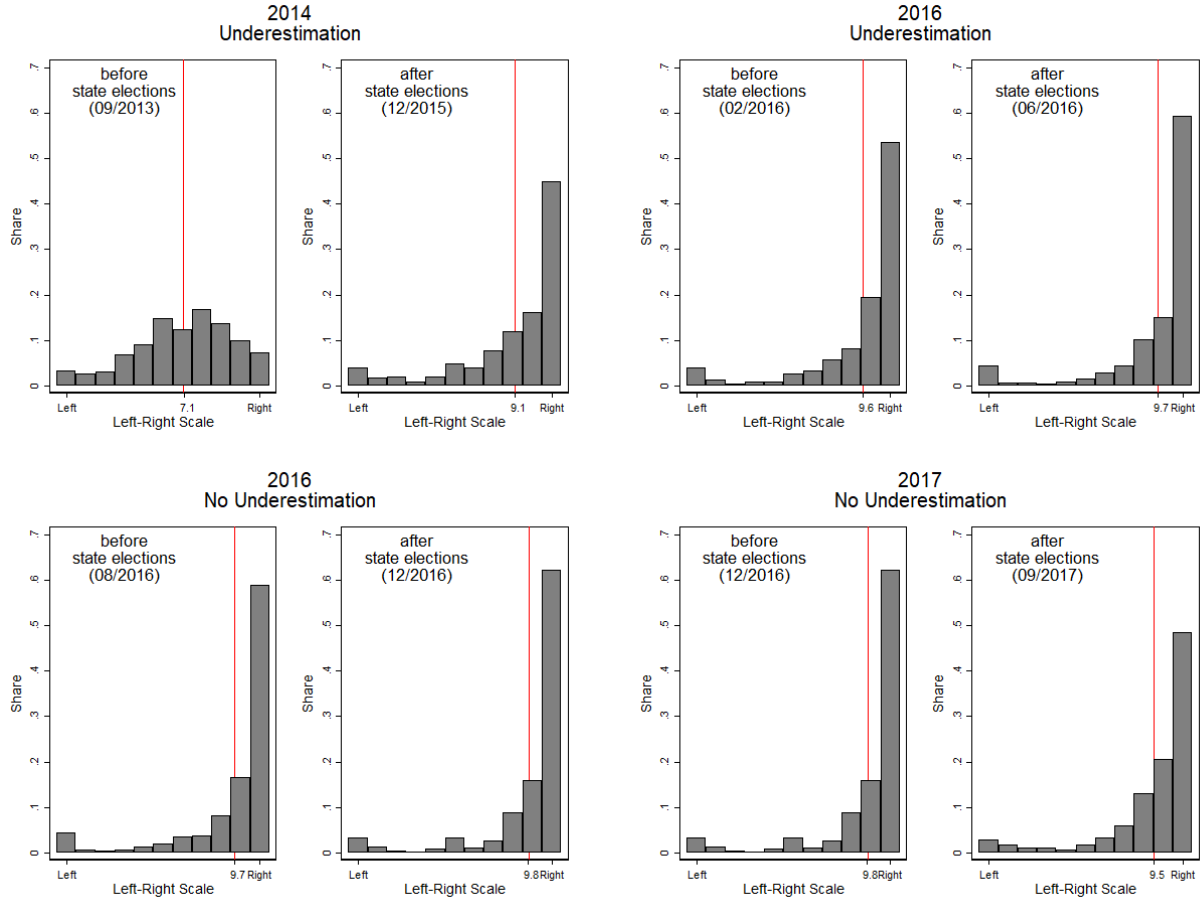


Binned Scatterplot



Notes: Top graphs plot kernel densities of fractionalisation and polarisation indices. Bottom graphs show bivariate scatter plots and linear fits of fractionalisation and polarisation indices.

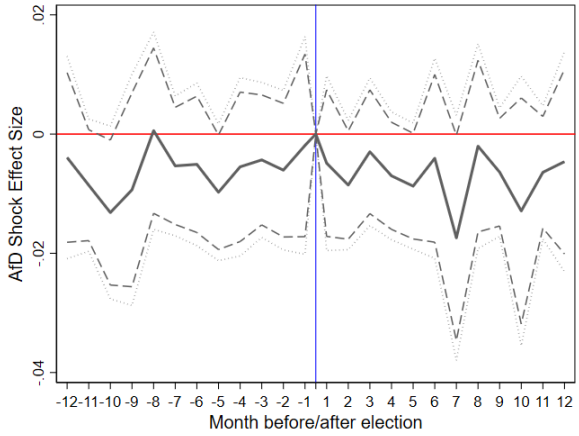
Figure G12: AfD Positioning on Left-Right Scale



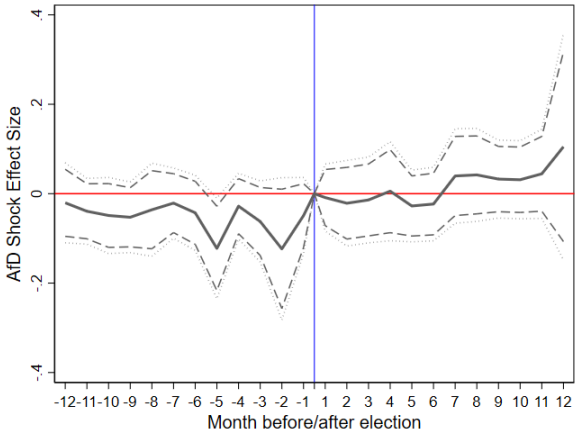
Notes: The graphs show where GLES survey respondents placed the AfD on the left-right spectrum before and after state elections in different periods of under- and no under-estimation. Red lines in the graph represent means.

Figure G13: Event Study Graphs of Further Outcomes

Antisemitic Incidents

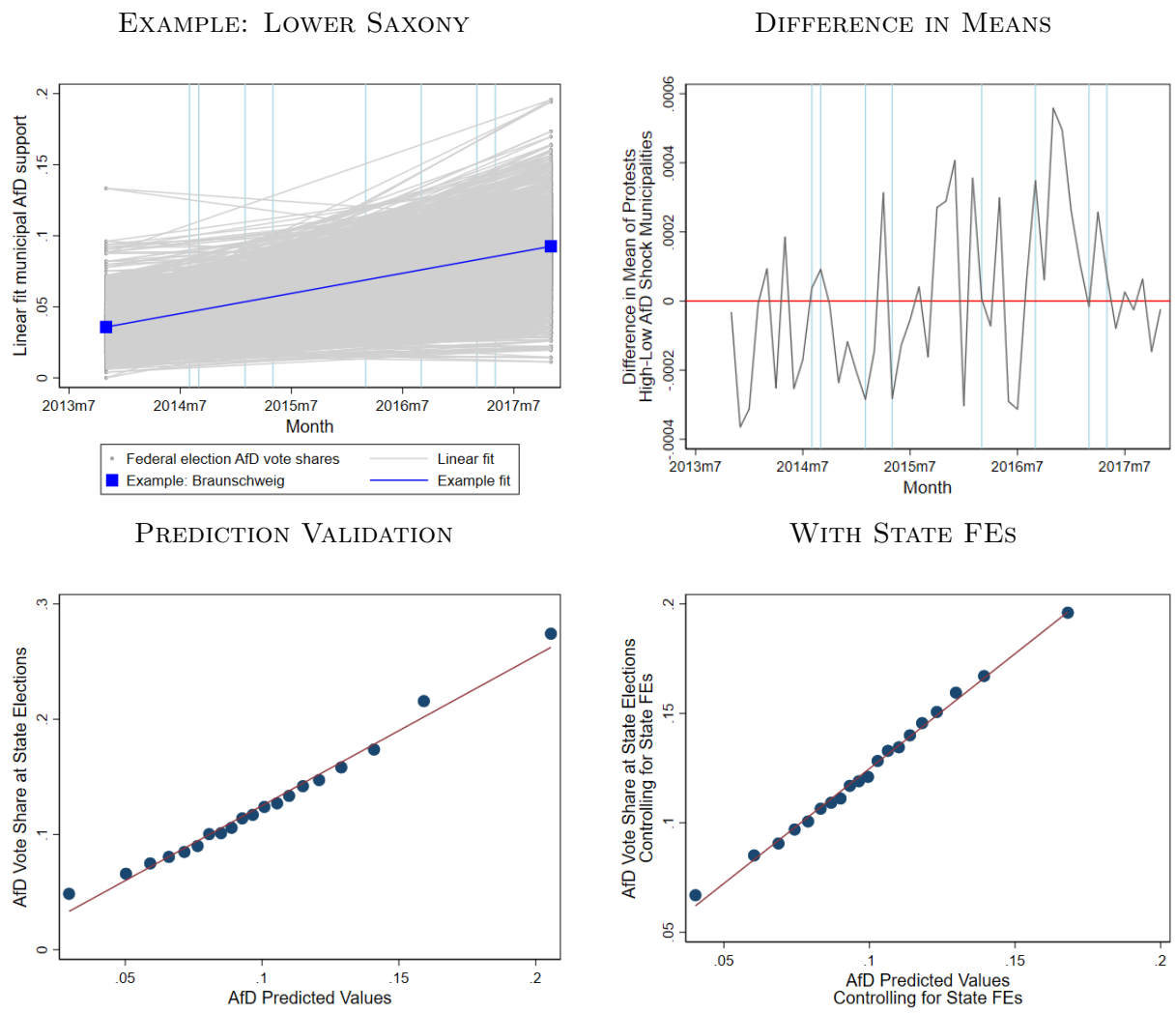


Attacks against Refugees



Notes: Graphs plot OLS regression coefficients of the interaction of AfD Shock with month dummies relative to state election for antisemitic incidents and attacks against refugees and their homes. Regressions include state, month, and relative month fixed effects, as well as AfD shock. Standard errors are clustered at the state-times-month level. Sample consists of 2-year monthly windows for German municipalities in states with state elections between the 2013-2017 federal elections, except city states. Omitted category is the election month.

Figure G14: AfD Shock Imputation in Non-Election States



Notes: The top left graph illustrates the imputation of municipal AfD support using Lower Saxony as example. Vertical lines indicate timing of other state elections. Each municipality's AfD support is imputed by fitting a line through its 2013 and 2017 AfD vote share at federal elections. The top right graph presents differences in mean of protests between high and low AfD shock municipalities, i.e. municipalities whose imputed AfD support is below or above the mean AfD support in a given month and given state. Vertical lines again indicate timing of state elections. The bottom two graphs objective is to validate the imputation. They show binned scatter plots of AfD vote shares at state elections and the imputed AfD support using the method illustrated in the top left graph. The bottom right graph shows the association controlling for state fixed effects.

Table G1: Correlation AfD vote share and AfD shock at state level election with 2013 federal AfD vote shares - before and after the rightward shift of the AfD (July 2015)

	Municipal AfD		AfD Shock	
	before	after	before	after
AfD 2013	0.549***	0.336***	-0.608***	-0.381***
Δ AfD 2013	-0.580***	-0.240***	0.612***	0.363***
Observations	1689	5931	1689	5931

Notes: Table reports correlation coefficients with significance stars. AfD 2013 refers to the municipal AfD vote share at the 2013 federal election. Δ AfD 2013 refers to differences in 2013 federal AfD vote shares from state averages. Municipal AfD denotes the AfD vote share of a municipality in a state election between 2013 and 2017. AfD Shock is the difference between the state average of a state election and the municipal AfD vote share. Before and after refers to the rightward shift of the AfD, dated at the party's congress in Essen in July 2015. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table G2: Pure municipal specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: No Controls											
AfD × Post	-0.0293***	-0.0204***	-0.0235***	-0.0190***	-0.0249***	-0.0121***	-0.0106**	-0.0162***	-0.0115**	-0.0098**	-0.0157***
	(0.0056)	(0.0050)	(0.0049)	(0.0048)	(0.0049)	(0.0046)	(0.0043)	(0.0044)	(0.0048)	(0.0045)	(0.0046)
Observations	373331	373331	373331	373331	373331	373331	373331	373331	373331	373331	373331
Panel B: With Controls											
AfD × Post	-0.0139***	-0.0097***	-0.0115***	-0.0084***	-0.0088***	-0.0103***	-0.0090***	-0.0095***	-0.0112***	-0.0099***	-0.0103***
	(0.0030)	(0.0029)	(0.0029)	(0.0029)	(0.0033)	(0.0028)	(0.0028)	(0.0031)	(0.0031)	(0.0031)	(0.0035)
State FEs		✓		✓	✓						
Month FEs			✓	✓	✓		✓	✓		✓	✓
Relative Month FEs					✓			✓			✓
County FEs						✓	✓	✓			
Municipality FEs									✓	✓	✓
Observations	256886	256886	256886	256886	256886	256886	256886	256886	256753	256753	256753

Notes: Table reports coefficients and standard errors from linear regressions. The dependent variable is the number of far-right protest marches. Independent variable of interest is the AfD vote share after a state election. Each column adds a different combinations of fixed effects. Panel A reports results without control variables, apart from 2013 AfD vote shares. Regressions in panel B include population, unemployment level, unemployment change, number of refugees, trade exposure and human capital controls, as well as 2013 AfD vote shares. The sample comprises $T = 49$ (months) and $N = 7,620$ (municipalities in states with state election in 2013-2017, except city states). Standard errors are clustered at the state-times-month level. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table G3: Main Results (OLS) - Event Study Period

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: No Controls											
AfD Shock × Post	0.0156*	0.0162*	0.0176**	0.0198**	0.0187**	0.0115*	0.0151**	0.0139*	0.0113*	0.0149**	0.0137*
	(0.0080)	(0.0089)	(0.0080)	(0.0092)	(0.0093)	(0.0065)	(0.0069)	(0.0071)	(0.0066)	(0.0070)	(0.0072)
Observations	177439	177439	177439	177439	177439	177439	177439	177439	177439	177439	177439
Panel B: With Controls											
AfD Shock × Post	0.0161**	0.0153**	0.0173**	0.0170**	0.0157**	0.0103*	0.0127**	0.0120*	0.0129**	0.0156**	0.0149**
	(0.0069)	(0.0068)	(0.0071)	(0.0070)	(0.0072)	(0.0060)	(0.0061)	(0.0063)	(0.0064)	(0.0066)	(0.0067)
State FEs		✓		✓	✓						
Month FEs			✓	✓	✓		✓	✓		✓	✓
Relative Month FEs					✓			✓			✓
County FEs						✓	✓	✓			
Municipality FEs									✓	✓	✓
Observations	120808	120808	120808	120808	120808	120808	120808	120808	120638	120638	120638

Notes: Table reports coefficients and standard errors from linear regressions. The dependent variable is the number of far-right protest marches. Each column adds a different combination of fixed effects. Panel A reports results without control variables, apart from 2013 AfD vote shares. Regressions in panel B include population, unemployment level, unemployment change, number of refugees, trade exposure and human capital controls, as well as 2013 AfD vote shares. The sample comprises $N = 7,620$ municipalities in states with state election in 2013-2017 (except city states) observed in 2-year windows around state elections. Standard errors are clustered at the state-times-month level. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table G4: Main Results (Poisson)

	State +Month + Relmonth FEs	Municipality +Month + Relmonth FEs	Extensive Margin	Full Sample	State-Month +Relmonth FEs
AfD Shock × Post	14.3591**	12.1409**	14.3997***	16.8953***	13.2434**
	(5.5869)	(4.8385)	(4.9266)	(3.9475)	(5.7122)
Observations	113676	1086	1086	256886	38705

Notes: Table reports coefficients and standard errors from poisson regressions. Dependent variable is the number of far-right protests. The AfD Shock variable is defined as the time-constant difference of a state election AfD result with the municipal AfD result at the time of the election (positive values denote higher state averages than municipal AfD vote shares). Sample includes monthly observations for German municipalities between the 2013 and 2017 federal elections in 2-year windows around state elections, and only in states that once had an election (except in column 4 where the full sample is used). All specifications use controls for AfD results at the 2013 federal election, refugees, population, unemployment, trade exposure, and human capital. Column 1 uses state, month, and relative month fixed effects. Column 2 uses municipality, month, and relative month fixed effects. Column 3 uses a binary the dependent variable (extensive margin). The full sample design in column 4 comprises $T = 49$ (months) and $N = 7,620$ (municipalities in states with state election in 2013-2017, except city states). Column 5 employs state-times-month and relative month fixed effects. Standard errors are clustered at the state-times-month level. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table G5: Summary Statistics of GSOEP variables

	(1) Obs.	(2) Mean	(3) Std.Dev.	(4) Min	(5) Max
Panel A: Individual Variables					
Interest in Politics	57,414	0.316	0.287	0	1
Acceptability	21,000	0.285	0.342	0	1
Mistrust Foreigners	6,832	0.454	0.350	0	1
Far-right Extremist	40,220	0.007	0.054	0	1
Pro-Refugees	41,809	0.040	0.131	0	1
Panel B: Aggregated at municipal level					
Interest in Politics	4,103	0.322	0.287	0	1
Acceptability	1,538	0.281	0.348	0	1
Mistrust Foreigners	500	0.467	0.359	0	1
Far-right Extremist	2,836	0.007	0.058	0	1
Pro-Refugees	2,928	0.041	0.135	0	1

Notes: Table reports number of observations, means, standard deviations, minimum and maximum of variables from the German Socio-Economic Panel (GSOEP) used in Table 2.9. Panel A reports statistics for variables at the individual level, panel B for the aggregated municipality level.

Table G6: Question used from GLES

Question 1: Immigrants must adopt German culture		Variable Name
Preamble:	There are different opinions on various topics in society. What is your opinion on the following statement? Immigrants should be compelled to adapt to German culture.	t308a
Possible Answers:	completely disagree (= 0); disagree (= 0); partly disagree (= 0); moderately agree (=1); completely agree (= 1)	
Question 2: AfD identification		
Preamble:	Generally speaking, do you have an inclination for a specific political party?	t46
Possible Answers:	CDU (= 0); CSU (= 0); SPD (= 0); DIE LINKE (= 0); GRÜNE (= 0); FDP (= 0); AfD (= 1); other party (= 0)	
Question 3: Left-right positioning AfD		
Preamble:	Where would you position the following party on a scale from 1 (left) to 11 (right)? AfD	t23a
Possible Answers:	1 - 11 (1 - 11)	

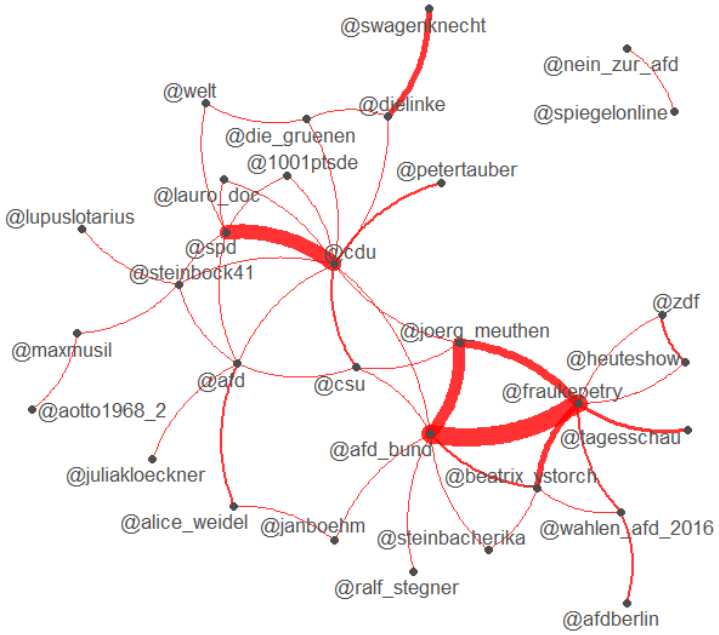
Notes: Table presents translations of questions used for GLES data.

Table G7: Question used from GSOEP

Question 1: Acceptability		Variable Name
Preamble:	Has recognition by others been a priority for choosing your job?	plb0530_h
Possible Answers:	Yes (= 1); No (= 0)	
Question 2: Political Interest		
Preamble:	Generally speaking, how interested are you in politics?	plh0007
Possible Answers:	Very interested (= 1); moderately interested (= 1); Not so interested (= 0); completely disinterested (= 0)	
Question 3: Far-Right Extremist		
Preamble:	Which party did you vote for [...] at the last general election (Bundestagswahl) on September 22, 2013?	plh0333
Possible Answers:	SPD (= 0); CDU (= 0); CSU (= 0); FDP (= 0); Die Grünen (= 0); Die Linke (= 0); AfD (= 0); NPD/ Republikaner/ Die Rechte (= 1); other party (= 0)	
Question 4: Pro-Refugees		
Preamble:	Have you done that since last year? Going to demonstrations or collecting signatures for initiatives to help refugees	plj0442
Possible Answers:	Yes (= 1); No (= 0)	
Question 4: Mistrust Foreigners		
Preamble:	What is you opinion on the following statement? It is better to be careful before trusting foreigners.	plm0502i03
Possible Answers:	Completely agree (= 1); Moderately agree (= 1); moderately disagree (= 0); disagree completely (= 0)	

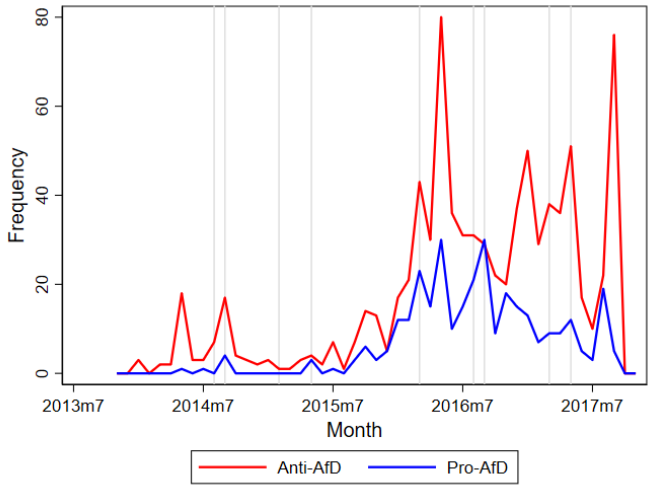
2.7.2 *Twitter Analysis Appendix Figures and Tables*

Figure H1: Network plot: top-40 users mentioned in #AfD tweets



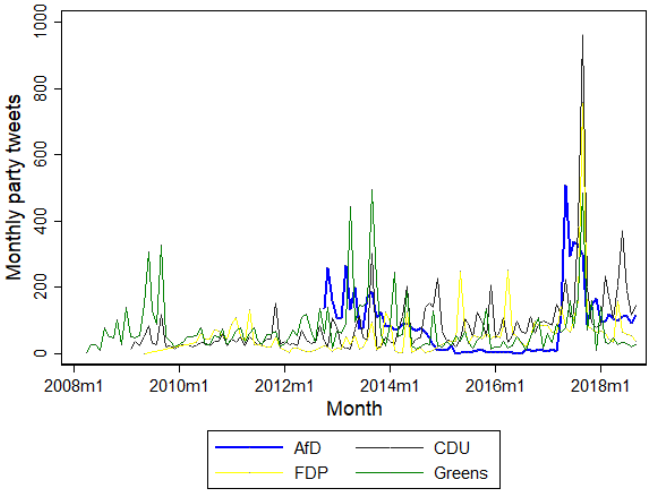
Notes: Graph shows a network plot of top 40 users mentioned in geolocalisable #AfD tweets. Thicker lines indicate higher co-occurrences of mentions within given tweets. The network plot shows both user names with AfD affiliations (such as joerg_meuthen and frauke_petry) as well as user names that are clearly anti-AfD (nein_zur_AfD).

Figure H2: Manually classified Geolocalisable #AfD Tweets



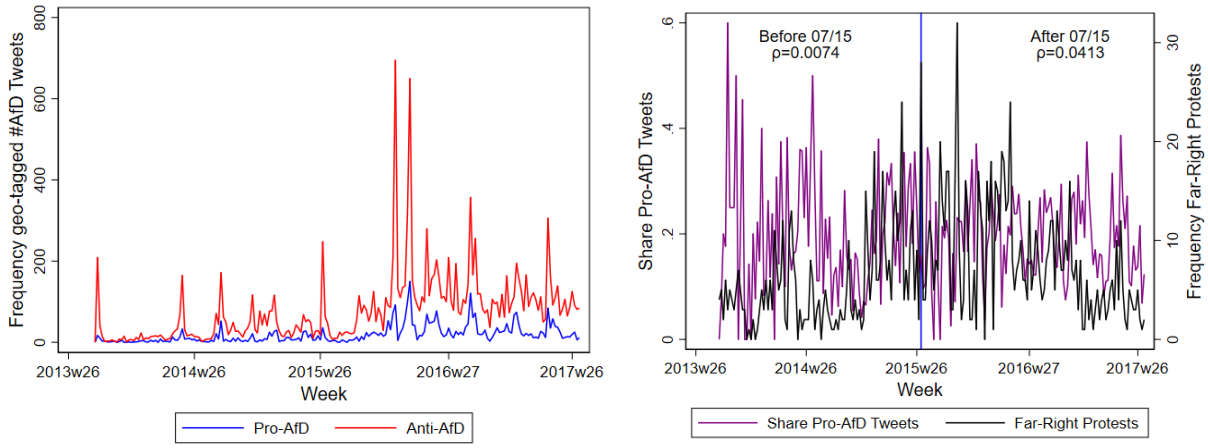
Notes: Graphs plots frequencies of manually classified anti- and pro-AfD geolocalisable Tweets over time. Shaded vertical lines indicate months of state elections.

Figure H3: Political party tweets



Notes: The graph plots frequencies of tweets from four selected Germany parties.

Figure H4: Tweets and Protests



Notes: The left graph shows frequency of geo-tagged tweets over time, classified by a naive bayesian algorithm into pro- and anti-AfD. The right graph plots the share of pro/anti-AfD tweets and the frequency of far-right protests over time.

Table H1: Top 15 words in manual #AfD tweet categories

Manual Classification	
Pro-AfD	Anti-AfD
#cdu	#pegida
#spd	#npd
#grüne	#noafd
#linke	#nazis
✠	@afd_bund
@1001ptsde	😂
wählen	#afd-
#merkel	wählen
wähler	partei
deutschland	gauland
heute	deutschland
warum	#cdu
#pegida	@faukepetry
@welt	#btw17
macht	#csu

Table H2: Tweets and Underestimation

	(1)	(2)	(3)	(4)
	Baseline All states	2014 Under-estimation	2016 Under-estimation	2017 No U.-estimation
Panel A: Anti-AfD Tweets				
AfD Shock × Post	0.0201**	0.0060	0.0353**	-0.0016
	(0.0086)	(0.0060)	(0.0159)	(0.0112)
Observations	177439	38421	90550	48468
Panel B: Pro-AfD Tweets				
AfD Shock × Post	0.0061*	0.0000	0.0083	0.0129*
	(0.0032)	(.)	(0.0052)	(0.0077)
Observations	177439	38421	90550	48468

Notes: Table shows coefficients and standard errors from OLS regressions. Dependent variable is the number of anti-AfD tweets (Panel A) and pro-AfD tweets (Panel B). Columns 2-4 restrict the sample to state elections according to underestimation of AfD vote share by polling institutes (see Figure 2.3) and according to before and after the rightward shift of the AfD (July 2015): Column 2 includes Saxony, Brandenburg, Thuringia. Column 3 includes Baden-Württemberg, Rhineland-Palatine, and Saxony-Anhalt. Column 4 includes Mecklenburg-Western Pomerania, the Saarland, Schleswig-Holstein, and North Rhine-Westphalia. All regressions include municipality fixed effects, as well as month and relative month fixed effects, and use the event-study sample of two-year windows around state elections. Standard errors are clustered at the state-times-month level. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Table H3: Tweets and Protests

	(1) 2013-2017 Full	(2) 2013-2017 Full	(3) 2013-2017 Full	(4) 2013-2017 Full	(5) 2013-2017 Full	(6) 2014 Under-estimation	(7) 2016 Under-estimation	(8) 2017 No U.-estimation
First Lead Pro-AfD Tweets	0.0255**				0.0223**	0.0227	0.0219*	0.0145
	(0.0113)				(0.0098)	(0.0245)	(0.0111)	(0.0104)
First Lead Anti-AfD Tweets	0.0027				-0.0001	0.0042	-0.0041**	0.0036
	(0.0025)				(0.0017)	(0.0039)	(0.0016)	(0.0028)
Pro-AfD Tweets		0.0113*			0.0005	-0.0071	-0.0170*	0.0095**
		(0.0066)			(0.0029)	(0.0138)	(0.0083)	(0.0038)
Anti-AfD Tweets		0.0064**			0.0041**	0.0052	0.0017*	0.0063**
		(0.0032)			(0.0016)	(0.0035)	(0.0008)	(0.0029)
First Lag Pro-AfD Tweets			0.0161**		0.0144**	0.0256	0.0126	0.0157**
			(0.0078)		(0.0063)	(0.0250)	(0.0102)	(0.0060)
First Lag Anti-AfD Tweets			0.0048		-0.0000	-0.0034	-0.0004	-0.0009
			(0.0030)		(0.0014)	(0.0041)	(0.0021)	(0.0031)
Second Lag Pro-AfD Tweets				0.0041	-0.0029	-0.0304*	0.0053	0.0013
				(0.0057)	(0.0035)	(0.0148)	(0.0065)	(0.0043)
Second Lag Anti-AfD Tweets				0.0061*	0.0031*	0.0037**	0.0041	0.0005
				(0.0035)	(0.0017)	(0.0016)	(0.0023)	(0.0019)
Observations	552295	552296	552295	552294	552293	259231	146523	146539

Notes: Table shows coefficients and standard errors from OLS regressions. Dependent variable is the number of far-right protests. Columns 6-8 restrict the sample to state elections according to underestimation of AfD vote share by polling institutes (see Figure 2.3) and according to before and after the rightward shift of the AfD (July 2015). Sample consists of monthly observations of all German municipalities. All regressions include municipality and month fixed effects. Standard errors are two-way clustered at the municipality and month level. One, two and three stars represent significance at the 10%, 5%, and 1% levels respectively.

Chapter 3

The Impact of Reducing School Duration on Gap Year Participation and Trust in the EU

3.1 Introduction

The decision of the United Kingdom to leave the European Union might be seen as symptomatic of what has been called the "European trust crisis" in recent economics literature (Dustmann et al., 2017). Low levels of trust not only engender the rise of populist parties (Algan et al., 2017), but also impede economic growth (Bjørnskov, 2017) and cast doubt on the European project itself (Ciaglia, Fuest, and Heinemann, 2018). It is, thus, not surprising that the question of how to promote trust into the European Union and its institutions has recently received substantial attention in both economics and political science.

Our paper sheds light on an interesting – though unintended – link between a school reform in Germany and trust levels in European Union institutions. Introduced subsequently in different German federal states in the early 2000s, the so-called G8 reform lowered school duration by one year for German academic-track high school students. In comparison to previous cohorts, treated individuals "gained" one year after graduation. The goal of the reform was to allow for earlier job market entries by academic-track high school and university graduates and to increase the tax base in view of demographic change in Germany. Many students, however, opt to delay entry into university or the labour market and choose to do a gap year abroad. We hypothesise that this contact with other countries led to a higher trust in EU institutions.

We show that a non-negligible share of treated graduates waits before entering the next career phase of their life after graduation, such as an apprenticeship, university, or employment. About half of the individuals in our sample use this time for voluntary services, which is often done abroad. Thus, the reform indirectly affected international experiences, which then affected attitudes towards

EU institutions.[1] The G8 reform is associated with a 20 percent increase in the probability to show trust in European Parliament and Commission in our analyses. Differentiating between different types of trust, we can show that the reform effect did not increase trust in general or trust in political parties, or political interest.

Our work is related to literature on the importance of trust and especially trust in EU institutions. First, trust has been recognised as a key outcome in the economics literature (e.g. Alesina and Giuliano, 2015; Algan and Cahuc, 2014). Dustmann et al. (2017) provide an extensive overview on the roots of distrust in EU institutions. Algan et al. (2017) show how economic insecurity after the Great Recession has led to both rising vote shares of populist parties and lower levels of trust in EU institutions. Dotti Sani and Magistro (2016) show that the decline in trust in the European Parliament after 2009 was most pronounced in countries that were hit hardest by the economic crisis, and among subjects with low social status.

The paper proceeds as follows. Section 3.2 provides information on the G8 reform in Germany. Section 3.3 details the data we use. Section 3.4 specifies our econometric setting. Main results are presented in section 3.5. Section 3.6 shows that the G8 reform did not generally affect trust levels and in particular in politics. Section 3.7 discusses further potential channels how the G8 reform may have affected trust in EU institutions, besides increasing gap year uptake. Section 3.8 concludes.

3.2 Background on the G8 Reform

The German G8 reform reduced school duration at academic track high schools from previously nine to eight years post-reform. The goal of the reform was to achieve younger high school graduates in order to compensate for demographic change, and increase international competitiveness by earlier job market entries.[2] The reform was never intended to increase gap year participation, volunteering, or affect trust in institutions, but was specifically labour market oriented.[3] Between 2001 and 2008,

1. Research on whether encouraging international exchanges and interactions impacts trust has come to mixed conclusions. Whereas Stoeckel (2016), for example, argues that social interaction abroad contributes to a European identity (and thus probably also to higher levels of trust into the EU), Kuhn (2012) argues that the Erasmus programme as a large exchange program misses its mark by addressing university students who are already very likely to feel European. On top of this selection and "preaching to the converted", Sigalas (2010) finds that Erasmus does not strengthen students' European identity, but on the contrary can have an adverse effect on it.

2. The Ministry of Education in Saarland (Ministerium für Bildung Kultur und Wissenschaft, 2001), for example argued: German high school graduates, and thus, also university graduates were at a disadvantage internationally due to their comparatively old age at graduation. Additionally, earlier job market entries would serve to increase the tax base to compensate for demographic change in Germany. One can expect the introduction of G8 to be independent of trust in European institutions. In another example Bavarian Parliament argued: "Germany is one of the countries with the longest education duration. Our university graduates are on average too old in international comparisons. [...] The reduction in school duration is furthermore essential on a social level. Long training periods [...] place a burden on our social security systems and inter-generational consensus." (Bayerischer Landtag, 2004)

3. Changes in trust in EU institutions were an unintended side-effect of the reform. The increase in gap year participation, for example, was also part of the public discussion on how the reform failed to produce the younger university graduates it had originally aimed for. See, for example, Michler (2017), or Meck (2017).

14 out of 16 German federal states reduced their school duration. It affected students beginning with school cohorts 1999. Academic track high schools are attended by over 30% of children in Germany (Hoffmann and Malecki, 2018).[4]

Figure 3.1 shows the timeline of reform introductions for all federal states.[5] The school years when the G8 reform was introduced are listed here, as well as the school cohorts that were affected by the reform. The reform was first introduced in Saarland in 2001/2002. The first affected cohorts, however, started secondary school in 1999 with the introduction of the reform in Saxony-Anhalt in 2003/2004. Altogether, the reform was introduced in 14 federal states, the last being Rhineland-Palatinate, and Schleswig-Holstein in 2008/2009. Several federal states have announced the re-introduction of the old G9 system, starting with Baden-Wuerttemberg in 2013. These changes, however, do not, affect the dataset in this setting, as the last observed cohorts pre-date these changes. In line with previous G8 literature, we exclude Hesse, Thuringia, Rhineland-Palatinate, and Saxony from the analysis, as the reform was only partially introduced here or the federal states always had a shorter schooling system in place.[6] Given this stepwise introduction of G8 in federal states in Germany, the number of treated individuals increases from 1999 until there are only treated individuals after 2008.

The reduction of school duration was accompanied by an increase in schooling intensity due to university entry qualifications remaining unchanged.[7]

Previous study has shown that the G8 reform increased participation in voluntary service or staying abroad, and decreased university enrolment rates (Meyer, Thomsen, and Schneider, 2018; Marcus and Zambre, 2019; Büttner and Thomsen, 2015). There was no general increase in volunteering, however, as the increase in schooling intensity reduced leisure time and, hence, also pro-social behaviour at school (Huebner et al., 2017; Meyer and Thomsen, 2015; Krekel, 2017). Repetition rates have increased due to the reform (Huebener and Marcus, 2017), but graduates are younger at graduation (Marcus and Zambre, 2019) compared to untreated cohorts.

Our research confirms this research by providing evidence that the probability to engage in a gap year is substantially increased and that a large share of individuals who take time off after graduation engage in a voluntary year. We further add to this research by showing that the reform had an unintended positive effect on trust in European institutions.

4. The majority of participants in a voluntary year are also academic-track high school graduates (BMFSFJ, 2006; AKLHUE, 2018).

5. Information on reform years and implementation is based on Ständige Konferenz der Kultusminister der Länder in der Bundesrepublik Deutschland (2018).

6. See, for example, Dahmann and Anger (2014) for an early reference.

7. The Standing Conference of Education Ministers specifies that all upper secondary students need to fulfil at least 265 yearly week hours until graduation, independent of total school duration.

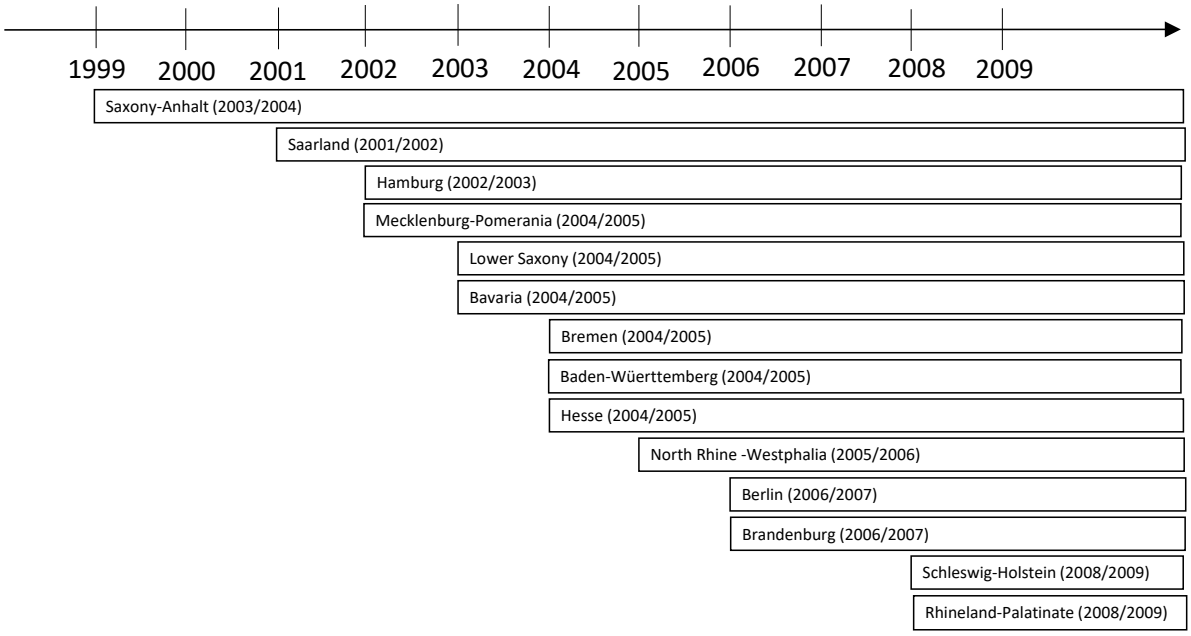


Figure 3.1: Timeline of introduction of G8 reform in the German federal states

Notes: The graph shows the timeline of school cohorts the G8 reform affected for all German federal states. It lists the year of reform introduction in parenthesis. Affected cohorts can pre-date the year of introduction as several federal states introduced the reform for more than just the first secondary school grade. Reform information is based on Ständige Konferenz der Kultusminister der Länder in der Bundesrepublik Deutschland (2018).

3.3 Data

The empirical analysis of this paper is based on two main sources. In order to show that the reform affected trust in the European Union, we use the German General Social Survey (allbus). We measure the increase in gap year uptake based on data from the German Socio-Economic Panel Study (SOEP). Many engage in a voluntary year during this sabbatical. We show that there has been a substantial increase in volunteering abroad.

3.3.1 Definition of Treatment

We define an individual as treated when she is part of a school cohort for which the reform was introduced or thereafter. All others are untreated and part of the old G9 system. There are two special cases of treatment. First, there is a double graduate year, where both the old G9 cohort and the new G8 cohort graduated during the same year. These cohorts may differ from others. Post-graduate competition, for example, for university was increased, which may have made these cohorts particularly likely to engage in a gap year. Second, several federal states introduced the reform for more than one grade, leading to partially treated cohorts who first experienced the old schooling system and where then treated. We routinely control for double graduate and partially treated individuals. Table I1 and Table I2 provide information on the distribution of treated and untreated individuals in both data sets.

SOEP data allows very accurate treatment assignment, as we often have direct information on the year, federal state, and type of school individuals enrolled in.[8] When information is missing, we extrapolate the federal state of secondary schooling from the current federal state of residence. School starting years are based on the year and month of birth taking individual states' deadlines for enrolment into account.[9] In the German school system it is generally the case that a child starts attending school if she turns six years before a certain deadline and waits one more year otherwise.[10]

Allbus provides information on the federal state where an individual grew up beginning with surveys in 2004. When this information is missing, we use the current federal state of residence as in SOEP. The survey further provides information on which type of school degree was achieved, hence, we can clearly identify academic-track high school students where treatment occurred. In allbus we fully rely on extrapolated treatment assignment based on the year and month of birth.[11]

8. Based on the cross-section of first observations for the gap year analysis.

9. We account for changes in deadline regulations across all federal states and across time. The deadline for school enrolment varies between June and December and was changed at several points in time for most federal states.

10. The extrapolated data shows that 50 percent of individuals started primary school by the age of six in the SOEP data. The Statistical Office lists 64% of all six-year-old children as enrolled in school in 2018 (Hoffmann and Malecki, 2018). Our number of treated individuals may be increased as having more seven-year-old children impacts school cohorts when the reform was introduced. These cohorts, however, are also part of the double graduate years when the last untreated and first treated cohorts graduated together, which we routinely control for.

11. Treatment assignment is, thus, noisier in allbus than in SOEP. When we compare treatment assignment in

SOEP data is more extensive and includes younger respondents than allbus, thus, our sample of treated individuals is larger here. Limited to academic-track student, where the reform was introduced, we have about 40 percent treated in SOEP and a little below 30 percent treated in allbus.

3.3.2 Trust in the European Union

Our source of data on trust in the European Union is the German General Social Survey (allbus) of the Leibniz Institute for the Social Sciences (gesis). The allbus survey provides rich data on attitudes and political opinions in Germany, and is conducted every two years as a repeated cross-section amongst a representative sample of on average 3,000 individuals. We restrict our sample to individuals born after 1980 in order to ensure comparability of cohorts. We have information on trust in European institutions from survey years 2000, 2008, and 2018. We obtain a sample with about 330 individuals with a university entry school degree. Given the small sample size, we additionally include students with other secondary degrees as untreated individuals. This increases our sample size to 861 individuals. This has the advantage of allowing for more statistical power, but it only increases the number of untreated individuals in our sample. Table I3 provides summary statistics for the sample. The number of observations is balanced on basic controls.

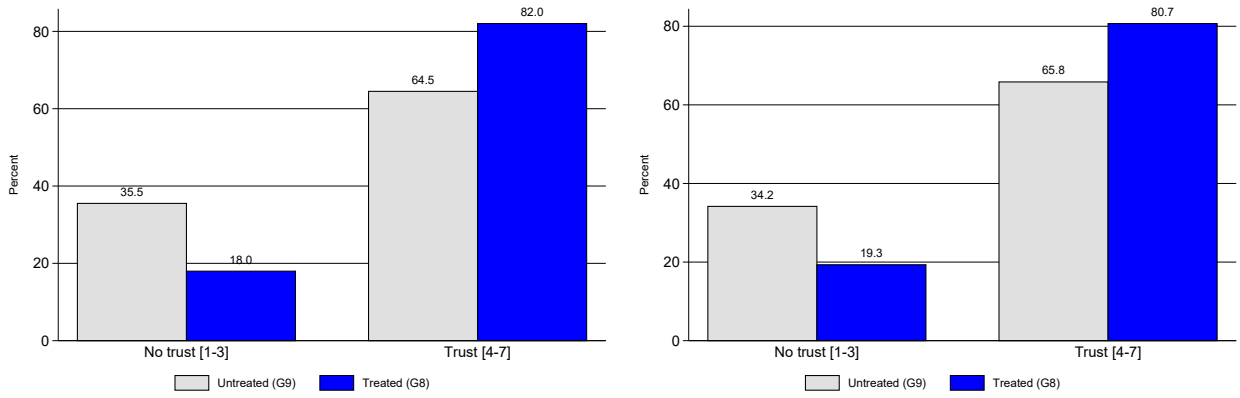
For the main outcomes of interest, individuals are asked to rate how much they trust institutions of the European Union on a scale from one (no trust at all) to seven (trust very much). For the main analysis, we define each variable of trust as a binary variable that turns one if the individual states that she trusts EU Parliament or Commission at least on a level of four, and zero if trust is below this. Figure 3.2 provides a comparison of the share of individuals in treated and untreated cohorts for these binary variables. In our academic-track high school graduate sample, about 69 percent trust EU parliament (65 percent in the overall sample) and 70 percent trust EU Commission (66 percent in the overall sample) at least on a level of four. Both graphs show an increase in the probability of trusting either EU institution for treated relative to untreated individuals. In additional robustness checks we test the reform effect on trust measured as discreet variables. Figure I1 in the appendix provides the same graph distinguished at the seven categories of trust levels instead of the binary variables.

3.3.3 Gap Year

We propose that a main contributor for the increase in trust in EU institutions is increasing gap year uptake in treated cohorts. We base this analysis on SOEP long format information from 2018 which provides a wide array of household and individual based information.[12] Every year

SOEP based on the "true" year of school enrolment and the fully extrapolated data we see that only 1 out of 900 individuals is re-assigned from the control to the treatment group. We, therefore, consider extrapolated treatment assignment reliable.

12. This paper is based on the 34^{th} wave, encompassing survey data from 1984 to 2017.



Trust in EU parliament

Trust in EU commission

Figure 3.2: Mean distribution for trust in EU parliament (a) and trust in EU commission (b) for treated and untreated cohorts

Notes: The graph shows means of different trust. We define trust as one if an individual states she trusts parliament/the commission at least on a level of four on a scale from one (no trust at all) to seven (high trust). Treated individuals (in blue) are G8 and untreated individuals (in grey) are G9. The sample is reduced to academic-track high school graduates.

over 25,000 households are surveyed. The SOEP includes readily usable education and occupation information. Based on this information we can construct a timeline of the career path for about 2,000 individuals which allows us to identify delays in entering university, a job, or an apprenticeship after graduating from high school.

We focus the analysis on individuals born after 1980 as above. We also limit the analyses to students at academic-track high schools for the main analyses, as these were unambiguously affected by the reform, and the SOEP data base is large enough to still identify a meaningful effect in this reduced sample.[13]. In 2000, the pool of participants in SOEP was enlarged beyond the adult age and includes adolescents at the age of 17. This survey additionally allows identification of childhood background information, as well as linking individuals to parents' backgrounds. We focus the analysis on survey years 2000 to 2017 when this additional information is also available. Our analysis is based on a cross-section of first observations when individuals are no longer at school and for whom we could identify the next step in life after graduation. The average age in our sample is 20.

Participation in a gap year is the main outcome of interest here. Gap year is a binary variable that turns one if an individual is at least one year older between achieving the high school degree and the next step. Figure 3.3 shows the mean uptake of a gap year in comparison for treated and untreated cohorts. In the cross-section based on observations for whom we observe all control

13. Comprehensive schools are, thus, excluded, as well as basic- and middle-track schools of the three-tiered German high school system

variables, 40 percent[14] of individuals engages in a gap year after graduation.[15] On average, treated individuals are more likely to participate in a sabbatical after graduation compared to untreated individuals. Table I4 provides descriptive statistic. The results are also confirmed at the intensive margin, measuring the number of years between career phases.[16]

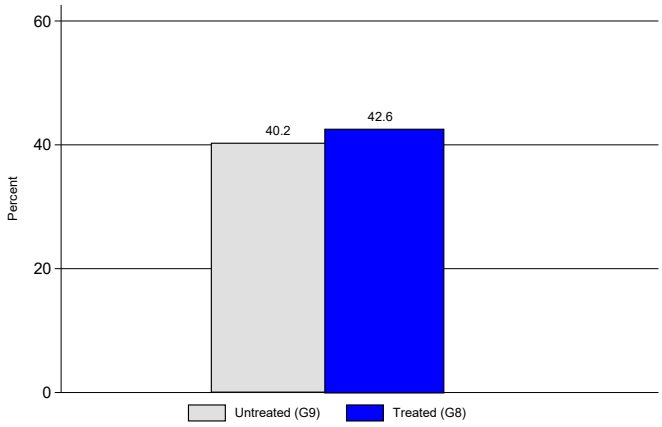


Figure 3.3: Mean distribution of gap year participation for treated (G8) and untreated (G9) individuals

Notes: The graph shows share of individuals who take a gap year after graduation for treated individuals (in blue) and untreated individuals (in grey) for the cross-section of first observations in the SOEP sample.

3.3.4 Further Outcomes

Half of the individuals in a gap year take part in a voluntary year in our sample. We measure volunteering as a dummy variable that turns one if an individual states that she is engaged in a voluntary service based on the youth questionnaire or her occupation information. Volunteering can, for example, be a civic, environmental, or military year. The official state-organised voluntary year was introduced in 2011 after the abolishment of compulsory military service in all federal

14. In the unbalanced cross section 30 percent show a gap between graduation and the next step. We are aware that we measure a large percentage of gap year takers. Gap year, may, however, also be generic in the sense that the next phase in life is simply reported after having turned one year older the same year. Measurement error is possible, but only of concern if it were correlated with treatment and systematically different for treated and untreated cohorts. We use the same sources of information for all individuals and only include individuals where we have listed information on their next phase in life after graduation. Furthermore, the implied increase in gap year participation relative to the mean in the main analyses of section 3.5 compares well to the increase of voluntary service participation, which is more precisely measured.

15. Almost 80 percent of gap year takers are one year older at their next phase in life. Voluntary services, for example, are often for one year up to 24 months.

16. Figure I2 shows a comparison of the age gap between graduation and the next career step for treated ("G8") versus untreated ("G9") cohorts. The figure shows the mean distribution of individuals who are the same age when graduating school and when continuing with university, an apprenticeship, or employment, individuals who are one year older, two years older, or three years older.

states. Before that, there were already vast options via private but state-sanctioned organisations, such as, through the church or welfare organisations. The Ministry of Family Affairs provides a list of organisations that offer civic and environmental voluntary year services (BMFSFJ, 2020). Volunteering, and in particular volunteering abroad, has substantially grown in demand over the past decade. Since its founding in 2012, for example, state-organised volunteering grew by about 20 percent in 2017 to about 42,000 participants (BAFZA, 2012; 2017). There are ten central offices organising regional agencies. Seven of these organisations also have specific listings for volunteering abroad. An evaluation of volunteering abroad lists over 7,000 individuals volunteering abroad in 2017.[17] A majority of activity is Europe-based (AKLHUE, 2011; 2018).

In addition to these results we provide further analyses. These are mainly based on SOEP data. We show that the G8 reform did not affect trust levels in general. We measure trust as binary variable that turns one if individuals state that they trust people in general at least on level four out of seven, and zero otherwise. Our sample consists of 1,338 individuals of whom more than 50 percent show general trust. We further show that the reform did not increase political interest in general. Political interest is provided on a scale from one (no interest) to four (very high interest). We measure political interest as a dummy variable for 2,359 individuals that turns one if an individual states at least being interested (level two), and zero otherwise. About 32 percent if individuals are interested in politics in general in this sample. Based on allbus we show that trust in political parties is also not affected by the reform based on 1,517 individuals. Finally, we estimate whether the G8 reform impacted the probability of individuals going abroad during their school time in order to show that we do not simply observe a displacement effect of spending time in a foreign country later in life after the reform. Thirteen percent of our sample of 2,218 individuals spent time abroad during high school.

3.3.5 Covariates

We routinely control for a rich set of fixed effects at state, cohort, and survey level. In addition, we include a variety of potentially confounding factors that may be correlated with both the reform and our outcomes of interest. These include individual level controls for age when answering the survey, age squared, gender, migration background/German nationality, living in eastern Germany, and being part of the double graduate cohort or partially treated.

Given the rich background information in SOEP, we can additionally include having lived in the countryside as a child. We further control for family background by adding parental controls for having a blue-collar-job working parent, a parent that is married versus single, having a parent with a tertiary degree, and having at least one sibling. In order to control for the financial background, we also include household income in additional robustness checks and occupational controls for

17. Information is based on a survey amongst international volunteering organisations from 2017 in cooperation with the Ministry of Family Affairs. More than 90 percent of volunteers abroad use state-organised programs. Figure I3 in the appendix shows the development of international volunteering from 2005 to 2017.

holding a blue-collar job, and being employed. These are not part of our preferred regression design, however, as this may also be endogenously influenced by the G8 reform. In gap year and voluntary service analyses we additionally control for past cohort's participation in the compulsory military or civic service.

3.4 Econometric Setting

We conduct two separate main analyses. In both these analyses we rely on the sequential introduction of the G8 reform across federal states and use a difference-in-difference design.

We first show that that the reform is connected to higher trust levels towards European Parliament and European Commission:

$$Trust_i = \alpha_0 + \alpha_1 G8_{s,c} + X_i'\lambda + \mu_s + \theta_t + \gamma_c + \epsilon_{1,i},$$

with the main coefficient of interest α_1 measuring the impact of the G8 reform in state s and school cohort c on the probability of trusting EU Parliament or Commission of individual i. We include state level, μ_i, and cohort, γ_c, fixed effects. This means we are taking out all differences in trust towards EU institutions that exist between cohorts, for example, because younger people in general have a more favourable view of the EU. By including state fixed effects we control for differences in trust between different states that are constant over time. For additional robustness we also include survey year, θ_t, fixed effects here. The number of surveys is fairly low in these analyses, and we mainly rely on information from 2018. Individual level control variables as described in the last section are gathered in vector X_i.

We argue that a main contributor for this unintended side-effect of the reform is due to an increase in probability to take a gap year due to the reform:

$$GapYear_i = \beta_0 + \beta_1 G8_{s,c} + Z_i'\phi + \mu_s + \gamma_c + \psi_t + \eta_g + \epsilon_{2,i},$$

with the main coefficient of interest β_1 measuring the impact of the G8 reform in state s and school cohort c on the probability of participating in a gap year of individual i. The model includes state, μ_s, and cohort, γ_c, fixed effects. The main estimations additionally include survey year fixed effects, ψ_t , in order to account for factors that may have affected answers in a particular survey year. We also include survey sub-group fixed effects, η_g, in the main specifications as SOEP is conducted for different focus groups. Individual level control variables are gathered in vector Z_i.

Standard errors are clustered at the state level. We account for the low number of clusters by additionally applying wild bootstrapping according to Cameron, Gelbach, and Miller (2008) and Roodman et al. (2019).

Several concerns arise in this analysis. First, the empirical approach relies on the comparison of cohorts prior and after the school reform based on students' school entry dates and on the

states' introduction dates of the shorter schooling system relative to individuals in states with the previous system. Underlying this approach is the assumption that cohorts would not have developed differently in absence of the reform. Several additional school reforms were introduced between 2005 and 2014.[18] None of these are perfectly collinear with the introduction of the G8 reform. They impacted the schooling system, and may, hence, influence school duration and also post-graduation decisions, however. These include the introduction of central exit examinations in eight states instead of individual school exams. The tracking grade at which students first change from primary to secondary school was changed in three states.[19] The secondary schooling choices outside of academic-track schools were limited in five states through combination of lower and middle secondary schools into one comprehensive school. Most importantly, seven federal states first introduced and then retracted university tuition fees. We test robustness of our main results by including dummy variables that turn one if an individual was affected by these reforms.

Second, and equally important for causal inference is the assumption that no sample selection occurred. It stands to reason that selection would have generated substantial costs as the G8 reform was introduced quickly for entire federal states and families would have had to move to different states in order to select in or out of the G8 treatment. Selection bias, therefore, seems unlikely. Furthermore, previous research has shown that graduation rates have not been affected by the G8 reform (Huebener and Marcus, 2015; 2017). The total number of grammar school students was not affected by the reform and has remained high (Hoffmann and Malecki, 2018).[20]

Third, Goodman-Bacon (2018) amongst others recently stressed the importance of changing treatment effects through variations in treatment timing. We, therefore, also provide event study analyses to explore the treatment effect over time.[21]

Fourth, there naturally might be several channels explaining why the reform may influence attitudes towards Europe. We consider gap year participation, for example used for volunteering, a very likely and plausible causal channel. In section 3.7 we survey existing research for alternative reform effects and present an evaluation of the possibility that these may present alternative channels for the G8 reform impacting trust in European institutions.

18. See, for example, Meyer, Thomsen, and Schneider (2018) or Huebener and Marcus (2015).

19. The tracking year in Germany varies between 5^{th} and 7^{th} grade.

20. Dahmann and Anger (2014), Andrietti (2015), Huebener, Kuger, and Marcus (2017), or Meyer and Thomsen (2015), for example, have previously analysed the possibility of selection bias in this treatment and come to the same conclusion.

21. It is important to note that the event study relies on the assumption that the treatment effect did not vary for the different federal states introducing the reform. The reform was introduced similarly throughout all federal states. All states experienced similar discussions after its introduction. Apart from the above discussed additional school system reforms, such as university fees, which we control for, we could not find evidence for differences in post-graduation decision processes across treated federal states.

3.5 Main Results

3.5.1 Trust in European institutions

Table 3.1 shows the first set of main results for the impact of the G8 reform on trust in EU Parliament in columns (1) to (4) and in the Commission in columns (5) to (8). Columns (4) and (8) show the effect limited to the sample of academic-track high school graduates where treatment effect is most accurately assigned, but the sample is small with only about 300 observations. In all other columns we additionally include non academic-track high school graduates in order to increase our sample size. We define all non-academic-track high school graduates as untreated.

Panel A includes all cohort fixed effects. We see a positive reform effect on trust in EU Parliament, which is close to the ten percent significance level. The small sample (in particular of treated individuals) raises concerns of too little statistical power, however. We have about 300 individuals at academic-track high school school, of whom 89 are treated. Adding all fixed effects implies we have over 30 dummy variables in the regression, thus, leaving few observations per independent variable. In panels B and C we account for this problem in two different ways. First, in Panel B, we substitute the school cohort fixed effects by a linear trend in order to still account for changes across these cohorts but reduce the number of covariates in our analysis.[22] The results show that coefficients are increased, whereas standard errors remain mostly very similar. P-values show a connection that is close to the ten percent significance level in particular for trust in Parliament in columns (1) to (4). Second, in Panel C, we, again include cohort fixed effects, as we take this as the most accurate model specification. Here we account for potential over-fitting by omitting insignificant cohort fixed effects as shown in Figure I4.[23]

In all panels we see positive coefficients and often close to significant reform effects. Panel C shows that the reform led to significant increase in trust when accounting for small sample problems. We see that the probability to trust European Parliament is higher by between 12 to 14 percentage points. Relative to the sample mean of 0.65 this implies an increase of roughly 19 percent[24] when including individual level controls in column (3).[25] The robustness checks in the appendix in Table I6 show that these results also hold when applying a probit instead of a linear probability model and when including additional school reform controls.

Trust in EU Commission shows a similar tendency. We see a significant increase in the probability to trust the Commission in treated cohorts of about 13 to 18 percentage points in Panel C. This implies an increase by 20 percent relative to the sample mean of 66 percent in column (7). The bootstrapped p-value is just above the 10 percent level when including individual controls. The robustness checks in the appendix show that the reform effect is significant here as well at the

22. We test an alternative quadratic trend and results are qualitatively the same.
23. The authors thank Marco Caliendo for pointing this out.
24. Calculated as $\frac{0.12}{0.65} = 0.185$
25. Table I5 shows the full regression results.

intensive margin and when including additional reform controls.

Table 3.1: The effect of decreasing school duration on the probability to trust EU institutions

	Trust Parliament				Trust Commission			
	(1) FE	(2) Survey FE	(3) Individual Controls	(4) Academic track	(5) FE	(6) Survey FE	(7) Individual Controls	(8) Academic track
Panel A. All cohort FE								
Reform	0.096	0.100	0.095	0.143	0.058	0.063	0.057	0.098
	(0.056)	(0.058)	(0.065)	(0.153)	(0.052)	(0.055)	(0.060)	(0.160)
Observations	864	864	864	334	847	847	847	328
WB p-value	0.113	0.111	0.162	0.500	0.310	0.301	0.366	0.644
Panel B. Linear cohort trend								
Reform	0.105	0.112	0.112	0.166	0.080	0.088	0.091	0.124
	(0.066)	(0.069)	(0.071)	(0.110)	(0.059)	(0.062)	(0.064)	(0.110)
Observations	864	864	864	334	847	847	847	328
WB p-value	0.135	0.128	0.133	0.123	0.218	0.200	0.200	0.288
Panel C. Significant cohort FE								
Reform	0.121*	0.136**	0.118*	0.145	0.154**	0.179**	0.125*	0.160
	(0.059)	(0.062)	(0.063)	(0.116)	(0.064)	(0.061)	(0.065)	(0.101)
Observations	864	864	864	334	847	847	847	328
WB p-value	0.048	0.026	0.077	0.232	0.050	0.016	0.112	0.192
Mean	0.653	0.653	0.653	0.692	0.658	0.658	0.658	0.698
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Survey year FE		✓	✓	✓		✓	✓	✓
Basic controls			✓				✓	
Clustered SE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The dependent variable in columns (1) to (4) is a dummy variable that turns one if an individual trusts EU Parliament at least on a level of four on a scale up to seven (high trust), and zero otherwise. The dependent variable in columns (5) to (8) is a dummy variable that turns one if an individual trusts the EU Commission at least on a level of four out of a scale up to seven (high trust), and zero otherwise. The mean values of the dependent variables are provided at the bottom of the table as "Mean". Panel A shows the regression results when including all cohort fixed effects. Panel B alternatively includes a linear trend variable for school cohorts. Panel C shows the regression results when including only significant cohort fixed effects in order to avoid over-fitting in this small sample. We include cohort year FE for: 1997, 1998, 1999, 2000, 2002, 2005, 2007, 2008, 2009 which all affect the outcome at a level of 1 percent significance in columns (1) to (4). We include cohort year FE for: 2002 and 2009 which affect the outcome at a level of 1 percent significance in columns (5) to (8). The main independent variable, reform, equals one if the individual was affected by the G8 reform in his or her federal state. Control variables are as indicated. The number of observations in all regressions is balanced on basic controls. Standard errors in parentheses are clustered at federal state level. The p-value when correcting standard errors according to wild-bootstrapping are provided at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Thus, the results reveal a significant increase in trust in both EU institutions of about 20 percent amongst treated relative to untreated cohorts. Stability of these results vary due to our small sample of treated. The event-study analysis in section 3.5.3 adds further evidence to the here presented results.

As the reform was, clearly, never intended as a means to increase support of European institutions, the reform effect is obviously an indirect one, driven by a different channel. The following section shows that gap year participation is significantly increased amongst treated cohorts and

often used for volunteering.

3.5.2 Gap Year Uptake

Our second set of results show that the G8 reform did indeed lead to significant uptake of gap year participation. Table 3.2, Panel A, presents the results when estimating the G8 reform on the binary variable indicating that an individual has a career path gap after graduation of at least one year. We first include cohort and state fixed effects in column (1), and subsequently add further controls. Column (2) includes survey year and survey sub-group fixed effects. Column (3) also includes individual level controls as described in the data section above. Column (4) adds family background controls for parents' and siblings' information. Column (5) controls for (past cohorts') participation in compulsory military service or its substitution by civic services. This is our preferred setting. Column (6) adds linear time trend variables interacted with dummy variables for reform regions as additional controls.[26]

In all settings we see a significantly higher gap year uptake in treated relative to untreated cohorts. The coefficient suggests a sizeable increase of about 14.9 percentage points in column (5). Relative to the mean of gap year participation in this sample of 42 percent, this implies that the probability to take time off after graduation has increased due to the reform by about 35 percent.

The results are very robust. Table I9 includes income and occupational controls, as well as additional school reform controls. The coefficient measuring the effect of the G8 reform remains significant and similar in magnitude throughout. Coefficient magnitude is reduced when controlling for compulsory military or civic service in column (5), which is natural as it implied a gap by law for a large part of male graduates before it was discontinued in 2011. The table further shows that the results also hold in the fully balanced sample, when applying a probit instead of the linear probability model and at the intensive margin.

We also estimate the reform effect specifically on the probability to engage in voluntary services after graduation in Panel B of Table 3.2.[27] We see a significant increase in the probability to participate in a voluntary year of 5.6 percentage points.[28] Our preferred estimation setting again includes all fixed effects, individual level controls, family background controls, and controlling for compulsory military or civic year participation in column (5). Without the reform, about one out of five graduates volunteered. Due to the reform effect this increased to about one out of four graduates.[29]

26. Table I7 in the Appendix provides the full regression results.

27. Table I8 in the Appendix provides the full regression results.

28. Meyer, Thomsen, and Schneider (2018), for example, find an increase of 10 percentage points in the probability to engage in a voluntary year comparing 2008 (pre reform) graduates to 2012 (post reform).

29. Relative to the sample mean of 0.19, a 5.6 percentage point increase implies a non-negligible increase of about 29 percent due to the decrease in school duration by one year.

Table 3.2: The effect of decreasing school duration on the probability to take a gap year and voluntary year

	(1) FE	(2) Survey FE	(3) Basic controls	(4) Background controls	(5) Comp. service	(6) Linear trend
Panel A. Gap year (mean: 0.4)						
Reform	0.175***	0.161***	0.189**	0.194***	0.149**	0.166**
	(0.025)	(0.026)	(0.064)	(0.058)	(0.053)	(0.057)
Observations	2,196	2,196	2,196	1,930	1,930	1,930
WB p-value	0.000	0.000	0.044	0.031	0.039	0.026
Panel B. Voluntary year (mean: 0.19)						
Reform	0.156***	0.142***	0.138***	0.112***	0.056**	0.064**
	(0.023)	(0.021)	(0.043)	(0.036)	(0.023)	(0.025)
Observations	2,196	2,196	2,196	1,930	1,930	1,930
WB p-value	0.000	0.000	0.029	0.035	0.021	0.039
Cohort FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Survey year FE		✓	✓	✓	✓	✓
Sub-sample FE		✓	✓	✓	✓	✓
Basic controls			✓	✓	✓	✓
Family controls				✓	✓	✓
Compulsory service					✓	✓
Linear trend						✓
Clustered SE	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is a dummy variable that turns one if an individual in a gap year as taking time off after graduation before going to university, starting an apprenticeship, or occupation in Panel A. Results are balanced on basic controls. The mean value of the dependent variable in Panel A is 0.415. Panel B shows the regression results in the cross section of first observations balanced on basic controls for the probability to engage in voluntary military or civic service after graduation. The mean value of the dependent variable is 0.19. The main independent variable, reform, equals one if the individual was affected by the G8 reform in his or her federal state given school starting year. Control variables are as indicated. Standard errors in parentheses are clustered at federal state level. The p-value when correcting standard errors according to wild-bootstrapping are provided at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

3.5.3 Event Study Analysis of the Reform Effect

We add further evidence to our findings above in an event study analysis. We change the econometric model from measuring an aggregate reform effect to analysing the effect for the individual school cohorts in order to show the dynamic of gap year uptake and trust in EU Parliament responses regarding the introduction of the G8 reform. We expect to see a rise in trust and gap year participation before the reform is introduced and no trend leading up to the reform.

We introduce lead and lags to our reform variable and replace the aggregate G8 reform coefficient with a series of dummy variables for whether an individual is part of a school cohort before or after the reform was introduced.

$$Outcome_i = \delta_0 + \sum_y \delta_y SchoolEntry^y_{s,c} + \Psi_s + \Theta_t + \Gamma_c + \epsilon_{3,i},$$

where $SchoolEntry$ refers to a series of ten dummy variables that turn one if individual i started high school as part of school cohort c, before or after y years of the federal state's introduction of the reform. Thus, we have three lag cohort years when individuals are part of the first treated cohort: start high school the year the reform is introduced, one year later, two years later, or three years later.[30] The series of dummy variables further includes leads for up to six years before the reform was introduced. Thus, these dummies turn one if an individual started secondary school six years before the reform was introduced, five years before the reform was introduced, etc. up to the last G9 cohort, one year before G8 introduction. The last G9 cohort is the omitted category. $Outcome$ is either a dummy variable indicating gap year participation or trusting EU Parliament.[31] Otherwise, the regression equations introduced in section 3.4 remain unchanged: we include state, cohort, and survey fixed effects.

The results are presented in Figure 3.4 confirm the aggregated analyses from above. In Panel (a) we see mostly positive but small coefficients and no clear trend leading up to the reform. As coefficients are relative to the excluded year this implies that individuals in school cohorts before the reform may have been more likely to go on a sabbatical after graduation than the last untreated cohort. The last G9 year had more pressure continuing on with university as they were immediately followed by the first G8 cohorts. All coefficients for lead dummies are insignificant, however. [32] After the reform is introduced the first treated cohort shows a significant upwards-movement in

30. We limit our analysis to this time-frame as the number of observations decreases in later cohorts.

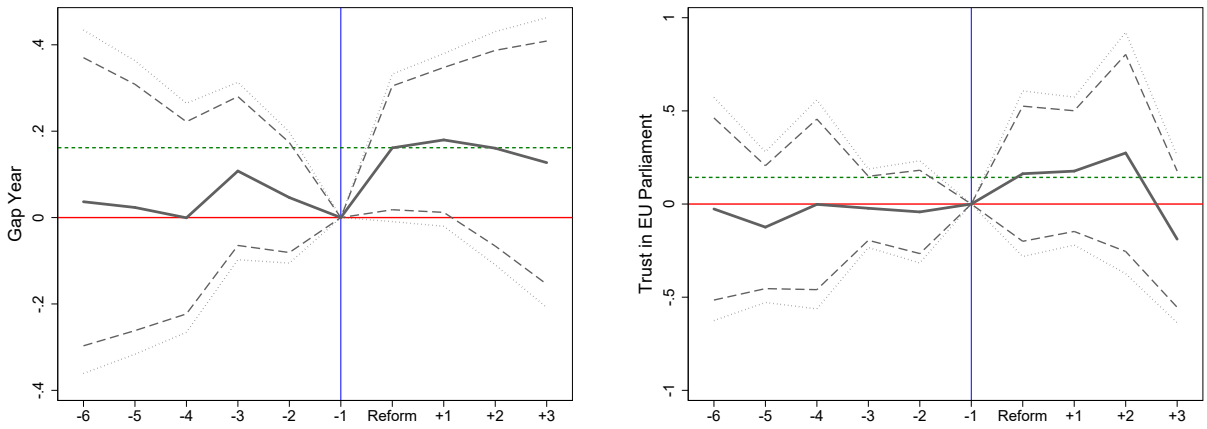
31. In the analysis for gap year participation we have at least well over 500 observations per year. The number of observations for trust in EU Parliament is lower as we focus the analysis on academic-track high school graduates that are clearly assigned to treatment and control group, but total number of observations is only about 300 here. Thus, we have between about 50 and 100 observations for each lead and lag year here, except for the last year after the reform, were the number of observations drops to 29. In order to increase the number of observations in this last cohort, we also test combining responses for individuals who started three and four years after the reform was introduced into the category starting three years post-reform introduction. This increases the number of observations in the last cohort to 53. The presented conclusion remains unchanged by this. Results are available upon request.

32. In order to alleviate concerns of mean reversion, we additionally test using the cohort three years before the reform is introduced as the omitted category and results remain unchanged. Results are available upon request.

the probability to engage in a gap year.

A similar picture is presented in Panel (b) for trust in EU Parliament. Leading up to the reform there is clearly no trend. In fact, coefficients are very close to zero. After the reform is introduced we see an upward trend indicating higher trust in treated cohorts. None-significance here is most likely due to the low number of observations per year. When we account for over-fitting as in the empirical analysis above, we see the same pattern, but stronger reform effects. The results are presented in Figure I5 in the appendix.

The event study, thus, first of all confirms our aggregate findings. We see that treated cohorts show different gap year and trust behaviour than untreated cohorts. Both are increased. We also see, however, that the measured effects seem largely driven by the first treated cohorts and declining afterwards. We do not see a lasting reform effect. The first treated cohorts were obviously the most aware of the "additional" time they had gained as they had the direct comparison with untreated cohorts. It is, therefore, not surprising that they show the largest reaction in our setting.



Gap year participation extensive

Trust in EU Parliament extensive

Figure 3.4: Event study and fixed-effects estimates of the G8 treatment on gap year and trust in EU Parliament

Notes: The graph shows regression coefficients and confidence intervals (90% as dashed line and 95% as dotted line) for estimating the development of gap year participation in (a) and trust in EU Parliament at least on level four on a scale from one (no trust) to seven (high trust) in (b) for the sample restricted to upper secondary school graduates. The last school cohort before the reform is the excluded category. The graph further shows the aggregated effect through difference-in-difference estimates in short dashed green lines in the given sample. Individuals were included when they started secondary school up to six years leading up to the reform, and up to three years after the reform. All regressions control for cohort fixed effects, survey year fixed effects, federal state fixed effects, individuals in the double graduate cohort, and partially treated individuals. Panel (b) additionally controls for individuals in the compulsory military or civic service. Standard errors are clustered at the federal state level.

3.6 The G8 Reform did not increase Trust in general

We analyse the effect of the reform on several further outcomes regarding political interest and trust in general and see no comparable effect of the reform. The effect of the reform on trust in EU institutions seems quite singular. The results are presented in Table 3.3.

We test the effect of the reform on general trust in political parties, on political interest, and on the probability to trust people in general. The G8 reform has no significant positive effect on any of these outcomes.

In fact, we can even see a significant negative effect of the G8 reform on political interest in Panel B.[33] The coefficient suggests a reduction in the probability to show interest in politics of about 14 percentage points when including all controls in column (5). Relative to the mean of 0.32 this suggests a large reduction by 43 percent, which is a sizeable effect.

The coefficient measuring the probability to trust political parties in Panel A (based on allbus data) is not affected by the G8 reform.[34]. The probability to trust people at least on a level of four out of seven also reveals a connection to the G8 reform that is very close to zero in Panel C.

We, furthermore, analyse whether the increase in gap year uptake is due to G8 cohorts postponing going abroad during their school time until after graduation. Regression results in Panel D do not show a significant difference between treated and untreated cohorts due to the G8 reform in the probability to spend time abroad while still at school.[35] We take this as evidence that the increase in sabbaticals after graduation is not due to a substitution effect.

As before, we additionally confirm our findings of this section in event study analyses. The results are presented in Figure I6. Panel (a) shows the development of trust in political parties for cohorts up to six years prior to the reform and three years after the reform. There is no clear trend either before or after the reform. The same holds for trust in people in general, in Panel (d). Panel (b) confirms the significant drop in political interest as a result of the reform. We see no trend leading up to the reform, but a sharp drop afterwards which is slow to recover in later treated cohorts. Panel (c) shows that going abroad during school time has been on a down-ward trend before the reform. This trend was momentarily stopped after the reform. There is no evidence that G8 students participate less in going abroad during their school time compared to previous cohorts.

Clearly, there is no overall increase in trust in general, or specifically in politics. The positive reform effect we observe for trust in European institutions is not reflected in any of our alternative trust measures.

33. This is evident when controlling for the share of our sample who are attending high school while answering this survey question. Our result is in line with previous research, for example, by Krekel (2017).

34. Table I10 shows that this also holds in our alternative model specifications when alternatively using a linear trend instead of cohort fixed effects and when including only significant cohort fixed effects.

35. A study by **weltweiser** on student exchange programs, for example, also shows an increase in total number of pupils going abroad during high school between 2003 and 2010, the main years of early reform impact.

Table 3.3: The effect of decreasing school duration on trust in political parties, political interest, trust in people, and going abroad during school time

	(1) State and cohort FE	(2) Survey FE	(3) Basic controls	(4) Background controls	(5) Income controls
Panel A. Trust political parties (mean: 0.53)					
Reform	0.028	0.032	0.053		
	(0.108)	(0.107)	(0.108)		
Observations	1,517	1,517	1,448		
WB p-value	0.838	0.813	0.695		
Panel B. Political interest (mean: 0.32)					
Reform	-0.139**	-0.141**	-0.148**	-0.139**	-0.137**
	(0.050)	(0.054)	(0.059)	(0.059)	(0.057)
Observations	2,356	2,356	2,356	2,356	2,356
WB p-value	0.008	0.064	0.083	0.097	0.101
Panel C. Trust people (mean: 0.51)					
Reform	-0.052	-0.024	-0.026	-0.014	-0.013
	(0.054)	(0.056)	(0.056)	(0.055)	(0.055)
Observations	1,340	1,340	1,340	1,340	1,340
WB p-value	0.345	0.706	0.676	0.814	0.831
Panel D. School time abroad (mean: 0.13)					
Reform	0.050	0.077	0.043	0.056	0.057
	(0.065)	(0.057)	(0.054)	(0.053)	(0.053)
Observations	2,204	2,204	2,204	2,204	2,204
WB p-value	0.638	0.364	0.606	0.482	0.481
Cohort FE	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓
Survey year FE		✓	✓	✓	✓
Sub-sample FE		✓	✓	✓	✓
Basic controls			✓	✓	✓
Family controls				✓	✓
Income controls					✓
Clustered SE	✓	✓	✓	✓	✓

Notes: The dependent variable is trust in political parties measured as a dummy variable that turns one if an individual shows trust on a level of 4 out of a scale up to 7 (high trust), and zero otherwise in Panel A. These analyses are based on allbus data. The mean value of the dependent variable is 0.525. All other panels are based on SOEP data. The dependent variable in Panel B is general political interest measured as a dummy variable that turns one if an individual is interested in politics at least on a level of two one a scale of one (no interest) to four (high interest). Panel C shows the results for the dummy variable that turns one if an individual trusts people at least on a level of four on a scale of one (no trust) to seven (high trust) in general, and Panel D shows the results for a dummy variable that turns one if a person spent some time abroad during high school, and zero otherwise. The mean value of the dependent variable is 0.275 in Panel B, 0.51 in Panel C, and 0.129 in Panel D. The main independent variable, reform, equals one if the individual was affected by the G8 reform in his or her federal state given school starting year. Control variables are as indicated. Individual level controls in Panels B to D additionally include being still at high school, as the shown questions are also answered by individuals who are still at school. Standard errors in parentheses are clustered at federal state level. The p-value when correcting standard errors according to wild-bootstrapping are provided at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

3.7 Further Potential Channels of Reform Impact on Trust in EU Institutions

Finally, the G8 reform naturally had a variety of effects. We believe that the positive link of the G8 reform and trust in EU institutions is connected to the increase in gap year participation of treated cohorts, as this sabbatical is, for example used to volunteer and volunteering abroad has largely gained in popularity over the past decade.

The following table provides a list of reform effects that have been shown to be relevant in previous literature. We show the outcome, the effect of the reform, the main source, and our assessment for whether the alternative outcomes are likely to affect trust in EU Institutions. The main observed outcomes for effects of the G8 reform focus on student performance, time availability, and well-being. Recently there has been a larger focus also on post-graduation reform effects.

Table 3.4: G8 reform effects shown in previous literature

Outcome	Effect of G8 reform	Source	Effect on trust
Student performance	Lower performance at graduation in mathematics	Büttner and Thomsen (2015)	∼
	Slightly reduced performance in English	Huebner et al. (2017)	↓
	Lower final grade point averages	Huebener and Marcus (2017)	∼
	Grade repetition increased (for boys)	Huebener and Marcus (2015), 2017	∼
	Graduation age reduced (stronger for girls)	Huebener and Marcus (2015)	↑
	No effect on graduation rates	Huebener and Marcus (2015), 2017	∼
	PISA test scores for high-performing students improved	Huebener, Kuger, and Marcus (2017) **andr19**	∼
	Reduced performance on intelligence tests	Bergold et al. (2017)	↓
Student activities	Less time for side-job	Meyer and Thomsen (2015)	∼
	Less time for voluntary activity	**Marcus2019**; Krekel (2017)	↓
	Reduced political interest	Krekel (2017)	↓
Student personality	More extroverted	Dahmann and Anger (2014)	↑
	Less emotionally stable	Dahmann and Anger (2014)	↓
Student well-being	More stressed	**Marcus2019**; Meyer and Thomsen (2015)	∼
Post graduation	Women less likely enrolled at university in year of graduation	Büttner and Thomsen (2015), Meyer, Thomsen, and Schneider (2018)	↓
	University enrolment lower up to three years after graduation	Marcus and Zambre (2019)	↓
	Increased voluntary service participation	Meyer, Thomsen, and Schneider (2018)	↑
	More likely to stay abroad after graduation	Meyer, Thomsen, and Schneider (2018)	↑
	Higher university drop out probability	Meyer and Thomsen (2015)	↓
	Higher probability to change majors	Marcus and Zambre (2019)	∼

Notes: Collection of main significant G8 reform effects established by previous research and evaluation on potential effects on trust in EU institutions. ∼ indicates neutral effect, ↓ symbolizes a potentially negative effect, ↑ symbolizes a potentially positive effect.

Much of the previous research has focused on student performance. Overall, performance seems slightly reduced and increased for high-performing students. Altogether, the reform was successful in reducing the age of school graduates but increased repetition rates indicating that on average the reduction is not an entire year. Importantly for our research, however, students still benefit from extra time after graduation in comparison to untreated cohorts. Students' grades in several subjects appear decreased.[36] Reduced English skills and intelligence may lead to less trust as it reduces understanding, but hardly more.

36. Better education in general is related to higher trust (Oreopoulos and Salvanes, 2011).

The G8 reform implies increased pressure on students and, hence, activities outside of school may be reduced (Miehling, 2020). Students' personalities further indicate that treated cohorts are more extroverted. We see this in favour of our proposed channel, as this may also increase the probability to seek inter-cultural exchanges.

Not going to university or dropping out may have a direct negative effect on trust (besides increasing gap year participation), as education is positively correlated with trust. Our proposed channel is, furthermore, indirectly supported by previous evidence that university enrolment is delayed and participation in voluntary services and going abroad are increased due to the reform.

The question naturally arises if volunteering itself may be connected to higher trust in EU institutions. It is possible, of course, that increased voluntary activity implies that individuals are more socially oriented and, thus, support the cooperative European project more. This may encourage higher gap year participation (and international voluntary service) in the first place. The research also suggest, however, that voluntary activity of students is reduced due to the reform by almost 20 percent (Miehling, 2020; Krekel, 2017). Thus, it does not appear to be the case that treated individuals are generally more civic minded.

3.8 Conclusion

Policy makers are asking how to increase trust in the European Union. Our research sheds light on an interesting unintended effect of a major German school reform which "gifted" students with an additional year after graduation. Compared to previous cohorts, students of the new G8 system received the same education but with one year less instructional time. We see that this reform increased trust in European institutions, in particular, European Parliament. This increase in trust in European institutions is not due to a general increase in trust through the reform. We see no reform effect on trust in political parties in general or in people. In fact, we show evidence that political interest seems to be decreased in treated relative to untreated cohorts. The reform effect on European institutions appears quite unique.

We argue that a main contributor to this increase is the higher probability to engage in a gap year by treated cohorts after graduation, which many use to go abroad, for example, through voluntary services. We show that participation in a gap year is robustly increased due to the reform. This increase is not due to a substitution effect of postponing going abroad during school to after graduation.

We, thus, show that increasing international cooperation through higher gap year uptake can lead to higher trust in EU institutions. Encouraging trust and participation in the European project is obviously relevant for the effective operation of the European Union. The European Union actively tries to increase international exchanges and cooperation towards this goal through a variety of projects. As recently as 2016, for example, the "European Solidarity Corps" was

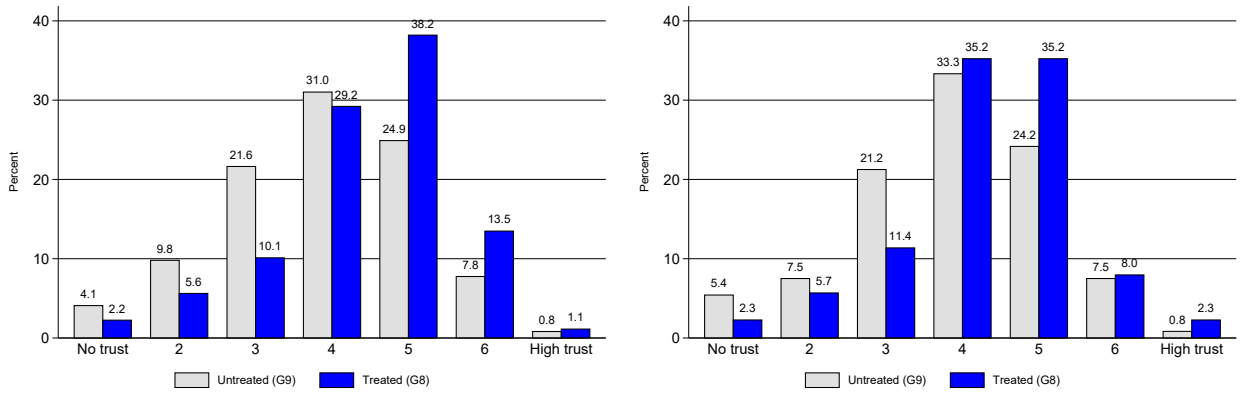
announced as a platform for organising and financing volunteering across the European Union.[37] For the years 2018 to 2020 a substantial budget of €341.5 million is planned for this endeavour (European Commission, 2017). The initiative is one of many that enable young graduates to gain international experience.

We show that the aggregate positive effect of the reform is driven by the early treated cohorts and the reform effect declines thereafter. Investments in international exchanges can be seen as fostering trust in EU institutions.

37. "The European Solidarity Corps will create opportunities for young people willing to make a meaningful contribution to society and help show solidarity [...] And those who work as volunteers are living European values each and every day" (European Commission, 2016a). The 27 Union Members further declared: "We need to improve the communication with each other [...]. We committed [...] to offer to our citizens in the upcoming months a vision of an attractive EU they can trust and support." (European Commission, 2016b)

3.9 *Appendices*

3.9.1 *Appendix Figures*



Trust in EU parliament

Trust in EU commission

Figure I1: Mean comparison for trust in EU parliament (a) and trust in EU commission (b) for treated and untreated cohorts measured at the intensive margin

Notes: The graph shows means of different levels of trust on a scale from 1 (no trust at all) to 7 (trust very much) on the x-axis for treated individuals (in blue) and untreated individuals (in grey) for the academic-track high school school sample balanced on observables.

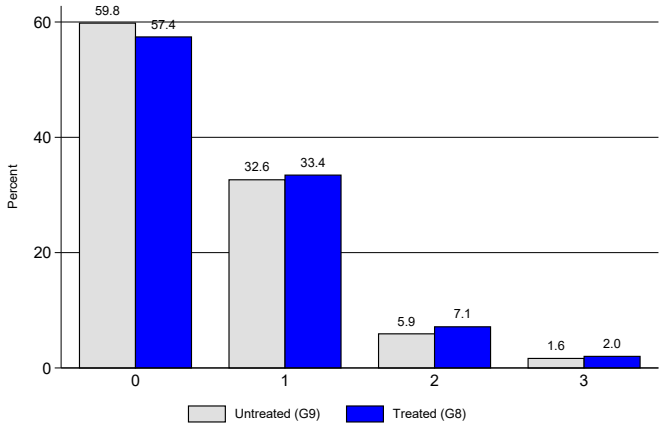


Figure I2: Mean comparison of years between high school graduation and next phase in life for treated (G8) and untreated (G9)

Notes: The graph shows mean years between finishing high school and beginning an apprenticeship, university, or employment on the x-axis for treated individuals (in blue) and untreated individuals (in grey) for the cross-section of first observations.

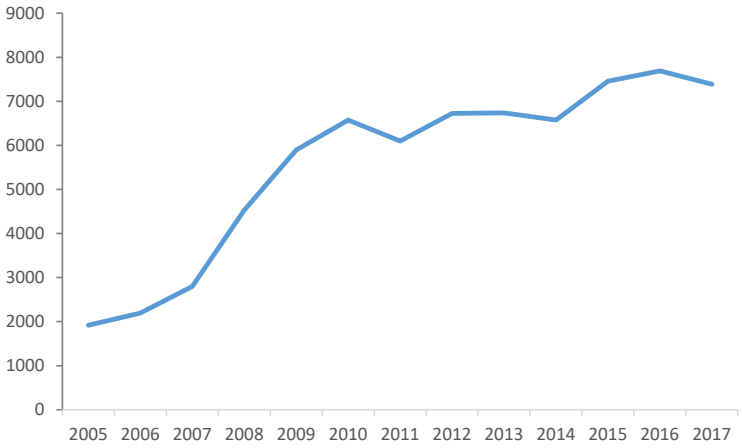


Figure I3: Development of total number of volunteers in international programs 2005 to 2017

Notes: The graph shows the total number of German volunteers in international programs from 2005 to 2017. Own graph based on information in AKLHUE (2018).

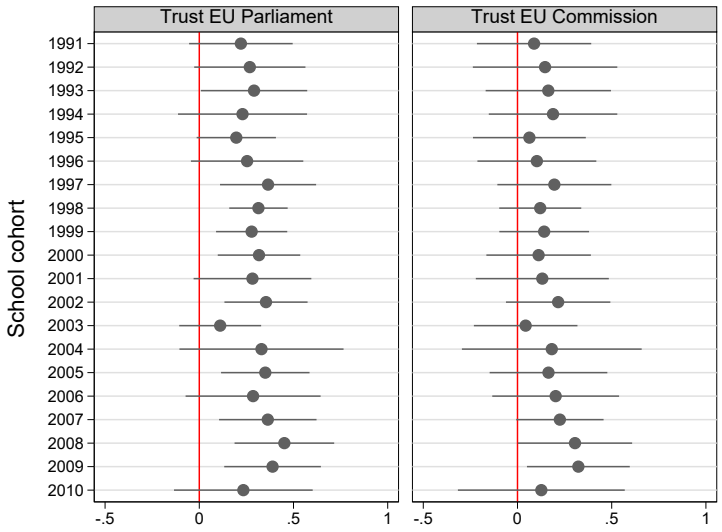
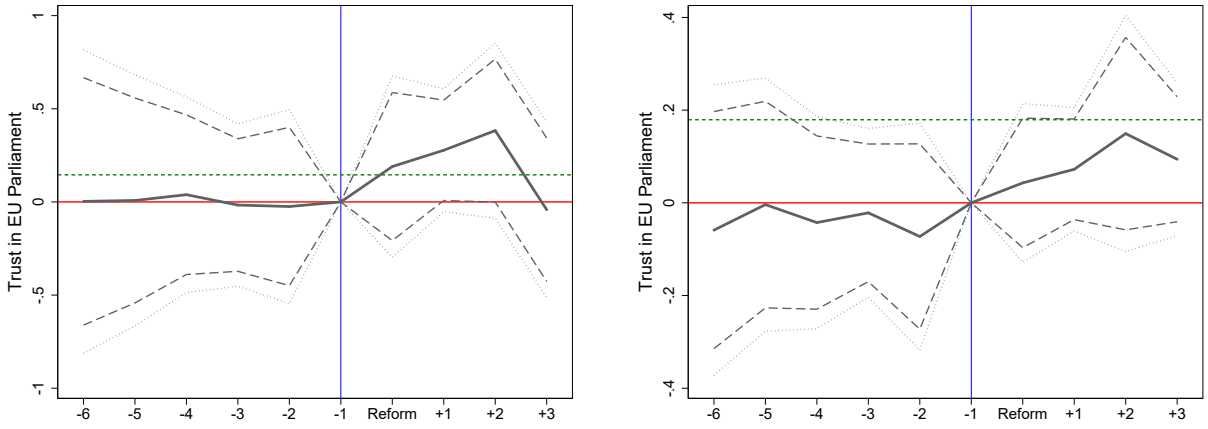


Figure I4: Cohort Fixed Effects in allbus

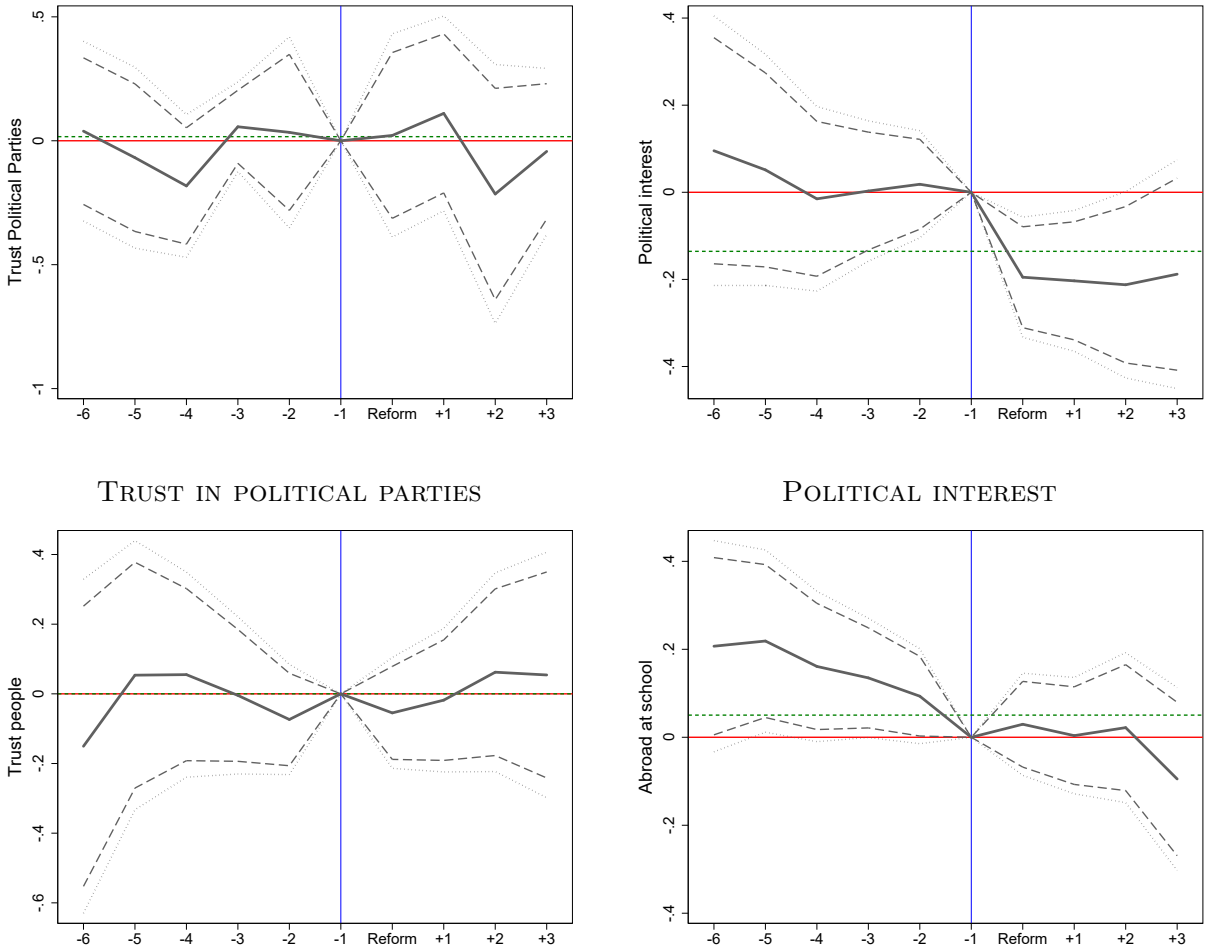
Notes: Data from ALLBUS. The graph shows regression coefficients and confidence intervals for school cohort fixed effects in a regression of trust in EU Parliament and trust in EU Commission on state and cohort fixed effects, as well as an indicator whether a cohort in a certain state was exposed to G8 and basic controls as described in the data section of this paper. The omitted category is the school cohorts of 1990. All regressions use robust standard errors clustered at the federal state level.



Trust in EU Parliament | Trust in EU Commission

Figure I5: Event study and fixed-effects estimates of the G8 treatment on the trust in EU Parliament and Commission including only significant cohort fixed effects

Notes: The graph shows regression coefficients and confidence intervals (90% as dashed line and 95% as dotted line) for estimating the development of trust in EU Parliament (a) and trust in EU Commission (b) at least on level four on a scale from one (no trust) to seven (high trust) in for the sample restricted to upper secondary school graduates and including only significant cohort fixed effects in order to avoid over-fitting. We include cohort year FE for: 1997, 1998, 1999, 2000, 2002, 2005, 2007, 2008, 2009 which are all correlated with the outcome at a level of 1 percent significance in (a). We include cohort year FE for: 2002 and 2009 which are correlated with the outcome at a level of 1 percent significance in (b). The last year before the reform is the excluded category. The graph further shows the aggregated effect through difference-in-difference estimates in short dashed green lines in the given sample. Individuals were included when they started secondary school up to 6 years prior to the reform, and up to 3 years after the reform. All regressions control for cohort fixed effects, survey year fixed effects, and federal state fixed effects. Standard errors are clustered at the federal state level.



Trust in political parties | Political interest

Trust people | Abroad during school time

Figure I6: Event study and fixed-effects estimates of the G8 treatment on trust in parties, political interest, trust in people, and going abroad during school time

Notes: The graph shows regression coefficients and confidence intervals (90% as dashed line and 95% as dotted line) for estimating the development of trust in political parties (a) at least on level four on a scale from one (no trust) to seven (high trust), political interest (b), trust in people in general (c), and the probability to go abroad at school in (b) for the sample restricted to academic-track students and graduates. The last school cohort before the reform is the excluded category. The graph further shows the aggregated effect through difference-in-difference estimates in short dashed green lines in the given sample. Individuals were included when they started secondary school up to six years leading up to the reform, and up to three years after the reform. All regressions control for cohort fixed effects, survey year fixed effects, federal state fixed effects, double graduate cohorts and partially treated individuals. Panels (b) to (d) additionally control for individuals still being at school. Standard errors are clustered at the federal state level.

3.9.2 *Appendix Tables*

Table I1: Distribution of treated and untreated individuals in allbus

Part of G8	No	Yes	Total
1 Schleswig - Holstein	6	1	7
2 Hamburg	5	1	6
3 Lower Saxony	23	14	37
4 Bremen	3	1	4
5 North Rhine-Westphalia	72	18	90
8 Baden-Wuerttemberg	45	10	55
9 Bavaria	38	19	57
10 Saarland	3	2	5
11 Berlin	3	6	9
12 Brandenburg	22	5	27
13 Mecklenburg-Pomerania	11	2	13
15 Saxony-Anhalt	14	10	24
Total	245	89	334

Notes: Number of observations per treatment (Yes) and control (No) group in the considered federal states for academic-track high school graduates based on availability of information in trust in European Parliament in allbus.

Table I2: Distribution of treated and untreated individuals in SOEP

Part of G8	No	Yes	Total
1 Schleswig - Holstein	75	13	88
2 Hamburg	19	18	37
3 Lower Saxony	118	133	251
4 Bremen	13	10	23
5 North Rhine-Westphalia	368	197	565
8 Baden-Wuerttemberg	215	130	345
9 Bavaria	140	143	283
10 Saarland	4	8	12
11 Berlin	69	26	95
12 Brandenburg	67	19	86
13 Mecklenburg-Pomerania	25	21	46
15 Saxony-Anhalt	50	49	99
Total	1,163	767	1,930

Notes: Number of observations per treatment (Yes) and control (No) group in the considered federal states in the cross section of first observations per individuals balanced on all controls.

Table I3: Descriptive statistics for balanced allbus data

	Mean	SD	Min	Max	Number
Reform	0.10	0.30	0.0	1.0	861
Trust EU Parliament	0.65	0.48	0.0	1.0	861
Trust EU Commission	0.66	0.47	0.0	1.0	841
Age	26.63	5.73	19.0	38.0	861
Age squared	741.85	321.11	361.0	1,444.0	861
Female	0.45	0.50	0.0	1.0	861
German nationality	0.96	0.19	0.0	1.0	861
Lives in East	0.24	0.43	0.0	1.0	861
German A-level	0.39	0.49	0.0	1.0	861
Double graduate cohort	0.09	0.29	0.0	1.0	861
Partially treated	0.04	0.20	0.0	1.0	861

Notes: Summary statistics based on the overall sample of individuals balanced on individual level controls for trust in EU parliament as outcome variable.

Table I4: Descriptive statistics for the cross section of first observations in the overall sample for gap year balanced on main observables

	Mean	SD	Min	Max	Number
Reform	0.40	0.49	0.0	1.0	1,930
Years between graduation and next step	0.52	0.69	0.0	3.0	1,930
Gap year	0.42	0.49	0.0	1.0	1,930
Age	19.86	1.25	17.0	26.0	1,930
Age squared	396.08	50.16	289.0	676.0	1,930
Female	0.54	0.50	0.0	1.0	1,930
Migration background	0.24	0.43	0.0	1.0	1,930
Lived in countryside	0.27	0.45	0.0	1.0	1,930
Lives in East	0.14	0.35	0.0	1.0	1,930
Monthly HH income (net)	3,515.30	1,620.19	500.0	7,500.0	1,670
Comp. military/civic year	0.21	0.41	0.0	1.0	1,930
Double graduate cohort	0.13	0.34	0.0	1.0	1,930
Partially treated	0.04	0.18	0.0	1.0	1,930
Parent: Blue collar	0.30	0.46	0.0	1.0	1,930
Parent: Married	0.81	0.39	0.0	1.0	1,930
Parent: Tertiary	0.41	0.49	0.0	1.0	1,930
Dummy for siblings	0.88	0.32	0.0	1.0	1,930

Notes: Summary statistics based on the cross-section of the first answer in the overall sample of individuals balanced on individual level and parental background controls.

Table I5: The effect of decreasing school duration on trust towards EU Parliament and Commission full regression results for individual controls

	Trust Parliament			Trust Commission		
	(1) All FE	(2) Linear Trend	(3) Sig. FE	(4) All FE	(5) Linear Trend	(6) Sig. FE
Reform	0.095	0.112	0.118*	0.057	0.091	0.125*
	(0.065)	(0.071)	(0.063)	(0.060)	(0.064)	(0.065)
Age	-0.038	-0.027	-0.028	0.063	0.056	0.013
	(0.053)	(0.042)	(0.020)	(0.045)	(0.038)	(0.018)
Age squared	0.001	0.000	0.000	-0.000	-0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Female	0.037	0.043	0.043	0.029	0.031	0.032
	(0.031)	(0.031)	(0.032)	(0.029)	(0.029)	(0.030)
German nationality	-0.017	-0.008	-0.006	-0.003	0.007	0.016
	(0.080)	(0.069)	(0.070)	(0.050)	(0.040)	(0.043)
Double graduate cohort	0.007	-0.040	-0.012	-0.019	-0.042	-0.036
	(0.061)	(0.055)	(0.063)	(0.059)	(0.055)	(0.057)
Partially treated	0.035	-0.030	-0.023	0.018	-0.035	-0.043
	(0.046)	(0.050)	(0.051)	(0.045)	(0.054)	(0.057)
Observations	864	864	864	847	847	847
WB p-value	0.162	0.133	0.077	0.366	0.200	0.112
Cohort FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Survey year FE	✓	✓	✓	✓	✓	✓
Basic controls	✓	✓	✓	✓	✓	✓
Clustered SE	✓	✓	✓	✓	✓	✓

Notes: In columns (1) to (3) the dependent variable is a dummy variable that turns one if an individual trusts EU Parliament at least on a level of 4 out of a scale up to 7 (high trust), and zero otherwise. Columns (4) to (6) show the results for the dependent variable measured as a dummy variable that turns one if an individual engages trusts EU Commission at least on a level of 4 out of a scale up to 7 (high trust), and zero otherwise. Columns (1) and (4) include all cohort fixed effects. Columns (2) and (5) include a linear cohort trend. Columns (3) and (6) only include significant cohort fixed effects in order to avoid over-fitting. We include cohort year FE for: 1997, 1998, 1999, 2000, 2002, 2005, 2007, 2008, 2009 which are all correlated with the outcome at a level of 1 percent significance in column (3). We include cohort year FE for: 1997, 1998, 1999, 2000, 2002, 2005, 2007, 2008, 2009 which are all correlated with the outcome at a level of 1 percent significance in column (6). The main independent variable, reform, equals one if the individual was affected by the G8 reform in his or her federal state given school starting year. Control variables are as indicated. Standard errors in parentheses are clustered at federal state level. The p-value when correcting standard errors according to wild-bootstrapping are provided at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table I6: The effect of decreasing school duration on the probability to trust EU institutions in probit, at the intensive margin, and adding further controls

	Trust Parliament					Trust Commission				
	(1) FE	(2) Survey FE	(3) Individual controls	(4) Additional controls	(5) Academic track	(6) FE	(7) Survey FE	(8) Individual controls	(9) Additional controls	(10) Academic track
Panel A1. Probit margins - all FE										
Reform	0.115	0.119	0.114	0.103	0.158	0.068	0.072	0.069	0.061	0.115
	(0.072)	(0.073)	(0.082)	(0.081)	(0.145)	(0.064)	(0.067)	(0.072)	(0.072)	(0.153)
Panel A2. Probit margins - linear trend										
Reform	0.125	0.131	0.134	0.133*	0.177	0.093	0.100	0.106	0.088	0.130
	(0.081)	(0.084)	(0.088)	(0.079)	(0.108)	(0.074)	(0.076)	(0.078)	(0.069)	(0.110)
Panel A3. Probit margins - significant FE										
Reform	0.138*	0.154**	0.136*	0.134*	0.156	0.110	0.131*	0.098	0.100	0.178
	(0.075)	(0.077)	(0.082)	(0.079)	(0.116)	(0.070)	(0.070)	(0.072)	(0.071)	(0.115)
Panel B1. Intensive - all FE										
Reform	0.042	0.058	0.078	0.064	0.106	0.003	0.020	0.060	0.049	0.027
	(0.147)	(0.158)	(0.163)	(0.161)	(0.379)	(0.142)	(0.156)	(0.174)	(0.175)	(0.348)
WB p-value	0.799	0.747	0.662	0.723	0.811	0.983	0.909	0.755	0.803	0.948
Panel B2. Intensive - linear trend										
Reform	0.159	0.183	0.201	0.194	0.318	0.092	0.117	0.151	0.154	0.091
	(0.165)	(0.175)	(0.180)	(0.184)	(0.291)	(0.141)	(0.150)	(0.156)	(0.165)	(0.245)
WB p-value	0.366	0.315	0.274	0.331	0.279	0.566	0.488	0.367	0.399	0.745
Panel B3. Intensive - significant FE										
Reform	0.227	0.290	0.226	0.205	0.435	0.378**	0.465***	0.304*	0.314**	0.375
	(0.184)	(0.194)	(0.186)	(0.179)	(0.356)	(0.147)	(0.147)	(0.141)	(0.141)	(0.215)
WB p-value	0.241	0.134	0.246	0.266	0.241	0.028	0.004	0.053	0.035	0.149
Observations	864	864	864	864	334	847	847	847	847	328
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Survey year FE		✓	✓	✓	✓		✓	✓	✓	✓
Basic controls			✓	✓				✓	✓	
Reform controls				✓					✓	
Clustered SE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The dependent variable in columns (1) to (5) is a dummy variable that turns one if an individual trusts EU Parliament at least on a level of four on a scale up to seven (high trust), and zero otherwise. The dependent variable in columns (6) to (10) is a dummy variable that turns one if an individual trusts the EU Commission at least on a level of four out of a scale up to seven (high trust), and zero otherwise. Columns (5) and (10) show regression results in the reduced sample limited to academic-track high school students. Panel A1 to A3 shows the regression results in a probit instead of a linear probability model. Panel B1 to B3 show the results measuring trust at the intensive instead of the extensive margin. In both sets of Panels, the first Panel includes all cohort fixed effects. The second Panel includes a linear school cohort trend. The third variable includes only significant cohort fixed effects. We include cohort year FE for: 1997, 1998, 1999, 2000, 2002, 2005, 2007, 2008, 2009 which are all correlated with the outcome at a level of 1 percent significance in columns (1) to (5). We include cohort year FE for: 2002 and 2009 which are correlated with the outcome at a level of 1 percent significance in columns (6) to (10). The main independent variable, reform, equals one if the individual was affected by the G8 reform in his or her federal state. Control variables are as indicated. The number of observations in all regressions is balanced on basic controls. Standard errors in parentheses are clustered at federal state level. The p-value when correcting standard errors according to wild-bootstrapping are provided at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table I7: The effect of decreasing school duration on the probability to take a gap year full results

	(1) FE	(2) Survey FE	(3) Basic controls	(4) Background controls	(5) Comp. service	(6) Linear trend
Reform	0.175***	0.161***	0.189**	0.194***	0.149**	0.166**
	(0.025)	(0.026)	(0.064)	(0.058)	(0.053)	(0.057)
Age			0.331**	0.313*	0.190	0.185
			(0.144)	(0.162)	(0.135)	(0.134)
Age squared			-0.008**	-0.008*	-0.005	-0.005
			(0.003)	(0.004)	(0.003)	(0.003)
Female			-0.151***	-0.157***	-0.020	-0.021
			(0.023)	(0.026)	(0.014)	(0.015)
Migration background			-0.038	-0.009	0.005	0.005
			(0.025)	(0.029)	(0.026)	(0.027)
Lives in East			0.059	0.049	-0.011	-0.006
			(0.087)	(0.104)	(0.065)	(0.066)
Double graduate cohort			-0.025	-0.014	0.018	0.013
			(0.036)	(0.036)	(0.030)	(0.030)
Partially treated			-0.032	-0.002	-0.053	-0.062
			(0.090)	(0.074)	(0.065)	(0.067)
Parent: Blue collar				-0.036	-0.033**	-0.034**
				(0.026)	(0.013)	(0.013)
Parent: Married				0.006	0.001	0.002
				(0.032)	(0.019)	(0.019)
Parent: Tertiary				0.001	-0.025	-0.025
				(0.028)	(0.016)	(0.017)
Dummy for siblings				0.047	0.036	0.035
				(0.033)	(0.024)	(0.024)
Comp. military/civic year					0.663***	0.661***
					(0.020)	(0.020)
Observations	2,196	2,196	2,196	1,930	1,930	1,930
WB p-value	0.000	0.000	0.044	0.031	0.039	0.026
Cohort FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Survey year FE		✓	✓	✓	✓	✓
Sub-sample FE		✓	✓	✓	✓	✓
Basic controls			✓	✓	✓	✓
Family controls				✓	✓	✓
Compulsory service					✓	✓
Linear trend						✓
Clustered SE	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is a dummy variable that turns one if an individual engages in a gap year as taking time off after graduation before going to university, starting an apprenticeship, or occupation. Results are balanced on basic controls. The mean value of the dependent variable in Panel A is 0.415. The main independent variable, reform, equals one if the individual was affected by the G8 reform in his or her federal state given school starting year. Control variables are as indicated. Standard errors in parentheses are clustered at federal state level. The p-value when correcting standard errors according to wild-bootstrapping are provided at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table I8: The effect of decreasing school duration on the probability to take a voluntary service full results

	(1) FE	(2) Survey FE	(3) Basic controls	(4) Background controls	(5) Comp. service	(6) Linear trend
Reform	0.156***	0.142***	0.138***	0.112***	0.056**	0.064**
	(0.023)	(0.021)	(0.043)	(0.036)	(0.023)	(0.025)
Age			0.128	0.083	-0.069	-0.070
			(0.108)	(0.107)	(0.087)	(0.090)
Age squared			-0.003	-0.001	0.002	0.002
			(0.003)	(0.002)	(0.002)	(0.002)
Female			-0.166***	-0.173***	-0.004	-0.004
			(0.015)	(0.019)	(0.005)	(0.005)
Migration background			-0.056***	-0.032	-0.015	-0.015
			(0.016)	(0.021)	(0.015)	(0.014)
Lives in East			0.085	0.082	0.007	0.003
			(0.060)	(0.077)	(0.019)	(0.019)
Double graduate cohort			-0.047*	-0.042*	-0.003	-0.002
			(0.024)	(0.021)	(0.010)	(0.010)
Partially treated			0.037	0.097	0.035	0.028
			(0.067)	(0.057)	(0.043)	(0.043)
Parent: Blue collar				-0.044	-0.039***	-0.040***
				(0.028)	(0.012)	(0.012)
Parent: Married				0.020	0.014	0.013
				(0.029)	(0.016)	(0.016)
Parent: Tertiary				0.047	0.015	0.015
				(0.032)	(0.012)	(0.012)
Dummy for siblings				0.023	0.009	0.009
				(0.030)	(0.010)	(0.010)
Comp. military/civic year					0.820***	0.821***
					(0.016)	(0.016)
Observations	2,196	2,196	2,196	1,930	1,930	1,930
WB p-value	0.000	0.000	0.029	0.035	0.021	0.039
Cohort FE	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Survey year FE		✓	✓	✓	✓	✓
Sub-sample FE		✓	✓	✓	✓	✓
Basic controls			✓	✓	✓	✓
Family controls				✓	✓	✓
Compulsory service					✓	✓
Linear trend						✓
Clustered SE	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is a dummy variable that turns one if an individual engages in in voluntary military or civic service after graduation. Results are balanced on basic controls. The mean value of the dependent variable in Panel A is 0.19. The main independent variable, reform, equals one if the individual was affected by the G8 reform in his or her federal state given school starting year. Control variables are as indicated. Standard errors in parentheses are clustered at federal state level. The p-value when correcting standard errors according to wild-bootstrapping are provided at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table I9: The effect of decreasing school duration on gap year participation

	(1) State and cohort FE	(2) Survey FE	(3) Basic controls	(4) Background controls	(5) Comp. service	(6) Occupation controls	(7) Reform controls	(8) Linear trend
Panel A. Extensive margin								
Reform	0.175***	0.161***	0.189**	0.194***	0.149**	0.149**	0.220***	0.199**
	(0.025)	(0.026)	(0.064)	(0.058)	(0.053)	(0.057)	(0.064)	(0.066)
Observations	2,196	2,196	2,196	1,930	1,930	1,669	1,930	2,196
WB p-value	0.000	0.000	0.042	0.033	0.038	0.024	0.036	0.036
Panel B. Fully balanced sample								
Reform	0.175***	0.164***	0.165**	0.160**	0.118**	0.149**	0.178***	0.181**
	(0.030)	(0.033)	(0.054)	(0.055)	(0.052)	(0.057)	(0.053)	(0.059)
WB p-value	0.000	0.001	0.018	0.022	0.031	0.024	0.014	0.015
Panel C. Margins of probit								
Reform	0.178***	0.167***	0.167***	0.161***	0.107**	0.150***	0.161***	0.182***
	(0.031)	(0.033)	(0.052)	(0.053)	(0.049)	(0.056)	(0.053)	(0.058)
Panel D. Intensive margin								
Reform	0.228***	0.223***	0.262**	0.256**	0.208**	0.245**	0.280**	0.286**
	(0.053)	(0.053)	(0.095)	(0.095)	(0.090)	(0.099)	(0.094)	(0.103)
Observations	1,669	1,669	1,669	1,669	1,669	1,669	1,669	1,669
WB p-value	0.002	0.002	0.021	0.024	0.026	0.027	0.018	0.020
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Survey year FE		✓	✓	✓	✓	✓	✓	✓
Sub-sample FE		✓	✓	✓	✓	✓	✓	✓
Basic controls			✓	✓	✓	✓	✓	✓
Family controls				✓	✓	✓	✓	✓
Compulsory service					✓			
Additional controls						✓		
School reform controls							✓	
Linear trend								✓
Clustered SE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is a dummy variable that turns one if an individual engages in a gap year as taking time off after graduation before going to university, starting an apprenticeship, or occupation. Panel A shows the regression results in the cross section of first observations balanced on basic controls. The mean value of the dependent variable in Panel A is 0.415. Panel B shows the same regression results in the cross section of observations balanced on all controls. The mean value of the dependent variable is 0.417. Panel C shows the fully balanced cross-sectional regression results as margins based on a Probit instead of LPM model. Panel D shows the fully balanced intensive margin analysis, using the number of years between graduation and the next phase in life as dependent variable. The mean of the dependent variable here is 0.5. The main independent variable, reform, equals one if the individual was affected by the G8 reform in his or her federal state given school starting year. Control variables are as indicated. Standard errors in parentheses are clustered at federal state level. The p-value when correcting standard errors according to wild-bootstrapping are provided at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

Table I10: The effect of decreasing school duration on the probability to trust in political parties in general

	(1) FE	(2) Survey FE	(3) Individual controls	(4) Academic track
Panel A. All cohort FE				
Reform	0.028	0.032	0.053	0.012
	(0.108)	(0.107)	(0.108)	(0.112)
WB p-value	0.838	0.813	0.695	0.931
Panel B. Linear trend				
Reform	0.024	0.034	0.029	-0.027
	(0.094)	(0.091)	(0.087)	(0.093)
WB p-value	0.834	0.760	0.792	0.806
Panel C. Significant FE				
Reform	0.092	0.095	0.050	0.081
	(0.084)	(0.083)	(0.086)	(0.051)
WB p-value	0.348	0.310	0.644	0.150
Observations	1,448	1,448	1,448	544
Cohort FE	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Survey year FE		✓	✓	✓
Basic controls			✓	
Clustered SE	✓	✓	✓	✓

Notes: The dependent variable is a dummy variable that turns one if an individual trusts political parties in general on a level of 4 out of a scale up to 7 (high trust), and zero otherwise. The mean value of the dependent variable is 0.547 in the small academic-track high school sample and 0.525 in the overall sample. Panel B shows the regression results for the probability to show trust when including a linear cohort trend. Panel C shows the results when only significant cohort fixed effects in order to avoid over-fitting in this small sample. We include cohort year FE for: 2008 and 2009 which affect the outcome at a level of 1 percent significance in Panel C. The main independent variable, reform, equals one if the individual was affected by the G8 reform in his or her federal state given school starting year. Control variables are as indicated. Individual level controls additionally include preference for right-wing parties. Standard errors in parentheses are clustered at federal state level. The p-value when correcting standard errors according to wild-bootstrapping are provided at the bottom of the table. *** p<0.01, ** p<0.05, * p<0.1.

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Beiblatt zur Dissertation: Erklärung zur Koautorschaft

Von Felix Hagemeister

Das Kapitel „Persistence and Activation of Right-Wing Political Ideology" meiner Dissertation ist in Zusammenarbeit mit Mark Westcott und Davide Cantoni entstanden.

Hierbei habe ich insbesondere zu der Aufbereitung der Daten und Erstellung der Tabellen und Graphiken beigetragen. Die Analysen mit dem Allbus Datensatz in Köln wurden größtenteils von mir vorbereitet und durchgeführt. Die Idee für das Papier lieferte ein Abschnitt in meiner Masterarbeit. Mark Westcott hat das historische Matching und die Textdatenanalyse beigesteuert. Davide Cantoni hat das Papier zum größten Teil aufgeschrieben und auf verschiedenen internationalen Konferenzen vorgestellt.

München, den 7.9.2020

Felix Hagemeister

Felix Hagemeister

Hiermit bestätige ich die Richtigkeit der Angaben.

Mark Westcott

Mark Westcott

Davide Cantoni

Beiblatt zur Dissertation: Erklärung zur Koautorschaft

Von Felix Hagemeister

Das Kapitel „The Impact of Reducing School Duration on Gap Year Participation and Trust in the EU" meiner Dissertation ist in Zusammenarbeit mit Daniela Miehling entstanden.

Hierbei habe ich die empirischen Analysen basierend auf der der German General Social Survey durchgeführt. Die Analysen basierend auf dem German Socio-Economic Panel wurde von Daniela Miehling durchgeführt. Das Papier wurde in Kooperation aufgeschrieben.

München, den 04.08.2020

Felix Hagemeister

Hiermit bestätige ich die Richtigkeit der Angaben.

Daniela Miehling

München, den 04.08.2020

Eidesstattliche Versicherung

Ich versichere hiermit eidesstattlich, dass ich die vorliegende Arbeit selbständig und ohne fremde Hilfe verfasst habe. Die aus fremden Quellen direkt oder indirekt übernommenen Gedanken sowie mir gegebene Anregungen sind als solche kenntlich gemacht. Die Arbeit wurde bisher keiner anderen Prüfungsbehörde vorgelegt und auch noch nicht veröffentlicht. Sofern ein Teil der Arbeit aus bereits veröffentlichten Papers besteht, habe ich dies ausdrücklich angegeben.

Datum: 13.09.2020
Unterschrift: **Felix Hagemeister**