No Ordinary Elections

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Essays in Empirical Political Economy

by

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Declarations

This thesis is submitted to the University of Warwick in accordance with the requirements of the degree of Doctor of Philosophy. I declare that any material contained in this thesis has not been submitted for a degree to any other university.

Christoph Koenig

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Abstract

Democracy substitutes election by the incompetent many for appointment by the corrupt few.

—George Bernard Shaw, *Man and Superman* (1903)

This thesis consists of three essays in the field of empirical political economy. The topics addressed in these essays are very diverse, as are the historico-institutional settings. What they share is the quantitative analysis of election results and – at least in the author's opinion – the inquiry of relevant research questions about political attitudes and institutions. This gives the dissertation its title "*No Ordinary Elections*".

Chapter one looks at the effect of war service on political attitudes. I analyse the impact of WWI veterans on changes in electoral support for Germany's anti-democratic right after 1918. In order to quantify the effect, I construct the first disaggregate estimates of German WWI veterans since official army records were destroyed. I combine this data with a new panel of voting results from 1881 to 1933. Differences-in-Differences estimates show that war participation had a strong positive effect on support for the right-wing at the expense of socialist parties. A one standard deviation increase in veteran inflow shifted voting patterns to the right by more than 2%. My findings are robust to a number of checks including an IV identification strategy based on draft exemption rules. The effect of veterans on voting is highly persistent and strongest in working class areas. Gains for the right-wing, however, are only observed after a time period of communist insurgencies. I argue that veterans' impact is consistent with the spread of a popular anti-communist conspiracy theory, the *stab-in-the-back* myth. I provide suggestive evidence that veterans must have picked this idea up during wartime, injected it into the working class and facilitated the rise of right-wing parties.

The second chapter documents evidence on how election fraud in authoritarian regimes can be used by lower-tier officials to cast signals about their loyalty or competence to the central government. I exploit a radical policy change in Russia in 2004 which allowed the president to replace governors of the country's 89 regions at his own will. As a result, federal elections after 2004 were organised by two types of governors: one was *handpicked* by the president, the other one *elected* before the law change and re-appointed. Even though both types faced removal in case of bad results, the need to signal loyalty was much lower for the first type.

In order to estimate the effect of handpicked governors on electoral fraud, I use a diff-in-diff framework over 7 federal elections between 2000 and 2012. For this time period, I construct a new indicator of suspicious votes for each region which correlates strongly with incidents of reported fraud. My baseline estimates show that in territories with a handpicked governor the share of suspicious votes decreased by more than 10% on average and dropped even further if the region's economy had done well over the past legislature. These findings suggest that officials have less need to use rigging as a signal once loyalty is assured unless faced circumstances raising doubts about their competence.

Finally, chapter three studies the Chernobyl nuclear disaster of April 1986 and voters' response in West Germany. The analysis uses a diff-in-diff estimation which exploits variation in proximity to the nearest nuclear power plant (NPP) across 301 counties. Proximity is used as proxy for the shock from perceived risk of a nuclear accident. Using data over a time period of almost 40 years and 11 elections, my results indicate that living closer to an NPP increased polarisation and benefited *anti*- but even more *pro*-nuclear parties. While gains of Greens are shown to be similar across social groups and therefore in line with home-voter effects, the increase of conservatives runs counter to most expectations. Heterogeneity analysis shows that the effect on conservatives is far stronger in areas with an above-median share of adolescents in their *impressionable years* and of higher average education. I argue that this can be explained by differences in assessing the economic losses from exiting nuclear power over the risk of a nuclear accident after the disaster. Using variation in the scheduling of state elections, I can also show that the pro-nuclear response was stronger in counties which did not vote in the immediate aftermath of Chernobyl leaving more time for a rational electoral choice.

Abbreviations

Chapter 1

War Service and the Flow of Political Ideas: Evidence from Germanys WWI veterans

1.1 Introduction

The economic analysis of war's detrimental effects goes back at least a century by now. Some of the earliest works in this field are probably Smith [1776] and Pigou [1919] who both study the long-run monetary costs of war for a country's society. Nowadays, the large inter-state wars analysed by Smith and Pigou have become rare events and most wars are taking place *within* countries. The main questions asked by economists, however are still the same and center around war's impact on physical and human capital. The effect of war on institutions and their well-established role for economic growth has so far been widely neglected as highlighted in a survey by Blattman and Miguel [2010]. One specific mechanism behind such a relationship could be the interaction of soldiers from different social backgrounds during army service.

This paper studies the effect of war service on political attitudes and spillovers to non-combatants. I investigate the case of Germany after the end of WWI in 1918 and the role of veterans in the spread of right-wing attitudes prior to the collapse of democracy in 1933.[1] Many historians have pointed out the general turn towards right-wing parties shortly after WWI [Bessel, 1990; Fritzsche, 1990; Mommsen, 1996]. The Nazis' take of power in 1933 was furthermore only made possible by a coalition with the conservative DNVP, the main right-wing party until the 1930s. Post-WWI Germany is an interesting historical setting to study the effect of war service on democratic institutions. Importantly, war was never fought on German soil and thus permits one to exclude many other effects of war such as the destruction of physical capital. The second notable feature is that national elections were taking place before and after the war. As a result, one can easily measure changes in support for any party between both periods.

I use two identification strategies to estimate the effect of veterans on voting behaviour: the first is a Differences-in-Differences approach which links changes in election results after WWI across areas to the population share of veterans. For this study, I constructed the first disaggregated estimates of German WWI veterans since official army records were destroyed in WWII. I combine this data with a unique panel of voting results from 1881 to 1933. The panel data allows me to track voting behaviour in 266 homogeneous geographic units (*precincts*) covering 2/3 of Weimar Germany over a period of more than 50 years and 17 parliamentary elections before and after the war. My baseline estimates show that after WWI, precincts with 1% more veterans increase in support for right-wing parties by slightly more than

[1] This implicitly addresses also war's impact on recovery since the Nazi party soon started a new war with devastating consequences for Germany's economy.

1%. These effects are significant in magnitude: a one standard deviation increase in veterans per capita, lifts right-wing votes by 2%, about 5% of the post-WWI average. The main losing party by far are socialist parties from the very left of the political spectrum. My results are robust to the inclusion of many determinants of war participation as well as precinct-linear time trends and other specifications.

My second identification strategy exploits exemptions for employees of war-related industries as an instrument for war participation. This addresses in particular remaining worries about endogeneity but also potential measurement error in the veteran estimate. Threats to the exclusion restriction may arise from natural support among industrial workers for democratic parties. I shut this channel down by controlling for the overall share of workers in manufacturing. The IV therefore relies on variation in the war-related employment share in areas with a *given* size of the working class. Another concern is that workers in war-related industries may have a natural tendency to vote for right-wing parties because of their revanchist policy aiming for another war. I was not able to find any evidence on such a mechanism during the Weimar Republic. Yet, if this actually was the case, my IV results should be biased towards zero. This is because I hypothesise a *negative* reduced-form relationship between war-related employment and right-wing votes. The IV results suggest that the effect of veteran inflow on right-wing votes might be twice as large as the OLS results. The weak first-stage relationship of 7.72 is supported by LIML estimates which deliver the same results.

To put these results in context, it is important to know that at the outbreak of WWI, Germany was a fast-growing federal monarchy with a large support for democracy. Even though national elections were not significant, votes for democratic parties reached about 77% in 1912. Upon facing defeat in 1918, army mutinies ended the war by turning Germany into a democracy. After the transition, coup attempts and economic crises quickly led to a dramatic fall in support for democracy. This development continued when the Great Depression hit Germany in 1930. Three years later, the anti-semitic Nazi party formed a coalition government with the conservative, anti-democratic German National People's Party (DNVP) which ended democracy and the Weimar Republic. Disaggregating the Diff-in-Diff estimates by years and parties, shows that veterans' impact resembles these patterns only to some extent. The positive impact of veterans on right-wing parties is mainly favouring the conservative DNVP rather than the Nazi party. Interestingly, the effect also only shows up in May 1924 – more than 5 years after WWI. Losses by the socialist parties are of similar magnitude but can be observed already in 1920. Both effects are highly persistent and last until the final Weimar election in 1933. Without any

prior assumption, the timing suggests that the effect on socialists was related to war service while the second one originated from the post-war period.

The paper investigates several channels through which war participation may have affected voting behaviour. Using data from two different sources on veteran benefit recipients in 1924 and 1929, I can rule out that impoverishment and exposure to higher levels of violence are driving the results. Rather, my findings are in line with the spread of an anti-communist conspiracy theory, the *stab-in-the-back* myth, which soldiers from the working class could have picked up during their army service. This conspiracy theory was spread by reactionary, right-wing circles and conveyed the message that democratic parties had betrayed the German population and were planning to surrender Germany to Bolshevism. I find that the effect was highest in precincts with a large share of the working class which narrows down the attention to this part of the society. Pre-WWI militarism, religion, and age composition of WWI eligible cohorts do not have any explanatory power.

Two events between 1920 and 1924 could explain the observed timing of the swing to the right: politicisation of veteran associations and the radicalisation of the German communists. In order to assess the first channel, I hand-collected, digitised, and geo-coded archival data on members of the three main political veteran associations in the Weimar Republic. Using this data, I can show that organised veterans do not explain my findings. Anti-communism, on the other hand, is supported by two different results. First, I demonstrate that veterans' effect on voting is mainly originating from areas with a comparatively high share of communist votes *after* their radicalisation as opposed to before when moderate members had not withdrawn yet. The second test uses the establishment of anti-communist paramilitary volunteer units (*Freikorps*) between 1918 and 1923. I digitised and geo-coded a comprehensive list of Freikorps paramilitaries which allows calculating each precinct's proximity to the nearest unit. My findings suggest that areas located closer to anti-communist volunteer units show a significant effect of veterans on voting.

Having narrowed down the attention to the spread of anti-communist messages among the working class, I continue by exploring the transmission *to* veterans and *from* them to others. In my analysis, I provide evidence that the effect was not only larger in working class areas but also restricted to those where exposure to ideologies different from socialism was particularly low. This is compatible with the idea that interaction of soldiers from different social backgrounds during wartime were particularly helpful at injecting new political ideas into a formerly secluded part of society – anti-communist in this case. In order to restrict the focus further to interaction among soldiers rather than soldiers and their superiors, I digitised

a military census from 1906 which gives me data on the recruiting patterns of the German officers corps. Using this data, I do not find any proof for a specific role of sergeants and other high-rank militaries. Finally, I explore settings under which veterans could have passed their thoughts on to others. I provide evidence which makes a transmission through the family network and to spouses appear unlikely. Rather, transmission seems to be conditional on high political competition in May 1924.

This paper contributes to the literature on the consequences of combat experience – positive and negative. Blattman [2009] shows that child combatants in Uganda displayed higher political activity in peacetime. In general, war experience seems to help overcoming collective action problems. This is also reflected in Campante and Yanagizawa-Drott [2015] who provide evidence that sons of former U.S. soldiers are more likely to volunteer in future wars. Jha and Wilkinson [2012] document that war experience of Indian soldiers in WWII facilitated thorough and peaceful ethnic cleansing during India's partition. My study addresses the open the question of how political *attitudes* rather than activity are influenced by war service. Grossman, Manekin, and Miodownik [2015] provide one of the few investigations into this crucial topic and highlight also the negative consequences of combat. Looking at Israeli recruits' exposure to violence during the Second Intifada, the authors find that battle lowers combatants' willingness for reconciliation and increases support for parties of the political right. I also argue for a negative impact of war participation but cannot find any specific effect coming from combat exposure. Rather, I argue that it is the interaction with different individuals which may drive changes in attitudes after military service.

My findings also speak to the study of war's long-run impacts. Researchers until now have mostly looked at physical and human capital destruction. Regarding political outcomes, Bellows and Miguel [2009] show that war violence led to higher political activity in affected households. Institutional aspects are only addressed by Acemoglu, Hassan, and Robinson [2011] who find that the systematic murder of middle-class Jews during the Holocaust in WWII had persistent negative effects on economic and political progress in Russian cities. I add to this literature in two ways. On the one hand, this is one of few investigations into the still open question of war's effects on political attitudes [Blattman and Miguel, 2010]. Secondly, my results suggest that, even if fought abroad, war can have negative consequences in the belligerent country through the transmission of detrimental political ideas. The persistent shift of votes from democratic to anti-democratic parties relates my work to the study of *democratic capital*, people's intrinsic valuation of democracy. Pio-

neered by Persson and Tabellini [2009], the determinants of democratic capital have also been evaluated empirically in a number of recent empirical studies [Giuliano and Nunn, 2013; Grosfeld and Zhuravskaya, 2015]. While most of these are looking at long-run institutional determinants, my study is – to the best of my knowledge – one of the first to document short-run changes in democratic capital. I also find evidence for the transmission of democratic values to others which has recently been conceptualised theoretically by Ticchi, Verdier, and Vindigni [2013].

Finally, this paper also contributes to the growing quantitative literature on the rise of the Nazi party in economic history and political economy. King et al. [2008] and Bromhead, Eichengreen, and O'Rourke [2013] relate the Great Depression to the rise of authoritarianism during the 1920s and 1930s in Germany and other countries. My paper focusses on the role of the post-war period and societal factors behind this development. Voigtländer and Voth [2012] demonstrate how anti-semitic attitudes from past centuries sparked up again after WWI and supported the rise of the Nazi party. Satyanath, Voigtländer, and Voth [2013], on the other hand, investigate the role of civic associations as tool for the Nazi party to infiltrate society. Crucially for my study, the authors also compare the effects of military vs non-military associations but do not find evidence for a particular role of veterans' associations in recruiting members for the Nazi party. I confirm these findings by showing that membership strengths of military associations cannot explain the positive effect of veterans on right-wing parties. To the best of my knowledge, my paper is the first one to empirically investigate the role of WWI veterans as well as the general success of anti-democratic, right-wing parties during the Weimar Republic. Rather than the Nazi party itself, I find that veterans were benefiting the like-minded but less prominent DNVP which played a crucial role in making Adolf Hitler Germany's Chancellor in 1933. I am also the first to empirically link the activity of German communists in the 1920s to the rise of right-wing support.

The rest of the paper proceeds as follows: Section 1.2 provides the reader with important historical background on Weimar democracy, the role of the former WWI soldiers therein, and the *stab-in-the-back* myth. In section 1.3 I give a detailed description of how the veteran estimate as well as the election panel dataset are constructed. Section 1.4 outlines the empirical strategies applied in this paper. Next, section 3.6 presents the main empirical results and and a number of robustness checks. Section 1.6 investigates the mechanisms underlying the baseline effect. Finally, section 1.7 concludes.

1.2 Historical background

1.2.1 World War I, the *stab-in-the-back* myth and the democratisation of Germany

Germany's path towards democracy reaches back as far as 1848 when a provisional national assembly was gathered in order to design a constitution for a still to be unified Germany. This democratic experiment was crushed soon afterwards leading to a period of restoration until Prussia's victory over France in 1871 resulted in the proclamation of the German Empire. It was a constitutional monarchy under Prussia's leadership that for the first time introduced a publicly elected parliament on German territory. Even though its competencies were limited at first, the *Reichstag*'s role increased as it had to approve the Empire's budget which became particularly important during WWI and the preceding arms build-up. Under Emperor Wilhelm II, the German Empire had started a period of unpredictable and provocative foreign policy which isolated it from most of its former European allies, most notably Russia and the United Kingdom. As a result, it took only a spark in form of the assassination of Archduke Franz Ferdinand of Austria to start the First World War on 28th of July 1914.

Even though the German Empire was quite successful at the beginning of the campaign, the progress at the western front came to a halt at the end of 1914 and was followed by four-year war of attrition with the highest death toll experienced until that point. By the end of September 1918, the situation of the German Army had deteriorated to such an extent that the Supreme Army Command (*Oberste Heeresleitung*) admitted defeat to the Emperor. A new grand government including members of the social democratic party was formed subsequently and few days later, US President Woodrow Wilson was officially asked for an armistice. When the Supreme Army Command rejected the conditions set by the Allied Forces in late October 1918, Chancellor von Baden sacked the leadership of the Supreme Army Command and issued political reforms which turned Germany into a parliamentary monarchy. The war, however, continued until the end of October when a mutiny by the German Navy in Kiel sparked a rebellion and the formation of socialist workers' and soldiers' councils. This rebellion quickly spread across the whole German Empire and eventually led to the proclamation of the German Republic and the abdication of Emperor Wilhelm II on the 9th of November [Büttner, 2008]. World War I officially ended two days later with the signing of an armistice.[2]

[2] One may question whether this transition can in fact be regarded as a democratisation. While Imperial Germany was not a full-blown autocracy, its constitution did not put any constraints

One of the key reasons for Weimar democracy's failure 15 years later was that the German Army was still fighting when the armistice was signed. This soon gave rise to the *stab-in-the-back myth*, a conspiracy theory according to which Germany had not lost World War I but was *stabbed in the back* by socialist and Jewish politicians and their supporters. The fact that social-democrats inherited power as the strongest parliamentary group and its ally, the left-liberal DDP, was traditionally popular among the Jewish population provided the material to fabricate a lie which many – especially militarists, monarchists as well as followers of the anti-semitic *Völkisch* movement – "*wanted to believe*" [Bessel, 1988]. The new state was therefore discredited from its very beginning as a project of unpatriotic cowards which made it very hard for large parts of the society to identify with the new democratic republic. This was further facilitated by a number of socialist rebellions which spread fear among the population of a violent *October Revolution*-style coup – allegedly tolerated or encouraged by the parties of the centre and left [Merz, 1995].

The stab-in-the-back myth can be regarded as *the* key mechanism for transmitting anti-democratic thought and eroding democratic capital during the Weimar Republic [Barth, 2003]. While being spread through various social groups such as paramilitaries or universities, there were only two main parties of the Weimar Republic who were more or less openly propagating its content and spreading right-wing anti-democratic sentiments. These were the extremely anti-semitic Nazi party NSDAP, including its predecessors, and the national-conservative *German National People's Party* DNVP [Mommsen, 1996].

1.2.2 WWI veterans' role during Weimar democracy

As highlighted by several authors, not all veterans were anti-democratic or right-wing. Those who became politically active and claimed to represent the *front generation*, however, were in great majority on the extreme right of the political spectrum [Diehl, 1975; Bessel, 1995]. Paramilitary units founded in the war's aftermath were officially disbanded in 1923, but continued to exist in non-military cover organisations or within right-wing veteran associations like the *Stahlhelm* [Büttner, 2008]. Membership in organisations could thus be an important mode of veterans interacting with the society and through which voting behaviour could be influenced.

on the executive and is therefore placed in the *grey zone* between democracy and dictatorship in 1914 on the *POLITY* scale (-10 to +10) with a value of 2 [Jaggers and Marshall, 2014]. This ambivalence also been noted by other scholars [Jesse, 2013]. After 1918, the power of the executive is bounded and the *POLITY* indicator jumps to a value of 6 where it remains until 1933. One can thus safely say that the revolution of 1918 resulted in a higher degree of democracy despite the unclear point of departure. Further arguments why 1918 can be regarded as a democratisation are reflected in the opinions voiced by its opponents in section1.A.1

As highlighted by Anheier [2003] for the Munich chapter of the Nazi party, anti-democratic activists tended to hold co-memberships in several paramilitary units, racist clubs, and political parties. While it is not possible to investigate each of them, my analysis of the channel focusses on ex-servicemen clubs since they had a clear political distinction and are one the few types of such organisations for which membership data has survived in archives.[3]

Joining one of many veteran associations was not only popular among anti-democratic veterans as membership numbers of the rightist *Stahlhelm* (500,000), the (social) democratic *Reichsbanner* (1,000,000) and the communist *Rotfront* (150,000) show [Ziemann, 1998]. Officially, those associations were not very political but rather meeting places for former soldiers of a specific social background to relive and commemorate their front experiences. Some members of these associations were also running as candidates in elections and veteran associations were very active in supporting the campaign of their favourite parties [Ziemann, 2013]. The *Stahlhelm* was initially loosely aligned with the conservative liberal, yet democratic, DVP and the authoritarian DNVP. However, the strong aversion against liberalism and socialism made it embrace soon also members of the anti-democratic paramilitary as well as anti-semitic extremists. In December 1924, the Stahlhelm started to openly support the nationalist parties in helping to organise rallies and organise marches [Klotzbücher, 1965]. Shortly afterwards, the *Stahlhelm* had turned into a political combat league and strongly involved in the increasing political violence between left and right [Berghahn, 1966]. The increasing political role of the veteran associations, was also recognized by politicians:

> "*Since 1924 a change has been noticeable. (...) The organizations no longer – or no longer exclusively – limit themselves to the field of soldierly activity, but increasingly are becoming engaged in the political struggle and are seeking to obtain political influence and political power (...).*"
> Albert Grzezinski (Prussian Minister of the Interior), quoted in Diehl [1975, p.173]

As the preceding sections have shown, veterans started to get politicised during the transition period especially where the new state was weak, threatened by uprisings and the need to rely on right-wing paramilitary was high. Anecdotal evidence has also highlighted the elevated role of soldiers within German society and the increasing political power of ex-servicemen clubs as potential mechanisms through

[3] Anheier [2003], for instance, provides an informative quick overview of the main types of organisations joined by radicalised veterans.

which soldiers could have influenced right-wing attitudes. The following section describes the construction and collection of the data used to analyse veterans' effect on political attitudes.

1.3 Data

1.3.1 Estimating Germany's World War I veterans

The data section starts by describing how I estimated the amount of German WWI veterans. Collecting data on German WWI soldiers is a challenging task since almost all primary material from the German Army Archive has been destroyed in an air raid during Second World War. This makes statistical data the only source to recover reliable information on WWI participation in the German Empire. The starting point is the exact number of soldiers having served in the German Imperial Army during 1914 and 1918 and not dying, *Veterans*. This number is transformed into a treatment intensity *Veterans per cap.*. The base population is taken from the 1910 census which gives the last reliable counts unaffected by WWI.[4] In order to save on notation, the term *per cap.* is omitted in the remainder of this section:

$$Veterans = Soldiers_{1913} + \sum_{t=1914}^{1918} SoldiersJoin_t - \sum_{t=1914}^{1918} SoldiersDead_t \tag{1.1}$$

Unfortunately, the components of this ideal measure are not readily available at a disaggregated level and veterans as such were also never subject of any statistical publication.[5] However, I will show that they can be estimated quite accurately with census data and are congruent with aggregate numbers from official sources.[6] The main data used in this study are two mid-war censuses conducted by the Office of War Nourishment's Economic Department (*Volkswirtschaftliche Abteilung des Kriegsernährungsamtes*) in December 1916 and 1917 as well as the first post-war census in October 1919.[7] The December 1917 census contains county level numbers on

[4] An alternative way of doing this, would be using the population from the first post-war census carried out in October 1919, about a year after the armistice of 11th November 1918. However, since the latter may be endogenous due to post-WWI migration, pre-war population seems a somewhat safer choice.

[5] An exception is the statistic of recipients of war-related benefits on 1929 which however covers less than 60,000 of the 11 million surviving German WWI participants and to which also widows and orphans were entitled. A per capita measure of benefit recipients is weakly negatively correlated with my measure of veterans at -0.08.

[6] See section 1.D.3 for details.

[7] According to Bessel [1993], a large amount of the 800,000 German prisoners of war had returned

the amount of military persons present at the time of the census, $SoldiersHome_{1917}$. The main problem is that soldiers serving in December 1917 were omitted from the census.[8] The way I resolve this issue is exploiting the fact that only men served in the army. This shows up as a notable gender gap in the mid-war censuses but crucially also in a considerably different population growth between women and men from 1917 to 1919.[9] Taking the gender-difference in population growth gives an estimate of men absent between 1917 and 1919, henceforth $MissingMen_{1917-1919}$:[10] This measure is, however, also driven by gender-specific differences in births, civilian deaths and migration. The first two can be estimated and are discussed in section 1.D.1 in the appendix. Differences in migration cannot be estimated and deducted and have to be accounted for by controlling for gender-specific migration 1910–1919.

Apart from gender-differences the sum of $SoldiersHome_{1917}$ and $MissingMen_{1917-1919}$ does also not account for fluctuations in and out of the army before and after December 1917. Dead soldiers are not problematic since neither those who die before or after 1917 are counted. However, a considerable number of soldiers had left the army before December 1917 for other reasons than death while others were still to join until the end of the war. Since age and desertion can be deemed negligible, those leaving the army alive should be roughly equivalent to the amount of severely wounded soldiers.[11] I thus also make use of a preliminary, unofficial version of the December 1916 census. This provides me with county-level data on war-disabled members of the German army. Disaggregated numbers on the 700,000 men who had left the army due to injury between 1916 and 1917 or were still to join the army in 1918 could not be retrieved [Statistisches Reichsamt, 1926]. These will be part of the composite measurement error discussed in section 1.4.1. Adding $WarDisabled_{1916}$ to the sum of $SoldiersHome_{1917}$ and a gender-corrected version of $MissingMen_{1917-1919}$

by late 1919.

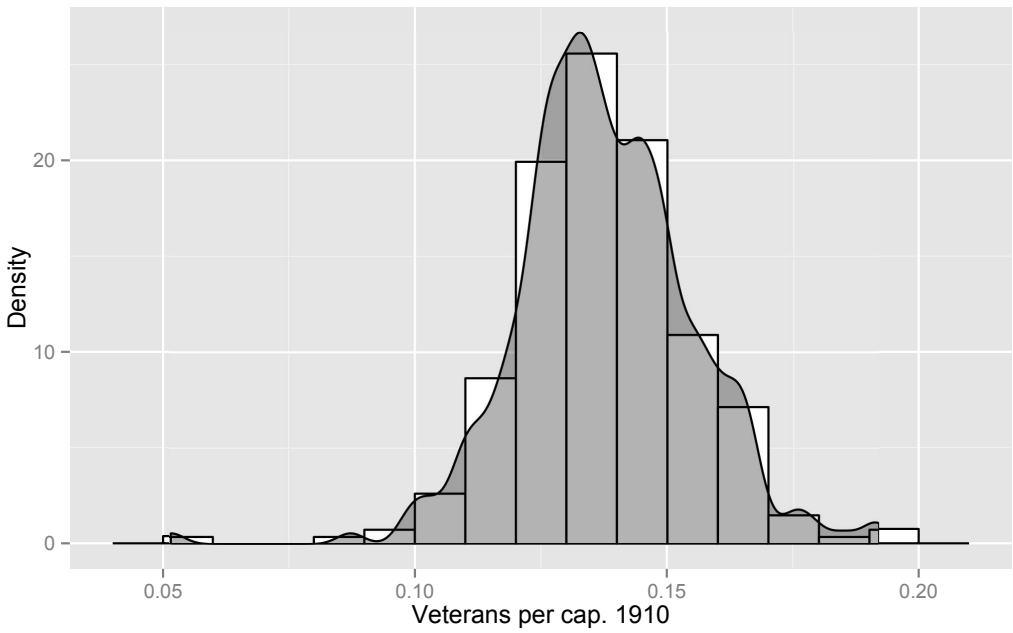
8 The equivalent census at the front did not collect data on soldiers' residence and could thus not be matched with the county level data. This practice was severely criticised among Germany's statisticians [Bayerisches Statistisches Landesamt, 1919].

9 Prisoners of war were also counted as local population in mid-war censuses and have been removed from $Male_{1917}$.

10 Further details on this calculation are provided in the appendix.

11 The study by Jahr [1998] estimates that no more than 50,000 out of almost 13 million German soldiers deserted. The rule of dropping out for leaving the conscripted age group between 17 and 45 was suspended in the German army during the First World War [Nash, 1977].

Figure 1.1: Density of veteran inflow per capita



completes the veteran estimate used in this study:

$$\begin{aligned}\widetilde{Veterans} = & SoldiersHome_{1917} + MissingMen_{1917-1919} \\ & - (Male\ Births_{1917-1919} - Female\ Births_{1917-1919}) \\ & + (Male\ CivilDeaths_{1917-1919} - Female\ Deaths_{1917-1919}) \\ & + WarDisabled_{1916}\end{aligned} \tag{1.2}$$

The density of the normalised estimate $\widetilde{Veterans}$ is depicted in figure 1.1. One can see that it is almost bell-shaped and ranging between 4 and 19% with a mean and median of 14% and 13.7%, respectively. Remaining issues about the veteran estimate such as measurement error and endogeneity will be discussed in further detail in section 1.4.1.

1.3.2 Panel data of Reichstag elections 1881−1933

In order to track changes in precincts' voting behaviour over time, I compiled a panel dataset covering 17 parliamentary elections held between 1881 and Hitler coming to power in 1933. The panel is based on two existing datasets on elections in Imperial and Weimar Germany from ICPSR [1991] and Falter and Hänisch [1990],

respectively. All voting data were initially taken from original publications by the German (Imperial) Statistical Office. A dataset comparing election results over almost 60 years, however, raises important issues regarding the units of analysis as well as the changes in Germany's party system.

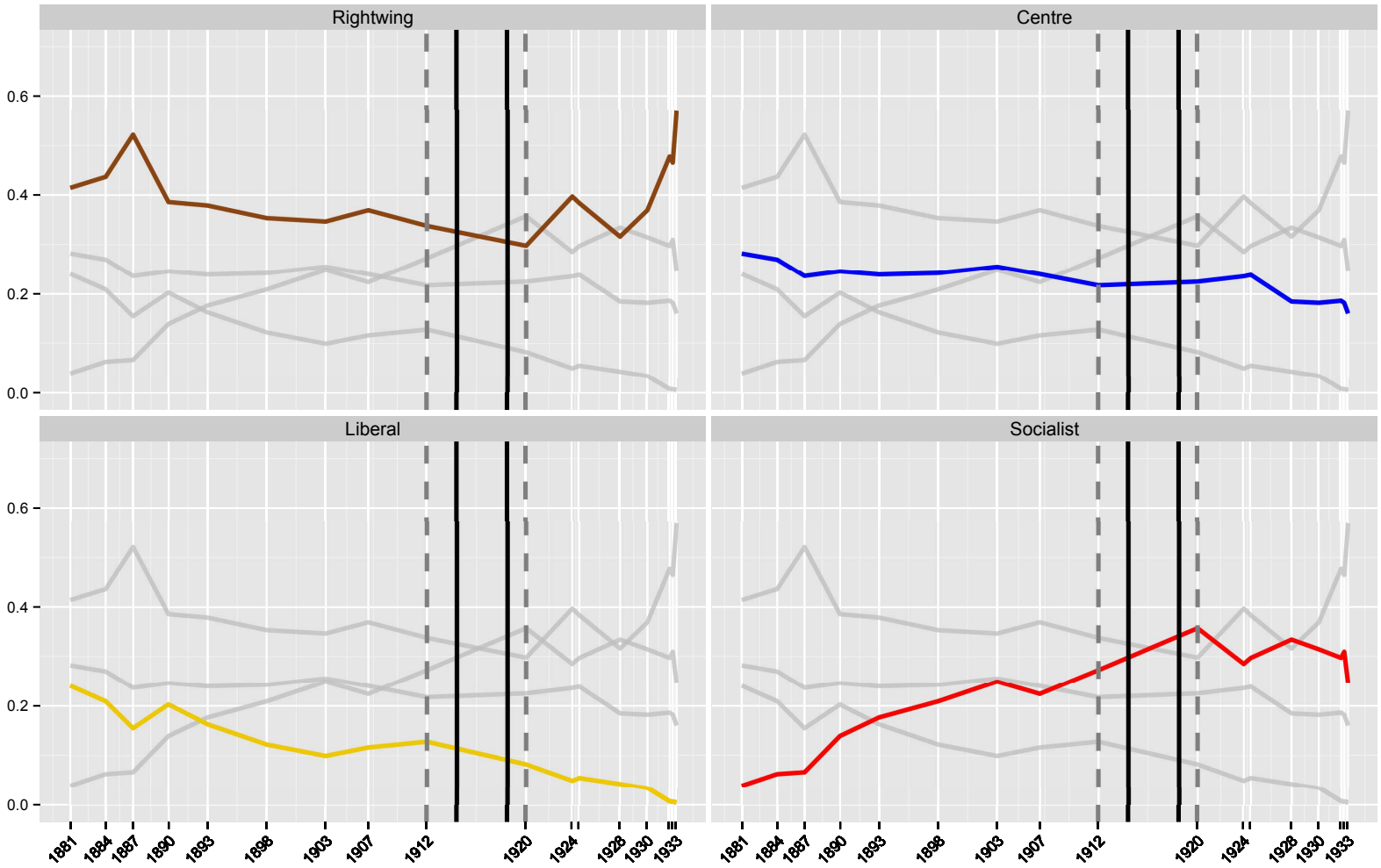
While the issue of area redistricting is discussed in section 1.3.3, the second major concern is the comparability of parties across time. A brief look into the history of the NSDAP illustrates this very well: during the German Empire there was no anti-semitic party of mass support but only various like-minded splinter parties such as the *Deutsch Reformpartei* (German Reform Party) or the *Wirtschaftliche Vereinigung* (Economic Union). The Nazi party was eventually founded under the name of the *Deutsche Arbeiterpartei* (DAP, German Workers' Party) in 1918 and changed its name into NSDAP (National-Socialist German Workers' Party) in 1920. After Hitler's first coup attempt in 1923 the party was banned and leading members of the NSDAP joined forces with the *Deutsch-Völkische Freiheitspartei* (German Völkisch Freedom Party, DVFP). From 1924 onwards, when it became re-allowed, the NSDAP became quickly the largest anti-semitic party.

This development is exemplary for almost any part of Germany's political spectrum and highlights the need for a more stable categorisation which accounts for the various name changes, mergers and splits in order to analyse long-term trends. I am relying on an established classification used in the study of historical German parties augmented by a separate category for anti-semitic groupings [Jesse, 2013]: 1) Anti-semitic; 2) (Protestant) Conservative; 3) Right-Liberals; 4) (Catholic) Centre; 5) Left-Liberals; 6) Socialist; 7) Agrarian/Particularist (Others).[12] The individual parties' votes are aggregated to their closest fit in the political spectrum and treated as *quasi parties* existing over the whole period of interest. The aggregates are then divided by the amount of total ballots cast in order to obtain vote shares. In the Weimar Republic, the main protagonists actively opposing democracy were *Antisemitic*, *Conservative*, and *Right-Liberals*.[13] My main outcome is the combined vote share of these three parties which I call *Right-wing*. The socialist party split

[12] An alternative classification is the one by Sperber [1997] who treats Anti-semitic and Conservative as a single conservative bloc and assigns the Centre party to the Agrarians/Particularists.

[13] Counting the right-liberals as right-wing is not straightforward. The main reasons are twofold: first, they were involved in many pre-election agreements with the conservatives during the German Empire which makes their vote shares difficult to separate. Second, despite participating in many governments the DVP opposed the draft of the post-war constitution and had many links to right-wing organisations such as the *Stahlhelm*. The DNVP also joined government during 1925 and 1927/1928, but research shows that this did not alter the party's general anti-democratic position. In fact, the party chairman Count Westarp was removed from office in 1928 because he refused to exclude a member urging for the acceptance of the Republic [Büttner, 2008; Gasteiger, 2014].

Figure 1.2: Long-term evolution of election results 1881-1933 (WWI start/end, solid lines; pre/post-WWI elections, dashed lines)



during WWI into social democrats and communists. I continue to use their sum as socialist votes after WWI to ensure comparability. In one specification I also add communist votes to the *Right-wing* which gives *Non-democratic* votes.

Figure 1.2 shows the aggregate voting data by political party over the used sample. What is remarkable is the stability of right-wing votes until the end of WWI and the sudden steep rise shortly after. While the centre parties remained very stable throughout this sixty years period, the results of liberals and social democrats show where the right-wing shares were coming from. Liberal votes had stabilised at about 20% until the war and then started to fall gradually to significantly below 10% in 1933. Socialist votes did not experience such a downturn but saw their clear upward trend during the German Empire come to a sudden halt during the 1920s and 1930s.

1.3.3 Construction of panel and control variables

This section describes the construction of the dataset and remaining control variables. The core of my dataset is a unique panel covering 17 parliamentary elections held between 1881 and Hitler coming to power in 1933. A panel over more than 60 years, however, requires stable units of analysis not only for the electoral results but also all other data to be merged to it. While most current work on Weimar Germany uses data at the city or county level, voting results during the German Empire were only published for each precinct. This unit was solely used for electoral purposes and only few exceptions followed political boundaries, e.g. for very small states and administrative districts. Each precinct typically consisted of a cluster of 2-4 counties with occasional but usually negligible overlaps. An attractive feature of those precincts is also that for political reasons they were never adjusted for the considerable population changes and remained stable from 1871 to 1912 [Jesse, 2013]. After World War I, Germany was divided into 35 new electoral precincts of larger but roughly equal size, but at the same time election data became published at much finer levels of aggregation such as counties and sometimes even larger municipalities. The smallest units of analysis with data available for pre- and post-WWI are thus the 397 former Imperial precincts. [14]

The counties they consisted of, however, were subject to frequent changes such as mergers, partial incorporations and splits. Hence, in a first step I coded all county reforms during the respective time period and constructed a set of *stable counties*. These are counties that existed at one point in time but where district reforms happened in such a way that numbers for the *stable county* can be reconstructed from adding up data of past or future sets of counties. If I was also able to

[14] For the remainder of this paper, *precincts* is referring to those of Imperial Germany.

re-construct the area of a whole precinct by adding up stable counties or if they coincided, this precinct was included in my dataset. In doing so, I was able to recover 266 out of the 397 Imperial precincts. About a quarter of the missing areas were from Alsace-Lorraine and Posen/West Prussia ceded to France and Poland after World War I. Another third is from densely populated – and often re-districted – agglomerations such as the *Ruhrgebiet* and very large cities with several precincts such as Berlin or Munich.

For this study I collected and digitised a number of additional data. One exception is the digitised Prussian version of the 1910 census which was taken from Galloway [2007]. To start with I digitised the German census of 1910 which provides me with data on religion and population size. I include population share of catholics and protestants and log(population) as controls variables. The 1910 census also provides me with the last pre-WWI data on cohort size by gender. Unfortunately, the latter are only reported in very large groups and does not allow to infer male cohorts born between 1869 to 1901 and thus eligible for WWI. I therefore use the far more detailed publication of the census results for Prussian population provided in [Galloway, 2007]. Together with data from the 1916 census this gives me the size of the male cohorts born 1869–1901 for about half my sample. In a two-step procedure I use this data to predict the cohort size 1869–1901 for the whole sample.[15] I also collected vital statistics for the German Empire for the time period 1910 to 1919 at the level of counties and administrative districts. I use this data to correct for gender-differences in $MissingMen_{1917-1919}$ and to calculate gender-specific migration between 1910–1919 and infant mortality in 1912.[16]

I also digitised the occupational census of 1882 which provides me with detailed county information on peoples' profession. From this I can calculate the share of the population working in manufacturing and in war-related industries. The latter forms my instrumental variable for war participation and is described in more detail in section 1.4.3. Finally, I control for turnout by dividing the amount of total votes by the size of the electorate. All control variables are at the cross-sectional level and included in the regression by interacting them with election fixed-effects. For the sake of brevity, I do not introduce them at this point but in the respective subsections of 1.6. I use a number of other variables in my mechanism analysis in section 1.6. Summary statistics for all variables relevant to the baseline specifications are reported in table 3.2.

[15] First, I run a simple regression of the actual cohort size 1869/1901 on the limited set of variables available from the all-German results. I then use these estimated coefficients to predict cohort size 1869/1901 for the rest of the sample

[16] I add perinatal births in 1912 to deaths within first-year of 1913 and divide by births in 1912.

Table 1.1: Descriptive statistics

	Obs	Mean	Std.Dev.	Min	Max
Veteran-related					
Veterans per cap.	266	0.14	0.02	0.05	0.19
Population 1910 in 1,000	266	152.50	106.90	24.16	937.38
Socio-economic					
% Protestants 1910	266	0.64	0.36	0.00	1.00
% Catholics 1910	266	0.34	0.36	0.00	1.00
% Infant mortality 1912	266	0.17	0.04	0.09	0.31
% Working in manufacturing 1882	266	0.13	0.05	0.04	0.31
% Working in war industries 1882	266	0.02	0.02	0.01	0.17
% WWI eligible men (born 1969-1901)	266	0.29	0.01	0.24	0.35
% ΔMale migration 1910-1919	266	-0.03	0.01	-0.09	0.02
Voting					
% Turnout	4,522	0.75	0.12	0.20	0.95
% Vote Anti-semitic	4,522	0.10	0.17	0.00	0.79
% Vote Conservative	4,522	0.19	0.21	0.00	0.99
% Vote Right-Liberal	4,522	0.11	0.16	0.00	0.97
% Vote Centre	4,522	0.23	0.29	0.00	1.00
% Vote Left-Liberal	4,522	0.10	0.15	0.00	0.91
% Vote Socialist	4,522	0.23	0.17	0.00	0.71
% Vote Communist (post-WWI)	2,128	0.08	0.06	0.00	0.33
% Vote Others	4,522	0.04	0.09	0.00	0.75

Notes: The unit of observation is one of the 266 precincts in the sample at election t. Variables provided at the cross-sectional level only are reported accordingly and used in the analysis by interacting them with either a post-WWI dummy or election fixed effects.

1.4 Identification strategy

1.4.1 Determinants of veteran inflow

In this subsection, I investigate the main drivers of war participation in Germany and I ensure that the treatment assignment is plausibly random conditional on observables. The main drivers of war participation across the German Empire were originating from the WWI conscription system. According to the law, all men aged 17 to 45 were liable to serve in the army and the share of male cohorts 1869 to 1901 is thus expected to be one of the main factors [Nash, 1977]. I include an estimate of this cohort relative to the 1910 population interacted with a post-WWI dummy into my set of control variables. Not all men in the relevant age groups, however, actually had to serve and a considerable amount was exempted. Being judged permanently unfit to fight was one main reason for exemption and at least at the beginning of the 20th century this decision was not entirely impartial but

Table 1.2: Determinants of veteran inflow

	Veterans p.c.				
	(1)	(2)	(3)	(4)	(5)
ΔMale migration$_{1910-1919}$	−0.035		0.080	0.091	0.144*
	(0.073)		(0.084)	(0.077)	(0.074)
1910 share of male cohorts 1869-1901		0.283**	0.318**	0.192	0.358***
		(0.113)	(0.134)	(0.135)	(0.120)
1882 share manufacturing		−0.034	−0.032	−0.085***	−0.001
		(0.023)	(0.023)	(0.024)	(0.028)
1882 share war-industries					−0.332***
					(0.117)
Infant mortality rate 1912				−0.002	−0.001
				(0.032)	(0.029)
Controls	N	N	N	Y	Y
Observations	266	266	266	266	266
R^2	0.001	0.046	0.050	0.185	0.271

Notes: Robust standard errors in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: Log(population) 1910; % Protestants 1910; % Catholics 1910; % New male voters post-WWI

allegedly also by factors such as parents' occupation and living location.[17] Data on conscription in the German Empire is only available before the war but does not corroborate such claims. The percentage of permanently unfit within the 1913 class, for example, ranged only between 4.3 and 5.9% across Germany's 25 military districts. During the war, these numbers were presumably even lower and more equal since a law from September 1915 allowed re-examining everyone judged unfit before. The intense *battle for manpower* [Feldman, 1966] during WWI in Germany makes it unlikely that political concerns of the commissions could have been a systematic driver of war participation.

The second part of exemptions was related to workers needed in war or war-related production. By the year 1918, about 1.3 million men – a sixth of the actual army size – was absorbed in such a way from the front to work in the factories and mines. War participation is thus expected to be significantly lower in areas employing a large share of men in the following industries: mining; iron and metal processing; production of iron, metal, and steel; construction of machines, tools, and vehicles; electrical, precision, and optical engineering.[18] This share is a confounder since it is highly correlated with the size of the working class which was at the

[17] The reason behind this was the army's general suspicion against the working class of supporting social democracy and being politically illoyal. See Brentano and Kuczynski [1900] and May [1917] for further discussions of this topic.

[18] This classification is taken from Kocka [1978].

same time also the main stronghold of the social democrats. Male employment in war industries is therefore expected to negatively affect both war participation and right-wing votes. The IV strategy presented in section 1.4.3 exploits the fact that war industries should have a direct effect on political attitudes *only* through the size of the working class. My main specifications rules this channel out by including interactions of time dummies with the employment share in manufacturing 1882. The final determinant of the veteran estimate is mismeasurement as discussed in section 1.3.1. I control for parts of this by controlling for $\Delta Male\ migration_{1910-1919}$.

Table 1.2 shows how the main drivers of war participation are related with my veteran estimate. In order to casually investigate what the remaining variation may be driven by, I construct the residuals from the specification in column (4) and plot their spatial distribution by quartile in figure 1.3. Unexplained variation seems to be slightly higher in north and south-west Germany. Reassuringly, figure 1.4 shows that the correlation of the residual with pre-WWI right-wing vote shares as of 1912 is only very weak and negative.

1.4.2 Differences-in-Differences

The panel structure of the data allows using unit and time fixed effects which identifies off the within-precinct variation after accounting for time-specific trends. In doing so, I can account for election-specific voting patterns due to candidates' abilities, for instance, and any time-constant omitted variable. Also confounders related to historical heritage are taken care of, given that their effect is constant over time. My first identification strategy exploits these features and uses a difference-in-differences methodology to investigate the level effect of veteran inflow across German precincts on right-wing voting. The estimated equation reads as follows:

$$y_{it} = \alpha + \gamma_i + \lambda_t + \beta_t(veterans_i \times postWWI_t) + \boldsymbol{\mu}\, \boldsymbol{X}_{it} + \epsilon_{it} \tag{1.3}$$

In the baseline model, I regress vote shares y_{it} one the election and precinct fixed effects γ_i and λ_t as well as a set of control variables $\boldsymbol{X}_{it}$ which is identical to the full set of variables in column 4 of table 1.2. The main variable of interest is the interaction of the one-time treatment intensity $veterans_i$ with a dummy variable taking on value 1 for each election after WWI (starting with the one in June 1920) and 0 otherwise. The estimated effect should thus be interpreted as an average shift in voting patterns across all elections after the end of the war proportionate to the estimated population share of veterans. Whether this effect is causal depends on two assumptions: the first is that areas of high and low treatment intensity follow

Figure 1.3: Residuals from table 1.2, column 4 across Imperial Germany's precincts, post-WWI borders in green

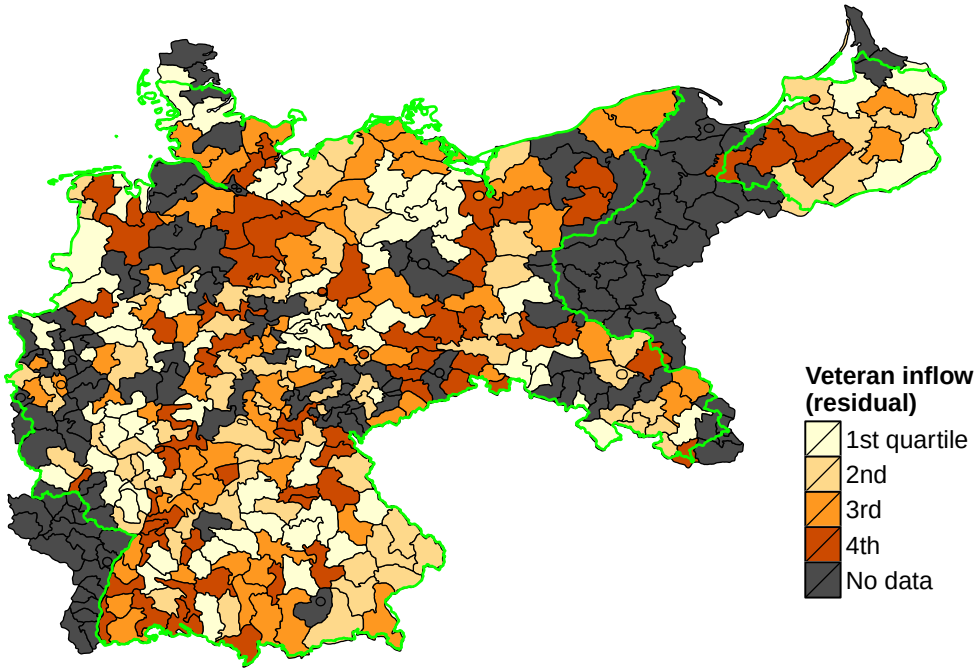
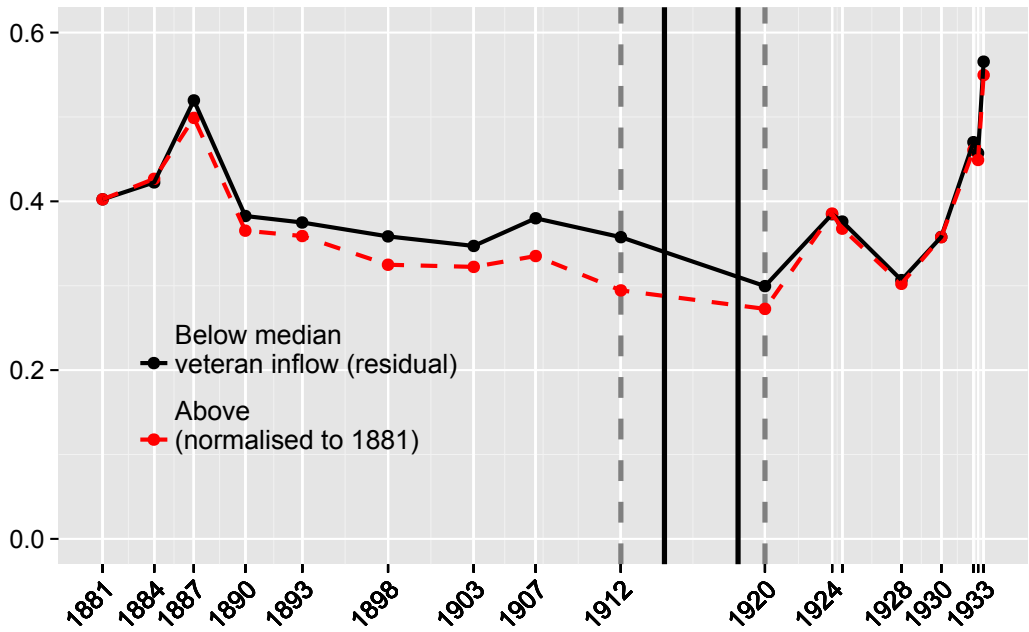


Figure 1.4: Correlation of unexplained veteran variation and right-wing votes in 1912

Figure 1.5: Average rightwing vote share before/after WWI depending on veteran inflow residual (median)



similar voting patterns before WWI and that the observed change is not part of a trend starting before WWI.

I tackle this concern in several ways: the most simple one is presented in figure 1.5 which plots the average right-wing vote share over time for precincts with above and below median values of the residual plotted in figure 1.3. As can be seen, the two lines are diverging before the war with less-treated districts exceeding the other ones by about 4%. After WWI, this trend reverses and by May 1924 the average votes in both groups are almost equal. The second test uses a non-linear version of the effect by interacting the treatment $veterans_i$ with 20 election fixed effects leaving out 1912 as the reference election. While being more demanding on the data, this allows exploring anticipating behaviour and explicitly test the common trends assumption. This would not be satisfied if precincts with higher treatment intensity started to show increasingly higher voting results for anti-democratic parties already *before* the war. A third alternative is the inclusion of area-specific election fixed effects and precinct-specific time trends. Both tests are presented as a robustness check in section 3.6.2.

The second necessary assumption is the absence of confounding events correlated with both the arrival of veterans and support for the extreme right. The vector

of control variables X_{it} features several factors deemed to fulfil these criteria. Apart from the determinants of (estimated) veteran inflow, I also include further data. The first of these is the natural log of the population and serves as a proxy for the precinct's size. Unlike the percentage of new female voters, the amount of new *male* voters is correlated with the treatment and thus included in the regression. New male voters are those born between 1896 and 1900 who would have not been allowed to vote in 1920 under the old law. I proxy this with the cohorts from 1895 to 1900 taken from the censuses 1910 and 1916 and create a new variable $NewMaleVoters_{it}$ which is zero before WWI and afterwards equal to the size of the newly enfranchised cohorts divided by the 1910 total population.

Furthermore, I include socio-economic characteristics of each precinct. In addition to the size of the working class and infant mortality, I also control for the religious composition of precincts using the shares of Protestants and Catholics in 1910. Including religion into the specification is necessary since Protestants were more supportive of the German Empire especially after the extremely polarising *Kulturkampf* secularisation period and it is possible that war volunteering was also higher among them. If the (predominantly Protestant) conservative parties had started to actively fight democratisation only after the war, this would be a source of bias. In order to include the time-invariant control variables into the fixed effect regression, each of them is interacted with a set of election dummies. Finally, the standard errors of the regression are clustered at the precinct level to account for correlation of unobservable characteristics over time.

1.4.3 Instrumenting veteran inflow

Even though the diff-in-diff specification already controls for a range of unobserveables, one should refrain from interpreting these estimates in a causal way. Many important factors are likely to have been omitted from the specification which could bias the estimated effect of veteran inflow. For example, economic activity and the treatment may be mis-measured in a systematic way and historical treats may change their effect over time and thus would not be captured by the precinct fixed effects. In order to tackle these concerns, I use a driver of war participation which is uncorrelated with unobserved determinants of right-wing voting: draft exemptions for male workers in war industries *conditional* on the size of the working class.[19] The first and second stage regressions of the corresponding 2SLS estimation are stated

[19] Similarly, Acemoglu, Autor, and Lyle [2004] are using discrimination in the conscription process as an instrument for war participation to estimate the effect of female labour supply during WWII on wages in the United States.

below:

$$veterans_i \times postWWI_t = \kappa + \theta_i + \psi_t + \delta_t(WarIndustry_i \times postWWI_t) + \boldsymbol{\eta}\boldsymbol{X}_{it} + \upsilon_{it} \quad (1.4)$$

$$y_{it} = \alpha + \gamma_i + \lambda_t + \beta_t(\widehat{veterans_i \times postWWI_t}) + \boldsymbol{\mu}\,\boldsymbol{X}_{it} + \epsilon_{it} \quad (1.5)$$

The identification strategy rests on the *conditional* exogeneity assumption that employment in war-industries 1882 affects right-wing votes in a precinct with a *given* size of working class only through its (negative) effect on veteran inflow. Another way of stating the exclusion restriction is that there is nothing else that makes areas with a high share of workers in war industries, given those of total manufacturing, more pro-democratic than its effect on war participation. One concern with the instrumental variable may be that workers producing goods needed by the army have an economic interest in continuing warfare which then translates into support for specific parties. In this case, however, areas producing weapons would be especially inclined *towards* belligerent parties which is the opposite of the reduced form relationship hypothesised above. Throughout Germany's history from 1881 to 1933 right-wing parties were – at least comparatively – the more fervent supporters of military action. While the self-interest of weapon-producers in military action cannot be entirely ruled out, it would make it only harder to find a significant effect of war-related employment on votes for the extreme right. The next section discusses the empirical results of these two identification strategies.

1.5 The effect of veterans on right-wing voting

1.5.1 Difference-in-Differences results

The results from the differences-in-differences in equation 3.1 for right-wing votes and its components are reported in table 3.3. The plain linear regression in column 1 yields already a strongly significant coefficient indicating that a one percentage higher veteran inflow after WWI is associated with an increase in right-wing votes of 0.17 percent. While the inclusion of precinct effects does not alter the results, specification (3) and (4) show that the effect was strongly distorted by the exclusion of election fixed effects and the control variables. According to the baseline specification in column (4), a unit percent increase of veteran inflow yields an almost double increase in votes for the extreme right of about 1.1%. Two out of the three constituting *quasi parties* are gaining from veteran inflow after WWI but estimates

are clearly driven by the conservatives rather than the anti-semitic parties. The veteran effect is thus independent of the success of the Nazi party and rather directed towards general authoritarianism and conservatism than anti-semitism. Taking into account the treatment variable's distribution, a 2% increase in veterans per capita – the equivalent a one standard deviation increase – translates into an increase of 2%. This is about 5% of the mean vote share of right-wing parties after WWI.

The positive link between the share of veterans and success of right-wing parties raises the question where those votes came from and which part of the politcal spectrum lost due to veteran inflow. Another crucial question is whether the effect of veteran inflow is benefiting anti-democratic parties of *any* political direction or whether it is restricted to the right-wing only. Table 3.4 sheds light on these questions and reports the estimates of the baseline specification for the combined votes of right-wing and communists (*Anti-democratic*) and all other *quasi parties*. Column (2) shows that adding communist to right-wing votes leaves the coefficient significant but decreases its size by about a quarter. The effect of veterans must therefore be negative on communist votes and benefits only the anti-democratic parties of the political right. Specifications (3) to (7) show that the right-wing was gaining from war participation at the expense of the socialists and other parties. The only exceptions were the Catholic Centre party is gaining insignificantly and the progressive left liberals have an effect near zero.[20] Reasons for this could be that there was far higher cohesion within those parties since they were particularly popular among adherents of particular faiths (Catholics for the Centre, Jews for the left-liberal DDP). The socialists experienced the most severe losses but also particularistic parties saw their votes decrease depending on the amount of veterans per capita. Even though table 3.1 showed that only one quasi-party gained, the fact that the losing counterparts are only two parties points in the direction that the turn towards the right as a response to war participation was restricted to specific parts of Weimar Germany's society.

1.5.2 Robustness of the baseline estimates

In the following section I investigate the reliability of the baseline results. Even though figure 1.5 does not show any divergence in voting patterns which would benefit my findings, it does not provide a rigorous check for the validity of the common trends assumption. I use two ways of testing the robustness of the results:

[20] It may seem at first that veterans are even significantly benefiting the Centre party. In table 1.8 I show that this effect originates from the 1907 election and does not seem to be related to WWI.

Table 1.3: Differences-in-Differences estimates (Baseline results)

	Rightwing				Anti-semitic	Conser-vative	Right-Liberal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Veterans p.c.	0.174***	0.117*	0.256	1.080**	0.109	1.685***	−0.714
	(0.065)	(0.065)	(0.511)	(0.467)	(0.212)	(0.483)	(0.500)
Precinct FE	N	Y	Y	Y	Y	Y	Y
Election FE	N	N	Y	Y	Y	Y	Y
Controls	N	N	N	Y	Y	Y	Y
Precincts	266	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.003	0.640	0.740	0.784	0.872	0.735	0.524

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

inclusion of region-specific time effects as well as precinct-specific linear-trends and allowing for a non-linear treatment effect. Table 3.5 reports the results of column (4) in table 3.3 for different combinations of province and district-specific election fixed effects as well as precinct-specific linear time trends. The first two absorb the effect of any unobservable varying at the province or district level independent of its functional form. Precinct-specific trends, on the other hand, prevent the treatment variable from picking up any linear change in voting behaviour over time in a given precinct. Reassuringly, the coefficient on the treatment variable does not change strongly and remains significant many specifications. Allowing for flexible area fixed-effects in column (2) and (3) slightly increases the treatment effect. The inclusion of precinct-linear trends in column (4) saturates the model and inflates the standard error but has not impact on the point estimate. Adding area-specific election fixed-effects only slightly decrease the treatment effect in the final specification (6). The fact that the inclusion of various linear- and non-linear trends does not wipe out the veteran effect lends further support to the common trends assumption.

The weakness of the precinct-specific trends is that they can only account for a *linear* pre-treatment patterns in each precinct. Testing for non-linear trends can be done by interacting veteran inflow with time FE instead of a post-WWI dummy and allowing for a time-varying treatment effect. The reference category in this case is the last pre-WWI election in 1912 and is therefore not interacted with the treatment. Figure 1.6 plots the 20 coefficients and the respective 10%

Table 1.4: The effect of veteran inflow on other parties

	Rightwing	RW+Communist	Centre	Left-Lib.	Socialist	Others
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans p.c.	1.080**	0.752	0.433	0.065	−0.962***	−0.604*
	(0.467)	(0.487)	(0.265)	(0.421)	(0.275)	(0.326)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.784	0.798	0.947	0.658	0.910	0.468

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

confidence intervals over time. The observed coefficients are reassuring and confirm that veteran inflow only had a positive effect on right-wing voting after WWI. This graph also highlights the persistence of the treatment effect until the end of the sample period in 1933. Crucially, the effect only really strikes in May 1924 rather than immediately after WWI.

Disaggregating the treatment effect on right-wing voting additionally also by parties, reveals an interesting pattern. A comparison between the coefficients in columns (3) and (4) before the war shows negative pre-trends of the conservative party mirrored by *positive* estimates of the right-liberals. At this point, it is important to know that pre-election agreements among Conservatives and Right-Liberals as their closest political ally were very frequent during the German Empire [Kühne, 2005]. In those agreements, parties would agree in advance that only one of their candidates would run in a specific districts, while the other party's candidate in a different precinct would face no competition from the second party. Such arrangements were common but also rational given the coexistence of majoritarian voting in a multi-party system. While official cooperation between Conservatives and Right-Liberals only occurred in the so-called *Kartellparteien* (cartel parties) in 1887 and 1890 and the *Bülow-Block* in 1907, the coefficients in specification (3) and (4) insinuate that pre-election agreements were probably starting from about 1878 onwards. The conclusion to be drawn from this is that conservative abstentions were far more frequent in areas of high veteran inflow than others.

Table 1.5: Baseline results and different FE specifications

	Rightwing vote share					
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans p.c.	1.080**	1.180***	1.526***	1.021	1.077	0.746
	(0.467)	(0.430)	(0.419)	(0.747)	(0.784)	(0.821)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Election×Province FE	N	Y	N	N	Y	N
Election×District FE	N	N	Y	N	N	Y
Precinct FE×t	N	N	N	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.784	0.855	0.877	0.852	0.896	0.911

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

1.5.3 Instrumental variable results

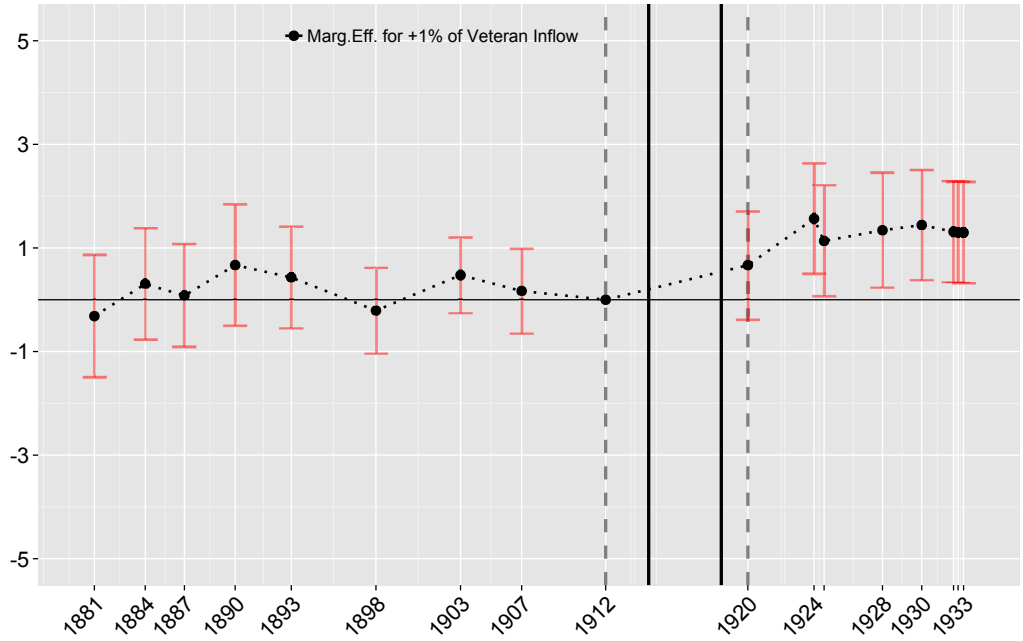
As the previous section has shown, there is strong support for the validity of the common trends assumption. The premise that could not be tested formally in the preceding section, however, is the absence of confounding events related to veteran inflow in magnitude and timing. Even though many potential confounders have already been included into the set on control variables, one cannot rule out all factors that might have driven the process of conscription or survival at the front. I tackle this problem by instrumenting veteran inflow with the employment share of war-related industries as of 1882 as described in section 1.4.3. A fundamental worry already arises the first-stage relationship between the potentially endogenous $veterans_i \times postWWI_t$ and the instrument $WarIndustry_i \times postWWI_t$ in column (1) of table 1.7. While the relation is significant and goes in the hypothesised direction, the rather low F statistic of 7.72 is not strong enough to rule out concerns about a weak instrument. This is also reflected in the insignificant reduced form and IV estimates. However, even though the instrumented effect on conservative vote share is insignificant as a result of the high standard errors, its magnitude remains similar to that of the Diff-in-Diff estimate. In order to back this up, I also re-estimated the model using a LIML which yielded near-identical point estimates for columns (4),

Table 1.6: Differences-in-Differences estimates with time-varying treatment effect

	Rightwing	Antisemitic	Conservative	Right-Liberal
	(1)	(2)	(3)	(4)
Veterans p.c.×1881	−0.316	0.255	−1.462***	0.891
	(0.716)	(0.256)	(0.563)	(0.768)
1884	0.304	0.255	−1.361**	1.411**
	(0.652)	(0.256)	(0.647)	(0.689)
1887	0.079	0.255	−1.932**	1.757**
	(0.603)	(0.256)	(0.950)	(0.893)
1890	0.669	0.298	−0.502	0.873
	(0.709)	(0.242)	(0.616)	(0.682)
1893	0.430	0.582**	−0.918	0.767
	(0.597)	(0.254)	(0.685)	(0.639)
1898	−0.209	0.313	−1.480**	0.958
	(0.503)	(0.355)	(0.617)	(0.583)
1903	0.475	0.126	−1.021**	1.370***
	(0.446)	(0.261)	(0.488)	(0.501)
1907	0.168	0.047	−0.745	0.866*
	(0.499)	(0.317)	(0.507)	(0.465)
1920	0.667	0.138	0.173	0.356
	(0.636)	(0.267)	(0.524)	(0.572)
May 1924	1.564**	0.899**	0.476	0.189
	(0.645)	(0.387)	(0.514)	(0.568)
Dec 1924	1.134*	0.367	0.661	0.107
	(0.652)	(0.312)	(0.544)	(0.557)
1928	1.341**	0.467	0.523	0.351
	(0.673)	(0.331)	(0.496)	(0.569)
1930	1.442**	0.420	0.745	0.277
	(0.650)	(0.343)	(0.465)	(0.556)
July 1932	1.313**	0.244	0.775	0.294
	(0.594)	(0.347)	(0.490)	(0.532)
Nov 1932	1.303**	0.096	0.879*	0.328
	(0.593)	(0.344)	(0.484)	(0.537)
1933	1.297**	0.133	0.870*	0.293
	(0.596)	(0.323)	(0.495)	(0.535)
Precinct FE	Y	Y	Y	Y
Election FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Precincts	266	266	266	266
Observations	4,522	4,522	4,522	4,522
R^2	0.785	0.872	0.736	0.525

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

Figure 1.6: Time-varying treatment effect estimates and 90% CI: Rightwing votes



(7), (10) and (13).[21]

The IV results are confirming the Diff-in-Diff estimates by returning treatment effects of similar or higher size. The effect on right-wing votes in specification (4) increases to 1.96 but loses statistical significance. Column (10) shows that most of this increase is due to a notably higher treatment effect of 2.80 on the conservative party which is also significant at the 10% level. The IV estimate for anti-semitic parties is still insignificant but now negative at -0.18. The effect on right-liberal parties remains negative and insignificant. Overall, the IV estimates underline the findings in table 3.3 that veteran inflow is exclusively benefiting the conservative DNVP. This should be born in mind when moving on to analysing the mechanisms behind the baseline findings. Given the weak first-stage, I will proceed with the differences-in-differences results as my preferred specification.

1.6 Mechanisms

1.6.1 A two-stage mechanism: Evidence from timing and parties

As figure 1.6 showed, veterans' effect on right-wing votes did not fully materialise right after the war but only in 1924. This raises doubts about whether it was

[21] Results are reported in table 1.22.

Table 1.7: Instrumental Variable estimates

Dep. var.	Vet. p.c.	Rightwing			Antisemitic			Conservative			Right-Liberal		
	OLS	Red.Form	OLS	IV	Red.Form	OLS	IV	Red.Form	OLS	IV	Red.Form	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
1882 % war-ind.	-0.332^{***}	-0.652			0.059			-0.932			0.221		
	(0.120)	(0.461)			(0.238)			(0.642)			(0.588)		
Veterans p.c.			1.080^{**}	1.963		0.109	-0.178		1.685^{***}	2.805^{*}		-0.714	-0.665
			(0.467)	(1.458)		(0.212)	(0.723)		(0.483)	(1.615)		(0.500)	(1.659)
Precinct FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266	266	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522	4,522	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.989	0.783	0.784	0.783	0.872	0.872	0.872	0.732	0.735	0.733	0.522	0.524	0.524
IV F-stat.	7.72												

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

actually WWI that turned veterans towards the right. In order to provide a better understanding about the timing of the effect, I investigate the yearly effects of veteran inflow also for the losing parties. The results in table 1.8 also revise some of the findings about other parties' reaction to veteran inflow in table 3.4. To start with, the perceived gain of the centre party in areas with higher war participation after WWI actually took place between 1903 and 1907. Virtually all post-WWI coefficients are identical to that of 1907. This insinuates that 1912 and 1920 may be regarded as outliers for the relation between veterans per capita and vote share of the centre party. A similar scenario can also explain the losses of *Other* parties. Again, the 1907 coefficient is very similar to all post-WWI coefficients. The identified drop of particularists' votes is therefore higher in areas with high war participation in the future but unrelated to war itself.

The most important result of table 1.8, however, is that losses of the socialist parties from veteran inflow already took place already in the first post-WWI Reichstag election 1920. A one unit increase in the population share of veterans leads to a drop of 0.8 in the socialist vote share in 1920 compared to 1912. The winners of this drop, however, were not only the rightwing parties but also the left-liberals and the centre. The main effect on right-wing votes observed in the baseline results, in fact, does not take place before May 1924. In this election the veteran effect drops or turns negative for all parties apart from the far-right. The negative effect on socialist votes, however, remains unchanged. The main findings in tables 3.3 and 3.4 therefore seem to be part of a two-stage mechanism: 1) a drop of socialist votes immediately after WWI in 1920 and 2) an increase in right-wing votes in May 1924 – both depending on war participation.

The timing of these mechanisms suggests that 1) is actually related to the war while 2) is a result of the post-war period. This also guides the remainder of this section. I start by extrapolating the war-related and social factors which determined veterans' negative effect on socialist votes. Then I explore the impact of political socialisation between 1920 and 1924 on the distinct swing to the right in areas with high war participation. Finally, I look at the channels through which political attitudes were transmitted to veterans and from them to others.

1.6.2 Direct effects of war participation

According to official statistics, the Imperial Army recorded about 4.2 million cases of non-fatal injuries Statistisches Reichsamt [1926].[22] However, due to the success-

[22] Unfortunately, this statistic did not differentiate between *cases* of injuries and *ever injured* soldiers in WWI.

Table 1.8: DID estimates with time-varying treatment effect for other parties

Vote share	Rightwing	Centre	Left-Liberal	Socialist	Others
	(1)	(2)	(3)	(4)	(5)
Veterans p.c.×1881	−0.316	−1.161***	1.260*	−0.277	0.510
	(0.716)	(0.338)	(0.724)	(0.370)	(0.409)
1884	0.304	−0.825*	0.006	0.039	0.486
	(0.652)	(0.453)	(0.554)	(0.365)	(0.388)
1887	0.079	−0.674*	0.022	0.202	0.414
	(0.603)	(0.365)	(0.571)	(0.334)	(0.343)
1890	0.669	−0.810**	−0.597	0.532	0.216
	(0.709)	(0.386)	(0.576)	(0.332)	(0.488)
1893	0.430	−1.023**	−0.302	0.690**	0.221
	(0.597)	(0.417)	(0.456)	(0.274)	(0.361)
1898	−0.209	−1.079***	−0.143	0.732**	0.805*
	(0.503)	(0.351)	(0.442)	(0.345)	(0.469)
1903	0.475	−0.727**	−0.280	0.023	0.522
	(0.446)	(0.339)	(0.379)	(0.166)	(0.333)
1907	0.168	−0.298	0.427	0.040	−0.328
	(0.499)	(0.254)	(0.320)	(0.113)	(0.290)
1920	0.667	0.019	0.411	−0.829***	−0.226
	(0.636)	(0.342)	(0.480)	(0.271)	(0.329)
May 1924	1.564**	−0.351	0.088	−0.886***	−0.316
	(0.645)	(0.385)	(0.473)	(0.254)	(0.339)
Dec 1924	1.134*	−0.212	0.119	−0.729***	−0.268
	(0.652)	(0.378)	(0.473)	(0.249)	(0.330)
1928	1.341**	−0.351	0.130	−0.715***	−0.357
	(0.673)	(0.343)	(0.480)	(0.264)	(0.433)
1930	1.442**	−0.340	0.168	−0.663***	−0.594
	(0.650)	(0.326)	(0.488)	(0.255)	(0.409)
July 1932	1.313**	−0.358	−0.014	−0.754***	−0.182
	(0.594)	(0.315)	(0.484)	(0.271)	(0.360)
Nov 1932	1.303**	−0.371	−0.010	−0.766***	−0.152
	(0.593)	(0.314)	(0.483)	(0.270)	(0.365)
1933	1.297**	−0.437	−0.023	−0.598**	−0.211
	(0.596)	(0.337)	(0.484)	(0.250)	(0.368)
Precinct FE	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522
R^2	0.785	0.947	0.660	0.911	0.470

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

ful re-integration of veterans into the labour market only a small fraction became dependent on state benefits. Bessel [1988], for instance, notes that many companies were trying very hard to find employment for their former workers even if they were actually not in need of additional labour. The amount of soldiers whose injury entitled them to state benefits was about 660,000 according to a survey of veteran benefit recipients in 1924. More than 25% of these had an earnings reduction above 50% [Statistisches Reichsamt, 1925]. Even though the German veteran benefit law was generous compared to other countries, the state did not manage to win veterans' support. Especially the bureaucratic pension system and the lack of special recognition of war-injuries bred discontent among former soldiers [Diehl, 1993]. As a consequence, ex-soldiers depending on state benefits may have developed a particular hatred against the state which would give an explanation for my findings above. I expect this effect to be even higher for those with substantial disabilities and little chances on the labour market. An alternative link between combat experience and extremist voting is provided in Grossman, Manekin, and Miodownik [2015] who show that war exposure increases prejudices and support for military conflict among Israeli recruits.

As a result, benefit receiving veterans and particularly those with more severe injuries could be driving the baseline effect. I exploit two sources of data to investigate the effect of war's direct consequences for veterans on right-wing support. The first one are numbers on recipients of veteran benefits in 1929 provided in Statistisches Reichsamt [1933]. This data is provided at the county level and has already been used by Adena et al. [2015] and Satyanath, Voigtländer, and Voth [2013] as a measure of war participation. Two downsides of this source of information are that it does not differentiate between veterans and their dependants and was collected 10 years after the end of the war when many veterans might have already passed away. In my heterogeneity analysis I investigate whether the veteran effect was significantly different in precincts above the median of veteran benefit recipients per capita. The second source comes from the aforementioned survey by the Statistisches Reichsamt [1925] of all benefit receiving individuals in 1924. Crucially, this publication lists the average reductions in earnings potential of benefit recipients for given larger areas (provinces). This data furthermore allows to explicitly focus on 1) former soldiers rather than dependants and 2) those who fought in WWI as opposed to other wars. From this I calculate the average earnings reduction among all benefit-receiving WWI veterans as a proxy for combat exposure. I interact veterans per capita with a linear measure of combat exposure rather than a median split dummy since the high level of aggregation may result in picking up other differences

across provinces.

Table 1.9 presents results of the baseline specification after adding the proxies discussed above (interacted with a post-WWI dummy) as well as their interaction with veteran inflow. The median split by recipients of veteran benefits in column 2 does not lead to a significant change in the baseline coefficient for the conservatives. The right-liberals seem to be losing more from veteran inflow in precincts with many benefit recipients but this difference is not significant. Also the effect of veterans on socialists is not affected. Combat exposure, on the other hand, seems play an important role in determining veterans' effect on socialist votes. However, since this effect is linear, one can only draw conclusions about veterans' actual impact by looking at the marginal effects which I have plotted in figure 1.7. The figures show the marginal effect of veteran inflow in dependence of the interacted variable, combat exposure in this case. The background shows a histogram of the interacted variable and thus gives information at which points the marginal effect actually matters. This analysis reveals that the marginal effect on socialist votes increases with combat exposure but does not depend on it. Only at the far left of the distribution, the treatment effect becomes insignificant.

Taken together, I find only mixed support for the widespread image of the war-disabled, impoverished veteran who becomes embittered by the Weimar society and radicalises. Using the precise numbers on veteran benefit recipients in 1929, the main results are left virtually unchanged. A channel working through impoverishment from war participation can therefore be ruled out. Combat exposure, on the other hand, seems to have some power in explaining the size of the effect on socialist but not its existence. These results are in line with those of Grossman, Manekin, and Miodownik [2015] cited above but should be interpreted with caution given the high level of aggregation and the presumably non-random selection into combat exposure in this empirical setup.

1.6.3 Effect heterogeneity across social groups

During a war, men from very different parts of the social strata are often serving in the same unit. The German army during WWI was no different in that respect. Even though Ziemann's analysis (2007) of German war letters suggests that soldiers tended to bond with others from nearby places and similar social background, one cannot entirely rule out such a mechanism. I continue by exploring in which parts of the society a transition of socialist to right-wing support in relation to war service was most likely. In order to do this, I look at the religion and social class as the two most important lines of social division in modern Germany.

Table 1.9: Veteran inflow and the social consequences of WWI

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans p.c.	1.080**	1.406*	21.937	−0.962***	−1.065**	37.469**
	(0.467)	(0.718)	(37.136)	(0.275)	(0.435)	(18.735)
Poor veterans>Median		0.035			−0.020	
		(0.134)			(0.078)	
Vet.×Poor vet.		−0.460			0.162	
		(0.975)			(0.555)	
Combat exposure (linear)			0.010			0.120**
			(0.109)			(0.057)
Vet.×Combat expos. (lin.)			−0.451			−0.827**
			(0.801)			(0.403)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.784	0.785	0.788	0.910	0.910	0.911

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

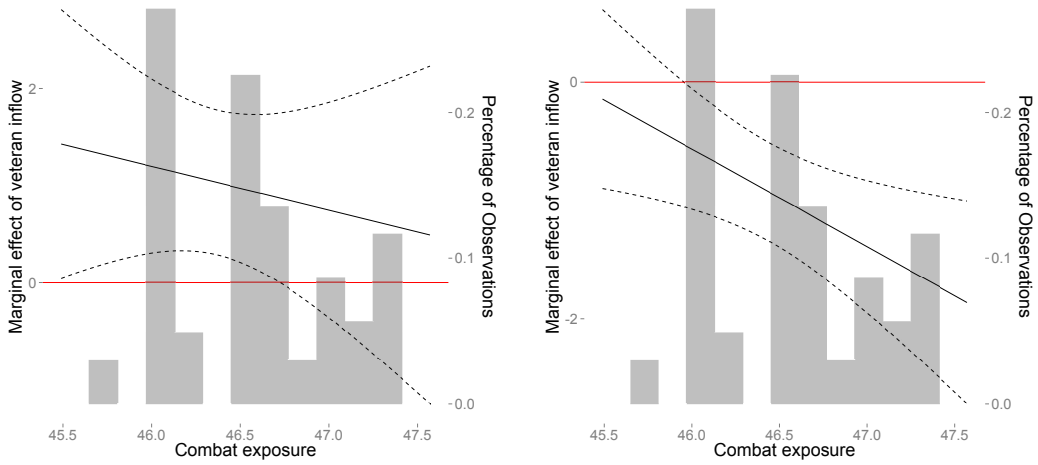


Figure 1.7: Marginal effect of veterans depending on share of highly disabled WWI soldiers

Table 1.10: Veteran inflow and social composition

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.080**	−0.123	0.439	−0.962***	−0.008	−0.826***
	(0.467)	(0.820)	(0.521)	(0.275)	(0.357)	(0.316)
Working class 1882>Median		−0.245*			0.226***	
		(0.134)			(0.069)	
Vet.×% Working class 1882		1.799*			−1.452***	
		(0.972)			(0.497)	
Protestants 1910>Median			−0.178			0.054
			(0.146)			(0.089)
Vet.×% Protestants 1910			1.692			−0.376
			(1.044)			(0.610)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.784	0.785	0.786	0.910	0.912	0.910

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

For my analysis, I investigate treatment effects in precincts with above median share of protestants in 1910 or share of population in manufacturing in 1882. Results for the corresponding regressions are displayed in table 1.10. The findings in columns (2) and (5) are important because they rule out that socialists were losing votes in non-working class areas. In precincts with a low share of the working class, the coefficients are close to zero. The veteran effect is therefore a distinct working-class phenomenon. Concerning the role of religion, columns (3) and (6) show that the gains of the right-wing from veteran inflow are only significant in precincts with an above median share of protestants. However, these appear to have been mostly at the expense of parties *other* than socialist ones. The interaction with *Protestants 1910 > Median* is negative for socialists but not significant. The plain treatment effect, on the other hand, remains highly significant and only marginally changes magnitude.

One explanation for the large effects in working class areas is that veterans could have picked up political attitudes during their service. If this was the case, the effect should also be higher in areas where socialists had an ideological monopoly

Table 1.11: Veteran inflow and political diversity

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.080**	−0.309	−0.296	−0.962***	−0.211	−0.208
	(0.467)	(0.753)	(0.731)	(0.275)	(0.345)	(0.361)
Socialist monopoly 1912>Median		−0.260**			0.154**	
		(0.128)			(0.067)	
Vet.×Soc. monopoly 1912>Median		2.296**			−1.252***	
		(0.942)			(0.475)	
Socialist monopoly 1907>Median			−0.241*			0.150**
			(0.128)			(0.072)
Vet.×Soc. monopoly 1907>Median			2.179**			−1.206**
			(0.924)			(0.504)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.784	0.788	0.788	0.910	0.911	0.911

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

before the war and new political ideas were presumably most efficient. In order to measure left ideological monopoly, I use a Herfindahl index for the elections immediately preceding WWI in 1912. Since high values could capture lack of diversity of the left and the right, I weight the index by the socialist vote share. The new variable is therefore highest in areas with low competition and high support for socialist parties and lowest in those with elevated competition and support for non-socialist parties. In order to make the analysis robust, table 1.11 uses median splits and reports also corresponding estimates using the 1907 elections for constructing the index. The estimates in column (2) show that the positive effect of veterans on right-wing votes is entirely driven by areas with a left monopoly before WWI. The same is also true for the loss in socialist votes in column (5). Specification (3) and (6) rule out that this finding might be driven by the peculiarly strong results of the socialist parties in the 1912 election.

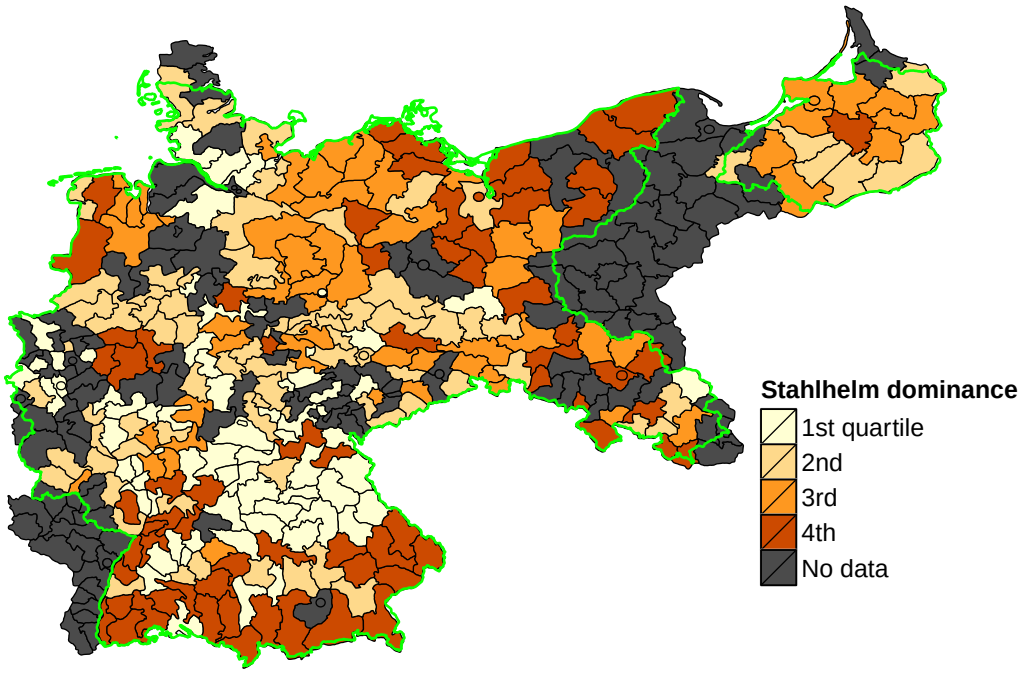
Putting the above findings together, I find that social class is a powerful socio-economic predictor of the veteran effect. The treatment effect on right-wing votes is restricted to precincts with an above median share of protestants and men

between 25 and 49 years. This factor, however, cannot explain the losses of the socialists and is therefore mainly informative about where the right gained but not where those votes were coming from. The share of the working class, on the other hand, highlights a direct link between war participation and the diversion of socialist votes to the right-wing in areas with a higher population share of veterans. This is particularly striking given the low treatment effect on the anti-semitic parties and the fact the predecessors of the DNVP and the DVP were representing the upper middle class and aristocracy. War participation was therefore crucial for the right to overcome class divisions and attract votes from the left during the Weimar Republic. Religious division, on the other hand, could not be overcome and treatment effects were highest in protestant areas which were already supporting the conservatives before the war. The results are consistent with a transmission of political thoughts and ideas. The strength of the effect in areas where the socialist party had a political monopoly suggests that veterans are associated with the inflow of new political ideas into an environment where such thoughts could not take place before. Section 1.B.2 in the appendix shows that informal social ties with former officers cannot explain this pattern. Rather, transmission seems to have taken place during the war among lower-ranked soldiers. One possible channel of spreading anti-communist thoughts in the working class milieu could be the conspiracy theory of the *stab-in-the-back* mentioned in section 1.2.

1.6.4 Socialisation: Veteran associations

One way in which the rightwing could have gained support from war participation between 1920 and 1924 is through socialisation in the ex-servicemen's clubs and combat leagues mentioned in section 1.2. Diehl [1975] highlights that the conservative *Stahlhelm* association started to get politically active around 1921/1922 which would coincide with the timing in table 1.8. Its two main competitors, the social-democratic *Reichsbanner* and the communist *Rotfront*, were each founded in the first half of 1924. In the following analysis I investigate whether higher popularity of the *Stahlhelm* can explain the veteran effect. A straightforward way to quantify the strength of associations is membership numbers relative to the local population. Apart from the NSDAP, where an impressive research project on party members has been carried out by Brustein and Falter [see Schneider-Haase, 1991, for details], obtaining data on followers of political organisations during the Weimar Republic is usually very difficult. Many organisations were too small to systematically collect information or their records were destroyed due to political or war-related reasons. Luckily, the *Stahlhelm* was not only a rather big association but also heavily influ-

Figure 1.8: Stahlhelm dominance based on veteran associations' membership data



enced by the proverbial Prussian passion for data collection. It therefore regularly demanded from its regional chapters not only reports on membership numbers but also on competing organisations such as the social-democratic Reichsbanner (RB) and the communist Rotfront (RF). Not all of these found their way into archives but I managed to collect and digitise almost completely the original sheets of the 6th Stahlhelm census (*6. Stärkemeldung*) in late 1929/early 1930 and reports on enemy organisation of early 1928.[23]

While being unique and extremely valuable for the study of veteran life, this data also has important drawbacks. Many areas only provided aggregates at a higher level and some areas are not covered at all. More generally, misreporting in any direction could be the case even though it does not appear too likely given the strong belief of the Stahlhelm in obedience. If no data was available for a given area, zero was assigned. Since this is particularly problematic for larger areas, time-varying district fixed effects are introduced into each regression. A more fundamental issue is that this information only provides a one-time snapshot of organisations' strengths and may not completely reflect its that of the past and near future. Both points are not negligible and should be born in mind when analysing the results. From the

[23] I collected this data entirely from the stocks of the German Federal Archive (see appendix for further details).

Table 1.12: Veteran inflow and measures of veterans' politicisation

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.526***	0.761	2.455	−0.735**	−0.542	−2.215
	(0.419)	(0.599)	(1.923)	(0.353)	(0.449)	(1.396)
Stahlhelm p.c.>Median		−0.083			−0.192*	
		(0.163)			(0.105)	
Vet.×Stahlhelm >Med.		0.121			0.068	
		(0.162)			(0.110)	
Reichsbanner p.c.>Median		−0.221*			0.116	
		(0.130)			(0.098)	
Vet.×Reichsbanner >Med.		0.566			1.252*	
		(1.087)			(0.716)	
Rotfront p.c.>Median		−0.260			−0.530	
		(1.164)			(0.819)	
Vet.×Rotfront >Med.		1.393			−0.745	
		(0.916)			(0.713)	
Stahlhelm rule			−0.750			−0.341
			(0.688)			(0.384)
Vet.×Stahlhelm rule			−1.776			2.829
			(3.379)			(2.505)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.877	0.880	0.877	0.943	0.944	0.943

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: $\text{Migration}_{1910-1919}$; % Men aged 9-41 1910; % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE)

membership data, I calculated two different measures: *Stahlhelm/RB/RF per cap.* divides an area's members by the corresponding population from the 1919 census.[24] $StahlhelmDominance_i$ is the share of Stahlhelm members over the sum of RB, RF and Stahlhelm members.[25] Finally, the distribution of veteran associations is not predetermined and potentially endogenous.

[24] Since Stahlhelm area borders do not precisely follow 1910 district or precinct borders, the matching is initially carried out at the district level with reported areas being treated as aggregates of several *political* districts. Precinct data is then formed as a population weighted average of each district's Stahlhelm/RB/RF per cap. measure. The 1919 census is used instead of 1910 because it allows a more accurate match with the Stahlhelm data of the late 1920s.

[25] In the base of zero membership numbers, the following procedure was applied: if only Stahlhelm or summed RB and RF members had value zero, $StahlhelmDominance_i$ was replaced with the highest/lowest value possible, i.e. 0 or 1. If both values were zero, a tie was imputed and value 0.5 assigned.

The spatial distribution of Stahlhelm dominance is shown in figure 1.8 and suggests that veterans were far more right-wing in the south and north-east of Germany. As mentioned above, in order to account for this strong spatial clustering, all regressions using the combat league membership data are using election-specific district fixed effects. The far bigger issue when including veteran membership data is its endogeneity. In order to alleviate this problem, I first regress each *bad control* on my baseline set of predetermined control variables. My analysis then uses the predicted value from these regressions as an *exogenised* version of the original variable. The analysis proceeds as follows. Regression (2) and (5) interact veteran inflow with dummies for having an above median members of Stahlhelm, Reichsbanner, and Rotfront. I use the median splits for of all three associations since competition among them might induce correlation in membership strengths. Second, specification (3) and (6) interact with a linear measure of *StahlhelmRule*. I use a linear term since the assignment of the value of 0.5 to any side is crucial and because the map in figure 1.8 revealed strong spatial clustering which might be picked up by a median dummy. The corresponding results are shown in table 1.12.

Looking at the coefficient of *Veterans×(Stahlhelm>Median)* in columns (2) and (5) shows that Stahlhelm strength does not explain the baseline effect. In fact, the coefficient is negative and for right-wing votes even significant at the 10% level. For socialist votes, the coefficient is also negative but tiny compared to the interactions with memberships of the left-wing combat leagues. The significant positive effect of *Veterans×(Reichsbanner>Median)* could be reflecting the findings in section 1.6.3 that the right-wing was particularly gaining from veteran inflow in working class areas. This is corroborated by the negative coefficient of the same variable in specification (5). The inclusion and interaction of *StahlhelmDominance* does not have strong explanatory power either. The plain treatment effect in regression (3) is almost unchanged compared to (1) which is also reflected in the marginal effect plot in figure 1.12. The marginal effect plot, however, also shows that the Stahlhelm had a significant negative effect on socialist votes only in areas where the Stahlhelm outnumbered its counterparts on the left. Keeping all caveats of the data in mind, this seems to suggest that combat leagues might have played a role in spreading anti-socialist propaganda to veterans. However, my results show no evidence to believe that the Stahlhelm turned veterans towards the political right.

1.6.5 Socialisation: Anti-communism

In this section I investigate the role of anti-communism in explaining the veteran effect on right-wing and socialist votes. Probably the most severe experience for

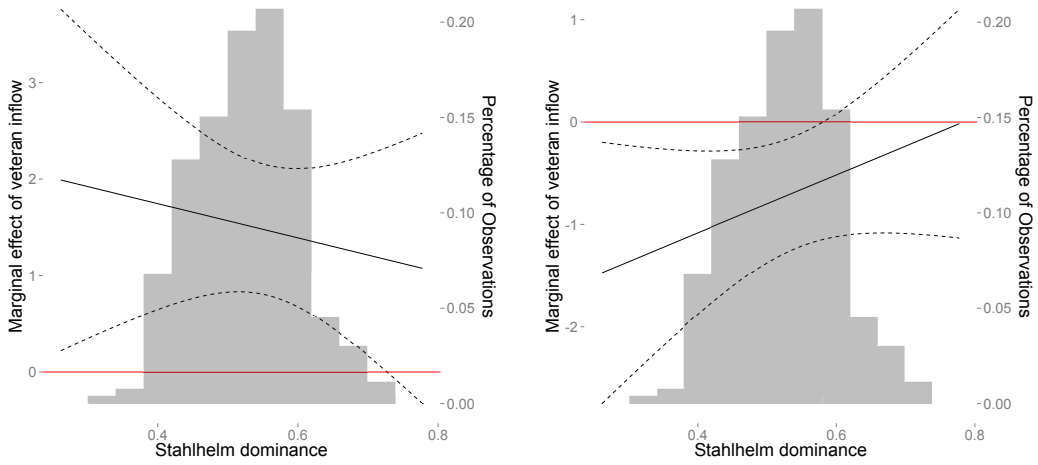


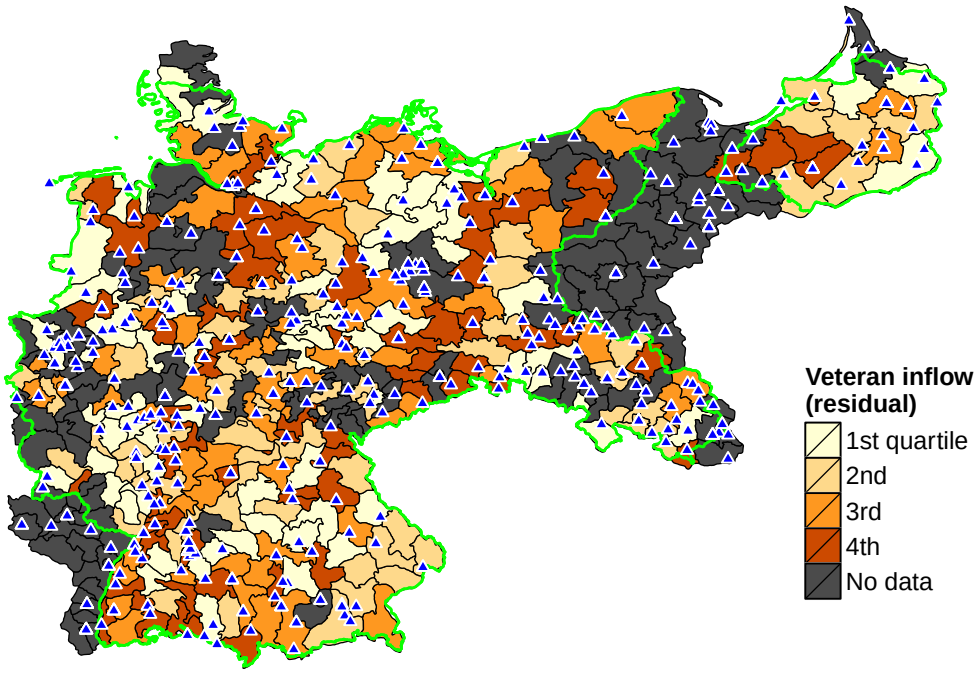
Figure 1.9: Marginal effect of veterans depending on Stahlhelm dominance

Table 1.13: Veteran inflow and support for communists 1920/1924

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.080**	0.134	−0.222	−0.962***	−0.443	−0.224
	(0.467)	(0.711)	(0.947)	(0.275)	(0.296)	(0.360)
Communist vote 1920>Median		−0.230*			0.130*	
		(0.129)			(0.069)	
Vet.×Comm. 1920>Med.		1.963**			−1.054**	
		(0.948)			(0.486)	
Communist vote May 1924>Median			−0.257*			0.198***
			(0.149)			(0.074)
Vet.×Comm. May 1924>Med.			1.977*			−1.293**
			(1.073)			(0.512)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.784	0.786	0.786	0.910	0.911	0.911

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: $\text{Migration}_{1910-1919}$; % Men aged 9-41 1910; % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE)

Figure 1.10: Freikorps locations and unexplained veteran variation across precincts (units outside the borders of Imperial Germany not shown)



many veterans was the return to a country ruled by soldiers' and workers' councils. The communist coup attempts in 1919 spread fears of a violent *Bolshevik* revolution and led to a radicalisation among the middle class [Fritzsche, 1990]. The uprisings also corroborated beliefs in the stab-in-the back myth, namely that the state was secretly working against the middle and upper class in order to establish a communist dictatorship. These fears were unjustified given that in the elections for the National Assembly 1919, only 7.6% went to communist parties. This changed dramatically in 1920 after a failed coup attempt by the far-right. Even though the vote share for socialist parties remained almost the same, communists now received 17.9% which was mainly at the cost of the more moderate social democrats. Over the following years, the German communists started to get heavily influenced by the Communist International and engaged in coup attempts in Central Germany (1921) and Hamburg (1923). This coincides with the strong increase of veterans' effect for right-wing votes who were also the most fervent and credible anti-communist parties.

I investigate the importance of anti-communism in two ways: my first test looks at whether the treatment effect is stronger in areas where a fear of communism was justified and communist parties received a vote share above the median.

To avoid endogeneity issues, I use again predicted values from a regression on predetermined covariates rather than the actual values. Since the radical phase of the communist party started after 1920, I interact veterans with a median dummy of communist votes in 1920 and 1924 as a cross-check. Table 1.13 reveals that this distinction does not make any difference. Specifications (2) and (3) show that the veteran effect on right-wing votes was higher in areas with above median communist support in both the 1920 and May 1924 elections. The effect on socialist parties in columns (5) and (6) mirrors this effect. The support for communists in May 1924, however, has a slightly more negative effect. This could be because many moderate communists had returned to the social democrats by that time, so that the vote in May 1924 is a more accurate measure of *radical* communist support.

My second test is related to paramilitary *Freikorps* units set up after 1919 in order to fight communist insurgencies in Germany and the Baltic states [Büttner, 2008]. Led by former officers, they consisted to a large part of former soldiers but also included many volunteers who were too young to fight in WWI. The peak membership of the Freikorps was between 100,000 and 400,000 and therefore represented at most 4% of all former WWI soldiers. Nevertheless, the existence of such volunteer units can be linked to deep anti-communism in a specific area. In order to construct a measure of Freikorps exposure, I digitised a comprehensive lists of Freikorps units by Tessin [1974] and geocoded these according to their origin town. I proxy precincts' exposure to anti-communism by calculating the inverse distance to the nearest Freikorps unit. Figure 1.10 depicts the spatial distribution of Freikorps over areas with different extents of unexplained variation in veteran inflow. Units' locations are scattered over the whole country which but show also slight concentrations. Some of these concentrations are around large cities which experienced communist uprisings such as Berlin, Magdeburg and the Ruhr area. Silesia in the South-East has more Freikorps units since they were also used to fight Polish separatist movements. The rural areas of Bavarian in the South and Pomerania in Central North have also a lower concentration. The correlation with veteran inflow, however, is only 0.15 (0.11 for the median split dummy). This is also reflected in figure which shows Freikorps units in areas of high and low unexplainable variation in veterans. In order to entirely rule out endogeneity issues, I use again an exogenised version as was done for the communist votes above.

The regressions in table 1.14 interact the treatment veteran inflow with a linear measure of $ProxFreikorps_i$ and a dummy closer to the nearest unit than the median. I report both measures because of the moderate spatial concentration which could result in picking up other factors. The results for the median split in columns

Table 1.14: Veteran inflow and exposure to anti-communist paramilitary

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.080**	0.349	2.533	−0.962***	−0.175	−3.137***
	(0.467)	(0.759)	(1.777)	(0.275)	(0.326)	(0.888)
Prox. Freikorps>Median		−0.224*			0.206***	
		(0.131)			(0.071)	
Vet.×Prox. Freikorps>Med.		1.306			−1.344***	
		(0.938)			(0.499)	
Prox. Freikorps (linear)			3.003			1.568
			(4.821)			(2.046)
Vet.×Prox. Freikorps (lin.)			8.795			−13.160***
			(10.542)			(4.915)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.784	0.786	0.784	0.910	0.911	0.911

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

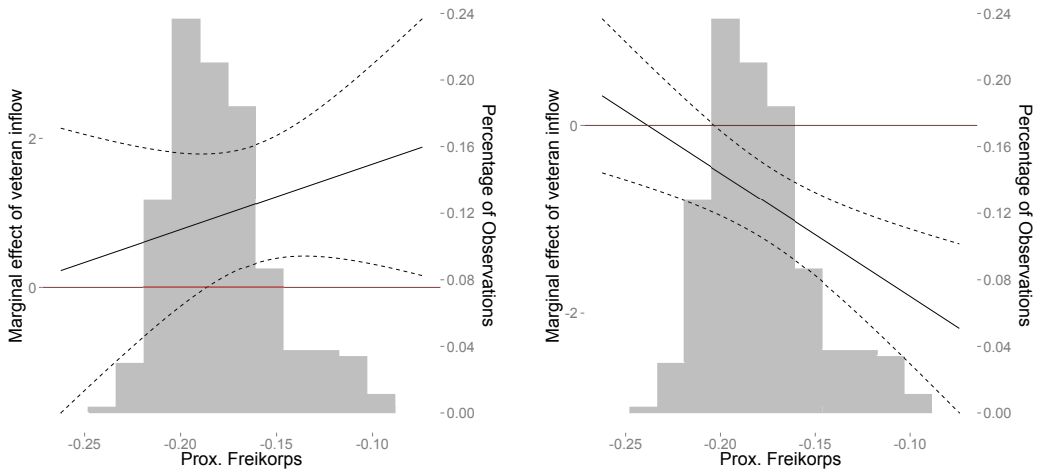


Figure 1.11: Marginal effect of veterans on on right-wing vote share

(2) show that areas close to the nearest Freikorps are also those who are driving the veteran effect on right-wing votes. Yet, the difference between both groups is not statistically significant. For communist votes, being this effect is highly significant. Precincts above the median of $ProxFreikorps_i$ are also those who are driving the negative veteran effect on socialist votes. The results in columns (3) and (6), on the other hand, show that the treatment effect is strong and significant only in precincts who are very close to the nearest Freikorps. Figure 1.11 illustrates that the veteran effect on each party is not significantly different from zero in areas in the lower half of the proximity distribution. Taken together, there is considerable support for a role of anti-communism in explaining veterans' effect on voting in the Weimar Republic.

1.6.6 Transmission mechanisms

The preceding sections have shown that the effect seems to be driven by anti-communist sentiments within the working class. One of the main questions which is still open concerns the mechanism how veterans spread anti-communist thoughts to others. The treatment effect of 1.08 cannot be solely attributed to former soldiers even if all of them had turned towards the right-wing. The higher membership numbers of Reichsbanner and Rotfront compared to the Stahlhelm point in the direction even such an extreme scenario was very unlikely. The following section looks at two transmission channels: first, I look at personal contacts within the family network through parents and spouses and second, I investigate impersonal contacts through election campaigning.

The first transmission channel explores the role of family networks and spouses. Galloway's data on Prussia provides me with the percentage of families among all households and the population share of women above the age of 20 in 1910. The first variable proxies how important families were with respect to single-person households while the second one measures the amount of new female voters, i.e. women above the age of 20 in 1920. Both variables proxy for different opportunities for veterans to influence the political thoughts of those in their immediate surroundings. Table 1.15 allows the treatment effect to vary in areas with an above median value of the above mentioned interaction terms. I do not find support that transmission within the family or couple is responsible for the baseline effect. Apart from specification (2), all interaction terms are insignificant and small in magnitude. Rather than being a stepping stone, column (2) suggests that areas with a higher share of family households had a significantly lower effect of veterans on right-wing votes. If anything, families therefore seem to have dampened political radicalisation

Table 1.15: Veteran inflow and transmission (Prussia only)

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.985***	2.219***	2.089***	−1.842***	−1.901***	−1.641***
	(0.542)	(0.509)	(0.645)	(0.352)	(0.344)	(0.373)
% Family HHs>Median		0.488***			0.015	
		(0.040)			(0.019)	
Vet.×% Family HHs>Med.		−0.418**			0.106	
		(0.199)			(0.103)	
% New female voters>Median			−0.449***			−0.017
			(0.043)			(0.020)
Vet.×% New fem. voters>Med.			−0.066			−0.128
			(0.186)			(0.096)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	144	144	144	144	144	144
Observations	2,448	2,448	2,448	2,448	2,448	2,448
R^2	0.833	0.835	0.833	0.922	0.922	0.922

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

among veterans. The amount of new female voters, on the other hand, does not change veterans' impact on voting behaviour.

The second set of tests looks at the specific role of campaigning. This factor is important because it measures how much parties were interacting with potential voters and how strong the need was to polarise and stand out among the competitors. In order to infer campaigning effort, I use the victory margin in a specific election.[26] This variable is constructed as the difference between the strongest party bloc and the runner-up and multiplied by −1. *VictoryMargin* therefore increases with the extent of (inferred) political contest and campaigning. I use again a median split of this new variable during the 1920 and May 1924 elections to investigate if and when campaigning mattered for the veteran effect. Concerns about the exogeneity of political contest with respect to veteran inflow are addressed by the use of two different election for the median split and by using predicted values rather than the original numbers. For both right-wing and socialist, the veteran effect is statistically different from zero only in areas with above median competition in May 1924 as

[26] See Ziblatt [2009] for a similar application to election in Imperial Germany.

Table 1.16: Veteran inflow and victory margin 1920/1924

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.080**	0.500	0.204	−0.962***	−0.904***	−0.324
	(0.467)	(0.526)	(0.677)	(0.275)	(0.351)	(0.317)
Victory margin 1920>Median		−0.229			0.030	
		(0.150)			(0.078)	
Vet.×Vic. margin 1920>Med.		1.563			−0.164	
		(1.052)			(0.541)	
Victory margin 1924>Median			−0.091			0.139**
			(0.125)			(0.069)
Vet.×Vic. margin 1924>Med.			1.595*			−1.162**
			(0.917)			(0.476)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.784	0.785	0.791	0.910	0.910	0.911

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

shown in columns (3) and (6). Specification (2) and (5) highlight that the victory margin of 1920 did not have a similar predictive power for the socialist party votes.

Overall, my results point in the direction that political attitudes were passed on through campaigning rather than the family network. The timing of the effects suggests that characteristics of the May 1924 election were more important than those of 1920. This is in line with the hypothesis that anti-communism became particularly salient in May 1924 after the communist party had radicalised. My results are congruent with such a mechanism but cannot provide a complete proof of the *stab-in-the-back* myth and its transfer to and from veterans. Knowing that the effect on socialists materialised already in 1920, the result could also be interpreted such that areas where socialists lost from veteran inflow were also those which would more fierce electoral competition in May 1924.

1.7 Conclusion

How does war service affect political attitudes? In this paper I provide empirical evidence on the role WWI veterans in shifting voting patterns in Weimar Germany from socialist parties to those of the right-wing after 1918. I show that the effect initially only harms socialists and only benefits the far-right few years later. This coincides with a radicalisation within the communist part of the socialist parties. I provide evidence that the effect primarily hits working class areas. The main beneficiary was the conservative DNVP, a party deeply rooted in the aristocracy and the wealthy upper class and thus ex-ante unlikely to receive votes from this part of society. This evidence points in the direction that veterans picked up a popular conspiracy theory – the *stab-in-the-back* myth. According to this theory, Germany had not lost the war but was betrayed by socialists and democrats who were trying to turn Germany into a Bolshevik country. The myth was especially used by the right-wing parties such as the Nazis and the DNVP. A possible channel is that the myth compromised socialist parties immediately but only benefited the right once the Bolshevik threat described in it turned real.

In line with this hypothesis is that the effect is strongest where support for radical communists was comparatively high. In other words, areas with a larger inflow of veterans reacted stronger to communist threat but did so in turning towards the extreme right of the political spectrum. I also find that areas with low exposure to alternative ideologies prior to WWI are reacting much stronger which corresponds to the relative power of this new political idea. Additional evidence suggests that high levels of political mobilisation in 1920 and political competition in May 1924 were also conducive for shifting votes to the right in areas with higher war participation.

The main lessons to be drawn from my results is that war can have substantial long-run effects through factors unrelated to physical damage. The fate of Weimar Germany who had not even fought WWI on its own soil is an illustrative example of war's indirect effect through political institutions. My case study focusses on the interaction between soldiers from various backgrounds as one potential mechanism through which such an indirect effect of war could materialise. I find persistent spill-over effects from war service on political attitudes and democratic capital in veterans' environment. From a policy perspective, my findings suggest that not only exposure to violence but also war service itself can have important effects on soldiers' attitudes. A diligent policy-maker should thus be very alert about the spread of extremist thoughts within the army since this might easily spread to wider

parts of the population and perpetuate the damaging effects of war.

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1.A Background information

1.A.1 The German National People's Party (DNVP) and Weimar democracy

In light of my main results, this historical description will only focus on the DNVP. A more detailed description of the NSDAP's political views can be found in Voigtländer and Voth [2012]. The DNVP represented the monarchist, strongly nationalist spirit common in late Imperial Germany's middle and upper class. It was formed as a merger of the conservative parties of Imperial Germany as well as of liberal and anti-semitic elements. Unlike its predecessors who represented mainly the large agrarian estate holders and the urban upper class, the DNVP had a much wider audience and received support across all social strata and parts of the Weimar Republic. Also labelled as the "*reservoir of the discontent*", the unifying element of its heterogenous following was the rejection of democracy and extreme nationalism [Ohnezeit, 2011].[27] The party manifesto of 1920, which was never changed throughout, expresses little sympathy for the democratisation and parliamentary government:

> "*[R]evolution became the big criminal who shattered morality, state system and economy (...). [T]he monarchic form of government conforms to Germany's character and historical development*"
> Deutschnationale Volkspartei [1920, p.2-5]

Also the use of the stab-in-the-back myth was a prominent tool in the DNVP's propaganda. A campaign poster for the December 1924 election, for instance, displays the murder of a fighting soldier by a masked thug and exploits this *image* to prevent people from voting for any democratic parties:

> "*Who supported social democracy in this [the stab-in-the-back]? Democrats and Erzberger's people [the centre party]. Now on the 7th of December, Germany is supposed to receive the second stab-in-the-back. Social democrats together with the democrats want to turn us into slaves of the Entente [the Allied Forces] and ruin us forever.*"
> Deutschnationale Volkspartei, reprinted in Barth [2003, p.299]

Even though the DNVP joined the first Hitler government and therefore played a crucial role in the Nazi party's rise to power, its stance on democracy appears

[27] Anti-semitism was also an important element of the DNVP. It was, however, not as defining as for the NSDAP. This is also exemplified by the secession of the racist Völkisch wing of the DNVP in 1922 to form its own party (DVFP) which was later absorbed by the Nazi party.

somewhat ambivalent given its participation in five other Weimar governments.[28] While this insinuates an acceptance of democratic governance, historians regard this as a result of the more pragmatic and moderate forces within the party arguing for a legal ascend to power [Liebe, 1956; Ohnezeit, 2011]. When the party entered government for the first time, in January 1925, party leader von Westarp commented this as follows:

> "*[The DNVP's] opposition was above all of a fundamental character since it was directed (...) against the republican-parliamentary system as such (...).*"
> Kuno von Westarp, quoted in [Mahlke, 1972, p.219]

This period of superficial cooperation only lasted until Oktober 1928 when Alfred Hugenberg of the party's radical wing took the leadership from von Westarp. While the DNVP was initially trying to cooperate and change the system from within, it now followed an entirely destructive and aggressively anti-democratic course [Mergel, 2003]. This is also exemplified in a comment by the member of parliament Reinhold Quaatz on the government crisis of 1930:

> "*General feeling: a thrust into the heart of parliamentarianism*"
> Reinhold Quaatz quoted in Lau [2008, p.394]

In sum, there is strong evidence for the anti-democratic character of the DNVP and the rejection of the Weimar constitution. Together with the NSDAP, it was the only right-wing party which consistently opposed parliamentary rule after democratisation. While it was very clear what the DNVP did *not* want, it remained vague about what system it wanted instead. Even a restoration of the monarchy was not undisputed within the party and later on abandoned in favour of *leader cult* around the new party leader Hugenberg [Lau, 2008; Ohnezeit, 2011].

1.A.2 WWI veterans' role during democratisation

Historic research has shown that by the end of WWI, the majority of German combatants had lost its morale and that the army was experiencing voluntary surrender and desertion and was in the process of disintegrating [Ulrich and Ziemann, 1997]. Being already regarded as an important pressure group, both social democratic and conservative veteran associations started to court soldiers and veterans in order to

[28] These were taking place in 1925 (Chancellor Luther), 1927/1928 (Chancellor Marx), 1930 (Chancellor Brüning), 1932 (Chancellor Papen) and 1932/1933 (Chancellor Schleicher).

increase their own post-WWI base of support. After democratisation and the introduction of generous pension laws for war-disabled, veterans stopped being at the focus of pro-democratic parties. At this time, the attitude towards the revolution was not clear at all as shown by excerpts from field post around the end of the war:

> "*We have lost the war so badly, since we will have to relinquish so much, that it is a shame. Hopefully those responsible for luring the poor population into destruction will not evade their deserved punishment.*"
>
> "*How are we going to do under this mob of bandits and criminals? Now people are expecting salvation from Erzberger [signer of the armistice] and Scheidemann [leader of the provisional government]. It was them who were undermining the inner resistance of the fatherland for years (...).*"
> German army field post, quoted in Ulrich and Ziemann [1997, p.31-32]

Upon their return, most soldiers were heartily welcomed at home. Towns were decorated and cleaned before their arrival, banners with welcoming messages were prepared and sometimes even small gifts for all combatants were handed out. Yet, many soldiers did not come to appreciate this gratitude – either because they did not return home with their army or because they had become estranged from society [Bessel, 1988, 1993]. In addition, a return to civilian life also seemed unappealing because of its lower prestige:

> "*The man in uniform was a representative of the great national cause on which his self-esteem and recognition within society was based. As soon as he has to put back on civilian clothes, he becomes an unknown soldier of the industrial army*"
> Ernst Simmel, quoted in Ulrich and Ziemann [1997, p.13]

For a number of veterans, the alienation from post-war society resulted in a desire to somehow continue the war. At the very beginning of the Republic, this desire could be accommodated since separatist movements in Germany's eastern provinces and especially the communist uprisings led to the foundation of numerous home-guards and *Freikorps* paramilitary. These were a popular opportunity for nationalist soldiers and militarist youths to organise themselves [Diehl, 1993]. The excitement about continuing the war among volunteers is exemplified by the quote below:

> "*[I] never want to return home. For my whole life, I would like to walk these country roads, search the sky, measure the world in grid squares*

and divisional sections and guess the time of the day from the strength of the artillery fire."
Friedrich Sieburg, quoted in Ulrich and Ziemann [1997, p.54]

The founding of these volunteer units and their fight against communists and separatists marked the beginning of veterans' politicisation. Despite being strongly reactionary and anti-democratic, they were useful for the democratic state in order to maintain its power and territorial integrity. For instance, in January 1919, when radical left Spartacists tried to stage a coup, the government had to call for paramilitary *Freikorps* units in order to ward off the rebellion. This *unholy alliance*, however, came to an abrupt end with the signing of the Versailles Treaty in June 1919. Not only was the extreme right infuriated by the reparations and territorial losses, but the reduction of the German army to 100,000 men and the forced dissolution of all paramilitary units also ended the military career of numerous young officers and dreams of volunteers for continuing life as a soldier after WWI. The resulting economic shock was particularly hard for those of the middle-class without alternative career options [Diehl, 1993]. A former Free Corps member depicts the disappointment within the units very well:

"*Everything was thus ready to take up the Great War anew. The morale of the troops was glowing. (...) Then one day from Königsberg came the report that the [politicians] considered the entire undertaking unfeasible. (...) Cold fury mixed with despair gripped officers and men of all the Free Corps. Once again, as at the end of the previous year [the signing of the armistice], they had been confronted with betrayal.*"
Friedrich Wilhelm von Oertzen, quoted in Waite [1952, p.143]

The Versailles Treaty thus led to a further radicalisation and some volunteer units now openly turned against the state. It was therefore no surprise that in 1920 Freikorps units tried to stage an – unsuccessful – coup themselves and were involved in the murder of several democratic politicians.

1.B Further results

1.B.1 Further heterogeneity across social groups

High esteem for the military could not greatly affect veteran inflow because of universal conscription but might have a played a crucial role after WWI. Accepting the severe cuts in army size demanded in the Versailles Treaty could have, however, turned the social democrats into an enemy especially in the pro-military parts of society. I proxy pre-WWI militarism using two variables provided in the Prussian version of the 1910 census digitised by Galloway [2007]: *members of the military per cap.* and *members of the military below 17 per cap.*. While the first one measures general participation in the military, the second one focusses particularly on militarism among the young. In the regressions displayed in table 1.17 I investigate whether the treatment effect was different in areas with an above median value of the interaction term. Given the data source, this analysis can only be carried out for the state of Prussia which accounts for more than 50% of my sample. The results do not lend support to an important role of pre-WWI militarism. None of the interaction terms are significant and the coefficients in (1) and (4) do not strongly change in the other specifications. The interaction terms in (2) and (5) have the opposite predicted signs, those of (3) and (6) both have a positive coefficient.

Another dimension I can explore is the age structure of the WWI eligible population. This analysis addresses the fact that the major share of men exposed to WWI were in the *impressionable years* of 18 and 25. Psychological research has shown that experiences during these years are crucial for a human's development of beliefs and attitudes [Krosnick and Alwin, 1989; Giuliano and Spilimbergo, 2014]. This could be a possible explanation for the persistent change in voting behaviour after WWI. I therefore interact veteran inflow with the population share of WWI eligible men in their formative years and those older than 25. As mentioned in section 1.4.1, data on precise cohort sizes is only available for the state of Prussia. Given this drawback, the findings are very informative and reveal that the share of eligible men during their impressionable years played no role in the veteran effect. Following specification (3), precincts below the median share of men between 25 and 49 show virtually no treatment effect on rightwing votes. If anything, having an above median share of draftable youths decreases the treatment effect marginally and not significantly by 0.242. The veteran effect on socialist votes is left completely unaffected by the age structure of the war eligible population.

Table 1.17: Veteran inflow and pre-WWI militarism (Prussia only)

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.985***	1.691**	1.528**	−1.842***	−2.155***	−2.055***
	(0.542)	(0.831)	(0.771)	(0.352)	(0.498)	(0.436)
% in military 1910>Median		0.037			−0.078	
		(0.153)			(0.093)	
Vet.×% in mil. 1910>Med.		0.012			0.564	
		(1.107)			(0.686)	
% under 17 in mil. 1910>Median			−0.094			−0.045
			(0.136)			(0.085)
Vet.×% u.17 mil. 1910>Med.			0.767			0.365
			(0.982)			(0.620)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	144	144	144	144	144	144
Observations	2,448	2,448	2,448	2,448	2,448	2,448
R^2	0.833	0.834	0.833	0.922	0.922	0.922

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

1.B.2 Transmission *to* veterans through military authority

Apart from comrades, also single individuals in elevated positions such as officers could play an important role in socialisation and the spread of political attitudes. The German officer corps was predominantly recruited from the upper middle-class and aristocracy and therefore naturally hostile towards communism. Social ties developed during wartime are important because they might last much longer than the actual army service and would not be captured by the analysis of veteran associations in section 1.6.4. My investigation of the transmission mechanism continues by looking at heterogeneous effects in areas with a comparatively larger share of high-rank military people and where it was more likely to remain under the influence of former superiors.

For testing the influence of high-rank military, I digitized data from the German military census of 1906. This gives me province-level information on the amount of sergeants (*Unteroffiziere*) and one-year volunteers (*Einjährig-Freiwillige*). While the elevated rank of the first group is straightforward, the second group is important because the German army used them as a backup-group during WWI to replace killed sergeants and officers [Diehl, 1975; Nash, 1977].[29] To obtain my interaction variables I divide each of the two groups by the amount of total military people in the respective provinces. This gives me a probability that veterans were still exposed to their former superiors. The reliability of this proxy is corroborated by the closeness of the military census to the outbreak of WWI and the high persistence in Germany's recruitment patterns for the higher ranks [Brentano and Kuczynski, 1900; Demeter, 1965]. A major drawback is that the data is at the province level and both variables have only 32 different values each. I therefore evaluate their impact on the baseline effects through a linear measure rather than a median split.

The results in table 1.19 highlight that the presence of former officers and sergeants only increase veterans' effect on the right. Looking at the marginal effect plot for specifications (2) and (3), for instance, shows that the treatment effect on socialist votes was significantly negative over almost the entire support of sergeants per soldiers in 1906. The same pattern also holds for one-year volunteers per soldiers in 1906. For the right-wing, however, the veteran effect is only significant in the upper half of the distribution. The amount of higher rank military people therefore cannot explain the transition of votes from left to right depending on the share of

[29] One-year volunteers were only doing two years of service rather than the usual minimum requirement of two but had to provide their own equipment and was thus a popular choice among young men of the wealthy middle class. In peacetime, one-year volunteers often became reserve sergeants and officers associated with a slightly lower social status than their professional military counterparts.

Table 1.18: Veteran inflow and age of WWI-eligible (Prussia only)

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.985***	2.152***	0.397	−1.842***	−1.722***	−1.707*
	(0.542)	(0.718)	(1.322)	(0.352)	(0.514)	(0.882)
% eligible (young) 1910>Median		0.138	0.086		−0.003	−0.006
		(0.119)	(0.107)		(0.086)	(0.084)
Vet.×% eligible (young)>Med.		−1.547*	−0.188		0.276	−0.011
		(0.862)	(0.156)		(0.614)	(0.106)
% eligible (old) 1910>Median			−1.192			0.293
			(0.767)			(0.603)
Vet.×% eligible (old)>Med.			1.662			0.033
			(1.167)			(0.769)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	144	144	144	144	144	144
Observations	2,448	2,448	2,448	2,448	2,448	2,448
R^2	0.833	0.836	0.837	0.922	0.923	0.923

Notes: identical to table 1.17

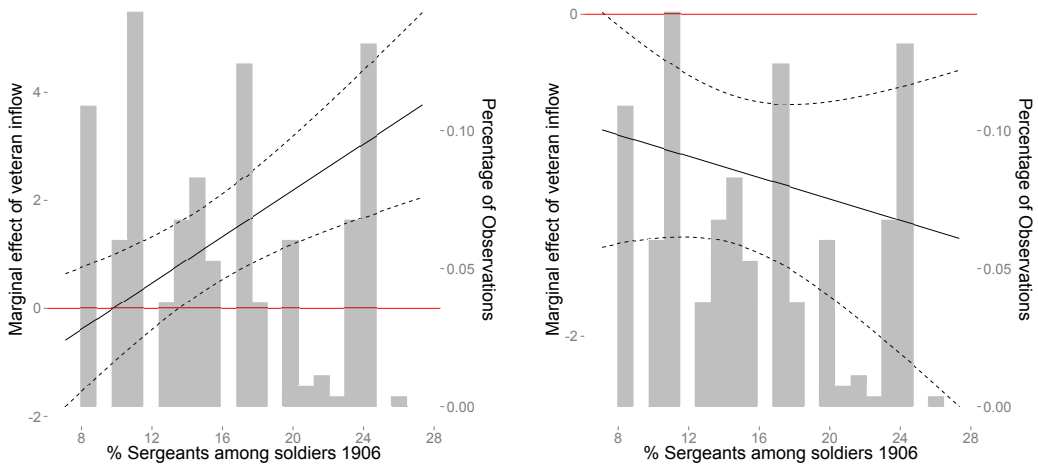


Figure 1.12: Marginal effect of veterans depending on main recruiting areas of sergeants

Table 1.19: Veteran inflow and military rank

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.080**	−2.115*	−2.237	−0.962***	−0.482	−1.578**
	(0.467)	(1.201)	(1.657)	(0.275)	(0.721)	(0.702)
% Sergeants 1906		−0.023**			0.006	
		(0.010)			(0.006)	
Vet.×% Sergeants 1906		0.215***			−0.033	
		(0.074)			(0.045)	
% 1-year volunteers 1906			−0.265**			−0.067
			(0.113)			(0.050)
Vet.×% 1-year vol. 1906			1.643**			0.318
			(0.767)			(0.313)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.784	0.790	0.786	0.910	0.911	0.911

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

Table 1.20: Veteran inflow and turnout 1920/1924

	Rightwing			Socialist		
	(1)	(2)	(3)	(4)	(5)	(6)
Veterans per cap.	1.086**	0.057	−0.452	−0.967***	−0.503	−0.571
	(0.467)	(0.803)	(0.796)	(0.277)	(0.371)	(0.369)
Turnout 1920>Median		−0.165			0.126*	
		(0.142)			(0.072)	
Vet.×Turnout 1920>Med.		1.673			−0.765	
		(1.023)			(0.511)	
Turnout 1924>Median			−0.345**			0.103
			(0.140)			(0.074)
Vet.×Turnout 1924>Med.			2.675***			−0.663
			(0.994)			(0.526)
Precinct FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Precincts	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.785	0.788	0.787	0.910	0.911	0.911

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

veterans. One way to rationalise the findings for the right-wing would be that the conservative DNVP was hiring predominantly former officers as leaders of their local party chapters [Liebe, 1956].

1.B.3 Transmission in high-turnout elections

This section investigates whether a specific electoral setup was most helpful for veterans to shift votes from the socialists to the extreme right. In order to do this, I look at turnout in the 1920 and May 1924 elections during which the switch of votes seems to have occurred. High turnout proxies for politicisation of a specific election and in particular the activation of the a-political part of the population. If information was passed on during elections, one would expect the treatment effect to be strongest in areas which had a higher-than-usual turnout. A transmission to uninformed voters would yield the same results. To explore the role of turnout and political mobilisation, I look at areas with an above median turnout in the 1920 and May 1924 elections. To avoid picking up other variables associated with high turnout such as civic capital, I also control for above median turnout in the last pre-

WWI in 1912 interacted with election fixed effects. The effect is therefore identified off precincts who became comparatively more or less politically active after the war. In addition, I also use values predicted from predetermined covariates rather than the actual one. The results in table 3.9 columns (2) and (5) show that the veteran effect was stronger in precincts with high turnout in 1920. These effects are, however, not statistically significant at the 10% level are larger in magnitude for the right-wing than for the socialist party. Turnout in May 1924, on the other hand, has strong predictive power for the positive effect on right-wing parties but not socialist ones. Taken together, there is weak evidence that mobilisation or the share of uninformed voters plays a role in shifting votes from left to right in areas with higher war participation.

1.C Tables

Table 1.21: Differences-in-Differences estimates with dummy treatment

	Rightwing				Anti-semitic	Conser-vative	Right-Liberal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Veterans p.c.>Median	0.021	0.023*	0.015	0.022	−0.002	0.030*	−0.005
	(0.013)	(0.013)	(0.018)	(0.016)	(0.008)	(0.016)	(0.016)
Precinct FE	N	Y	Y	Y	Y	Y	Y
Election FE	N	N	Y	Y	Y	Y	Y
Controls	N	N	N	Y	Y	Y	Y
Precincts	266	266	266	266	266	266	266
Observations	4,522	4,522	4,522	4,522	4,522	4,522	4,522
R^2	0.002	0.640	0.740	0.783	0.872	0.732	0.522

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

Table 1.22: LIML estimates

	Rightwing	Antisemitic	Conservative	Right-Liberal
	(1)	(2)	(3)	(4)
Veterans p.c.	1.963***	−0.178	2.805***	−0.665
	(0.589)	(0.35)	(0.721)	(0.691)
Precinct FE	Y	Y	Y	Y
Election FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Precincts	266	266	266	266
Observations	4522	4522	4522	4522

Notes: Heteroscedastic-robust standard errors in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Working in manufacturing 1882; Log(population) 1910; % Protestants 1910; % Catholics 1910; Infant mortality 1912 (all interacted with Election FE); ΔMale Migration$_{1910-1919}$; % Male cohort 1869/1901 (1910); % New male voters post-WWI (all interacted with post-WWI dummy)

1.D Data

1.D.1 Further details on the estimation of German WWI veterans

Additional formulae used for calculating the estimate of German WWI veterans:

$$\begin{aligned} SoldiersHome_{1917} = Soldiers_{1913} + \sum_{t=1914}^{1917} SoldiersJoin_t - \sum_{t=1914}^{1917} SoldiersDead_t \\ - \sum_{t=1914}^{1917} SoldiersQuit_t - SoldiersFront_{1917} \end{aligned} \tag{1.6}$$

$$\begin{aligned} Male\ PopGrowth_{1917-1919} =& Male\ Pop_{1919} - Male\ Pop_{1917} \\ =& Male\ Births_{1917-1919} - Male\ CivilDeaths_{1917-1919} \\ &+ Male\ Migration_{1917-1919} + SoldiersFront_{1917} \\ &- SoldiersDead_{1917-1919} \\ \\ Female\ PopGrowth_{1917-1919} =& Female\ Pop_{1919} - Female\ Pop_{1917} \\ =& Female\ Births_{1917-1919} - Female\ Deaths_{1917-1919} \\ &+ Female\ Migration_{1917-1919} \end{aligned} \tag{1.7}$$

$$\begin{aligned} MissingMen_{1917-1919} =& Male\ PopGrowth_{1917-1919} - Female\ PopGrowth_{1917-1919} \\ =& SoldiersFront_{1917} - SoldiersDead_{1917-1919} \\ &+ (Male\ Births_{1917-1919} - Female\ Births_{1917-1919}) \\ &- (Male\ CivilDeaths_{1917-1919} - Female\ Deaths_{1917-1919}) \\ &+ \big(Male\ Migration_{1917-1919} - Female\ Migration_{1917-1919}\big) \end{aligned} \tag{1.8}$$

In the style of equation 1.9, one can then construct gender-specific numbers for births and deaths. For the latter, however, one needs to recall that one needs to account for the difference in deaths between women and male non-combatants and split the district aggregates on male deaths into soldiers and non-soldiers. I infer dead soldiers by comparing the sudden increase in the ratio of dead men to women

from 1913 to each of the war years:

$$Female\ Births_i \approx Births_i * \frac{Female\ Births_d}{Births_d} \tag{1.9}$$

$$\begin{aligned} DeadSoldiers_{dt} &\approx \left(\frac{Male\ Deaths_{dt}}{Deaths_{dt}} - \frac{Male\ Deaths_{d1913}}{Deaths_{d1913}} \right) * Male\ Deaths_{dt} \\ DeadSoldiers_{it} &\approx Deaths_{it} * \frac{DeadSoldiers_{dt}}{Deaths_{dt}} \\ Male\ CivilDeaths_{it} &\approx Male\ Deaths_{it} - DeadSoldiers_{it} \end{aligned} \tag{1.10}$$

Even though each of the components is not readily available from the statistical publications, it can also be approximated. Data on births and deaths can be constructed for each precinct but not differentiated by gender. This information can, however, be retrieved for Germany's districts, which are the next higher administrative level. Numbers of female births, for instance, can therefore be constructed as follows: The approximation's validity rests on the assumption that absent deaths from battle, men and women would have experienced the same changes in mortality between 1914 and 1918.

$$\begin{aligned} &SoldiersHome_{1917} + Missing\ men_{1917-1919} \\ =&Soldiers_{1913} + \sum_{t=1914}^{1917} SoldiersJoin_t - \sum_{t=1914}^{1918} SoldiersDead_t - \sum_{t=1914}^{1917} SoldiersQuit_t + u \end{aligned} \tag{1.11}$$

1.D.2 Measurement error in the veteran estimate

The veteran estimate is not perfectly measured. While this could result in simple attenuation bias, it could also systematically distort the estimates if it is also correlated with the outcome, political attitudes in this case. For our veteran estimate, the remaining measurement error can be obtained by taking the difference between $\widetilde{Veterans}$ and equation 1.1:

$$\begin{aligned} &\widetilde{Veterans} - Veterans \\ =& - (SoldiersJoin_{1918} + SoldiersQuit_{1917}) \\ &+ \left(Male\ Migration_{1917-1919} - Female\ Migration_{1917-1919}\right) \end{aligned} \tag{1.12}$$

While $SoldiersJoin_{1918}$ and $SoldiersQuit_{1917}$ cannot be estimated, they must be *dis*proportional to $\widetilde{Veterans}$ *and* correlated with an omitted variable in order to pose a threat to the estimates' validity. Gender-specific migration between 1917 and 1919 can also not be estimated since the 1917 male totals are incomplete as highlighted above. However, I can construct a measure of gender-specific migration between *1910* and 1919 which should be a reasonable proxy for that between 1917 and 1919. Even though this does not allow directly subtracting gender-specific migration from the treatment variable, it can still be included as a control in order to purge the respective endogenous part from $\widetilde{Veterans}$ and reduce the chances of biased estimates.

1.D.3 Robustness and distribution of veteran measure

In order to give at least a rough idea how far the proposed variable is away from what it is supposed to measure, I compare my measure to potential alternatives and official aggregate figures on war participants (see table 1.23). von Altrock [1922] provides aggregates of war participants for the four German armies as well as the corresponding estimates of dead soldiers. As can be seen from the first panel in table 1.23, the difference between von Altrock's numbers (vA) gives an estimate of almost 11 million war participants. As panel two shows, using official numbers of dead soldiers published by the Imperial Department of Health (*Reichsgesundheitsamt*, RGA) does not change these totals as well as their distribution across armies considerably.

Panel three reports figures on recipients of veteran benefits in 1929 published by the German Statistical Office in 1933 which have been used in few recent studies as a measure of *WWI participation* or *war veteran density* [see e.g. Adena et al., 2013; Voigtländer and Voth, 2014, respectively]. This measure of veterans appears already somewhat problematic since it explicitly includes surviving dependants which did not have any war experience. A comparison of the aggregates with the official figures from panel one and two additionally calls into question the numerical accuracy of this proxy for veterans: not only are the aggregates about 0.5% of the official figures in panel one and two but also the distribution across armies differs strongly from that of all other estimates. The veteran estimate presented here could thus provide a good alternative to existing measures of WWI participation.[30]

[29] Numbers on Prussia include all remaining German states.

[30] In fact, the correlation between recipients of veterans benefits in 1929 and the veteran estimates – normalised by the 1910 population and aggregated to the precinct level – is −0.08.

Table 1.23: Comparison of veteran estimates with official aggregates

		Prussia	Bavaria	Saxony	Wurtemb.	Total
(1)	Participating soldiers (vA)	9,957,000	1,360,000	913,400	479,000	12,709,400
(2)	Dead soldiers (vA)	1,417,449	190,015	126,180	74,911	1,808,555
(1)-(2)	Veterans (vA)	8,539,551	1,169,985	787,220	404,089	10,900,845
	as % of total	78.34%	10.73%	7.22%	3.71%	100.00%
(1)	Participating soldiers (vA)	9,957,000	1,360,000	913,400	479,000	12,709,400
(3)	Dead soldiers (RGA)	1,306,484	167,840	121,524	73,339	1,669,187
(1)-(3)	Veterans (vA/RGA)	8,650,516	1,192,160	791,876	405,661	11,040,213
	as % of total	78.35%	10.80%	7.17%	3.67%	100.00%
(4)	Recipients of veteran benefits 1929	42,726	4,287	5,545	5,211	57,769
	as % of total	73.96%	7.42%	9.60%	9.02%	100.00%
(5)	Soldiers 1917	2,156,282	365,423	219,574	129,239	2,870,518
(6)	*Missing men* 1917	4,307,110	546,482	446,300	191,882	5,491,774
(7)	War disabled 1916	1,216,894	87,498	34,517	35,765	1,374,674
(5)+(6)+(7)	Veterans (this study)	7,680,286	999,403	700,391	356,886	9,736,966
	as % of total	78.88%	10.26%	7.19%	3.67%	100.00%

Chapter 2

Competence vs. Loyalty: Political Survival and Electoral Fraud in Russia's Regions 2000–2012

> I think you need to pay attention to those areas where our people have denied United Russia serious trust. Not because it is a tragedy, but because it is a signal for the authorities.
>
> Dimitri Medvedev, 2011

2.1 Introduction

One common explanation for the poor economic performance of autocracies is the failure to hold leaders accountable for bad policy. While democracies use free and fair elections in order to punish or reward politicians, the voting process under dictatorship is often manipulated and seen as a meaningless political ritual. Recent literature has called this view into question arguing that authoritarian elections are actually used by the ruling circle to hold officials accountable. Similar to the role of education in the labour market, elections may be used by subordinates to cast signals about their loyalty or competence to their superiors [Gandhi and Lust-Okar, 2009]. For instance, high results may be rewarded by directing additional resources under the control of the respective official or by advancing his party career chances through promotion [Martinez-Bravo, 2014]. With legal barriers largely absent, this creates strong incentives for public officials to artificially change results and engage in electoral fraud. The extent to which such concerns are driving ballot rigging in autocracies has, however, remained a largely understudied topic since institutional setups under dictatorship tend to be rigid, and reliable data on the intensity of fraud is scarce.

This paper uses detailed data on Federal elections in Russia and a novel tool for detecting suspicious results to study the effect of a radical policy change in accountability to the central government. After the 2004 Beslan hostage crisis, which exposed severe inefficiencies in the local administration, president Putin signed a law which abolished governor elections in the country's 89 regions. From December 2004 onwards, regional leaders thus had to be appointed by the president in order to stay in office after their term but could also be dismissed without any legal barriers [Hill, 2012]. While this gave the central government an important *stick* to punish notoriously under-performing and corrupt leaders, it also severely altered their motivation to please their superiors by delivering the *right* results in federal elections which they can organise in their territory at large discretion. The reality of this threat can be seen from the low election results of United Russia in December

2011 which resulted in the dismissal or *voluntary resignation* of 6 governors over the following 4 months [Chaykovskaya, 2011; Moraski and Reisinger, 2013].

The legal change, however, also started a transition period from 2005 onwards during which *handpicked* and *elected* governors coexisted and organised 4 national ballots. This allows me to study the differential effect of abolishing governor elections on two types of leaders: one whose loyalty is assured but uncertainty about competence may still exist, and another one where neither of these is known [see also Egorov and Sonin, 2011]. In my conceptual framework I argue that, for given levels competence, appointments may induce *lower* levels of election fraud since officials do not need to use rigging as a means to signal their loyalty to the centre. In a recent paper, Martinez-Bravo [2014] studied the effect of local officials on election fraud in Indonesia where a democracy inherits a set of potentially not trustworthy officials from an autocracy. The author demonstrates empirically that appointed village heads have a higher need to convince their supervisors of their suitability than elected ones. The mechanism I propose is dealing with the opposite problem faced by an authoritarian government: until all positions are filled with loyal party cadres, officials chosen under the previous regime are particularly suspicious of disloyalty and may therefore try to over-compensate. Moreover, my model claims that, when facing a bad economic performance, also leaders selected by the central government may engage in fraud to send a signal about their competence and keep their position.

In order to empirically test my predictions and study the effect of this policy change on election fraud, I use unique highly disaggregated data at the voting station level for all 7 national elections – parliamentary and presidential – held in the Russian Federation from 2000 to 2012. For each region I calculate the percentage of votes cast in districts with highly suspicious results. A district's result is deemed suspicious if the turnout and vote share regression coefficient (TVSC) takes values greater or equal to one. The TVSC has been widely used in the study of electoral fraud in the Russian context and is appropriate for detecting ballot stuffing and other turnout-inflating types of manipulation [Myagkov et al., 2009]. Legitimate doubts about its reliability and shortcomings are met by a number of tests. First, I show that my measure is significantly correlated with reported incidents of fraud during the 2011 and 2012 elections and decreases with the introduction of electronic vote scanners across Russian regions. Second, I benchmark the TVSC against other indicators using first- and second digit distributions of incumbent vote and valid ballot totals which do not seem to have similar power in detecting rigging in Russian elections.

I quantify the treatment effect of governor selection using a differences-in-differences estimation which controls for region-specific and election-specific determinants of rigging and the time-varying effect of pre-2000 democratic institutions. Competence of a governor is measured by an index of regions' changes in unemployment and GDP per capita growth over the last legislature of either president of parliament. Both variables are then aggregated to a single index of economic performance over the last 4 years. The regression specification includes this index as well as its interaction with the treatment in order to study the differential response to economic fluctuations across both handpicked and elected governors. The baseline results show that regions with a handpicked governor obtain on average more than 10% less votes from districts with highly suspicious results. Second, the negative effect of a handpicked governor on fraud is even stronger during times of good economic development. Only under extremely bad conditions does the negative marginal effect of a handpicked governor disappear completely. I use various checks to address concerns about the endogeneity of governor replacement such as placebo tests for different time periods and outcome variables. The coefficients are robust to the inclusion of region-specific time trends and election fixed effects for each Federal district as well as different definitions of economic performance. Furthermore, placebo experiments show no effect if treatment is moved one or two elections forward and no response on unrelated election outcomes such as votes for other parties.

Several empirical studies have investigated the fate of Russia's governors after 2005 but mainly focussed on the selection mechanism [Reuter and Robertson, 2012; Moraski and Reisinger, 2013] or outcomes other than election fraud [Moraski and Reisinger, 2009; Rochlitz, 2013][1]. Kalinin and Mebane [2011] are studying how federal transfers affected election fraud in the 1990s and 2000s using a number based approach and aggregated data. I contribute to this literature by linking the incentive structures of governors after December 2004 to changes in election fraud over time. The TVSC method for detecting fraud used in this study has furthermore been used in various papers on the Russian context [Filippov and Ordeshook, 1997; Myagkov et al., 2009; Lukinova et al., 2011; Enikolopov et al., 2013], yet so far without a systematic application over all regions and several elections. My paper adds to this work by providing further evidence on the TVSC's ability to capture fraud and by making it usable for cross-regional comparison over time. Studies of fraud have been carried out for other countries using different indicators of ballot rigging. Examples of these are Ziblatt [2009] for Imperial Germany, Ichino and Schündeln [2012] for

[1] See Rochlitz [2013] for further references on this topic.

Ghana, and Cantu and Saiegh [2011] for Argentina. With the exception of Ziblatt [2009], my work is one of few papers studying the evolution of electoral fraud over time. In terms of the studied mechanism, the closest piece of work is the previously mentioned study by Martinez-Bravo [2014] who looks at the topic from the side of a democracy and proxies election fraud through high votes for the dominating party instead of a direct indicator of rigging.

The paper starts with description of important institutional details and the conceptual framework used to analyse the changing incentive structures of election fraud in Russia. After briefly describing the data used, I present the main fraud indicator with a special emphasis on its reliability and comparison with other potential alternatives. Next, I outline the differences-in-differences approach used in the empirical analysis and discuss the validity of its assumptions in the studied context. The baseline results are presented in the subsequent section which is followed by robustness checks and placebo tests. The last section concludes.

2.2 Institutional and theoretical background

2.2.1 Relevant aspects of Russia's political system

As in any presidential system, the president is paramount in Russia's constitution. He appoints the government as well as the members of the constitutional court and the supreme court. In addition, he has the right to veto laws passed by the legislative and can also initiate laws himself [Chaisty, 2012]. Moreover, he can dissolve the State Duma under extreme circumstances, rule by decrees without consent of the parliament and call for an emergency state which gives him the power to even ignore civil freedoms. The president is chosen in national elections and the length of term has recently been extended from four to six years from 2012 onwards [Sakwa, 2008]. The legislature of the Russian Federation consists of two chambers - the State Duma and the Federal Council. The main task of the Federal Council is to represent the Russian regions. It has 178 members, with two representatives for each administrative unit. Representatives are appointed by the regional executive and the regional parliament and can be withdrawn by these institutions. The State Duma's 450 members, in turn, are elected through a national ballot. Until 2007, half of the deputies were elected by majority in single-member districts and the other half proportionally through party lists. This system was, however, abandoned in favour of a purely proportional representation. Like the president, both chambers have the power to initiate new laws [Sakwa, 2008].

As in most federal states, each subdivision has its own legislative and exec-

utive. Each Russian region is headed by a governor.[2] The members of the regional assemblies are chosen in local elections. Similar to the national level, these assemblies are dominated by the executive making governors the most important political institution in the regions. From 1996 until December 2004 they were chosen in local elections. However, in 2004 the constitution was changed in favour of a direct appointment of governors by the president [Slider, 2012]. This drastic constitutional change was decided in the aftermath of the Beslan Massacre. On the 1st of September 2004, a multinational terror squad took over 1000 hostages in a school in Beslan, a town in the Republic North Ossetia-Alania close to Chechnya. When security forces attempted to free the hostages, more than 300 people were killed.

This national tragedy demonstrated the increased power of Chechen insurgents and their allies but also showed the lack of coordination between federal and regional authorities.[3] Very soon after the attacks, president Vladimir Putin initiated a law which re-introduced the appointment of governors. The draft passed both chambers of the Federal Assembly and came into effect in December 2004. What may seem puzzling is that the new law was accepted by both the population and the governors without any major opposition. Goode [2007], who analysed the parliamentary debates in late 2004, concludes that a combination of rally-around-the-flag effects and an appeal to Soviet legacies made it impossible to reject the new law. Additionally, being independent of the local electorate and depending only on the central executive was in the interest of many governors.

2.2.2 Conceptual framework

Election fraud in this paper is perceived as the result of an interaction between the president P and a governor. The latter can be of two types, either elected (G_E) or handpicked (G_H). Governors organise national elections in which P runs for office and can exert fraud in order to influence the results in P's favour. P's stay in power is assumed not to depend on the election outcome but he cares about the governor's competence C and loyalty L.[4] Competence is appreciated by P for reasons uncorrelated with election outcomes such as international reputation or development assistance. In line with Egorov and Sonin [2011] I assume, however, that L matters far more than C for P. While L is known by P, he uses election results to infer the

[2] Many regions use different titles such as *President*, *Head of the Republic*, or *Head of the administration*. For the sake of simplicity, I refer to all these in this paper simply as *governors*.

[3] This was apparent even though a lot of information about the Beslan hostage crisis was actually withheld from the Russian public [Haraszti, 2004].

[4] One could imagine, for instance, that centralised state propaganda ensures high levels of political support for the incumbent.

competence $\hat{C}$. The interaction between P and G can be separated into two phases. In the first one, G_E is elected by popular vote and therefore L is unknown. For simplicity, I assume that L_E is 0. Since fraud is a costly action and P cannot hold G_E accountable, there is no fraud in this phase.

In the second phase, P is now equipped with the power to remove an elected governor G_E. P's decision is based on whether it is more beneficial to select a new governor G_H from his cadres who is loyal with probability 1 but of uncertain competence. This decision is based on evaluating the last election results. For simplification, I make the strong assumption that P is not well informed about voters' decision making process. In fact, he interprets his vote share as a linearly increasing function of $\hat{C}$. Governors can, however, exert fraud in order to artificially increase the result and influence P's conclusion about $\hat{C}$. P's decision about keeping or dismissing governors depends on how their loyalty and competence compares with that of a handpicked new governor. If $\hat{C}$ is below of the minimum required competence $\hat{C}^{min}$, he will be removed. As a result, minimum competence levels will vary by type such that $\hat{C}_E^{min} > \hat{C}_S^{min}$. Hence, the only way for G_E to compensate his lack of loyalty and stay in office is through artificially increasing $\hat{C}$ by means of electoral fraud which motivates the first hypothesis.

Hypothesis 1: *For given levels of competence, handpicked governors will engage in less rigging than elected ones.*

The second difference between the two types of governors is the way they are evaluated by the electorate. Unlike his elected counterpart, G_H knows that his own economic performance has an influence on votes for P. The reason for this is, that voters may want to punish or reward P for his choice of governor.[5] Assuming that economic performance is stochastic and independent of C to some degree, it follows that if a handpicked governor does a bad job, he is more likely to be fired. This is because P will infer a lower quality, unless G_H compensates this through rigging. Vice versa, a good performance allows him to reduce the amount of fraud even further. The second hypothesis therefore goes as follows:

Hypothesis 2: *Handpicked governors will engage in less rigging if they performed well during the last election period.*

[5] That such behaviour is actually at work is well exemplified in the study by Szakonyi [2012] who investigates the political reactions to the wildfires in Russia in 2010.

Having presented the main hypotheses to be tested in this paper, I now proceed to discuss my measure of election fraud used in the empirical analysis.

2.3 Measuring election fraud

2.3.1 The turnout/vote share correlation

The turnout/vote share indicator was first applied by Sobyanin for the 1993 constitutional referendum and is probably the most widely used tool for detecting election rigging in Russia. It is most suited for turnout-inflating cases of fraud and relies on the assumption that within a given entity and absent manipulation there should be no correlation between how many people vote and their choice across lower-tier areas. Figure 2.1 illustrates this with a brief example similar to Myagkov et al. [2009]: there are 24 voting stations in an area with a given homogenous support of 75% for candidate i. Half of the stations are in high-turnout areas where 60% of the electorate casts their ballot, whereas the remaining ones only have a turnout of 40%. Absent fraud, a 1% higher turnout T is associated with an increase of 0.75% in votes for i out of the total electorate, V/E. A simple OLS regression thus yields a turnout/vote-share coefficient (henceforth $TVSC$) equal to the average support of the candidate. This relation, however, would not hold in the case of ballot stuffing or other turnout inflating methods of manipulating the outcome as can be seen from the right panel in figure 2.1. In this scenario eight of the formerly low-turnout stations see their turnout artificially increased with all additional votes going to candidate i. The TVSC thus changes from 0.75 to 1.07 which cannot be equal to i's natural support in that area anymore.[6]

Following Myagkov et al. [2009], one can distinguish between the cases when 1) the TVSC exceeds the candidates vote share in the respective area but is smaller than one and 2) the TVSC is bigger or equal to one. In the first scenario, the conclusion is ambiguous and will only be a safe detector if one can rule out that the favoured candidate – absent fraud – would have fared particularly well in lower-tier areas of high turnout – a premise which is quite difficult to check. TVSC ≥ 1 appears to be a stronger indicator of manipulative turnout inflation, but it is also prone to fallacies as depicted in figure 2.2. One may think of a region with uniform support of 75% for candidate i across its three districts with 4, 12, and 8 voting stations respectively. The voting stations, however, are not homogeneous since districts differ

[6] One could imagine a scenario in which fraud is conducted in such a way that turnout and vote share are identical in each voting station. In this case there would be no variation and a TVSC could not be calculated. While this is theoretically possible, it is very difficult to implement in reality. I did not encounter such a case during the construction of my fraud data.

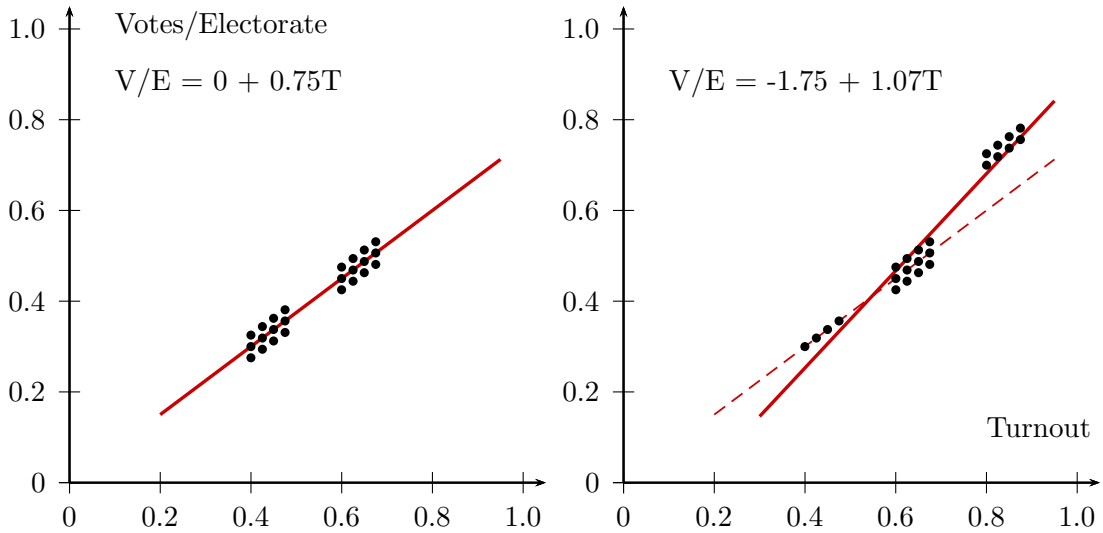


Figure 2.1: Example of TVSC absent fraud (left) and present (right)

substantially in their average turnout level. A regression of V/E on T across the whole region will thus suffer from aggregation bias and yield a TVSC larger than one even though fraud did not take place. While this error cannot be ruled out entirely, it can be mitigated by using highly disaggregated data and calculating the TVSC for reasonably homogeneous areas. Enikolopov et al. [2013], for instance, have shown that the random allocation of election observers across voting stations in the city of Moscow during the 2011 Duma election significantly decreased the TVSC calculated for the United Russia party.

Further estimates of election fraud in Russia's regions using the TVSC have been scarce so far and mostly relied on district aggregates [e.g. Myagkov et al., 2009]. In these cases the assumption of homogeneity is difficult to defend and the amount of districts/observations may be very low.[7] The availability of election results at the voting stations level since 2000 allow me to calculate TVSCs in each district of a given region [e.g. Lukinova et al., 2011] and to construct new and more robust estimates of election rigging across Russia's regions. As a new measure of regional fraud intensity, I propose the *share of votes from districts with a TVSC* ≥ 1. This indicator has the main advantage of using data from comparatively small areas such as districts but simultaneously provides a regional aggregate from this information. It is supposed to capture the intensity of rigging rather than its mere existence which has been observed in virtually every region across the country and therefore

[7] The city of Moscow, for instance, has 130 TIKs while the Nenets Autonomous Okrug has only three.

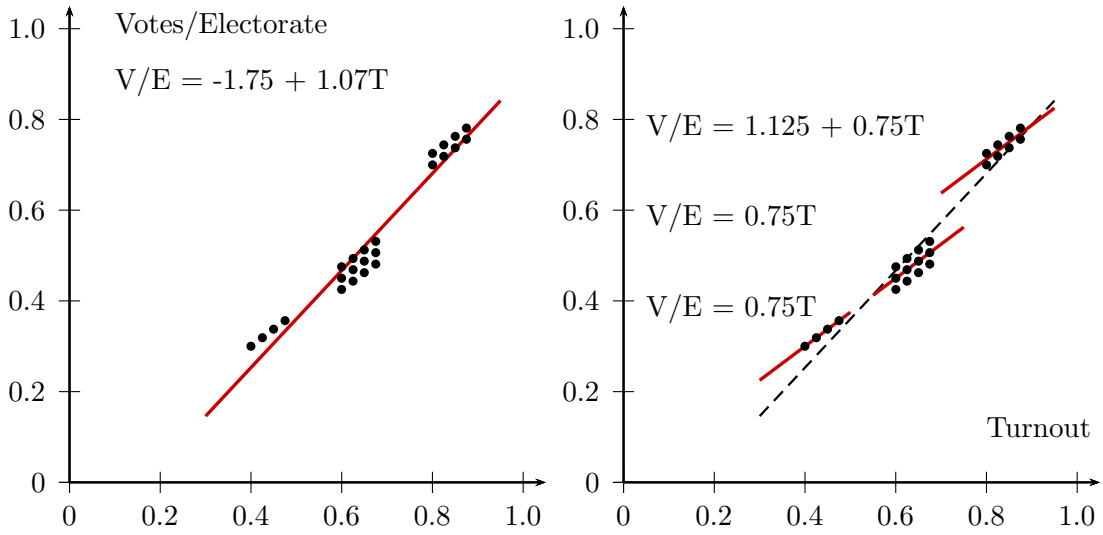


Figure 2.2: Example of biased TVSC in the case of non-homogeneous areas

does not provide much information. Another interpretation of the indicator is the percentage of votes likely to be affected by manipulation or, simply, the *suspicious vote share*.

The choice of this indicator appears arbitrary at first and one may think of several alternatives: the first option could be to deem a district's votes suspicious if the TVSC exceeds the candidate's vote share and calculate the suspicious vote share based on this rule. Alternatively, one could estimate a single region-specific TVSC from all voting stations and calculate either a dummy for whether it exceeds one or obtain the continuous difference from the candidate's actual vote share. The following section compares the proposed indicator of fraud to these alternatives in terms of reliability and provides further checks of its validity.

2.3.2 Reliability and validity checks

Before proceeding with a specific indicator of election fraud, one needs to assure that it is reliable and valid. Verifying reliability beyond anecdotal evidence is particularly difficult in the context of election rigging since officials usually try to hide their actions. I tackle this issue with fraud report data from the NGO *GOLOS* (Russian for *vote* or *voice*). This provides information on the region where election irregularities were witnessed. Subsequently, one can form a regional measure and relate it to various types of election rigging for each subdivision. Table 2.1 presents the 32 coefficients from regressing the amount of each 8 types of election irregularity

Table 2.1: Reported irregularities 2011-2012 and fraud indicators based on TVSC

Reports per 100k electorate	Effect of TVSC based on			
	vote share, with TVSC		regional aggregate, with TVSC	
	≥ 1	$\geq$ Incumbent vote	≥ 1	$\geq$ Incumbent vote
	(1)	(2)	(3)	(4)
Improper counting	0.547***	0.224	0.130	−0.028
	(0.194)	(0.214)	(0.112)	(0.118)
Exclusion of voters	0.193*	0.234	−0.042	−0.076
	(0.113)	(0.161)	(0.134)	(0.185)
Illegal campaigning	0.036	0.019	0.102	0.096
	(0.084)	(0.158)	(0.107)	(0.101)
Observers excluded	0.567**	0.453	0.176	0.070
	(0.265)	(0.333)	(0.169)	(0.200)
Faulty ballot box	0.056	−0.129	−0.039	−0.108
	(0.142)	(0.297)	(0.124)	(0.124)
Secrecy violated	0.169**	0.104	0.086	0.079
	(0.066)	(0.092)	(0.054)	(0.057)
Illegal voting	0.216	0.426	0.050	0.100
	(0.228)	(0.347)	(0.165)	(0.191)
Other violations	0.872**	0.497	0.283	0.249
	(0.398)	(0.564)	(0.319)	(0.373)
Election FE	Y	Y	Y	Y

Notes: Standard errors clustered at the region level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Population with internet access; UR members per cap.

on 4 variants of the TVSC measure presented in the preceding section. In addition to that, the regressions also control for election specific trends and the incumbent party's strength proxied by party members per capita. The latter ensures validity of the TVSC which could also be driven by a party's advantages in mobilising voters and in turn increase the likelihood of reporting irregularities out of revenge. Finally, since fraud reports are likely to be incomplete and affected by technical impediments such as the lack of internet access, I also include the percentage of households with internet access into the regressions.

The results indicate that only the regional vote share of districts with a TVSC exceeding 1 is reliable. Most notably, it is strongly correlated with reports on those irregularities associated with fraud such as *improper counting* and *exclusion of observers*. Also other violations like *exclusion of voters*, *violation of secrecy*, and *other violations* seem to be correlated with the first indicator. The measure in column 2 produces similar but far less precise estimates. Indicators 3 and 4 which assume a uniform distribution of fraud are only weakly and sometimes even negatively affecting the detection of irregularities. This makes the vote share of districts

Table 2.2: High TVSC and introduction of electronic ballot boxes, 2000-2012

	Vote share with TVSC ≥ 1			
	(1)	(2)	(3)	(4)
% Electronic ballot boxes	0.352	-0.685^{**}	-0.365^{***}	-0.366^{***}
	(0.263)	(0.296)	(0.117)	(0.115)
County FE	N	N	Y	Y
Election FE	N	Y	Y	Y
Controls	N	N	N	Y
Counties	71	71	71	71
Observations	497	497	497	497
R^2	0.003	0.105	0.762	0.762

Notes: Standard errors clustered at the region level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: log(GDP per capita)

with a TVSC exceeding 1 the prime candidate for detecting election fraud in the Russian context. The inclusion of United Russia members per capita furthermore ensures reliability and that the results are not driven by other potential correlates of the TVSC.

Another desirable property for a good detector of rigging intensity is that it co-moves with the opportunities for falsifying results. A significant decrease in such opportunities was marked by the start of electronic vote counting via optical scanners (KOIB) during the mid-2000s across Russia in about 5% of all voting stations. The introduction was staggered starting with the 2007 Duma elections which makes it unlikely to be correlated with other incentives for fraud. From official government documents I collected information on the numbers of KOIBs in each region across federal elections and calculated the percentage of voting stations equipped with such a device. Table 2.2 shows the results from regressing the vote share with TVSC$\geq$ 1 on the share of regions' voting stations equipped with a KOIB over the time period 2000 to 2012. Once election and region fixed effects are controlled for, one can see that having 1% more stations with electronic ballot boxes in a region reduces the share of votes from suspicious districts by 0.3%. The results of this test suggest again that the indicator is reliable and valid.

Admittedly, the share of suspicious votes based on TVSC$\geq$ 1 is not a perfect measure of rigging intensity but the findings above suggest that it is strongly correlated with what it is supposed to measure and that many potential concerns can be ruled out. Unfortunately, there is no way to look at fraud reports before 2011 so some warranted doubt may still remain. In the following section I briefly

present other indicators common in the analysis of rigged elections and check their explanatory strength in the case of Russia 2000 to 2012.

2.3.3 Alternative indicators

The preceding chapters have solely investigated quantitative tools for detecting turnout inflating types of rigging and the vast amount of evidence indeed points in the direction of this being the most widely used technique of manipulating the results in favour of a specific candidate [Filippov and Ordeshook, 1997; White, 2011; Enikolopov et al., 2013]. Yet, anecdotal evidence from the Russian republics – Tatarstan, Ingushetia and Dagestan in particular – suggests that election results in some areas may not only be manipulated but entirely fabricated [Myagkov et al., 2009; Lukinova et al., 2011]. In detecting this kind of fraud I follow the methodology of Beber and Scacco [2012] who rely on human preferences for specific numbers and biases in number generation. The main argument is that, under fairly generous assumptions, the final digit as well as the distance between the last and second-last digit of the vote count should follow a uniform distribution. In order to create alternative indicators of the share of suspicious votes, I adapt the methodology of Beber and Scacco to identify fraud at the district level and then aggregate this to the regional level using the share of affected votes as for the TVSC. In detail, I proceeded as follows: first, I calculated for each district the p-values of a Pearson's chi-squared test of uniform distribution of the last digit and the distance between last and second-last digits and repeated this procedure for both the reported valid votes as well as incumbent votes. In a second step, I obtained the share of votes in each region from districts where the hypothesis of uniform distribution could be rejected at a significance level of either 5 or 1%. This procedure has the advantage of being completely agnostic about the kind of bias, i.e. whether there is a bias towards fives in one district vs. eights in another, and only assumes whether votes for the incumbent or the number of valid votes/turnout were affected. The measures are furthermore also formed as ratios and therefore easily comparable to the indicators of suspicious votes presented above.

Table 2.3 repeats the analyses of table 2.1 for each of the 8 measures of fabricated votes. Interestingly, none of them has a significant positive effect on any reported election irregularity. If anything, some kinds of reports per capita are negatively affected by the share of votes from districts with numeric anomalies. Unusual digit distributions in the incumbent votes, for example, are associated with less reports on *illegal campaigning*, *faulty ballot boxes*, and *illegal voting*. Anomalies in the distance between the last two digits in the number of valid votes, on the

Table 2.3: Reported irregularities 2011-2012 and numeric anomalies

Reports per 100k electorate	Effect of vote share with numeric anomalies for							
	Valid votes				Incumbent votes			
	last digit, with $p \leq$		Δ last 2 digits, with $p \leq$		last digit, with $p \leq$		Δ last 2 digits, with $p \leq$	
	0.05	0.01	0.05	0.01	0.05	0.01	0.05	0.01
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Improper counting	−0.303	0.379	0.305	0.388	−0.373	−0.489	0.625	−0.071
	(0.377)	(1.874)	(0.642)	(0.899)	(0.373)	(1.035)	(0.937)	(0.902)
Exclusion of voters	−0.295	3.647	2.136	−0.619	−0.274	−0.296	1.217	0.526
	(0.396)	(2.999)	(1.903)	(0.622)	(0.240)	(0.873)	(0.880)	(0.921)
Illegal campaigning	0.608	0.747	1.272	0.484	−0.473*	−0.147	−0.308	−0.045
	(0.525)	(1.276)	(0.947)	(1.140)	(0.258)	(0.839)	(0.403)	(0.520)
Observers excluded	0.002	3.157	−0.438	−0.788	−0.709	−0.351	1.570	−1.486
	(0.727)	(3.529)	(0.841)	(1.045)	(0.766)	(2.321)	(1.343)	(1.267)
Faulty ballot box	1.003	4.475	0.568	−0.890*	−0.969**	−0.918	−1.157*	−0.721
	(0.748)	(4.023)	(0.905)	(0.539)	(0.429)	(0.778)	(0.643)	(0.764)
Secrecy violated	−0.152	1.250	0.042	−0.505*	−0.072	0.053	0.360	0.257
	(0.178)	(1.252)	(0.215)	(0.300)	(0.145)	(0.461)	(0.340)	(0.423)
Illegal voting	0.653	12.040	0.300	−1.161	−1.254*	−0.757	0.404	0.055
	(1.078)	(8.138)	(0.922)	(0.711)	(0.675)	(1.249)	(1.165)	(2.180)
Other violations	2.057	15.814	1.201	−1.218	−0.543	−0.492	0.439	0.610
	(1.935)	(12.250)	(1.748)	(1.446)	(1.050)	(1.837)	(1.655)	(1.943)
Election FE	Y	Y	Y	Y	Y	Y	Y	Y

Notes: Standard errors clustered at the region level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: % Population with internet access

other hand, significantly reduces also reports on *faulty ballot boxes* as well as the *violation of secrecy*. In particular, it does not seem to be correlated with *exclusion of observers*. One possible interpretation of these results is that fabricating votes may be a complement to turnout inflating methods which does apparently not require actions observable to the public and hence results in lower amounts of reported fraud. Voters' inability to detect and report entirely invented results makes it hard to verify the reliability of any fraud indicator built on numeric anomalies. The results in table 2.3 should thus also be regarded as a reminder that some fraud may still not be detected by the fraud indicator used in the remainder of this paper. Bearing this caveat in mind, I now turn towards the extent of potentially fraudulent election outcomes and its changes over time.

2.3.4 The evolution of suspicious results 2000–2012

Figure 2.3 plots for each election the share of votes with TVSC$\geq$ 1 in each Russian region. This helps understanding the variation in the main outcome variable and at the same time also shows how rigging changed at the extensive margin over the time period studied. While always present to some degree in few subdivisions, suspicious results started to take off during the March 2004 Presidential election from an average of 8 to 20%. During the 2007 Duma election it kept on rising to about 24-25% and remained roughly stable onwards. The distribution is skewed to the left resulting in median values below the corresponding means. Since the year 2003, however, also these gaps have been widening from 5 to about 10 percentage points indicating that intensity has surged disproportionately at the right tail.

Figure 2.4 zooms into the district level measures of rigging for the two elections before and after 2005. The plotted variable is a dummy whether the TVSC is exceeding unity in a given district and visualises how fraud evolved at the intensive margin across the Russian Federation. Already in 2003 suspicious districts are strongly concentrated in particular regions, most notably in the Republics of Tatarstan and Mordovia in Western Russia. In the March 2004 election suspicious votes start showing up in a number of formerly *clean* areas and additionally further regions start showing almost uniformly ballot counts with a TVSC$\geq$ 1 – Republics of Tuva in South-Central and Bashkortostan in the South-West. Other areas, particularly in the West and conflict-ridden South-Western Causcasus territory, are joining in during the next round of elections 2007/2008. This extreme concentration suggests that time and region-specific characterstics could indeed be an important driver of prolific fraudulent election outcomes since 2000.

2.4 Data

2.4.1 Elections and fraud reports

The organisation of federal elections in Russia is roughly corresponding to its administrative divisions, both horizontally and vertically. The highest authority is the Central Election Commission (CIK), a permanent body whose members are nominated by the president, the second chamber *Council of Regions* and the *Duma* parliament. The CIK's main tasks are the coordination of the 83 Regional Election Commissions (IKS) and the organisation of the Federal elections. The IKS fulfils the same role as the CIK at the regional level and coordinates the territorial election commissions (TIKs). Unlike the CIK, its members are appointed by recommendation of the Federal government. The next administrative level below the region is the district (*rayon*). Like regions, the districts can vary considerably in size and population but unlike the former there may be several TIKs within the same rayon. This is especially often the case in larger cities or former *closed towns*. Over the period studied there were almost 3,000 TIKs. The members of these are permanent delegates by the regional executive, legislative and parties. The TIKs are therefore the level of electoral administration where the long arm of the central government starts to lose its power. At the lowest level, about 95,000 precinct election commissions (UIKs) are responsible for the local organisation of all elections and, most importantly, the vote counting. Unlike the other commissions, they are only formed one month before the elections and are nominated by the electorate. It remains unclear to what extent authorities can still exert control over the composition of the UIKs but their ad-hoc nature makes them unlikely to be the driving force behind organised large-scale fraud [OSCE, 2000, 2004a,b, 2012a,b].

All voting data used in this project comes from the organisation GOLOS, an independent Russian NGO concerned with election monitoring. The data covers each of presidential and parliamentary election since 2000 and reports results in absolute terms at the UIK level for the entire Russian Federation. The dataset also features official numbers on the electorate as well as valid and invalid votes required to calculate turnout in each precinct. I matched the results at the district/TIK level which is the main unit of observation in calculating the measures of electoral fraud explained in section 2.3. From GOLOS I also obtained direct indicators of election rigging. During the 2011 and 2012 elections, the association ran the *karta narusheniy* (map of violations) project which provided a platform for citizens to anonymously report incidents of fraud in federal, regional, and local elections and send detailed reports of observed electoral law violations via phone, internet, and text message. In

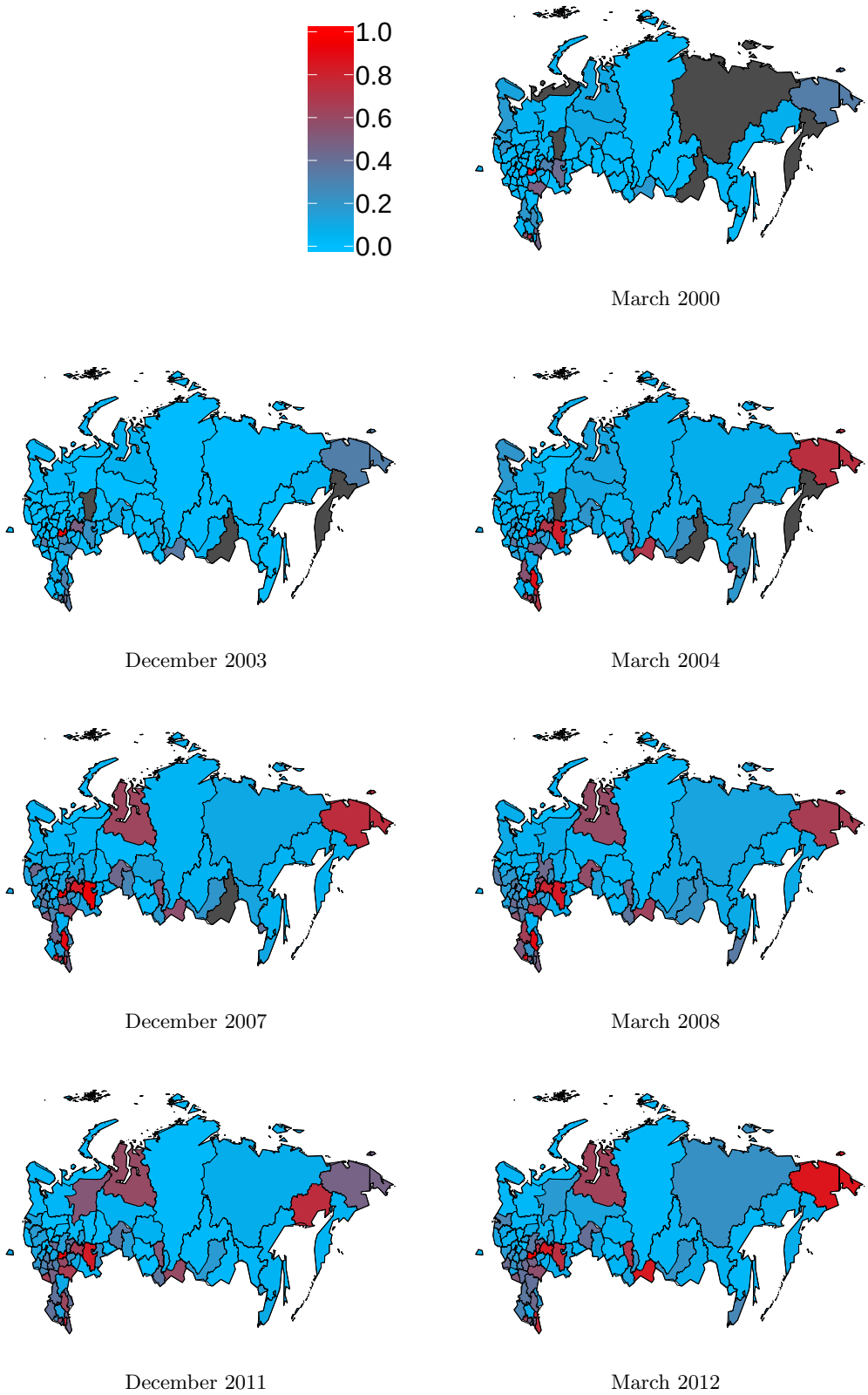


Figure 2.3: Share of suspicious votes before/after the abolition of governor elections in December 2004

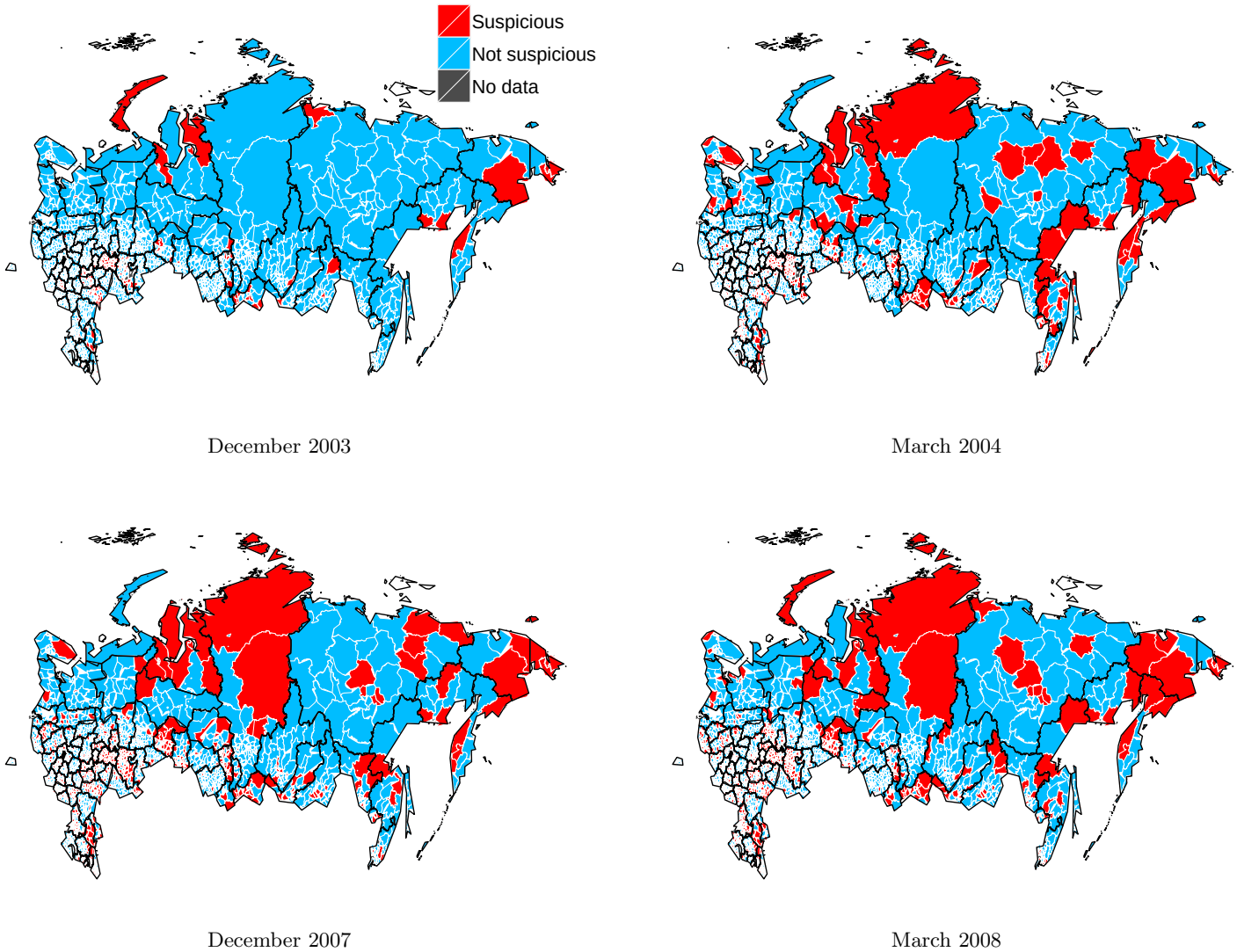


Figure 2.4: Suspicious districts before/after the abolition of governor elections in December 2004

addition to that, the users could also give information whether the action happened during campaigning or on election day and which type of fraud had taken place. Of particular interest for this research project are the categories *distortion of results* and *exclusion of observers, committee members, or media*. Lastly, the observers could also provide information on the location where the action was witnessed. I used this information to match each *violations reports* to a specific district which was possible for about 80% of all 12,800 reports (Duma 2011: 6,200/7,800; President 2012: 4,000/5,000). The analysis also draws on a number of other variables which are explained in the following.

2.4.2 Socio-economic data, further variables, and sample

According to reports, the main goal of the central government apart from delivering high vote results and legitimacy is the maintaining of social and economic stability. What is far less clear, however, is how such stability is evaluated by Russia's leadership. Obvious indicators relate to the economic performance of a region, especially those also related to the well-being of the population. I therefore obtained panel data from the Russian Federal State Statistics Service (GKS) on regions' log GDP per capita and unemployment rate. Assuming that governors are inferring the electorate's support for the incumbent party and presidential candidate based on their economic performance over the term time, I calculate 4-year changes in both variables. In order to get a single indicator of economic prosperity, the changes are then aggregated to a single index using principal component analysis. In order to assess the index' validity as a proxy for political support, I use data from the *Public Opinion Foundation* compiled in Reuter and Robertson [2012]. This yearly measure is the % of survey respondents in 68 Russian regions answering positively to the question whether their governor is doing a good job or not. From figure 2.5 one can see that the index is positively correlated with approval of a region's head. A simple OLS regression with region clustered errors and region fixed effects yields a positive and significant coefficient with a t-statistic of 5.4. Finally, in section 2.3 I make use of panel data on regions' population with internet access which was also obtained from GKS.

The switch from an *elected* governor to a *handpicked* governor is coded in the main treatment variable $HandpickedGovernor_{it}$. It has value 1 if the governor ruling region i at time t started his term after the 12th of December 2004 – and therefore had to be selected by the Russian president – or 0 otherwise. The precise dates of when a region's head entered office were retrieved from the website *rulers.org*. Another important piece of information are numbers on the members of the ruling

Figure 2.5: Economic performance and governor popularity

party *United Russia* by region. This information was gathered from reports of Russian Federal Ministry of Justice for the years 2009 until 2012 and turned into a per capita measure using yearly population counts at the region level from GKS.

The final panel dataset covers 71 out Russia's 83 regions over all 7 federal elections during the period 2000 to 2012. During this time there were 5 mergers between 2 or 3 regions which reduced the initial amount of 89 subdivisions to 83. Such mergers are likely to fundamentally change the power structure of a governor and make it difficult to compare the new units especially since all mergers took place after the abolishment of governor elections. For this reason I excluded all 10 regions affected by a merger. In addition to that, Chechnya lacked information on economic outcomes until the early 2000s and the Republic of Sakha didn't provide precinct-level voting data in 2000. Both regions were hence dropped from the dataset. Summary statistics of the final sample are reported in table 3.2.

Table 2.4: Descriptive statistics

Variable	Obs	Mean	Std.Dev.	Min	Max
Share of ballot w/ TVSC≥1	497	0.21	0.24	0.00	1.00
% Incumbent vote	497	0.59	0.16	0.25	0.99
% Turnout	497	0.65	0.11	0.44	0.98
Electorate in 100,000	497	13.60	12.35	0.34	73.10
Reports per 100k electorate on...					
Improper counting	142	0.36	0.49	0.00	2.56
Exclusion of voters	142	0.26	0.58	0.00	6.31
Illegal cmapaigning	142	0.12	0.32	0.00	2.92
Observers excluded	142	0.55	0.79	0.00	5.07
Faulty ballot box	142	0.24	0.42	0.00	2.92
Secrecy violated	142	0.12	0.16	0.00	0.93
Illegal voting	142	0.45	0.72	0.00	5.59
Other violations	142	0.87	1.11	0.00	8.86
% Pop. w/ internet	142	0.41	0.14	0.01	0.72
United Russia members p.c.	142	16.47	8.95	5.61	53.45
Handpicked governor	497	0.30	0.46	0	1
Handpicked governor b/w 2004/2007	497	0.34	0.47	0	1
Population in 100,000	497	17.99	17.17	0.51	118.43
4-year Δ Economy	497	0.02	1.14	−2.56	4.06
log(GDP p.c.)	497	11.37	0.91	8.75	14.06
4-year Δ log(GDP p.c.)	497	0.86	0.29	0.17	1.67
Unemployment rate	497	0.09	0.06	0.01	0.57
4-year Δ Unemployment rate	497	−0.02	0.04	−0.18	0.12

Notes: The unit of observation is one of the 71 regions in the sample at election t. Non-voting data available over several periods is linearly interpolated to the time of the election. Variables provided at the cross-sectional level only (i.e. with only 71 observations) are reported accordingly and used in the analysis by interacting them with either a post-2004 dummy or election fixed effects.

2.5 Empirical analysis

2.5.1 Identification

The main predictions of section 2.2.2 are that handpicked governors 1) have in general less incentives to rig elections since loyalty is assured and 2) they use rigging to compensate expected lower election results. One would therefore expect that in regions who had a handpicked governor, overall fraud levels decrease but that social instability could drive them up again. I model this mechanism in a difference-in-differences specification analysing the changing effect of economic performance on the extent of election fraud. The treatment in this setup is the forced change to a

governor chosen by the central administration:

$$
\begin{aligned}
ShareSuspicious_{it} = \alpha + \gamma_i + \lambda_t + \boldsymbol{\mu}\, \boldsymbol{X}_{it} \\
+ \beta\, HandpickedGovernor_{it} + \mu\, \Delta Economy_{it} \\
+ \theta\, HandpickedGovernor_{it} \times \Delta Economy_{it} + \epsilon_{it}
\end{aligned}
\tag{2.1}
$$

The addition of election-specific and region fixed effects λ_t and γ_i restricts the focus only to variation in suspicious votes within regions off any election specific trend. Election FEs account for the strong upward shift in suspicious votes over time that is by construction correlated with the arrival of handpicked governors after 2005. Area-specific fixed effects further control for permanently strong political machines originating from times of the Soviet Union [Hale, 2003]. Further controls $\boldsymbol{X}_{it}$ are including regions' aggregate democracy rating between 1991 and 2001 interacted with election fixed effects. The standard errors are clustered at the region level to account for autocorrelation of region-specific unobservables which may downward bias conventional robust estimates of the residuals' variance. In order to consistently estimate the effect of having a handpicked governor and its interaction with economic performance on the share of suspicious votes, one needs to assure that treated regions did not systematically differ from non-treated ones and that replacement is not correlated with other simultaneous changes in the respective areas. The first assumption is casually checked in figure 2.6 which displays the mean share of potentially fraudulent votes over time for regions with and without a replacement of governor between 2004 and 2008. As can be seen, the two groups follow roughly similar trends before the new law, even during the first major increase of suspicious votes in the presidential elections of March 2004. In the following elections, the patterns start diverging with *not replaced* regions displaying notably higher levels of suspicious votes. After an initial peak in 2008 with a difference in averages of 10%, *replaced* regions remain about 5% below the level of their counterparts.

Unlike the common trends assumption, the absence of confounding events is not straightforward to check. Replacing a region's leader may not only mean a change in loyalty but also in many other factors potentially correlated with rigging incentives. Two such confounders may be that the central government was targeting either particularly unpopular governors or particularly unsuccessful ones. Despite lacking pre-treatment data, figure 2.7 gives the hint that, if anything, handpicked governors were less popular than elected ones at the beginning but caught up over time. Given this, one would expect that handpicked governors were to engage in *more* fraud than old ones since their popularity does not allow them to turn voters

Figure 2.6: Average suspicious vote share before/after introduction of governor appointments

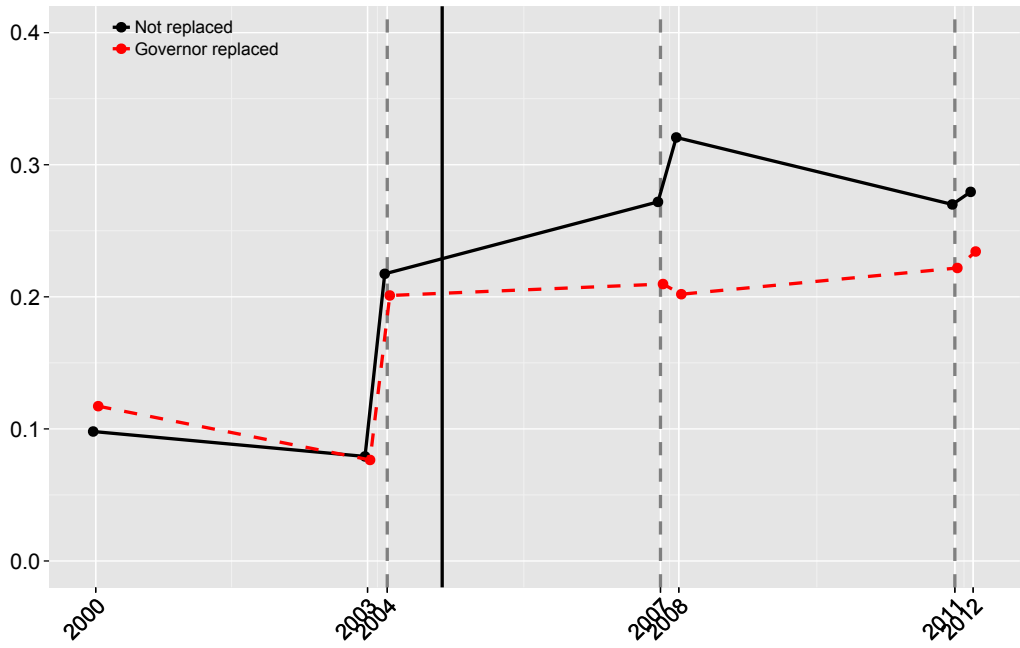


Figure 2.7: Average popularity of governors

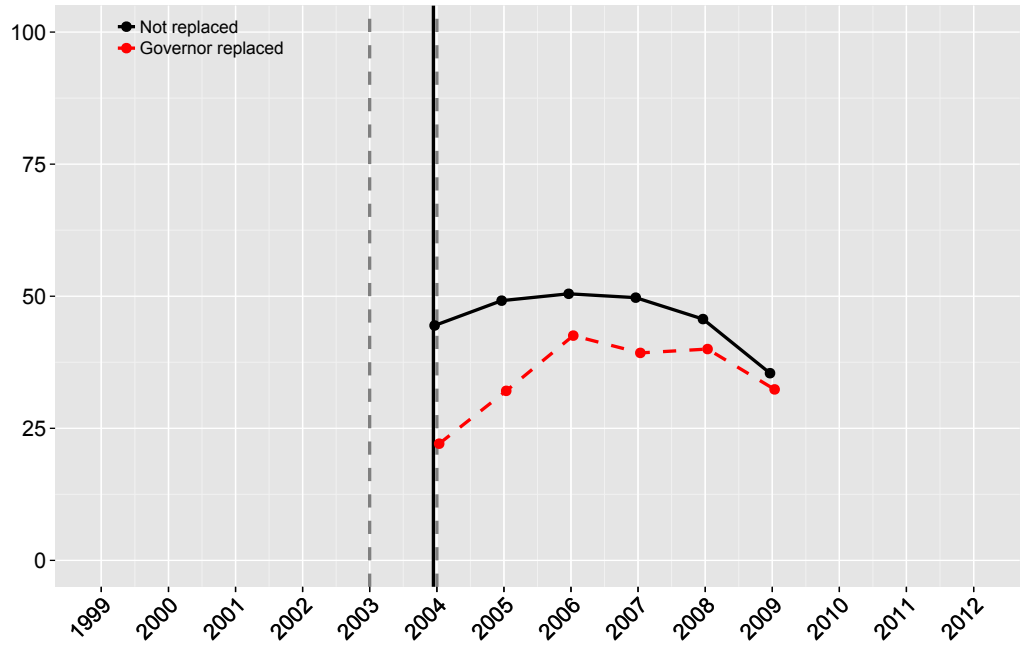


Figure 2.8: Average incumbent vote share before/after introduction of governor appointments

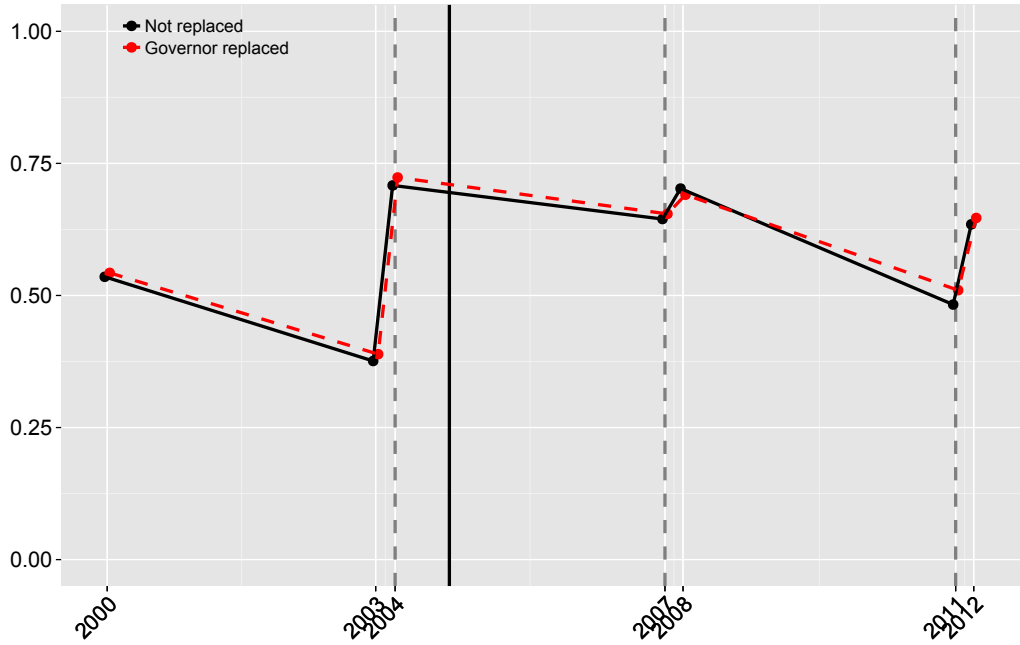
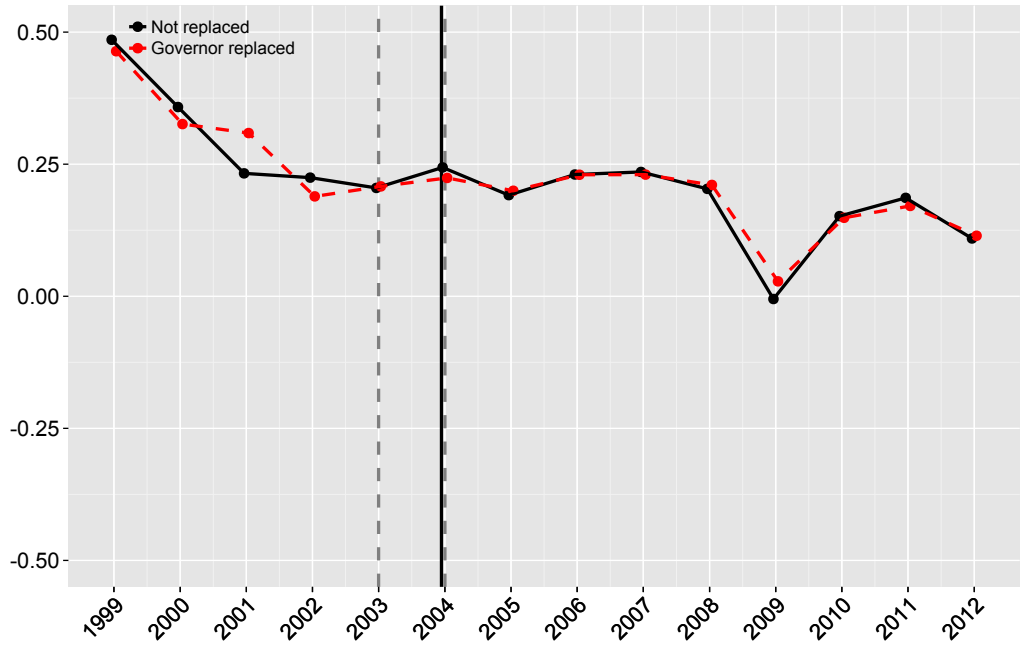


Figure 2.9: Average yearly GDP p.c. growth before/after introduction of governor appointments



in favour of the central government. Figure 2.8 also highlights that incumbent votes in federal elections were at a similar level and that replacement does not appear to have been a punishment for pre-2005 election results. The fact that incumbent votes do not respond to replacement after 2005 is striking at first but is not informative about governors' popularity given the changing intensity of fraud used to produce those results. A more reliable check whether success was a selection criterion or a confounder is provided in figure 2.9 which compares the average yearly growth in regional GDP per capita across the two subgroups. Again, there is no evidence for substantial differences across regions before and after the appointment of governors. It does not appear that a new region's head spurred economic growth which could have led to lower need to rig federal elections.

2.5.2 Baseline results

Table 3.3 reports the main results of the difference-in-differences estimation. As can be seen from the first 2 columns, *HandpickedGovernor* is positively correlated with potentially fraudulent elections even when controlling for time-invariant regional characteristics. This, however, is due to the general rise in replaced governors and fraud over time which leads to a notable upward bias. Once election fixed effects are accounted for in column 3 the coefficient flips sign but remains highly significant. According to the estimates, having a handpicked governor reduces suspicious votes by 8.5%. This is even larger than the 5% difference observed in the raw data in figure 2.6 and equivalent to a third of a standard deviation or moving from the median to the 25th percentile of the distribution. The result tentatively confirms the theoretical prediction that if the government selects its own candidate, the need to signal his loyalty by rigging elections is substantially diminished.

The coefficient remains virtually unaffected by the inclusion of control variables and 4-year economic performance in the next two specifications. Especially column 5 is reassuring that replacement is not endogenous to a leader's performance and that socio-economic development prior to the election has on average no effect on suspicious votes. The final specification allows this effect to differ for regions with a handpicked governor and shows that in this case, economic performance significantly lowers the share of suspicious votes in federal elections. In other words, the negative correlation between having an appointed governor and potentially fraudulent votes turns even more negative when the economy has done well over the last four years. Given that the minimum value of *4-year* Δ *Economy* is -2.56, handpicked governors deliver similarly suspicious results *only* when the region's economic development was extremely bad. This finding is consistent with hypothesis 2 in section 2.2.2

Table 2.5: Difference-in-Differences Results

	Share of suspicious votes					
	(1)	(2)	(3)	(4)	(5)	(6)
Handpicked Governor	0.037	0.064***	−0.086***	−0.086***	−0.086***	−0.108***
	(0.027)	(0.017)	(0.027)	(0.027)	(0.027)	(0.029)
4-year Δ Economy					0.005	0.016
					(0.013)	(0.014)
Handpicked Governor × 4-year Δ Economy						−0.047**
						(0.021)
Region FE	N	Y	Y	Y	Y	Y
Election FE	N	N	Y	Y	Y	Y
Controls	N	N	N	Y	Y	Y
Regions	71	71	71	71	71	71
Observations	497	497	497	497	497	497
R^2	0.005	0.674	0.770	0.787	0.787	0.790

Notes: Standard errors clustered at the region level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: $\text{Democracy}_{1991-2001}$ × Election FE

that even selected officials may use rigging to signal competence in the absence of political success.

2.5.3 Sensitivity and robustness checks

As a first test for the stability of the baseline estimates, I re-estimate the original model including additional fixed effect specifications. The two setups I am using are time shifters for each of the 8 Federal Districts and region-specific linear time trends. Despite looking similar at first, their inclusion serves different purposes. Federal Districts were created in 2000 by President Putin as an intermediary subdivision between the federal government and the regions and cover between 6 to 18 of these. The corresponding *plenipotentiaries* are directly appointed by the president and were used to tighten control over territories' leaders [Hill, 2012]. A particularly ambitious district leader could therefore replace corrupt governors and simultaneously disincentivise ballot rigging or introduce manipulation techniques that the TVSC cannot capture which would give similar results to the ones in section 2.5.2. Region-specific linear time trends, on the other hand, provide a test whether the effect could be driven by diverging trends in fraud between treated and non-treated regardless of governor replacements. Table 3.5 depicts the baseline estimates and their sensitivity to including the two additional fixed effect specifications jointly and by themselves. Reassuringly, the estimates for *HandpickedGovernor* and

Table 2.6: Baseline results and different FE specifications

	Share of suspicious votes			
	(1)	(2)	(3)	(4)
Handpicked Governor	-0.108^{***}	-0.096^{***}	-0.079^{**}	-0.082^{**}
	(0.029)	(0.030)	(0.035)	(0.037)
4-year Δ Economy	0.016	0.008	0.011	0.004
	(0.014)	(0.015)	(0.016)	(0.018)
Handpicked Governor $\times$ 4-year Δ Economy	-0.047^{**}	-0.035	-0.049^{**}	-0.050^{*}
	(0.021)	(0.023)	(0.025)	(0.027)
Region FE	Y	Y	Y	Y
Election FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Election $\times$ Fed.Distr. FE	N	Y	N	Y
Region FE $\times$ t	N	N	Y	Y
Regions	71	71	71	71
Observations	497	497	497	497
R^2	0.790	0.821	0.853	0.873

Notes: Standard errors clustered at the region level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: $\text{Democracy}_{1991-2001}$ $\times$ Election FE

HandpickedGovernor$\times$*4-year* Δ *Economy* are not changing substantially in magnitude and remain significant. The level treatment effect increases to about -0.08 in column 3 and 4, suggesting that the baseline estimate was slightly biased downwards due to diverging trends. The interaction with economic performance is less precisely estimated but remains almost identical even in the most flexible fixed effects setup in the final column. Overall, it seems unlikely that characteristics at the federal district level or diverging patterns in election fraud are driving the baseline results.

Another important question is the sensitivity of the baseline findings for *HandpickedGovernor* $\times$ *4-year* Δ *Economy* to alternative definitions of economic performance. In order to evaluate this, I investigate in table 3.7 the sensitivity of the baseline results to varying time-horizons of economic performance as well as the individual components of the index. Column 2 to 4 show that using the PCA of changes in unemployment and log GDP per capita over time horizons closer to the election data does not change the treatment effect. The coefficient on the interaction effect *HandpickedGovernor*$\times$*4-year* Δ *Economy*, however, halves in magnitude and loses its significance. The choice of a 4-year horizon thus seems to be a crucial choice for finding evidence on the competence mechanism. Finally, specifications 5 and 6 reveal that 4-year changes in unemployment are probably the main driver of the baseline results, while changes in log GDP per capita over the same time period per

Table 2.7: Sensitivity of results to definition of economic performance

Outcome variable	PCA of Δ Unemployment rate and log(GDP p.c.) over				4-year Δ of	
	4 years (baseline)	3 years	2 years	1 year	Unemployment rate	log(GDP p.c.)
	(1)	(2)	(3)	(4)	(5)	(6)
Handpicked Governor	-0.108^{***}	-0.093^{***}	-0.098^{***}	-0.093^{***}	-0.071^{***}	-0.001
	(0.029)	(0.028)	(0.026)	(0.027)	(0.026)	(0.065)
Δ Economy	0.016	0.010	0.017	0.023	-0.380	0.050
	(0.014)	(0.013)	(0.012)	(0.015)	(0.356)	(0.061)
Handpicked Governor × Δ Economy	-0.047^{**}	-0.019	-0.028	-0.023	1.287^{**}	-0.120
	(0.021)	(0.024)	(0.019)	(0.020)	(0.587)	(0.085)
Region FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Regions	71	71	71	71	71	71
Observations	497	497	497	497	497	497
R^2	0.790	0.787	0.789	0.790	0.790	0.788

Notes: Standard errors clustered at the region level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: $\text{Democracy}_{1991-2001}$ × Election FE

se do not allow reproducing these findings.

2.5.4 Placebo tests and related outcomes

The plausibility of the common trends assumption can also be tested through a placebo experiment in which the treatment period is moved forward by several time periods. In doing so, one can test whether there was a significant level difference between treatment and control group prior to the selection of a new governor which may have not been captured by the region-specific time trends in the preceding section. For instance, a new law in 2001 made it possible to prosecute governors for criminal activities [Sharafutdinova, 2010]. If under-performing, to-be-removed governors were decreasing election fraud in response to this law or any other policy change or if fraud reduction was in fact an anticipatory behaviour, this may still yield results similar to the baseline. Table 2.8 reports the initial estimates along with two further specifications looking only at pre-2005 data where the replacement of a governor is moved either one or two elections forward. As can be seen from column 2 and 3, this manipulation of the treatment variable halves the corresponding point estimate of *HandpickedGovernor* and leaves it insignificant. The interaction with *4-year* Δ *Economy* loses its significance in specification 3 but flips sign and remains highly significant in the second case. This finding can be explained by the fact that the 2004 surge in rigging was particularly strong in the Northern Caucasus regions such as Dagestan and North Ossetia whose governors were among the first ones to be replaced and at the same time saw their economies recovering after the nearby Second Chechen War 1999 to 2000. Once the 6 regions of the Northern Caucasian Federal district are omitted, the coefficient drops and becomes insignificant (see table A1).

One implicit assumption of this study is that election fraud in Russia between 2000 and 2012 has been mainly turnout increasing. This means that additional votes for the advantaged candidate are generated through unused ballot sheets or biased mobilisation of voters rather than stealing votes from other candidates or parties. Even the vote share of the incumbent may not be affected, given reports on specific *vote targets* which could be achieved by legal and non-legal means [White, 2011]. Hence, one would expect turnout but not other parties to respond to having a handpicked governor which offers another insightful placebo test. I therefore re-estimate equation 3.1 using turnout and vote shares of the incumbent and other parties (Communist, Ultranational, and Democratic) as outcomes. The findings presented in table 2.8 are roughly in line with the assumption of turnout increasing election rigging. Turnout in column 2 is decreasing by 2% on average in regions with

Table 2.8: Results from placebo tests on pre-treatment data

Treatment	Handpicked Governor$_t$	Handpicked Governor$_{t+1}$	Handpicked Governor$_{t+2}$
	(1)	(2)	(3)
Handpicked Governor	-0.090^{***}	-0.062	-0.059
	(0.028)	(0.038)	(0.049)
4-year Δ Economy	0.009	-0.009	-0.003
	(0.013)	(0.017)	(0.018)
Handpicked Governor $\times$ 4-year Δ Economy	-0.031	0.067^{**}	0.020
	(0.021)	(0.030)	(0.021)
Region FE	Y	Y	Y
Election FE	Y	Y	Y
Controls	Y	Y	Y
Sample	2000–2012	2000–2004	2000–2004
Regions	69	69	69
Observations	483	207	207
R^2	0.790	0.849	0.847

Notes: Standard errors clustered at the region level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: Democracy$_{1991-2001}$ $\times$ Election FE

an appointed governor, significant at the 10% level. The marginal effect turns even more negative depending on economic performance but is not statistically significant. The effect on incumbent vote share in specification 3 yields qualitatively similar but insignificant results. For the remaining parties, both the *HandpickedGovernor* and *HandpickedGovernor*$\times$*4-year* Δ *Economy* coefficients are far smaller and never significant. The coefficients on *4-year* Δ *Economy* indicate that, even in the presence of large-scale fraud, voters may hold the central government accountable to some extent and give their votes to opposition parties if their economic situation worsens. An effect of having a handpicked governor on vote shares of the incumbent or any other party can, however, not be found.

In sum, the results of section 2.5.2 have proven stable throughout a number of robustness and falsification checks. The interaction with regional economic growth has, however, turned out slightly less stable and appears to depend strongly on the time dimension used to construct the principal components.

2.6 Conclusion

In this paper, I study the importance of incentive structures of local officials on the dynamics of election fraud in Russia. I exploit a radical law change in December 2004

Table 2.9: Results for different election outcomes

Dependent variable	Share suspicious votes	% Turnout	% Incumbent	% Communist	% Ultra-national (LDPR)	% Democratic (Yabloko)
	(1)	(2)	(3)	(4)	(5)	(6)
Handpicked Governor	-0.108^{***}	-0.020^{*}	-0.013	0.006	-0.005	0.004
	(0.029)	(0.011)	(0.013)	(0.007)	(0.003)	(0.002)
4-year Δ Economy	0.016	0.005	0.022^{***}	-0.013^{***}	-0.002^{*}	0.000
	(0.014)	(0.004)	(0.005)	(0.003)	(0.001)	(0.002)
Handpicked Governor $\times$ 4-year Δ Economy	-0.047^{**}	-0.012	-0.010	0.004	-0.003	0.000
	(0.021)	(0.013)	(0.012)	(0.006)	(0.003)	(0.003)
Region FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Regions	71	71	71	71	71	71
Observations	497	497	497	497	497	497
R^2	0.790	0.825	0.894	0.857	0.898	0.860

Notes: Standard errors clustered at the region level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: Democracy$_{1991-2001}$ $\times$ Election FE

which allowed the central government to remove governors without any constraints and thus created strong motivation for the latter to use rigging in order to stay in office. Hypotheses from a simple conceptual framework predict that governors handpicked by the central government have less need to engage in fraud than elected (and not yet replaced) ones since their loyalty is assured. Handpicked governors on the other hand are assumed to respond with rigging in the face a bad economic performance and lower expected votes. The paper develops and extensively tests a new indicator of electoral fraud for Russian regions between 2000 and 2012 which is created from a unique micro-level dataset of election results at the voting station level.

The effect of having a handpicked governor on the share of suspicious votes in a region is estimated using a differences-in-differences estimation. The baseline results support the hypotheses and showed that regions with a handpicked governor have on average 10% less suspicious votes than those with elected ones. Furthermore, also the interaction of the treatment variable with economic performance is negative and indicates that in the case of a very bad economic performance the extent of fraud by handpicked governors would be equal to that of elected ones. In this sense, loyalty and competence can be regarded as complementary. Both effects were highly significant and passes several robustness checks concerning the validity of the common trends assumption and placebo treatments. While the share of suspicious votes is affected by the law change and handpicked governors, I also show that election outcomes of the incumbent as well as other parties did not respond. In other words, the incentive structures of governors does not change the results of elections, but only the way they are generated.

Despite focussing only on the case of Russia, the findings provide interesting insights into the functioning of competitive authoritarian systems in general. Unlike in a totalitarian system, elections can actually still function as an arena for political competition, albeit only among lower-tier officials. Contrary to common knowledge, I show conceptually and empirically that rigging is far less common among the dictator's cronies due to their certain loyalty. From a policy perspective, this means that advocating for the co-optation of non-cadres to government positions in an authoritarian regime may actually have counter-productive results and lead to higher levels of election fraud. Also the fact that bad economic performance induces even higher levels of election fraud should be born in mind when deciding about sanctions.

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2.A Tables

Table A1: Results from placebo tests on pre-treatment data (omitting Northern Caucasus district)

Treatment	Handpicked Governor$_t$	Handpicked Governor$_{t+1}$	Handpicked Governor$_{t+2}$
	(1)	(2)	(3)
Handpicked Governor	-0.069^{**}	-0.050	-0.044
	(0.027)	(0.041)	(0.048)
4-year Δ Economy	0.021	-0.002	0.003
	(0.014)	(0.020)	(0.021)
Handpicked Governor $\times$ 4-year Δ Economy	-0.013	0.052	0.009
	(0.020)	(0.036)	(0.023)
Region FE	Y	Y	Y
Election FE	Y	Y	Y
Controls	Y	Y	Y
Sample	2000–2012	2000–2004	2000–2004
Regions	65	65	65
Observations	455	195	195
R^2	0.801	0.867	0.866

Notes: Standard errors clustered at the region level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Controls: Democracy$_{1991-2001}$ $\times$ Election FE

Chapter 3

The Political Fallout of Chernobyl: Evidence from West-German Elections

3.1 Introduction

The impact of experience on people's belief formation has recently received wide attention in the economics literature. The exposure to unusual, highly traumatic or joyful events in one's life has been shown to leave large footprints in people's minds and set the path for various future outcomes such as trust, political attitudes, or marriage decisions. The research until now has also been very diverse in terms of the analysed event and geography. Madestam and Yanagizawa-Drott [2012], for instance, look at how patriotic events affect political preferences in the United States and Chen and Yang [2015] investigate the impact of the Great Famine on political trust in China. Malmendier and Nagel [2011] and Giuliano and Spilimbergo [2014] both look at how recessions can shape young individuals' attitudes towards risk and redistribution. Nunn and Wantchekon [2011], on the other hand, study the effect of a long-term event like the slave trade on mistrust in contemporary Africa. What most studies have in common is the focus on first-hand experiences or events in people's immediate surroundings. Evidence on the impact of remote phenomena with an entirely psychological effect, however, has remained scarce until now.

This paper studies a distant event, the Chernobyl nuclear disaster of April 1986, and its political effects on Western Germany.[1] At the time of the accident, 18 nuclear power plants (henceforth *NPP*) were operating in Germany and 14 more were at a planning or construction stage. The use of nuclear energy was not a salient political issue until the disaster and only the recently founded Green party was openly opposing nuclear energy [Joppke, 1990]. Protest against new NPPs was either highly localised or coming from radical leftist groups. My empirical analysis exploits the county variation in distance to the nearest nuclear facility to study the impact of the disaster on voting behaviour in the short and long-run. Proximity to the nearest facility is interpreted as the intensity at which the informational shock about the dangers of nuclear energy was perceived and has been analysed in similar applications [Abadie and Dermisi, 2008; Pignataro and Prarolo, 2012; Bauer, Braun, and Kvasnicka, 2014]. I add to this literature by applying the proximity measure to the first and most important nuclear disaster in Europe.

The dataset features a list of all nuclear facilities in West Germany and the near abroad as well as results of 11 Federal elections in 301 counties in West Germany over the time period 1976 to 2013. Results are provided for the four main German parties including the emerging Green party. My empirical analysis

[1] For the sake of simplicity, I refer in the remainder to the Federal Republic of Germany and post-unification Germany simply as *Germany* and to the German Democratic Republic as *East Germany*.

exploits this data structure for a differences-in-differences estimation to study the change in proximity's impact on voting behaviour after April 1986. Concerns about the endogeneity of power plants' locations are met by controlling for a range of socio-economic factors such as age structure, economic well-being and education. My baseline results show that counties closer to the nearest NPP experience see a significant increase in votes for the leftist Green party as well as the centre-right conservative CDU/CSU at the expense of ideologically less extreme parties. Moving from the bottom to the top decile in proximity, results in a 0.25% higher vote share for Greens and a 1.34% increase for the CDU/CSU. My findings are robust to various robustness checks and are not driven by elections in the immediate aftermath of the disaster. Rather, polarisation continues through to the latest German Bundestag election in 2013.

In order to get at the mechanisms driving my results, I investigate three different channels. First, I look at other political outcomes to establish that proximity did not lead to a punishment of the parties who approved the respective power plant but on the other hand significantly increased turnout and polarisation of election results. Second, building on the importance of formative years and the persistence of political preferences, I also investigate variation in time to the next election and counties' age structure. I find that that late post-Chernobyl elections and higher share of 15-25 year old individuals at the time of the accident are positively increasing the proximity effect of the conservatives but not the Green party. A special role of this age group is in line with research on the importance of *impressionable years* for the formation of political beliefs [Krosnick and Alwin, 1989]. Finally, I demonstrate that the effect is not depending on differences in economic well-being and that educational differences in areas of higher NPP-proximity are only benefiting the conservatives.

Taken together, the results suggest that living closer to a nuclear power plant during the Chernobyl accident had two separate effects on political attitudes in Germany. The first one is higher support for the *anti-nuclear* Green party which appears to be stemming from areas with lower numbers of adolescents and otherwise is independent of socio-economic characteristics. This finding is in line with the general aversion of citizens towards high-risk facilities in their surroundings, also known as NIMBY (Not In My BackYard) effects. The second effect is a rise in votes for the *pro-nuclear* CDU/CSU in areas closer to an NPP among the young and educated. Despite appearing counter-intuitive at first, this resembles similar findings on the 1976 California primary elections in which an anti-nuclear initiative was opposed particularly by the more educated part of society [Kuklinski, Metlay,

and Kay, 1982]. These authors find that voters with higher knowledge were relying stronger on a cost-benefit analysis in their decision process rather than ideology or peers. This provides a link between the level of education and voting conservative after Chernobyl. In my context, such local benefits could be job opportunities and positive externalities such as investments in infrastructure. I argue that especially young and educated individuals may oppose the shut-down of nuclear facilities since they are more likely to adapt *and* have a more accurate assessment of the actual risk of a nuclear disaster [Shaw, 1996].

This paper links to several research areas. Most closely related are the studies of life-changing experiences and the formation and persistence of beliefs mentioned above. Furthermore, there exists a good amount of research on the health effects of the Chernobyl disaster [e.g. Lüning et al., 1989; Almond, Edlund, and Palme, 2009; Danzer and Danzer, 2014]. I add to this work by investigating the political effects of Chernobyl in West Germany. Also the effect of NPP-proximity has been investigated in few recent studies. Pignataro and Prarolo [2012] look at how living closer to a planned power plant affected voting in the 2011 Nuclear referendum in Italy and find a positive effect on anti-nuclear voting decisions. Schumacher [2014], on the other hand, looks at how distance to an NPP correlates with votes for the German Green party between 1998 and 2009. Distance to an NPP can, however, also affect economic outcomes as Bauer, Braun, and Kvasnicka [2014] show in their study on the changes in housing prices after the Fukushima accident in 2013. My paper extends this literature in several ways. First, I relate proximity to an NPP also to votes of parties other than the Greens. Second, I study the effect of proximity to NPP at a point when the dangers of nuclear energy were most likely to be perceived as an informational shock.

The remainder of the paper is structured as follows. First, I will give a brief discussion of the conceptual framework and the predicted effects of the Chernobyl disaster in Germany according to the current state of research. This is followed by a description of the political and historical background of nuclear power usage in Germany in order to provide the reader with the necessary context of this case study. The next two sections discuss the data and the identification strategy. My empirical analysis starts by presenting the baseline results and several robustness checks and then moves on to exploring the mechanisms driving my findings on voting behaviour in West Germany. The final section concludes.

3.2 Conceptual framework

The political effects of the Chernobyl disaster can be divided into two categories. The first one concerns the persistence of the disaster's political impact. As exemplified in the study by Giuliano and Spilimbergo [2014], one would expect that Chernobyl as a formative event per se has long-lasting effects particularly on young people between the age of 18 and 25. Focus on this age group is motivated by psychological research which has shown that *impressionable years* are crucial for shaping beliefs and attitudes of an individual [Krosnick and Alwin, 1989]. A long-term change in political behaviour could, however, also be rationalised through the general persistence in people's voting decisions. Spontaneous, one-time protest votes in response to the Chernobyl disaster could thus turn into long-term changes in electoral support. An empirical example of this mechanism are Kaplan and Mukand [2014] who show that party registrations in response to the 9/11 terrorist attacks strongly predict future party support.

The other category addresses differences in voters' electoral response to Chernobyl. Living closer to an NPP is a typical case in which one would expect *NIMBY* and *home-voter* effects. In essence, both theories predict that voters will opt *for* policy choices increasing the value of their home and *against* those which diminish it or are expected to be harmful.[2] Following these mechanisms would predict an increase in votes for anti-nuclear parties as a response to the Chernobyl disaster depending on how close voters live to the nearest NPP. This view is supported also for the issue of nuclear power plants in the study by Pignataro and Prarolo [2012] on the 2011 Nuclear Referendum in Italy and Schumacher [2014] in his work on the determinants of Green votes between 1998 and 2009.

The rejection of nuclear power among voters is, however, ambiguous. Kuklinski, Metlay, and Kay [1982], for instance, investigated the 1976 California primary elections in which the majority of people voted *against* a phase-out from nuclear power. Even though this event took place before the disastrous Three Mile Island accident in March 1979, which boosted the anti-nuclear movement in the United States, the study offers important insights. The authors find, for example, that educated people were more likely to object the 1976 proposal and that this may be based on their different abilities to assess the costs and benefits from abandoning nuclear power. Following this view, voters of higher education may react differently to an increased awareness from NPP-proximity and decrease or even offset the NIMBY effects described above.

[2] See [Pignataro and Prarolo, 2012] for a detailed discussion of the two mechanisms and an application to the issue of NPPs.

Having described the conceptual framework of the analysis, I will now move on to give a detailed historical description of nuclear power usage in West Germany, the anti-nuclear movement and the aftermath of the Chernobyl.

3.3 Historical background

3.3.1 Nuclear energy in West Germany, the new social movements, and the early years of the West German Greens

Germany's experience with nuclear energy goes back as far as WWII to the *Uranium Project* of the Nazi regime and the construction of several research reactors. After being banned from nuclear research until 1955, the first research reactor in Garching near Munich went into operation in October 1957 [Hassel, Koester, and Pabst, 1997]. The civilian usage of nuclear power in West Germany started in November 1960 with the NPP in Kahl near Frankfurt am Main which was followed by five further plants during the 1960s. Table 3.1 provides an overview of all NPPs and their operation and approval. Initially, the arrival of this novel energy source was not politicised in any way and hailed by all three major German parties CDU/CSU, FDP, and SPD. This elite consensus was sustained by population and the media alike who showed little to no interest in the new technology until about the mid 1970s. Another sign of the low priority of nuclear power issues was the delegation of responsibilities regarding commissions and NPP sites to state ministries [Joppke, 1993].

The oil crises of the 1970s fundamentally changed the situation for several reasons: first, it encouraged a massive expansion of nuclear energy as it made Western Germany less dependent on oil-producing countries and secondly, it brought the limits of economic growth to the attention of the wider public and raised awareness about the environmental impact of growth [Joppke, 1990]. Citizens' initiatives inspired by the student movement of the 1960s became the first anti-nuclear groups of West Germany and started to organise local protests against existing or planned NPPs. Anti-nuclear initiatives themselves differed substantially in respect to their usage of violent means as well as their social origins. While some were peaceful and organised with the broad support of the local population, others were dominated by radical communist and autonomous groupings from larger cities. For these, the opposition to nuclear energy was just one way of fighting the capitalist state in which the use of violence was legitimate and even encouraged [Joppke, 1993]. Overall, the protest movement was thus confined to the urban radical left and the population living in the immediate surroundings of nuclear power plants.

The roots of the German Green party lie within the new social movements

Table 3.1: Nuclear power plants in West Germany

Nuclear power plant	State	Start of operation	Approving state government
Kahl	Bavaria	01/02/1962	CSU, GB/BHE
MZFR Karlsruhe	Baden-Württemberg	19/12/1966	CDU, FDP
GundremmingenA	Bavaria	12/04/1967	CSU
Lingen	Lower Saxony	01/10/1968	SPD, FDP
Obrigheim	Baden-Württemberg	01/04/1969	CDU, FDP
Juelich	Northrhine-Westphalia	19/05/1969	CDU
Grosswelzheim	Bavaria	02/08/1970	CSU
Stade	Lower Saxony	19/05/1972	SPD, CDU
Niederaichbach	Bavaria	01/01/1973	CSU
KNK KarlsruheI	Baden-Württemberg	21/02/1974	CDU
BiblisA	Hesse	26/02/1975	SPD
Wuergassen	Northrhine-Westphalia	11/11/1975	SPD
Neckarwestheim1	Baden-Württemberg	01/12/1976	CDU, SPD
BiblisB	Hesse	31/01/1977	SPD
Brunsbuettel	Schleswig-Holstein	09/02/1977	CDU, FDP
KNK KarlsruheII	Baden-Württemberg	03/03/1979	CDU
Isar/Ohu1	Bavaria	21/03/1979	CSU
Unterweser	Lower Saxony	06/09/1979	SPD
Philippsburg1	Baden-Württemberg	26/03/1980	CDU, SPD
Grafenrheinfeld	Bavaria	17/06/1982	CSU
Kruemmel	Schleswig-Holstein	28/03/1984	CDU
GundremmingenB	Bavaria	19/07/1984	CSU
GundremmingenC	Bavaria	18/01/1985	CSU
Grohnde	Lower Saxony	01/02/1985	SPD
Philippsburg2	Baden-Württemberg	18/04/1985	CDU
Brokdorf	Schleswig-Holstein	22/12/1986	CDU
Hamm-Uentrop	Northrhine-Westphalia	01/06/1987	CDU, FDP
Muelheim-Kaerlich	Rhineland-Palatinate	01/10/1987	CDU
Isar/Ohu2	Bavaria	09/04/1988	CSU
Emsland	Lower Saxony	20/06/1988	CDU
Neckarwestheim2	Baden-Württemberg	15/04/1989	CDU

(NSPs) of the 1960s and 1970s from which also the anti-nuclear movements originated and who distinguished themselves from established parties primarily through their focus on post-material values. Focal issues included environmental awareness and opposition to nuclear energy but also emancipation of women, gay rights, peace and civil rights. Yet, it was especially ecological initiatives which started from 1977 to form electoral alliances and to participate in local elections [Probst, 2013]. After initial successes in municipal and state elections in northern Germany, the initially loose alliances in several states started to cooperate and formally registered as parties. Simultaneously, ecological alternatives also participated in the elections to the European Parliament 1979 as *SPV Die Grünen* which attained 3.2% in Germany.[3] This entitled *SPV Die Grünen* to 4.5 million Deutsche Mark of campaign funding

[3] SPV is short for *Sonstige Politische Vereinigung*(Other Political Association The Greens).

and provided a crucial stepping stone for transforming the alliance into the new party *Die Grünen* in January 1980 [Falter and Klein, 2003]. In the 1983 election, *Die Grünen* received 5.6% of the total votes and for the first time entered the German parliament (*Bundestag*) and was now also represented in 6 out of 11 West German State parliaments.

3.3.2 Chernobyl and its effect on public opinion

On the 26th of April 1986, an accident in the Chernobyl nuclear power plant (Soviet Union, now Ukraine) led to a reactor explosion and the release of enormous amounts of radioactive material. The consequences for the local population and nature were devastating: areas within a 30km radius of the plant (about 330.000 inhabitants) were evacuated and are still not inhabitable today due to radiation. Research by the *Chernobyl Forum* estimated about 9,000 cancer deaths in highly contaminated areas directly related to the disaster [Ebermann and Junkert, 2011]. Due to wind and rain patterns in the immediate aftermath of the accident, also wider parts of Europe were exposed to radioactive fallout. In Germany, contamination was strongly concentrated in the very South-East but so far could not be linked to direct health effects on the local population in scientific studies [Cort, 1998]. The German Radiation Protection Commission (*Strahlenschutzkommission*) released recommended maximum radiation values for raw milk and leafy vegetables in May 1986 which led to the destruction of harvests particularly in Southern Germany [Ebermann and Junkert, 2011].

The German public had already been aware to some extent about nuclear energy's dangers before the accident. Since most of this awareness was coming from anti-nuclear initiatives rather than the state, there was a deep distrust towards official information and advice regarding the Chernobyl disaster's consequences. Attention-seeking media titles such as *"Mass death after Chernobyl"* or *"Chernobyl is killing Munich's children"* and the uncoordinated reactions by the official authorities turned Germans' scepticism about nuclear power into outright fear and even panic [Ebermann and Junkert, 2011]. While the federal government was not taking any actions and played down the dangers of the disaster, some states started to introduce measures such as tolerance levels of radiation and bans on the certain vegetables which further deepened the pre-existing mistrust [Joppke, 1990].

A long-term result of the Chernobyl disaster was the drop in public support for nuclear energy and new power plants [Boer and Catsburg, 1988]. At the political level, only the social democrats responded by starting to oppose the usage of nuclear energy while conservatives and liberals remained neutral and soon afterwards

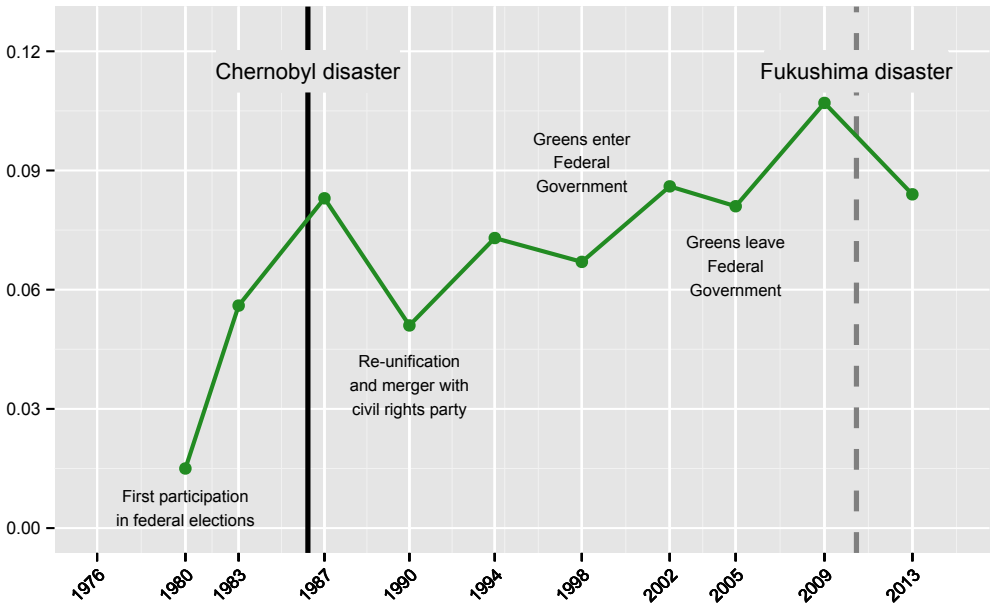
returned to a pro-nuclear stance. At the local level, the disaster led on the one hand to a radicalisation of the existing anti-nuclear power movement and on the other hand to the emergence of a more civilised middle-class protest movement. This new movement consisted mainly of concerned citizens preferring information campaigns and lobbying of politicians to violent rallies. The wider appeal of this grass-roots type of protest as opposed to its radical counterpart is exemplified by a petition against a nuclear power plant in Bavaria which gained almost 900,000 signatures [Joppke, 1990].

3.3.3 The Green party's ascend to power and Germany's exit from nuclear energy

During the 1987 elections – 10 months after the Chernobyl disaster and despite severe tensions between the orthodox and moderate party factions – the Greens could increase their vote share to 8.3%. A major success was the formation of a coalition with the social democrats in Hesse and the first entry into a state government. The German reunification in 1989 and the subsequent Bundestag election in December 1990 were an ambivalent experience for the party: on the one hand they started cooperating with the East German civil rights party *Bündnis 90*, on the other hand it was only for this cooperation and a one-time exception in the electoral law that the Greens remained in parliament. While the West German branch of *Die Grünen* only received 4.8%, its East German counterpart attained 6.1% and thus managed to cross the 5% hurdle at least in one part of the reunified country. The Greens' focus on environmental topics and the deliberate neglect of current issues such as the unification severely backfired in this case [Probst, 2013]. The 1990 alliance was formally turned into an association in January 1993 and the party changed its name officially to *Bündnis 90/Die Grünen*. Figure 3.1 depicts the evolution of the German Green party using their vote shares in German Federal elections.

Throughout the 1990s the German Greens continued their trend towards more moderate policy positions and managed to increase their vote share to 7.3% in the 1994 Federal parliamentary election and enter state governments in Northrhine-Westphalia, Schleswig-Holstein, and Hamburg. Importantly, the decline of the more extreme wing within the party did not mean a more compromising position on the usage of nuclear power or environmental issues in general. This proved detrimental when in 1998 they adopted an electoral platform arguing for raising the price of petrol to 5 Deutsche Mark per liter over time. Political enemies of the Greens exploited this topic extensively which almost led to a repetition of the 1990 experience and the Greens received only 6.7% remaining well below their expectations. The

Figure 3.1: Voting for the German Greens over time



strong gains of the social democrats, however, still made it possible to form the first federal government of SPD and *Bündnis/Die Grünen*. Apart from the foreign ministry, which was now headed by party leader Joschka Fischer, the Greens also took over the ministries of health and environment [Falter and Klein, 2003]. In June 2000 the new government reached a first agreement with energy suppliers about a complete exit from nuclear power within about 30 years depending on the spread of a negotiated maximum production cap over the existing NPPs. This agreement was turned into law in April 2002, only few months before the next parliamentary election. As a result, two power plants ceased operation in 2003 and 2005 [Rüdig, 2000; Jahn and Korolczuk, 2012].

After the 2005 election which led to a *Grand Coalition* between CDU/CSU and SPD without the Greens, the new nuclear law was left untouched despite the conservatives' strong favour for nuclear energy. This changed in 2009 when a coalition of conservatives and liberals took power and all parties responsible for the nuclear phase-out had left the government. Already in December 2010, law was changed anew to substantially extended operation times of existing NPPs. The Fukushima accident in March 2011 turned this policy into a boomerang for the new government. Facing three important state elections and an upset electorate, the CDU/CSU/FDP government hastily abandoned its pro-nuclear policy and decided

on the immediate shutdown of the 8 oldest power plants and a complete exit from nuclear energy by 2022. Nevertheless, the CDU lost power after 58 years in the state of Baden-Württemberg which has since been led by a first Green Minister-President of Germany [Gabriel and Keil, 2012; Jahn and Korolczuk, 2012].

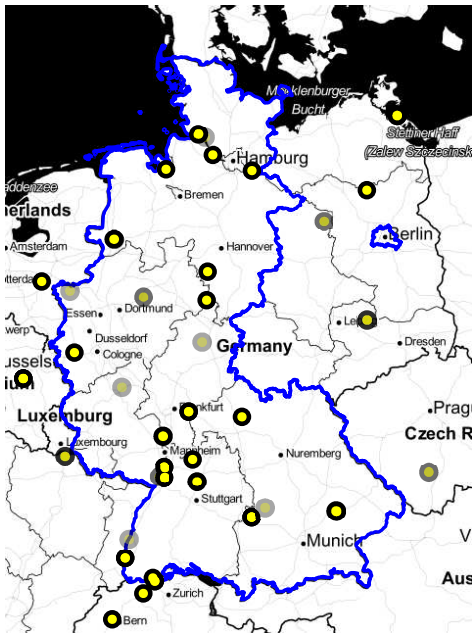
3.4 Data

3.4.1 Distance to Nuclear facilities

In order to construct my treatment variable, I collected data on nuclear facilities in Germany and the near abroad from several sources. Lists of reactors were taken from Bredberg et al. [2015] for Germany and from *nucleopedia.org* for all other European countries. Both sources also feature key dates of each facility such as start and end of operation, beginning of construction, and approval. Furthermore, the data allows me to differentiate between power plants and research reactors. From the website *election.de* I also obtained the names of the parties forming the state government at the time of approval and thus responsible for a specific facility in West Germany. Finally, I geocoded the location of all reactors using the website *OpenStreetMap.org* which is displayed in figure 3.2. This information is paired with a map of West German counties from the Federal Agency for Cartography and Geodesy [2014] and raster data on population density from CIESIN [2005].

Combining this information allows me to calculate the distance of each county's population centroid from each nuclear facility. In the baseline version of my treatment variable, I assign to each district the distance from the nearest nuclear power plant (NPP) in Western Europe only. This is because knowledge about sites in Eastern Europe might have been less accurate given the tight travelling restrictions to Communist countries in the 1980s. The literature also does not report any protests in West Germany against NPPs in Communist countries which could be related to the general leftist orientation of West Germany's anti-nuclear movement. In the analysis I also differentiate between active NPPs and those in the planning/construction stage in April 1986 and later on. In order to obtain proximity rather than distance to the respective facility, values are multiplied by -1. Figure 3.3 and 3.4 plot the geographic dispersion and density of proximity to the nearest operating or planned NPP at the time of the Chernobyl accident. The treatment has a mean about -50 and the median of -60 which reflects the distribution's skewness to the left.

Figure 3.2: Locations of facilities: Operating and planned NPPs



3.4.2 Federal and state election data 1980–2013

Election results are the main outcome used in the empirical analysis in section 3.6. The first type are the Federal parliamentary (*Bundestag*) elections taking place every four years or after a dissolution of parliament. The electoral system is a *mixed member proportional representation* in which each citizen has two votes: one is for the nominated party candidates in his precinct, the second one is for a specific party and its list of candidates. The so-called *first vote* (Erststimme) follows a pure majority rule and determines which candidates win a seat in the Bundestag irrespective of their position in the party list. Votes for minority parties thus face a high risk of not being counted at all and are often given to a candidate of a different party with higher chances of winning. Since such incentives lead to heavy distortions of voters' actual political preferences, I focus on the *second vote* (Zweitstimme) which determines what fraction of total seats are going to be held by the respective party in general. Seats are allocated by the party lists in each state.

For each Federal election between 1980 and 2013 I obtained results at the county level from the Federal Statistical Office (FSO) and the State Statistical Offices (SSOs). Most of the recent data was made available on the FSO's website *Regionalstatistik.de* or the corresponding State equivalents. Older results had to be collected directly from the State offices. The parties I'm focussing on are Greens

Figure 3.3: *NPP-Proximity*$_i$ on April, 26th 1986 across Germany counties (state borders in blue, non-sample states grey)

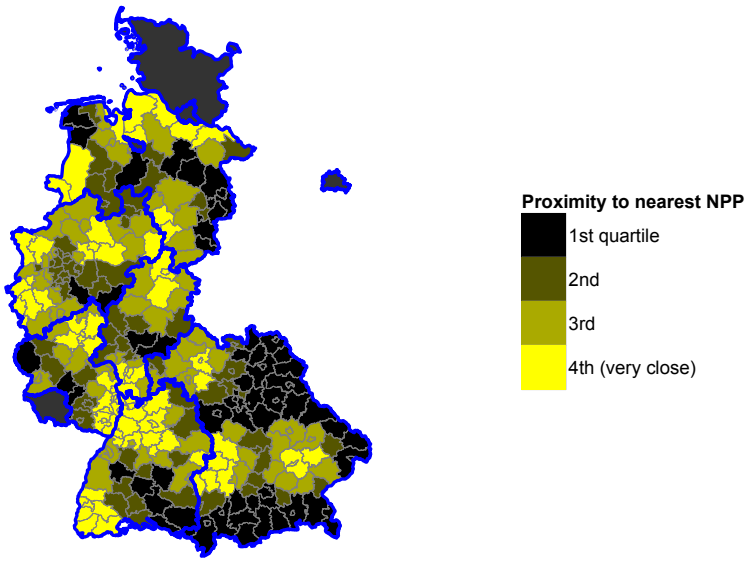


Figure 3.4: Histogram of *NPP-Proximity*$_i$ on April, 26th 1986

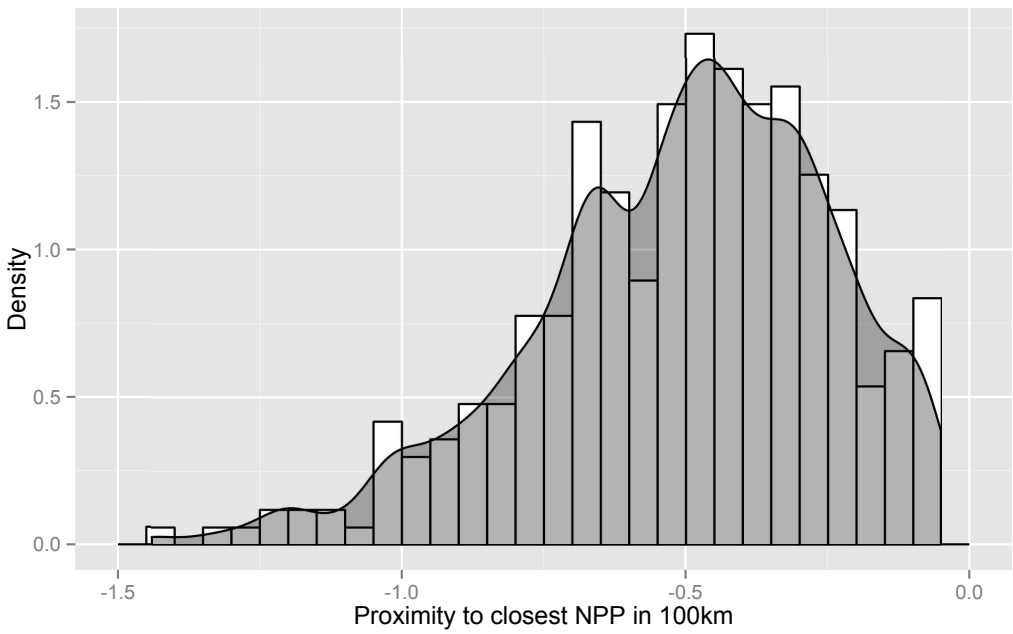
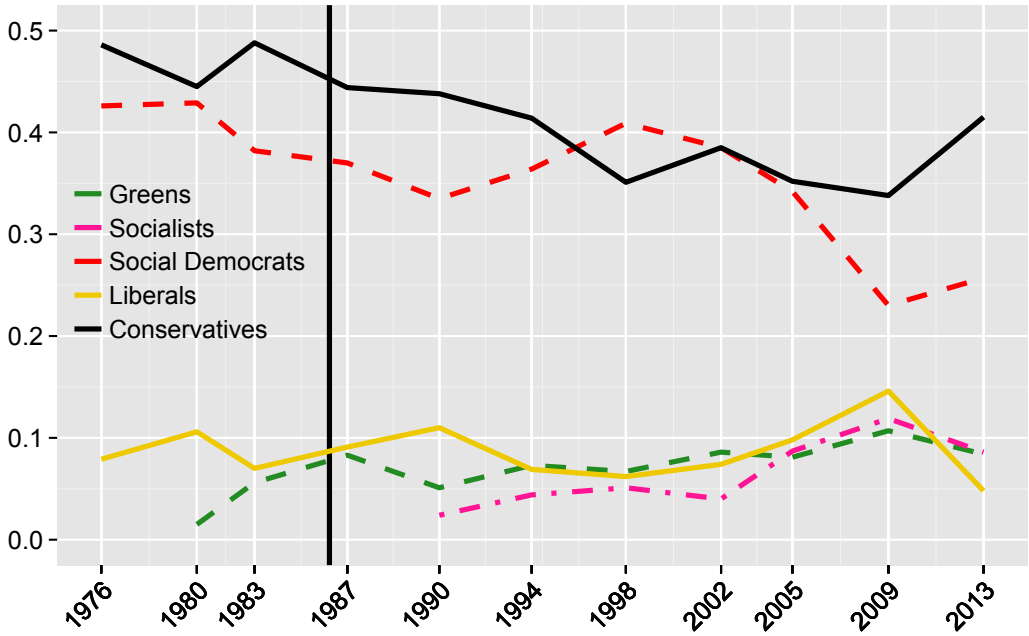


Figure 3.5: Evolution of election results in Germany (before 1990 only FRG)



(B90/Die Grünen), Social-Democrats (SPD), Liberals (FDP), and Conservatives (CDU/CSU) whose shares are calculated as ballots cast for the respective party in each county and election divided by total ballots cast. Figure 3.5 shows the evolution of aggregate election results of West and post-unification Germany over the studied period. Results are displayed for the four main parties as well as the socialists who entered the German party system after the unification.

3.4.3 Control variables and construction of panel dataset

Using the same sources as the voting data, I also acquired extensive information on each county's socio-economic characteristics over the time period studied. The first is the log of the total population in each county which accounts for the notable differences in urbanisation. Two important determinant of Green votes which could also be confounders are age and gender which I account for by including the share of women and the share of 6 specific age groups in the overall population.[4] Other potential confounders are economic well-being and educational attainment: I measure well-being as the percentage of the total population receiving state benefits. Unlike average income, this variable has the advantage of being resistant to outliers

[4] The included groups are 15–20, 25–30, 30–40, 40–50, 50–65, ≥65. The share of 0–15 year olds is omitted as the baseline category.

Table 3.2: Descriptive statistics

	Obs	Mean	Std.Dev.	Min	Max
Vote share Greens	3,685	0.06	0.04	0.00	0.29
Vote share Conservatives	3,685	0.45	0.11	0.19	0.77
Vote share Social Democrats	3,685	0.35	0.11	0.10	0.64
Vote share Liberals	3,685	0.09	0.03	0.02	0.23
% Turnout	3,685	0.81	0.07	0.58	0.95
Prox. closest NPP (planned)	3,685	-0.71	0.39	-1.91	-0.05
Prox. closest NPP (operating)	3,685	-0.61	0.30	-1.44	-0.06
Prox. closest NPP (op. & plan.)	3,685	-0.51	0.26	-1.44	-0.05
Prox. closest reactor (op. & plan.)	3,685	-0.43	0.26	-1.44	-0.01
Population in 1,000	3,685	182.80	145.89	33.21	1,330.44
% female	3,685	0.52	0.01	0.49	0.57
% benefit recipients (−2005)	2,680	0.03	0.02	0.00	0.12
% benefit recipients (2005−)	1,005	0.01	0.01	0.00	0.09
% share of pupils in prep school	3,685	0.18	0.10	0.00	0.88

Notes: The unit of observation is one of the 301 counties in the sample at election t. Non-voting data available over several periods is linearly interpolated to the time of the election. Variables provided at the cross-sectional level only are reported accordingly and used in the analysis by interacting them with either a post-Chernobyl dummy or election fixed effects.

on the top of the distribution and gives a precise estimate of the poor part of the local population. Education is an important factor given the Green party's focus on post-material values and is reflected in particularly good results in university towns and among people of higher education in general. My final two control variables are therefore distance to the closest university and the share of 0–15 years olds attending grammar schools (*Gymnasium*).[5] All control variables are interacted with election dummies to allow for changing importance in people's voting decisions over time.[6]

The states for which all of the information mentioned above could be retrieved were Baden-Württemberg, Bavaria, Hesse, Lower Saxony, North Rhine-Westphalia, and Rhineland-Palatinate.[7] Together, these six regions account for about 90% of West Germany's area and population. As mentioned before, the unit of observation is county i at election t. Since there were almost no boundary changes during the studied period, I can track 301 counties over the whole period from 1980 to 2013. The values of the control variables are linearly interpolated to the election

[5] A list of German-speaking universities in West Germany and Austria along with their geographic coordinates was kindly provided to me by Fabian Waldinger.

[6] Another important reason is the radical change in Germany's benefit laws in 2005, the so-called *Hartz* laws. This reduced the amount of benefit recipients dramatically without significant changes in poverty.

[7] I am currently in the process of digitising information for the city states Berlin, Bremen and Hamburg and the two smallest states of Schleswig-Holstein and Saarland.

date since they are usually measured at a statistical reference date.[8] Table 3.2 shows summary statistics for the variables used in the empirical analysis.

3.5 Identification strategy

3.5.1 Differences-in-Differences specification

The panel structure of the data allows the simultaneous use of county and election fixed-effects. In doing so, one can account for all election- and county- specific characteristics. This is particularly helpful because the Chernobyl accident led to a general rise in Green votes as shown in figure 3.5 which is mechanically correlated with the changing effect of NPP-proximity after April 1986 and could thus result in a strong upwards bias. But also fixed local characteristics crucial to the location decision of an NPP such as proximity to navigable rivers and railroads could turn out to be problematic. Since better infrastructure could be correlated with higher income and education, this would open up an alternative link between nearby NPPs and good electoral performance of the Greens. After including both types of fixed effects, the remaining variation is only within counties and off any country-wide election-specific trend.

The main variable of interest is the interaction of $\textit{NPP-proximity}_i$ with a dummy variable for any election after the Chernobyl disaster which measures the average change in NPPs effect on electoral outcomes after the disaster as opposed to before. The set and state of nuclear facilities is restricted to that of April, 26th 1986. This has the disadvantage of not being responsive to changes in status and location of plants but on the other hand is less prone to issues stemming from the endogeneity of NPP shutdown decisions. Such a scenario could arise, for instance, if voters were punishing the Greens for a shutdown in their vicinity which would open up an additional channel between proximity and voting outcomes.[9] Adding a set of control variables completes the baseline regression specification:

$$y_{it} = \alpha + \gamma_i + \lambda_t + \beta_t(\textit{NPP-Proximity}_i \times \textit{postChernobyl}_t) + \boldsymbol{\mu}\, \boldsymbol{X}_{it} + \epsilon_{it} \tag{3.1}$$

[8] This is usually the 31st of December, apart from school enrolment data which takes mid-October (15th) as reference date.

[9] An alternative would be omitting shutdowns and confining attention only to NPPs starting to operate over time. I investigate this alternative specification as a robustness check in section 3.6.2.

3.5.2 Threats to identification

The identifying assumptions of the differences-in-differences estimation are twofold: the first is the absence of confounding events which would require any correlate of NPP location to change its effect on voting around the time of the Chernobyl disaster. Riester [2010], provides a short list of criteria used in Germany for choosing the sites of nuclear facilities:

1. Earthquake/flood-proof, suitable building ground
2. Not located in an urban agglomeration
3. Well connected to public road, train, and waterway network
4. Site should be already designated industrial estate
5. Vicinity to river with sufficient flow
6. Located in a region of high and preferably increasing energy consumption and with link to high-voltage grid

Criteria 2 and 6 together insinuate a quadratic relationship between proximity and urbanisation/population size. Since inhabitants of cities could systematically change their political preferences after April 1986, *log(population)* and *log(population)*^{2} seem to be crucial confounders and are therefore included in the regressions. Other factors such as connection to transportation, rivers, and soil quality are highly unlikely to change their effect on voting behaviour after Chernobyl and are thus already accounted for through the use of county fixed effects. Another endogeneity issue could be systematic selection into treatment. For example, environmental activists could move closer to NPPs after Chernobyl in order to facilitate protests and raise awareness of the local population. Alternatively, the Green party could also target campaigning towards affected areas. The first concern is addressed by the inclusion of age and education which captures changes in the electorate's composition towards those parts of the population most likely to support the Greens. The second one is met by checking for endogeneity of turnout with respect to NPP-proximity after Chernobyl which should be another consequence of increased campaign intensity.

The second main assumption is that election outcomes in counties of different proximity would follow identical patterns absent treatment and conditional on control variables. This condition breaks down if areas of different treatment intensities were already starting to diverge before the actual treatment due to anticipation or unobserved correlates. One could, for example, imagine that areas closer to an

Figure 3.6: Average Green vote share before/after Chernobyl depending on NPP-Proximity (median)

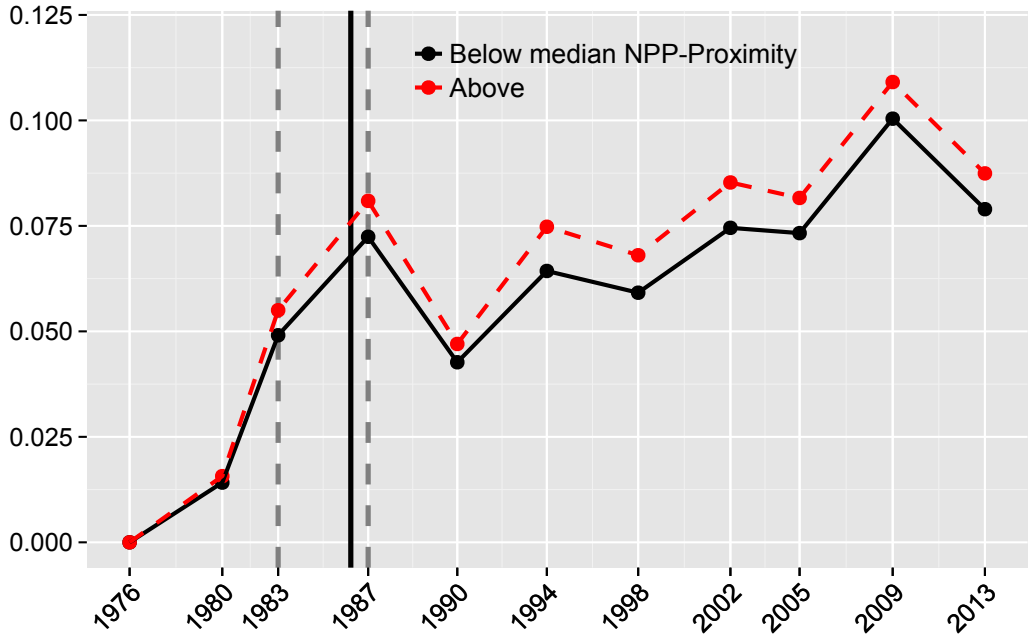
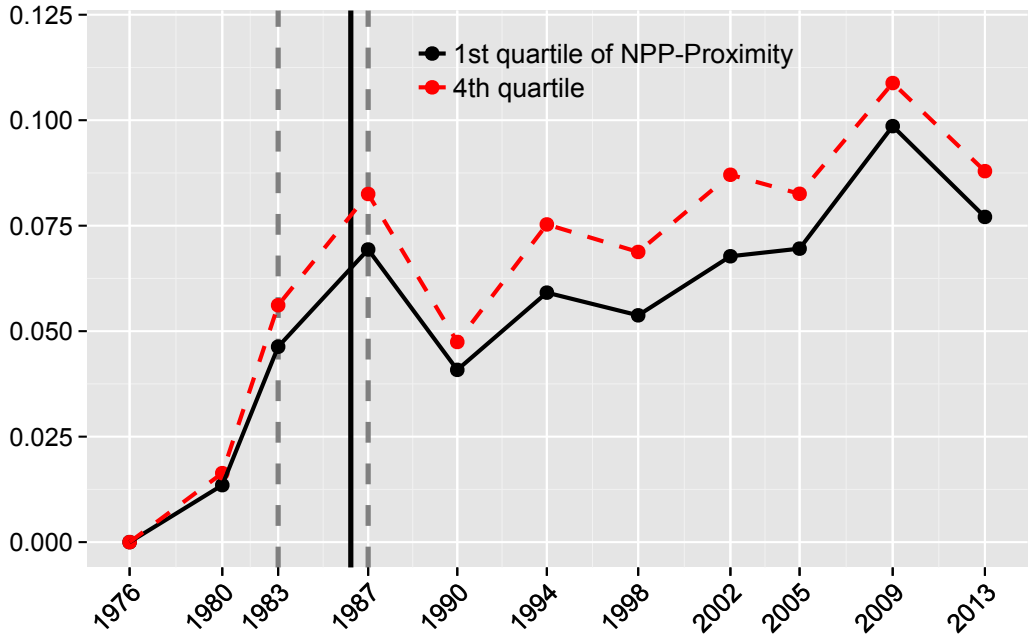


Figure 3.7: Average Green vote share before/after Chernobyl depending on NPP-Proximity (quartile)



NPP were already better informed about the dangers of nuclear energy and would have increased their Green votes share even in the absence of Chernobyl. In a regression this would mechanically load the widening gap onto the interaction term $NPP\text{-}Proximity_i \times postChernobyl_t$ and lead to a large overstatement of the treatment effect. As a first attempt to investigate this issue, I plot the average vote share for the Green party over time in areas above and below the median of proximity to the nearest NPP in figure 3.6. Reassuringly, both groups have fairly similar support levels before April 1986 and start to diverge in the 1987 election where counties closer to an NPP have about 1% higher Green vote shares. After a quick drop in 1990, the gaps continue to widen with a peak in 2002 until they are almost back to their pre-treatment gap in 2009.

However, looking at the more extreme case of counties in the lowest and highest quartile of the NPP-proximity in figure 3.7, divergence across groups may have already taken place between 1980 and 1983. This prompts for a more thorough investigation of the common trends assumptions which I carry out in several ways: first, I allow for non-linear trends at higher levels of aggregation by including state and district-specific time fixed-effects. In addition to that, I also include unit-specific trends which allow each county to exhibit an arbitrary linear pattern over the whole time period. Both procedures should leave the coefficient of interest unaffected in the absence of the corresponding pre-trend. Finally, I also interact the treatment with election fixed effects instead of a post-Chernobyl dummy. This makes it possible to investigate the changes in the effect of NPP-proximity with respect to a baseline election and to capture any non-linear relationship with the outcome of interest before the Chernobyl disaster. Bearing those caveats in mind, I will now proceed to the baseline results.

3.6 The effect of nuclear facilities on elections in West Germany

3.6.1 Baseline results

Table 3.3 shows the effect of NPP-Proximity on Green party votes after subsequently accounting for two-way fixed effects and control variables. Initially, there seems to be a strong negative correlation between proximity to the nearest NPP and Green votes which is even increasing in magnitude after the inclusion of fixed effects in specification 2. The only takeaway from this is that the omitted time-invariant local characteristics are correlated in the same way with the outcome and the treatment

Table 3.3: Baseline estimates

	Green party vote share			
	(1)	(2)	(3)	(4)
NPP-Proximity	-0.043^{***}	-0.079^{***}	0.013^{***}	0.004^{*}
	(0.004)	(0.004)	(0.004)	(0.002)
County FE	N	Y	Y	Y
Election FE	N	N	Y	Y
Controls	N	N	N	Y
Counties	301	301	301	301
Observations	3,685	3,685	3,685	3,685
R^2	0.135	0.574	0.902	0.970

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)

variable $NPP\text{-}Proximity_i \times postChernobyl_t$. Once I also include election fixed effects in the next column, the negative correlation observed before remains significant but turns positive. Since the correlation between $NPP\text{-}Proximity_i \times postChernobyl_t$ and t is negative by construction and the Green party share is increasing over time, this does not come as a surprise. The final specification in column 4 controls for an extensive set of county characteristics. While this decreases the coefficient of interest further to 0.004 it remains significant at 10%. According to the baseline estimate, counties located 100km closer to an operating or planned NPP have on average a 0.4% higher support for the Green party in Bundestag elections after the Chernobyl disaster, ceteris paribus.

Since the gains of the Greens from NPP-Proximity have to be at the expense of other parties, I investigate in table 3.4 the treatment effect on the rest of Germany's party system. Interestingly, it turns out that proximity to an NPP after April 1986 did *not* lead to a general swing to the left but instead lowered votes for the Social Democrat and the Liberal parties and benefited the Conservative CDU/CSU even more than the Greens. Also minority parties saw their combined vote share marginally decline. The large responses of the conservative and social-democratic parties are, however, mainly due to their overall higher amount of votes: standardizing the coefficients reveals that a one standard increase in NPP-Proximity yields an increase of 0.034 standard deviations in Green vote share as opposed to 0.058

for the conservatives.[10] This means that although the nominal effect on the main parties is notably higher, the impact of NPP-Proximity on Green and Liberal vote shares given their scale is not very different. In sum, the baseline results suggest a polarisation of voting patterns in response to the Chernobyl accident depending on being located closer or farther away from an NPP where both the most left-wing and the most right-wing of the main parties gained. The next section evaluates the validity of the identifying assumptions and the sensitivity of the baseline results.

3.6.2 Robustness checks

The first tests will address the doubts about the validity of the common trends assumption raised by figure 3.7. To start with, I include area-specific election fixed effects into the baseline regression. Doing so purges the entire effect of state/district specific variables from the estimation. If the observed effect of NPP-Proximity was driven by pre-treatment divergence from state or district unobservables, this should leave the coefficient of interest insignificant. In the next step, I add county-specific time trends to take into account arbitrary long-term linear developments in voting patterns which might be mistakenly identified as a treatment effect. Finally, I also use combinations of these additional variables. Columns 2 and 3 in table 3.5 show that the baseline estimate is particularly sensitive to the inclusion of state- and district-specific election fixed effects. County-specific trends in specification 4, on the other hand, strengthen the baseline estimate. Also when using both linear trends and area-specific election dummies, the coefficient for NPP-Proximity drops in size and is insignificantly different from zero. Taken together, these results caution that non-linear pre-trends stemming from differences at the district or state level could be driving the results.

Another way to assess the prevalence of diverging patterns before the Chernobyl accident is allowing for a time-varying treatment effect. This can be implemented by interacting NPP-proximity with election fixed effects instead of a post-Chernobyl dummy and normalising the effect to zero at the last election before the disaster. Doing so provides a placebo test and makes it possible to check whether proximity to a nuclear plant already had an effect on voting outcome prior to April 1986 and whether this effect was already increasing over time beforehand. Figure 3.8 plots the estimated time-varying effect of NPP-proximity on the vote share of the Green party from 1976 up to 2013. Reassuringly, the results suggest that the effect of living closer to an NPP in 1983 was not significantly different from that of the two elections before. After 1983, the coefficient rises from around 0 to almost 0.005

[10] The standardized coefficients are reported in table 3.13 in the appendix.

Table 3.4: The effect of NPP-proximity on other parties

Vote share	Greens	Social-Democrats	Liberals	Conservatives	Others
	(1)	(2)	(3)	(4)	(5)
NPP-Proximity	0.004*	−0.018***	−0.004	0.020***	−0.002
	(0.002)	(0.005)	(0.003)	(0.006)	(0.003)
County FE	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Counties	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685
R^2	0.970	0.979	0.936	0.969	0.961

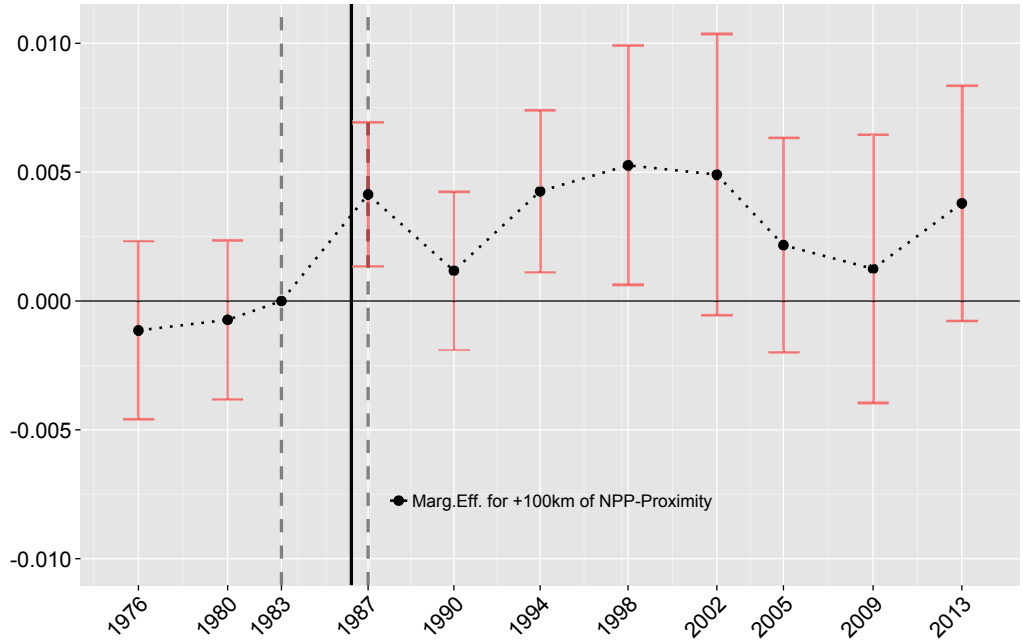
Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)

Table 3.5: Baseline results and different FE specifications

	Green party vote share					
	(1)	(2)	(3)	(4)	(5)	(6)
NPP-Proximity	0.004*	−0.000	0.000	0.005**	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
County FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Election × State FE	N	Y	N	N	Y	N
Election × District FE	N	N	Y	N	N	Y
County FE × t	N	N	N	Y	Y	Y
Counties	301	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685	3,685
R^2	0.970	0.977	0.983	0.985	0.989	0.992

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)

Figure 3.8: Time-varying treatment effect estimates and 90% CI: Green party vote



where it remains until 2002 with the exception of the unification election in 1990. The effect drops sharply for the 2005 and 2009 election and sees a slight rebound in 2013. Taken together, these patterns are compatible with the view that Chernobyl led to a persistent increase in Green votes until a phase-out from nuclear energy was officially decided in 2002. The result for 1990 does not fit the pattern but was dominated by one of the most important events in Germany's contemporaneous history which the Green party deliberately ignored. Germany's abandonment of the exit strategy from nuclear energy and the Fukushima accident brought this topic back to the public's attention. This can also be seen from the rising coefficient in 2013 which is, however, not significant at the 10% level.

The final set of checks is concerned with the estimates' robustness to alternative definitions of NPP-proximity regarding the set of facilities used to calculate proximities as well as the functional form used. The analysis starts with table 3.7 which presents the regression results for 8 alternative treatment specifications. The specifications differ on three dimensions: 1) status (planned or operating), 2) purpose (including research reactors or not) and 3) location (Germany, Western Europe, or the entire Europe). Column 5 repeats the baseline results for easier comparison. From the first three columns one can see that the most of the effect is driven by the proximity to *planned* nuclear power plants rather than an *operating* ones. When

looking only at facilities in Germany as done in column 1, the two separate effects actually go into the opposite direction. A speculative explanation of this finding could be that within Germany, operating plants may be an important employer and the Greens are thus seen as a threat to local employment after the Chernobyl disaster.[11] When pooling operating and planned facilities together, the two effects cancel each other out in column 4 while in 5 and 6 this pooling only adds some noise and the point estimate remains almost the same. The last three specifications look at the impact of proximity to the nearest NPP *or* research reactor. Doing so diminishes the size of the coefficients in columns 4 to 6 and leaves them insignificant. Overall, the results in table 3.7 show that the baseline result is not completely dependent on the set of NPPs chosen for the proximity measure. The restriction to one single coefficient rather than two separate ones for planned and operating NPPs, however, also comes at the cost of masking some heterogeneity in the effect. The precise way in which NPP-proximity was affecting voting behaviour and which parts of the population were most responsive is addressed in the following section.

[11] An alternative story could be that people had a strong dislike for NPPs being replaced by less-clean fossil fuel power plants like coal.

Table 3.6: Differences-in-Differences estimates with time-varying treatment effect

Vote share	Greens	Social-Democrats	Liberals	Conservatives	Others
	(1)	(2)	(3)	(4)	(5)
NPP-Proximity × 1976	−0.001	−0.000	−0.004*	−0.000	0.006
	(0.002)	(0.004)	(0.002)	(0.006)	(0.005)
1980	−0.001	0.004	0.002	−0.007	0.001
	(0.002)	(0.004)	(0.003)	(0.005)	(0.002)
1987	0.004**	−0.014***	0.001	0.006	0.002
	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)
1990	0.001	−0.008**	−0.003	0.014*	−0.003
	(0.002)	(0.004)	(0.003)	(0.007)	(0.004)
1994	0.004**	−0.013***	−0.007***	0.014**	0.002
	(0.002)	(0.005)	(0.002)	(0.007)	(0.003)
1998	0.005*	−0.024***	−0.005**	0.022***	0.003
	(0.003)	(0.006)	(0.002)	(0.008)	(0.004)
2002	0.005	−0.005	−0.005	−0.002	0.007***
	(0.003)	(0.009)	(0.003)	(0.012)	(0.002)
2005	0.002	−0.016*	−0.007*	0.021**	0.000
	(0.003)	(0.008)	(0.004)	(0.009)	(0.004)
2009	0.001	−0.024***	−0.004	0.030***	−0.004
	(0.003)	(0.007)	(0.006)	(0.011)	(0.005)
2013	0.004	−0.023***	−0.012***	0.034***	−0.002
	(0.003)	(0.007)	(0.003)	(0.010)	(0.005)
County FE	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Counties	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685
R^2	0.971	0.979	0.936	0.969	0.961

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)

Table 3.7: Sensitivity to alternative treatment definitions

	Green party vote share								
Used facilities	Nuclear power plants						NPPs + research reactors		
Sample	Germany	Western Europe	Europe	Germany	Western Europe	Europe	Germany	Western Europe	Europe
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
NPP-Proximity				0.002	0.004*	0.004*	0.002	0.003	0.003
				(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
NPP-Proximity (operating)	−0.004*	0.001	0.001						
	(0.002)	(0.002)	(0.002)						
NPP-Proximity (planned)	0.004**	0.003**	0.004**						
	(0.002)	(0.001)	(0.002)						
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	301	301	301	301	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685	3,685	3,685	3,685	3,685
R^2	0.971	0.971	0.971	0.970	0.970	0.970	0.970	0.970	0.970

Notes: Standard errors clustered at the precinct level in parantheses, $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, $\geq$65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)

3.7 Mechanisms

3.7.1 Changes in the political landscape

As the results in table 3.4 have already shown, it was not only the Green party which gained after April 1986 in areas closer to an NPP but also the conservative CDU/CSU. The parties losing were SPD and FDP who were positioned more at the centre of the political spectrum. In general, there are many ways of how to explain this notable change in the political landscape after Chernobyl. One of these would be that voters systematically punish politicians responsible for the NPP in their area. Since the location decision is carried out at the state level [Joppke, 1993], this would imply punishing the parties in power at the time of approval. In order to investigate the relevance of such *punishment votes*, I construct for each county the party vote share of the government in power during the approval of the nearest NPP. This, however, can only be done using the sample of NPPs inside West Germany, assuming that voters cannot punish foreign governments. Another way in which the disaster could have changed voting patterns is by raising political awareness and participation in political life in favour of parties with more extreme positions. As mentioned in chapter 3.5.2, this could be either because of NPP-proximity itself or as a result of targeted campaigning in the surrounding areas of nuclear power plants. Any proximity effect on turnout will thus not be able to differentiate between these two mechanisms. A third additional explanation would be that the sudden politicisation of nuclear power usage led to political polarisation in the local population. Potential lines of conflict in this case could be economic dependency on the NPP as an employer and the perceived danger or awareness of nuclear energy's risks. Voting patterns could have diverged since only the parties on the very left and right would guarantee the implementation of a distinct pro- or anti-nuclear policy. In order to measure political polarisation, I construct an index similar to Xezonakis [2012] for each election in each county.[12]

The three mentioned channels are investigated in table 3.8. Since the punishment effect can only be evaluated using nuclear facilities in West Germany, I also report the results for the Greens and the CDU/CSU in columns 1 and 2. Column 3 shows that the parties approving the respective plant did *not* receive less votes in areas located closer to the NPP but, if anything, actually *gained* after the Chernobyl accident. Hence, there is no empirical support for a punishment mechanism.

[12] The formula used is: $Polarisation = \sqrt{\sum_{j=1} VoteShare_{jk}(Ideology_{jk} - \overline{Ideology}_k)^2}$. The four main parties are placed on a discrete left-right ideology scale between -2 and 2 (omitting 0) for the sake of simplicity.

Table 3.8: The effect of NPP-proximity on the political landscape

Vote share	Greens	Conservatives	NPP Approvers	Turnout	Polarisation
	(1)	(2)	(3)	(4)	(5)
NPP-Proximity (West. Germany)	0.002	0.022***	0.008		
	(0.002)	(0.006)	(0.009)		
NPP-Proximity (West. Europe)				0.009***	0.023***
				(0.003)	(0.006)
County FE	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Counties	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685
R^2	0.970	0.969	0.921	0.978	0.962

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)

Turnout, on the other hand, increases significantly as a response to NPP-proximity after April 1986. According to the estimate, living 100km closer to an NPP increases election participation by almost 1% which is about 1/8 of a standard deviation of turnout. Given the size of the coefficient, higher participation could explain the comparatively small gains of the Green party but not the much larger ones of the conservatives. Finally, in accordance with the single party results, also polarisation increases significantly after Chernobyl in areas closer to a nuclear power plant. Areas located 100km closer to a nuclear power plant see polarisation increase by 0.023 which is about one quarter of a standard deviation of the constructed index.

3.7.2 Chernobyl as a formative event

The results in table 3.6 not only provide a robustness test but also reveal that the effect of NPP-proximity on voting behaviour persists even more than 20 years after the Chernobyl accident. In the following, I investigate the reasons and mechanisms underlying this persistence. I evaluate two channels which are both concerned with the role of Chernobyl as a formative event in people's life. The first of these is considering the path-dependence of voting decisions taken shortly after the accident. As recent empirical research has shown, voters are very hesitant in adjusting political choices made in the past such as party registration [Kaplan and Mukand, 2014].

Spontaneous electoral choices made under the emotional influence of a formative event such as the Chernobyl disaster may therefore have long-lasting effects on voting. In order to investigate this channel, I exploit the fact that two large German states were holding state elections between April 1986 and the next Bundestag election in January 1987: Lower Saxony (15th of July 1986) and Bavaria (12th of October 1986). This allows me to check whether the effect of NPP-proximity lasted longer depending on the amount of time voters had before going to the ballot boxes.

Table 3.9 shows that the proximity effect changes depending on months until the next election. The impact on the large parties CDU/CSU and SPD seems to be benefiting from voters' having more time to think about their choice for the next election. As table 3.9 shows, the negative effect on the social democrats is mainly driven by counties who could vote shortly after Chernobyl while that of the conservatives only gains significance in areas which did not vote immediately after the disaster. This could be because coherence within the SPD was low during this time and facilitated a spontaneous swing to left-wing fringe parties. Both regression output and marginal effect plot in figure 3.9 also show that NPP-proximity did not notably change its effect on Greens' votes. The effect of proximity is decreasing for Liberals and other parties in months until the next election. As table 3.9 shows, fringe parties are actually significantly benefiting from NPP-proximity and close elections. Path dependency originating from early elections after Chernobyl is thus, if anything, mostly favouring fringe parties.[13]

The second channel looks into the role of age as a proxy for how formative the effect of NPP-proximity was for the average population of a county. This assumes that the political beliefs of youths are easier to shape than those of elder people. Giuliano and Spilimbergo [2014], for instance, have shown that experiencing economic recession during the *impressionable years* of 18 to 25 has considerable long-term effects on people's political views. In order to explore this mechanisms, I interact the treatment variable with the share of 15 to 25 years olds at the time of the accident.[14] The regression results in table 3.10 immediately show that mostly conservatives and others are affected by the interchange between NPP-proximity and the share of adolescents around April 1986. The effect on the Green party, in contrast, is decreasing in the affected counties' share of 15 to 25 year olds and is

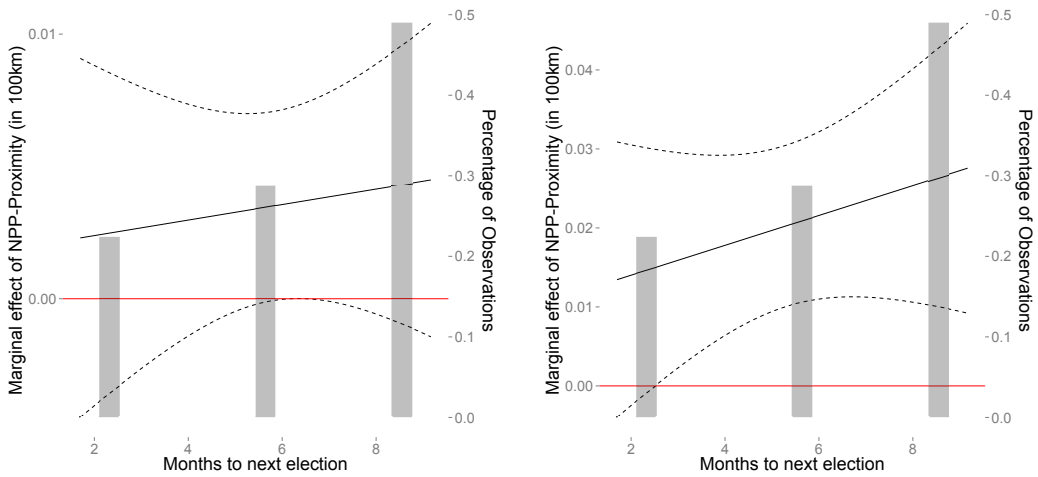
[13] Liberals are not counted as fringe parties in this case. The marginal effect of NPP-proximity on the FDP is close to zero and insignificant in counties with an early election and significantly negative otherwise.

[14] This procedure requires the inclusion of *% aged 15-25*$_{1983}$ $\times$ *post-Chernobyl* along with *% aged 15-25*$_t$ $\times$ *election* which could create a multicollinearity problem. I thus use pre-Chernobyl controls as of 1983 interacted with election fixed-effects in all regressions looking at the effect of pre-Chernobyl characteristics.

Table 3.9: Memory effect of NPP-proximity

	Greens	Social-Dem.	Liberals	Conser-vatives	Others	Turnout
	(1)	(2)	(3)	(4)	(5)	(6)
NPP-Proximity	0.002	-0.028^{**}	0.006	0.010	0.011	0.004
	(0.005)	(0.012)	(0.007)	(0.014)	(0.007)	(0.008)
Months until next election	0.001^{**}	0.001	0.000	-0.002^{*}	−0.000	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Prox. × Mon.s next elec.	0.000	0.002	-0.002^{*}	0.002	-0.002^{*}	0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
County FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Counties	301	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685	3,685
R^2	0.971	0.979	0.937	0.969	0.961	0.979

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)



Bündnis/Die Grünen CDU/CSU

Figure 3.9: Marginal effect of NPP-Proximity on voting conditional on months to next election after Chernobyl

only significant in the lower third of the distribution. Figure 3.10 shows that the treatment effect for the CDU/CSU is strongest for the upper half of the adolescents' distribution and insignificant or even negative for the other one. The positive effect of NPP-proximity on conservative votes is thus driven by areas with a young population during the Chernobyl accident which insinuates that young people in those areas were mainly socialised towards *pro*-nuclear parties rather than the Greens.

3.7.3 The socio-economic dimension of the Chernobyl effect

The final mechanism I am investigating addresses how different parts of society were responding to the Chernobyl disaster and living closer or farther away from a nuclear power plant. For this analysis, I look at effect heterogeneity along two important socio-economic dimensions – economic well-being and education – which I can measure before the treatment at the county level. Table 3.11 shows the results of the baseline specification after interacting NPP-proximity with the population share of benefit recipients before April 1986. Doing so only decreases the coefficient for the conservatives but leaves the remaining parties unaffected. The marginal effect plots in figure 3.11 for the parties benefiting from NPP-proximity illustrate that the treatment effect on both Greens and CDU/CSU is strongest in the middle of the distribution even though the marginal effects are never significant. Overall, differences in economic well-being seem to have little explanatory power for heterogeneity in the proximity effect.

Given that these results could also reflect differences in educational attainment, the analysis proceeds with exploring the heterogeneity coming from variation in the level of education. Unlike age and economic well-being, education seems to be a dimension which only increases the effect from NPP-proximity for the conservative parties. Looking at the non-interacted coefficients on NPP-proximity in table 3.12, the gains of the CDU/CSU from nuclear plants after Chernobyl turns negative for (hypothetical) counties with no children attending preparatory school, while the coefficient on the Green party barely changes. The fact that the baseline effect is shifted far more towards the conservatives is, according to the estimates in table 3.12 mostly a result of county differences in educational attainment. The higher the share of students in preparatory schools, the more the CDU/CSU is gaining from NPP-proximity and the more social democrats and liberals are losing from proximity to the nearest NPP. In the marginal effect plots in figure 3.12, one can see that the marginal effect for the Greens is almost invariant to educational attainment unlike the conservatives who see their effect strongly rise in counties of higher education.

Table 3.10: Effect heterogeneity of NPP-proximity depending on age

	Greens	Social-Dem.	Liberals	Conser-vatives	Others	Turnout
	(1)	(2)	(3)	(4)	(5)	(6)
NPP-Proximity	0.028	0.062	0.035	-0.228^{***}	0.103^{***}	-0.017
	(0.027)	(0.050)	(0.032)	(0.063)	(0.037)	(0.035)
Prox. × % aged 15-25 pre-Cherno.	-0.153	-0.487	-0.211	1.455^{***}	-0.605^{**}	0.137
	(0.170)	(0.314)	(0.204)	(0.406)	(0.239)	(0.214)
County FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Counties	301	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685	3,685
R^2	0.968	0.979	0.936	0.975	0.956	0.974

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout (all as of 1983 and interacted with election FE)

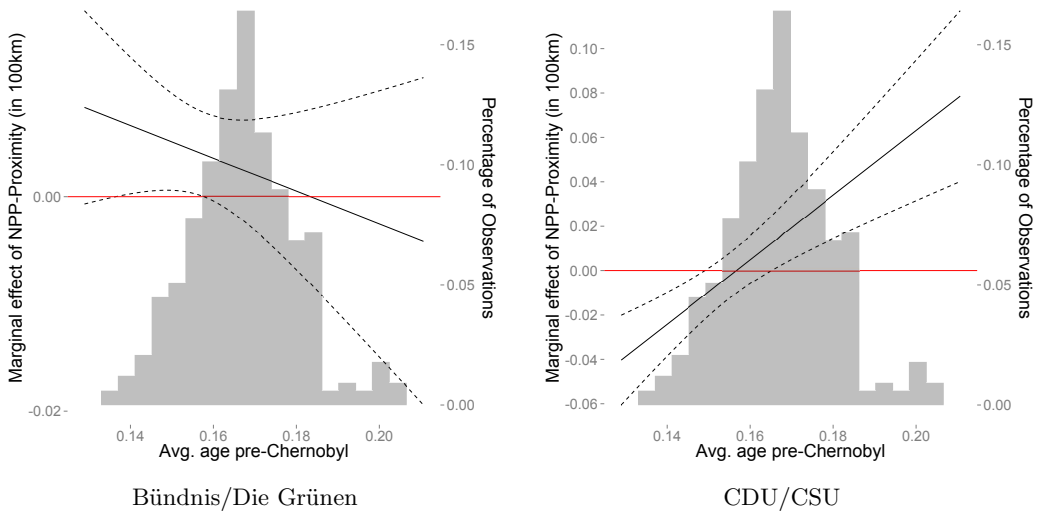


Figure 3.10: Marginal effect of NPP-Proximity on voting conditional on % ages 15-25 before Chernobyl

Table 3.11: Effect heterogeneity of NPP-proximity depending on economic well-being

	Greens	Social-Dem.	Liberals	Conser-vatives	Others	Turnout
	(1)	(2)	(3)	(4)	(5)	(6)
NPP-Proximity	0.004	−0.019	0.004	0.002	0.009	−0.001
	(0.007)	(0.013)	(0.007)	(0.014)	(0.007)	(0.008)
Prox. × % Benefit recip. pre-Ch.	−0.050	0.107	−0.158	0.336	−0.234	0.353
	(0.343)	(0.561)	(0.338)	(0.624)	(0.371)	(0.378)
County FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Counties	301	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685	3,685
R^2	0.968	0.979	0.936	0.975	0.956	0.974

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout (all as of 1983 and interacted with election FE)

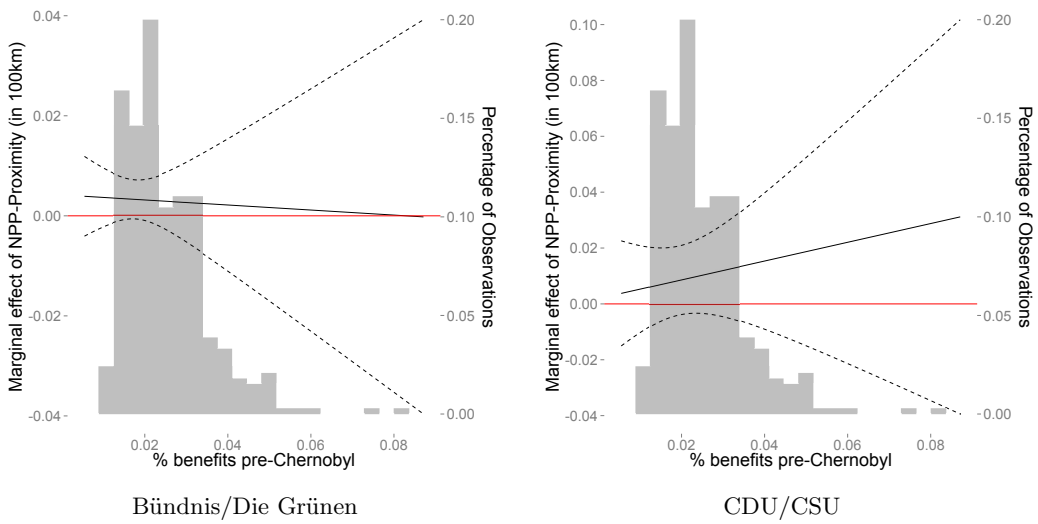
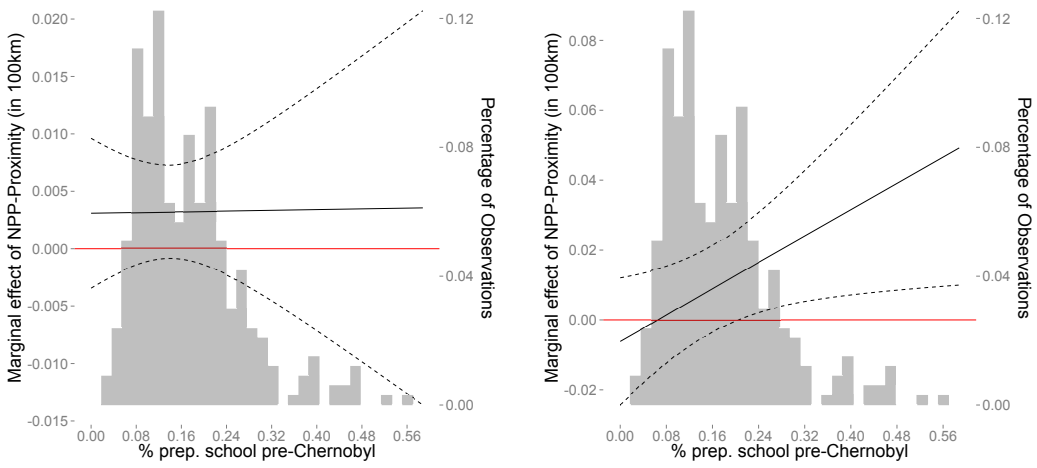


Figure 3.11: Marginal effect of NPP-Proximity on voting conditional on % benefit recipients before Chernobyl

Table 3.12: Effect heterogeneity of NPP-proximity depending on education

	Greens	Social-Dem.	Liberals	Conser-vatives	Others	Turnout
	(1)	(2)	(3)	(4)	(5)	(6)
NPP-Proximity	0.003	−0.012	0.007	−0.006	0.008*	0.001
	(0.004)	(0.010)	(0.004)	(0.011)	(0.005)	(0.005)
Prox. × % Prep. school pre-Cherno.	0.001	−0.034	−0.040*	0.094*	−0.021	0.027
	(0.023)	(0.046)	(0.022)	(0.052)	(0.028)	(0.027)
County FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Counties	301	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685	3,685
R^2	0.968	0.979	0.936	0.975	0.956	0.974

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout (all as of 1983 and interacted with election FE)



Bündnis/Die Grünen CDU/CSU

Figure 3.12: Marginal effect of NPP-Proximity on voting conditional on % prep. school before Chernobyl

Without an in-depth analysis, one can only speculate about the reasons for the patterns described in this section. It seems, however, that polarisation from NPP-proximity after Chernobyl consists of two different effects: higher votes for the Green party may actually be the result of increased environmental concerns which would explain to some extent the losses of the ideologically close social-democrats. Given the results on education and share of benefit recipients, this effect seems to be independent of socio-economic characteristics. The treatment effect on the CDU/CSU, in turn, was shown to originate from counties with a high levels of education. If one also takes into account the positive impact of adolescents on the conservative gains after Chernobyl, one can conclude that comparatively young and educated counties near a nuclear power plants were switching their votes more towards the conservative parties after Chernobyl. A possible explanation for this could be fear of declining economic prosperity after a shut-down of the nuclear facility or different level a backlash caused by the hysteria about the safety of NPPs in Germany immediately after the Chernobyl disaster.

3.8 Conclusion

This paper investigated the effect of an experience with a mainly psychological impact on political beliefs in a distant country. In a case study, I looked at the electoral response to the Chernobyl disaster in West Germany and analysed how counties located closer or farther away from the nearest nuclear power plant responded in Bundestag elections after April 1986. The disaster can be regarded as a formative experience since nuclear energy and awareness of its dangers were not a salient political issue at this time and only opposed by the small Green party and minor groupings. At the political level, I find a small, significant impact of NPP-proximity after April 1986 on Green party vote which is in line with research on the NIMBY effect. The results, however, also indicate a general and long-lasting polarising effect and highlight that the party benefiting the most from proximity were in fact the conservatives. Since they were the only pro-nuclear party after Chernobyl, this vote can be interpreted as a signal of support for nuclear energy.

In terms of mechanisms, I demonstrate that the role of Chernobyl as an experience only materialises in the gains of the conservative CDU/CSU who are particularly benefiting from proximity in areas with a larger share of 15-25 year olds. Further analysis reveals that counties with high educational attainment counties exhibit a similar pattern. The results are similar to findings on the the 1976 California primary elections in which particularly educated citizens voted against an

exit from nuclear energy. Economic differences across areas do not explain the intensity of this proximity effect. The results can be reconciled by the fact that knowledge about an NPP's actual danger may lower risk perception and turn citizens attention more towards the economic consequences of abandoning nuclear energy. This is in line with economic research on the relation between education and risk assessment [Shaw, 1996].

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3.A Tables

Table 3.13: The effect of NPP-proximity on other parties (Standardised coefficients)

Vote share	Greens	Social-Democrats	Liberals	Conservatives	Others
	(1)	(2)	(3)	(4)	(5)
NPP-Proximity	0.034*	−0.053***	−0.040	0.058***	−0.011
	(0.018)	(0.016)	(0.026)	(0.019)	(0.018)
County FE	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Counties	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685
R^2	0.970	0.979	0.936	0.969	0.961

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)

Table 3.14: Baseline results using pre-treatment controls only

	Greens	Social-Dem.	Liberals	Conservatives	Others	Turnout
	(1)	(2)	(3)	(4)	(5)	(6)
NPP-Proximity	0.003	−0.017***	0.001	0.008	0.005	0.005
	(0.003)	(0.006)	(0.003)	(0.007)	(0.004)	(0.004)
County FE	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Counties	301	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685	3,685
R^2	0.968	0.979	0.936	0.975	0.956	0.974

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout (all as of 1983 and interacted with election FE)

Table 3.15: Baseline results and different functional forms

	Green party vote share			
	(1)	(2)	(3)	(4)
NPP-Proximity	0.004*	−0.002	−0.008	−0.024
	(0.002)	(0.007)	(0.014)	(0.032)
NPP-Proximity^2		0.006	0.016	0.064
		(0.006)	(0.026)	(0.095)
NPP-Proximity^3			−0.006	−0.059
			(0.013)	(0.107)
NPP-Proximity^4				0.020
				(0.040)
County FE	Y	Y	Y	Y
Election FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Counties	301	301	301	301
Observations	3,685	3,685	3,685	3,685
R^2	0.970	0.971	0.971	0.971

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)

Table 3.16: Memory effect of NPP-proximity (detailed)

	Greens			Conservatives			Turnout		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
NPP-Proximity	0.004*	0.002	0.002	0.020***	0.013*	0.010	0.009***	0.007	0.004
	(0.002)	(0.003)	(0.005)	(0.006)	(0.007)	(0.014)	(0.003)	(0.004)	(0.008)
Late election		0.010***			0.004			0.006*	
		(0.003)			(0.008)			(0.004)	
NPP-Proximity × Late election		−0.001			0.026*			0.004	
		(0.005)			(0.015)			(0.007)	
Months to next election			0.001**			−0.002*			−0.001
			(0.001)			(0.001)			(0.001)
NPP-Proximity × Months to next election			0.000			0.002			0.001
			(0.001)			(0.002)			(0.001)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Election FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	301	301	301	301	301	301	301	301	301
Observations	3,685	3,685	3,685	3,685	3,685	3,685	3,685	3,685	3,685
R^2	0.970	0.972	0.971	0.969	0.969	0.969	0.978	0.978	0.979

Notes: Standard errors clustered at the precinct level in parantheses, *p<0.1; **p<0.05; ***p<0.01; Notation: all displayed coefficients are interacted with a *postChernobyl* dummy, which is omitted from the table for better visualisation; Controls: Log(population); Log(population)^2; % Female; % Population aged 15–25, 25–30, 30–40, 40–50, 50–65, ≥65 by gender; % Recipients of social benefits; % Pupils of prep school in age cohort 0–15; Proximity to closest university; % Turnout 1983 (all interacted with election FE)