Going Viral:
Protests and Polarization in 1932 Hamburg*

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Abstract

Political polarization is a growing concern in many countries. Are mass protests merely a sign of increasing cleavages, or do they polarize societies? In this paper, we estimate the impact of Nazi marches in 1932 Hamburg, using granular data from 622 voting precincts during 6 elections. We show propaganda can persuade – but it does by raising the share of areas with high levels of Nazi support. Importantly, marches can also backfire, repelling voters. Thus, protest marches lead to polarization. These effects diffused through social networks, measured as contagion patterns across neighborhoods from the 1918 Spanish flu outbreak. The electoral effects of social spillovers are of similar importance as direct exposure, and grow over time.

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"Better than 10 meetings, 1,000 posters and 10,000 pamphlets are mass rallies in the open air."

-Internal Memo of the Nazi Party, 1932[1]

1 Introduction

In recent years, political polarization has increased around the globe. Disagreements over policy issues have deepened, a trend particularly visible in the US and five other countries out of the 12 examined by Boxell et al. (2020). In the US, supporters of the two main parties display "affective polarization", viewing each other with dislike and distrust (Iyengar et al. 2019; Haidt 2012).[2] At the same time, protests and rallies by extremists groups have grown explosively: From the far-right "Unite the Right" rally in Charlottesville in 2017 to the recent anti-immigrant and anti-Muslim 'Pegida' demonstrations in Germany, almost every developed country has recently witnessed gatherings of extremist groups. Ortiz et al. (2022) document that the number of mass protests has more than tripled between 2006 and 2020.[3]

Rallies and protests could merely be signs of growing cleavages in society, or they could be key drivers of radicalization. Overwhelmingly, the literature on "campaign effects" has found only minimal effects of political agitation on *average* voting behavior (Finkel 1993; Lazarsfeld, Berelson, and Gaudet 1968; Bartels 1985; Brady, Johnston, and Sides 2006; Kalla and Broockman 2018; Le Pennec and Pons 2019). While some forms of media exposure and public "shows of strength" have electoral consequences (DellaVigna and Kaplan 2007; Madestam et al. 2013), persuasive effects of canvassing or campaign advertisements on actual voting itself appear to be generally small or short-lived (Bennett and Iyengar 2008; Gerber et al. 2011; Broockman and Kalla 2023).[4] El-Mallakh (2020) presents evidence of backlash after Arab Spring protests. An alternative interpretation is that campaigning creates growing support from a group's core constituency, while repelling other voters. In other words, "backlash" may lead to low average effects, but

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[1] Quoted according to Noakes (1971).

[2] Half of all Republicans and one third of Democrats are opposed to marrying someone from the other political camp, when the corresponding number sixty years ago was almost zero (Wilson et al. 2020). Across Western Europe and in the UK, similar trends are visible (Vachudova 2019; Down and Wilson 2010).

[3] Cantoni et al. (2024) document an explosive growth of protests after 2011.

[4] Broockmann and Kalla (2016) find effects of door-to-door canvassing on reducing transphobic attitudes. However, this study does not provide evidence that this impacts actual voting behaviors. In general, turnout seems more susceptible to campaigns than political support for particular parties.

could be a key driver of *polarization.*[5]

In this paper, we use unique, high-frequency data on voting and demonstrations from interwar Germany to examine the direct and indirect effects of political demonstrations by the Nazi party. In contrast to recent research casting doubt on the effectiveness of Nazi election propaganda (Selb and Munzert 2018), we find that mass marches of Nazi supports through the streets of Hamburg increased average support. They also had a polarizing effect, with some areas turning away from the Nazis after being exposed to their marching columns – while others voted for the party in greater numbers.[6] In the run-up to Hitler seizure of power in 1933, German national politics became increasingly dominated by extreme parties. While the old "Weimar coalition" parties, broadly representing the bourgeois middle of the political spectrum, withered from 1930 onwards, the two extremes – the Communist Party and the Nazi Party – grew in strength. By 1932, they received more than half of all votes; no democratic government with a parliamentary majority could be formed. Growing social, economic, and political cleavages thus preceded the 1933 "seizure of power" (Bracher 1978).[7]

We focus on Hamburg, an old Hanseatic trading city and major industrial port, where unusually rich data exist – and broad political trends mirror those in Germany as a whole. Voting in Hamburg became markedly more polarized in 1932. Figure 1 shows the distribution of vote gains for the Nazi Party across Hamburg, for elections before and after two major rallies and marches. After the marches, the distribution became increasingly bimodal as initial differences in attitudes hardened and became magnified. We argue that the Nazi marches were responsible for this growing polarization, with marked increases in support in some electoral groups – and a backlash in others. Both the direct effect of propaganda marches and their indirect effect via social spillovers drove the increasing chasm in voter attitudes.

We first document which households were directly exposed to propaganda marches, using detailed information on Nazi marching routes derived from police records. Our difference-in-differences approach allows us to trace the persuasion effect of propaganda over time: Voting districts with many households directly exposed to the marches saw

[5] Along similar lines, Baysan (2022) finds polarizing effects of campaigning in modern-day Turkey.

[6] More specifically, we find a positive impact of the marches in the upper and negative treatment effects in the lower tail of the Nazi vote share distribution. We interpret the disparate effect of marches on different moments of the Nazi vote distribution as evidence of polarization.

[7] We also explore this broader notion of polarization, i.e. the divergence of political attitudes away from the center, towards ideological extremes in our discussion section.

greater increases in Nazi support than similar, unexposed districts. The further a polling station was from the marching route, the smaller the gains. Average effects are large: compared to the least exposed areas, Nazi vote share increased by almost 2 percentage points along the marching path, or 21% more than the average Nazi increase.

Our second main finding links Nazi marches with increased polarization. Using a distribution regression approach, we find evidence that persuasion from exposure was particularly strong in strengthening the upper tail of the NSDAP vote share distribution. In the lower tail, we find no evidence of persuasion. Instead, we detect a backlash, with support for the Nazi Party decreasing as a result of exposure to the marches. The disparate impact of marches on voting, i.e. the combination of persuasion and backlash, lead to increasing polarization in voters' attitudes towards the Nazis *across* neighborhoods within the city.

Our third main contribution is to demonstrate that the marches produced sizeable social spillovers. These are normally difficult to pin down, as they elude standard diff-in-diff approaches. To capture social links and personal interactions, we exploit data from the spread of the 1918 "Spanish flu." The influenza virus is airborne and spreads through personal contact. We measure excess mortality during this period and construct an index of connectedness between neighborhoods. Our measure of connectedness is largely uncorrelated with physical distance. We show that support for the Nazis spread from the area "treated" by the marches to the rest of the population via social interactions, just as the flu had 14 years before; areas untreated by marches also swung strongly towards the Nazis if they were connected to "treated" areas that had had similar timing of the 1918 flu outbreak.[8] The indirect effect of propaganda spreading through social interactions accounts for up to half of the overall effect of the propaganda marches and lasts for at least twelve months. Just as with direct effects, indirect effects also led to polarization, with little or negative effects at low NSDAP vote share thresholds, and strong, positive ones on the upper tail of the NSDAP vote share distribution.

On net, marches were effective in persuading the population of Hamburg of the Nazi Party's political appeal. This effect was most visible in areas affected by propaganda marches, and those precincts indirectly connected to that area. Both growing support and backlash spread across space and over time, increasing polarization in voters' attitudes

[8] In our baseline results we scale indirect exposure by the change of Nazi support in connected neighborhoods, to account for the heterogeneous effect of the direct treatment. In a robustness exercise we consider a simpler, unscaled version of indirect exposure and find similar results.

towards the Nazis. In combination, our evidence demonstrates that public demonstrations by an extreme political movement can be both persuasive and divisive, with social interactions operating as an amplification mechanism, leading to greater polarization of the electorate overall.

We contribute to the growing literature on the determinants and consequences of protests and demonstrations (see Cantoni et al. (2024) for a recent review). There is already evidence that the overall persuasion effort (total participants of demonstrations, say) is subject to strategic considerations among potential participants in general (Cantoni et al. 2019), and that social networks in particular can play a key role for sustained mobilization (Bursztyn et al. 2021). Similarly, people's connections with each other via Facebook and similar links can affect protest participation (Enikolopov, Makarin, and Petrova 2020), as well as economic decisions more broadly (Bailey et al. 2018). In contrast, we do not study the determinants of participation in mass events, but the consequences for the broader population. While there is evidence that protests and demonstrations can be highly effective in persuading voters and policy-makers (Madestam et al. 2013), the mechanisms behind this finding are not well understood.

The literature on support for extremist movements has emphasized bandwagon effects and "pluralistic ignorance", where public signals become self-reinforcing, in particular when actions are observable (Bursztyn, Egorov, and Fiorin 2020; Abel and Childers 1986). Our main outcome of interest is voting – a private action. In this sense, our paper contributes to the rich literature studying political persuasion. This literature has mostly focused on the effects of exposure to particular messages via mass media. Examples include studies on Fox News or Berlusconi-owned TV stations in Italy (DellaVigna and Kaplan 2007; Durante and Knight 2012). This literature has demonstrated that differential exposure to news can persuade people and lead them to change their beliefs or behavior after exposure (DellaVigna and Gentzkow 2010). In our context, Adena et al. (2015) demonstrate the persuasive effects of pro-Weimar and pro-Nazi radio broadcasts. We also know that persuasion efforts via protests and demonstrations can create such effects, just like media exposure (Lohmann 1994; Madestam et al. 2013; Gillion and Soule 2018; Mazumder 2018; Wasow 2020). Our analysis builds on a recent set of papers demonstrating spillover effects in social networks

(González 2020).[9] Such diffusion effects are anticipated by a rich theoretical literature on social learning, which predicts that private beliefs and actions can be influenced through network connections (e.g. Golub 2017). Indeed, we detect that as Nazi propaganda spread, it gave rise to a quantitatively important "persuasion multiplier". To the best of our knowledge, our study is the first example of such effects of protests spreading through social networks over time.

Our evidence also speaks to the literature on political polarization. Polarization has risen sharply in most countries in recent decades (Iyengar et al. 2019). In many models with information provision and learning, polarization should not occur – in both Bayesian and non-Bayesian approaches, agents should eventually converge to a shared "truth" (Blackwell and Dubins 1962; DeGroot 1974). Recent theoretical work by Gentzkow et al. (2021) argues that differences in trust of information sources can create divergent opinions. Using Turkish data, Baysan (2022) shows that political campaigning can increase voting for a party in one area while undermining it in another, depending on the level of underlying support. Similarly, Enikopolov et al. (2023) find polarizing effects of access to independent media in Russia. Closest to our context, Adena et al. (2015) show that radio propaganda by the Nazis can persuade or backfire depending on listeners' priors about the message. Our results demonstrate that protest marches led to polarization of the electorate; both direct and indirect exposure via social networks can lead to growing cleavages in political orientation.

Finally, we contribute to the large literature on the rise of the Nazi Party in interwar Germany. While initial research either emphasized economic motives or the effects of Hitler's charisma, research in the last 30 years has demonstrated the Nazi Party's broad appeal across social groups, and its role as a "party of protest" (Bullock 1994; Falter 1991; Arendt 1973). Other scholars have emphasized that the rise of the Nazi party was part of broader-based polarization in Weimar society, with anti-democratic parties holding a majority of seats from the summer of 1932 onwards (Bracher 1978). A small literature has argued that propaganda and effective campaigning were important for the Nazi party's rise (Kershaw 1983; Adena et al. 2015), and that clubs and associations played an important role in increasing membership (Satyanath, Voigtländer, and Voth 2017). At the same time, empirical support for this supposition is distinctly mixed (Selb and

[9] Related work has emphasized that social networks can act as coordinating devices (Arias et al. 2019; Battaglini, Morton, and Patacchini 2020; Manacorda and Tesei 2020; Enikolopov, Makarin, and Petrova 2024). There is also evidence that social connections can influence voter turnout (Foos and de Rooij 2017).

Munzert 2018).[10] We are among the first to provide clear-cut evidence that one of the key propaganda tools used by the Nazi Party before it seized power was highly effective, partly because social interactions magnified its impact.

2 Historical Background

In this section, we briefly summarize the historical background and context of our study.

2.1 The rise of the Nazi Party. The Nazi Party had its origins in Munich, where its immediate predecessor was founded in 1919. With few members and limited funds, it played only a small role in national and Bavarian politics until 1923. Then, its leaders attempted a coup in Munich – the so-called "Beerhall Putsch". It quickly collapsed in a hail of police bullets; many leading Nazis fled. Hitler himself was arrested, tried, and convicted. The Nazi Party was declared illegal (Kershaw 2001).

In prison, Hitler wrote "Mein Kampf" ("My struggle"), and received a string of prominent right-wing visitors. The Nazi Party was legalized once more in 1925 and contested the subsequent elections. In 1928, it polled 2.8% of the national vote. What transformed its electoral fortunes was agitation in 1929 against the Young Plan – a rescheduling of Germany's reparations debt in exchange for foreign loans. In September 1930, the Nazis received 16% of the vote, making it the 2nd largest party (Evans 2006).

As the Great Depression worsened, support for liberal democracy declined. In particular, extremist parties grew in strength, while the bourgeois middle saw its electoral support dwindle. Unable to command a parliamentary majority, the federal government ruled by presidential decree (Bracher 1978). When Germany went to the polls in 1932, unemployment was close to 6 million (25%) (Dimsdale, Horsewood, and van Riel 2006). Foreign trade had collapsed, and incomes had fallen sharply since 1929. Firms and farmers struggled under the ever-growing burden of debt as deflation took hold (James 1986). While the unemployed themselves rarely voted for the Nazi party, small owner-proprietors and salaried employees threatened by economic collapse frequently did (Falter 1991; Falter and Hänisch 2013).

March and April 1932 saw two rounds of voting for the president of the republic. The incumbent, Field Marshall von Hindenburg, only narrowly defeated Hitler in the run-off. In the parliamentary election in July 1932, the Nazis scored their best result in a free

[10] A related literature in political science examines campaign effects. It has rarely found evidence for their empirical importance. One of the leading interpretations emphasizes spillovers, making treatment effects harder to identify (Hillygus 2010).

election, polling 37.2% of the vote. Polarization at the polls was an important driver of political gridlock: After July 1932, Nazis and Communists together held an absolute majority of seats, making a democratic majority government impossible. Hitler was confident of becoming Chancellor. However, the aging president's distaste for the 'little corporal' prevented a Nazi government in the summer of 1932 (Kershaw 2001).

In November 1932, votes for the Nazi Party declined, and the President appointed a cabinet without Nazi ministers. By December, many observers thought the Nazis would never come to power (Turner 1997). However, in late January 1933, conservative advisors convinced the elderly president to appoint Hitler as Chancellor. As soon as the Nazis were in office, they used control of the police to suppress the opposition. The Hitler government held one more parliamentary election – in March 1933 – and then dismantled German democracy (Falter 2020). By the summer of 1933, all parties except the Nazi Party were banned, and all unions abolished. Joseph Goebbels, the newly appointed Minister for "Propaganda and People's Enlightenment", controlled all media, from print to radio, and closely supervised the music and film industry.

2.2 The Nazi Party in Hamburg. Hamburg was a traditional center of the German labor movement – "Red Hamburg". After 1918, communists and social democrats together often received half of the vote. The rise of the Nazis in Hamburg therefore created deep divisions.

The Nazi party's electoral fortunes in Hamburg broadly followed trends at the national level. After receiving only 2.6% of the vote in Hamburg in 1928, a period of internal power struggles followed (Büttner 1982). The party had sought to recruit workers for the Nazi cause, expressing opposition to capitalism. This policy failed. The party recruited few workers. After 1929, under its new leader Karl Kaufmann, the party in Hamburg shifted towards a more 'bourgeois' message (Brustein 1998; Büttner 1982). This was in line with policy in the Reich as a whole. Kaufmann entered into close contact with local captains of commerce and industry, and promoted cooperation with the arch-conservative DVNP party as well as the veterans organization *Stahlhelm* (literally, steel helmet). Party propaganda dropped its earlier emphasis on socialist ideals.

Under Kaufmann's aegis, the Nazi Party in Hamburg also collaborated with civic organizations like the property owners association, participating in rallies and assemblies for specific causes. By the late 1920s, the Nazi Party owned two local newspapers – the *Hansische Warte* and the *Hamburger Tageblatt*. In Hamburg as elsewhere, the party's breakthrough came in 1930, following agitation against the Young Plan. By the end of July 1932, the party received the highest share of the vote yet – 33.7%.[11] Given the strength of the labor movement in the city, this was an impressive showing.

2.3 Nazi propaganda and marches. Before 1933, the Nazi party had access to few propaganda tools. Radio was state-controlled and hence, closed to the party message (Adena et al. 2015); Nazi newspapers were frequently shut down or censored. Door to door canvassing, as well as personal campaigning by its leaders, became crucial for the party's success. As Eugen Hadamovsky (1933), Goebbels' deputy, put it: *"He [Hitler] had no choice but to reach the masses directly through constantly growing mass rallies."*

In the run-up to every election, the Nazi party organized tens of thousands of events across the country (Kershaw 2001). Its local chapters were in charge of recruiting and day-to-day propaganda, using local speakers, often trained via correspondence courses (O Broin 2016). Regular meetings and speeches by local members mobilized party members and potential recruits in normal times. Election time saw a massive rise in propaganda activity. Hitler himself put in 243 campaign stops in 1932 alone (Selb and Munzert 2018). In the final years of the Weimar Republic, the Nazi Party increasingly used mass rallies to sway voters. By 1932, the party had realized through experimentation and close observation of electoral outcomes that mass gatherings were particularly effective tools of electioneering (Noakes 1971):

> *"Better than 10 meetings, 1,000 posters and 10,000 pamphlets are mass rallies in the open air. …For example, in Hanover, we got hundreds of thousands out. …* ***On the day of the election its effect showed in the marked increase in votes at the polls near the site of the rally.****"* (emphasis added)

Casual empiricism thus laid the foundations for a hallmark tool of Nazi propaganda. Rallies, marches, and assemblies were banned before and during the federal presidential election of 1932; the ban was only lifted after April 10. In Hamburg in 1932, the Nazi Party organized two big marches in April 1932, on the 17th and 20th. The overall number of participants was 13,000, including 9,300 Nazi Party members. Thousands of spectators lined the marching route. The second march was held after dusk and illuminated by the torches carried by participants (Hamburger Nachrichten, 1932b). Marches, rallies and demonstrations increased the party's appeal amongst the bourgeoisie by conveying an

[11] Figure A.1 in the appendix plots electoral results in Hamburg and nation-wide side-by-side.

image of discipline, order, and strength.

> *"...mass demonstrations and marches in the streets drove out rational discourse... The marching columns ... conveyed order and dependability as well as ruthless determination. Banners and flags ceaseless activism and idealism."* (Evans 2004)

This contrasted favorably with the perceived weakness of Weimar democracy. As one historian of the Nazis' rise to power argued: [Nazi] "forms of military pageantry proved very successful in a highly nationalistic, but largely demilitarized, country" (Fischer 2002). Bystanders often recall an almost mystic appeal of witnessing Nazi marches, creating a deep emotional bond with the party and its cause. As one young woman recalled:

> *" 'We want to die for the flag', the torch-bearers had sung... I was overcome with a burning desire to belong to these people ... I wanted to escape from my childish, narrow life and ... attach myself to something that was great and fundamental."* (Evans 2004, p. 313)

In the run-up to the federal election in July 1932, marches were banned once more in Hamburg. Frequent, violent street fighting accompanied many demonstrations during the Weimar Republic, motivating a ban. Marches often targeted Communist strongholds, seeking to create conflict. Indeed, a bloody confrontation between Communists and Nazis in Altona – a suburb of Hamburg but officially a part of Prussia – demonstrated how easily violence could break out. There, communist youth attacked a march of 7,000 storm troopers through a working-class neighborhood in July 1932. Two SA men were shot; the police, heavily outnumbered, began to shoot as well. In the end, two storm troopers and 16 innocent bystanders died (Büttner 1982).

3 Data

To construct our data, we use five main sources: Police records and newspapers for the path of marches, the address book of Hamburg to geo-locate households and capture their main characteristics, polling-station level data on voting behavior, a digitization of Hamburg's road network, and mortality records from 1918 for the spread of the Spanish flu. We complement these data with newspaper articles from the *Hamburger Anzeiger* around the treatment period.

3.1 Nazi marches in Hamburg. Nazi marches on April 17 and 20 started at several locations across the city. Individual 'marching columns' then met at a mid-point and proceeded to a final assembly point, where Nazi leaders addressed the masses. Before these public events, the Hamburg storm troopers sent detailed plans of both marches to the police for approval (State Archive Hamburg 1932a; 1932b). Marching routes generally maximized the share of wide streets – marching columns appear more impressive if they involve large numbers of men moving in lockstep, and in wide streets, more spectators can witness the spectacle.[12]

We digitize the complete route of each of the marching groups from these archival documents and newspapers of the time (Hamburger Nachrichten 1932a; 1932b; Hamburger Tageblatt 1932). We verify the routes recorded by the police with the accounts of several newspapers. This ensures that measurement error in our explanatory variable is minimal. Figure A.3, Panel A shows the paths of the two marches.

3.2 Voting data. Elections until March 1933 were free and fair. It is only from March 1933 onwards that intimidation at the polling booth began to play a role (Evans 2006). Each household in Hamburg was assigned to a polling station. In 1932, the city had 756 polling stations in total, 622 of which were located in the city itself (meaning in one of the 17 boroughs of the inner city of Hamburg).[13] We match each household to its polling station using the official bulletins that assign each address (street and house numbers) to an electoral district.

The average polling station saw around 1,300 valid votes cast, with a range of 488 to 1,943. Invalid votes were few, less than 1% of all votes. For each of the polling stations in the city itself, we digitize the full election returns from the statistical bulletin of Hamburg for the two presidential elections of 1932 (first round: 13 March, runoff: 10 April), and for the *Reichstag* elections held on 14 September 1930, 31 July and 6 November 1932 and 5 March 1933 (Sköllin 1930; 1932a; 1932b; 1933). We geo-locate each of these polling stations using their exact address. In Figure A.3, Panel B, we mark each polling station with a black cross and show their location on a map of Hamburg.

Electoral district boundaries were fairly stable in 1932 and 1933, but the exact location where citizens cast their votes underwent occasional changes. Around 10% of polling stations changed their location between March 1932 and March 1933 at least once;

[12] Only 11% of Hamburg streets were "wide" (i.e. broader than 20 metres). However, 19% of streets used by the marches were wide (and 36% were wider than 16 metres, vs 25% of all streets in Hamburg).

[13] We consider polling stations in the city proper those located in the following districts: Altstadt, Barmbeck, Billwerder Ausschlag, Borgfelde, Eilbeck, Eimsbüttel, Eppendorf, Hamm, Harvestehude, Hohenfelde, Horn, Neustadt, Rotherbaum, St. Georg, St. Pauli, Uhlenhorst, Winterhude. We also discard polling stations farther than 2km from one of the two marches: these are 22 polling stations in all, located in rural areas surrounding Hamburg.

they never moved by more than 500 meters. To our knowledge, voters were not re-assigned to different polling stations as a result of these changes. Because our measures of exposure are computed from voters' addresses, these changes should not affect our analysis. Nonetheless, we show in Table A.1 that results are unaffected when we drop the 62 polling stations that changed location. 107 polling stations underwent major re-assignments of voters to different polling stations between 1930 and 1932. Most of these changes in the south-eastern districts of Hamm (47) and Billwerder Ausschlag (29) and to a lesser degree in the north-eastern districts of Winterhude (11) and Barmbeck (11). Due to major re-assignments of voters in these polling stations, we cannot map the voting outcomes of the 1930 election to the 1932 and 1933 outcomes for the full sample. Hence, we only use the 1930 election results to test for long pre-trends in an unbalanced panel.

3.3 Household-level data. As in every major German city during the interwar period, annual address books were published for Hamburg (*Hamburger Adreßbuch* 1932). In the days before telephones were common, these address books provided detail on who lived where, as well as useful information on the location of administrative offices, school districts, opening hours, and the like. We digitize the entries for every household located in any one of inner Hamburg's 17 districts; the 400,000 digitized entries represent the full population of households. Figure A.3, Panel B shows a map of Hamburg with all the household locations in our data, together with the polling stations.

For each household, we know whether they owned a telephone, had central heating at home (luxury items at the time), and in the majority of cases, the occupation of the head of household and his/her surname. We classify households into 33 sectors of occupations and into 9 occupational standing categories following the classification scheme of the 1933 census (Statistisches Reichsamt 1933). We infer the regional origin of households from their surnames, using the distribution of surnames in the German telephone book of 2015 (*Das Telefonbuch Deutschland* 2015).[14]

We measure *direct exposure* to Nazi marches at the polling station level in two ways: First, we calculate the distance to the closest of the two marches for each household, and calculate the average distance of households of every polling station. Second, for every polling station we compute the share of households living within 200m from one of the marches. The 200m threshold is arbitrary but reasonable – these households could either

[14] This is the earliest available edition of the German telephone book. The most common regional surnames we find in the 1932 Hamburg address book are surnames that in 2015 are also distinctive of the city of Hamburg. This gives us confidence that the regional distribution of German surnames has remained relatively stable over time.

observe the march or would notice it because of its proximity. In the robustness section we show that alternative thresholds yield similar results. Figure 2 shows a map of households that were directly exposed according to the second method (indicated in red). This is not to say that households that lived further away than 200m did not observe the march; but the probability must have declined with distance, potentially in a nonlinear fashion.

3.4 Street network. We digitize a historical map of Hamburg for 1932, drawn on a scale of 1:5,000 and provided by the *Landesbetrieb Geoinformation und Vermessung Hamburg* (2020). This allows us to reconstruct the street network of 1930s Hamburg. To this end, we geo-locate all streets of inner Hamburg using ArcGIS. This gives us a street network of 1,381 streets (polylines) in inner Hamburg with information on their 1930s street name, geographic location, start- and endpoints, intersections with other streets, street length and street width.

3.5 The Spanish flu in Hamburg. The Spanish flu reached Europe in 1918. It caused approximately 50 million deaths worldwide (Breitnauer 2019). By the fall of 1918, it reached Hamburg. We collect and digitize the universe of 60,000 death records from the 1917-1919 death registers of Hamburg (State Archive Hamburg 1917; 1918; 1919). Between 17 September and 18 November of 1918, weekly death rates spiked, diverging from the 1917 and 1919 pattern – at its peak, weekly deaths ran at 350% of their average 1917/19 value (Figure A.4, Panel A). For this period of the 1918 flu peak, we digitize the full information recorded on all 3,000 death registration cards: name, date of birth, date of death, civil registry office where the death is registered and the exact address of the deceased. This allows us to match individual deaths to polling stations using the voting lists (see Voting data, above) and to create a daily panel of flu deaths by polling station. We use the co-movement of flu-peak deaths by neighborhood to construct a measure of social connectedness between different parts of the city, described in detail in Section 4.3.

4 Main results: Persuasion and polarization

Can mass demonstrations convince voters at the polls? Do they sway some voters and repel others? And are effects confined to areas exposed to propaganda, or can they spread through social networks, like a contagious virus? In this section, we test for direct and indirect effects of Nazi marches in 1932 Hamburg, as well as their effect on polarization.

4.1 Direct effects. To estimate the impact of marches on voting, we calculate polling stations' physical distance to the marches. Comparing electoral results across polling stations that are close to or far from the marches in the cross-section only identifies the causal effect of marches if proximity is orthogonal to other omitted determinants of voting. If Nazi planners chose to march through "friendly" neighborhoods to mobilize supporters, this assumption will be violated. Conversely, if Nazis paraded through Communist strongholds to intimidate and showcase their strength, this would lead to downward bias.

To address these concerns, we use a simple difference-in-difference strategy, leveraging the high frequency of elections in 1932. Figure A.2 in the Appendix summarizes the timing of key events. Two elections immediately preceded the marches – the presidential elections on March 13 and April 10. The marches themselves, on April 17 and 20 were followed by three additional elections, all for national Parliament: in July 1932, November 1932 and March 1933. We estimate the following simple difference-in-difference equation:

$$N_{it} = \alpha_i + \alpha_t + \beta M_i \times Post_t + \sum_t \delta_t \mathbf{X}_i' + u_{it} \quad (1)$$

In equation (1), N_{it} is the NSDAP vote share in polling station I in election t, α_i and α_t are polling station and time fixed-effects, $Post_t$ is an indicator for elections held after the two Nazi marches, M_i is exposure to the marches, and $\mathbf{X}_i'$ is a vector of covariates. We use two alternative measures of march exposure: the (log of) average distance to the march among household voting in a polling station and the share of households in a polling station who lived within 200 meters from a march. This second variable is our preferred measure of exposure.[15] Election fixed effect (α_t) capture broad (Hamburg-wide) changes in NSDAP voting. The inclusion of polling station fixed effects (α_i) is crucial, allowing us to control for fixed, underlying characteristics that affected Nazi support. The vector $\mathbf{X}_i'$ includes: log number of voters in the 10 April 1932 election, share of blue-collar workers, share of households with a telephone, and share of households with centralized heating.[16] In 1932, telephone and heating were available only to relatively affluent households: together with the share of blue-collar workers, these variables allow us to

[15] To the extent that people living further than 200 meters also were exposed to the marches, it will lead to a downward bias in our estimates.

[16] In a robustness check, we show that our main estimates remain largely unchanged when including additional occupational information on the share of civil servants and shopkeepers in our vector of covariates (see Table A.16).

create proxies of socio-economic status at the polling station level. All controls are pre-determined and measured before the marches took place. In the most conservative specification, we allow these characteristics to have a different effect in every election ($\delta_t \mathbf{X}_i'$). We also estimate a flexible specification of (1), where we allow the effect of exposure β to vary over time and additionally include the September 1930 national Parliament election to test for long pre-trends.

If marches mattered, we expect $\beta \neq 0$ in equation (1). A priori, the direction of average effects is not clear. If marches *persuaded* those directly exposed, we expect $\beta > 0$. However, there could also be a *backlash*: militaristic shows of force by Nazi supporters may have convinced voters that alternatives were more desirable ($\beta < 0$). If both persuasion and backlash occurred for specific sub-population, β captures the average net effect.

What effects would one expect in subsequent elections? A simple Bayesian benchmark predicts that beliefs and voting follow martingales: in the absence of new information, today's outcomes represent the best prediction for tomorrow. Thus, direct exposure to the marches shaped perceptions about the Nazi Party among the bystanders; unless information arrived differentially in the treated vs. non-treated areas, the initial effect should persist over time. This we test in the dynamic specification:

$$N_{it} = \alpha_i + \alpha_t + \sum_t \beta_t M_i + \sum_t \delta_t \mathbf{X}_i' + u_{it} \tag{2}$$

Whatever the value of β_4, measuring the impact of the marches on the first election after they took place, we expect $\beta_4 = \beta_s$ for $s = 5, 6$. Since there is no obvious alternative to test, we will provide two-sided test statistics against that null hypothesis.[17]

Our difference-in-difference approach identifies the causal effect of march exposure M_i on support for Nazi Party if, conditional on controls, voting would have evolved similarly in exposed and not exposed areas without the marches. The parallel trends

[17] That is not to say that theoretical alternatives are inconceivable. For example, it is possible that some sub-population in the directly exposed areas did not observe the marches but heard from neighbors that did. In that case, there would be some diffusion over time even within treated areas. Such communication is likely to happen relatively rapidly, and we expect this form of diffusion to be rather quick and be incorporated in the effect in $t = 4$, some three months after the marches. Alternatively, voters may be non-Bayesian, as in the model of DeMarzo et al. (2003). In their model, persuasion bias arises due to individuals failing to account for repetition of information. In our context, this corresponds to the case where the same Nazi message circulates repeatedly in treated neighborhoods, and voters are unable to fully discount this form of repetition. This type of deviation from the Bayesian benchmark implies $\beta_4 < \beta_s$ for some $s = 5,6$.

assumption would be violated if Nazi planners deliberately targeted areas with *growing* NSDAP support. Prior to the marches, there is no violation of the parallel trends assumption: In Table 2, cols. 1 to 3, we focus on the two 1932 Presidential elections, both held *before* the Nazi marches. There is no evidence that polling stations closer to Nazi marches were already experiencing faster growth in NSDAP voting before the marches, or lower vote shares for the Communists (KPD). The same is true for turnout. These results suggest that Nazi planners did not target areas according to electoral swings in recent elections.[18] Similarly, Table 2, cols. 4 to 6 tests for long pre-trends between the September 1930 and the April 1932 elections. Reassuringly, there is no evidence of faster growth in NSDAP voting and turnout even over the two-year period. However, there is some indication that vote shares for Communists experienced a small increase in polling stations closer to the Nazi march between 1930 and 1932.

To give a first, visual impression of the impact of Nazi marches, Figure 3 overlays changes in Nazi vote shares after April on a map of Hamburg; darker areas experienced greater vote gains for the Nazis. On average, areas traversed by marches saw much larger vote gains by the Hitler movement. In Figure 4, Panel A, we show a binscatter where we plot Nazi gains against the distance to the marching route: the closer the marching path, the bigger the swing towards the Nazis was. In combination, the graphical evidence suggests that areas closer to Nazi marches strongly increased their average support for the Hitler movement.

To go beyond the visual evidence, we estimate the difference-in-differences model in (1). Results are in Table 3, cols. 1-2 analyze Nazi votes as a function of log distance to the march, interacted with a post-march dummy, controlling for polling stations and election fixed effects. Although fixed effects absorb much of the variation in the data, the coefficient on distance is large and highly significant. In col. 2, we include controls interacted with election fixed effect; the coefficient on log distance to the march remains highly significant. The estimates imply that moving from 100 to 1,000 meters distance from the marching path reduced Nazi vote gains by around 0.4 p.p. This compares with an average vote gain for the Nazi party of 5.6 p.p. between the pre- and post-march elections in Hamburg overall (equivalent to 7.1% of the average increase). The estimates are also precise: the 95 percent confidence interval can rule out effects smaller than 0.2

[18] This makes intuitive sense: Marches took place seven and ten days after the second Presidential election. Since both planning and police authorization took place in advance, it is unlikely that there was enough time to process polling station-level results and adjust paths accordingly.

p.p. (4% of the average increase).

In cols. 3-4 we predict Nazi voting with the share of households living within 200m of the marches. This simple setup suggests that polling stations fully treated by the march (compared to those with zero exposure) voted 1.1 p.p. more for the Nazis ($p < 0.001$) than those without "exposure". In the most saturated specification (col. 4) the coefficient drops by 10%, but remains highly significant. It implies that direct exposure to the Nazi marches added 1 p.p. – or about 18% of the average increase in Nazi voting (95 percent interval covers effects between +0.5 and +1.6 p.p.: 9% to 28% of the average effect). In sum, the evidence is consistent with our hypothesis that the Nazi message *persuaded* voters, on average.

Next, we examine the effect of direct exposure over time. Figure 4, Panel B, plots β_t from equation (2): the impact of direct exposure in each separate election (full estimates in Table 3, col. 5). Prior to the marches, distance to marches is only weakly and insignificantly associated with Nazi voting. The effect of the marches is large and persistent: in the first federal election after the marches, polling stations closer to their path voted significantly more for the NSDAP. These gains persist well into 1933, more than 300 days later – the NSDAP continued to receive around 1 p.p. more votes in fully "exposed" polling stations.[19] Thus, we cannot reject the second null hypothesis that $\beta_4 = \beta_s$ for $s = 5,6$: direct effects appear to be time-invariant.

4.2 Polarization. So far, we have focused on the average effect of Nazi marches. However, the marches may have induced greater support in areas already leaning towards the Nazis, whereas in areas where opposition parties were strong, the effect may have been the opposite ("backlash"). In short, the marches may have led to *polarization*.

To examine this possibility, we employ a distribution regression approach. The procedure maps the direct effect along the cumulative distribution functions (CDF) of the Nazi vote share.[20] We estimate a linear probability model for the likelihood that the vote share is above a particular level x. For each dummy N_{itx}, indicating whether the

[19] Point estimates suggest that more than 80% of the effect in the last three elections appeared immediately after the marches. Formal tests can not reject the null that the effect is identical in every period after the marches (see Table 3, col. 5). Confidence intervals grow slightly overtime but remain tight even in the last election: 95 percent interval for β_6 is +0.3 - +1.8 p.p.

[20] The distribution regression approach is appealing due to its simplicity. Previous applications include Duflo (2001), who estimates the difference-in-differences impact of school construction on the CDF of years of education. Our approach is analogous to hers. For a discussion on the relationship between distribution regressions and quantile regressions, see Chernozhukov et al. (2013).

neighborhood I, at election t, is above the Nazi vote share x, we estimate

$$N_{itx} = \alpha_i + \alpha_t + \beta_x M_i \times Post_t + \sum_t \delta_t \mathbf{X}_i' + u_{itx} \quad (3)$$

where x ranges from the 5[th] and 95[th] percentile of N_{it}, in steps of two percentage points. We plot the effect of exposure to the marches in Figure 5. In Nazi-leaning neighborhoods, with a pre-treatment level of support above 29%, the marches boosted support. In contrast, in areas where opposition parties were strong, there is no evidence that the marches were persuasive. If anything, the estimates indicate a Nazi backlash, with point estimates consistently below zero. We test the hypothesis whether the direct effect is positive ("persuasion") above or negative ("backlash") below a crossing point of 29%. Formally, for the backlash hypothesis we test that all β_x for x < 29% are jointly different from zero. To allow for the joint test, we estimate a seemingly unrelated regression (SUR) model as introduced by Zellner (1962) consisting of equations for each x below the crossing point. Analogously, for the persuasion hypothesis we test that all β_x for x > 29% are jointly different from zero. We find strong evidence for both mechanisms – at low levels of support for the Nazi Party (29% or less) the backlash effect is clearly visible and jointly different from zero at a 5% significance level. At higher levels of NSDAP support (above the crossing point at 29%), the F-test supports the persuasion hypothesis. In combination, these results demonstrate the polarizing effect of marches across Hamburg, resulting in a more polarized city electorate overall.[21]

4.3 Indirect effects. Can propaganda spread through social networks, like a contagious virus? We exploit the geography of the Spanish flu's spread in 1918-20 Hamburg to derive a measure of social connectedness *before* the marches themselves.

The 1918 flu virus infected through social interactions, such as encounters between family members, friends, co-workers and business partners. While it eventually reached all areas of Hamburg, it diffused at differential speeds, causing spikes at different times. We use co-movements in deaths between areas to measure diffusion, indirectly capturing a combination of the frequency and intensity of interactions between people of Hamburg. Our flu measure is

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[21] Similarly, analysis of KPD marches also indicates polarizing effects (see Figure A.14). However, marches by the KPD were targeted towards areas of already growing Communist strengths and we therefore abstain from a causal interpretation of their impact.

$$Flu_i^M = std(\rho_i^T - \rho_i^C) \tag{4}$$

where ρ_i^T is the correlation of flu deaths in district i with flu deaths in the march-treated area T. One limitation of this measure is that it may pick up general connectivity to other parts of the city, whether exposed or not exposed to the march. To address this issue, we adjust for connections to areas of Hamburg that are *not* exposed to the marches adjusting for correlations ρ_i^C with other, unexposed districts C. To take into account the heterogenous response to the treatment, we scale ρ_i^T and ρ_i^C by the change in NSDAP vote share in connected districts j.[22] In other words, we expect larger spillovers in areas that are more strongly connected to parts of the city that saw a post-march surge in Nazi voting. Appendix A describes the derivation in detail.[23]

Figure 6 focuses on a small part of Hamburg to build intuition. Panel B shows indirect exposure to the march with different shades of blue; darker dots represent polling stations with stronger social connections to directly affected areas. Panel C shows the average change in Nazi vote share after the marches, with darker colors indicating greater gains. While the correlation is not perfect, areas with greater indirect exposure (like the most southern and eastern polling stations) also saw greater vote gains for the Nazi party. The binned scatter in Figure 7, Panel A, shows that this relationship holds for the universe of Hamburg polling stations: areas with greater social links to polling stations traversed by the marches saw greater increases in Nazi voting after the marches occurred.

Next, we investigate the dynamic impact of social connections Flu^M on voting results, augmenting the difference in difference equations (1) and (2):

$$N_{it} = \alpha_i + \alpha_t + \beta M_i \times Post_t + \gamma Flu_i^M \times Post_t + \sum_t \delta_t \mathbf{X}_i' + u_{it} \tag{5}$$

$$N_{it} = \alpha_i + \alpha_t + \sum_t \beta_t M_i + \sum_t \gamma_t Flu_i^M + \sum_t \delta_t \mathbf{X}_i' + u_{it} \tag{6}$$

[22] Appendix C describes an alternative approach constructing separate measures for exposure to persuasion and backlash neighborhoods to account for heterogeneity in effects of direct exposure.

[23] We validate the use of the flu measure by showing that it correlates with measures of homophily – the socio-economic similarity of different districts as derived from the 1932 address book. Appendix B describes its construction in detail. In Figure A.5, Panel A, we show that this measure correlates with the spread of the flu in 1918, as we would expect. In addition, Figure A.5, Panel B, shows that flu correlation is not related to physical distance. Hence, our measure aims at capturing meaningful, direct social interactions that increase the likelihood of transmission of the flu as well as information, and not just physical proximity between individuals living in adjacent neighborhoods. Figure A.6 demonstrates that the spread of the flu is also not simply a function of a district's distance to the city center. Figure A.8, Panel A and A.9 show robustness of the main effect to using an unscaled version of the flu measure. Figure A.8, Panel (b) shows robustness to using the level of April 1932 NSDAP vote share to scale the flu measure.

Equations (5) and (6) add the interaction between flu-based indirect exposure Flu^M and the *Post* indicator. One concern could be that indirect exposure partly reflects direct exposure. This would be true if physical proximity to the marches correlates with Flu_i^M. We already showed that physical and social proximity are only mildly correlated (Figure A.5, Panel B). Here, we present results with and without the direct treatment interaction, M_i to assess how sensitive the results are to such a correlation structure.

Table 4 reports the results. The estimate in col. 1 controls for election and polling station fixed effects and shows that social exposure to the march has a positive and significant effect on NSDAP vote share. Adding the full set of controls interacted with election fixed effects in col. 2 reduces the point estimate but not the significance of the coefficient, which remains well below conventional levels. In col. 3 we additionally control for direct exposure. The point estimate is barely affected and remains highly significant. This suggests that the indirect treatment is not picking up direct exposure. It also suggests that if there is any measurement error in the indirect effect, the error is not correlated with direct exposure. The stability of the point estimates shows that social proximity to the march captures a determinant of voting orthogonal to physical proximity.

Columns 4 and 5 of Table 4 examine the dynamic pattern of these social effects: γ_t in equation (6). We present results without (col. 4) and with (col. 5) controls for the corresponding effects of direct exposure, and plot the coefficients of indirect exposure from col. 5 in Figure 7, Panel B. As in the case of direct effects, indirect exposure to the march is uncorrelated with NSDAP voting in the election preceding the marches (Table A.2) The absence of pre-trends in indirect exposure strengthens the credibility of our empirical approach. Second, indirect effects become large immediately after the marches. Indirect exposure is significant at 6 percent in t4, and below 2 percent in t5 and t6; we can clearly reject the null of no effects for all post-marches elections. Third, in contrast with the direct effect, the impact of social exposure grows over time, in the three subsequent elections. By November 1932 – 200 days after the marches – the point estimate is 72.4% larger than for the July elections. The fact that it appears to take time for information to diffuse is in line with standard assumptions in social learning models (e.g., DeMarzo et al. 2003; Golub 2017).

We also find that indirect effects influenced polarization. Figure A.7 repeats the distribution regression exercise for indirect effects. The same pattern as before emerges: indirect effects were positive in the upper tail of the Nazi vote share distribution. At low levels of the CDF, none of the point estimates are positive.

4.4 Interpreting the results. The existence of indirect effects may complicate the causal interpretation of our estimates. The main challenge lies in the definition of the comparison group: if the effect of the marches spread through the city via social connection, no area of Hamburg is truly unaffected by the treatment. The lack of "pure controls" would then prevent us from reading our estimates as the causal effect of the marches on voting.[24] While a valid concern in theory, the problem is less serious in practice. Figure A.10 partitions polling stations into 16 groups of roughly similar size, based on the quartile of residual physical (x-axis) and social (y-axis) distance to the march. We color-code cells based on the residual post-marches change in NSDAP vote. The heatmap shows large swings in polling stations with top exposure along the two dimensions, and the greatest increase in places that were close to the marches both in the physical and social space. However, the figure also suggests that the effect fades at lower levels of exposure, which do not seem to be significantly different from each other.[25] This analysis thus suggests that the marches did not affect everyone in Hamburg, and that the consequences of top exposure were additive and similar for direct and indirect exposure.

4.5 Assessing magnitudes. How large are the direct and indirect effects? We quantify magnitudes in Table A.4. Our estimates imply that a one standard deviation change in *direct* exposure accounts for 13.1% of a one standard deviation change in voting between the first post-treatment election and the one immediately before the marches. By t=6 (March 1933 election), a one standard deviation change in direct exposure still accounts for 9.9% of a one standard deviation difference in NSDAP votes. Table A.4, Panel B shows that the implied effect of a one standard deviation change in our measure for *indirect* exposure accounts for 6.7% in t=4 and for 10% of a one standard deviation difference in NSDAP votes in the March 1933 election. Table A.4, Panel C calculates the implied relative importance of these effects. It shows that the relative importance of the indirect effect grows from a third in July to around 50% in November 1932 and March 1933.

[24] Note that to the extent that people living further than 200 meters or living in unconnected neighborhoods also were exposed to the marches, this will downward bias our estimates. Appendix D documents a set of simulations to quantify the potential bias arising from unobserved spread to untreated areas of Hamburg.

[25] Table A.3 and makes this statement more precise reporting the regression estimates of direct (cols. 1+3) and indirect (cols. 2+3) effects by quartiles of march exposure. Figure A.10, Panel B and C provide a further break down into the temporal pattern of the effects by quartile of physical and social distance to the march.

4.6 Mechanisms. Why did two marches make such a deep impression? While it is hard to give a definitive answer, the data and the historical record help evaluate different hypotheses. First, the data does not support the idea that marches mobilized voters: the first three columns of Table A.5 show that direct and indirect exposure had no impact on voters' turnout. Similarly, within-city relocation (migration) is unlikely to explain our results: moving houses takes time and our elections take place in rapid succession (three months). Moreover, the last three columns of Table A.5 show that the marches had no direct nor indirect effect on the total number of voters registered, ruling out the possibility that marches led to net inter-city migration.

Leaders often have an outsized impact on social behavior (Becker et al. 2020; Bai, Jia, and Yang 2023): could the marches influence voters who recognized local leaders marching with the Nazis? This seems unlikely, as march participants were SA members (generally from low status occupations) and were also recruited from outside the city. Thus, march witnesses were unlikely to recognize anyone they knew, let alone influential personalities. Social acceptability is another possible explanation: the marches may have demonstrated how broad Nazi support was, convincing many to act on their sympathies for the party (Bursztyn, Egorov, and Fiorin 2020). While we do not exclude that marches may have changed the perception of Nazi among the population, we do not expect a particularly strong effect on the outcome we study. Voting is a private action, and even people who would not admit publicly their support of the Nazi party could do so in private before the 1932 marches. In addition, by April 1932 Hitler had already received 25% and 30% of all Hamburg votes in the two presidential rounds and his movement had already moved from the political fringe to the center stage.

Finally, it is possible that the marches provided some meaningful signal about the NSDAP's ability to organize complex events, and that some voters were persuaded that the leadership of the party was not only able to protest but also to organize. Anecdotal evidence supports this view: Nazi marches and military events impressed an anxious middle class weary of the chaos of the Weimar Republic and nostalgic of the pre-WWI imperial order. Shopkeepers, skilled workers, and white-collar workers the quintessential *petit bourgeois*, may have been particularly susceptible to these effects. Blue-collar workers in turn might be particularly inclined to react with disgust to the display of militaristic power by the Nazis taking the streets. To shed light on the characteristics associated with backlash and persuasion, we first note that the distribution regression is identified from variation by polling stations crossing different thresholds of NSDAP support. It is thus instructive to study the characteristics of polling stations that start with different levels of baseline NSDAP support. Table A.19 provides descriptive evidence on the characteristics of neighborhoods at different parts of the baseline NSDAP vote share distribution. Polling stations between 15% and 20% of NSDAP vote share in April 1932 have a 43% probability to cross the 20% threshold in July 1932. However, the chances of crossing the threshold are much higher for polling station not directly exposed to the march (44%) than for treated polling stations (38%). These places tend to be more working class: 50% blue collar and vote more for KPD before the marches (22%). In contrast, polling stations that start between 35% and 40% of NSDAP vote share in April 1932 have a 69% chance of crossing the 40% threshold in the July 1932 election. Here the marches promote crossing thresholds: treated polling stations have a 77% probability of switching above the threshold, compared to 66% in the group of not directly exposed neighborhoods. These polling station appear more *petit bourgeois* with only 26% blue-collar workers and higher shares of skilled (8% vs. 5% in neighborhoods below the 20% threshold) and white-collar workers (6% vs. 4%). They also have many more shopkeepers (14% vs. 8%) and much greater diffusion of telephones (17% vs. 4%) and central heating (9% vs. 1%).

This provides suggestive support to the idea that marches were especially effective in persuading middle-class voters, who before the marches may have been skeptical about the ability of NSDAP to bring back Germany's past strength. We thus conclude tentatively that in this setting marches affected behavior because they provided voters with new information.

5 Robustness and additional analysis

In combination, the results from difference-in-difference estimation suggest that Nazi marches were successful in *persuading* people directly exposed to them, at least on average. Second, the marches had a *polarizing effect*, with strong increases in Nazi voting in the upper part of the NSDAP vote share distribution, and a backlash elsewhere. Third, the effect was powerful enough to *persist* for almost one year – despite numerous political events, inside and outside Hamburg. Fourth, the effect is arguably *causal.* Here, we examine the robustness of our findings to a number of potential issues.

5.1 Spatial standard errors. Both voting and distance to Nazi marches vary in space. This creates spatial autocorrelation, biasing standard errors downwards. Using Moran's I, we find that there is no spatial autocorrelation beyond 3km.[26] Table A.6 presents results for re-estimating equation (6) accounting for spatial autocorrelation, for direct exposure only (col. 1), indirect exposure only (col. 2) and direct and indirect exposure combined (col. 3). We first account for serial correlation by clustering at the polling station level. We use the Conley (1999) formula to correct standard errors by allowing serial correlation across the 5 main periods (March 1932 to March 1933). Overall, standard errors remain stable across specifications, and significance is largely unaffected. We conclude that spatial correlation is unlikely to drive our results.

5.2 Non-random exposure to the march. Another concern are non-random patterns in the treatment assignment that might confound our estimates. For direct exposure, one might worry that certain streets and neighborhoods are more likely to be treated than others. For example, wide streets are preferred by the protest planners to make the march appear more impressive. Central neighborhoods are more likely to be traversed as any march that connects start-, mid- and endpoints from different corners of the city tends to pass through central areas. If these characteristics determining the likelihood of treatment are correlated with other characteristics associated with change in vote share, such as presence of certain occupational groups with a higher propensity to swing in favor of the Nazis, our estimates would suffer from omitted variable bias. This concern extends to the identification of the spillover effects. Neighborhoods that are strongly connected to central parts of the city or wide streets will likely comprise similar social groups, creating a similar challenge to distinguish unobserved shocks affecting these groups from the exposure to the marches.

Borusyak and Hull (2023) provide a solution to address such concerns, which builds on viewing the realized treatment as one draw from a shock assignment process and considering counterfactual sets of exogenous shocks that could also have been drawn. They show that omitted variable bias can be purged by controlling for expected treatment, measured as the average treatment of each unit across many counterfactual sets of shocks. In our case, the shock assignment process is determined by the geography of the city, the pre-existing social networks and the preferences of the Nazi planners. To create

[26] We estimate Moran's I z-score of regression residuals from estimating equation (6). Figure A.11 reports Moran's I z-score for distance cut-offs from 0.1 km to 4 km. At 3km, the z-score drops to 0.

counterfactual shocks, we focus on two main objectives of the planner taking city geography and social networks as given. First, the distance between start-, mid-, and endpoints of the march should not be too short to allow for proper marching and coverage of multiple neighborhoods of the city. Second, the planner prefers the use of wide streets for the march to allow for marching in lockstep to impress the observers (see footnote 8).

Building on this intuition, we create random marches that mimic the characteristics of the observed marches. We first generate seven random points on the street network of Hamburg (five starting-, one mid- and one endpoint) that are at least 500m away from another. Next, we compute least-cost paths connecting all start- to mid- and the mid- to endpoint with a bonus for streets in the first width tercile and a penalty on narrow streets in the lowest width tercile. We repeat this procedure a second time to mimic the two treatment marches and thereby obtain 12 marching sections constituting a counterfactual shock (Figure A.12 shows three examples). Next, we compute direct and indirect exposure to such a counterfactual march following the same steps as we did for the realized marches. We repeat this procedure 500 times. Following Borusyak and Hull (2023), we compute expected direct and indirect exposure as the average treatment of each unit across these 500 counterfactual marches. As common exposure of observations to observed and unobserved shocks generate complex dependencies across observations that make conventional asymptotic analysis inapplicable, we follow Borusyak and Hull (2023) using randomization inference (RI) leveraging the simulated counterfactuals and their proposed randomization test statistic to construct valid confidence intervals.

Table A.7 reports the results of this exercise. Column 1 reports estimates of equation (5) additionally controlling for expected exposures. The point estimate for direct exposure is largely unaffected and the RI confidence interval (.201 to 1.754) reject the null of no effect in the three post-marches elections. Similarly, the point estimate for indirect exposure in the post-marches period is slightly reduced compared to our main specification (ca. 16.8% lower) but the RI confidence interval (.120 to .334) rejects the null of no effect. When we move to the flexible specification (6) in Column 2 we again stable and positive effects for both direct and indirect effect, though the direct effect becomes less precisely estimated in the last period and the indirect effect is tightly estimated in t5 and t6, as we would expect from an effect that grows slowly overtime.

The implementation of the Borusyak and Hull (2023) method provides a very conservative test of our hypotheses, because the random marches we construct resemble in many ways the actual marches and absorb much of the variation of the true treatments.

Yet, the exercise indicates that voters living along the routes of the true marches turned to the Nazis significantly more than voters living in similarly central and connected neighborhoods which did not witness the marches. This in turn lends additional support to our conclusions.

5.3 Matching exercises. In this section, we demonstrate the robustness of our difference-in-difference results using three alternative estimation approaches. First, in Table A.8, Panel A we estimate the treatment effect with nearest neighbor matching, defining treatment as having 80% of households within 200m of the march. We match polling stations based on coordinates and demographic controls (cols. 1, 3 and 5) and on coordinates, demographic controls and within district (cols. 2, 4 and 6). In cols. 1-2 we look for a single match, in cols. 3-4 for 3 matches and in cols. 5-6 for 5 matches. Nearest neighbor estimates point once again to a positive effect of direct exposure. We repeat the matching exercise Table A.8, Panel B for indirect exposure, defining treatment as having above median flu-based indirect exposure. Both set of results confirm a positive and statistically significant effect of direct and indirect exposure.

Second, we apply the method of Hainmueller (2012) and re-weight treated and control observations. Table A.9, Panel A displays mean and standard deviation of covariates before (cols 1-2) and after (cols. 3-4) re-weighting. The procedure creates balance between treated and control polling stations. We then re-estimate equation (1) with weighted least squares, using entropy weights. The point estimate is reported in col. 2 of Table A.9, Panel B: it is almost identical to our baseline estimate (col. 1 for comparison).

Third, we follow Iacus et al. (2012) and re-estimate equation (1) with Coarsened Exact Matching (CEM). We find exact matches within broad cells defined by number of voters, share of households with telephone, of share of blue collars workers (4 quartiles) as well as weather at least on household has heating or not (dummy). Within these cells, we can find exact matches for 76% of our polling stations, and we re-estimate equation (1) on these observations only. The result is reported in col. 3 of Table A.9, Panel (B), showing a point estimate slightly larger than our baseline.

5.4 Other propaganda. Did other propaganda activities confound our results? Table A.10 gives an overview of other propaganda events in 1932/1933 Hamburg. First, we test whether marches by other parties have similar effects? The main other marches during the period were by the Communist Party (KPD), and by the Social Democrats (SPD).

Figure A.13 shows their paths. We can analyze the impact of the Communist march along the same lines. However, since the Social Democrats did not field their own candidate for President, we cannot easily do so for their march. In Table A.11, we examine the impact of the KPD and SPD marches on Nazi and KPD votes. We find that the Communist march had a similar (direct) effect to the Nazi marches, increasing their own votes, but it is smaller in magnitude and not significant. The SPD march appears to have reduced KPD votes, but again, effects are not significant (Panel A). We also find that alternative marches reduced Nazi voting (Panel B). However, marches by the SPD and KPD were targeted towards areas of already growing strengths – in Figure A.14, Panel A, we see evidence of growing support in KPD march-exposed districts. Figure A.14, Panel B provides evidence that the KPD marches display a polarizing effect similar to the one found for the Nazis. For low and medium thresholds of KPD support, the effect is negative ("backlash"). Only at high thresholds of KPD vote support, the marches are persuasive.

Second, we test whether other propaganda by the Nazis could confound our main findings. Table A.12 shows that our estimates for direct and indirect exposure remain largely unchanged when controlling for distance to any of the Hitler speeches held in Hamburg and its surroundings.[27] Figure A.15 and Table A.14 show that distance to the two treatment marches is unrelated to the number of Nazi rallies held in Hamburg. The Nazi Party also staged a large march in February 1933, after its "seizure of power". We digitize the marching route and repeat our analysis. We find no significant effects (Table A.11, Panel B, cols. 4-6). This is in line with our conceptual framework – the marches "worked" before the Nazis were in power because marches were one of the few forms of propaganda available to the party and provided novel information on their organizational capacity.[28] Once Hitler was Chancellor, and radio and other media could be used for propaganda (Adena et al. 2015), the marches had no additional effect – highlighting the importance of public demonstrations for the rise of extremist movements and groups while

[27] Table A.13 shows that the treatment marches are also unrelated to the location of the first Hitler speech and a rally by the Eiserne Front, an alliance close to the social democrats founded to defend the Weimar republic against extremist movements. Figure A.19 shows that distance to the speeches does not display the same polarizing effect as protest marches do. This makes intuitive sense as protest marches, similar to door-to-door canvassing or radio propaganda, for which existing studies document polarizing effect (Baysan 2022; Adena et al. 2015), treat bystanders independent of their predisposition to react positively to the message transmitted. Exposure to speeches requires people to actively sort into attending, making it unlikely that people who might react with backlash get exposed.

[28] Figure A.16 illustrates that major Nazi propaganda events were not accompanied by disproportional coverage of the Nazi party in the leading neutral newspaper (*Hamburger Anzeiger*). This further highlights the importance of public demonstrations as means of reaching voters directly and via word of mouth.

still in opposition, but not thereafter.

5.5 Cutoff sensitivity. In an additional exercise, we also experiment with our definition of direct and indirect exposure. Table A.15 reports estimates when we consider directly exposed all households living within 150m, 200m or 250m (col 1). We also re-define indirectly exposed households as those connected to households in polling stations that are at least 90% treated (cols. 2-3), 80% (cols. 4-5) or 70% (cols 6-7). Point estimates and confidence intervals remain stable and comparable to baseline results.

5.6 Extremist vote share. In a final exercise, we turn to the polarization of the political landscape overall. We ask whether the marches contribute to a strengthening of the political extremes, effectively hollowing out the democratic center. To examine this question, we look at the combined extremist vote share of NSDAP and KPD. Table A.18 reports estimates of equation (6) using extremist vote share as the dependent variable. Albeit not statistically significant at the 5% level, the point estimates suggest that marches on average strengthened the extremes of the political spectrum. Crucially, we also observe meaningful heterogeneity in the effect of the marches along the distribution of extremist vote share. We repeat the distribution regression approach estimating equation (3), using indicators of extremist vote share being above a given thresholds as dependent variable. Figure A.18 shows the results. The march is particularly effective in pushing neighborhoods above the 50% cutoff. This indicates that many neighborhoods turn to an extremist majority in response to the treatment which might go hand-in-hand with the erosion of local social norms that support the democratic order (Bursztyn, Egorov, and Fiorin 2020).

6 Conclusions

In this paper, we examine the effects of Nazi propaganda. Focusing on Nazi marches as a propaganda tool, and on one German city – Hamburg in 1932 – we find strong evidence that marches were a powerful propaganda tool: They persuaded citizens to vote for the Nazi Party. Gains for the party were immediate after the marches in areas traversed by the marching columns; they remain visible in our data for at least a year. We arguably identify causal effects by using a difference-in-difference strategy. Importantly, the effect is not uniform across area. The marches increased Nazi support in the upper tail of the Nazi vote share distribution; they reduced support in the lower tail of the Nazi vote share distribution -- a "backlash".

Our study goes beyond measuring the direct effect of mass gatherings. Social spillovers have long been hypothesized in the campaign effects literature, but there is little evidence in real-life settings of them magnifying propaganda effects. Our unique setting allows us to show that major social spillovers can exist. We exploit contagious disease diffusion in Hamburg, using the 1918 flu epidemic, as a proxy for connectedness between neighborhoods. This predetermined measure of indirect exposure has substantive predictive power for where the Nazi party gained votes in 1932/33. In contrast with direct treatment, social spillovers took time to diffuse. Its impact grew during the year following treatment.

Did mass rallies, campaign speeches and marches matter for the fall of Germany's first democracy? We make both methodological and substantive contributions. While no history of the Nazi party's rise to power fails to emphasize the appeal of its mass gatherings, research analyzing campaign effects often finds little or no significant impact. This is also true of Hitler's campaign speeches, arguably the most high-profile type of event held by the Nazi Party (Selb and Munzert 2018; Bursztyn, Egorov, and Fiorin 2020). Our results suggest that widespread doubts about the existence of campaign effects, both generally and during Weimar Germany's twilight years, may reflect measurement issues rather than a lack of causal effects. Our study highlights two challenges to the interpretation of average treatment effects. First, the existence of social spillovers in political campaigns is widely suspected to blur the distinction of treated and untreated areas, making it harder to find effects (Bennett and Iyengar 2008). Our results on social spillovers strongly suggest that this is one reason why earlier papers have often failed to find campaign effects. Second, our evidence on heterogenous effects – with some areas witnessing "backlash" – provide another explanation for the difficulty to pin down average treatment effects. We observe a similar pattern for communist marches in our setting and together with evidence of heterogenous effects of campaign effects in other settings (Adena et al. 2015; Baysan 2022; Enikolopov et al. 2023), this can help to reconcile the often limited and fleeting campaign effects found in much of the existing literature (Brady, Johnston, and Sides 2006; Kalla and Broockman 2018; Le Pennec and Pons 2019; Broockman and Kalla 2023) with other evidence demonstrating the persuasive power of campaigning.

Mass rallies were a key propaganda tool for the Nazis: Barred from state-controlled radio and faced with frequent bans on its newspapers, marches, meetings, and rallies were a crucial propaganda tool. Despite its large role in narrative accounts of the rise of Hitler, their effectiveness has so far not been convincingly demonstrated empirically.[29] The Hamburg marches were part of a broader propaganda effort: In the run-up to the Presidential election in 1932, the Nazi party held no fewer than an astonishing 34,000 rallies, meetings and marches all over Germany. We cannot determine the effect of this nationwide wave of gatherings from Hamburg data. Nonetheless, our findings strongly suggest that mass rallies and marches were highly effective: If our results for Hamburg are indicative of broader patterns, mass demonstrations facilitated the party's big breakthrough in 1932, when it became the single largest party in Germany. They also contributed importantly to growing polarization in the final days of Germany's first democracy.

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[29] Selb and Munzert (2018) argue that their results "cast doubt on the omnipotence of Nazi propaganda... its full effect [became visible] only after the Nazis had ... totalitarian control over the state apparatus... and the emerging mass media."

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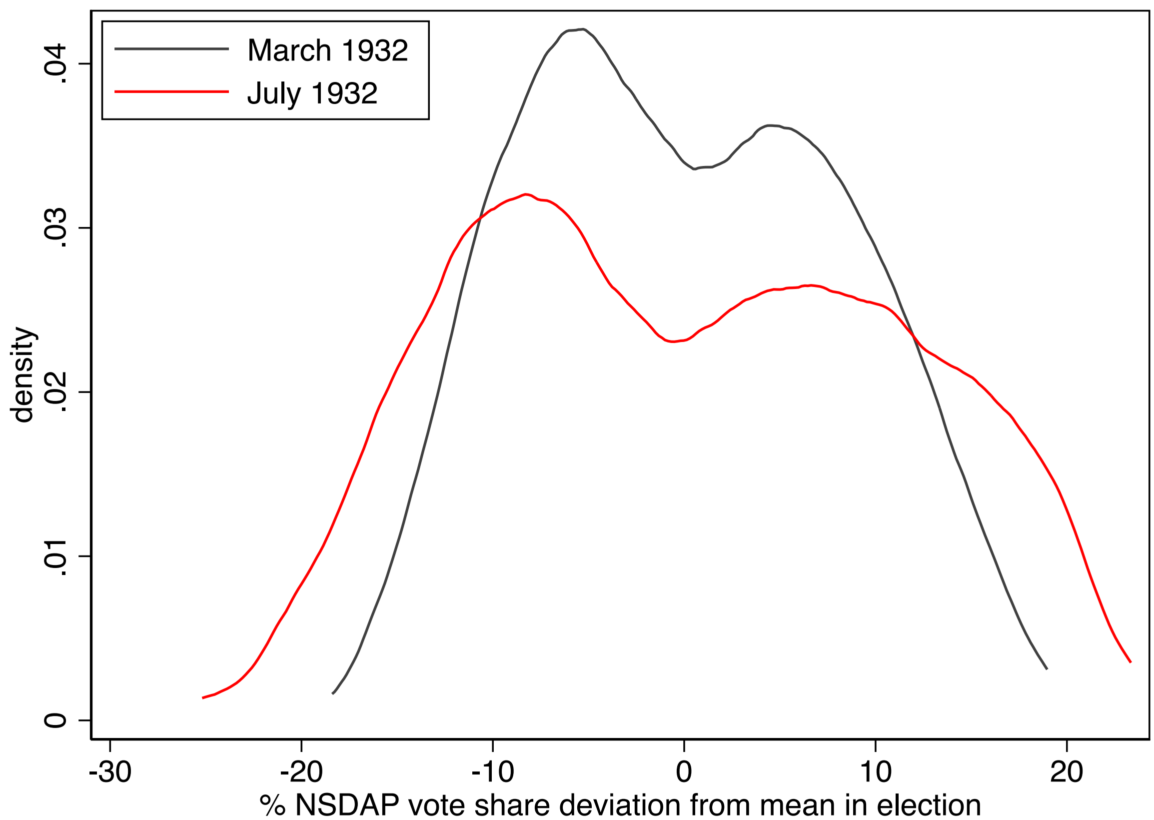
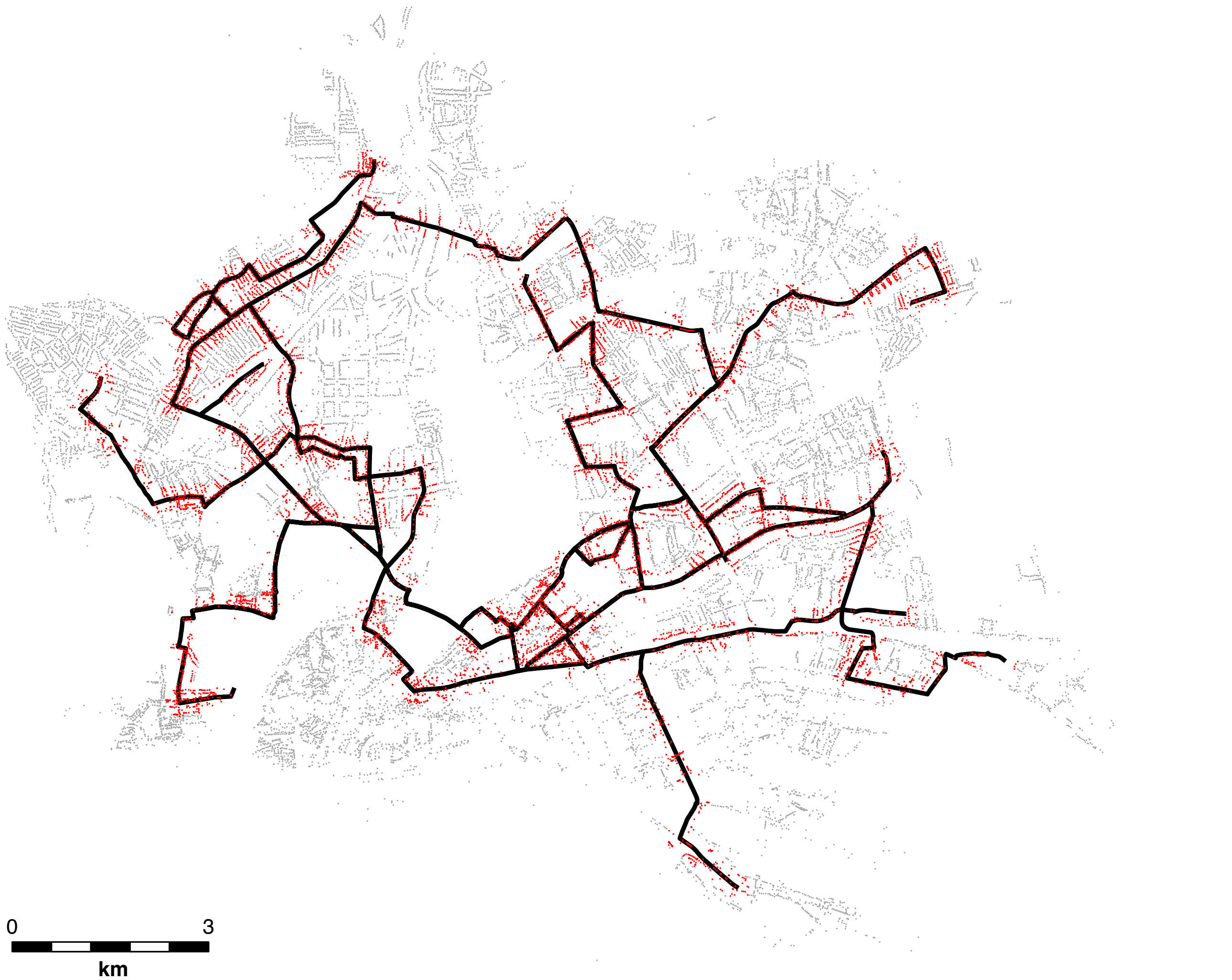
Figures

Figure 1: Polarization in 1932 Hamburg.



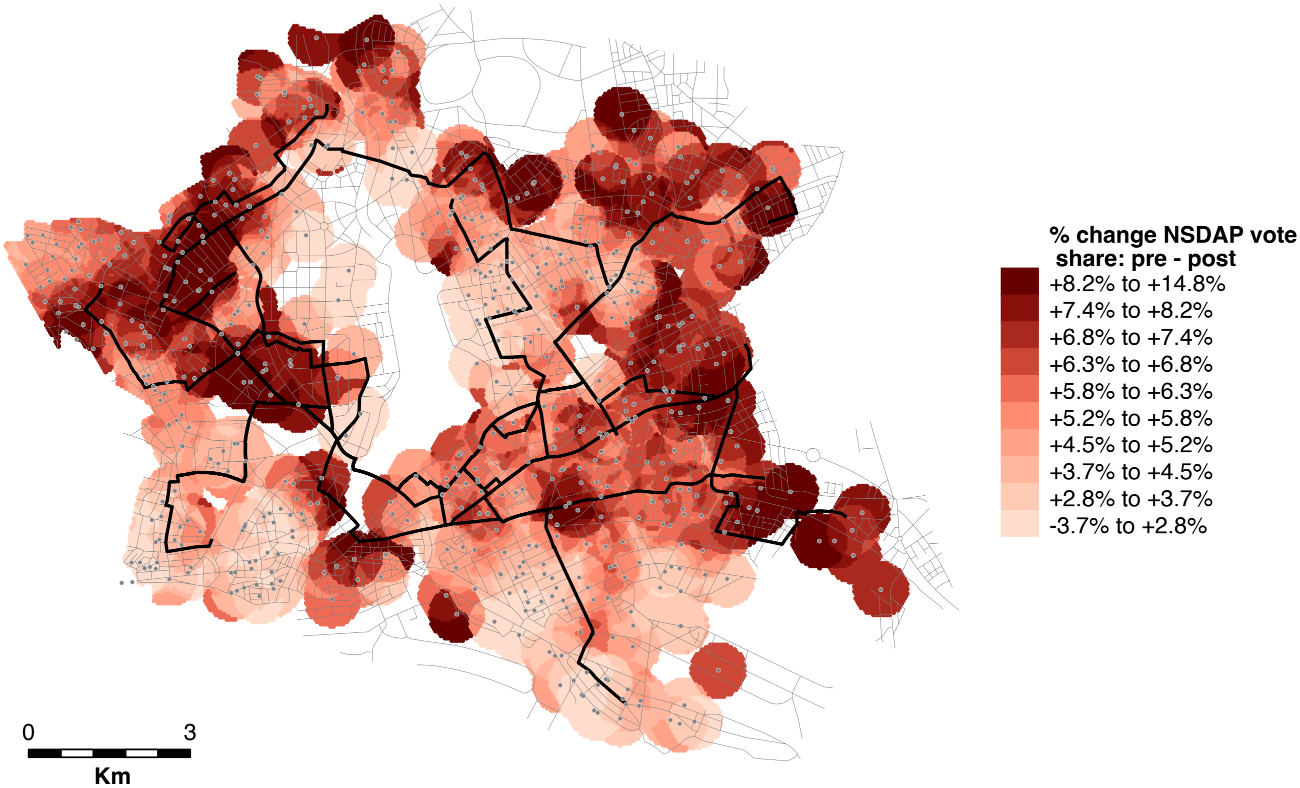
Note: The figure shows the distribution of NSDAP vote share deviations from the mean of the 1st presidential election round (13 March 1932: solid black line) before the Nazi marches took place and the 31 July 1932 Reichstag election (solid red line) after the Nazi marches. we observe voting behavior at the polling station level. Sources: voting data: statistical bulletin of Hamburg (Sköllin, 1932a; 1932b).

Figure 2: Direct exposure.



Note: The map shows the routes of the two Nazi marches in April 1932 (black lines) and the addresses of the 400,000 households living in Hamburg in 1932. Red dots are addresses located less than 200m from one of the marches' routes; grey dots are all other addresses. Sources: households data: 1932 Hamburg address book (Hamburger Adreßbuch, 1932); Nazi marches: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b).

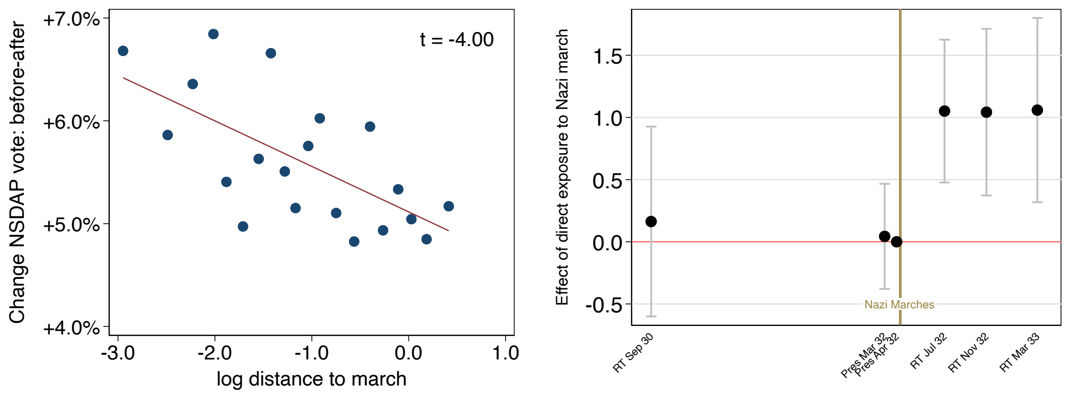
Figure 3: Nazi party swing.



Note: The map shows a heatmap of the change in NSDAP vote share after the marches. Change in NSDAP vote is calculated between the two elections before the marches (13 March and 10 April 1932) and the three after (31 July 1932 to 5 March 1933). We observe voting behavior at the polling station level. We compute average change in NSDAP vote share using a spatial kernel with a fixed bandwidth (500m around polling station). We divide by change in NSDAP vote share into 10 equally sized groups. Color intensity increases with higher positive change in favor of the NSDAP. We overlay the map with the routes of the two Nazi marches in April 1932 (black lines). Sources: voting data: statistical bulletin of Hamburg (Sköllin, 1932a; 1932b; 1933); Nazi marches: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b).

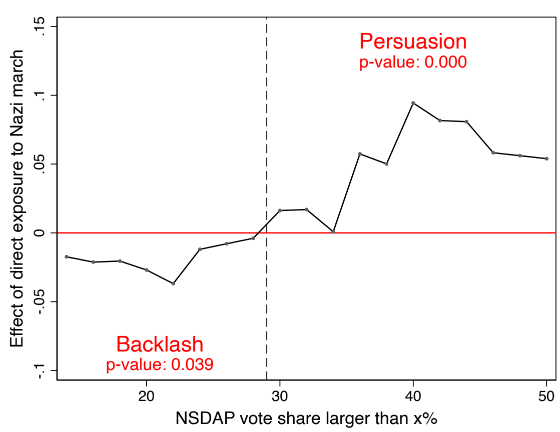
Figure 4: Direct effect.

(a) Direct exposure (before-after) (b) Direct exposure (over time)



Note: Panel A: The figure plots a bin-scatter of log distance to the closest Nazi march (x-axis) against the change in NSDAP vote share after the marches (y-axis). Change in NSDAP vote is calculated between two election before the marches (13 March and 10 April 1932) and three elections after (31 July; 6 November 1932 and 5 March 1933). t-statistic is estimated from a bivariate regression with robust standard errors. Panel B: Plot of estimates of direct exposure (share of households within 200m of Nazi march) estimated from equation (2) and corresponding 95% confidence intervals by election (computed from Table 3, column 5). Sources: Nazi marches: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b); voting data: statistical bulletin of Hamburg (Sköllin, 1930; 1932a; 1932b; 1933).

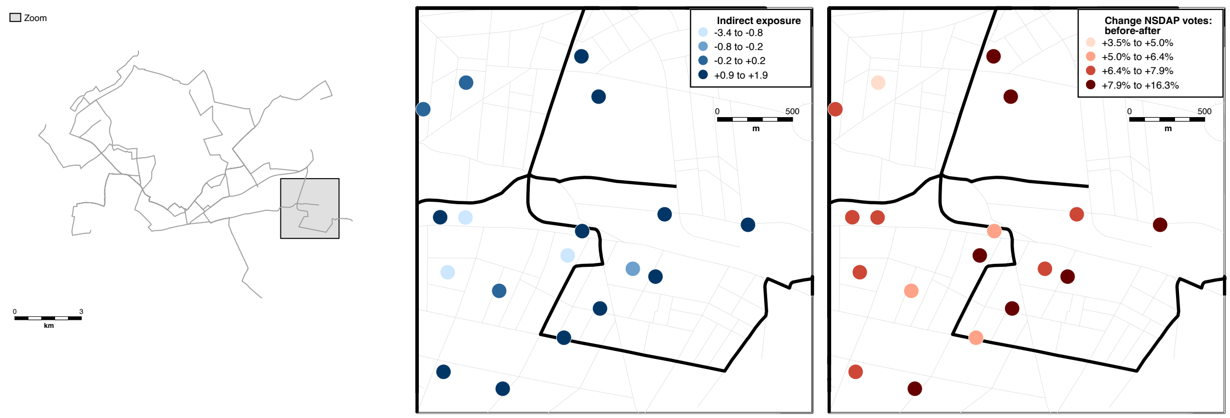
Figure 5: Distribution regression of direct exposure.



Note: The figure plots coefficients of direct exposure estimated from equation (3) using the main panel of 1932 and 1933 elections(y-axis). Dependent variable is an indicator = 1, if NSDAP vote share is above the threshold indicated on the x-axis. We use thresholds ranging from the 5^{th} to the 95^{th} percentile of the NSDAP vote share distribution in steps of two percentage points. The vertical line indicates the crossing point between backlash (= negative effect of direct exposure to the march) and persuasion (= positive effect of direct exposure to the march). The p-value for backlash is obtained from testing against H0: All coefficients of direct exposure for NSDAP vote share thresholds below 29% are jointly equal to zero. For the joint F-test, we estimate a seemingly unrelated regressions (SUR) model as introduced by Zellner (1962) with equations for all NSDAP vote share thresholds between 14% (5^{th} percentile of the NSDAP vote share distribution) and 29% (crossing point) in steps of two percentage points as dependent variables and the right-hand-side from equation (3). Analogously, we obtain the p-value for persuasion from testing against H0: All coefficients of direct exposure for NSDAP vote share thresholds above 29% are jointly equal to zero following the same procedure. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. We additionally control for indirect exposure to the march. Standard errors are clustered at the polling station level.

Figure 6: Visualization indirect effect.

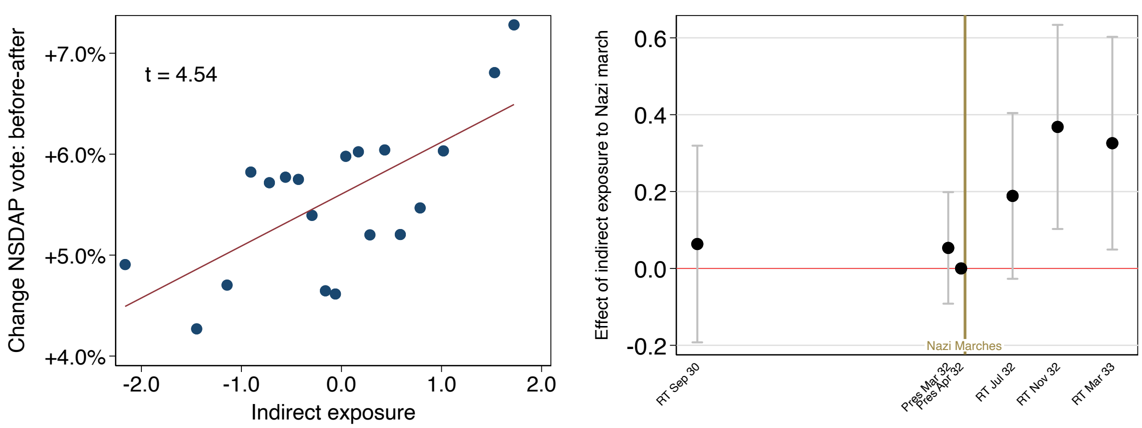
(a) Hamburg (b) Indirect exposure (c) NSDAP change (before-after)



Note: Panel A: The maps shows the location of the snapshot of Hamburg shown in panels B and C. Panel B/C: The maps show the location of polling stations. Change in NSDAP vote is calculated between the two elections before the marches (13 March and 10 April 1932) and the three after (31 July 1932 to 5 March 1933). For the full sample we split polling stations by flu-based indirect exposure/vote change into 5 equally sized groups. Color intensity increases with higher indirect exposure/vote change. We overlay each map with the street network of Hamburg (grey lines) and the routes of the two Nazi marches in April 1932 (black lines). Sources: voting data: statistical bulletin of Hamburg (Sköllin, 1932a; 1932b; 1933); indirect exposure: death records (State Archive Hamburg, 1917; 1918; 1919): Nazi marches: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b); street network: historical map of 1930-1940 Hamburg (Landesbetrieb Geoinformation und Vermessung Hamburg, 2020).

Figure 7: Indirect effect.

(a) Indirect exposure (before-after) (b) Indirect exposure (over time)



Note: Panel A: The Figure plots a bin-scatter of flu-based indirect exposure (x-axis) against the change in NSDAP vote share after the marches (y-axis). Change in NSDAP vote is calculated between two election before the marches (13 March and 10 April 1932) and three elections after (31 July; 6 November 1932 and 5 March 1933). t-statistic is estimated from a bivariate regression with robust standard errors. See main text and appendix for construction of the flu-based indirect exposure measure. Panel B: Plot of estimates of flu-based indirect exposure estimated from equation (6) and corresponding 95% confidence intervals by election (computed from Table 4, column 5). See main text and appendix for construction of the flu-based indirect exposure measure. Sources: Nazi marches: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b); indirect exposure: death records (State Archive Hamburg, 1917; 1918; 1919); voting data: statistical bulletin of Hamburg (Sköllin, 1930; 1932a; 1932b; 1933)

Tables

Table 1: Summary statistics.

	Min	Mean	Max	St. dev.	Obs.
Election results					
NSDAP vote share 14 September 30 (pre)	3.430	19.692	37.918	7.138	515
Hitler vote share 13 March 32 (pre)	5.864	24.260	43.231	8.282	622
Hitler vote share 10 April 32 (pre)	8.163	30.417	55.901	10.307	622
NSDAP vote share 31 July 32 (post)	8.253	33.443	56.788	11.291	622
NSDAP vote share 6 November 32 (post)	6.433	26.922	47.785	9.273	622
NSDAP vote share 5 March 33 (post)	10.624	38.465	59.760	10.917	622
Marches					
Average distance to closest Nazi march (km)	0.026	0.481	1.939	0.405	622
Share households directly exposed to Nazi march	0	32.514	100	36.340	622
Share households directly exposed to KPD march	0	30.632	100	38.556	622
Share households directly exposed to SPD march	0	21.093	100	32.009	622
Connection to march					
Indirect exposure of households	-3.437	-0.000	1.885	1.000	622
Demographic controls					
Number of voters at polling station (10 April 32)	501	1295.413	1940	171.677	622
Share of blue collar workers	0	35.519	63.793	14.533	622
Share of civil servants	0	6.068	53.922	4.525	622
Share of shopkeepers	0	11.442	27.993	4.172	622
Share of households with telephone	0	11.578	65.525	11.620	622
Share of households with heating	0	5.783	73.929	11.979	622
Street network controls					
Distance to closest extreme point (km)	0.069	1.386	3.855	0.711	622
Distance to closest straight line between extreme points (km)	0.005	0.833	3.060	0.582	622
Number of streets within 200m of polling station	1	4.584	15	1.973	622
Share of streets in top tercile of width	0	40.926	100	29.998	622
Share of streets in bottom tercile of width	0	20.125	100	24.194	622

Note: The unit of observation is a polling station in Hamburg. Votes for Hitler and for NSDAP, number of voters and location of polling stations come from the statistical bulletin of Hamburg (Sköllin, 1930; 1932a; 1932b; 1933). To calculate the average distance to Nazi marches: (1) we reconstruct the path of the Nazi marches on the 17th and 20th of April 1932 from SA Hamburg documents (State Archive Hamburg, 1932a; 1932b); (2) We digitize and geolocate the address of each of the 400,000 households using the 1932 Hamburg address book (Hamburger Adreßbuch, 1932); (3) calculate for every household the distance to the closest marching route; (4) assign every household to his 1932 polling station based on his address and the official voting lists; (5) calculate the average distance to the march of the households allocated to every polling stations. The share of household within 200m from a marching route is calculated using the same sources. See main text and appendix for construction of the flu-based indirect exposure measure. Share of households with telephone, with heating, share of blue collar workers, shopkeepers and civil servants come from the 1932 Hamburg address book (Hamburger Adreßbuch, 1932). Distance to the closest extreme point and distance to the straight lines connecting extreme points are calculated using the marching routes digitized from the SA Hamburg documents (State Archive Hamburg, 1932a; 1932b). Number and width of streets within 200m from the polling station is calculated from the digitized street network (Landesbetrieb Geoinformation und Vermessung Hamburg, 2020).

Table 2: Pre-trends.

	13 March 1932 - 10 April 1932			12 Sep 1930 - 10 April 1932		
	(1)	(2)	(3)	(4)	(5)	(6)
	Δ NSDAP	Δ KPD	Δ turnout	Δ NSDAP	Δ KPD	Δ turnout
log distance to march	0.070	-0.027	-0.017	0.140	-0.399	-0.207
	[0.087]	[0.057]	[0.063]	[0.163]	[0.124]	[0.110]
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Street controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.552	0.509	0.146	0.521	0.580	0.127
Mean change in Y	6.157	-2.793	-4.757	11.472	-5.809	0.790
Observations	622	622	622	515	515	514

Note: Columns 1 to 3 show immediate pre-trends (first presidential election on 13 March 1932 round to second presidential election on 10 April 1932) of election outcomes. Columns 4 to 6 show long pre-trends (12 September 1930 parliamentary election to second round presidential election 10 April 1932). Estimates of regressing NSDAP vote share (cols. 1 and 4); KPD vote share (cols. 2 and 5); voter turnout (cols. 3 and 6) as dependent variable on log of average distance to Nazi march. In all regressions we control for demographic and street network controls. Standard errors clustered at polling station level in brackets.

Table 3: Direct effect.

	% NSDAP votes				
	(1)	(2)	(3)	(4)	(5)
log distance to march × post march	-0.443	-0.422			
	[0.111]	[0.105]			
Share households directly exposed (200m) × post march			1.127	1.029	
			[0.284]	[0.269]	
Share households directly exposed (200m) × t6 (post)					1.059
					[0.378]
Share households directly exposed (200m) × t5 (post)					1.042
					[0.342]
Share households directly exposed (200m) × t4 (post)					1.051
					[0.293]
Share households directly exposed (200m) × t2 (pre)					0.044
					[0.216]
Share households directly exposed (200m) × t1 (pre)					0.163
					[0.390]
Election & polling station FEs	Yes	Yes	Yes	Yes	Yes
Demographic controls × election FEs	No	Yes	No	Yes	Yes
Street controls × election FEs	No	Yes	No	Yes	Yes
R^2	0.864	0.915	0.864	0.914	0.933
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417
Direct effect t6 = t4: p-value	.	.	.	.	0.974
Direct effect t5 = t4: p-value	.	.	.	.	0.970
Direct effect t4 = t2: p-value	.	.	.	.	0.000
Observations	3110	3110	3110	3110	3625

Note: Estimates of equation (1) with log distance to march (col. 1 and 2) and share of households within 200m of march (col. 3 and 4) as measure of exposure using the main panel of 1932 and 1933 elections. Col. 5 shows estimates of equation (2) with share of households within 200m of march as measure of exposure. Dependent variable is the share of NSDAP votes. In col. 5, we additionally test whether the effect of direct exposure is stable over time, i.e. $\beta_4 = \beta_s$ for $s = 5, 6$ (two-sided test against H0: Diffusion is time-invariant). In all specifications, we control for polling station and election fixed effects. Col. 2, 4 and 5 additionally include street and demographic characteristics interacted with election fixed effects. Standard errors clustered at polling station level in brackets.

Table 4: Indirect effect.

	% NSDAP votes				
	(1)	(2)	(3)	(4)	(5)
Indirect exposure of households × post march	0.515	0.291	0.268		
	[0.113]	[0.101]	[0.101]		
Indirect exposure of households × t6 (post)				0.350	0.326
				[0.141]	[0.141]
Indirect exposure of households × t5 (post)				0.392	0.368
				[0.135]	[0.135]
Indirect exposure of households × t4 (post)				0.214	0.189
				[0.110]	[0.110]
Indirect exposure of households × t2 (pre)				0.054	0.053
				[0.074]	[0.074]
Indirect exposure of households × t1 (pre)				0.065	0.064
				[0.131]	[0.131]
Share households directly exposed (200m) × post march			0.979		
			[0.268]		
Share households directly exposed (200m) × t6 (post)					0.998
					[0.377]
Share households directly exposed (200m) × t5 (post)					0.974
					[0.339]
Share households directly exposed (200m) × t4 (post)					1.016
					[0.292]
Share households directly exposed (200m) × t2 (pre)					0.034
					[0.216]
Share households directly exposed (200m) × t1 (pre)					0.153
					[0.390]
Election & polling station FEs	Yes	Yes	Yes	Yes	Yes
Demographic controls × election FEs	No	Yes	Yes	Yes	Yes
Street controls × election FEs	No	Yes	Yes	Yes	Yes
R^2	0.864	0.914	0.915	0.933	0.933
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417
Indirect effect t6 > t4: p-value				0.066	0.066
Indirect effect t5 > t4: p-value				0.019	0.019
Indirect effect t4 = t2: p-value	.	.	.	0.086	0.149
Direct effect t6 = t4: p-value	.	.	.	.	0.944
Direct effect t5 = t4: p-value	.	.	.	.	0.856
Direct effect t4 = t2: p-value	.	.	.	.	0.000
Observations	3110	3110	3110	3625	3625

Note: Estimates of equation (5) with flu-based indirect exposure as only measure of exposure (col. 1 and 2) and both, flu-based indirect exposure and share of households within 200m of march (col. 3) as measures of exposure using the main panel of 1932 and 1933 elections. Estimates of equation (6) with flu-based indirect exposure as only measure of exposure (col. 4) and both, flu-based indirect exposure and share of households within 200m of march (col. 5) as measures of exposure. Dependent variable is the share of NSDAP votes. In col. 4 and 5, we test the *dynamic persuasion hypothesis* for flu-based indirect exposure, i.e. $\gamma_s > \gamma_4$ for $s = 5, 6$ (one-sided test against H0: Diffusion is not growing over time). In col. 5, we additionally test whether the effect of direct exposure is stable over time, i.e. $\beta_4 = \beta_s$ for $s = 5, 6$ (two-sided test against H0: Diffusion is time-invariant). In all specifications we control for polling station and election fixed effects. Col. 2 to 5 additionally include street and demographic characteristics interacted with election fixed effects. Standard errors clustered at polling station level in brackets.

Appendix for Online Publication

A. Flu exposure measure

To construct flu-based exposure to the marches Flu^M, we proceed in steps. First, we use the exact address and date of death of everyone who died in Hamburg during the peak of the Spanish flu pandemic (17 September and 18 November 1918) to create a time series of deaths for every polling station. Second, we compute a matrix of pairwise correlations of deaths between all polling stations: $[\rho_{ij}]$. Around 94% of these correlations are small and not significant at the 5% level: in these cases, we assume that people in the two polling stations do not interact regularly and set these correlations to 0. We take the significant correlations as evidence of social interaction among people living in pairs of polling stations: Figure A.4, Panel B provides examples of three pairs of such polling stations. The final matrix of significant correlations $[\rho^*_{ij}]$ allows us to measure social connections across areas of Hamburg. Third, we define polling stations that are directly treated by the marches as those with at least 80% of households within 200 meters from one of them. We are now in a position to compute a measure of interaction with areas that were directly exposed to the marches. We do so by taking for every polling station the sum of all significant correlations with polling stations that were directly treated j^T. We scale each significant correlation by the change in NSDAP votes between the 10 April 1932 and 31 July 1932 elections. The intuition behind this scaling is that backlash, i.e., negative change in NSDAP votes in polling station j tends to be associated with spreading negative information regarding the Nazi party whereas positive change in NSDAP votes will spread positive information regarding the Nazi party with increasing intensity as the change in NSDAP votes increases:

$$\rho_i^T = \sum_{j^T}^{J^T} \rho^*_{ij} * \Delta NSDAP_j$$

One limitation of this measure is that it may pick up general connectivity to other parts of the city, whether exposed or not exposed to the march. To address this concern, we compute a measure of connection to areas of Hamburg that are *not* exposed to the marches (j^C), as:

$$\rho_i^C = \sum_{j^C}^{J^C} \rho_{ij}^* * \Delta NSDAP_j$$

Our preferred measure is the *difference between* ρ_i^T and ρ_i^C which we standardize:

$$Flu_i^M = std(\rho_i^T - \rho_i^C)$$

B. Homophily measure

To construct a homophily-based measure of connectivity between two polling stations, we proceed in steps. First, we use the individual household information from the Hamburg address book (*Hamburger Adreßbuch* 1932) on occupational status, occupational sector and surname origin, which we match each household to the corresponding polling station based on voting lists of the statistical bulletins (see sections 3.2 and 3.3). Second, for every pair of polling stations, we compute a matrix of pairwise homophily by computing the average number of shared characteristics (same occupational status, occupational sector and surname origin) between any two households in these two polling stations: $[s_{\mathrm{ij}}^*]$. For example, if all individuals of polling station i shared all three characteristics with individuals of polling stations j (perfect homophily), this would lead to an average similarity of $s_{ij}^* = 3$. On average we observe an average similarity of .18 shared characteristics of any two households between all polling station pairs. The matrix of average similarity $[s_{ij}^*]$ allows us to measure social connections across areas of Hamburg. We use this measure to validate our flu correlation measure ρ_{ij}^* of connectivity between two polling stations (see Figure A.5, Panel B).

C. Alternative flu exposure measure

As an alternative approach to take heterogeneity in direct exposure into account when deriving a measure of indirect exposure, we construct two new indirect exposure measures, one to capture indirect exposure to "persuasion areas", i.e. the ones that experience a positive NSDAP change in response to the marches, and one to capture indirect exposure to "backlash areas", i.e. the ones that experience a negative NSDAP vote share change.

We apply the same logic of constructing indirect exposure as in our main result but divide the set of polling stations into positive NSDAP change and negative NSDAP change neighborhoods.

Specifically, the set of polling stations to compute indirect exposure to persuasion neighborhoods is the subset of all polling stations j with positive $\Delta NSDAP_j$.

$$\rho_i^{T\ (persuasion)} = \sum_{j^T}^{J^{T\ (persuasion)}} \rho_{ij}^* \ if\ \Delta NSDAP_j > 0$$

One limitation of this measure is that it may pick up general connectivity to persuasion neighborhoods in other parts of the city, whether exposed or not exposed to the march. To address this concern, we compute a measure of connection to areas with positive $\Delta NSDAP_j$ of Hamburg that are not exposed to the marches ($j^{C\ (persuasion)}$), as:

$$\rho_i^{C\ (persuasion)} = \sum_{j^C}^{J^{C\ (persuasio)n}} \rho_{ij}^* \ \ if\ \Delta NSDAP_j > 0$$

Our preferred measure is the difference between $\rho_i^{T\ (persuasion)}$ and $\rho_i^{C\ (persuasion)}$ which we standardize:

$$Flu_i^{M\ (persuasion)} = std\left(\rho_i^{T\ (persuasion)} - \rho_i^{C\ (persuasion)}\right)$$

We follow exactly the same steps to compute $Flu_i^{M\ (backlash)}$ using the subset of polling stations j with negative or zero $\Delta NSDAP_j$.

We use these two distinct measures for indirect exposure to separately estimate the effect of indirect exposure to persuasion and backlash areas. Table A.17 shows the results of this exercise. Notably, indirect exposure to persuasion areas is associated with a positive change in NSDAP vote share in connected neighborhoods, while exposure to backlash neighborhoods has virtually no effect on change in NSDAP vote share in connected neighborhoods. While this specification stretches the limits of our data and estimates for the indirect effect of connection to the persuasion area is weakly or weakly not statistically significant on the 5% level, we take these results as suggestive evidence that there is no offsetting effect of being connected to backlash areas. The indirect effect we detect in our main specifications is driven by exposure to the, also more numerous, persuasion areas which spread the persuasive nature of the march further whereas the backlash information seems not be propagated in a meaningful way.

D. Simulations of bias

We welcome the suggestion by a referee to run simulations taking the spatial distribution of the exposure to marches, social connection networks, and the magnitudes of direct and indirect effects as given, to explore the conditions under which unobserved further spread might render the estimation of direct and indirect effects questionable.

All simulations follow the same basic protocol. We start from t3 in our study, i.e., Hamburg in April 1932. We keep the 622 polling stations of our main sample, their baseline NSDAP support, and the marching route and network of social connections as measured. We set direct and 1st round indirect effect to what we estimate in our study, i.e. *NSDAP (t4) = NSDAP (t3) +* β_4 ** direct exposure (dt) +* γ_4 ** indirect exposure (it).* We then assume that there is a 2nd round of exposure, and test how estimates of β and γ are affected if we ignore this 2nd round of exposure.

We explore four different scenarios of potential 2nd (or higher order) round effects spreading through the social network. The specific structure of this 2nd round spread explored in the different scenarios will be decisive for the implied bias of our estimates. Below we outline the protocol for the data generating process of the simulations that will differ in the structure and timing of 2nd round spread (see points 6. and 7. in the protocol).

1. Start from a database of 622 polling stations on a map of Hamburg
2. Assign direct (dt) and indirect exposure (it) as observed in Hamburg 1932
3. Assign base (t3) NSDAP vote share as observed in Hamburg, April 1932
4. Define $\beta_4 - \beta_6$= 1.016 (estimated coefficient for direct exposure in t4)
5. Define γ_4 = .189 (estimated coefficient for indirect exposure in t4), γ_5 and γ_6 = .368 (estimated coefficient for indirect exposure in t5)
6. Define 2nd round exposure and correlation structure with direct and indirect exposure:
    a. Scenario 1: “Random 2nd round spread”
        i. randomly assigned independent of direct and indirect exposure measure (rnormal(1, .25))

b. Scenario 2 and 4: "Targeted 2nd round spread"
    i. randomly assigned independent of direct and indirect exposure measure (rnormal(1, .25))
    ii. multiplied by .5 for polling stations with direct exposure >= .5
    iii. multiplied by (additional) .5 for polling stations with indirect exposure >= 0 (above average)
c. Scenario 3: "Strongly targeted 2nd round spread"
    i. randomly assigned independent of direct and indirect exposure measure (rnormal(1, .25))
    ii. multiplied by .5 for polling stations with direct exposure >= .5
    iii. multiplied by (additional) .5 for polling stations with indirect exposure >= 0 (above average)
    iv. multiplied by 1.5 for polling station with direct exposure == 0 AND indirect exposure < 0 (below average)

7. Define timing and strength of 2nd round effect:
    a. Scenario 1 to 3:
        i. $\lambda_4 = 0$
        ii. $\lambda_5 \in [0,1]$
        iii. $\lambda_6 = \lambda_5 + \lambda_5 / 2$ [30]
    b. Scenario 4:
        i. $\lambda_4 \in [0,1]$
        ii. $\lambda_5 = \lambda_4 + \lambda_4 / 2$
        iii. $\lambda_6 = \lambda_5 + \lambda_5 / 2$
8. Generate simulated NSDAP vote shares
    a. nsdap4 = nsdap3 + β_4*dt + γ_4*it + λ_4* 2nd round spread
    b. nsdap5 = nsdap3 + β_5*dt + γ_5*it + λ_5* 2nd round spread
    c. nsdap6 = nsdap3 + β_6*dt + γ_6*it + λ_6* 2nd round spread
9. Estimate equation (5) and (6)
10. Repeat 100 times, collect estimates and compute average estimate and 95% CIs

In short, all simulations allow for the direct effect to spread more after the first

[30] This operationalization is chosen to reflect a potential decay in strength of the 2nd round effect over time. We explore three variants of the decay factor. No decay, i.e. $\lambda_6 = \lambda_5 + \lambda_5$; 50% decay, i.e. $\lambda_6 = \lambda_5 + \lambda_5 / 2$ or quadratic decay, i.e. $\lambda_6 = \lambda_5 + \lambda_5 * \lambda_5$. The qualitative pattern does not depend on the specific choice of decay factor.

round of indirect exposure. However, Scenario 1 assumes that this 2nd round of infection is uncorrelated with both direct exposure and the first round of infection. Scenario 2 ensures that 2nd round spread is muted in neighborhoods that are already directly and/or indirectly affected in the 1st round. Scenario 3 follows the setup of Simulation 2, but additionally assumes an amplified 2nd round effect in neighborhoods with zero direct and very low 1st round indirect exposure, e.g. an extremely targeted spread to the control group in our setup. Scenario 4 follows the setup of scenario 2, but assumes that the 2nd round effect already starts in t4, i.e. directly after the march.

All simulation figures show the estimated direct (Panels A) and indirect effect (Panels B) on the y-axis for varying levels of 2nd round exposure λ on the x-axis. Black dots indicate the average estimated effect using the corresponding scenario specifications. The dashed red line indicates the true (set as a parameter) effect size according to our protocol. Hence, the gap between black dots and red line is the bias in our estimate that would occur if we omitted the 2nd round spread.

Scenario 1 shows that if there is completely random second round spread, our estimates will not be affected (this resembles the case that you describe in comment 4). In this special case, directly treated, indirectly treated, and spread all grow proportionally to another. Hence, the unaffected estimates documented in Figure A.20, Panel A and B. However, note that the 2nd round spread with strength 1 would imply an additional 1 percentage point increase in NSDAP vote share in Hamburg in t5 and 1.5 percentage point increase in t6.

Scenario 2 indicates a case in which spread to already directly or indirectly units is muted. Spread therefore primarily affects previously untreated units, i.e. is designed to dilute the control group. Figure A.21, Panel A and B show that this additional spread would reduce the estimated direct effect and indirect effect by up to 30%, while a 2nd round effect coefficient size of 1 would imply an additional 0.93 percentage point increase in NSDAP votes share in Hamburg in t6.

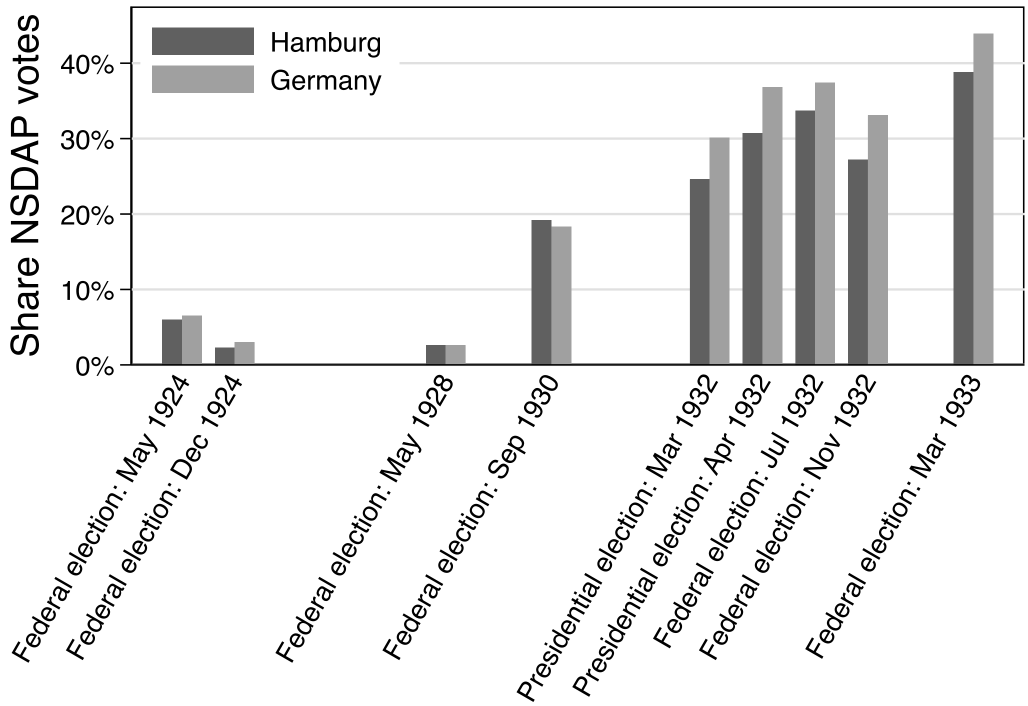
Scenario 3 indicates a case in which spread to already directly or indirectly units is muted as in scenario 2, but we additionally put extra weight in the spread on units that were previously almost completely untreated (no direct treatment at all and low level of indirect treatment). This would constitute an extreme case of additional spread that is clearly targeted at the control group diluting any direct and indirect effect. Figure A.22, Panel A and B show that this additional spread would reduce the estimated direct effect and indirect effect by up to 50%, while a 2nd round effect coefficient size of 1 would imply an additional 1.08 percentage point increase in NSDAP votes share in Hamburg in t6.

Scenario 4 is set up as scenario 2, i.e. designed to have some targeted spread to previously untreated units. However, we vary the timing of the spread. Instead of only occurring from t4 to the subsequent periods, we assume 2nd round spread already occurs between t3 and t4, i.e. directly after the treatment. Figure A.23, Panel A and B show that this additional spread would reduce the estimated direct effect and indirect effect by up to 60%, while a 2nd round effect coefficient size of 1 would imply an additional 1.11 percentage point increase in NSDAP votes share in Hamburg in t6.

In sum, we take the simulations as additional supporting evidence that unobserved 2nd round spread, while a valid concern in theory, in our setting does not provide a threat for identification and a causal interpretation of the effects of the march.

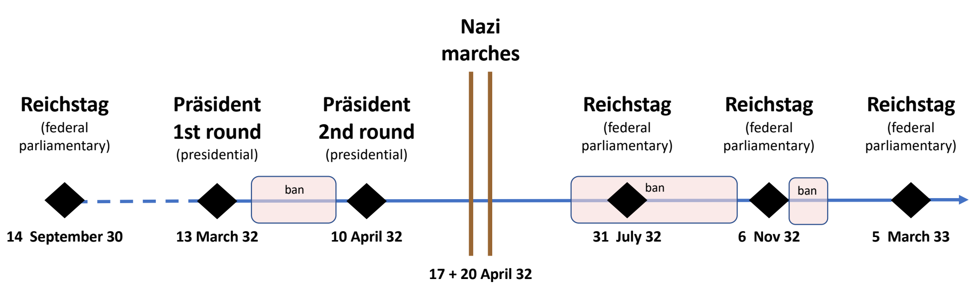
E Appendix Figures and Tables

Figure A.1: City and National Election Results in Hamburg, NSDAP, 1924-1933.



Note: The Figure reports the NSDAP vote share in parliamentary and Hitler vote share in presidential elections for Germany and Hamburg between 1924 and 1933. Sources: Germany (Falter, 1986); Hamburg (Büttner, 1982).

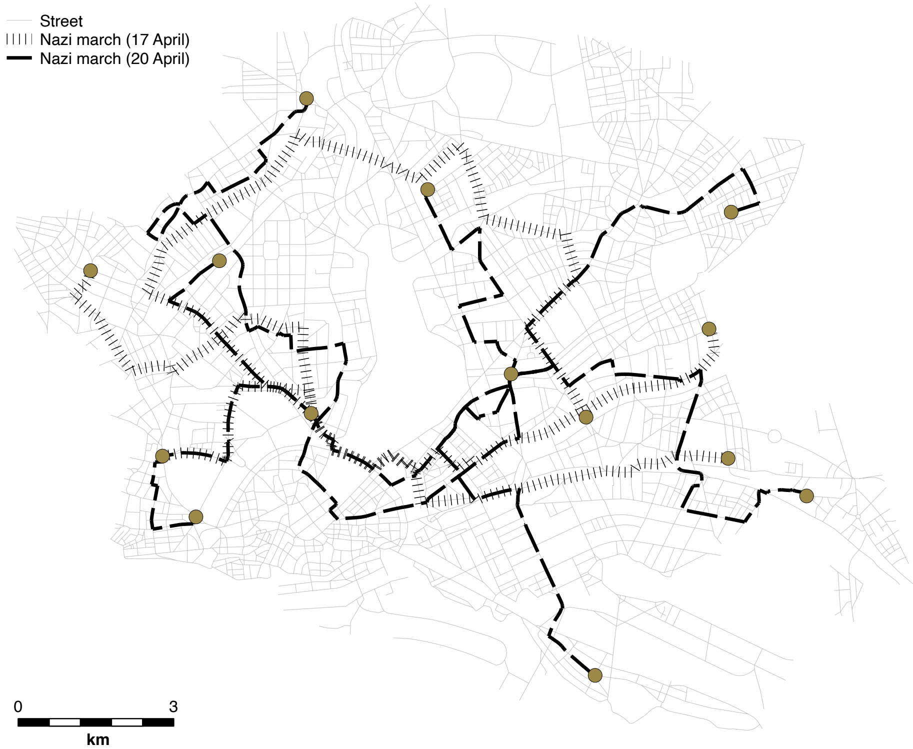
Figure A.2: Timeline of events, Hamburg, 1930-1933.



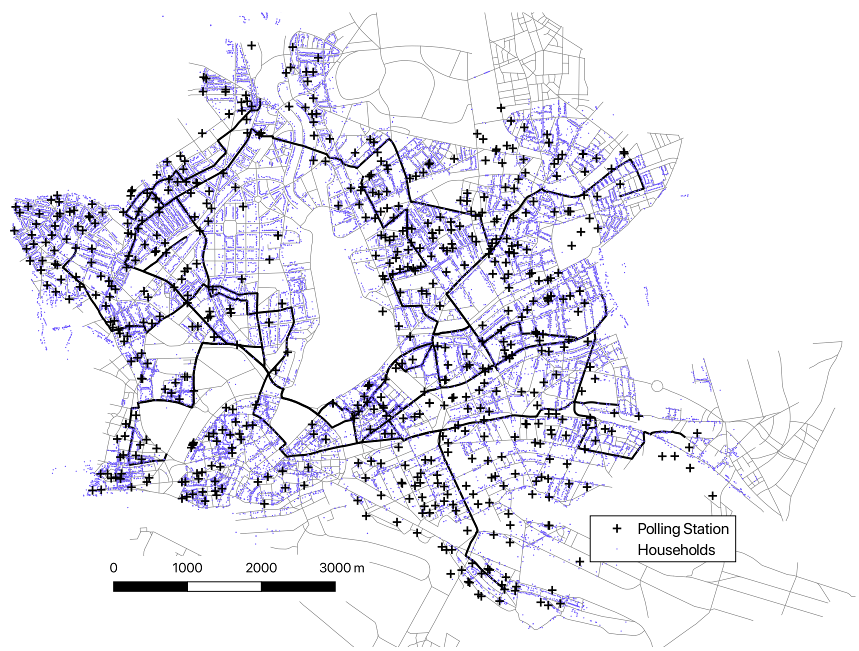
Note: The Figure shows a timeline of the major events. Between 20 March and 03 April 1932, between 18 July and 31 August 1932 and between 06 November and 19 November, all public outdoor political events were banned (Reichsministerium des Inneren, 1932, Nr. 17, 46, 52, 54, 72).

Figure A.3: Marches, households and polling stations.

(a) Nazi marches in Hamburg in April 1932



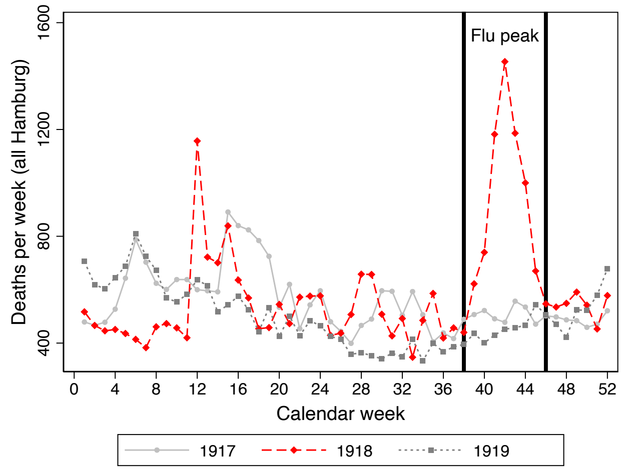
(b) Households and polling stations



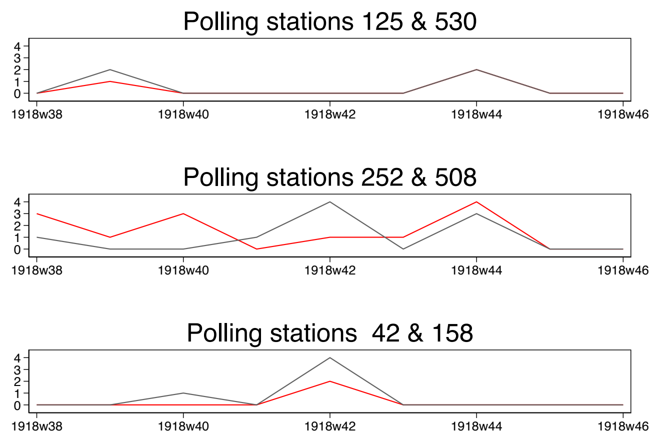
Note: Panel A: The map shows the routes of the two Nazi marches on April 17th, 1932 (short dash) and April 20th, 1932 (long dash). Brown circles mark the start-, mid- and end-points of the marches. Panel B: The map shows the location of the polling stations in Hamburg (black crosses) and the addresses of the 400,000 households living in Hamburg in 1932 (blue dots). We overlay these locations on the street network of Hamburg. Sources: Nazi marches: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b); street network: historical map of 1930-1940 Hamburg (Landesbetrieb Geoinformation und Vermessung Hamburg, 2020); households data: 1932 Hamburg address book (Hamburger Adreßbuch, 1932).

Figure A.4: Spanish flu deaths.

(a) Weekly deaths (Hamburg)



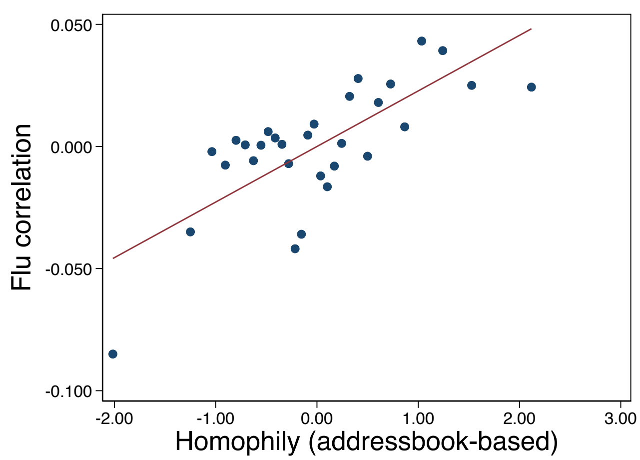
(b) Weekly deaths during peak of pandemic (6 polling stations)



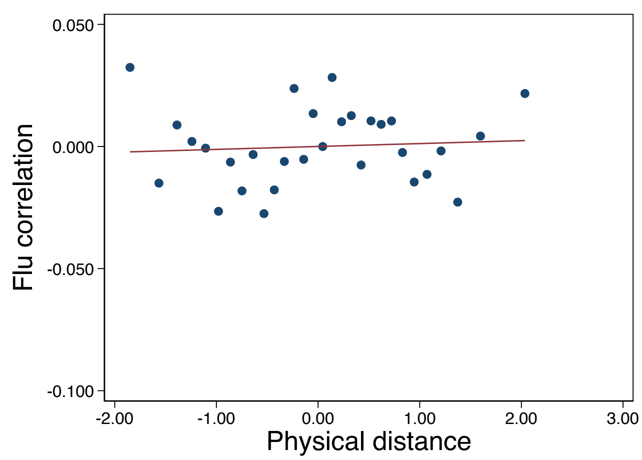
Note: Panel A: Weekly deaths in the city of Hamburg for 1917 to 1919. Panel B: Weekly deaths in 6 separate polls during peak of the pandemic. Source: Hamburg death records (State Archive Hamburg, 1917; 1918; 1919).

Figure A.5: Validation flu correlation measure.

(a) Flu correlation and homophily

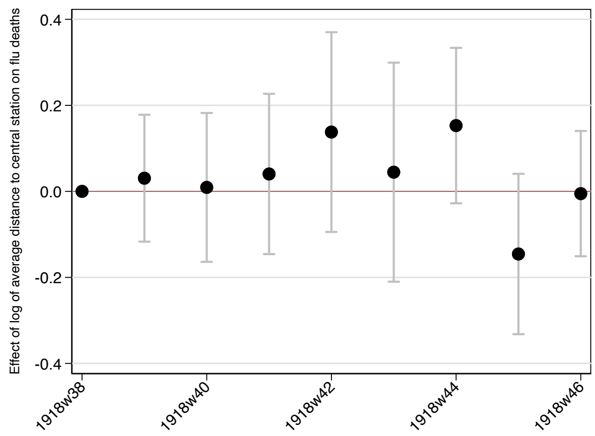


(b) Flu correlation and physical distance



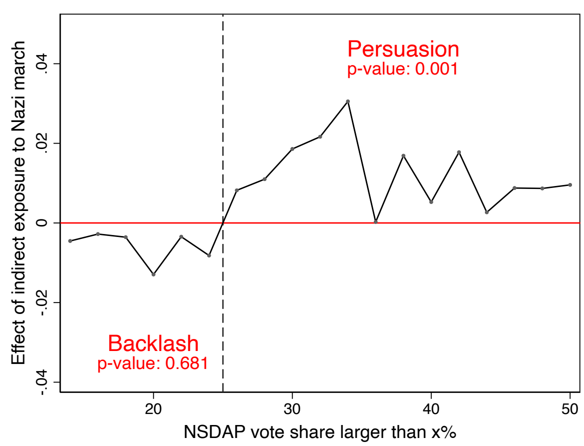
Note: The unit of observation is a polling station pair. Panel A: Bin-scatter of homophily between two polling stations (x-axis) against their correlation in influenza deaths (y-axis) conditional on physical distance between two polling stations and sociodemographic controls. Panel B: Bin-scatter of physical distance between two polling stations (x-axis) against their correlation in influenza deaths (y-axis) conditional on sociodemographic controls. See appendix for construction of correlation in influenza deaths and homophily measures.

Figure A.6: Spread of Spanish flu.



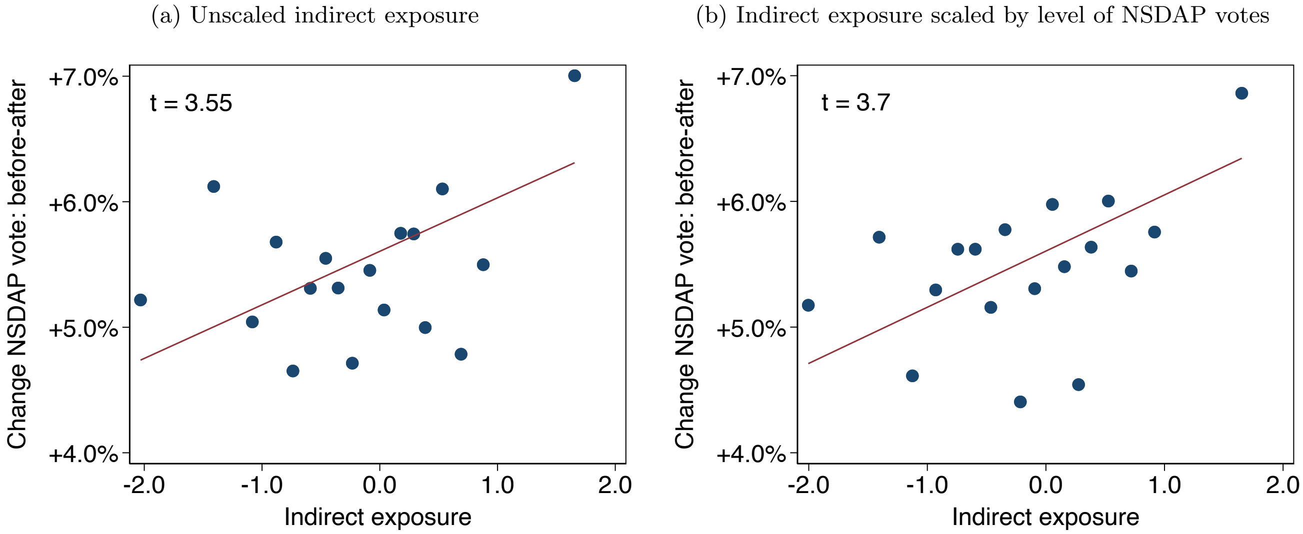
Note: Plot of coefficient estimates of regression of flu deaths on log distance to central train station by week of 1918 flu peak including full set of demographic and street controls interacted with week fixed effects as well as polling station and week fixed effects. Source: Hamburg death records (State Archive Hamburg, 1917; 1918; 1919).

Figure A.7: Distribution Regression of indirect exposure.



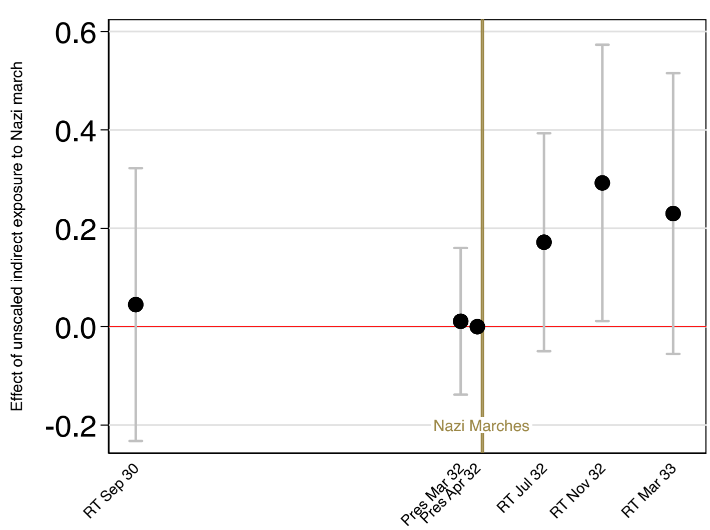
Note: The figure plots coefficients of flu-based indirect exposure estimated from equation (3) using the main panel of 1932 and 1933 elections (y-axis). Dependent variable is an indicator = 1, if NSDAP vote share is above the threshold indicated on the x-axis. We use thresholds ranging from the 5^{th} to the 95^{th} percentile of the NSDAP vote share distribution in steps of two percentage points. The vertical line indicates the crossing point between backlash (= negative effect of indirect exposure to the march) and persuasion (= positive effect of indirect exposure to the march). The p-value for backlash is obtained from testing against H0: All coefficients of indirect exposure for NSDAP vote share thresholds below 25% are jointly equal to zero. For the joint F-test, we estimate a seemingly unrelated regressions (SUR) model as introduced by Zellner (1962) with equations for all NSDAP vote share thresholds between 14% (5^{th} percentile of the NSDAP vote share distribution) and 25% (crossing point) in steps of two percentage points as dependent variables and the right-hand-side from equation (3). Analogously, we obtain the p-value for persuasion from testing against H0: All coefficients of indirect exposure for NSDAP vote share thresholds above 25% are jointly equal to zero following the same procedure. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. We additionally control for direct exposure to the march. Standard errors are clustered at the polling station level.

Figure A.8: Scaling of indirect exposure.



Note: Robustness: Panel A: The Figure plots a bin-scatter of flu-based indirect exposure without scaling by change in NSDAP votes in connected regions (x-axis) against the change in NSDAP vote share after the marches (y-axis). Change in NSDAP vote is calculated between two election before the marches (13 March and 10 April 1932) and three elections after (31 July; 6 November 1932 and 5 March 1933). *t*-statistic is estimated from a bivariate regression with robust standard errors. Panel B: The Figure plots a bin-scatter of flu-based indirect exposure scaled by the t3 (10 April 1932) level of NSDAP votes in connected regions (x-axis) against the change in NSDAP vote share after the marches (y-axis). Change in NSDAP vote is calculated between two election before the marches (13 March and 10 April 1932) and three elections after (31 July; 6 November 1932 and 5 March 1933). *t*-statistic is estimated from a bivariate regression with robust standard errors. See main text and appendix A for construction of the flu-based indirect exposure measure.

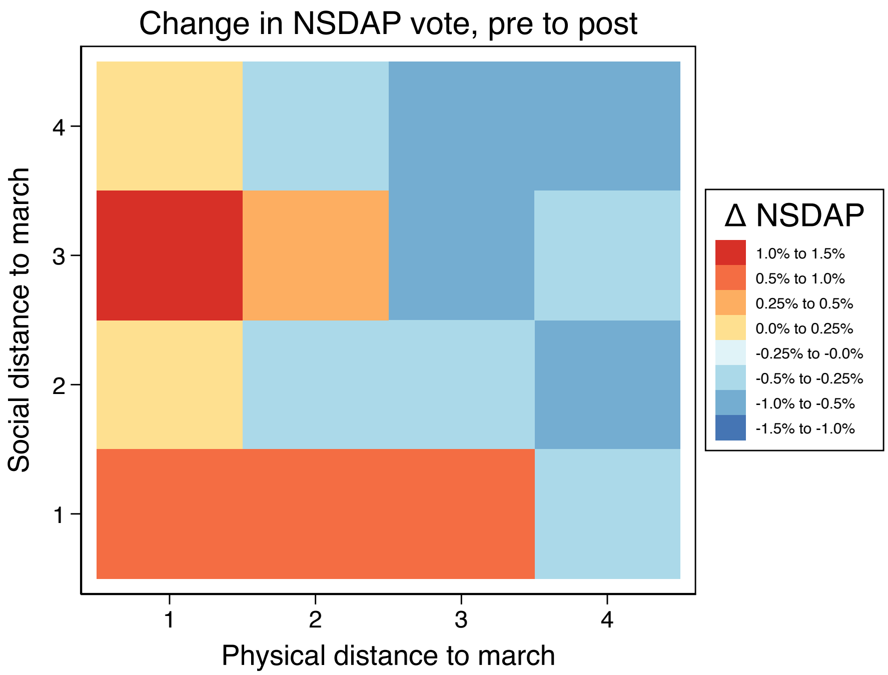
Figure A.9: Unscaled indirect exposure (flu-based).



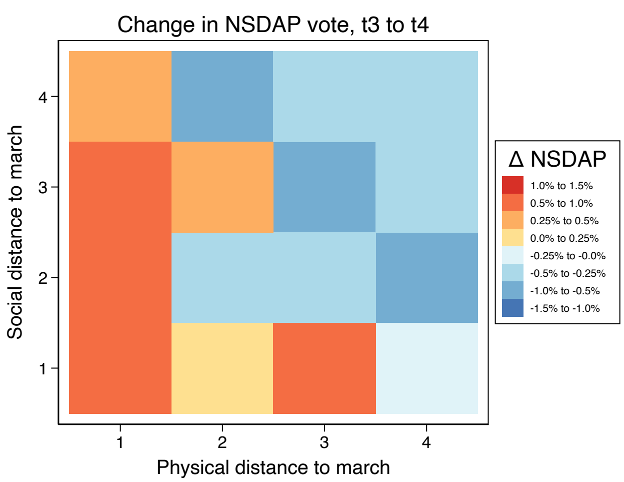
Note: Robustness: Plot of estimates of indirect exposure based on flu connections without scaling by change in NSDAP votes in connected regions estimated from equation (6) and corresponding 95% confidence intervals by election. See main text and appendix A for construction of the flu-based indirect exposure measure.

Figure A.10: Change in vote share by exposure quintiles.

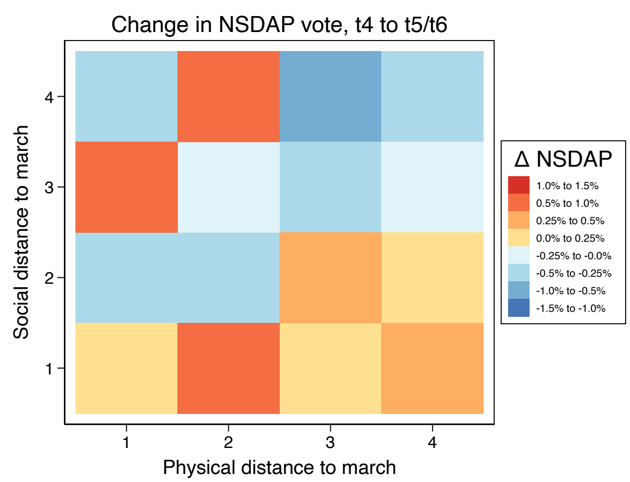
(a) Pre to post



(b) t3 to t4

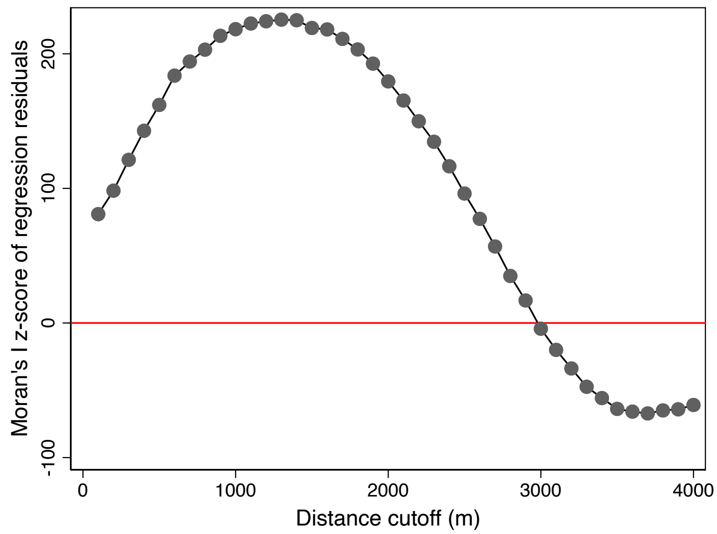


(c) t4 to t5/t6



Note: Plot of change in vote share by treatment status. We partition polling stations into 16 groups of roughly similar size, based on the quartile of residual physical (x-axis) and residual social (y-axis) distance to the march. The first quartile indicates highest exposure each. We color-code cells based on the change in residual NSDAP vote. Dark red indicating the strongest positive and dark blue indicating the strongest negative change. Residual physical distance, social distance and NSDAP vote are obtained from estimating regressions using the full set of demographic and street network characteristics and election fixed effects as regressors. Panel A plots the change in residual NSDAP vote calculated between two election before the marches (13 March and 10 April 1932) and three elections after (31 July; 6 November 1932 and 5 March 1933). Panel B plots the change in residual NSDAP vote calculated between the election right before the marches (10 April 1932) and the first post treatment election (31 July 1933). Panel C plots the change in residual NSDAP vote calculated between the first post treatment election (31 July 1933) and the two elections after (6 November 1932 and 5 March 1933).

Figure A.11: Spatial autocorrelation of regression residuals.



Note: Robustness: Plot of Moran's I z-score of regression residuals (y-axis) estimated from equation (5) using the main panel of 1932 and 1933 elections. Moran's I is computed using a binary spatial weight matrix with varying distance cutoffs (x-axis).

Figure A.12: Random marches.

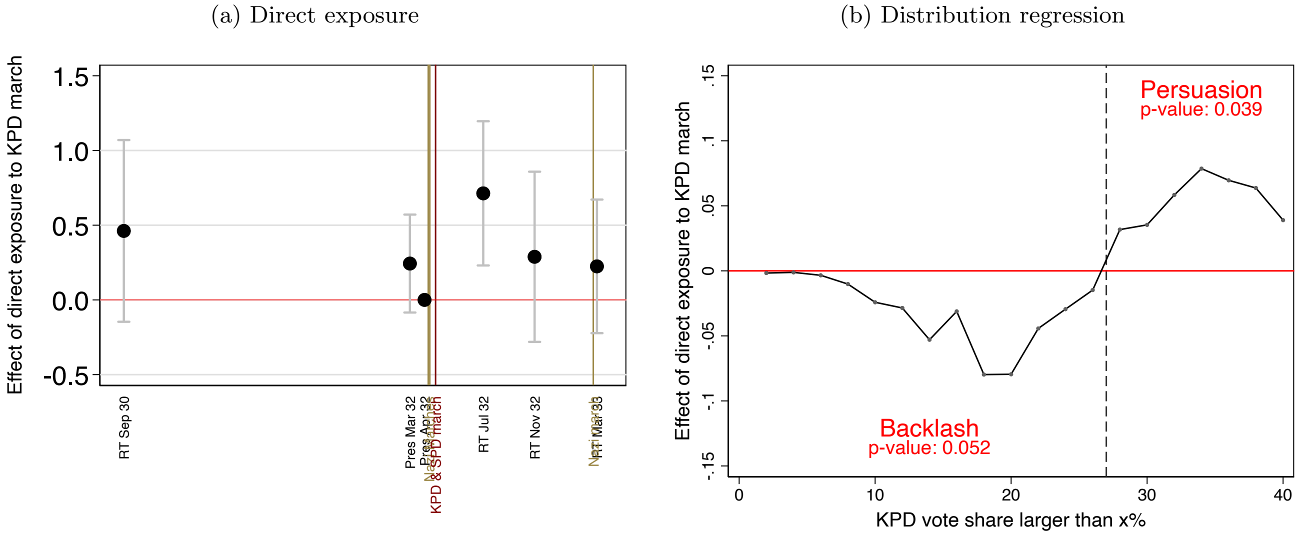
(a) Random march examples

Note: Robustness: Illustration of randomly generated marches. We first generate seven random points (five starting-, one mid- and one endpoint) that are at least 500m away from another. Next, we compute least cost paths with a preference for wide streets between each start- and midpoint and between mid- and endpoint. We repeat this to obtain 12 marching sections in total mimicking the structure of the actual marches. We then compute distance of households to the random march and indirect exposure to the random march using flu connectivity between neighborhoods. We run 500 simulations of this procedure. The figure plots three examples of random marches over the street network of Hamburg.

Figure A.13: Propaganda marches.

Note: Robustness: Panel A shows the route of the two main Nazi marches (17 and 20 April 1932); Panel B shows the route of the 28 February 1933 Nazi march. Panel C shows the route of the 1 May 1932 KPD (communists) march. Panel D shows the route of the 1 May 1932 SPD (social democrats) march.

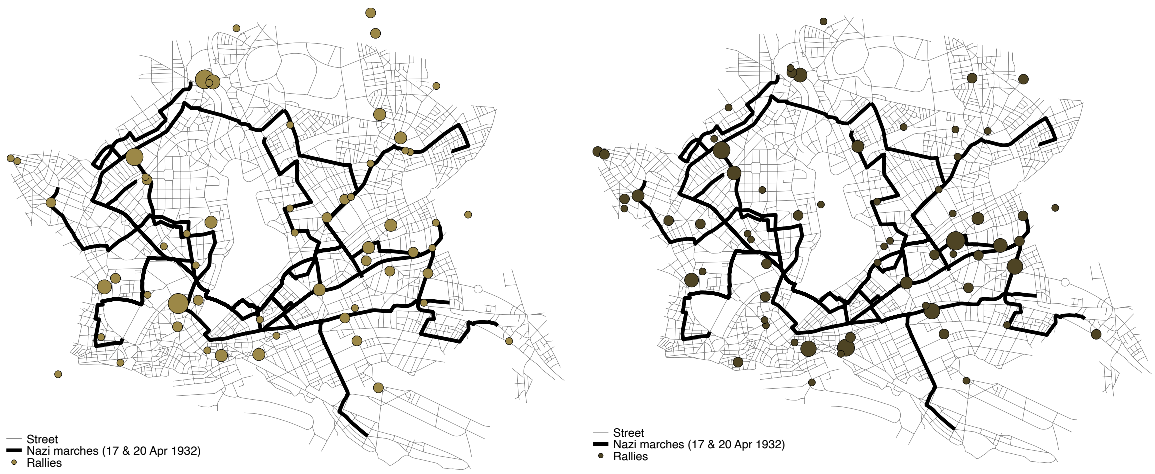
Figure A.14: Communist march.



Note: Robustness: Panel A plots estimates of direct exposure (share of households within 200m of KPD march) estimated from equation (6) and corresponding 95% confidence intervals by election. Dependent variable is KPD vote share. Panel B: The figure plots coefficients of direct exposure estimated from equation (3) using the main panel of 1932 and 1933 elections (y-axis). Dependent variable is an indicator = 1, if KPD vote share is above the threshold indicated on the x-axis. We use thresholds ranging from the 5^{th} to the 95^{th} percentile of the KPD vote share distribution in steps of two percentage points. The vertical line indicates the crossing point between backlash (= negative effect of direct exposure to the march) and persuasion (= positive effect of direct exposure to the march). The p-value for backlash is obtained from testing against H0: All coefficients of direct exposure for KPD vote share thresholds below 27% are jointly equal to zero. For the joint F-test, we estimate a seemingly unrelated regressions (SUR) model as introduced by Zellner (1962) with equations for all KPD vote share thresholds between 2% (5^{th} percentile of the KPD vote share distribution) and 27% (crossing point) in steps of two percentage points as dependent variables and the right-hand-side from equation (3). Analogously, we obtain the p-value for persuasion from testing against H0: All coefficients of direct exposure for KPD vote share thresholds above 27% are jointly equal to zero following the same procedure. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. Standard errors are clustered at the polling station level.

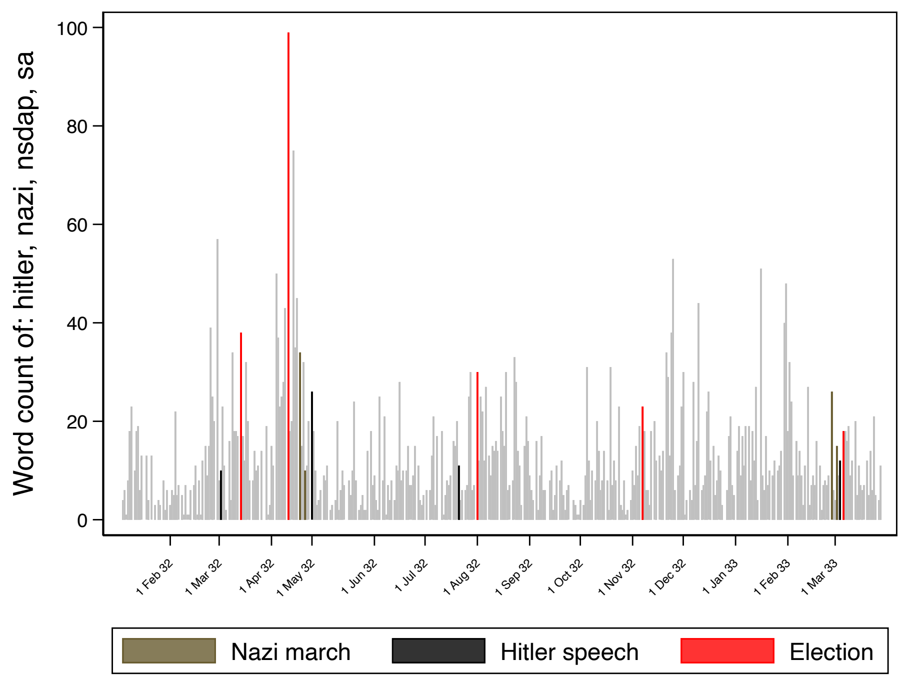
Figure A.15: Nazi rallies.

(a) Before 20 Apr 1932 (b) After 20 Apr 1932



Note: Robustness: Panel A shows location of Nazi rallies before 20 April 1932 (before marches). Panel B displays location of Nazi rallies after 20 April 1932. Size of the circle is dependent on number of rallies held at that location.

Figure A.16: Media coverage.

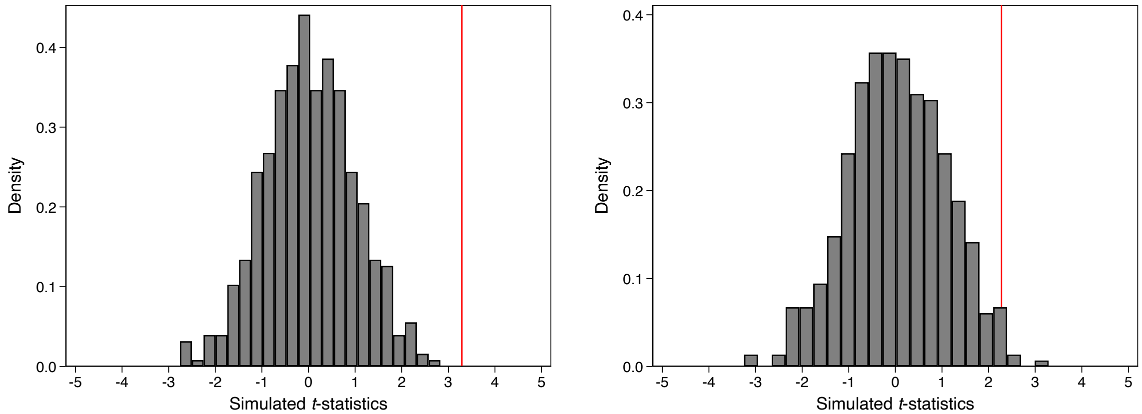


Note: Robustness: Bars show frequency of NSDAP mentions (measured as wordcount of 'Hitler', 'Nazi', 'NSDAP' OR 'SA') in digitized fulltext version of the *Hamburger Anzeiger* over time (from 1 January 1932 to 31 March 1933). Brown bars mark day of first newspaper after a march. Black bars mark day of first newspaper after a Hitler speech. Red bars mark day of first newspaper after a national election.

Figure A.17: Randomization inference.

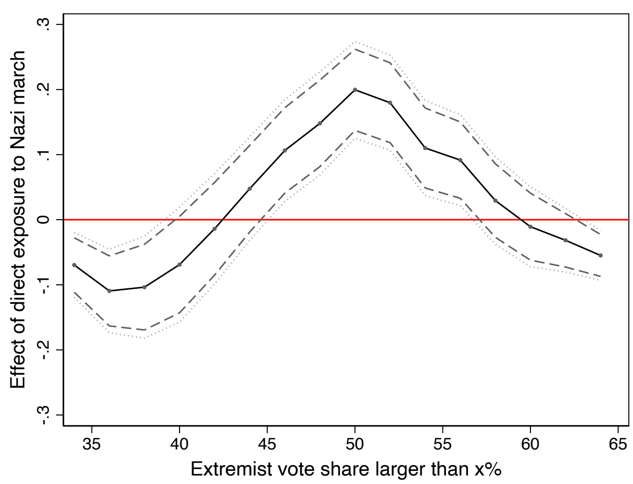
(a) t-stats direct exposure

(b) t-stats indirect exposure



Note: Robustness: Panel A reports distribution of t-statistics for direct exposure obtained from 500 simulations of estimating equation (5) using the main panel of 1932 and 1933 elections permuting the location of polling stations. In each iteration, we recalculate direct exposure and street characteristics after permuting the location of polling stations and keep everything else (sociodemographic characteristics, indirect exposure, NSDAP vote shares) fixed. Pabel B reports t-statistics for flu-based indirect exposure obtained from 500 simulations of estimating equation (5) permuting the connectivity measure between polling station pairs using the main panel of 1932 and 1933 elections. In each interation, we recalculate indirect exposure to the march after permuting the connectivity network and keep everything else (direct exposure, street and sociodemographic characteristics, NSDAP vote shares) fixed. The red lines indicate the t-statistic obtained from estimating equation (5) with our real data.

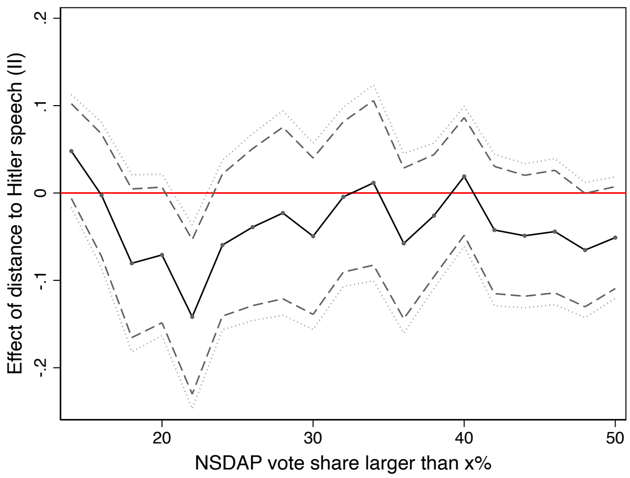
Figure A.18: Extremist vote share distribution regression.



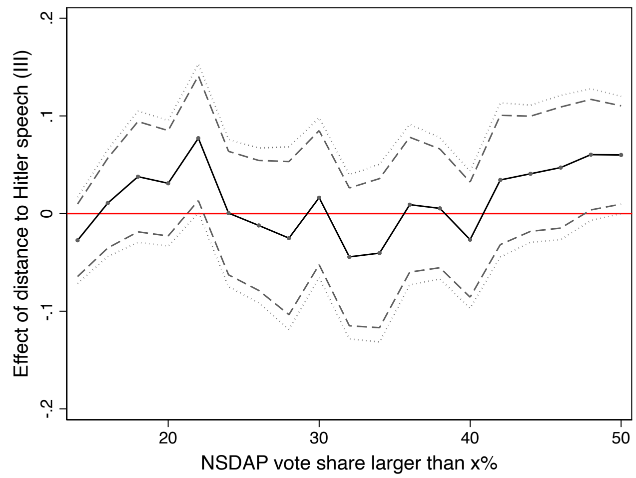
Note: The figure plots coefficients of direct exposure estimated from equation (3) using the main panel of 1932 and 1933 elections (y-axis). Dependent variable is an indicator = 1, if extremist vote share (NSDAP + KPD) is above the threshold indicated on the x-axis. We use thresholds ranging from the 5^{th} to the 95^{th} percentile of the extremist vote share distribution in steps of two percentage points. The long dashed line indicates 90% confidence intervals for each step of the vote share distribution. The short dashed lines indicates 95% confidence intervals for each step of the vote share distribution. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. We additionally control for indirect exposure to the march. Standard errors are clustered at the polling station level.

Figure A.19: Hitler speeches distribution regression.

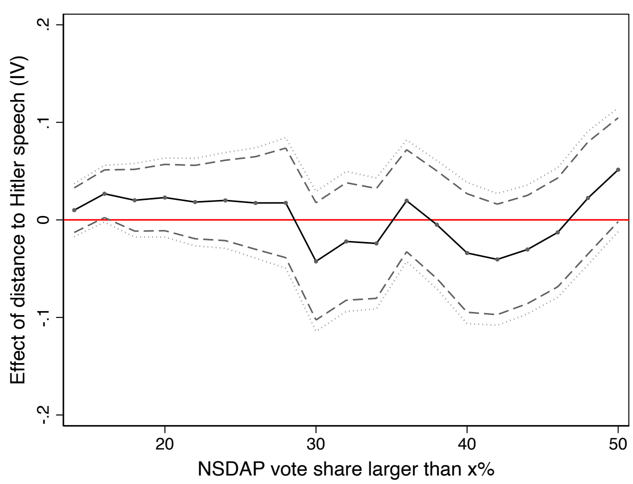
(a) Speech II



(b) Speech III



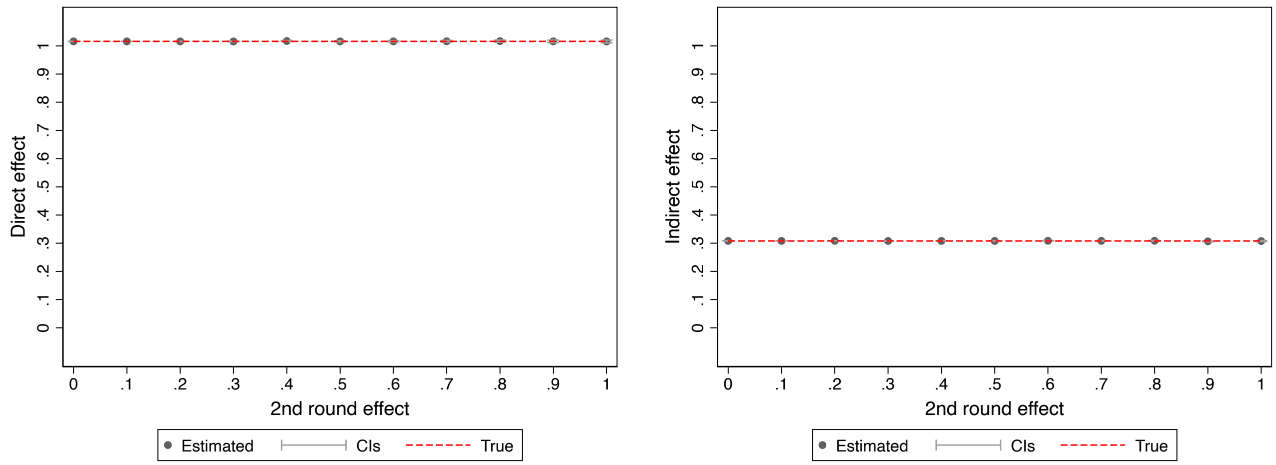
(c) Speech IV



Note: *Note*: The figure plots coefficients of direct exposure to Hitler speeches estimated from augmenting equation (3) with exposure to the three Hitler speeches using the main panel of 1932 and 1933 elections (y-axis). Dependent variable is an indicator = 1, if NSDAP vote share is above the threshold indicated on the x-axis. We use thresholds ranging from the 5^{th} to the 95^{th} percentile of the NSDAP vote share distribution in steps of two percentage points. The long dashed line indicates 90% confidence intervals for each step of the vote share distribution. The short dashed lines indicates 95% confidence intervals for each step of the vote share distribution. Standard errors are clustered at the polling station level. Panel A plots estimtates of log of distance to Hitler speech (II) at Dirt-Track-Bahn Fuhlsbüttel (23 April 1932), Panel B of log of distance to Hitler speech (III) at Victoria-Stadion Hoheluft (20 July 1932), Panel C of log of distance to Hitler speech (IV) at Zoo Hallen (03 March 1933) interacted with an indicator = 1 for election after corresponding speech. Hitler speech (I) takes place at Sagebiel Säle on 1 March 1932. This is before the first election observed in the main time period of study. Hence we cannot observe the impact in the first difference setting for the full sample. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. We additionally control for indirect exposure to the march. Standard errors are clustered at the polling station level.

Figure A.20: Second round spread simulations: Scenario 1.

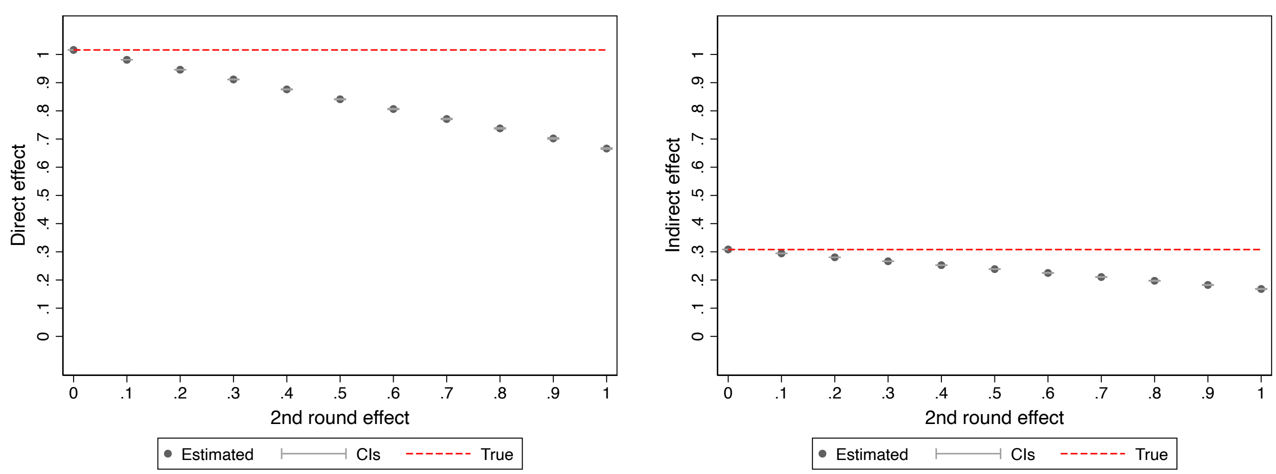
(a) Direct exposure (b) Indirect exposure



Note: The figure plots results of the bias simulations described in detail in Appendix D using the setup of Scenario 1. We plot the estimated direct (Panels A) and indirect effect (Panels B) on the y-axis for varying levels of 2nd round exposure λ on the x-axis using the main panel of 1932 and 1933 elections. Black dots indicate the average estimated effect obtained from 100 simulations using the setup of Scenario 1. The dashed red line indicates the true (set as a parameter) effect size according to our protocol. Hence, the gap between black dots and red line is the bias in our estimate that would occur if we omitted the 2nd round spread. Confidence intervals plotted in grey are obtained from the standard error in estimated effect size across 100 simulations.

Figure A.21: Second round spread simulations: Scenario 2.

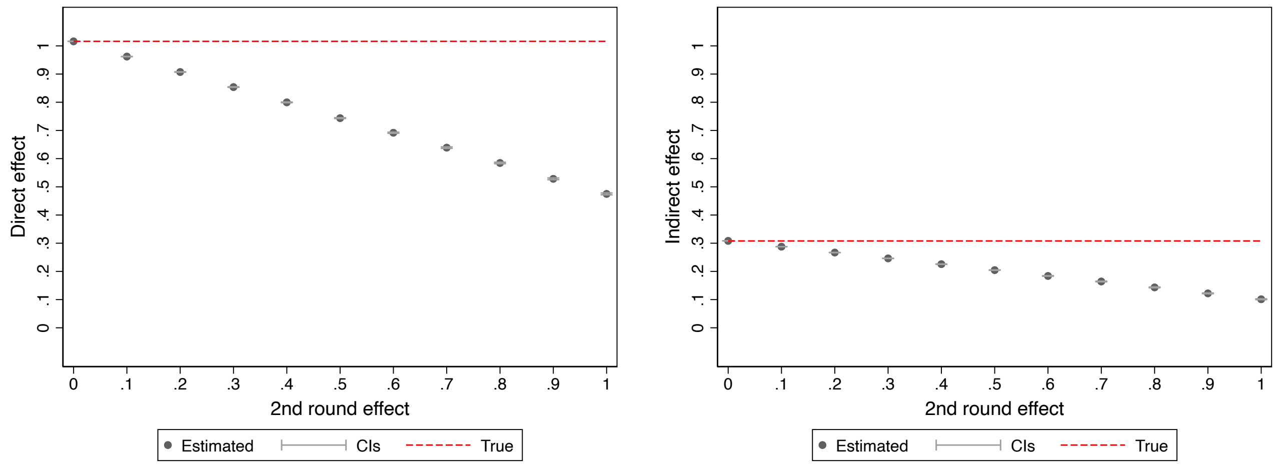
(a) Direct exposure (b) Indirect exposure



Note: The figure plots results of the bias simulations described in detail in Appendix D using the setup of Scenario 2. We plot the estimated direct (Panels A) and indirect effect (Panels B) on the y-axis for varying levels of 2nd round exposure λ on the x-axis using the main panel of 1932 and 1933 elections. Black dots indicate the average estimated effect obtained from 100 simulations using the setup of Scenario 2. The dashed red line indicates the true (set as a parameter) effect size according to our protocol. Hence, the gap between black dots and red line is the bias in our estimate that would occur if we omitted the 2nd round spread. Confidence intervals plotted in grey are obtained from the standard error in estimated effect size across 100 simulations.

Figure A.22: Second round spread simulations: Scenario 3.

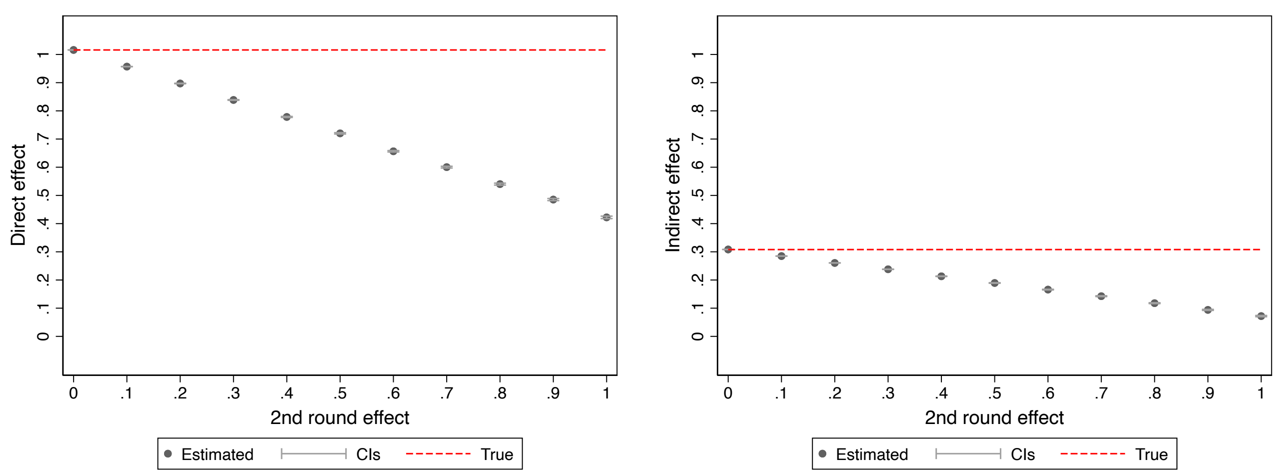
(a) Direct exposure (b) Indirect exposure



Note: The figure plots results of the bias simulations described in detail in Appendix D using the setup of Scenario 3. We plot the estimated direct (Panels A) and indirect effect (Panels B) on the y-axis for varying levels of 2nd round exposure λ on the x-axis using the main panel of 1932 and 1933 elections. Black dots indicate the average estimated effect obtained from 100 simulations using the setup of Scenario 3. The dashed red line indicates the true (set as a parameter) effect size according to our protocol. Hence, the gap between black dots and red line is the bias in our estimate that would occur if we omitted the 2nd round spread. Confidence intervals plotted in grey are obtained from the standard error in estimated effect size across 100 simulations.

Figure A.23: Second round spread simulations: Scenario 4.

(a) Direct exposure (b) Indirect exposure



Note: The figure plots results of the bias simulations described in detail in Appendix D using the setup of Scenario 4. We plot the estimated direct (Panels A) and indirect effect (Panels B) on the y-axis for varying levels of 2nd round exposure λ on the x-axis using the main panel of 1932 and 1933 elections. Black dots indicate the average estimated effect obtained from 100 simulations using the setup of Scenario 4. The dashed red line indicates the true (set as a parameter) effect size according to our protocol. Hence, the gap between black dots and red line is the bias in our estimate that would occur if we omitted the 2nd round spread. Confidence intervals plotted in grey are obtained from the standard error in estimated effect size across 100 simulations.

Table A.1: Robustness. Results excluding polling stations that changed address.

	% NSDAP votes		
	(1)	(2)	(3)
Share households directly exposed (200m) × t6 (post)	0.952		0.874
	[0.408]		[0.408]
Share households directly exposed (200m) × t5 (post)	0.905		0.819
	[0.373]		[0.371]
Share households directly exposed (200m) × t4 (post)	1.007		0.969
	[0.310]		[0.309]
Share households directly exposed (200m) × t2 (pre)	0.024		0.021
	[0.237]		[0.239]
Share households directly exposed (200m) × t1 (pre)	0.034		0.027
	[0.414]		[0.414]
Indirect exposure of households × t6 (post)		0.336	0.307
		[0.149]	[0.149]
Indirect exposure of households × t5 (post)		0.364	0.338
		[0.144]	[0.145]
Indirect exposure of households × t4 (post)		0.184	0.152
		[0.114]	[0.114]
Indirect exposure of households × t2 (pre)		0.014	0.013
		[0.079]	[0.080]
Indirect exposure of households × t1 (pre)		0.031	0.033
		[0.142]	[0.142]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
R^2	0.933	0.933	0.934
Mean NSDAP vote in 10 Apr '32 election	30.611	30.611	30.611
Observations	3258	3258	3258

Note: Robustness: Regressions on sub sample of polling stations with stable addresses in 1932 and 1933. 62 polling stations change their address at least once between the different 1932 and 1933 elections. See statistical bulletin of Hamburg (Sköllin, 1930; 1932a; 1932b; 1933) for list of polling station addresses in each election. Dependent variable is the share of NSDAP votes. Sample in all columns excluding polling station that change their address at least once. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. Standard errors clustered at polling station level in brackets.

Table A.2: Pre-trends.

	13 March 1932 - 10 April 1932			12 Sep 1930 - 10 April 1932		
	(1)	(2)	(3)	(4)	(5)	(6)
	Δ NSDAP	Δ KPD	Δ turnout	Δ NSDAP	Δ KPD	Δ turnout
Share households directly exposed (200m)	-0.034	0.075	-0.007	-0.261	0.779	0.461
	[0.216]	[0.141]	[0.160]	[0.401]	[0.314]	[0.271]
Indirect exposure of households	-0.053	-0.043	-0.013	-0.092	0.014	0.102
	[0.074]	[0.051]	[0.054]	[0.136]	[0.112]	[0.108]
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Street controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.552	0.510	0.146	0.521	0.576	0.128
Mean change in Y	6.157	-2.793	-4.757	11.472	-5.809	0.790
Observations	622	622	622	515	515	514

Note: Columns 1 to 3 show immediate pre-trends (first presidential election on 13 March 1932 round to second presidential election on 10 April 1932) of election outcomes. Columns 4 to 6 show long pre-trends (12 September 1930 parliamentary election to second round presidential election 10 April 1932). Estimates of regressing NSDAP vote share (cols. 1 and 4); KPD vote share (cols. 2 and 5); voter turnout (cols. 3 and 6) as dependent variable on log of average distance to Nazi march. In all regressions we control for demographic and street network controls. Standard errors clustered at polling station level in brackets.

Table A.3: Effect of the march by bins of direct and indirect exposure.

	% NSDAP votes		
	(1)	(2)	(3)
Distance to march (1 quartile) × post march	1.090		1.086
	[0.270]		[0.269]
Distance to march (2 quartile) × post march	0.415		0.422
	[0.262]		[0.263]
Distance to march (3 quartile) × post march	0.330		0.329
	[0.269]		[0.265]
Indirect exposure (1 quartile) × post march		0.697	0.665
		[0.270]	[0.268]
Indirect exposure (2 quartile) × post march		0.561	0.578
		[0.258]	[0.258]
Indirect exposure (3 quartile) × post march		-0.070	-0.078
		[0.266]	[0.265]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
R^2	0.915	0.914	0.916
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417
Observations	3110	3110	3110

Note: Dependent variable is share NSDAP votes. Col. 1: Estimates of equation (1) with quartiles of distance to march (1 quartile = closest to march) as exposure measure. The fourth quartile is the omitted reference category. Col. 2: Estimates of equation (5) with quartiles of indirect exposure (1 quartile = highest indirect exposure to march) as exposure measure. The fourth quartile is the omitted reference category. Col. 3: Estimates of equation (5) with both, quartiles of distance to march and quartiles of indirect exposure as exposure measures. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. We use the main panel of 1932 and 1933 elections. Standard errors clustered at polling station level in brackets.

Table A.4: Panel A. Standard deviation effects of direct exposure.

	β (D1)	1 sd H^T/H (D2)	1 sd % NSDAP_t - % NSDAP_3 (D3)	Effect of 1 sd (D4: D1×D2/D3)
t=4	1.02	0.36	2.8	13.08%
t=5	0.97	0.36	3.7	9.58%
t=6	1.00	0.36	3.6	9.95%

Table A.4: Panel B. Standard deviation effects of indirect exposure.

	γ (I1)	1 sd $\rho^T - \rho^C$ (I2)	1 sd % NSDAP_t - % NSDAP_3 (I3)	Effect of 1 sd (I4: I1×I2/I3)
t=4	0.19	1.00	2.8	6.68%
t=5	0.37	1.00	3.7	9.96%
t=6	0.33	1.00	3.6	8.93%

Table A.4: Panel C. Relative standard deviation effect sizes.

	Total (D4+I4)	Direct (D4/Total)	Indirect (I4/Total)
t=4	19.76%	66%	34%
t=5	19.54%	49%	51%
t=6	18.88%	53%	47%

Note: Panel A: (D1) lists estimates of the coefficient of the share of households within 200m of Nazi march as measure of exposure estimating equation (6). (D2) displays the magnitude of a one-standard deviation in share of households within 200m of Nazi march. (D3) reports the magnitudes of a one standard deviation change in NSDAP vote share between post-treatment elections (t4-t6) and the last pre-treatment election (t3). (D4) reports the effect explained by a one standard deviation change in the exposure variable relative to the overall change in NSDAP vote share for each post-treatment election. Panel B: (I1) lists estimates of the coefficient of flu-based indirect exposure estimating equation (6). (I2) displays the magnitude of a one-standard deviation in indirect exposure. (I3) reports the magnitudes of a one standard deviation change in NSDAP vote share between post-treatment elections (t4-t6) and the last pre-treatment election (t3). (I4) reports the effect explained by a one standard deviation change in the exposure variable relative to the overall change in NSDAP vote share for each post-treatment election. Panel C: Col. 1 shows the combined effect of one standard-deviation changes in direct and indirect exposure. Col. 2 reports the relative size of the direct effect over time. Col. 3 displays the relative size of the indirect effect by post-treatment election. See main text and appendix for construction of the flu-based indirect exposure measure.

Table A.5: Direct and indirect effect: turnout and log voters.

	Turnout			log voters		
	(1)	(2)	(3)	(4)	(5)	(6)
Share households directly exposed (200m) × t6 (post)	-0.028		-0.037	0.007		0.006
	[0.224]		[0.225]	[0.005]		[0.005]
Share households directly exposed (200m) × t5 (post)	-0.744		-0.746	0.008		0.007
	[0.238]		[0.240]	[0.004]		[0.004]
Share households directly exposed (200m) × t4 (post)	-0.179		-0.192	0.002		0.002
	[0.295]		[0.294]	[0.004]		[0.004]
Share households directly exposed (200m) × t2 (pre)	0.009		0.007	-0.002		-0.002
	[0.160]		[0.160]	[0.002]		[0.002]
Share households directly exposed (200m) × t1 (pre)	-0.417		-0.401	-0.009		-0.009
	[0.271]		[0.271]	[0.010]		[0.010]
Indirect exposure of households × t6 (post)		0.045	0.046		0.005	0.005
		[0.078]	[0.078]		[0.002]	[0.002]
Indirect exposure of households × t5 (post)		-0.004	0.014		0.003	0.003
		[0.090]	[0.090]		[0.002]	[0.002]
Indirect exposure of households × t4 (post)		0.064	0.069		0.002	0.002
		[0.111]	[0.111]		[0.002]	[0.002]
Indirect exposure of households × t2 (pre)		0.013	0.013		0.000	0.001
		[0.054]	[0.054]		[0.001]	[0.001]
Indirect exposure of households × t1 (pre)		-0.090	-0.079		0.001	0.001
		[0.107]	[0.107]		[0.005]	[0.005]
Election & polling station FEs	Yes	Yes	Yes	Yes	Yes	Yes
Log of number of voters (t3) × election FEs	Yes	Yes	Yes	No	No	No
Other demographic controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.860	0.860	0.860	0.162	0.161	0.164
Mean dep. var. in Apr. 32 election	83.670	83.670	83.670	7.157	7.157	7.157
Observations	3624	3624	3624	3624	3624	3624

Note: Cols. 1 to 3: Dependent variable is turnout. Cols. 4 to 6: Dependent variable is log number of voters. Cols. 1 and 4: Estimates of equation (2) with share of households within 200m of Nazi march as exposure. Cols. 2 and 5: Estimates of equation (2) with flu-based indirect exposure to the march as measure of exposure. Cols. 3 and 6: Estimates of equation (6) with both, share of households within 200m of Nazi march and flu-based indirect exposure to the march as exposure measures. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. See main text and appendix for construction of the flu-based indirect exposure measure. Standard errors clustered at polling station level in brackets.

Table A.6: Robustness. Standard errors corrected for spatial autocorrelation.

	% NSDAP vote		
	(1)	(2)	(3)
Share households directly exposed × post march	1.029		0.979
Baseline: s.e. clustered at polling station level	[0.269]		[0.268]
Conley (1999) s.e.: cutoff at 200m	[0.272]		[0.271]
Conley (1999) s.e.: cutoff at 500m	[0.286]		[0.286]
Conley (1999) s.e.: cutoff at 1km	[0.308]		[0.310]
Conley (1999) s.e.: cutoff at 1.5km	[0.319]		[0.322]
Conley (1999) s.e.: cutoff at 2km	[0.330]		[0.332]
Conley (1999) s.e.: cutoff at 2.5km	[0.340]		[0.344]
Conley (1999) s.e.: cutoff at 3km	[0.345]		[0.349]
Indirect exposure of households × post march		0.291	0.268
Baseline: s.e. clustered at polling station level		[0.101]	[0.101]
Conley (1999) s.e.: cutoff at 200m		[0.100]	[0.100]
Conley (1999) s.e.: cutoff at 500m		[0.101]	[0.101]
Conley (1999) s.e.: cutoff at 1km		[0.101]	[0.101]
Conley (1999) s.e.: cutoff at 1.5km		[0.103]	[0.102]
Conley (1999) s.e.: cutoff at 2km		[0.103]	[0.103]
Conley (1999) s.e.: cutoff at 2.5km		[0.103]	[0.103]
Conley (1999) s.e.: cutoff at 3km		[0.103]	[0.102]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
Observations	3110	3110	3110

Note: Robustness: Correction for spatial correlation with formula of Conley (1999). Row 1: Baseline results reporting standard errors clustered at polling station level. Rows 2-5: standard error corrected with the formula of Conley (1999). Cutoff is 200m (row 2), 500m (row 3), 1km (row 4), 1.5km (row 5), 2km (row 6), 2.5km (row 7) and 3km (row 8). Dependent variable is share NSDAP votes. Col. 1: Estimates of equation (1) with share of households within 200m of Nazi march as exposure. Col. 2: Estimates of equation (1) with flu-based indirect exposure to the march as exposure. Col. 3: Estimates of equation (5) with both, share of households within 200m of Nazi march and flu-based indirect exposure to the march as exposure measures. We use the main panel of 1932 and 1933 elections. See main text and appendix for construction of the flu-based indirect exposure measure.

Table A.7: Robustness. Control for expected direct and indirect exposure.

	% NSDAP votes	
	(1)	(2)
Share households directly exposed (200m) × post march	0.939	
	[0.201,1.754]	
Share households directly exposed (200m) × t6 (post)		0.901
		[-0.176,2.051]
Share households directly exposed (200m) × t5 (post)		0.990
		[0.149,1.899]
Share households directly exposed (200m) × t4 (post)		1.009
		[0.367,1.674]
Share households directly exposed (200m) × t2 (pre)		0.056
		[-0.814,0.899]
Share households directly exposed (200m) × t1 (pre)		0.226
		[-0.676,1.135]
Indirect exposure of households × post march	0.223	
	[0.120,0.334]	
Indirect exposure of households × t6 (post)		0.272
		[0.114,0.406]
Indirect exposure of households × t5 (post)		0.297
		[0.160,0.441]
Indirect exposure of households × t4 (post)		0.067
		[-0.039,0.179]
Indirect exposure of households × t2 (pre)		-0.021
		[-0.139,0.084]
Indirect exposure of households × t1 (pre)		-0.032
		[-0.162,0.117]
Expected direct exposure × post march	Yes	No
Expected indirect exposure × post march	Yes	No
Expected direct exposure × election FEs	No	Yes
Expected indirect exposure × election FEs	No	Yes
Election & polling station FEs	Yes	Yes
Demographic controls × election FEs	Yes	Yes
Street controls × election FEs	Yes	Yes
R^2	0.915	0.934
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417
Observations	3110	3625

Note: Robustness: Control for non-random exposure to shock following Borusyak and Hull (2023). Dependent variable is NSDAP vote share. Col. 1: Estimates of equation (5) with both, share of households within 200m of Nazi march and flu-based indirect exposure to the march as exposure measures using the main panel of 1932 and 1933 elections. Col. 2: Estimates of equation (6) with both, share of households within 200m of Nazi march and flu-based indirect exposure to the march as exposure measures. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. We additionally control for expected direct and indirect exposure. Following Borusyak and Hull (2023), we compute expected direct and indirect exposure as the average treatment of each polling station across 500 counterfactual marches. See main text for construction of counterfactual marches. Confidence intervals using randomization inference following the procedure by Borusyak and Hull (2023) in brackets.

Table A.8: Panel A. Nearest neighbor match: first difference results for direct exposure.

	Δ % NSDAP vote (before-after)					
	(1)	(2)	(3)	(4)	(5)	(6)
SATT	0.694	0.306	0.545	0.554	0.607	0.603
	[0.307]	[0.286]	[0.249]	[0.239]	[0.237]	[0.237]
Number of matched pairs	109	109	327	327	545	545
Number of matches per treated unit	1	1	3	3	5	5
Matching on coordinates	Yes	Yes	Yes	Yes	Yes	Yes
Matching on demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Matching within district (17)	No	Yes	No	Yes	No	Yes

Note: Robustness: Nearest neighbor matching. Treatment variable is dummy = 1 if more than 80% of households assigned to polling station are located within 200m of Nazi march (109 polling stations are treated). Dependent variable is the change in share of average NSDAP votes between the two elections before the marches (13 March and 10 April 1932) and the four after (24 April 1932 to 5 March 1933). Cols. 1, 3 and 5: matching on longitude, latitude and demographic controls. Cols. 2, 4 and 6: matching on longitude, latitude and demographic controls within city district (17 districts). Number of matches per treated unit: 1 (cols. 1-2), 3 (cols. 3-4) and 5 (cols. 5-6). We use the main panel of 1932 and 1933 elections. Standard errors in brackets.

Table A.8: Panel B. Nearest neighbor match: first difference results for indirect exposure.

	Δ % NSDAP vote (before-after)					
	(1)	(2)	(3)	(4)	(5)	(6)
SATT	0.725	0.536	0.630	0.546	0.639	0.605
	[0.218]	[0.208]	[0.184]	[0.179]	[0.181]	[0.177]
Number of matched pairs	311	311	933	933	1555	1555
Number of matches per treated unit	1	1	3	3	5	5
Matching on coordinates	Yes	Yes	Yes	Yes	Yes	Yes
Matching on demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Matching within district (17)	No	Yes	No	Yes	No	Yes

Note: Robustness: Nearest neighbor matching. Treatment variable is dummy = 1 if above median flu-based indirect exposure (311 polling stations are treated). Dependent variable is the change in share of average NSDAP votes between the two elections before the marches (13 March and 10 April 1932) and the four after (24 April 1932 to 5 March 1933). Cols. 1, 3 and 5: matching on longitude, latitude and demographic controls. Cols. 2, 4 and 6: matching on longitude, latitude and demographic controls within city district (17 districts). Number of matches per treated unit: 1 (cols. 1-2), 3 (cols. 3-4) and 5 (cols. 5-6). We use the main panel of 1932 and 1933 elections. Standard errors in brackets.

Table A.9: Panel A. Entropy balancing: balance before and after re-weighting.

	Before re-weighting		After re-weighting	
	Control	Treated	Control	Treated
log voters	7.155	7.166	7.166	7.166
	[0.020]	[0.026]	[0.017]	[0.025]
Share households with telephone	0.111	0.139	0.139	0.139
	[0.013]	[0.149]	[0.016]	[0.015]
Share households with heating	0.057	0.060	0.060	0.060
	[0.014]	[0.014]	[0.013]	[0.014]
Share households who are blue-collar	0.361	0.327	0.327	0.327
	[0.218]	[0.017]	[0.021]	[0.017]
log distance to extreme point	7.065	7.101	7.101	7.101
	[0.409]	[0.336]	[0.367]	[0.336]
log distance to straight line	-0.522	-0.747	-0.746	-0.747
	[1.138]	[1.256]	[1.582]	[1.256]
Number of streets	4.579	4.606	4.605	4.606
	[4.111]	[2.889]	[4.132]	[2.889]
Share streets in 1st width tercile	0.416	0.378	0.378	0.378
	[0.092]	[0.079]	[0.086]	[0.079]
Share streets in last width tercile	0.205	0.182	0.182	0.182
	[0.061]	[0.049]	[0.054]	[0.049]

Table A.9: Panel B. Results from matching exercises.

	% NSDAP vote		
	(1)	(2)	(3)
	Base	Entropy	CEM
=1, if more than 80% households directly exposed × post march	0.996	0.971	1.030
	[0.254]	[0.243]	[0.269]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
R^2	0.915	0.921	0.917
Mean dep. var.:	30.417	30.417	31.096
Observations	3110	3110	2295

Note: Robustness: Entropy balancing and Coarsened Exact Matching. Treatment variable is dummy = 1 if more than 80% of households assigned to polling station are located within 200m of Nazi march (109 polling stations are treated). Panel A: Difference in covariates in polling stations with share of households located within 200m of Nazi march below and above 80%. Cols. 1-2: average before re-weigthing. Cols. 3-4: average after re-weighting with the formula of Hainmueller (2012). Panel B: Regressions results with entropy balancing and Coarsened Exact Matching. Dependent variable is the share of NSDAP votes. Col. 1: Baseline estimates. Col. 2: Estimates after entropy balancing. Col. 3: Estimates on the sub-sample of polling stations matched by the Coarsened Exact Matching algorithm. We find exact matches within cells defined by number of voters (split into quartiles), share of households with telephone (split into quartiles), share of households with heating (above 0 / 0) and share of blue collar workers (split into quartiles). In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. We use the main panel of 1932 and 1933 elections. Standard errors clustered at polling station level in brackets.

Table A.10: Panel A: Major other propaganda in Hamburg 1932/1933.

Organisation	Event	Date	Robustness
NSDAP	Hitler speech (I)	01 March 1932 (pre)	Table A.13: column 2
Eiserne Front	Rally and march	18 Apr 1932 (pre)	Table A.13: column 1 (marching route unknown)
NSDAP	Hitler speech (II)	23 April 1932 (post)	Table A.12: row 3
KPD	March	01 May 1932 (post)	Table A.11.A: row 3; Table A.11.B: row 3
SPD	March	01 May 1932 (post)	Table A.11.A: row 4; Table A.11.B: row 4
NSDAP	Hitler speech (III)	20 July 1932 (post)	Table A.12: row 4
Kampfbund Schwarz-Weiß-Rot	Rally	26 Feb 1933 (post)	Location unknown
Eiserne Front	Rally and march	26 Feb 1933 (post)	Table A.13: column 1 (marching route unknown)
NSDAP	March	26 Feb 1933 (post)	Table A.11.A: row 6; Table A.11.B: row 6
NSDAP	March	01 March 1933 (post)	Marching route unknown
NSDAP	Hitler speech (IV)	03 Mar 1933 (post)	Table A.12: row 5

Table A.10: Panel B: Nazi rallies in Hamburg 1932/1933.

Time	Location	Frequency	Robustness
	Indoor	137	
Pre	Outdoor	1	Table A.14
	Unclear	10	
	Indoor	148	
Post	Outdoor	6	Table A.14
	Unclear	1	

Note: Robustness: Other propaganda. Panel A gives an overview of major other propaganda events in 1932 and early 1933 Hamburg. Panel B summarizes the key characteristics of Nazi rallies in 1932 and early 1933 Hamburg. Column 'Robustness' points to analysis in which robustness to other propaganda is tested.

Table A.11: Panel A. Robustness. Other marches: KPD vote share.

	% KPD votes					
	(1)	(2)	(3)	(4)	(5)	(6)
Share households directly exposed (200m) × post march	-0.666	-0.506	-0.688	-0.574	-0.643	-0.706
	[0.184]	[0.181]	[0.190]	[0.180]	[0.189]	[0.192]
Indirect exposure of households × post march	-0.080	-0.098	-0.085	-0.098	-0.079	-0.085
	[0.064]	[0.064]	[0.063]	[0.064]	[0.064]	[0.063]
Share households directly exposed to KPD march (200m) × post march	0.434				0.450	
	[0.193]				[0.194]	
Share households directly exposed to SPD march (200m) × post march		-0.199			-0.264	
		[0.202]			[0.206]	
Share households directly exposed to KPD or SPD march (200m) × post march			0.378			0.364
			[0.177]			[0.178]
Share households directly exposed to Feb33 Nazi march (200m) × post march				0.391	0.383	0.334
				[0.197]	[0.195]	[0.198]
Election & polling station FEs	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.918	0.918	0.918	0.918	0.918	0.918
Mean KPD vote in 10 Apr '32 election	13.183	13.183	13.183	13.183	13.183	13.183
Observations	3110	3110	3110	3110	3110	3110

Table A.11: Panel B. Robustness. Other marches: NSDAP vote share.

	% NSDAP votes					
	(1)	(2)	(3)	(4)	(5)	(6)
Share households directly exposed (200m) × post march	1.124	1.140	1.197	0.988	1.258	1.199
	[0.280]	[0.273]	[0.287]	[0.267]	[0.282]	[0.286]
Indirect exposure of households × post march	0.245	0.267	0.247	0.267	0.247	0.247
	[0.102]	[0.102]	[0.102]	[0.101]	[0.102]	[0.102]
Share households directly exposed to KPD march (200m) × post march	-0.527				-0.463	
	[0.250]				[0.250]	
Share households directly exposed to SPD march (200m) × post march		-0.784			-0.730	
		[0.298]			[0.299]	
Share households directly exposed to KPD or SPD march (200m) × post march			-0.585			-0.583
			[0.248]			[0.248]
Share households directly exposed to Feb33 Nazi march (200m) × post march				-0.127	-0.025	-0.035
				[0.369]	[0.365]	[0.366]
Election & polling station FEs	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.915	0.915	0.915	0.915	0.916	0.915
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417	30.417
Observations	3110	3110	3110	3110	3110	3110

Note: Robustness: Other propaganda marches. Panel A shows estimates of equation (5). Dependent variable is KPD vote share. Panel B shows estimates of equation (5). Dependent variable is NSDAP vote share. Column 1 adds share of households within 200m of KPD march interacted with a post KPD march indicator (post 1 May 1932). Column 2 adds share of households within 200m of SPD march interacted with a post SPD march indicator (post 1 May 1932). Column 3 adds share of households within 200m of either KPD or SPD march interacted with a post SPD/ KPD march indicator (post 1 May 1932). Column 4 adds share of households within 200m of February 1933 Nazi march interacted with a post February 1933 Nazi march indicator (post 28 February 1932). Column 5 adds exposure to SPD, KPD and 1933 Nazi march. Columns 6 adds exposure to either SPD or KPD march and 1933 Nazi march. In all specifications we control for polling station and election fixed effects as well as for demographic and street controls interacted with election fixed effects. We use the main panel of 1932 and 1933 elections. Standard errors clustered at polling station level in brackets.

Table A.12: Robustness. Hitler speeches.

	% NSDAP votes			
	(1)	(2)	(3)	(4)
Share households directly exposed (200m) × post march	0.948	0.907	0.990	1.037
	[0.264]	[0.267]	[0.267]	[0.270]
Indirect exposure of households × post march	0.298	0.291	0.267	0.297
	[0.101]	[0.102]	[0.101]	[0.101]
Log average distance to Hitler spech (II) × post speech	-0.757			-1.544
	[0.209]			[0.501]
Log average distance to Hitler spech (III) × post speech		-0.432		0.603
		[0.187]		[0.433]
Log average distance to Hitler spech (IV) × post speech			0.226	0.418
			[0.259]	[0.253]
Election & polling station FEs	Yes	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes	Yes
R^2	0.916	0.916	0.915	0.916
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417
Observations	3110	3110	3110	3110

Note: Robustness: Hitler speeches. The table shows estimates of equation (5). Dependent variable is NSDAP vote share. Column 1 adds log of distance to Hitler speech (II) at Dirt-Track-Bahn Fuhlsbüttel (23 April 1932), column 2 adds log of distance to Hitler speech (III) at Victoria-Stadion Hoheluft (20 July 1932), column 3 adds log of distance to Hitler speech (IV) at Zoo Hallen (03 March 1933) interacted with an indicator = 1 for election after corresponding speech. Column 4 adds log of distance to all three speeches interacted with corresponding post speech indicator. Hitler speech (I) takes place at Sagebiel Säle on 1 March 1932. This is before first election observed in the main time period of study. Hence we cannot observe the impact in the first difference setting for the full sample. We do investigate whether log of distance to speech (I) is correlated with our main treatment (Table A.10: col. 2). In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. We use the main panel of 1932 and 1933 elections. Standard errors clustered at polling station level in brackets.

Table A.13: Robustness. Other propaganda and distance to march.

	Within 500m from:	log distance to
	(1)	(2)
	EF rally	Speech (I)
Share households directly exposed (200m)	-0.000	-0.033
	[0.001]	[0.051]
Indirect exposure of households	-0.000	0.000
	[0.000]	[0.018]
Constant	-0.142	9.463
	[0.142]	[1.233]
Demographic controls	Yes	Yes
Street controls	Yes	Yes
R^2	0.013	0.315
Mean dependent variable	0.002	1.616
Observations	622	622

Note: Robustness: Other propaganda events and exposure. Col. 1: Dependent variable is an indicator whether a polling station is located within 500m of location of a Eiserne Front rally. Col. 2: Dependent variable is log distance to Hitler speech (I) at Sagebiel Säle on 1 March 1932. We include demographic and street controls in all specifications. Robust standard errors in brackets.

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Table A.14: Robustness. Rallies.

	Change pre - post
	(1)
	Δ rallies
log distance to march	-0.027
	[0.024]
Indirect exposure of households	0.022
	[0.017]
Constant	0.850
	[1.124]
Demographic controls	Yes
Street controls	Yes
R^2	0.030
Mean change in number of rallies	0.016
Observations	622

Note: Robustness: OLS estimates of regressing change in number of rallies in a polling station neighborhood from before the marches (pre 20 April 1932) to after (post 20 April 1932) on log of distance to Nazi marches (17 and 20 April 1932). We include demographic and street controls in the regression. Robust standard errors in brackets.

Table A.15: Cutoff sensitivity: first difference results.

	Dep var: % NSDAP votes						
	Direct exposure	Indirect exposure					
		>90% cutoff		>80% cutoff		>70% cutoff	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. 150m cutoff							
Share households directly exposed × post march	1.260		1.219		1.225		1.215
	[0.303]		[0.300]		[0.300]		[0.300]
Indirect exposure of households × post march		0.336	0.320	0.321	0.306	0.325	0.307
		[0.107]	[0.106]	[0.105]	[0.104]	[0.104]	[0.104]
Panel B. 200m cutoff							
Share households directly exposed × post march	1.029		0.969		0.979		0.992
	[0.269]		[0.268]		[0.268]		[0.271]
Indirect exposure of households × post march		0.354	0.331	0.291	0.268	0.180	0.148
		[0.104]	[0.103]	[0.101]	[0.101]	[0.096]	[0.097]
Panel C. 250m cutoff							
Share households directly exposed × post march	0.862		0.820		0.825		0.836
	[0.248]		[0.247]		[0.249]		[0.250]
Indirect exposure of households × post march		0.291	0.272	0.197	0.170	0.133	0.103
		[0.101]	[0.101]	[0.096]	[0.097]	[0.090]	[0.090]
Election & polling station FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417	30.417	30.417
Observations	3110	3110	3110	3110	3110	3110	3110

Note: Robustness: Changing treatment cutoffs. Each panel report results for different distance thresholds for direct exposure. Panel A uses a 150m threshold, Panel B a 200m and Panel C a 250m distance threshold. For each distance threshold, ndirect exposure is computed relative to a different direct treatment cutoff, from 90% (cols. 2-3) to 70% (cols. 6-7) of households within the distance threshold of the march as cutoff points to determine *treated area*. Dependent variable is the share of NSDAP votes. Col. 1: Estimates of equation (1) with share of households within the distance threshold of Nazi march as exposure. Even columns: Estimates of equation (1) with flu-based indirect exposure to the march as measure of exposure. Odd columns (> 1): Estimates of equation (5) with both, share of households within the distance threshold of Nazi march and flu-based indirect exposure to the march as exposure measures. In all specifications we control for polling station and election fixed effects as well as for demographic and street controls interacted with election fixed effects. See main text and appendix for construction of the flu-based indirect exposure measure. We use the main panel of 1932 and 1933 elections. Standard errors clustered at polling station level in brackets.

Table A.16: Robustness. More occupational controls.

	% NSDAP votes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share households directly exposed (200m) × post march	0.979	0.989	0.977	0.981				
	[0.268]	[0.268]	[0.263]	[0.262]				
Share households directly exposed (200m) × t6 (post)					0.998	1.017	0.995	1.004
					[0.377]	[0.375]	[0.367]	[0.366]
Share households directly exposed (200m) × t5 (post)					0.974	0.992	0.971	0.980
					[0.339]	[0.337]	[0.331]	[0.331]
Share households directly exposed (200m) × t4 (post)					1.016	1.028	1.014	1.020
					[0.292]	[0.291]	[0.284]	[0.284]
Share households directly exposed (200m) × t2 (pre)					0.034	0.047	0.032	0.040
					[0.216]	[0.213]	[0.213]	[0.210]
Share households directly exposed (200m) × t1 (pre)					0.153	0.162	0.127	0.134
					[0.390]	[0.386]	[0.378]	[0.376]
Indirect exposure of households × post march	0.268	0.261	0.249	0.247				
	[0.101]	[0.101]	[0.098]	[0.098]				
Indirect exposure of households × t6 (post)					0.326	0.313	0.295	0.290
					[0.141]	[0.139]	[0.136]	[0.135]
Indirect exposure of households × t5 (post)					0.368	0.355	0.337	0.332
					[0.135]	[0.134]	[0.131]	[0.130]
Indirect exposure of households × t4 (post)					0.189	0.180	0.167	0.164
					[0.110]	[0.109]	[0.106]	[0.106]
Indirect exposure of households × t2 (pre)					0.053	0.044	0.036	0.031
					[0.074]	[0.072]	[0.072]	[0.071]
Indirect exposure of households × t1 (pre)					0.064	0.048	0.037	0.030
					[0.131]	[0.128]	[0.129]	[0.127]
Election & polling station FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Shopkeepers × election FEs	No	Yes	No	Yes	No	Yes	No	Yes
Civil servants × election FEs	No	No	Yes	Yes	No	No	Yes	Yes
R^2	0.915	0.916	0.918	0.918	0.933	0.934	0.935	0.935
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417	30.417	30.417	30.417
Observations	3110	3110	3110	3110	3625	3625	3625	3625

Note: Robustness: Additional occupational controls. Dependent variable is NSDAP vote share. Cols. 1 to 4: Estimates of equation (5) with both, share of households within 200m of Nazi march and flu-based indirect exposure to the march as exposure measures using the main panel of 1932 and 1933 elections. Cols. 5 to 8: Estimates of equation (6) with both, share of households within 200m of Nazi march and flu-based indirect exposure to the march as exposure measures. In all specifications we control for polling station and election fixed effects as well as for street and demographic characteristics interacted with election fixed effects. In cols. 2 and 6, we add share of shopkeepers interacted with election fixed effect as additional controls. In cols. 3 and 7, we add share of civil servants interacted with election fixed effect as additional controls. In cols. 4 and 8, we add both, share of shopkeepers and share of civil servants interacted with election fixed effect as additional controls. See main text and appendix A for construction of the flu-based indirect exposure measure. Standard errors clustered at polling station level in brackets.

Table A.17: Indirect effect of exposure to persuasion and backlash areas.

	% NSDAP votes		
	(1)	(2)	(3)
Indirect exposure of households to persuasion area × post march	0.491	0.250	0.228
	[0.135]	[0.118]	[0.119]
Indirect exposure of households to backlash area × post march	-0.074	-0.006	0.002
	[0.145]	[0.132]	[0.130]
Share households directly exposed (200m) × post march			0.999
			[0.269]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	No	Yes	Yes
Street controls × election FEs	No	Yes	Yes
R^2	0.864	0.914	0.915
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417
Observations	3110	3110	3110

Note: Estimates of equation (5) with flu-based indirect exposure to persuasion area and flu-based indirect exposure to backlash area as measures of exposure (col. 1 and 2), and with flu-based indirect exposure to persuasion area, flu-based indirect exposure to backlash area and share of households within 200m of march (col. 3) as measures of exposure. Dependent variable is the share of NSDAP votes. In all specifications we control for polling station and election fixed effects. Col. 2 and 3 additionally include street and demographic characteristics interacted with election fixed effects. We use the main panel of 1932 and 1933 elections. Standard errors clustered at polling station level in brackets. See Appendix C for construction of the flu-based indirect exposure to persuasion and backlash area measures.

Table A.18: DiD Extremist Parties.

	% extremist parties votes		
	(1)	(2)	(3)
Share households directly exposed (200m) × post march	0.464		0.432
	[0.264]		[0.263]
Indirect exposure of households × post march		0.180	0.169
		[0.100]	[0.100]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
R^2	0.935	0.935	0.936
Mean extremist parties vote in 10 Apr '32 election	43.601	43.601	43.601
Observations	3110	3110	3110

Note: Estimates of equation (5) with share of households within 200m of march (col. 1) as only measure of exposure, with flu-based indirect exposure as only measure of exposure (col. 2) and both, flu-based indirect exposure and share of households within 200m of march (col. 3) as measures of exposure. Dependent variable is the share of extremist parties (NSDAP + KPD) votes. In all specifications we control for polling station and election fixed effects. We use the main panel of 1932 and 1933 elections. All specifications additionally include street and demographic characteristics interacted with election fixed effects. Standard errors clustered at polling station level in brackets.

Table A.19: Distribution regression descriptives.

	20% threshold	40% threshold	
	Mean	Mean	Difference
Voting			
Delta NSDAP pre to post	1.907	4.013	2.106
NSDAP vote share 31 July 32 (post)	19.633	41.595	21.962
Hitler (NSDAP) vote share 10 April 32 (pre)	17.726	37.582	19.856
Delta KPD pre to post	7.953	4.081	-3.872
KPD vote share 31 July 32 (post)	29.881	11.883	-17.998
Thälmann (KPD) vote share 10 April 32 (pre)	21.928	7.802	-14.126
Sociodemographic characteristics			
Number of voters at polling station (10 April 32)	1259.326	1320.763	61.437
Share of day laborers/unemployed	7.695	9.006	1.311
Share of blue collar workers	49.635	26.178	-23.457
Share of skilled workers	5.261	7.593	2.332
Share of white collar workers	4.217	6.308	2.091
Share of managers	1.198	2.129	0.931
Share of low grade civil servants	6.010	6.106	0.096
Share of high grade civil servants	0.289	0.885	0.596
Share of shopkeepers	8.173	13.530	5.357
Share of retired	1.389	1.401	0.012
Share of households with telephone	3.921	17.237	13.316
Share of households with heating	0.986	9.307	8.321
Street network characteristics			
Distance to closest extreme point (km)	1.594	1.373	-0.221
Distance to closest straight line between extreme points (km)	0.926	0.749	-0.177
Number of streets within 200m of polling station	4.837	4.839	0.002
Share of streets in top tercile of width	41.403	39.964	-1.439
Share of streets in bottom tercile of width	22.324	25.395	3.071
Observations	86	93	

Note: The unit of observation is a polling station in Hamburg. Votes for Hitler (NSDAP), Thälmann (KPD), NSDAP, KPD, and number of voters come from the statistical bulletin of Hamburg (Sköllin, 1930; 1932a; 1932b; 1933). Share of households with telephone, with heating, shares of day laborers/unemployed, blue collar workers, skilled workers, white collar workers, managers, low and high grade civil servants, shopkeepers, abd retired come from the 1932 Hamburg address book (Hamburger Adreßbuch, 1932). Distance to the closest extreme point and distance to the straight lines connecting extreme points are calculated using the marching routes digitized from the SA Hamburg documents (State Archive Hamburg, 1932a; 1932b). Number and width of streets within 200m from the polling station is calculated from the digitized street network (Landesbetrieb Geoinformation und Vermessung Hamburg, 2020). Column 1 (20% threshold) reports means for the 86 polling stations with between 15% and 20% of Hitler (NSDAP) vote share in the election of 10 April 1932. Column 2 (40% threshold) reports means for the 93 polling stations with between 35% and 40% of Hitler (NSDAP) vote share in the election of 10 April 1932. Column 3 (Difference) reports difference in means between column 1 and 2.