Going Viral: Propaganda, Persuasion and Polarization in 1932 Hamburg[*]

Bruno Caprettini — *U Zurich*
Marcel Caesmann — *U Zurich*
Hans-Joachim Voth — *U Zurich and CEPR*
David Yanagizawa-Drott — *U Zurich and CEPR*

First version: 11-Mar-19
This version: 28-May-21

Abstract

Propaganda can convince or repel. Social interactions can magnify these effects. We estimate the impact of Nazi marches in 1932 Hamburg, using granular data on all households. Direct exposure immediately affected voting -- propaganda was persuasive. To study diffusion, we measure social connections using contagion patterns from the 1918 Spanish flu, combined with social similarity. Nazi support spread to other parts of the city along the predicted contagion paths. Social spillovers are of similar importance as direct exposure. The marches were also polarizing the electorate – in opposition strongholds, they backfired, and gains were concentrated in areas with high Nazi support.

[*] We would like to thank Alberto Alesina, Philippe Aghion, Marianne Bertrand, Volha Carnysh, Armin Falk, Raquel Fernandez, Sergei Guriev, Carlo Horz, Holger Kern, Ulrike Malmendier, Massimo Morelli, Dominic Rohner, Mathias Thoenig, Benno Torgler, John van Reenen, Yona Rubinstein, Harald Uhlig, Noam Yuchtman, and Ekaterian Zhuravskaya for helpful advice, as well as participants at the Paris School of Economics, CEPR Political Economy Meeting in Villars, briq, the 2nd Paris Workshop on Formal and Informal Institutions, the 1st Economic History Workshop at Bologna, the CCI seminar at LSE, the VWAR Sessions, the UZH faculty lunch, as well as the Australasian Rational Choice Conference in Brisbane for their comments.

"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

Political movements often begin as small, marginal groups; some of them grow into mass movements seizing the reins of power. Mass rallies, marches, and protests often act as catalysts: Several turning points in history, from the March on Rome to the storming of the Bastille occurred in this fashion. Fringe groups can become mainstream through two broad mechanisms: Public signals about the acceptability of a group or a particular view can reduce stigma or increase perceived merit, enhancing legitimacy (Bénabou and Tirole 2006). Such a coordination channel can be powerful, but is limited to publicly observable actions; it cannot explain changes in private actions. For private actions, like voting, persuasion is key. A large literature has shown that propaganda and media exposure can sway opinions (DellaVigna and Kaplan 2007; Adena et al. 2015; Enikolopov, Makarin, and Petrova 2020). However, despite the fact that voting and political participation are inherently social acts, there is no well-identified evidence on social spillovers in propaganda – where convincing some citizens leads to others becoming persuaded in turn. Currently, we do not know whether second-round effects exist, how large they are, or the mechanisms behind them.

Establishing social spillovers in persuasion is challenging for four main reasons: First, when someone is persuaded, their change in private beliefs is rarely observable to the econometrician unless this leads to private, consequential actions. Second, information on social interactions is required.[2] Third, initial treatment has to be specific to certain subgroups or locations – the "insertion point" of new information or exposure to an event should ideally affect only some, but not others (Banerjee et al. 2013). Fourth, to capture the dynamic effect of persuasion, we need to measure outcomes at a high frequency, before and after treatment.

[1] Quoted according to Noakes (1971).

[2] Contagion in political decisions has long been hypothesized to matter, but it is hard to demonstrate empirically: For example, voting behavior in general is correlated within groups of friends, co-workers, and neighbors (Kenny 1992) as well as Facebook friends (Bond et al. 2012). However, like-minded individuals form networks or live in the same neighborhoods, and are often exposed to the same information (Nickerson 2008; Mutz and Martin 2001), creating serious identification problems.

In this paper, we use unique data to overcome these four challenges. We analyze the spillover effects of political marches on Nazi voting in 1932 Hamburg. We focus on voting, a private action with important consequences -- others cannot see which party an *individual* voter supports, but we do observe group-level outcomes. We can exploit granular voting data from more than 600 municipal polling stations for multiple elections at a high frequency. To capture social links and personal interactions, we combine two measures: We map the spread of the 1918 "Spanish flu" outbreak across neighborhoods in Hamburg. In addition, since homophily is a well-known determinant of social interactions, we also use the locations and socio-economic characteristics of the universe of households corresponding to approximately 400,000 in total.[3] These two measures are correlated, validating each other. Finally, we use detailed information on Nazi marching routes derived from police records. This allows us to determine which households were directly exposed to propaganda. Using highly disaggregated, high-frequency data on voting, we employ a difference-in-differences approach to trace the persuasion effect of propaganda over time -- from a subgroup's initial exposure, via social interactions, to consequential private actions in the rest of population.

Three main findings emerge. First, demonstrations directly influenced voting. *On average*, within a few days, households living in neighborhoods directly exposed to the marches showed greater increases in Nazi support than similar, unexposed districts. To illustrate this effect, Panel A of Figure 1 shows that the further a polling station was from the marching route, the smaller the gain for the party. The effects are quantitatively important. In the least exposed areas, the Nazi vote share increased by 4.6 percentage points; in the most exposed district, the gain was 6.3 percentage points, or 37% larger. Second, support spread to the rest of the population via social interactions. Areas untreated by marches also swung strongly towards the Nazis if they were connected to 'treated' areas, as captured by more socio-economic similarity or more rapid influenza transmission in 1918. Figure 1, Panel B illustrates the strength of this mechanism. We show that the indirect effect is similar in magnitude to the direct effect, grows in size, and lasts for at least

[3] Influenza is transmitted through the spread of droplets, aerosols, or contact with secretions. Longer exposure to an exposed person increases the risk of being infected (Tellier 2006). Thus, the spread of the flu contains information on physical interactions. Since homophily predicts social links (Kossinets and Watts 2009; McPherson, Smith-Lovin, and Cook 2001), we use the similarity of household characteristics as an indicator of likely linkages, in the spirit of an intention-to-treat analysis. We validate the approach by showing that the strength of flu correlations 15 years earlier in 1918 is strongly correlated with our measure of homophily, even conditional on physical distance (see Appendix Figure A.5).

twelve months. After a year, it accounts for almost two thirds of the marches' overall impact in the city as a whole. Thus, the evidence shows that, *on net*, the marches were effective in persuading the population of Hamburg of the Nazi Party's political appeal. This effect operated through both direct and indirect exposure. To our knowledge, our paper provides the first evidence that persuasion as a result of propaganda can be 'contagious', spreading through social interactions.

While the marches were effective *on average*, our third main result shows that the impact was not uniform across the city. Using a distribution regression approach, we find evidence that persuasion from direct and indirect exposure was particularly strong in neighborhoods where the Nazi Party was already relatively popular. In areas where the opposition was strong, 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.

In combination, our evidence demonstrates that public demonstrations by a controversial political movement can be both persuasive and divisive – social interactions operated 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. 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. 2020). 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.

When it comes to people openly joining and supporting an extremist movement, the literature 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 rich literature studying political persuasion. This literature has mostly focused on the effects of exposure to particular messages via mass media. Examples includes 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 mind in surveys and at the polling station. We also know that persuasion efforts via protests and demonstrations can create such effects, just like media exposure (Madestam et al. 2013). In this literature, a unifying metric is the "persuasion rate": the share of people who change their beliefs or behaviour after exposure (DellaVigna and Gentzkow 2010). In our setting, for example, Adena et al. (2015) estimate persuasion rates of pro-Weimar and pro-Nazi radio broadcasts in the range of 10-37 percent. Where evidence in the literature is lacking, however, is estimates of diffusion beyond direct persuasion, i.e. beyond those immediately treated.[4] 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, our estimates suggest an indirect persuasion rate – arising from households being socially connected to households directly exposed to the Nazi marches – in the range of 4-8 percent, initially relatively small but growing over the next two hundred days. Thus, 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 propaganda 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. (2020) argues that differences in trust of information sources can create divergent opinions. Using Turkish data, Baysan (2021) 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. Our results similarly demonstrate that propaganda led to polarization of the electorate; both direct and indirect exposure can lead to growing cleavages in political orientation.

[4] Related work in political science has emphasized that social networks can act as coordinating devices (Arias et al. 2019). There is also evidence that social connections can influence voter turnout (Foos and de Rooij 2017).

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" (Arendt 1973; Bracher 1979; Falter 1991; Bullock 1994). 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 Munzert 2018).[5] We are among the first to provide clear-cut evidence that one of the key propaganda tools used by the Nazi Party was highly effective, in part 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).

[5] 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).

As the Great Depression worsened, support for liberal democracy declined. 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. 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 fully free election, polling 37.2% of the vote. Polarization at the polls was an important determinant of political dysfunctionality: After July 1932, Nazis and Communists together held the absolute majority of seats, making it impossible for the remaining democratic parties to form a majority government. Hitler was confident of becoming Chancellor, with the support of bourgeois parties. However, the aging president's distaste for the 'little corporal' prevented a Nazi government in the summer of 1932 (Kershaw 2001); a minority government remained in office.

In November 1932, votes for the Nazi Party declined, and the President appointed a cabinet without Nazi ministers. By December, many observers were confident that the Nazis would never come to power (Turner 1997). However, in late January 1933, conservative advisors convinced the elderly president that Hitler could be controlled and should become Chancellor – but in a cabinet dominated by ministers from other right-wing parties. As soon as the Nazis were in office, they out-maneuvered their coalition partners and seized power. Ruthlessly using control of the police, the Hitler government held one more parliamentary election – in March 1933 – and then set out to dismantle German democracy. Soon, millions of Germans applied to become members of the Nazi Party (Falter 2020). The first boycotts and attacks on Jewish shops and property followed by late spring (Evans 2006). By the summer of 1933, the Nazi government had banned all other parties, and it had abolished the unions. 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 traditionally a center of the German labor movement – “Red Hamburg”. After 1918, communists and social democrats in combination often received half of the vote. The rise of the Nazis in Hamburg therefore created even deeper divisions than elsewhere.

The Nazi party’s electoral fortunes in Hamburg broadly followed trends at the national level. It only received 2.6% of the vote in Hamburg in the federal elections of 1928. A period of internal power struggles followed (Büttner 1982). The party in Hamburg had sought to recruit workers for the Nazi cause. It also often argued for far-left positions, expressing opposition to capitalism and bourgeois parties. This policy did not succeed. The party recruited few workers, and failed to found party cells in most companies. After 1929, under a new leader, the career politician Karl Kaufmann, the party in Hamburg increasingly 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 toned down the 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. In local elections in 1931 and 1932, the Nazi party registered major gains in electoral support. By the end of July 1932, the party received the highest share of the vote yet – 33.7%.[6] Given the strength of the labor movement in the city, this was an impressive showing.

2.3 Nazi propaganda and marches. Prior to coming to power, the Nazi party only had access to a limited set of propaganda tools. Radio was state-controlled and hence, closed to the Nazi 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:

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

> *"All the means of public opinion were denied to Hitler. His newspapers were banned, he was denied use of the radio, his brochures and leaflets were confiscated. He had no choice but to reach the masses directly through constantly growing mass rallies."*

In a bid to reach ordinary Germans, the Nazi party organized tens of thousands of events across the country in the run-up to every election (Kershaw 2001). Its local chapters were in charge of recruiting and day-to-day propaganda, using local speakers. These were often trained via correspondence courses organized by the national propaganda organization (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. This rally is of a size which has only been paralleled during the revolution days of 1918.* ***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 what became a hallmark tool of Nazi propaganda – during the late Weimar period, but also in the big annual gatherings organized by the Party after 1933. In Hamburg in 1932, the Nazi Party organized two big marches in April 1932. Rallies, marches, and assemblies were banned before and during the federal presidential election of 1932; the ban was only lifted after April 10. The two major marches took place on April 17 and 20. The overall number of participants was 13,000, including 9,300 Nazi Party members. Newspaper accounts suggest that many more witnessed the event. The second march was held after dusk and illuminated by the torches carried by participants; it made a particularly strong impression (Hamburger Nachrichten, 1932b). The city police department had banned the storm troopers (SA) from marching in their uniforms, but the event itself had a strongly militaristic overtone. The Nazi newspaper *Hamburger Tageblatt* wrote:

> *"four massive columns roll forward, seemingly unending* [in number]*, impossible to ignore… flags fly over this army of thousands, stirring fighting songs are heard, the stomping of innumerable boots on the ground. ... And then they march, march so that red Hamburg has to take notice…"*[7]

Marches, rallies and demonstrations increased the party's appeal amongst the bourgeoisie because they conveyed an image of discipline, order, and strength, reminiscent of the frequent military marches during an idealized pre-war period. Evans (2006) has argued:

> *"...mass demonstrations and marches in the streets drove out rational discourse ... in favour of easily assimilated stereotypes that mobilized a whole range of feelings, from resentment and aggression to the need for security and redemption. The marching columns ..., the stiff salutes and military poses ... conveyed order and dependability as well as ruthless determination. Banners and flags projected the impression of ceaseless activism and idealism."*

This contrasted favorably with the perceived weakness and chaos of the democratic 'Weimar system'. 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 witness, a young woman taken by her conservative parents to see a Nazi march, 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 for whom it was a matter of death and life ... I wanted to escape from my childish, narrow life and I wanted to attach myself to something that was great and fundamental."* (Evans 2004, p. 313)

In the run-up to the federal election on July 31, 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. Communist youth attacked a march of 7,000 storm troopers

[7] Hamburger Tageblatt April 18, 1932.

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).

Figure A.2 in the Appendix summarizes the timing of key events. Two elections preceded the marches – the presidential elections on March 13 and April 10. The marches themselves, on April 17 and 20 were followed in quick succession by the municipal election on April 24, and then, 3 months later, by the national election on July 31.

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 information from two additional sources: the 1931-32 Hamburg city book and 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.[8]

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). Figure A.3, Panel A shows the routes of the two marches.

3.2 Voting data. 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

[8] 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).

(meaning in one of the 17 boroughs of the inner city of Hamburg).[9] We assign each household to its polling station using the official voting lists. The official bulletins contain voting lists that assign each address (street and house numbers) to an electoral district. This allows a precise matching of each household to the polling station to which they belonged. 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).

The average polling station saw around 1,300 valid votes cast, with a range of 501 to 1,940. 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), the 1932 municipal election (*Bürgerschaftswahl*) on 24 April (Sköllin 1932a), and for the 31 July 1932, 6 November 1932 and 5 March 1933 *Reichstag* elections (Sköllin 1932b; 1932c; 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, but the location of polling stations underwent occasional changes. Around 10% of polling stations changed their location between March 1932 and March 1933 at least once; they never moved by more than 500 meters. We always use the March 1932 address for our calculations, and in our robustness section, show that all our results are unaffected when we drop the 62 polling stations that changed location. To our knowledge, voters were not re-assigned to different polling stations as a result of these changes.

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

---

[9] 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. Results with these 22 polling stations are similar to the ones we present and are available upon request.

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).[10]

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 observe the march or would notice the march because of its proximity. 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.

[10] 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.

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 all 60,000 death records from the 1917-1919 death registers of Hamburg (State Archive Hamburg 1917). 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 registers: 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.

3.6 Contemporaneous events. To explore the possibility that contemporaneous events could confound our estimates, we collect data from the *Hamburger Anzeiger* (Hamburger Anzeiger 1932). The *Hamburger Anzeiger* had the highest circulation within Hamburg pre-WW2 (Sonntag 2006). The newspaper was closest to the bourgeois camp and considered non-partisan and more neutral than the *Hamburger Tageblatt*, which was closely tied to the NSDAP. We read the first pages of every edition within the 14-day window separating the second Presidential vote and the City Diet election between the 10th and 24th of April. We classify the main articles by topic and whether they address a local or national matter. Of the 43 main articles we identify 33% as local, the remainder as national news (Table A.1).

3.7 District-level information. We complement these data with information from the 1931-32 "Hamburg city book" (Statistisches Landesamt Hamburg 1932). Statistical offices regularly published statistical digests during this period. These reported a variety of recent socio-economic statistics for different areas of major cities. We use information from the 1931-32 edition, which provides detailed information on all 17 city districts. We use the average rental value of a property to proxy for the average wealth of districts. We also use population statistics to validate data collected from the address book. Finally, we assign polling stations to the district in which they fall using their address.

4 Direct Persuasion: Specifications and Results

Can mass demonstrations convince voters at the polls? And is the effect confined to areas exposed to propaganda, or can it spread through social networks, like a contagious virus? We examine these questions with detailed data on the path of two major Nazi marches in 1932 Hamburg, high-frequency electoral results from the population of Hamburg polling stations in 1932-33, and granular measures of social connections across the city (Appendix A describes sources and measures). We expect propaganda to spread via social interactions if there is an immediate, first-order, direct effect of exposure. We therefore describe in this section how we test for direct effects, the hypotheses to be examined, and the results.

4.1 Direct effects: Difference in differences. To estimate the impact of marches on voting, we begin by calculating polling stations' physical proximity to the marches. Comparing electoral results across polling stations that are close to or far from the marches identifies the causal effect of marches only if physical proximity is orthogonal to other determinants of voting. Physical proximity may not be orthogonal if Nazi planners chose to march through "friendly" neighborhoods to mobilize supporters. If these districts were already favoring the Nazi Party, this would lead to upward bias. Conversely, we expect a downward bias if Nazis paraded through Communist strongholds to showcase their strength in front of their enemies (as they did in July 1932 in the nearby suburb of Altona).

We address these concerns with a simple difference-in-difference strategy, leveraging the high frequency of elections in 1932. Table 1 gives an overview of the main statistical properties of key variables. Between March and November 1932, Hamburg voters participated in five elections. We also observe an additional parliamentary election in March 1933. In April 1932, Nazi members marched through the streets of Hamburg twice during a 14-day window separating two of the 1932 elections. This rich setting with relatively high-frequency voting data allows us to test specific hypotheses regarding the dynamics of persuasion. For ease of exposition, we start by testing the simple hypothesis that marches mattered for voting. We estimate the following simple difference-in-difference equation:

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

In equation (1), N_{it} is the NSDAP vote share in polling station i, election t, $Post_t$ is an indicator for elections held after the two Nazi marches and M_i is exposure to the marches. 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.[11] Election fixed effect (α_t) capture broad (Hamburg-wide) changes in NSDAP voting. The inclusion of polling station fixed effects (α_i) is crucial, as it allows us to control for any underlying characteristic that may make exposed areas different from the rest of Hamburg. The vector X_i includes: log voters, share of blue-collar workers, share of households with a telephone and share with centralized heating. 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 create precise measures of socio-economic status at the polling station level. In the most conservative specification, we allow these characteristics to have a different effect in every election ($\delta_t \cdot X_i$).

We also estimate a flexible specification of (1), where we allow the effect of exposure β to vary over time:

$$N_{it} = \alpha_i + \alpha_t + \sum_t \beta_t \times M_i + \sum_t \delta_t \times X_i + u_{it} \qquad (2)$$

Here β_t is the estimated impact of march exposure in election *t*. In this specification we take as our reference point the 2nd Presidential election of 1932, which was held seven (ten) days before the first (second) march, and fourteen days before the election on 24 April 1932. Thus, equation (2) allows us to examine the full dynamic pattern of the effect of marches.

4.2 Hypotheses. If marches mattered, we would 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*: observing Nazi supporters marching may have convinced people that political alternatives are more desirable. In this case, we would expect $\beta < 0$. It is also possible that both persuasion and backlash co-existed, with some sub-population becoming persuaded while another was turned against the Nazi message. In this case, β captures the average net effect. Ultimately, it is an empirical question whether persuasion or backlash dominated. We will provide two-sided test statistics against the null that there was no average effect.

Equation (2) allows us to investigate these effects over time. If the marches persuaded bystanders who were directly exposed ($\beta > 0$ in equation (1)), we expect an immediate effect on voting in the first election following the marches: $\beta_3 > 0$ in equation

[11] 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.

(2). Bystanders would acquire information on the day of the march. The election four days later would therefore mainly reflect immediate persuasion. The same logic applies if marches induced a backlash: $\beta < 0$ in equation (1) and $\beta_3 < 0$ in equation (2).

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 of equation (2): whatever the value of β_3, we expect $\beta_3 = \beta_s$ for $s = 4,5,6$. Since there is no obvious alternative to test, we will provide two-sided test statistics against that null hypothesis.[12]

To summarize, we test the following two hypotheses for direct effects:

H0: $\beta = 0$. Marches have no direct effect on voting.

H1: $\beta \neq 0$. Marches either persuaded ($\beta > 0$) or induced backlash ($\beta < 0$), and:

H0: $\beta_3 = \beta_s$, for all $s = 4,5,6$. Direct effects are time-invariant.

H1: $\beta_3 \neq \beta_s$, for some $s = 4,5,6$. Direct effects are time-variant.

4.3 Identification assumptions. In the ideal experiment, the marching path would be randomly assigned. In the absence of such an experiment, our difference-in-difference approach identifies the causal effect of march exposure *M* on support for NSDAP if, conditional on controls: absent the marches, voting would have evolved similarly in exposed and not exposed areas. The parallel trends assumption would be violated if Nazi planners deliberately targeted areas with growing NSDAP support. Here, we show that exposed and not exposed areas followed parallel electoral trends *prior* to the marches (parallel *pre*-trends). We estimate first-difference regressions in Table 2, examining changes in vote shares and turnout between the two 1932 Presidential elections, both held *before* the Nazi marches. We include our standard set of controls *X* and report the

---

[12] 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 $t = 3$. 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 $\beta_3 < \beta_s$, for some $s = 4,5,6$.

coefficient β in Table 2, Panel A, columns 1, 3 and 5. There is no evidence that polling stations closer to Nazi marches were experiencing faster growth in NSDAP voting before the marches, or 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. This makes intuitive sense: First, 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. Because Nazis planners were constrained to march through wide boulevards, the physical constraints imposed by street network were likely to be the major determinants of the final marching routes.

In addition, to rule out that differential shocks confounded the impact of marches, in Panel B we examine whether known shocks that occurred within the 14-day window separating the second Presidential vote and the City Diet election were correlated with our direct treatment variable. First, we read the first page of every edition of the *Hamburger Anzeiger* between the 10th and 24th of April, finding 43 main news items we identify that could, in principle, affect voting across Hamburg. Of them, 33% are local (Table A.1). We find only four events, which we geolocate, that may potentially have had a localized impact on elections: the murder of two SA members (10 April), a Nazi meeting in a *Bierhalle* (14 April), a rally of a pro-democracy association, the *Eiserne Front* (16 April) and a Hitler rally which took place close to Hamburg but outside the area we study (23 April). Columns (1), (3) and (5) show that areas around the first three events did not have a significantly different share of march-treated households. Because the Hitler rally took place further away from Hamburg itself, we cannot conduct the same test. However, col. 7 shows that the distance to Hitler's rally is not correlated with the share of treated households.[13] Overall, these results suggest that contemporaneous shocks are unlikely to confound our estimates.

4.4 Results: Simple difference in differences. We begin with a simple visualization of our data. Figure 3 shows changes in Nazi votes after the April 1932 marches. The map also reports the path of the marches. It is readily apparent that areas traversed by Nazi marches experienced much larger vote gains for the Hitler movement. In Figure 1, Panel A, we

[13] The Hitler speech itself may have had an effect. Recent research, however, suggests that the campaign effect of his speeches may have been lower than often thought (Selb and Munzert 2018).

already showed a binscatter with distance to the marches and the Nazi "swing", demonstrating how much greater gains were near the marching route. In combination, the graphical evidence provides strong support for the hypothesis that Nazi electoral gains were markedly greater in neighborhoods closer to the marches.

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 and the coefficient on log distance to the march remains highly significant. The estimates imply that moving from 100m to 1,000 meters from the marching path reduced Nazi vote gains by 0.3-0.5 pp. This compares with an average vote gain for the Nazi party of 5.13 pp between the pre- and post-march elections in Hamburg overall, i.e., the effect is equivalent to about 6-10% of the average.

In cols. 3-4 we use our preferred specification, predicting Nazi voting with the share of households living within 200m of the marches. The simplest 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 ($p < 0.001$). In the most conservative specification (col. 4) the coefficient drops by one-fifth, but remains highly significant. It implies that direct exposure to the Nazi marches added 0.9 p.p. – or about 19% of the overall increase in Nazi voting. In sum, the evidence rejects the first null hypothesis and shows that $\beta > 0$ in equation (1), consistent with the alternative hypothesis that the Nazi message *persuaded*, on average.

4.5 Results: Dynamics. Next, we examine the dynamic effect of direct exposure. Figure 4 plots β_t from equation (2): the impact of direct exposure in each separate election (full estimates in Table 3, col. 5). We find three results. First, the share of households within 200m from the march is not correlated with changes in NSDAP support in the two elections preceding the 2nd Presidential round. This confirms the absence of pre-trends. Second, the effect of the marches appears immediately: four days after the marches, polling stations closer to their path voted significantly more for the NSDAP. Third, the initial gains persist well into 1933, more than 300 days later, when the NSDAP still received around 1 p.p. more votes in polling stations where every household had been exposed to the march. The effect on political beliefs as reflected in voting behavior is *stable* and *persistent,* at least over the time span for which we have data. 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). Thus, we cannot reject our second null hypothesis that $\beta_3 = \beta_s$ for s = 4,5,6: direct effects appear to be time-invariant.

These results suggest three conclusions. First, marches were successful in *persuading* people directly exposed to them, at least on average. Second, the effect was powerful enough to *persist* for almost one year – despite numerous political events, inside and outside Hamburg. Third, after the initial impact, there were relatively few additional gains in areas close to the marches. This implies that the Nazi message diffused quickly in directly treated areas, and most who could be directly persuaded had made up their mind to support the Nazis by the end of April 1932. This is consistent with a simple Bayesian model of belief update in response to information.

The effect of the marches could travel beyond the areas immediately affected if people directly exposed to the marches spread the message among their friends, acquaintances, or co-workers living in other parts of the city. We now explore this possibility.

5 Indirect Effects: Did Propaganda Spread?

The previous section showed that areas directly exposed to the marches experienced an immediate and persistent swing in favor of the Nazi Party. This leads us to the core question of the paper: Can propaganda spread through social networks, like a contagious virus? We address this question with detailed data on the spread of the Spanish influenza and on the socio-economic characteristics of the population of Hamburg.

5.1 Measurement and specifications. Measuring social interactions is challenging. If we knew exactly who interacts with whom, we could identify which households had a chance to hear about the march via directly exposed contacts. Because most social interactions go unrecorded, we do not have access to a precise measure of social exposure to the marches. To make progress, we combine information from two newly digitized sources: the geography of the Spanish flu's spread in 1918-20 Hamburg, and a measure of homophily based on the 1932 Hamburg address book.

These two sources have different strengths. The 1918 flu virus infected through physical proximity. While it eventually spread to all areas of Hamburg, it arrived at different times. Since incubation periods are relatively short and stable, we can use co-movements in the number of deaths between different areas to measure the flu's geographic diffusion, indirectly capturing a combination of the frequency and intensity of physical meetings between people of Hamburg. We calculate the flu measure as

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

, where ρ_i is the correlation of flu deaths in district *i* with flu deaths in the march-treated area *T*, adjusting for correlations with other unexposed districts *C*. Appendix A describes the derivation in detail.

One limitation of the flu measure is that the virus could spread also among strangers who did not talk to each other, but simply spent time in close proximity (e.g., in cinemas or on public transport).[14] We augment the flu-based measure of exposure to the marches with a measure of homophily between exposed and unexposed areas. Because a large literature establishes that people interact more with people that are like themselves (McPherson, Smith-Lovin, and Cook 2001), we use homophily in the spirit of an intention-to-treat variable, reflecting potential social interactions. Figure 5 illustrates how differential exposure of two occupational groups affects our measure of connectedness. We plot the locations of train conductors and clock makers in panels A and B. Some 18% of train conductors were directly exposed to the marches according to our 200m cut-off; but a proportion more than twice as high, 38%, of clock makers was affected by the marches. The figure highlights affected households in red. Our approach is predicated on the assumptions that in areas far from the marches, but with more clock makers, *indirect* exposure to the Nazi marches was greater; whereas in areas with more dock workers, indirect exposure was less likely.

Specifically, we calculate social similarity between exposed and unexposed areas as

$$Hom_i^M = \mathrm{std}(s_i^T - s_i^C) \tag{5}$$

, where s_i is the average number of three shared characteristics (same occupational status, occupational sector and surname origin) in area *i* vis-a-vis the exposed (*T*) minus the unexposed part of town (*C*). Appendix B discusses the measures in more detail.

[14] Another limitation is that the flu broke out 14 years before the marches. Nonetheless, since social interactions tend to be relatively stable, social interactions in 1918 are arguably a good proxy for social interactions 14 years later. The cross-validation of our two measures lends credibility to this assumption.

Figure A.5, Panel A shows that the flu-based and homophily-based measure of social interactions are strongly correlated. Figure A.5, Panel B and C demonstrate that the same measures are less correlated with physical distance, emphasizing the limitations of proxying social proximity with physical proximity.

Our preferred measure of indirect exposure to the marches is the average of flu and homophily-based exposure to the marches:

$$S_i = \left(\frac{Flu_i^M + Hom_i^M}{2}\right) \tag{6}$$

S therefore captures the extent to which polling station *i* has *more* links to the march-treated area of Hamburg relative to links to unexposed parts of the city. Appendix section A and B provide full details on the construction of (6).

Figure 6 provides a visual representation of patterns and effects, focusing on one area of Hamburg to build intuition. Panel A plots indirect exposure to the march with different intensities of blue; darker dots represent polling stations with greater social connections to areas directly affected by the march. Panel B displays social exposure to the march following the same pattern, but focusing on a small area in the north-eastern part of Hamburg. The map shows that social exposure to the march varies even within small areas. It is not necessarily correlated with physical distance to the march. Panel C reports the change in NSDAP vote share between the two pre-treatment and the four post-treatment elections with different intensities of red (with darker colors indicating greater Nazi gains). The contrast between the two panels suggests that, far from the actual marching path, part of the variation in Nazi support can be explained by social exposure to the marches. The binned scatter in Figure 1, Panel B, shows that this relationship extends to the universe of Hamburg polling stations: areas with greater social links to polling stations traversed by the marches (on the x-axis) supported the Nazis more after the marches occurred (on the y-axis).

Next, we investigate the relationship in a regression framework. To do so, we augment the difference in difference equations (1) and (2) to allow social exposure to the march *S* to affect the NSDAP vote share:

$$N_{it} = \alpha_i + \alpha_t + \beta M_i \times Post_t + \gamma S_i \times Post_t + \sum_t \delta_t \times X_i + u_{it} \tag{7}$$

$$N_{it} = \alpha_i + \alpha_t + \sum_t \beta_t \times M_i + \sum_t \gamma_t \times S_i + \sum_t \delta_t \times X_i + u_{it} \tag{8}$$

Equations (7) and (8) add the interaction between indirect exposure S_i and the *Post* indicator. One concern may be that our measure of indirect exposure partly picks up direct exposure. This would be true if physical proximity to the marches correlates with S_i. We already showed physical and social proximity are only mildly correlated (Figure A.5, Panel B and C). Here we will present results with and without the direct treatment interaction, M_i, to assess how sensitive the results are to such a correlation structure.

While our data encompasses the universe of households in the city of Hamburg, our measure S_i is almost certainly noisy and measures social interactions between households residing in different neighborhoods with an error. If measurement error is classical, which seems plausible, that means we may expect γ to suffer from attenuation bias. If so, the estimate may be interpreted as a lower bound of the true indirect effects.

5.2 Hypotheses. If the persuasion created by the marches diffuses through social links, we should see areas closely connected to the march voting more for the Nazis afterwards. A "persuasion multiplier" implies $\beta > 0$ accompanied by $\gamma > 0$. The intuition is straightforward: Imagine two neighborhoods, A and B. The marching path goes through neighborhood A but not B; only households residing in A are directly exposed. $\beta > 0$ implies that significantly more households in A come to favor the Nazi Party. As the previous section showed, this effect is immediate: $\beta_3 > 0$. Now imagine two other neighborhoods, C and D. Neither is directly on the marching path, but in neighborhood C, many households interact socially with households from neighborhood A. In contrast, households in D are mostly connected to households in B. In this case, households in C are more likely to either just hear about the Nazi message from directly exposed households (an information relay effect) or to hear that the Nazis are an appealing political option (an endorsement effect). In either case, if such communication persuades households in C to vote for the Nazi Party, then $\gamma_s > 0$ for some $s = 3,4,5,6$. In the simple difference in difference specification of equation (1), we pool all the post-treatment periods into one dummy, so that γ captures the average effect in $s = 3,4,5,6$. Formally our first hypothesis for indirect effects is:

H0: $\gamma = 0$. Indirect exposure to the marches has no effect on voting.

H1: $\gamma > 0$. Indirect exposure to the marches persuades voters.

Equation (8) allows us to go beyond this test and examine belief dynamics in networks. Information takes time to travel across social networks, a core observation in diffusion or social learning models (e.g., DeMarzo et al. 2003; Golub 2017). Therefore, the social impact of the marches on voting will grow gradually, at least over some time interval. In the simple example above, if communication between households residing in neighborhoods A and C is frequent – especially communication regarding politics in general and the Nazi marches/party in particular – diffusion may be relatively rapid. This seems plausible given marches were highly salient and politically controversial events. In that case, some effects could arise even within 10 days of the first march: $\gamma_3 > 0$. In contrast, if communication is relatively slow, the immediate effect may be small to non-existent: $\gamma_3 = 0$. Importantly, regardless of whether indirect effects can occur within days, effectively all standard models on diffusion in networks predict that the magnitude of effects are time-dependent. Given our estimation framework, that gives rise to the following competing hypotheses:

H0: $\gamma_3 = \gamma_s$, for all s = 4,5,6. Diffusion is time-invariant.

H1: $\gamma_3 < \gamma_s$, for some s = 4,5,6. Diffusion is time-variant, increasing over some period of time.

In what follows, we will show *p*-values testing these competing hypotheses. Since the alternative hypothesis proposed by standard theory is one of growing indirect persuasion (H1), this implies a one-sided test of two competing hypotheses. We will also include standard errors allowing for regular confidence intervals of point estimates to be calculated.

An appealing aspect of equation (8) is that the specification allows for transparent examination of the effects in each election. However, elections are not evenly spaced across time. The two pre-marches elections were held 35 and 7 days before the first march, whereas the post-marches elections were held 4, 102, 200 and 319 days after the second march. An alternative way to test whether the indirect effects are time-dependent is to run an augmented specification where the indirect treatment is interacted with the number of days since the first post-marches election ($s=3$). Alternatively, we also use days since the first march.[15] We provide evidence using such an approach, complementing equation (8).

5.3 Identification assumptions. Analogous to the assumptions needed for direct effects, our difference-in-differences approach requires that, in the absence of the marches, Nazi

[15] In contrast with equation (8), which is fully flexible, this analysis requires making functional form assumptions on how diffusion depends on time. A linear assumption seems unwise, since at some point one would expect the indirect effects to flatten out. In fact, in the very long run, one may even expect them to disappear, if people in the city are strongly connected and beliefs evolve according to some simple DeGroot social learning dynamics where there is convergence as time goes to infinity. Here, we can estimate effects for about 300 days. We employ the simplest possible functional form – log days – which is not linear and allows for the indirect effects in absolute terms to effectively stabilize.

support in neighborhoods with strong social connections to the marching paths would have evolved similarly to Nazi support in neighborhoods with weak social connections. Other contemporaneous shocks that differently affected neighborhoods that are socially connected to households residing along the marching path would also violate the identifying assumptions.

We provide support for this assumption in even columns of Table 2, Panel A shows no evidence of pre-trends in support for the Nazi Party (right-wing extremism: col. 2), the Communists (left-wing extremism: col. 4), or political participation (turnout: col. 6). This is true conditional on direct exposure as well as unconditionally (Table A.2, Panel A). Similarly, even columns of Table 2, Panel B show no evidence that other events at the time of the marches were correlated with indirect exposure. Table 2 thus lends credibility to the main assumptions required to identify indirect effects.

5.4 Results. We report estimates of equation (7) in Table 4. 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 × election fixed effects in col. 2 reduces the point estimate but not the significance of the coefficient, which remains well above conventional levels ($p < 0.001$). Importantly, 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. In short, the stability of the point estimates across col. 2 and 3 highlights that social proximity to the march captures a determinant of voting that is not captured by physical proximity.

Columns 4 and 5 of Table 4 examine the dynamic pattern of these social effects: γ_t in equation (8). 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 on Figure 7. Three results stand out. First, as in the case of direct effects, social exposure to the march is uncorrelated with NSDAP voting in the election preceding the marches. The absence of pre-trends in social exposure strengthens the credibility of our empirical approach and confirms that changes in NSDAP support are not correlated with marching paths nor with areas that were socially connected to them. Second, we already observe an increase in NSDAP support during the City Diet election in polling stations where people would have heard of the marches via social connections. Third, in contrast with the direct effect, the evidence points to the impact of social exposure growing over time, at least in the first three elections after the marches. By the November 1932 election – 200 days after the marches – the point estimate is 88% larger than for the April 24 elections (0.576 and 0.307, respectively). We formally test the null hypothesis that diffusion was time invariant: the *p*-value of 0.045 allows us to reject the null in favor of the alternative suggested by standard models of diffusion. This is consistent with our hypothesis that propaganda spread progressively through social networks and took time to persuade those who were, in principle, "susceptible".[16]

Table A.3 shows the results of our complementary approach, adding the interaction with log days since the first post-election (columns 1-3), or log days since the first march (columns 4-6). The point estimate on the triple interaction is positive, consistent with increasing effects as a function of days since exposure. This specification also leads us to reject the null hypothesis that effects are time-invariant, in favor of our alternative hypothesis.

5.5 Assessing magnitudes. How economically significant are the direct and indirect effects? The quantification of effect sizes shows that the dynamic pattern of social effects is important. According to Table A.4, Panel A, our estimates imply that a one standard deviation change in *direct* exposure accounts for 16% of a one standard deviation change between the first post-treatment election and the one immediately before the marches. In contrast, by t=6 (March 1933 election), a one standard deviation change in direct exposure only accounts for 9% of a one standard deviation difference in NSDAP votes. Table A.4, Panel B illustrates that the implied effect of a one standard deviation change in our measure for *indirect* exposure accounts for 17% in t=3 and for 15% 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, and shows that the contribution of the indirect effect to the aggregate effect of the marches grows from 50% in April 1932 to 62% in November 1932.

---

[16] In Appendix Figure A.7 we show the high degree of robustness of our conclusions when we measure social exposure with either social homophily or flu correlation separately, instead of taking the average of the two.

5.6 Heterogeneous effects: From persuasion to backlash. On average, the marches persuaded Hamburg voters to support the Nazi Party -- in eq. 8, we find $\beta > 0$ combined with $\gamma > 0$. This does not tell us whether the effects were uniform – the Nazi Party, while admired by some, was loathed and feared by others. Observing the marches may have induced greater support in areas already leaning towards the Nazis – a form of bandwagon effect – 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 direct and indirect effects along the cumulative distribution functions (CDF) of the Nazi vote share, the probabilities that the vote share is below a certain level.[17] We estimate a linear probability model for the likelihood that the vote share is below some level x. For each dummy N_{itx}, indicating whether the neighborhood i, at election t, is below the Nazi vote share x, we estimate:

$$N_{itx} = \alpha_i + \alpha_t + \beta M_i \times Post_t + \gamma S_i \times Post_t + \sum_t \delta_t X_i + u_{it} \qquad (9)$$

, where we let x range across the support between the 5th and 95th percentile of N_{it}, in steps of two percentage points. We present the results in Figure 8. For ease of interpretation, we present positive estimates as reflecting a probability above the threshold, i.e., stronger Nazi support. In Panel A, we plot the effects of direct exposure. In Nazi-leaning neighborhoods the marches were persuasive. The estimates are strongly positive and highly significant in areas with substantial Nazi support, consistent with bandwagon effects of exposures. 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, as the point estimates are consistently negative at low levels of the CDF. However, these estimates are not statistically significant at conventional levels (only at ten percent significance).

Panel B of Figure 8 plots the estimates for indirect effects. The same pattern emerges: indirect persuasion occurs primarily in Nazi stronghold areas. Note, however, that since voting is a private act, this bandwagon effect is unlikely to be driven by social image concerns (Bursztyn, Egorov, and Fiorin 2020). Instead, a subset of a neighborhood's

[17] The distribution regression approach is appealing due to its simplicity, ease of interpretation and flexibility in tracing out impacts on the entire CDF. Previous applications include Duflo (2001), who estimates the difference-in-differences impact of school construction on the CDF of years of education, showing that the impact occurs for low levels of education during primary school years. Our approach is analogous to hers, except we estimate the difference-in-differences impact on the Nazi vote share. For a recent discussion on the relationship between distribution regressions and quantile regressions, which typically require stronger assumptions, see Chernozhukov et al. (2013).

population was persuaded, presumably because they were already leaning towards the Nazi party. Moreover, as with direct effects, in areas of low Nazi support there is no evidence of persuasion via social interactions. At low levels of the CDF, none of the point estimates are positive. Among the 6 negative estimates, 2 of them reject persuasion at the 5 percent level.

In panel C, we test the hypothesis that both the direct and indirect effects are jointly positive ("persuasion") or jointly negative ("backlash"). Formally, we test that $\beta + \gamma > 0$ (persuasion) and $\beta + \gamma < 0$ (backlash). We plot the *p*-value of this test over the distribution of vote shares for the Nazi Party, and find strong evidence for both mechanisms – at low levels of support for the Nazi Party (20% or less) the backlash effect is highly significant. At higher levels of Nazi support (above 35%), persuasion becomes significant. In combination, these results suggests that marches had a polarizing impact across Hamburg.[18] In this sense, contentious Nazi marches appear to have induced stronger divisions across neighborhoods, resulting in a more polarized city electorate overall.

6 Robustness

We examine the robustness of our findings to a number of potential issues. In particular, we examine whether spatially correlated errors lead us to understate standard errors, and find no evidence that this is the case (Appendix C.1). We also conduct a number of randomization inference exercise, to demonstrate the strength of our statistical findings (Appendix C.2). In addition, we use several matching estimators to show that even when we restrict analysis to highly balanced treatment and control groups, findings remain strong (Appendix C.4). We also show that results do not depend on the distance cut-off for direct treatment effects (Appendix C.5), that other marches by the Communists and the social democrats did not have the same effect, and turnout is not driving our results (Appendix C.6 and 7).

7 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 served a powerful tool persuading 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. This finding is in line with earlier studies

[18] This effect is reminiscent of the polarizing impact of information campaigns in Baysan (2021).

on mass demonstrations, as in the case of the Tea Party rallies in the US in 2009 (Madestam et al. 2013).

Our study goes beyond the direct persuasion effect of mass gatherings. Social spillovers have long been hypothesized in the campaign effect literature, but there is little evidence in real-life settings of them magnifying propaganda effects. Our unique setting allows us to demonstrate major social spillovers. We use two proxies for connectedness – contagious disease diffusion in Hamburg, using the 1918 flu epidemic, and a homophily-based measure derived from household characteristics. These two variables have 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; final spillovers were twice as large as the direct effect. Mass rallies also polarized voting. In areas with high levels of Nazi support, the marches increased support, but they led to a backlash in areas with low support. In areas with a high share of blue-collar workers and the unemployed, Nazi voting declined via both direct and indirect effects.

Did mass rallies, campaign speeches and marches matter for the fall of Germany's first democracy? Mass gatherings were only one dimension of the Nazi party's propaganda effort, but an important one: Barred from state-controlled radio and often faced with a ban on its newspapers, marches, meetings, and rallies were a crucial propaganda tool. In the run-up to the Presidential election in 1932, for example, the Nazi party held no fewer than an astonishing 34,000 rallies, meetings and marches all over Germany. A recent influential paper fails to find any significant effect of Hitler's campaign speeches, arguably the most high-profile type of event held by the Nazi Party (Selb and Munzert 2018). Our results suggest that widespread doubts about the existence of campaign effects, both generally and in Weimar Germany's twilight years, may reflect measurement issues rather than a lack of causal effects. In particular, the existence of social spillovers may blur the distinction of treated and untreated areas, making it harder to find effects. Of course, we cannot determine the effect of the nationwide wave of gatherings from Hamburg data. Nonetheless, our findings create a strong prior that mass rallies and marches were important tools for the Nazi Party, facilitating its big breakthrough in 1932, when it became the single largest party in Germany.

References

Abel, Theodore Fred, and Thomas Childers. 1986. *Why Hitler Came into Power*. Cambridge, Mass: Harvard University Press.

Adena, Maja, Ruben Enikolopov, Maria Petrova, Veronica Santarosa, and Ekaterina V. Zhuravskaya. 2015. 'Radio and the Rise of The Nazis in Prewar Germany'. *The Quarterly Journal of Economics* 130 (4): 1885–1939.

Arendt, Hannah. 1973. *The Origins of Totalitarianism*. Boston, Mass.: Houghton Mifflin Harcourt.

Arias, Eric, Pablo Balán, Horacio Larreguy, John Marshall, and Pablo Querubín. 2019. 'Information Provision, Voter Coordination, and Electoral Accountability: Evidence from Mexican Social Networks'. *American Political Science Review* 113 (2): 475–98.

Bailey, Michael, Ruiqing Cao, Theresa Kuchler, and Johannes Stroebel. 2018. 'The Economic Effects of Social Networks: Evidence from the Housing Market'. *Journal of Political Economy* 126 (6): 2224–76.

Banerjee, Abhijit, Arun G. Chandrasekhar, Esther Duflo, and Matthew O. Jackson. 2013. 'The Diffusion of Microfinance'. *Science* 341 (6144): 1236498.

Baysan, Ceren. 2021. 'Persistent Polarizing Effects of Persuasion: Experimental Evidence from Turkey'. *Working Paper* April 2021.

Bénabou, Roland, and Jean Tirole. 2006. 'Incentives and Prosocial Behavior'. *American Economic Review* 96 (5): 1652–78.

Blackwell, David, and Lester Dubins. 1962. 'Merging of Opinions with Increasing Information'. *The Annals of Mathematical Statistics* 33 (3): 882–86.

Bond, Robert M., Christopher J. Fariss, Jason J. Jones, Adam D. I. Kramer, Cameron Marlow, Jaime E. Settle, and James H. Fowler. 2012. 'A 61-Million-Person Experiment in Social Influence and Political Mobilization'. *Nature* 489 (7415): 295–98.

Bracher, Karl Dietrich. 1978. *Die Auflösung der Weimarer Republik*. Königstein/Ts.; Düsseldorf: Athenäum-Verlag; Droste.

———. 1979. *Die deutsche Diktatur: Entstehung, Struktur, Folgen d. Nationalsozialismus*. 6th ed. Frankfurt/M.: Ullstein.

Breitnauer, Jaime. 2019. *The Spanish Flu Epidemic and Its Influence on History: Stories from the 1918-1920 Global Flu Pandemic*. Yorkshire: Pen and Sword History.

Brustein, William. 1998. *The Logic of Evil: The Social Origins of the Nazi Party, 1925-1933*. New Haven; London: Yale University Press.

Bullock, Alan. 1994. *Hitler: A Study in Tyranny*. New York: Harper Perennial.

Bursztyn, Leonardo, Davide Cantoni, David Y. Yang, Noam Yuchtman, and Y. Jane Zhang. 2020. 'Persistent Political Engagement: Social Interactions and the Dynamics of Protest Movements'. *American Economic Review: Insights*, Forthcoming.

Bursztyn, Leonardo, Georgy Egorov, and Stefano Fiorin. 2020. 'From Extreme to Mainstream: The Erosion of Social Norms'. *American Economic Review* 110 (11): 3522–48.

Büttner, Ursula. 1982. *Hamburg in der Staats- und Wirtschaftskrise, 1928-1931*. Vol. 16. Christians.

Cantoni, Davide, David Y. Yang, Noam Yuchtman, and Y. Jane Zhang. 2019. 'Protests as Strategic Games: Experimental Evidence from Hong Kong's Antiauthoritarian Movement'. *The Quarterly Journal of Economics* 134 (2): 1021–77.

Chernozhukov, Victor, Iván Fernández-Val, and Blaise Melly. 2013. 'Inference on Counterfactual Distributions'. *Econometrica* 81 (6): 2205–68.

Conley, T. G. 1999. 'GMM Estimation with Cross Sectional Dependence'. *Journal of Econometrics* 92 (1): 1–45.

Das Telefonbuch Deutschland. 2015. TGV Telefonbuch- und Verzeichnisverlag.

DeGroot, Morris H. 1974. 'Reaching a Consensus'. *Journal of the American Statistical Association* 69 (345): 118–21.

DellaVigna, Stefano, and Matthew Gentzkow. 2010. 'Persuasion: Empirical Evidence'. *Annual Review of Economics* 2 (1): 643–69.

DellaVigna, Stefano, and Ethan Kaplan. 2007. 'The Fox News Effect: Media Bias and Voting'. *The Quarterly Journal of Economics* 122 (3): 1187–1234.

DeMarzo, Peter M., Dimitri Vayanos, and Jeffrey Zwiebel. 2003. 'Persuasion Bias, Social Influence, and Unidimensional Opinions'. *The Quarterly Journal of Economics* 118 (3): 909–68.

Duflo, Esther. 2001. 'Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment'. *American Economic Review* 91 (4): 795–813.

Durante, Ruben, and Brian Knight. 2012. 'Partisan Control, Media Bias, and Viewer Responses: Evidence from Berlusconi's Italy'. *Journal of the European Economic Association* 10 (3): 451–81.

Enikolopov, Ruben, Alexey Makarin, and Maria Petrova. 2020. 'Social Media and Protest Participation: Evidence From Russia'. *Econometrica* 88 (4): 1479–1514.

Evans, Richard J. 2004. *The Coming of the Third Reich*. London: Penguin.

———. 2006. *The Third Reich in Power*. London: Penguin.

Falter, Jürgen W. 1986. *Wahlen und Abstimmungen in der Weimarer Republik: Materialien zum Wahlverhalten 1913-1933*. Statistische Arbeitsbücher zur neueren deutschen Geschichte. Munich: C. H. Beck.

———. 1991. *Hitlers Wähler*. Munich: C. H. Beck.

———. 2020. *Hitlers Parteigenossen: Die Mitglieder der NSDAP 1919-1945*. Frankfurt: Campus Verlag.

Falter, Jürgen W., and Dirk Hänisch. 2013. 'Die Anfälligkeit von Arbeitern gegenüber der NSDAP bei den Reichstagswahlen 1928-1933'. *Historical Social Research / Historische Sozialforschung. Supplement*, 25: 145–93.

Fischer, Conan. 2002. *The Rise of the Nazis*. Manchester University Press.

Foos, Florian, and Eline A. de Rooij. 2017. 'All in the Family: Partisan Disagreement and Electoral Mobilization in Intimate Networks—A Spillover Experiment'. *American Journal of Political Science* 61 (2): 289–304.

Golub, Benjamin. 2017. 'Learning in Social Networks'. *SSRN Electronic Journal*.

Hadamovsky, Eugen. 1933. *Propaganda und nationale Macht. Die Organisation der öffentlichen Meinung für nationale Politik*. Oldenburg: Verlag Gerhard Stalling.

Hainmueller, Jens. 2012. ‘Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies’. *Political Analysis* 20 (1): 25–46.

Hamburger Adreßbuch. 1932. Hamburg: Hamburger Adreßbuch-Verlag.

Hamburger Anzeiger. 1932, April 1932.

Hamburger Nachrichten. 1932a. ‘Der Sonntag vor der Wahl: Nationalsozialisten marschieren’, 18 April 1932.

———. 1932b. ‘Nächtlicher Werbemarsch’, 21 April 1932.

Hamburger Tageblatt. 1932. ‘Fahnen auf Sturm: Der Marsch des deutschen Hamburg’, 18 April 1932.

Hillygus, D. Sunshine. 2010. ‘Campaign Effects on Vote Choice’. In *Oxford Handbook on Elections and Political Behavior*, edited by Jan Leighly and George C. Edwards III, 326–45. Oxford: Oxford University Press.

Iacus, Stefano M., Gary King, and Giuseppe Porro. 2012. ‘Causal Inference without Balance Checking: Coarsened Exact Matching’. *Political Analysis* 20 (1): 1–24.

Iyengar, Shanto, Yphtach Lelkes, Matthew Levendusky, Neil Malhotra, and Sean J. Westwood. 2019. ‘The Origins and Consequences of Affective Polarization in the United States’. *Annual Review of Political Science* 22 (1): 129–46.

James, Harold. 1986. *The German Slump: Politics and Economics, 1924-1936*. Oxford: Oxford University Press.

Kelly, Morgan. 2019. ‘The Standard Errors of Persistence’. *SSRN Electronic Journal*.

Kenny, Christopher B. 1992. ‘Political Participation and Effects from the Social Environment’. *American Journal of Political Science* 36 (1): 259–67.

Kershaw, Ian. 1983. ‘How Effective Was Nazi Propaganda?’ In *Nazi Propaganda: The Power and the Limitations*, 180–205. London: Croom Helm.

———. 2001. *Hitler, 1889-1936: Hubris*. London: Allen Lane.

Kossinets, Gueorgi, and Duncan J. Watts. 2009. ‘Origins of Homophily in an Evolving Social Network’. *American Journal of Sociology* 115 (2): 405–50.

Landesbetrieb Geoinformation und Vermessung Hamburg. 2020. ‘Historische Karten 1: 5000. Jahrgang 1930-1940’. Retrieved from https://metaver.de/trefferanzeige?docuuid=7A32A730-8B7A-4BB1-9D7F-D0BB37805696.

Madestam, Andreas, Daniel Shoag, Stan Veuger, and David Yanagizawa-Drott. 2013. ‘Do Political Protests Matter? Evidence from the Tea Party Movement’. *The Quarterly Journal of Economics* 128 (4): 1633–85.

McPherson, Miller, Lynn Smith-Lovin, and James M Cook. 2001. ‘Birds of a Feather: Homophily in Social Networks’. *Annual Review of Sociology* 27 (1): 415–44.

Mutz, Diana C., and Paul S. Martin. 2001. ‘Facilitating Communication Across Lines of Political Difference: The Role of Mass Media’. *American Political Science Review* 95 (1): 97–114.

Nickerson, David. W. 2008. ‘Is Voting Contagious? Evidence from Two Field Experiments’. *American Political Science Review* 102 (1): 49–57.

Noakes, Jeremy. 1971. *The Nazi Party in Lower Saxony, 1921-1933*. London: Oxford University Press.

O Broin, Turlach. 2016. ‘Mail-Order Demagogues: The NSDAP School for Speakers, 1928–34’. *Journal of Contemporary History* 51 (4): 715–37.

Satyanath, Shanker, Nico Voigtländer, and Hans-Joachim Voth. 2017. ‘Bowling for Fascism: Social Capital and the Rise of the Nazi Party’. *Journal of Political Economy* 125 (2): 478–526.

Selb, Peter, and Simon Munzert. 2018. ‘Examining a Most Likely Case for Strong Campaign Effects: Hitler’s Speeches and the Rise of the Nazi Party, 1927–1933’. *American Political Science Review* 112 (4): 1050–66.

Sköllin, ed. 1932a. *Statistische Mitteilungen über den hamburgischen Staat*. Vol. 28. Hamburg: Lütcke & Wulff.

———, ed. 1932b. *Statistische Mitteilungen über den hamburgischen Staat*. Vol. 29. Hamburg: Lütcke & Wulff.

———, ed. 1932c. *Statistische Mitteilungen über den hamburgischen Staat*. Vol. 30. Hamburg: Lütcke & Wulff.

———, ed. 1933. *Statistische Mitteilungen über den hamburgischen Staat*. Vol. 31. Hamburg: Lütcke & Wulff.

Sonntag, Christian. 2006. *Medienkarrieren: biografische Studien über Hamburger Nachkriegsjournalisten 1946-1949*. Forum Kommunikation und Medien 5. München: M Press.

State Archive Hamburg. 1917. ‘Hamburger Sterberegister, 1917-1919’. 332-5: Standesämter, Personenstandsregister, Sterberegister, 1876-1950.

———. 1932a. ‘Anlage 2: Werbe-Marsch am Mittwoch dem 20.4.32’. 614-2/5_B 212: Werbemärsche und Kundgebungen im Freien (1930-1937).

———. 1932b. ‘Werbe-Marsch am 17.4.32’. 614-2/5_B 212: Werbemärsche und Kundgebungen im Freien (1930-1937).

Statistisches Landesamt Hamburg, ed. 1932. *Statistisches Jahrbuch für die Freie und Hansestadt Hamburg 1932*. Hamburg: Lütcke & Wulff.

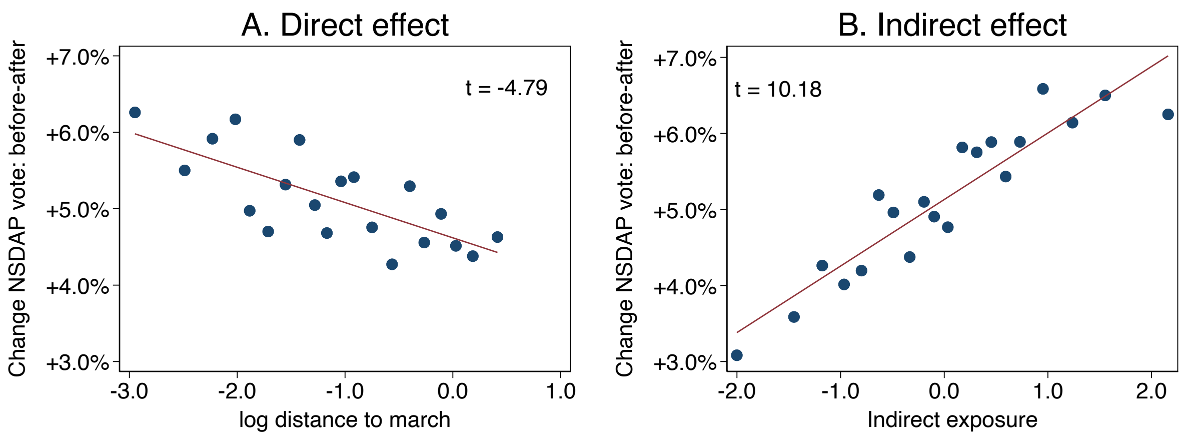
Statistisches Reichsamt. 1933. ‘Die Ergebnisse der Volks-, Berufs- und Betriebszaehlung vom 16.6.1933’. *Statistik des Deutschen Reichs* Band 453.

Tellier, Raymond. 2006. ‘Review of Aerosol Transmission of Influenza A Virus’. *Emerging Infectious Diseases* 12 (11): 1657–62.

Turner, Henry Ashby. 1997. *Hitler’s Thirty Days to Power: January 1933*. New York: Basic Books.

Figures

Figure 1: Basic patterns.

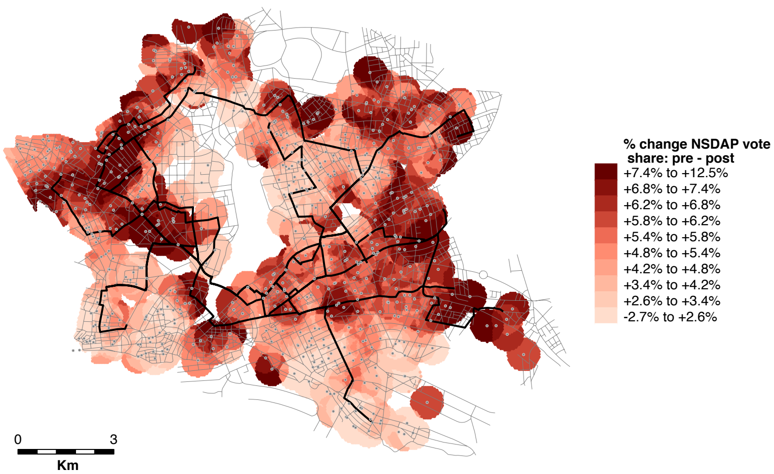


Note: Panel A: The Figure plots a bin-scatter of log distance to the closest Nazi march (x-axis) against the change in NSDAP vote share after the marches (y-axis). Change in NSDAP vote is calculated between two election before the marches (13 March and 10 April 1932) and four elections after (24 April; 31 July; 6 November 1932 and 5 March 1933) *t*-statistic is estimated from a bivariate regression. Panel B: The Figure plots a bin-scatter of indirect exposure (x-axis) against the change in NSDAP vote share after the marches (y-axis). See main text and appendix for construction of indirect exposure measure. Sources: Nazi marches: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b); Indirect exposure: 1932 Hamburg address book (Hamburger Adreßbuch, 1932) and death records (State Archive Hamburg, 1917); voting data: statistical bulletin of Hamburg (Sköllin, 1932a; 1932b; 1932c; 1933).

Figure 2: Direct exposure.

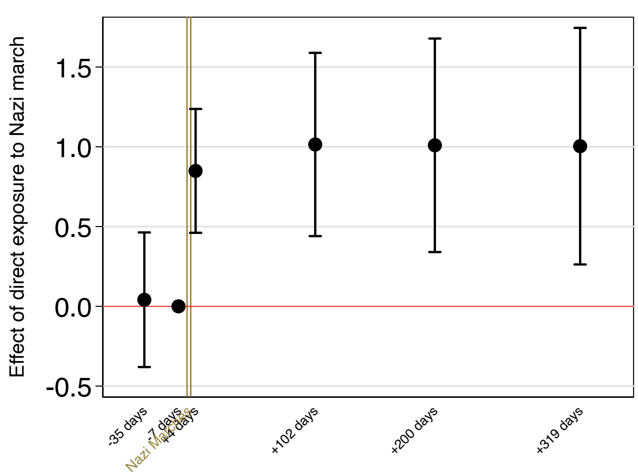
Note: The map shows the routes of the two Nazi marches in April 1932 (black lines) and the addresses of the 400,000 households living in Hamburg in 1932. Red dots are addresses located less than 200m from one of the marches' routes; gray dots are all other addresses. Sources: households data: 1932 Hamburg 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 four after (24 April 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; 1932c; 1933); Nazi marches: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b).

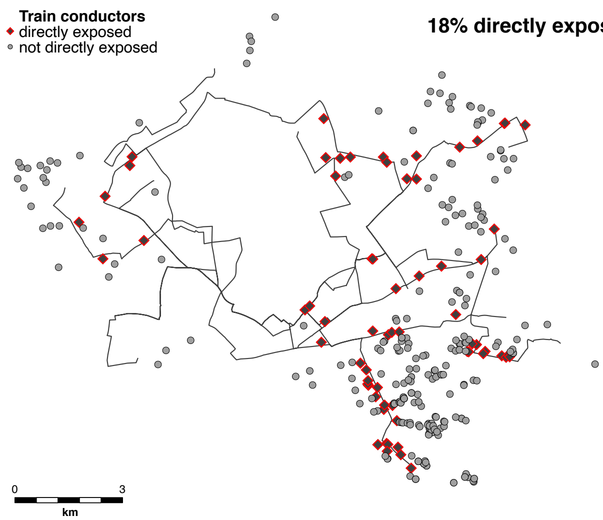
Figure 4: Direct effect over time.



Note: 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).

Figure 5: Illustration of occupational information.

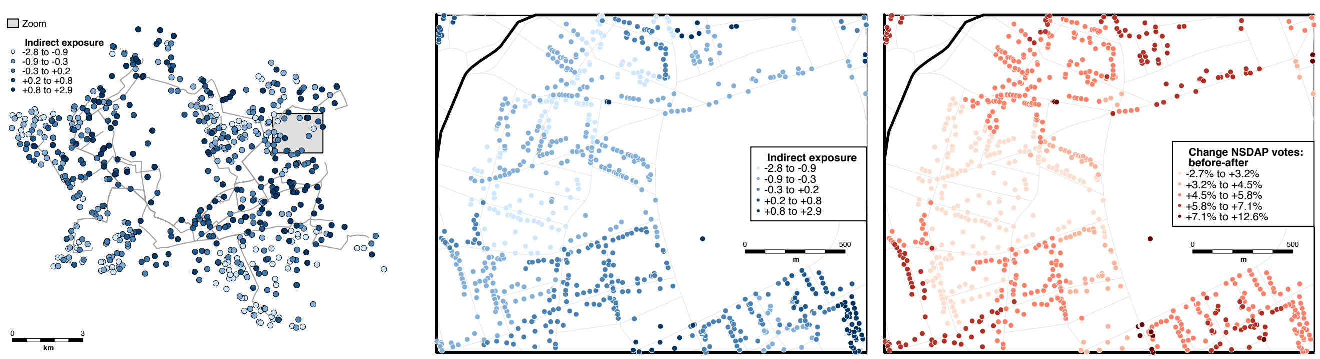
(a) Train conductors (b) Clock makers



Note: Panel A: Households with a train conductor as head of household. Panel B: Households with a clock maker as head of households. Dark grey diamonds with red outline mark directly exposed households (within 200m of marching route). Dots with light grey filling represent households farther than 200m from marching route. Source: 1932 Hamburg address book (Hamburger Adreßbuch, 1932); Nazi marches: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b).

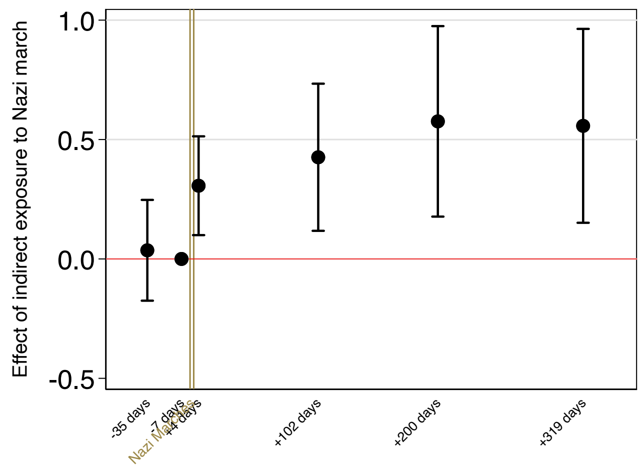
Figure 6: Visualization indirect effect.

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



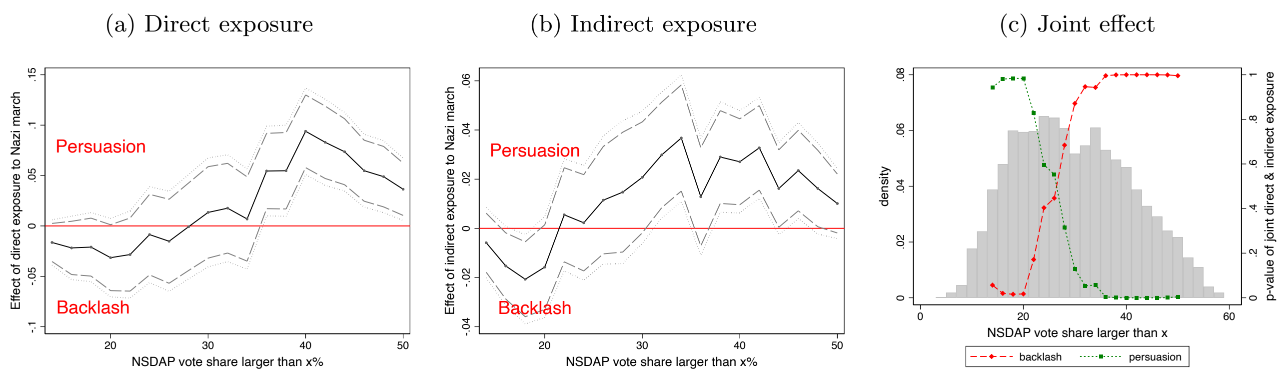
Note: Panel A: The maps shows the location of the polling station in Hamburg (blue dots). We split polling stations by indirect exposure measure into 5 equally sized groups. Color intensity increases with higher indirect exposure to the march. See main text and appendix for construction of indirect exposure measure. Panel B/C: The maps show the location of households farther than 200m from the march (= not directly treated). Change in NSDAP vote is calculated between the two elections before the marches (13 March and 10 April 1932) and the four after (24 April 1932 to 5 March 1933). We attribute indirect exposure (Panel B) and change in NSDAP votes (Panel C) of the polling station the household is assigned to. For the full sample we split households by 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; 1932c; 1933); indirect exposure: 1932 Hamburg address book (Hamburger Adreßbuch, 1932) and 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 over time.



Note: Plot of estimates of indirect exposure estimated from equation (8) and corresponding 95% confidence intervals by election (computed from Table 4, column 5). See main text and appendix for construction of indirect exposure measure.

Figure 8: Heterogeneity: Distribution Regression.



Note: Heterogeneity: Plot of estimates of direct (Panel A) and indirect exposure (Panel B) estimated from equation (7) with corresponding 90% (dashed line) and 95% (dotted line) confidence intervals (y-axis). Dependent variable is an indicator = 1, if NSDAP vote share is above threshold (x-axis). Panel C displays p-values of the joint effect of direct and indirect exposure estimated from equation (7). Green line shows p-value for persuasion (one-sided test against H0: joint effect ≤ 0); red line shows p-value for backlash (one-sided test against H0: joint effect ≥ 0). Grey bars display a histogram of the NSDAP vote share distribution across all six elections. Panel A, B and p-values in Panel C are created over support between the 5^{th} and 95^{th} percentile of the NSDAP vote share distribution. See main text and appendix for construction of indirect exposure measure.

Tables

Table 1: Summary statistics.

	Min	Mean	Max	St. dev.	Obs.
Election results					
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 24 April 32 (post)	7.938	31.047	53.465	10.582	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	28.135	100	45.002	622
Share households directly exposed to SPD march	0	19.775	100	39.862	622
Connection to march					
Indirect exposure of households (std.)	-2.781	0.000	2.852	1.022	622
Indirect exposure (flu-based) of households (std.)	-2.888	0.000	1.851	1.001	622
Indirect exposure (homophily-based) of households (std.)	-2.559	0.000	2.826	1.001	622
Demographic controls					
Number of voters at polling station	501	1295.413	1940	171.677	622
Share of blue collar workers	0	35.802	63.793	14.526	622
Share of households with telephone	0	12.168	73.165	12.331	622
Share of households with heating	0	6.011	75.000	12.281	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, 1932a; 1932b; 1932c; 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 indirect exposure measures. Share of households with telephone, with heating, share of blue collar workers 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 based on 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: Panel A. Pre-trends: 13 March-10 April 1932.

	Change 13 March - 10 April 1932					
	(1)	(2)	(3)	(4)	(5)	(6)
	Δ NSDAP	Δ NSDAP	Δ KPD	Δ KPD	Δ turnout	Δ turnout
Share households directly exposed	-0.039	-0.037	0.062	0.067	-0.018	-0.009
	[0.216]	[0.216]	[0.140]	[0.140]	[0.160]	[0.160]
Indirect exposure of households		-0.021		-0.066		-0.095
		[0.108]		[0.069]		[0.079]
Constant	-4.298	-4.135	2.965	3.477	2.674	3.414
	[4.313]	[4.384]	[4.308]	[4.270]	[3.487]	[3.515]
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Street controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.551	0.551	0.512	0.513	0.146	0.148
Mean change in Y	6.157	6.157	-2.793	-2.793	-4.757	-4.757
Observations	622	622	622	622	622	622

Table 2: Panel B. Contemporaneous events.

	Within 500m from:						log distance to	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	SA murder	SA murder	Nazi rally	Nazi rally	EF rally	EF rally	Hitler rally	Hitler rally
Share households directly exposed	-0.008	-0.007	-0.004	-0.004	-0.000	-0.000	-0.034	-0.026
	[0.006]	[0.006]	[0.006]	[0.005]	[0.000]	[0.000]	[0.049]	[0.049]
Indirect exposure of households		-0.000		-0.000		0.001		-0.020
		[0.003]		[0.002]		[0.001]		[0.018]
Constant	0.009	0.009	0.008	0.008	0.002	0.002	2.203	2.200
	[0.005]	[0.005]	[0.004]	[0.004]	[0.002]	[0.002]	[0.025]	[0.025]
R^2	0.001	0.001	0.000	0.000	0.000	0.000	0.001	0.003
Mean dependent variable	0.006	0.006	0.006	0.006	0.002	0.002	2.192	2.192
Observations	622	622	622	622	622	622	622	622

Note: Panel A: Immediate pre-trends (first presidential election round to second presidential election) of election outcomes (Sköllin, 1932a). Estimates of equation (3) with vote share (cols. 1-2); KPD vote share (cols. 3-4); voter turnout (cols. 5-6) as dependent variable. Odd columns (1, 3 and 5) use share of households within 200m of Nazi march as only exposure measure. Even columns (2, 4 and 6) use both, share households within 200m of Nazi march and indirect exposure as exposure measures. In all regressions we control for demographic and street network controls. Panel B: Contemporaneous events and exposure. OLS estimates of regressing: indicator whether a polling station is located within 500m of location of murder of SA men (cols. 1-2); Nazi rally (cols. 3-4); Eiserne Front rally (cols. 5-6); log of distance to Hitler rally (cols. 7-8) as dependent variable. Odd columns (1, 3, 5 and 7) use share of households within 200m of Nazi march as only exposure measure. Even columns (2, 4, 6 and 8) use both, share households within 200m of Nazi march and indirect exposure as exposure measures. Contemporaneous events are coded and geolocated from newspaper articles between 10 April 1932 and 24 April 1932 spanning the time between the last pre-treatment and first post-treatment election (Hamburger Anzeiger, 1932). 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	-0.462	-0.379			
	[0.096]	[0.091]			
Share households directly exposed × post march			1.179	0.949	
			[0.246]	[0.229]	
Share households directly exposed × t6 (post)					1.004
					[0.378]
Share households directly exposed × t5 (post)					1.009
					[0.341]
Share households directly exposed × t4 (post)					1.014
					[0.293]
Share households directly exposed × t3 (post)					0.849
					[0.198]
Share households directly exposed × t1 (pre)					0.041
					[0.215]
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	0.852	0.904	0.852	0.904	0.904
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417
Observations	3732	3732	3732	3732	3732
Dynamic persuasion hypothesis tests					
Direct effect t6 = t3: p-value					0.607
Direct effect t5 = t3: p-value					0.550
Direct effect t4 = t3: p-value					0.455

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. 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. $\beta_3 = \beta_s$ for $s = 4, 5, 6$ (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	0.875	0.470	0.449		
	[0.086]	[0.126]	[0.125]		
Indirect exposure of households × t6 (post)				0.580	0.557
				[0.208]	[0.207]
Indirect exposure of households × t5 (post)				0.599	0.576
				[0.204]	[0.204]
Indirect exposure of households × t4 (post)				0.449	0.426
				[0.158]	[0.157]
Indirect exposure of households × t3 (post)				0.326	0.307
				[0.106]	[0.106]
Indirect exposure of households × t1 (pre)				0.038	0.036
				[0.108]	[0.108]
Share households directly exposed × post march			0.908		
			[0.226]		
Share households directly exposed × t6 (post)					0.952
					[0.375]
Share households directly exposed × t5 (post)					0.955
					[0.337]
Share households directly exposed × t4 (post)					0.976
					[0.290]
Share households directly exposed × t3 (post)					0.822
					[0.196]
Share households directly exposed × t1 (pre)					0.038
					[0.215]
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	0.857	0.904	0.905	0.904	0.905
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417
Joint direct and indirect effect: p-value			0.000		
Observations	3732	3732	3732	3732	3732
Dynamic persuasion hypothesis tests					
Indirect effect t6 > t3: p-value				0.056	0.060
Indirect effect t5 > t3: p-value				0.042	0.045
Indirect effect t4 > t3: p-value				0.145	0.153
Direct effect t6 = t3: p-value					0.667
Direct effect t5 = t3: p-value					0.619
Direct effect t4 = t3: p-value					0.488

Note: *Note*: Estimates of equation (7) with indirect exposure as only measure of exposure (col. 1 and 2) and both, indirect exposure and share of households within 200m of march (col. 3) as measures of exposure. Estimates of equation (8) with indirect exposure as only measure of exposure (col. 5) and both, 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 indirect exposure, i.e. $\gamma_s > \gamma_3$ for $s = 4, 5, 6$ (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. $\beta_3 = \beta_s$ for $s = 4, 5, 6$ (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:

$$\rho_i^T = \sum_{j_{\text{treated}}}^{J_{\text{treated}}} \rho_{ij}^*$$

One limitation of this measure is that it may pick up general connectivity to other parts of the city, both exposed and 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, as:

$$\rho_i^C = \sum_{j_{\text{not treated}}}^{J_{\text{not treated}}} \rho_{ij}^*$$

Our preferred measure is the *difference between* ρ^{T}_{j} and ρ^{C}_{j}, which we standardize:

$$\text{Flu}_i^M = \text{std}(\rho_i^T - \rho_i^C)$$

B. Homophily measure

To construct homophily-based exposure to the marches Hom^M, 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. Next, 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: $[s^*_{ij}]$. 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. 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 average of all average similarities with polling stations that were directly treated:

$$s_i^T = \frac{\sum_{j_{\text{treated}}}^{J_{\text{treated}}} s^*_{ij}}{J_{\text{treated}}}$$

One limitation of this measure is that it may pick up general connectivity to other parts of the city, both exposed and 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, as:

$$s_i^C = \frac{\sum_{j_{\text{not treated}}}^{J_{\text{not treated}}} s^*_{ij}}{J_{\text{not treated}}}$$

Our preferred measure is the *difference between* s^T_j and s^C_j, which we standardize:

$$\text{Hom}_i^M = \text{std}(s_i^T - s_i^C)$$

C. Robustness

In this section, we demonstrate the robustness of our main findings.

C.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.[19] Table A.5 presents results for re-estimating equation (7) 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. When we use the Conley (1999) formula, we correct standard errors by allowing serial correlation across the 6 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.

C.2 Randomization inference. The assignment to (direct) treatment in our study is based on the distance of households to the marching route. To ensure that our statistical tests are not influenced by this factor, we perform a series of simulation studies in which we (i) randomize the outcome variable (NSDAP vote shares), (ii) randomize the outcome variable imposing spatial correlation and (iii) randomize the treatment by creating random marching routes on the street network of Hamburg. First, we draw random NSDAP vote share (assuming a normal distribution with mean and standard deviation from the actual data) for our 622 polling stations and five election periods. We estimate Equation 7 1,000 times, using the randomized NSDAP vote share as the dependent variable, collecting *t*-statistics and *p*-values. Only 5% of the simulated regressions lead to a *p*-value of below 5%. None of these simulations outperforms the actual regressions; the measured effects are unlikely to be the result of chance (Table A.6, Panel A and Figure A.8).

Second, we follow Kelly (2019), creating random variation in the outcome, re-running our regressions 1,000 times while imposing spatial correlation on the initial NSDAP vote share and NSDAP vote changes. As Appendix Table A.6, Panel B and Figure A.9 show, outperformance of the randomized coefficients (absolute value of simulated *t*-stat is larger than from actual data) is rare – below or only slightly above 5% (for the most conservative range of correlation: 0.01 degrees).

[19] We estimate Moran's I z-score of regression residuals from estimating equation (7). Appendix Figure A.6 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.

In our third simulation, we create random marches that mimic the observed marches. We first generate seven random points (five starting-, one mid- and one endpoint) that are at least 500m away from another. Computing optimal paths between them gives us 14 marching sections, as in the actual data (see Figure A.10, Panel B). We then re-estimate our model as stated in equation (1). The outperformance rate is well below 5% which suggest that our main statistical results are valid (Table A.6, Panel C).

C.3 Sample restrictions. Over the course of six elections (March 1932 to March 1933), 62 polling stations changed location. These changes affect 10% of our main sample. Moreover, the new location was always within 500 meters of the old one. In Table A.7 we drop these polling stations. Estimates and significance remains virtually unchanged in the sample of polling stations that did not move.

C.4 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 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 to an effect that is only slightly smaller than our baseline effect.

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 (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.

C.5 Indirect exposure effects for different cutoffs. To compute our indirect exposure measure, we use a cutoff of having at least 80% of households within 200m of the marches. We also experiment with five different cutoffs of 50%, 60%, 70%, 80% or 90% of households are located within 200m of the march. We compute similarity measures in identical fashion as before, exploiting the *difference* in flu connections and homophily between the treated relative and the untreated area. Table A.10 shows that our main results are robust to varying the direct treatment cutoff.

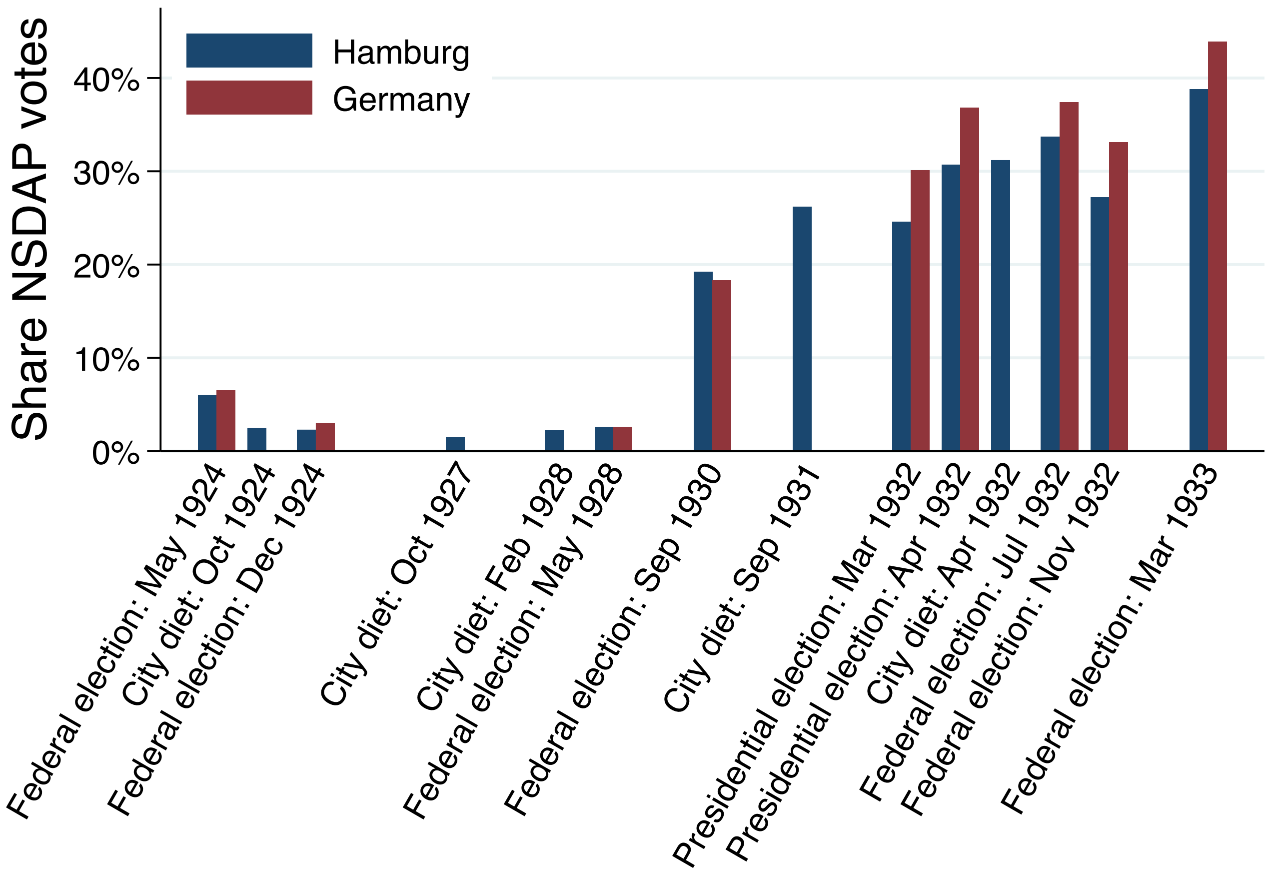
C.6 Other marches. Both the Social Democrats (SPD) and the Communist Party (KPD) also organized large scale marches and rallies in Hamburg. We digitize the marching route of the two marches, compute distances to the marches, and include the fraction of households within 200m to the KPD and SPD as additional regressors in our baseline specification. When using the full set of demographic and street network controls, there is no independent effect of either the SPD or the KPD march on changes in the NSDAP vote share (Table A.11, Panel A, cols. 2, 4 and 6). Our estimates for direct and indirect exposure of the Nazi marches remain virtually unchanged (Table A.11, Panel B).

C.7 Turnout. Did marches actually change voter's preferences, or motivate additional people to cast their vote on election day? While we cannot directly observe whether the same voters cast their vote in all elections, we find no evidence that turnout changed due to the marches (Table A.12) This strengthens the case for political persuasion changing voters' private beliefs.

C.8 Separate indirect exposure measures. Our main estimates combine two measures of indirect exposure – influenza and homophily. Figure A.7 shows results of estimating equation (8) for each of these two exposure measures separately. Both measures individually show a broadly similar picture as our combined measure. There are no significant pre-trends in either case. The post-treatment effect starts small, but grows in magnitude in both cases.

D 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 Germany and Hamburg between 1924 and 1933. Sources: Germany (Falter, 1986); Hamburg (Büttner, 1982).

Figure A.2: Timeline of events, Hamburg, 1932-1933.

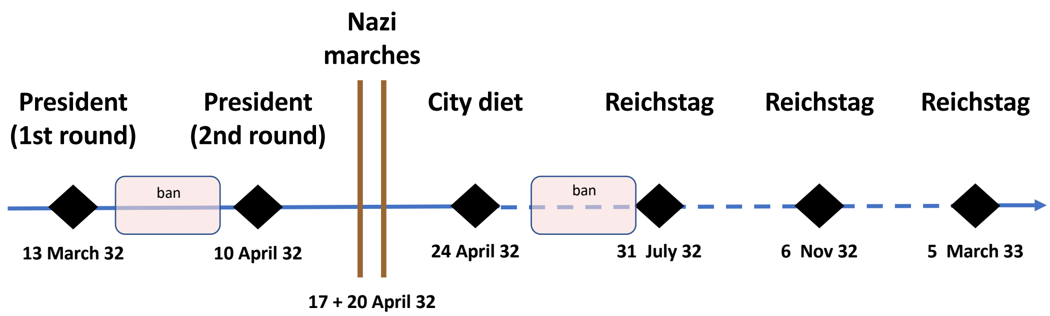
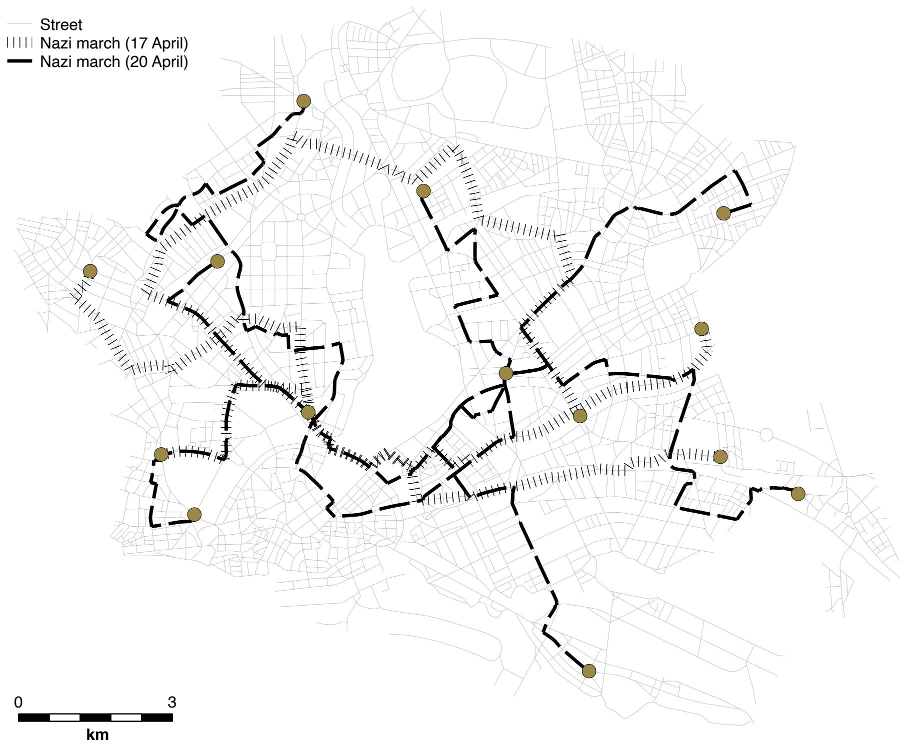
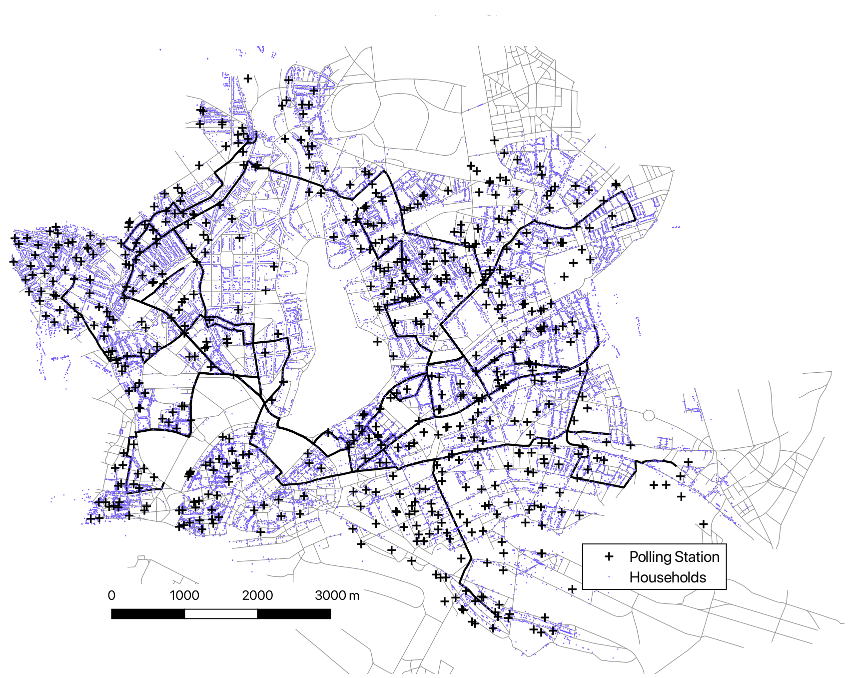


Figure A.3: Marches, households and polling stations.

(a) Nazi marches in Hamburg in April 1932.

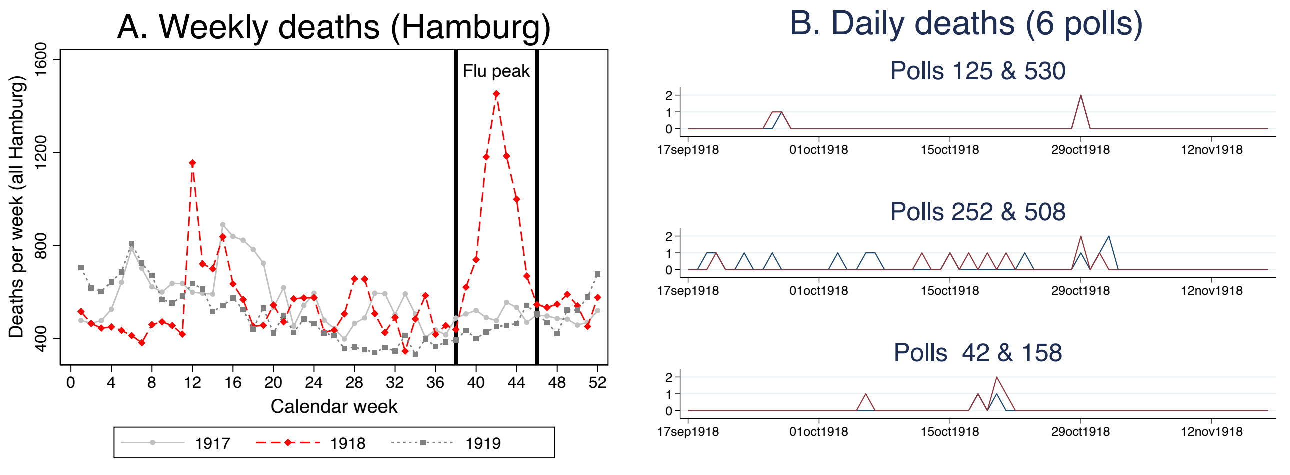


(b) Households and polling stations.



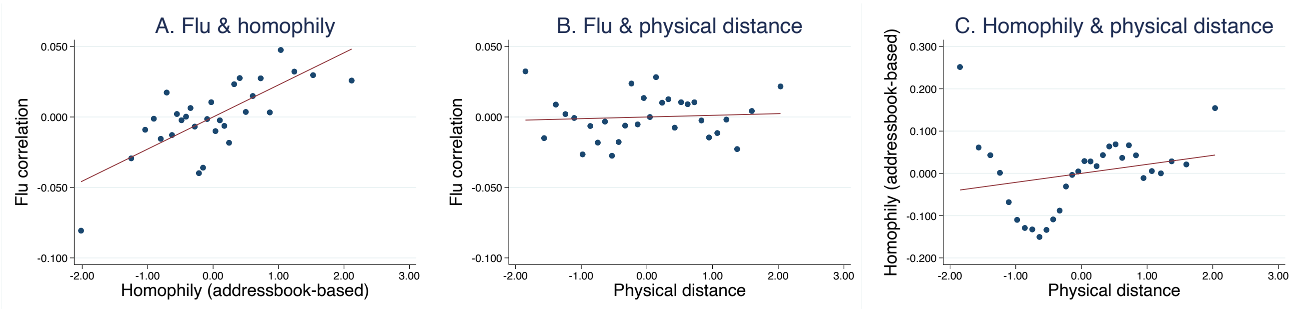
Note: Panel A: The map shows the routes of the two Nazi marches on April 17th, 1932 (short dash) and April 20th, 1932 (long dash). Brown circles identify the starting and ending point of the marches. Panel B: The map shows the location of the polling stations in Hamburg (black crosses) and the addresses of the 400,000 households living in Hamburg in 1932 (blue dots). We overlay these location 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); voting data: statistical bulletin of Hamburg (Sköllin, 1932a; 1932b; 1932c; 1933); households data: 1932 Hamburg address book (Hamburger Adreßbuch, 1932);.

Figure A.4: Spanish flu deaths.



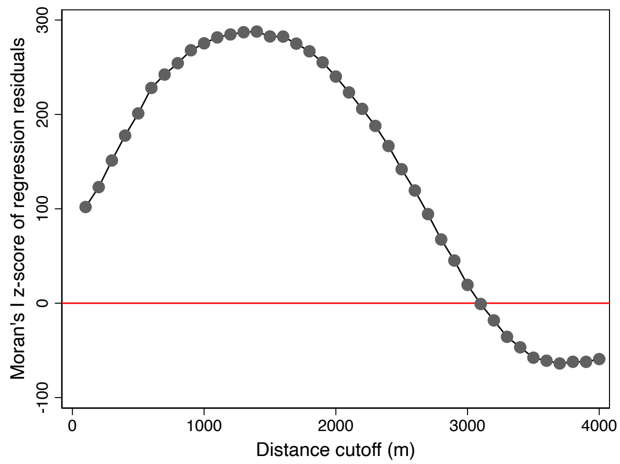
Note: Panel A: Weekly deaths in the city of Hamburg for 1917 to 1919. Panel B: daily deaths in 6 separate polls. Source: Hamburg death records (State Archive Hamburg, 1917).

Figure A.5: Validation flu correlation measure.



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

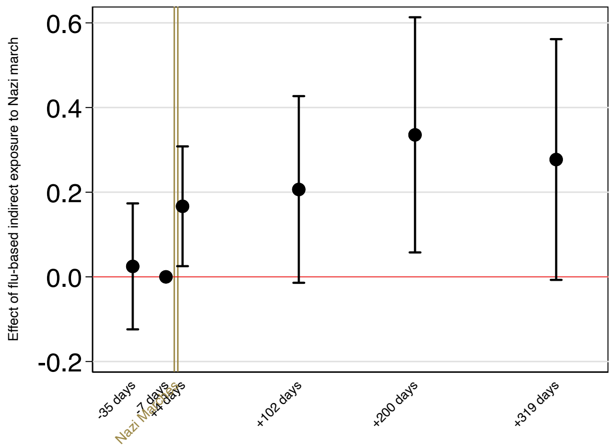
Figure A.6: Spatial autocorrelation of regression residuals.



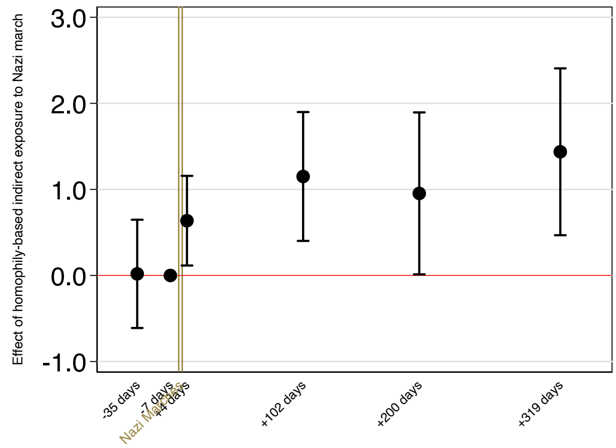
Note: Robustness: Panel A: Plot of Moran's I z-score of regression residuals (y-axis) estimated from equation (7). Moran's I is computed using a binary spatial weight matrix with varying distance cutoffs (x-axis).

Figure A.7: Individual indirect exposure measures.

(a) Flu-correlation measure



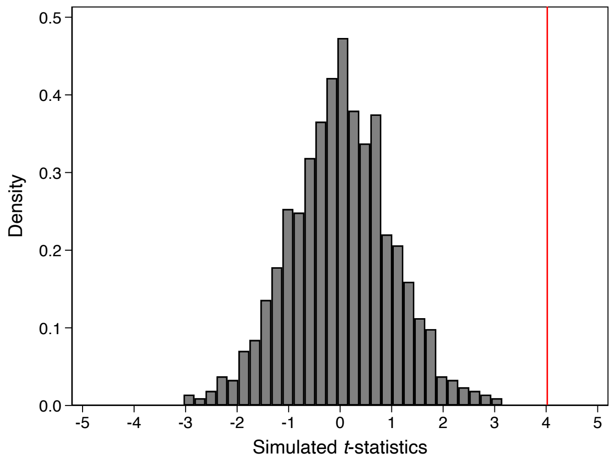
(b) Homophily (addressbook-based)



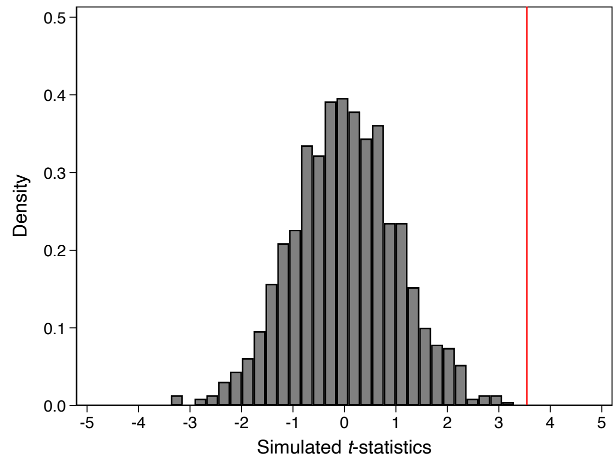
Note: Robustness: Panel A: Plot of estimates of indirect exposure based on correlation in influenza deaths estimated from equation (8) and corresponding 95% confidence intervals by election. Panel B: Plot of estimates of indirect exposure based on homophily estimated from equation (8) and corresponding 95% confidence intervals by election. See main text and appendix for construction of indirect exposure measure.

Figure A.8: Random NSDAP vote share.

(a) Direct exposure



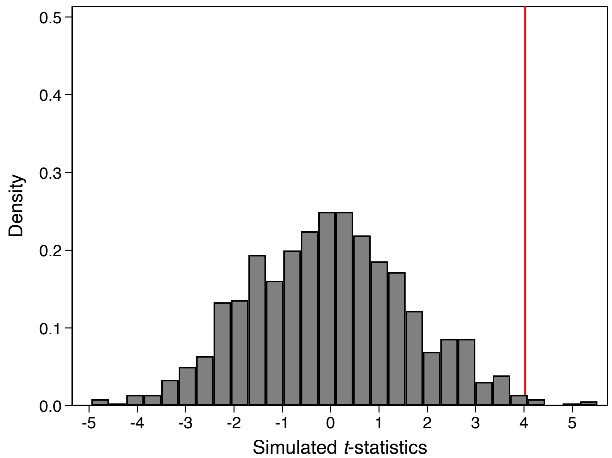
(b) Indirect exposure



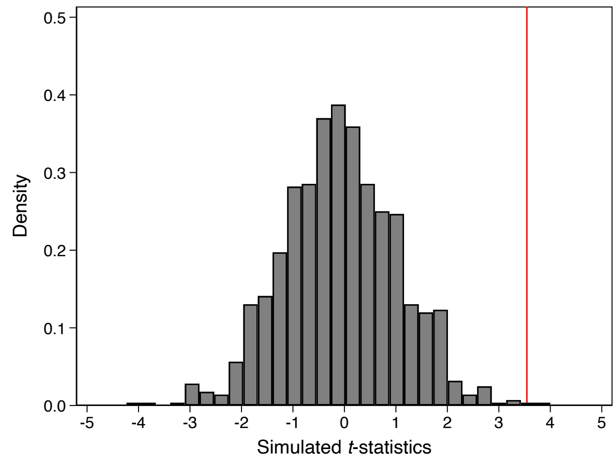
Note: Robustness: Panel A: Distribution of 1000 simulated t-statistics from estimates of direct exposure (share of households within 200m of Nazi march). Panel B: Distribution of 1000 simulated t-statistics from estimates of indirect exposure from estimating equation (7). Dependent variable is random NSDAP vote share (see main text for construction procedure). The red vertical line indicates the t-statistic obtained from estimating direct (Panel A) and indirect exposure (Panel B) on the actual data. See main text and appendix for construction of indirect exposure measure.

Figure A.9: Spatially correlated random NSDAP vote share.

(a) 0.005 degrees correlation range: direct exposure



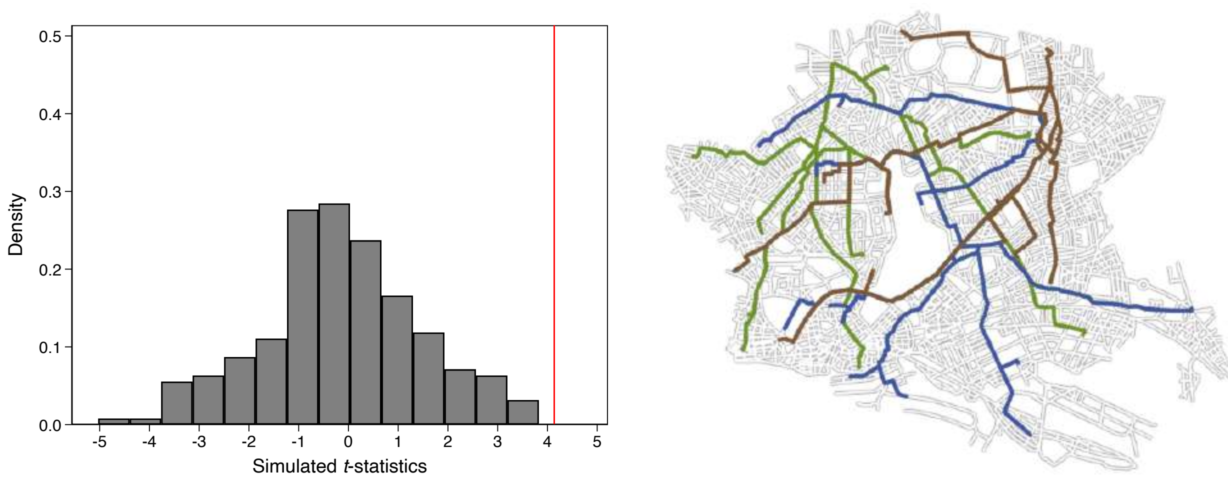
(b) 0.005 degrees correlation range: indirect exposure



Note: Robustness: Panel A: Distribution of 1000 simulated t-statistics from estimates of direct exposure (share of households within 200m of Nazi march). Panel B: Distribution of 1000 simulated t-statistics from estimates of indirect exposure from estimating equation (7). Dependent variable is spatially correlated random NSDAP vote share (see main text for construction procedure). Panels A and B use 0.005 degrees as correlation range in generating spatially correlated data. The red vertical line indicates the t-statistic obtained from estimating direct (Panel A) and indirect exposure (Panel B) on the actual data. See main text and appendix for construction of indirect exposure measure.

Figure A.10: Random marches.

(a) Direct exposure (b) Random march examples



Note: Robustness: Panel A: Distribution of 200 simulated t-statistics from estimates of direct exposure (share of households within 200m of a random march) from estimating equation (1). Dependent variable is NSDAP vote share. See main text for procedure of generating random marches. Panel B plots three examples of random marches obtained by this procedure over the street network of Hamburg.

Table A.1: Hamburger Anzeiger major topics (10 Apr - 24 Apr 1932).

(a) National news

	Frequency	%
Geneva conference	9	31.03
Manchuria conflict	4	13.79
Prussian election	4	13.79
SA prohibition	4	13.79
Presidential election results	3	10.34
Police action against communists	2	6.90
Reichsbanner prohibition	1	3.45
other	2	6.90

(b) Hamburg local news

	Frequency	%
Burgerschaftswahl election	4	28.57
Murdered SA men on election day	4	28.57
Gas cloud	2	14.29
Eiserne Front rally	1	7.14
NSDAP rally	1	7.14
other	2	14.29

Note: We collected the *Hamburger Anzeiger* (Hamburger Anzeiger, 1932) articles for the time between 10 April 1932 and 24 April 1932 (last pre-treatment to first post-treatment election) to identify potentially distorting events. We focussed on the main articles (national news and Hamburg specific news - excluding sports and business sections) and hand-coded the topics.

Table A.2: Panel A. Pre-trends indirect effect: 13 March-10 April 1932.

	Change 13 March - 10 April 1932		
	(1)	(2)	(3)
	Δ NSDAP	Δ KPD	Δ turnout
Indirect exposure of households	-0.022	-0.064	-0.095
	[0.108]	[0.069]	[0.078]
Constant	-4.145	3.496	3.412
	[4.370]	[4.259]	[3.509]
Demographic controls	Yes	Yes	Yes
Street controls	Yes	Yes	Yes
R^2	0.551	0.512	0.148
Mean change in Y	6.157	-2.793	-4.757
Observations	622	622	622

Table A.2: Panel B. Contemporaneous events.

	Within 500m from:			log distance to
	(1)	(2)	(3)	(4)
	SA murder	Nazi rally	EF rally	Hitler rally
Indirect exposure of households	-0.001	-0.001	0.001	-0.022
	[0.003]	[0.002]	[0.001]	[0.018]
Constant	0.006	0.006	0.002	2.192
	[0.003]	[0.003]	[0.002]	[0.018]
R^2	0.000	0.000	0.000	0.002
Mean dependent variable	0.006	0.006	0.002	2.192
Observations	622	622	622	622

Note: Panel A: Immediate pre-trends (first presidential election round to second presidential election) of election outcomes (Sköllin, 1932a). Estimates of equation (3) with vote share (col. 1); KPD vote share (col. 2); voter turnout (col. 3) as dependent variable and indirect exposure as exposure measure. In all regressions we control for demographic and street network controls. Panel B: Contemporaneous events and indirect exposure. OLS estimates of regressing: indicator whether a polling station is located within 500m of location of murder of SA men (col. 1); Nazi rally (col. 2); Eiserne Front rally (col. 3); log of distance to Hitler rally (col. 4) on indirect exposure as exposure measure. Contemporaneous events are coded and geolocated from newspaper articles between 10 April 1932 and 24 April 1932 spanning the time between the last pre-treatment and first post-treatment election (Hamburger Anzeiger, 1932). See main text and appendix for construction of indirect exposure measure. Standard errors clustered at polling station level in brackets.

Table A.3: Dynamic effects as function of days since exposure.

	% NSDAP votes					
	(1)	(2)	(3)	(4)	(5)	(6)
Indirect exposure of households × post march	0.297		0.278	0.213		0.195
	[0.089]		[0.089]	[0.106]		[0.107]
Share households directly exposed × post march		0.833	0.809		0.783	0.767
		[0.173]	[0.172]		[0.205]	[0.205]
Indirect exposure of households × post march × log days since t3	0.044		0.044			
	[0.025]		[0.026]			
Share households directly exposed × post march × log days since t3		0.029	0.025			
		[0.046]	[0.046]			
Indirect exposure of households × post march × log days since march				0.060		0.059
				[0.035]		[0.035]
Share households directly exposed × post march × log days since march					0.039	0.033
					[0.062]	[0.062]
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	0.904	0.904	0.905	0.904	0.904	0.905
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417	30.417
Observations	3732	3732	3732	3732	3732	3732
Dynamic persuasion hypothesis tests						
Indirect exposure dynamic hypothesis: p-value	0.041		0.044	0.041		0.044
Direct exposure dynamic hypothesis: p-value		0.259	0.291		0.265	0.297

Note: Estimates of estimating $NSDAP_{it} = \alpha_i + \alpha_t + \beta_1 M_i \times Post_t + \beta_2 M_i \times Post_t \times T_t + \gamma_1 S_i \times Post_t + \gamma_2 S_i \times Post_t \times T_t + \sigma T_t \times Post_t + \sum_t \delta_t X_i + u_{it}$ with S_i = indirect exposure to the march only (col. 1 and 4); M_i = share of households within 200m of Nazi march (col. 2 and 5) and both, indirect and direct exposure to the march (col. 3 and 6) as measures of exposure. T_t captures log of days since first post-treatment election (24 April 1932) in col. 1 to 3 and log of days since march (20 April 1932) in col. 4 to 6. Dependent variable is the share of NSDAP votes. Additionally, we test the *dynamic persuasion hypothesis*: the effect is growing over time, i.e. $\beta_2 > 0$ for direct exposure and $\gamma_2 > 0$ for indirect exposure. 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 indirect exposure measure. 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 % $NSDAP_t$ - % $NSDAP_2$ (D3)	Effect of 1 sd (D4: D1×D2/D3)
t=3	0.82	0.36	1.9	16.07%
t=4	0.98	0.36	2.8	12.56%
t=5	0.96	0.36	3.7	9.39%
t=6	0.95	0.36	3.6	9.48%

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

	γ (I1)	1 sd $\rho^T - \rho^C$ (I2)	1 sd % $NSDAP_t$ - % $NSDAP_2$ (I3)	Effect of 1 sd (I4: I1×I2/I3)
t=3	0.31	1.00	1.9	16.51%
t=4	0.43	1.00	2.8	15.09%
t=5	0.58	1.00	3.7	15.60%
t=6	0.56	1.00	3.6	15.26%

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

	Total (D4+I4)	Direct (D4/Total)	Indirect (I4/Total)
t=3	32.57%	49%	51%
t=4	27.65%	45%	55%
t=5	24.99%	38%	62%
t=6	24.77%	38%	62%

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 (8). (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 (t3-t6) and the last pre-treatment election (t2). (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 indirect exposure estimating equation (8). (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 (t3-t6) and the last pre-treatment election (t2). (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 indirect exposure measure.

Table A.5: Robustness. Standard errors corrected for spatial autocorrelation.

	% NSDAP vote		
	(1)	(2)	(3)
Share households directly exposed × post march	0.949		0.908
Baseline: s.e. clustered at polling station level	[0.229]		[0.226]
Conley (1999) s.e.: cutoff at 200m	[0.232]		[0.228]
Conley (1999) s.e.: cutoff at 500m	[0.246]		[0.244]
Conley (1999) s.e.: cutoff at 1km	[0.268]		[0.267]
Conley (1999) s.e.: cutoff at 1.5km	[0.279]		[0.280]
Conley (1999) s.e.: cutoff at 2km	[0.288]		[0.291]
Conley (1999) s.e.: cutoff at 2.5km	[0.298]		[0.301]
Conley (1999) s.e.: cutoff at 3km	[0.303]		[0.305]
Indirect exposure of households × post march		0.470	0.449
Baseline: s.e. clustered at polling station level		[0.126]	[0.125]
Conley (1999) s.e.: cutoff at 200m		[0.124]	[0.124]
Conley (1999) s.e.: cutoff at 500m		[0.127]	[0.126]
Conley (1999) s.e.: cutoff at 1km		[0.131]	[0.129]
Conley (1999) s.e.: cutoff at 1.5km		[0.135]	[0.134]
Conley (1999) s.e.: cutoff at 2km		[0.140]	[0.138]
Conley (1999) s.e.: cutoff at 2.5km		[0.141]	[0.139]
Conley (1999) s.e.: cutoff at 3km		[0.140]	[0.139]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
Observations	3732	3732	3732

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 (7) with share of households within 200m of Nazi march as exposure. Col. 2: Estimates of equation (7) with indirect exposure to the march as exposure. Col. 3: Estimates of equation (7) with both, share of households within 200m of Nazi march and indirect exposure to the march as exposure measures. See main text and appendix for construction of indirect exposure measure.

Table A.6: Panel A. Random NSDAP votes.

	Direct exposure		Indirect exposure	
	Frequency	%	Frequency	%
p >=5%	950	95.00	939	93.90
p <5%	50	5.00	61	6.10
outperforms	0	0	0	0

Table A.6: Panel B. Spatially correlated random NSDAP votes.

	Direct exposure		Indirect exposure	
	Frequency	%	Frequency	%
(i): 0.002 degrees correlation range				
p >=5%	892	89.20	952	95.20
p <5%	108	10.80	48	4.80
outperforms	0	0	1	0.10
(ii) 0.005 degrees correlation range				
p >=5%	750	75.00	931	93.10
p <5%	250	25.00	69	6.90
outperforms	14	1.40	3	0.30
(iii): 0.01 degrees correlation range				
p >=5%	622	62.20	880	88.00
p <5%	378	37.80	120	12.00
outperforms	67	6.70	6	0.60

Table A.6: Panel C. Random Nazi marches.

	Direct exposure	
	Frequency	%
p >=5%	153	76.50
p <5%	47	23.50
outperforms	1	0.50

Note: Robustness: Randomization inference. Panel A: Result of 1000 simulations with random NSDPA vote share as dependent variable. Rows 1 and 2 report the frequency of estimates with a p-value above (row 1) or below 5% (row 2). Row 3 reports the frequency of outperformance (absolute value of t-statistic of simulation larger than t-statistic obtained from actual data). Panel B: Result of 1000 simulations with spatially correlated random NSDPA vote share as dependent variable. Panel (i) uses 0.002 degrees, panel (ii) 0.005 degrees and panel (iii) 0.01 degrees as correlation range in generating spatially correlated data. Rows 1 and 2 report the frequency of estimates with a p-value above (row 1) or below 5% (row 2). Row 3 reports the frequency of outperformance (absolute value of t-statistic of simulation larger than t-statistic obtained from actual data). Panel C: Result of 200 simulations with random marches as treatment (share of households within 200m of random march). Rows 1 and 2 report the frequency of estimates with a p-value above (row 1) or below 5% (row 2). Row 3 reports the frequency of outperformance (absolute value of t-statistic of simulation larger than t-statistic obtained from actual data).

Table A.7: Robustness. Results excluding polling stations that changed address.

	% NSDAP votes		
	(1)	(2)	(3)
Share households directly exposed × t6 (post)	0.898		0.826
	[0.407]		[0.405]
Share households directly exposed × t5 (post)	0.875		0.802
	[0.371]		[0.367]
Share households directly exposed × t4 (post)	0.974		0.923
	[0.310]		[0.307]
Share households directly exposed × t3 (post)	0.795		0.757
	[0.213]		[0.211]
Share households directly exposed × t1 (pre)	0.022		0.023
	[0.236]		[0.237]
Indirect exposure of households × t6 (post)		0.549	0.520
		[0.218]	[0.218]
Indirect exposure of households × t5 (post)		0.553	0.526
		[0.217]	[0.217]
Indirect exposure of households × t4 (post)		0.409	0.378
		[0.165]	[0.164]
Indirect exposure of households × t3 (post)		0.314	0.288
		[0.111]	[0.110]
Indirect exposure of households × t1 (pre)		-0.011	-0.013
		[0.115]	[0.115]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
R^2	0.904	0.904	0.905
Mean NSDAP vote in 10 Apr '32 election	30.611	30.611	30.611
Observations	3360	3360	3360
Dynamic persuasion hypothesis tests			
Direct effect t6 = t3: p-value	0.750		0.833
Direct effect t5 = t3: p-value	0.785		0.878
Direct effect t4 = t3: p-value	0.441		0.476
Indirect effect t6 > t3: p-value		0.080	0.085
Indirect effect t5 > t3: p-value		0.076	0.080
Indirect effect t4 > t3: p-value		0.215	0.230

Note: Robustness: Regressions on sub sample of polling stations with stable addresses. 62 polling stations change their address at least once between the different elections. See statistical bulletin of Hamburg (Sköllin, 1932a, 1932b, 1932c, 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 col. 2 and 3, we test the *dynamic persuasion hypothesis* for indirect exposure, i.e. $\gamma_s > \gamma_3$ for $s = 4, 5, 6$ (one-sided test against H0: Diffusion is not growing over time). In col. 1 and 3, we test whether the effect of direct exposure is stable over time, i.e. $\beta_3 = \beta_s$ for $s = 4, 5, 6$ (two-sided test against H0: Diffusion is time-invariant). 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.8: Nearest neighbor match: first difference results.

	Δ % NSDAP vote (before-after)					
	(1)	(2)	(3)	(4)	(5)	(6)
SATT	0.441	0.350	0.544	0.570	0.548	0.569
	[0.250]	[0.224]	[0.199]	[0.200]	[0.197]	[0.202]
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). 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]	[0.018]	[0.026]
Share households with telephone	11.69	14.41	14.41	14.41
	[149.4]	[159.7]	[175.6]	[159.7]
Share households with heating	5.976	6.178	6.179	6.178
	[152.2]	[145.6]	[134.2]	[145.6]
Share households who are blue-collar	36.41	32.94	32.94	32.94
	[218.4]	[168]	[213.8]	[168]
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]	[1.587]	[1.256]
Number of streets	4.579	4.606	4.605	4.606
	[4.111]	[2.889]	[4.106]	[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]	[0.053]	[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	0.877	0.858	1.066
	[0.212]	[0.205]	[0.224]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
R^2	0.904	0.911	0.904
Mean dep. var.:	30.417	30.417	30.834
Observations	3732	3732	2820

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 (5 categories), share of households with telephone (5 categories), share of households with heating (above 0 / 0) and share of blue collar workers (5 categories). Standard errors clustered at polling station level in brackets.

Table A.10: Cutoff sensitivity: first difference results.

	% NSDAP votes		
	(1)	(2)	(3)
Panel (a): >90% cutoff			
Share households directly exposed × post march	0.949		0.905
	[0.229]		[0.226]
Indirect exposure of households: >90% cutoff × post march		0.447	0.424
		[0.124]	[0.123]
Panel (b): >80% cutoff			
Share households directly exposed × post march	0.949		0.908
	[0.229]		[0.226]
Indirect exposure of households: >90% cutoff × post march		0.466	0.446
		[0.126]	[0.126]
Panel (c): >80% cutoff			
Share households directly exposed × post march	0.949		0.905
	[0.229]		[0.226]
Indirect exposure of households: >70% cutoff × post march		0.432	0.408
		[0.125]	[0.125]
Panel (d): >60% cutoff			
Share households directly exposed × post march	0.949		0.914
	[0.229]		[0.227]
Indirect exposure of households: >60% cutoff × post march		0.405	0.384
		[0.121]	[0.121]
Panel (e): >50% cutoff			
Share households directly exposed × post march	0.949		0.913
	[0.229]		[0.228]
Indirect exposure of households: >50% cutoff × post march		0.372	0.349
		[0.125]	[0.125]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417
Observations	3732	3732	3732

Note: Robustness: Changing treatment cutoffs. Each panel report results with a different direct treatment cutoff, from 90% (panel a) to 50% (panel e) of households within 200m of march as cutoff point. Dependent variable is the share of 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 indirect exposure to the march as measure of exposure. Col. 3: Estimates of equation (7) with both, share of households within 200m of Nazi march and 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 indirect exposure measure. Standard errors clustered at polling station level in brackets.

Table A.11: Other marches: Panel A. Direct effect.

	% NSDAP votes					
	(1)	(2)	(3)	(4)	(5)	(6)
Share households directly exposed × post march	1.253	1.010	1.246	0.988	1.309	1.042
	[0.245]	[0.228]	[0.247]	[0.228]	[0.245]	[0.227]
Share households directly exposed to KPD march × post march	-0.452	-0.283			-0.431	-0.268
	[0.194]	[0.187]			[0.193]	[0.187]
Share households directly exposed to SPD march × post march			-0.338	-0.233	-0.298	-0.209
			[0.226]	[0.210]	[0.222]	[0.208]
Election & polling station FEs	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls × election FEs	No	Yes	No	Yes	No	Yes
Street controls × election FEs	No	Yes	No	Yes	No	Yes
R^2	0.852	0.904	0.852	0.904	0.852	0.904
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417	30.417
Observations	3732	3732	3732	3732	3732	3732

Table A.11: Other marches: Panel B. Indirect effect.

	% NSDAP votes					
	(1)	(2)	(3)	(4)	(5)	(6)
Share households directly exposed × post march	0.870	0.960	0.901	0.943	0.922	0.989
	[0.234]	[0.225]	[0.232]	[0.225]	[0.233]	[0.224]
Share households directly exposed to KPD march × post march	-0.133	-0.237			-0.113	-0.223
	[0.187]	[0.187]			[0.187]	[0.187]
Share households directly exposed to SPD march × post march			-0.284	-0.206	-0.274	-0.186
			[0.219]	[0.208]	[0.219]	[0.207]
Indirect exposure of households × post march	0.820	0.438	0.828	0.444	0.819	0.435
	[0.087]	[0.126]	[0.087]	[0.126]	[0.087]	[0.126]
Election & polling station FEs	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls × election FEs	No	Yes	No	Yes	No	Yes
Street controls × election FEs	No	Yes	No	Yes	No	Yes
R^2	0.858	0.905	0.858	0.905	0.858	0.905
Mean NSDAP vote in 10 Apr '32 election	30.417	30.417	30.417	30.417	30.417	30.417
Observations	3732	3732	3732	3732	3732	3732

Note: Robustness: Panel A: Estimates of equation (1) with share of households within 200m of Nazi march as measure of exposure. Dependent variable is the share of NSDAP votes. In all specifications we control for polling station and election fixed effects. Col. 2, 4 and 6 additionally include street and demographic characteristics interacted with election fixed effects. Col. 1 and 2 additionally control for exposure to KPD march (share of households within 200m of KPD march × post KPD march). Col. 3 and 4 additionally control for exposure to SPD march (share of households within 200m of SPD march × post SPD march). Col. 5 and 6 use both, exposure to KPD and SPD march as additional regressors. Panel B: Estimates of equation (7) with share of households within 200m of Nazi march as measure of direct exposure and indirect exposure as exposure variables. Dependent variable is the share of NSDAP votes. In all specifications we control for polling station and election fixed effects. Col. 2, 4 and 6 additionally include street and demographic characteristics interacted with election fixed effects. Col. 1 and 2 additionally control for exposure to KPD march (share of households within 200m of KPD march × post KPD march). Col. 3 and 4 additionally control for exposure to SPD march (share of households within 200m of SPD march × post SPD march). Col. 5 and 6 use both, exposure to KPD and SPD march as additional regressors. See main text and appendix for construction of indirect exposure measure. Standard errors clustered at polling station level in brackets.

Table A.12: Direct and indirect effect: turnout.

	turnout		
	(1)	(2)	(3)
Share households directly exposed × t6 (post)	-0.035		-0.040
	[0.228]		[0.228]
Share households directly exposed × t5 (post)	-0.737		-0.732
	[0.239]		[0.240]
Share households directly exposed × t4 (post)	-0.195		-0.227
	[0.300]		[0.300]
Share households directly exposed × t3 (post)	-0.030		-0.013
	[0.164]		[0.165]
Share households directly exposed × t1 (pre)	-0.006		-0.008
	[0.162]		[0.163]
Indirect exposure of households × t6 (post)		0.050	0.051
		[0.112]	[0.112]
Indirect exposure of households × t5 (post)		-0.069	-0.051
		[0.120]	[0.120]
Indirect exposure of households × t4 (post)		0.341	0.347
		[0.151]	[0.151]
Indirect exposure of households × t3 (post)		-0.202	-0.201
		[0.090]	[0.090]
Indirect exposure of households × t1 (pre)		0.024	0.025
		[0.080]	[0.081]
Election & polling station FEs	Yes	Yes	Yes
Demographic controls × election FEs	Yes	Yes	Yes
Street controls × election FEs	Yes	Yes	Yes
R^2	0.884	0.884	0.885
Mean dep. var.	83.670	83.670	83.670
Observations	3732	3732	3732

Note: Robustness: Dependent variable is turnout. Col. 1: Estimates of equation (2) with share of households within 200m of Nazi march as exposure. Col. 2: Estimates of equation (2) with indirect exposure to the march as measure of exposure. Col. 3: Estimates of equation (8) with both, share of households within 200m of Nazi march and 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 indirect exposure measure. Standard errors clustered at polling station level in brackets.