# Contagious Extremism: Nazi Marches and Radical Voting*

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**Abstract**

How do extremist movements go 'viral'? To answer this question, we examine the rise of the Nazi Party in Hamburg in 1932. The July 1932 parliamentary election saw the Nazi Party's biggest triumph before coming to power. In the preceding period, the Nazi party staged massive marches. We examine how these public shows of strength created growing support for the Nazi movement. Areas close to the marching route saw much larger electoral gains. Wide streets were favored because they made marching columns look more impressive. We exploit this fact in our IV strategy, and find large effects. In addition, social network linkages across neighborhoods facilitated the spread of extremism throughout the city. The new data on the characteristics and location of more than 400,000 households show that, even in areas far from the marching routes, gains for the Nazi party were more sizable where numerous inhabitants had connections with others who witnessed the march.

**Keywords:** Right-Wing Extremism, Rallies, Marches, Social Networks, Diffusion

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

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

# 1 Introduction

Extremist groups have become a frequent sight on the streets of Western countries: From the "Unite the Right" rally in Charlottesville to the infamous 'Pegida' demonstrations in East Germany[2] and St Kilda Beach protest in Australia, far-right, neo-fascist, and anti-immigrant groups have been on the march in almost every modern democracy. Groups opposed to the established order are typically behind extremist protests, rallies, and demonstrations, militating in favor of radical changes to existing policy. What is the effect of such public gatherings? Do they create more support – or do moderate parts of society turn away in disgust?

In this paper, we test whether public shows of force by political extremists have a direct effect on support, and whether they can create knock-on effects through social interactions. The logic is as follows: Some extreme group takes to the streets, displaying its strength. This action, in turn, triggers some bystanders to join the cause – activating supporters, or persuading moderates that radical politics is what society needs. Once people have been radicalized, they proselytize for their new cause among friends, neighbors, and co-workers (Satyanath et al., 2017). In this manner, extremism can become contagious, spreading through society like a virus. To our knowledge, there is no causal evidence that public displays of radical political preferences – whether through marches, demonstrations or rallies – can give rise to such domino effects within social networks, and how much they may matter quantitatively.

We study the effects of Nazi Party loyalists marching through the streets of Hamburg in 1932 – a pivotal year for Hitler's rise to power.[3] Marches of tightly-packed formations of stormtroopers, dressed in brown military-style uniforms, were an integral part of the Nazi Party's propaganda. We study the effect of these marches on electoral outcomes in three national elections in March, April and July 1932, and one municipal election in April. Marches were banned before the first two rounds of elections, and were only allowed during a brief period in April. Because there are numerous elections in our period, we can pinpoint short-term changes in Nazi Party support.[4]

To analyze the effects of marches, we first use Hamburg police records and local newspaper reports to reconstruct the paths of the two marches that took place in mid-April 1932. We collect detailed data on more than 400,000 Hamburg households from the 1932 address book, and merge it with electoral results using highly granular voting data for 600 polling districts. The main outcome variable is the vote share for the Nazi Party (NSDAP) in a neighborhood, which we measure four

[1] Quoted according to Noakes (1971).
[2] Pegida demonstrations gathered up to 25,000 supporters opposed to the 'Islamisation of the Occident'.
[3] Hamburg was also one of the key battlegrounds for the Nazi Party. Its electoral fortunes closely resemble those of the Reich as a whole (Figure A.1).
[4] Figure A.2 in the Appendix shows the timeline of key events.

times, including only a few days before and after the marches took place. Conceptually we distinguish between two types of treatments: *direct* exposure to the marches by witnessing them, and *indirect* exposure by being socially connected to those that were directly exposed – *contagion.*

We first examine the effects of direct exposure. Neighborhoods swung more towards the Nazi Party where households lived in close proximity to marching routes. Figure 1 illustrates the basic pattern. Electoral districts closest to the march witnessed a gain of more than 6%; those furthest away, of no more than 4%.

Streets where Nazi activists and storm troopers marched were not chosen at random – areas or streets were targeted in order to maximize impact. This is the key identification challenge for our analysis. Using a difference-in-differences strategy, we first show that the basic relationship between proximity to the marches and voting for the Nazi Party displayed in Figure 1 is not driven by a pre-trend in support prior to the marches. It is also robust to a large set of socio-economic controls. Importantly, the high frequency of our data allows us to rule out contemporaneous shocks driving our results: polling stations close to the marches swing towards the Nazi after only 4 days from these events. The effect persists in the next election several months later.

The parallel trends assumption underpinning the differences-in-differences strategy is ultimately untestable. Marching paths and rally points may have been chosen to maximize their electoral impact. It is possible that the marching paths traversed more neighborhoods that would have swung in favor of the Nazi Party anyway, even without the marches. In this case, the parallel trends assumption would fail and the differences-in-differences estimates would not reflect causal effects.

To sidestep the problem, we employ an IV strategy. This exploits the propaganda logic of Nazi marches. For each march, there were multiple assembly points outside the city center. Nazi supporters would then march along a pre-specified path, congregating at a large public square in the city center, where mass rallies were held in front of large crowds. Marches then continued to end points outside the city center. Nazi planners apparently preferred marches to pass through *wide streets*. The wider a street was, the more impressive the spectacle of large groups of party members marching in lockstep. We use this implicit objective to extract plausibly random variation from the routes of the Nazi marches. We first calculate paths based on the shortest distance through the city street network, connecting starting points, rallying points and end points. As a second step, we calculate optimal paths that give priority to wide streets. The IV-strategy is based on the *difference* in the paths created by giving greater weight to wider streets, *conditional* on being close to a wide street. Thus, we effectively compare two neighborhoods with similar street widths – but only one of them located along the Nazi march because somewhere else in Hamburg's street network, the choice of wider streets forced the march to pass through it. Since we control for street characteristics of streets in each neighborhood, the identifying variation of paths is driven by *far-away* streets, and is arguably as good as random. Consistent with this argument, we show that the instrument is uncorrelated with pre-existing levels and trends of Nazi Party support.

Street width along the marching route overall is a good predictor for where the marchers actually went. Crucially, the instrumented part of the variation in marching routes has a large effect on electoral outcomes: being within 200m of the marching route caused a swing in favor of the Nazis by 5.4 percentage points (p.p.). We interpret this as strong evidence of the marches' radicalizing effect. This is the first key contribution of our paper.

We then turn to the second key contribution: evidence of social *contagion*. We are interested in how much word-of-mouth effects created further support for the Nazi Party. Ideally, we would want to know how much 'treated' households interacted with those that did not witness the marches themselves. Unfortunately, actual social interaction frequencies between individuals (or neighborhoods) are not observable. To overcome this challenge, we build on the concept of *homophily*, which is generally associated with greater interaction frequency.[5] Using the 1932 address book of Hamburg, we collect a range of key household characteristics relating to occupation, social standing, schooling, and regional origin for close to the universe of households in Hamburg (400'000 households). We use this fact to create measures of social similarity across neighborhoods.

The results show that social connections between areas treated by the march, and those further away, influenced electoral outcomes: The tighter the connection with areas traversed by a Nazi march, the greater the increase in voting for the German fascists. This effect is also large: A one standard deviation decrease in our measure of social distance predicts an additional 1.2 p.p. change in Nazi support. Because we measure social connections with noise, the latter is also likely to be a downward-biased estimate of the true indirect effect. Overall, our evidence highlights an important mechanism for how extremism can spread in society.[6]

Why did street marches lead Hamburg voters to support the Nazis? Information revelation could be one potential reason (Lohmann, 1993; García-Jimeno et al. 2018). At a time when many German middle-class voters longed for a return to the order and stability of the Empire, the well-organized, disciplined marches of the Nazi Party arguably conveyed information about its organizational capacity (Evans, 2006). Personal experience, however, went beyond mere information – the fact that the Nazis staged numerous, well-organized rallies, meetings, and marches was widely known. Instead, two other channels may be at work. Marches may have given people a reason to discuss the Nazi Party and their views of it. As the party's support surged at the polls, its core message went from a heavily stigmatized minority view to a widely accepted political preference. Marches may have become topics of everyday conversation, thus reducing 'pluralistic ignorance'– meaning that voters were likely to reveal their private, supportive view once they got talking (Bursztyn et al., 2017; Zuckerman, 2005). This may have combined with the marches to create a 'bandwagon' effect, with additional people voting for the party that was gaining more support (Kuran, 1989). The second possible channel could be based on the special emotional effect of participating in mass gatherings (Durkheim, 1915) – for which there is some evidence in the Nazi case, which we discuss separately below.

Historically, protests and marches – by both extremists or moderate groups – often coincide with shifts in public opinion and voting behavior. However, there is only limited causal evidence on the effect of marches. Madestam et al. (2013) analyze the conservative 'Tea Party' movement in the US, and show that the Republican Party gained votes and public opinion shifted to the right after mass rallies. Similarly, Mazumder (2018) shows that protests by the U.S. civil rights movement created additional support for the Democratic Party. While both studies have an instrumentation strategy,

[5] Aral et al. (2009).

[6] There are various theoretical network models of why and how contagion/diffusion may occur, such as the Bass Model, Percolation Theory, the SIR model and the SIS model. We do not attempt to separate between various models of contagion. Instead, the goal is to establish whether contagion exists in our political context, in a manner consistent with a broad set of models (Jackson, 2008).

neither provides evidence of contagion, i.e. of social ties affecting voting patterns after a march. In other contexts, however, there is burgeoning evidence that social networks matter. Cantoni et al. (2019) study protests by the Hong Kong Democracy Movement. They conduct experiments and argue that the very act of protesting is a strategic substitute among peers (students).[7] Our study is one of the first to provide direct causal evidence of extremist voting spreading as a result of protests, marches or rallies. It is also the first to show how network effects amplify this mechanism.[8]

The rest of the paper is organized as follows. Section II summarizes the historical background. Section III introduces our data, and Section IV, our empirical results. Section V discusses robustness, and Section VI concludes.

# 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 after armed police intervened; 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 negotiated by the German and foreign governments, which would have stretched out payments to compensate the victors of WW I until the 1980s. In September 1930s, the Nazis received 16% of the vote, making it the 2nd largest party (Evans, 2006).

As the Great Depression worsened, support for liberal democracy declined. 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. 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 & 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 parliamentary

[7] Other examples of unrest and protests spreading through networks include González (2017), Aidt et al. (2017), González-Bailon et al. (2011), and Enikolopov et al. (2019). Other related work on the effect of media on unrest includes Acemoglu et al. (2018) and Manacorda and Tesei (2020).

election in July 1932, the Nazis scored their best result in a fully free election, polling 37.2% of the vote. In combination, Nazis and Communists had an absolute majority of votes and seats, making it impossible for the remaining democratic parties to form a government. Hitler was confident of becoming Chancellor. However, the aging president's distaste for the 'little corporal' prevented a Nazi government in the summer of 1932 (Bracher, 1978).

Since no coalition with a parliamentary majority could be formed, the country had to go to the polls again. In November 1932, votes for the Nazi Party declined, and a government was formed without them. By December, many observers were confident that they would never come to power (Turner, 1997). However, in January 1933, conservative advisors convinced the elderly president to give Hitler a chance – but in a cabinet dominated by ministers from other right-wing parties. As soon as the Nazis were in office, they outmanoeuvred their coalition partners and seized power. Ruthlessly using control of the police to this end, the Hitler government held one more parliamentary election – in March 1933 – and then set out to dismantle German democracy. Millions of Germans applied to become members of the Nazi Party. 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

The Nazi party's electoral fortunes in Hamburg mirrored those at the national level. It 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 failed. 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 and the veterans organization *Stahlhelm* (literally, steel helmet). Party propaganda turned down the earlier emphasis on socialist ideals.

Under Kaufmann's aegis, the Nazi Party in Hamburg also made common cause 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%.[9] While the Hamburg trend paralleled that in Germany overall, its showing was particularly impressive for "Red Hamburg", where the Communists and Social Democrats had traditionally received half of the votes.

## 2.3 Nazi propaganda and marches

The Nazi party in Hamburg used a wide range of propaganda instruments, as it did in the rest of the country. From leaflets and door-to-door visits to speeches by regional and national party leaders, the Nazi party organized tens of thousands of events across the country in the run-up to every election – 34,000 meetings before the presidential election in 1932 alone (Kershaw, 2001). Its local chapters were in charge of recruiting and day-to-day propaganda, using local speakers. These were trained via correspondence courses organized by the national propaganda organization (O Broin, 2016). Regular meetings and speeches by local members attracted and engaged party members and potential recruits in normal times. Election time saw a massive rise in propaganda activity. Hitler himself gave dozens of speeches before each election (Selb and Munzert, 2018). In Hamburg, for example, he spoke to a crowd of 120,000 people on April 23, immediately before the municipal elections (Sandner, 2017).

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. This propaganda weapon, which up till now was used almost solely by the Marxists, must in the future be made use of in a completely different way. 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."*
>
> *(Noakes, 1971)*

Casual empiricism thus laid the foundations for what became a hallmark tool of Nazi propaganda– during the late Weimar period, but also 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, but newspaper accounts on the days following the marches testify that many more witnessed the event from sidewalks and windows (Hamburger Nachrichten, 1932a; Hamburger Tageblatt, 1932). The second march was held past dusk and made a particularly strong impression (Hamburger Nachrichten, 1932b). The city police

[9] Figure A1 in the appendix plots electoral results in Hamburg and nation-wide side-by-side.

department had banned the storm troopers (SA) from marching in their uniforms, but the event itself had a strongly militaristic overtone regardless. 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. They form up on the Moorweide, column by endless column. And then they march, march so that red Hamburg has to take notice…"*[10]

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. As Richard Evans (2006) has argued:

> *"...mass demonstrations and marches in the streets drove out rational discourse and verbal argument 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 of the brownshirts, the stiff salutes and military poses of the Nazi leaders 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 the 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. 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 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).

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[10] Hamburger Tageblatt April 18, 1932.

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

In this section, we describe our data. We use four main sources: polling-station level data on voting behavior, SA Hamburg documents and newspapers for the path of marches, the address book of Hamburg to geo-locate households and capture their main characteristics, and a digitization of Hamburg's road network. We complement these data with a fifth source: the 1931-32 Hamburg city book.

## 3.1 Nazi marches in Hamburg

Nazis marches on April 17 and 20 started at several locations all 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 SA Hamburg sent detailed plans of both marches to the police and other local SA groups stating the exact routes to take (State Archive Hamburg, 1932a; 1932b). 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). The two panels (a) and (b) in Figure 2 show the routes of the two marches separately; panel (c) shows the two marches pooled.

## 3.2 Household-level data

Every German city in the interwar period published an address book of all households annually, and Hamburg was no different (Hamburger Adreßbuch, 1932). In the days before everyone had a telephone, it provided detail on who lived where, as well as useful information on the location of administrative offices, school districts, opening hours, and the like. We digitize the entries for every household located in any one of inner Hamburg's 17 districts. The 400,000 digitized entries represent the full population of households in the inner city of Hamburg. For each household, we know whether they owned a telephone, had heating at home (luxury items at the time), and in the majority of cases, the occupation of the tenant and his surname. We use the occupations stated in the address book to 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 regional distribution of surnames in the German telephone book of 2015 (Das Telefonbuch Deutschland, 2015).[11]

Each household at the time was assigned to a polling station. Hamburg in 1932 had 756 polling stations in total, 622 of which were located in the city itself (meaning in one of the 17 boroughs of the inner city of Hamburg).[12] We assign each household to its polling station using the official voting lists (see Voting Data, below). Figure 3 shows a map of Hamburg with all the household locations in our data, together with the polling stations.

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 2 marches for each household, and calculate the average distance of households of every polling station. Second, for every polling stations 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 4 shows a map of households that were directly exposed according to our definition. This is not to say that households that lived further away than 200m did not observe the march; but it is likely that the probability declined with distance, potentially in a nonlinear fashion.

## 3.3 Voting data

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 municipal election (*Bürgerschaftswahl*: 24 April: Sköllin, 1932a) and for the July *Reichstag* election (31 July 1932: Sköllin, 1932b). We geo-locate each of these polling stations using their exact address. Less than 1% (3%) of polling stations changed their location between March and April (April and July), and never by more than 500 meters. We always use the March 1932 address for our calculations, and in section V.B show that all our results are unaffected when we drop the 17 polling stations that changed location. To our knowledge, voters were not re-assigned to different polling stations as a result of these changes. The average polling station saw around 1,000 valid votes cast, with a range of 126 to 1,638. Invalid votes were few, less than 1% of all votes.

## 3.4 Street network

We digitize a historical map of Hamburg for the period 1930-1940, drawn on a scale of 1:5,000 and provided by the *Landesbetrieb Geoinformation und Vermessung Hamburg* (2020). This allows us to reconstruct the location and width of streets in 1930s Hamburg. To this end, we first geo-locate all

[11] 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 overtime.

[12] 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, Uhlenhosrt, Winterhude. We also discard polling stations farther than 2 Km 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.

streets of inner Hamburg using ArcGIS. Second, we exploit the fact that the map is drawn to scale to generate data on street width. We use the "Measure" tool in ArcGIS to obtain the width of each street in meters. This gives us a street network of 1,381 streets (polylines) in inner Hamburg with information on their 1930s street name, geographic location, start- and endpoints, intersections with other streets, street length and street width.

### 3.5 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 detail for the 17 districts of inner Hamburg. We use the average house rent to proxy for the average wealth of districts. We also use population statistics to validate data collected from the address book. We assign polling stations to the district in which they fall using their address.

## 4 Effects of Direct Exposure

In this section, we answer our first main research question: *Can extremists that march the streets persuade households, inducing support for their agenda?* We first present our difference-in-difference analysis, showing the size and significance of the marches' direct impact on Nazi votes. We then explain our IV-strategy that allows us to argue that the estimated relationship is causal.

### 4.1 Direct Exposure: Difference-in-Differences

We begin with a simple visualization of our data. Figure 5 shows the April-July change in Nazi votes by electoral precinct of Hamburg, superimposed on a map of Hamburg. The map also displays the path of the marches. Together with Figure 1, which shows that areas very close to the marching path swung about 2 percentage points more towards the Nazi Party when compared to places far away from the path, this provides strong graphical evidence that Nazi electoral gains were markedly greater in neighborhoods closer to the marches.

To go beyond the visual evidence, we estimate the following difference-in-differences model:

$$NSDAP_{it} = \alpha_i + \beta\ distmarch_i \times Post_t + \gamma\ X_i \times t + \delta_t + e_{it} \qquad (1)$$

where $NSDAP_{it}$ is the share of votes for the Nazi in polling station $i$, month $t$ (March, 10 or 24 April or July); $distmarch_i$ is the distance to the closest Nazi march, $Post_t$ is a dummy equal to 1 in the two elections after the marches. In our preferred specification, *distmarch*$_i$ is the share of households within 200 meters of a march, but for completeness we will also show results using the logged average distance. We always control for polling station fixed effects ($\alpha_i$) and election fixed effect ($\delta_t$). The simple identifying assumption is that of parallel trends: in the absence of the marches, neighborhoods within 200m of the march would have had similar *trends* as neighborhoods further away. The assumption allows for any arbitrary factor to differ across neighborhoods that is time-invariant, or any factor that changes over time in Hamburg as a whole. In addition, $X_i$ is a vector of controls including (log of) number of voters, share of households with a telephone, share of households with central heating and share of blue-collars as well as street characteristics such as number and width of the streets around the polling station. These variables do not change over time, but for robustness we allow polling stations with different levels of these variables to follow different trends by interacting them with a linear time trend ($X_i \times t$). In the most conservative specification, we interact them with election fixed effects and estimate:

$$NSDAP_{it} = \alpha_i + \beta \; distmarch_i \times Post_t + \sum_t \gamma_t X_i + \delta_t + e_{it} \qquad (2)$$

Table 1 reports results: Column 1 shows a simple specification, explaining Nazi votes as a function of log distance to the march, interacted with a post-march dummy and controlling for polling stations and election fixed effects. We find a large and highly significant coefficient. Despite the fact that the fixed effects absorb much of the variation in the data, the distance measure is large and highly significant. As we add controls in columns 2-3, the $R^2$ rises, but the size of the coefficient remains stable. In column 4, we include controls interacted with election fixed effect. Combined with our main explanatory variable, we can account for nearly all of the variation in our data. Nonetheless, the coefficient on log distance to the March remains highly significant. The estimates imply that going from a distance of 100m to 1,000m reduced Nazi vote gains by 0.3-0.5 pp. This compares with an average vote gain for the Nazi party of 4.9 p.p. between the pre- and post-march elections in Hamburg overall.

Columns 5-8 uses our preferred specification, considering the share of households living within 200m of the marches. Here, the simplest setup suggests that compared to polling stations where none was treated, polling stations where every household witnessed the march voted 1.5 p.p. more for the Nazis ($p < 0.001$). Again, adding controls interacted with time trends leaves the coefficient unchanged. Only in the most conservative estimate in column 8 does the coefficient drops by one third; even then, the estimate is significant at the 1 percent level and implies that direct exposure to the Nazi marches added 1 p.p. – or 18% of the overall increase in Nazi voting.

Figure 6 plots 'event study' difference-in-differences estimates, interacting the <200-meter dummy with dummies for three different moment in time: the first Presidential election, on March 13, the second round on April 10, the municipal election on April 24, and the federal election on July 31. The Nazis marched through Hamburg between the second presidential election in April and just before the municipal election. Thus, the timing of events and the panel structure of the data enable us to estimate the effect of the marches in the late April and July election, and to verify the absence of pre-trends. There was also no pre-trend. Thereafter, we find a significant, positive effect, adding 0.5-0.7 p.p. to the Nazis electoral success.

Could unobservable characteristics correlated with both NSDAP voting and our treatment drive these results? To answer this question, we use the method of Altonji et al. (2005). Despite major increases in the explanatory power, the coefficient of interest remains large and significant. Unobservables would have to account for at least 1.5 times the effect of observables for the effect of distance on NSDAP vote to disappear (column 4). For the specification with the share of households treated, the ratio is even higher: unobservables would need to have 2.1 times the effect of observables to render the coefficient in column 8 insignificant. Hence, selection on unobservables is unlikely to drive our results.

## 4.2 Direct Exposure: Instrumental Variable Strategy

The difference-in-difference approach suggests a strong, positive effect of marches on Nazi gains. However, exposure to a Nazi march was not randomly assigned. The Nazi Party used marches as a key propaganda tool. This makes it more likely that specific areas were targeted deliberately. Residents of streets selected for marches may have been more amenable to Nazi ideology – for example, they may have been more bourgeois, increasing the likelihood of them shifting their support to the Nazi party. In this case, the true effect of the marches would be smaller than the one we estimate. If, conversely, the Nazis deliberately marched through hostile territory, in the hope of intimidating voters with very different political preferences, the parallel trends assumption may be violated. In this case, the difference-in-differences estimate would be biased downwards.[13]

While Nazis planned their marches strategically, they were constrained by Hamburg's geography. They also had to trade off practical considerations about the length of the marching route against other factors like targeting the right areas. To estimate the causal effect of marches on voting, we need to identify sections of the marches chosen not because of strategic considerations, but because of other constraints. We focus on constraints arising from the geography of Hamburg's street network. We do so in three steps. First, we start from the list of starting and terminal points of the marches and consider all possible paths connecting these locations (minimizing the distance covered). Second, we repeat the exercise but constrain the optimal paths to give preference to wide streets. In order for marches to look impressive, they need to pass along wide boulevards, allowing a large phalanx of men (and sometimes, women) to march in lockstep. Many streets in Hamburg were too narrow. Because the marches avoided narrow streets, the overall trajectory had to shift, exposing areas and households further down the line that would not otherwise have witnessed a Nazi march. This is the share of the variation our instrument seeks to capture.

We take the 14 start- and terminal-points of marches as given, allowing them to potentially be endogenously selected. Start- and end-points were determined by the need to gather in large open spaces, accommodating thousands of supporters, as well as the availability of rail and other transport links. We assume that Nazi planners could stage a march between any of these points. This yields 91

[13] Intimidation was a key aim of Nazi electoral campaigns (Selb and Munzert, 2018).

possible combinations across pairs of points. We then look at least-cost paths, calculated from the actual 1930s street network of Hamburg. The share of optimal paths out of 91 possible ones near a polling station is our measure of how much of the marching activity in any one location is driven by the Nazis' need to march between assembly and rallying points. This we call our "naïve" measure of optimal paths. Thus, whether a polling station is only close to the march because it is on a street between assembly and rallying points is captured by the naïve optimal path measure.

Second, we calculate least cost paths assuming that Nazi planners gave priority to wide streets (top tercile of street width of Hamburg). We then compute the share of paths traversing the polling station area as a result of marches being routed through wide streets. The standardized difference in exposure (the likelihood of a march passing through) between the width-based and the naïve optimal path serves as our instrument.

Figure 7 graphically illustrates the intuition behind the construction of our instrument. To build intuition about the identifying variation, consider the possible paths between the start and end point in Figure 7. If the Nazis simply wanted to march to the end point to hold a mass rally, they would be indifferent between these equidistant paths. However, if the planners preferred to march through wide streets (our assumption), then they would strictly prefer the lower route in the graph (solid red line). Now, consider the individuals living along Street B and Street C. They are both located along potential paths, both narrow streets. All individuals living in neighborhood $C_1$ and $C_2$ will be "treated", i.e. the march will pass by. In contrast, individuals in neighborhood $B_1$ and $B_2$ are in the "control" group because the march will not pass through their street. Crucially, the *reason* that the $C_1$ and $C_2$ will be "treated", and that $B_1$ and $B_2$ are in the "control" group, is *not* that $C_1$ and $C_2$ are located along a wide street. Instead, the treatment is determined by the availability of wide streets in *other* locations, and the fact that the Nazis will march from the start to the terminal point. Thus, conditional on being located next to a wide street or not, and on being located near a potential path, the residual variation is arguably as-good-as randomly assigned. In particular, we would not expect it to be correlated with other determinants of changes in Nazi support, such as pre-existing levels and pre-trends in Nazi vote shares before the marches take place (an assumption we can test directly).

The logic of the identification strategy has implications for the sample used in our analysis. The neighborhoods of the start/mid/terminal points do not provide exogenous variation. Nazi planners selected these locations either because they believed they were important or because they wanted to cover areas between these points -- or both. Our strategy cannot identify the causal effect of marches for these polling stations. We therefore drop them from our main sample.[14]

If our instrument is as-good-as randomly assigned, there should be no correlation with either pre-existing support for the Nazi Party right before the election (April election) or pre-trends (from 13 March to 10 April). Figure 9 panel (a) examines the balancedness of key characteristics along the optimal path. It plots beta-coefficients for the difference of with-based and optimal paths. We find no significant association with the level of Nazi support before the marches – either on March 13 or on April 10. There is also no pre-trend in terms Nazi vote gains. The size of electoral districts is near-identical. The share of blue-collar workers is slightly negatively associated with the instrument, but

[14] This is standard in the literature of least cost paths (cf. Faber, 2014).

the difference is not significant. The same is true of rents. Other indicators of socio-economic status (share of households with a telephone, share with central heating) are uncorrelated with the instrument. These results confirm that our instrument captures variation that is orthogonal to observable characteristics of the neighborhoods.

Figure 8 maps this variation across neighborhoods, showing where the instrument implies a positive shock. The figure also shows the actual marching routes as black lines. While the link is not perfect, the figure demonstrates that width considerations must have played an important role in the selection of roads used for the Nazi march.

We implement our identification strategy by first estimating the following first-stage equations:

$$logdist_i = \alpha + \beta\ \Delta[\ path\_wide_i - path\_naive_i\ ] + \gamma X_i + e_i \qquad (3a)$$

$$march\_treated_i = \alpha + \beta\ \Delta[\ path\_wide_i - path\_naive_i\ ] + \gamma Xi + e_i \qquad (3b)$$

where $logdist_i$ is log distance to the march, $march\_treated_i$ is the share of households In the polling station living within 200m from the march, and $\Delta[\ path\_wide_i - path\_naive_i\ ]$ is the difference in the likelihood of the marching path going through a neighborhood between the naïve and the width-adjusted algorithm. For ease of exposition, we will refer to our instrument as the *path shift variable*. Our preferred specification also includes the presence of wide streets in the neighborhood (number of streets, share of wide and share of narrow streets within 200m of the polling station), distance to the start and terminal points, and distance to the straight line between start/mid/terminal points. The optimal path calculated after considering the width of the roads will identify the part of the variation in exposure to Nazi marches driven only by Nazi planners' desire to march along wide roads. Moreover, since we control for street network characteristics around these polling stations, we exploit the variation in the instrument that is generated by streets in the network far away from the polls themselves. If our assumption that planners preferred wide streets along the path is correct, we would expect $\beta < 0$ in (3a) and $\beta > 0$ in (3b) – being far away from the 'treated' part of Hamburg streets should result in smaller increases in Nazi support.

Figure 9 panel (b) presents the first stage for our panel graphically. We find that the path shift instrument is clearly associated with significant increases in the likelihood of treatment. Table 3, panel (a) shows the statistical results. Both versions of the dependent variable – log distance to the march and the dummy for being within 200m of the march – are strongly predicted by the path shift instrument. This is true whether we only control for street characteristics (columns 1 + 3), or add controls for demographic features (columns 2 + 4). The F-statistic is somewhat below the standard rule-of-thumb value of 10; below we use inference that is robust to weak instruments.

The corresponding reduced form regression is:

$$NSDAP_{it} = \alpha_i + \beta\ \ \Delta[\ path\_wide_i - path\_naive_i\ ] \times Post_t + \gamma\ X_i \times t + \delta_t + e_{it} \qquad (3)$$

where variables are defined as in (1), (2) and (3a)-(3b). If the marches led to greater support for the Nazi Party, we would expect $\beta > 0$. In contrast, if the marches create a backlash, we would expect $\beta < 0$. The *exclusion restriction* is that the variation due to path width optimization affects the swing in favor of the Nazis between April and July only because it lowered the distance to the march.

Figure 9, panel (c) shows a binscatter version of the reduced form analysis: we plot on the x-axis our path shift instrument, and on the y-axis the change in Nazi Party vote share between the pre marches and post marches elections. The higher the increase in the likelihood of witnessing a march due to width considerations, the greater the increases in electoral support for the Nazis. The corresponding results in Table 3, panel (b) show that Nazi support increased sharply after the marches where the path shift variable has high value (columns 1-2). The IV results in columns 3 and 4 imply large effects, with predicted vote gains of 5.4 p.p. in polls where every household witnessed the marches. The Anderson-Rubin test indicates that the instrument has a significant effect on NSDAP support, and suggests that the low F-statistic of the first stage is not a concern. While the IV-coefficient in panel (b) is large, it is similar in size to the unconditional correlation between marches and NSDAP support.[15]

In Figure 3, we repeat the analysis, but now estimate effects separately for each election. Distances on the x-axis are scaled to reflect the length of time intervals. In both the reduced form and the 2SLS-results, we see that there is no pre-trend towards greater Nazi voting prior to the two marches. Thereafter, the coefficient on the treatment variable jumps discretely after the marches. It is already large for the municipal election, showing a significant, positive effect on Nazi support; for the federal election in July, it increases even further, but there is no significant difference in magnitudes.

# 5 Indirect Exposure: Contagion through Networks

Did the message conveyed by the Nazi marches only sway those who could see the marching columns from their kitchen window? Or did the message spread? And if so, how? In this section, we first describe our empirical strategy for estimating whether the direct exposure translated into greater radicalization via social connections. We then present the results.

## 5.1 Empirical Strategy

We want to know whether districts with more indirect treatment – areas where individuals had close social ties to individuals exposed to the marches – showed greater shifts in favor of the Nazis. Ideally, we would like to know how much ‘treated’ households interacted with those that did not witness the

[15] Note that the size of the coefficient for IV will reflect the share of compliers in the population relative to never-takers. This ratio is 4 (see below) in our case, implying that the IV coefficient should be approximately 4 times larger than the OLS.

marches. Unfortunately, actual social interactions between individuals (or neighborhoods) are not observable.

To overcome this challenge, we build on the concept of *homophily*, which is generally associated with greater interaction frequency, and exploit the granular data on household characteristics across the city. We measure social distances between two polling stations *a* and *b* by Euclidian distances, defined over *n* characteristics of the population:

$$d(a,b) = \sqrt{\sum_{i=1}^{n}(a_i - b_i)^2}$$

where $a_i$ is the value of characteristic *i* for polling station *a*, and $b_i$ for polling station *b*. As characteristics, we use the share of Hamburg residents belonging to a set of 33 sectors, 9 different social classes, as well as the share of households with surnames typical in each of the 100 2 digits zip codes of Germany.

To calculate social network exposure to polling districts traversed by Nazi marches, we first calculate the simple Euclidian difference between an untreated district *a* and the untreated area *b*; *b* is the entire set of polling stations that had at least 80% of households living within 200m from the marching route. We then compare this Euclidian distance to the "treated" area with its distance to other, untreated areas, which we call *c* for expositional simplicity:

$$\theta_i = \sqrt{\sum_{i=1}^{n}(a_i - b_i)^2} - \sqrt{\sum_{i=1}^{n}(a_i - c_i)^2}$$

where *c* excludes *a*. We standardize the distribution of θ so that it has zero mean and a standard deviation of unity.

Figure 10 shows a neighborhood of Hamburg, illustrating the wide variation in our social distance measure. Black lines represent marching routes and black dots are the addresses of households living within 200m from these marches. For the households living farther than 200m, graduated colors indicate the social distance to directly treated households: red are socially close and yellow socially distant. Households few block apart have very different measures of social distance. Interestingly, social and physical distance to the march are almost entirely unrelated (cf. Appendix Figure 4).

## 5.2 Results

Differences in social distance in general predict changes in voting. Figure 11 plots changes in vote shares between the 2nd presidential election on April 10 (before the march), the Bürgerschaftswahl on April 24 (after the march), and July 31. In the municipal election, within a week of both marches, there were no additional gains in districts that were socially connected to areas through which the march passed. By July 31, however, the consequences of social proximity become evident -- close connections with districts exposed to the march predict sharply larger gains for the Nazi Party.

Next, we examine the statistical significance of these patterns. Table 4 gives an overview of results. Across specifications, we find that the more different in social terms an area was relative to the one exposed to the march, the smaller the vote gain after mid-April. This does not depend on the exact specification, and is robust to including election and polling station fixed effects, a time trend interacted with street controls and demographic characteristics, and a measure of direct exposure: the log distance to the march itself. Our social distance variable is standardized: hence, the coefficient on col. 4 in Table 4 implies that one standard deviation increase in social distance translated into 1.2 p.p. more votes for the Nazi party after the marches. Appendix Table A.2 allows the effect of social distance to vary by election. In cols. 1 and 2 when we control for election and polling station fixed effects as well as street characteristics interacted with election fixed effects we find a significant effect of social distance in the later election. However, when we include demographic characteristics interacted times election fixed effects in cols. 3-4 the coefficient of social distance becomes smaller and insignificant. The demographic characteristics we control in cols. 3-4 are the same characteristics we use to build our social distance variable, and we conjecture that in this demanding specification they are absorbing useful variation from our explanatory variable.

# 6 Robustness

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

## 6.1 Spatial standard errors

The geographic nature of voting and distance to Nazi marches creates spatial autocorrelation and may bias standard errors. We address spatial autocorrelation by re-estimating standard errors with the formula proposed by Conley (1999). The method allows for error terms of polling stations to be correlated, and assumes that correlation decays linearly with distance until a cut-off. We experiment with four different cut-offs (0.2, 0.5, 1 and 1.5 kilometers) and report cluster-robust standard errors for comparison. Appendix Table 3 reports results for difference-in-differences (col. 1), reduced form (col. 2) and IV (col. 3). In the baseline specification we account for serial correlation by clustering at the polling station level. When we use Conley (1999) formula we correct standard errors by allowing serial correlation across the 4 periods.

Overall, standard errors remain remarkably stable across specifications and significance is largely unaffected. We conclude that spatial correlation is unlikely to drive our results.

## 6.2 Sample restrictions

Across our four elections, 29 polling stations changed location. These changes affect 5% of our main sample. Moreover, the new location was always within 500 meters of the old one. Nothing suggests that polling stations moved strategically in response to marches or electoral results and records do not suggest that households were reassigned to different polling stations as a result of these changes. To ensure that moving polling stations are not driving our results, in Appendix Table 4 we also reproduce our results after dropping these observations. Column 1 reports difference-in-difference estimates. Column 2 reports the reduced form, and column 3, the two-stages least squares. If anything, estimates are larger and more significant in the sample of polling stations that did not move.

## 6.3 Matching exercises

In this section we demonstrate the solidity of our difference-in-difference results using three alternative approaches to estimate causal effect without instruments. For each of these methods we focus on the binary version of our treatment: the dummy for having at least 80% of households within 200m from the march.

First, in Appendix Table 5 we estimate the treatment effect with nearest neighbor matching. To do so, we turn to the first difference version of equation (1) and look at the effect of being within 200m from the march on the change in NDSAP support from before to after the Nazi marches. We match polling stations based on coordinates and demographic controls (cols 1 + 3 + 5) and on coordinates, demographic controls and within district (cols 2 + 4 + 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 slightly larger than our baseline effect.

Second, we apply the method of Hainmueller (2012) and re-weight treated and control observations so that they have similar observable characteristics. Appendix Table 6, 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. Next, we re-estimate equation (2) with weighted least squares, using entropy weights. The point estimate is reported in col 2 of Appendix Table 6, 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 (2) with Coarsened Exact Matching (CEM). We find exact matches within cells defined by number of voters, share of households with telephone, of share of blue-collars (4 quartiles) as well as share of households with heating (dummy). Within these cells, we can find exact matches for 88% of our polling stations, and we estimate equation (2) on these observations only. The result is reported in col. 3 of Appendix Table 6 panel (b), showing a point estimate near-identical to our baseline.

## 6.4 Social distance for different cutoffs

To compute our difference in social distance measure, we rely on defining a group of polling stations as treated based on a cutoff of at least 80% of households being located within 200m from the march. In this section we explore how changing the threshold for the binary version of our treatment affects our contagion effect results. We experiment with five different cutoffs: a polling station is defined as treated if more than 50%, 60%, 70%, 80% or 90% of households are located within 200m of the march. We compute Euclidean distance to the treated and untreated area and take the standardized difference between the two values for each cutoff as described in section 5.1. Appendix Table A.7 reports the results of this exercise. The estimates remain remarkably stable and significant for lower cutoffs (panel b-e). For the 90% cutoff (panel a) we obtain slightly lower point estimates for the effect of social distance. Overall, the effect of social distance is strong and significant for all five cutoffs for defining the binary version of our treatment reassuring that our baseline results for social distance are not driven by an arbitrary choice of direct treatment.

# 7 Conclusion

When humans congregate in large numbers, they can act in new ways. From the Woodstock festival in 1968 to the crowds storming the Bastille in 1789 large groups of people acting in concert have contributed to extraordinary changes in human history (Rudé 1981). Emile Durkheim observed that "the very act of congregating is an exceptionally powerful stimulant… a sort of electricity … launches [people] to an extraordinary state of exaltation" (Haidt, 2012). Politicians and journalists are convinced that this powerful stimulant can be used as to gather support – but support for this belief is limited. Granular evidence of a causal effect, and of how such an effect might spread to a wider group, is largely conspicuous by its absence.

Our paper first analyses the impact of Nazi marches at a highly disaggregated level, and then uses granular data to present evidence of 'contagion' – how the effect of marches spreads via social networks. To this end, we assemble data on almost 400,000 Hamburg households, digitize their socio-economic characteristics and location, and geo-code the interwar street network as well as the locations of more than 600 polling stations.

The effect of marches on voting can be pinned down by looking at marching paths. To look particularly awe-inspiring, marches had to take place on wide boulevards and avenues, not narrow streets. Our IV strategy demonstrates that there is a large, causal effect of exposure to Nazi marches and Nazi voting on electoral outcomes.

While we do not observe social interactions directly, we construct measures of similarity for households in "treated" and "untreated" electoral districts, using information on occupation, surname (indicating regional origin), and social standing. Overall, direct and indirect treatment had large effects on outcomes. In the twilight years of the Weimar Republic, both the Nazi party and the Communists staged thousands of rallies and marches all across Germany. Our results imply that public shows of force were every bit as important as Goebbels believed – creating a pageant of power that was to prove irresistible for large parts of the middle class.

Our findings strongly suggest that anti-democratic forces taking to the streets can create more support for their own position. They benefit from basic democratic rights and freedoms guaranteed by the very political system they oppose. This idea led the Anglo-German philosopher Karl Popper

(1945) to conclude that a tolerant society's self-preservation requires the suppression of intolerant political movements, leading to what his well-known 'paradox of tolerance'. The extent to which a society should be willing to live with this paradox is a direct function of how contagious extremism is. We find that revoking the bans on Nazi marches that had been common in many parts of Germany in the 1920s was corrosive for the survival of Germany's first democracy, facilitating the gathering of additional electoral support. In other words, because extremism can be contagious, there is a strong case for denying the freedom to assemble to the extremists themselves.

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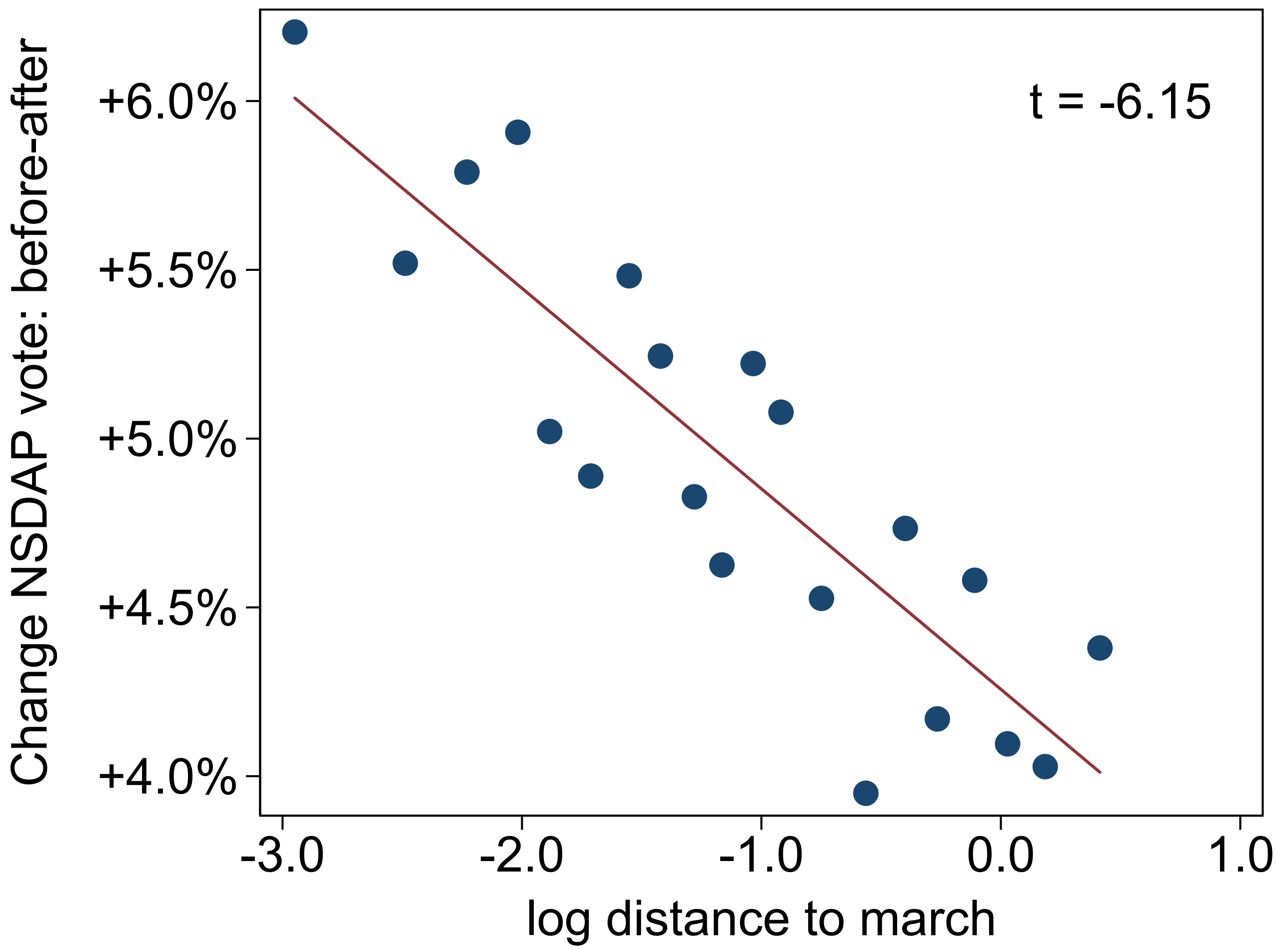
## Figures

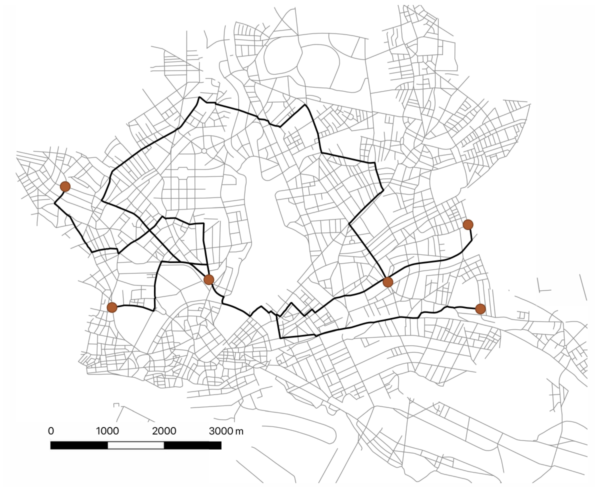
Figure 1: Simple correlations.



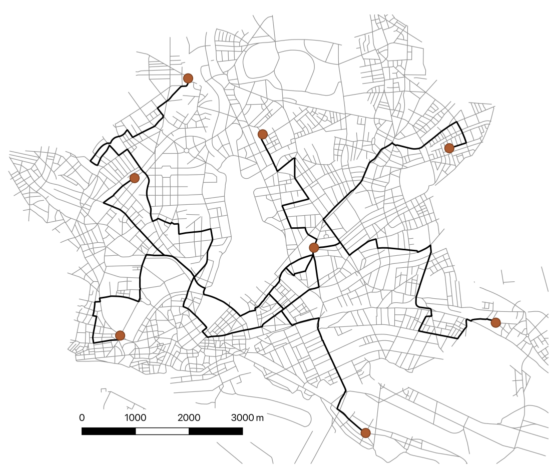

*Note*: The Figure plots a bin-scatter of log distance to one of the Nazi marches (x-axis) against the change in NSDAP vote share after the marches (y-axis). Change in NSDAP vote is calculated between the two elections before the marches (13 March and 10 April 1932) and the two after (24 April and 31 July 1932). $t$-statistic estimated from a bivariate regression. Sources: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b) (Nazi marches) and statistical bulletin of Hamburg (Skllin, 1932a; 1932b) (voting data).

Figure 2: Nazi marches in Hamburg in April 1932.

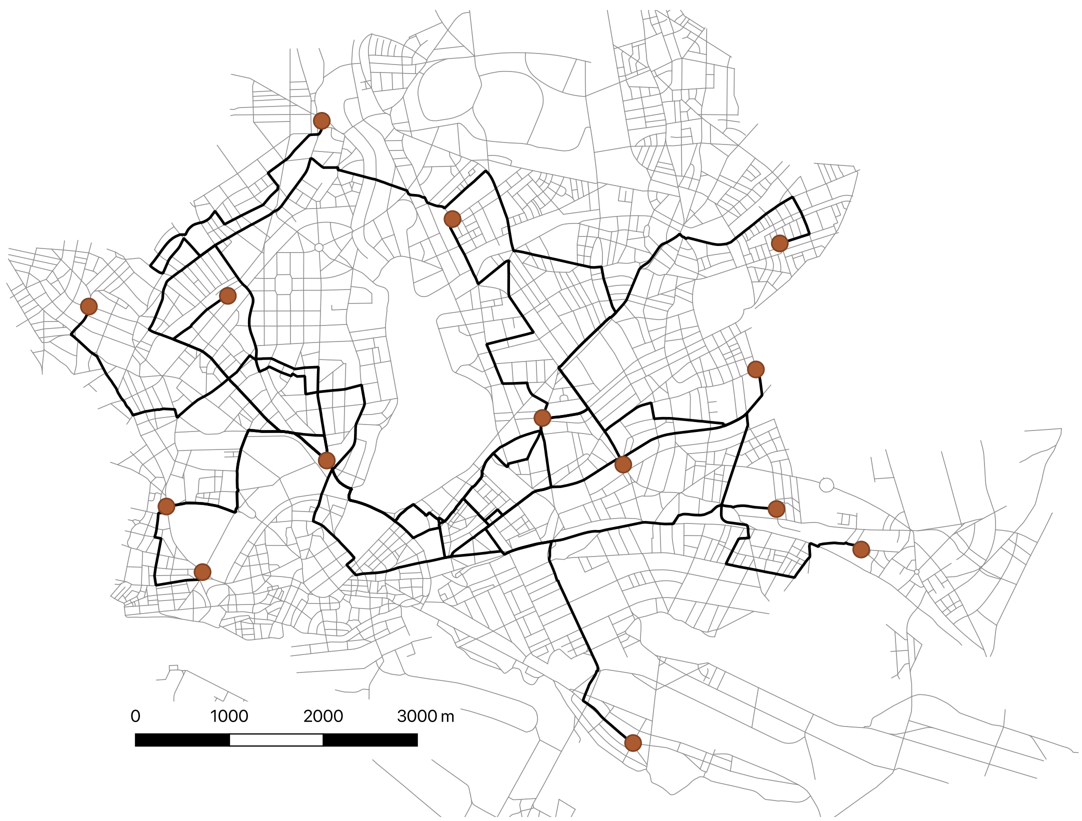
(a) 17 April 1932.




(b) 20 April 1932.

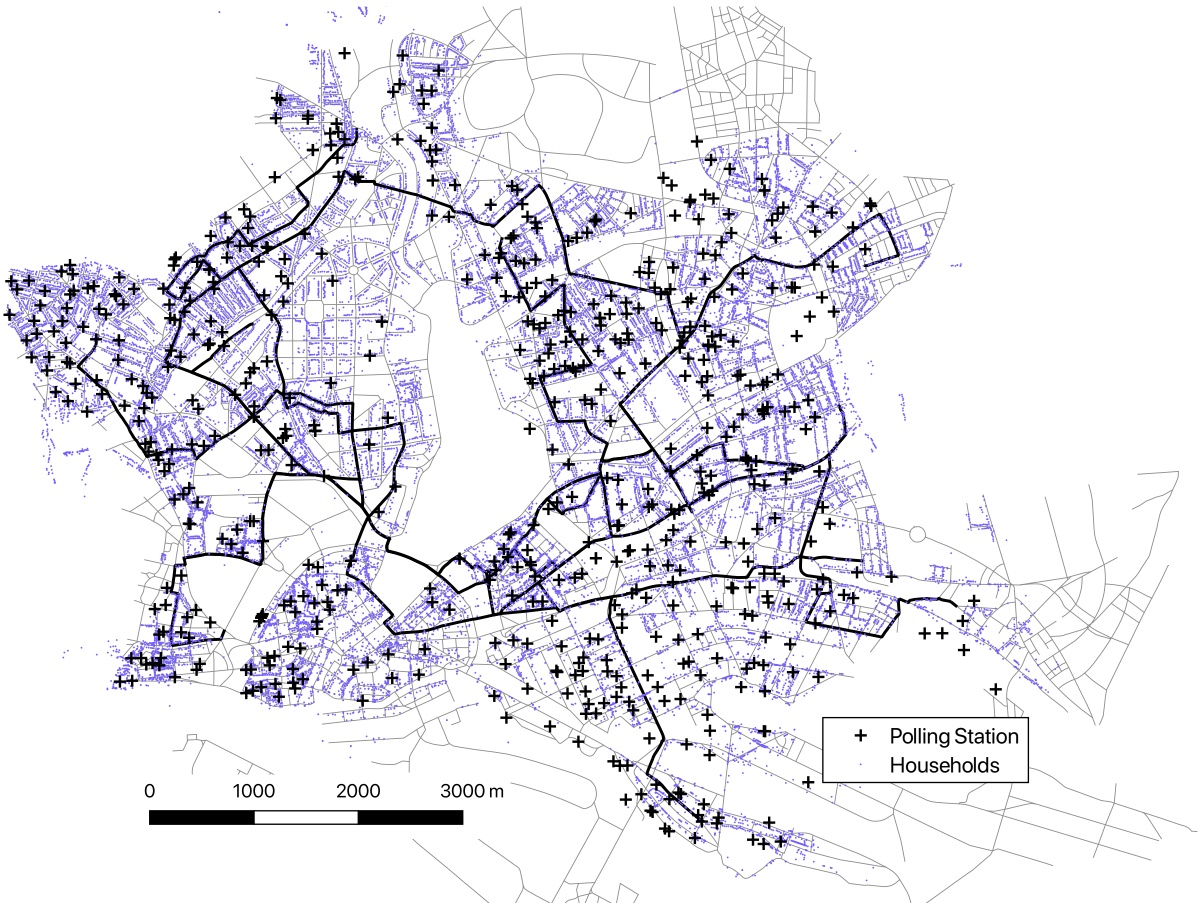



(c) Pooled.




*Note*: The maps show the routes of the two Nazi marches on April 17th, 1932 (top left panel), April 20th, 1932 (top right panel), as well as the combined routes (bottom panel). Brown circles identify the starting and ending point of the marches. Source: SA Hamburg documents (State Archive Hamburg, 1932a; 1932b).

Figure 3: Households and polling stations.

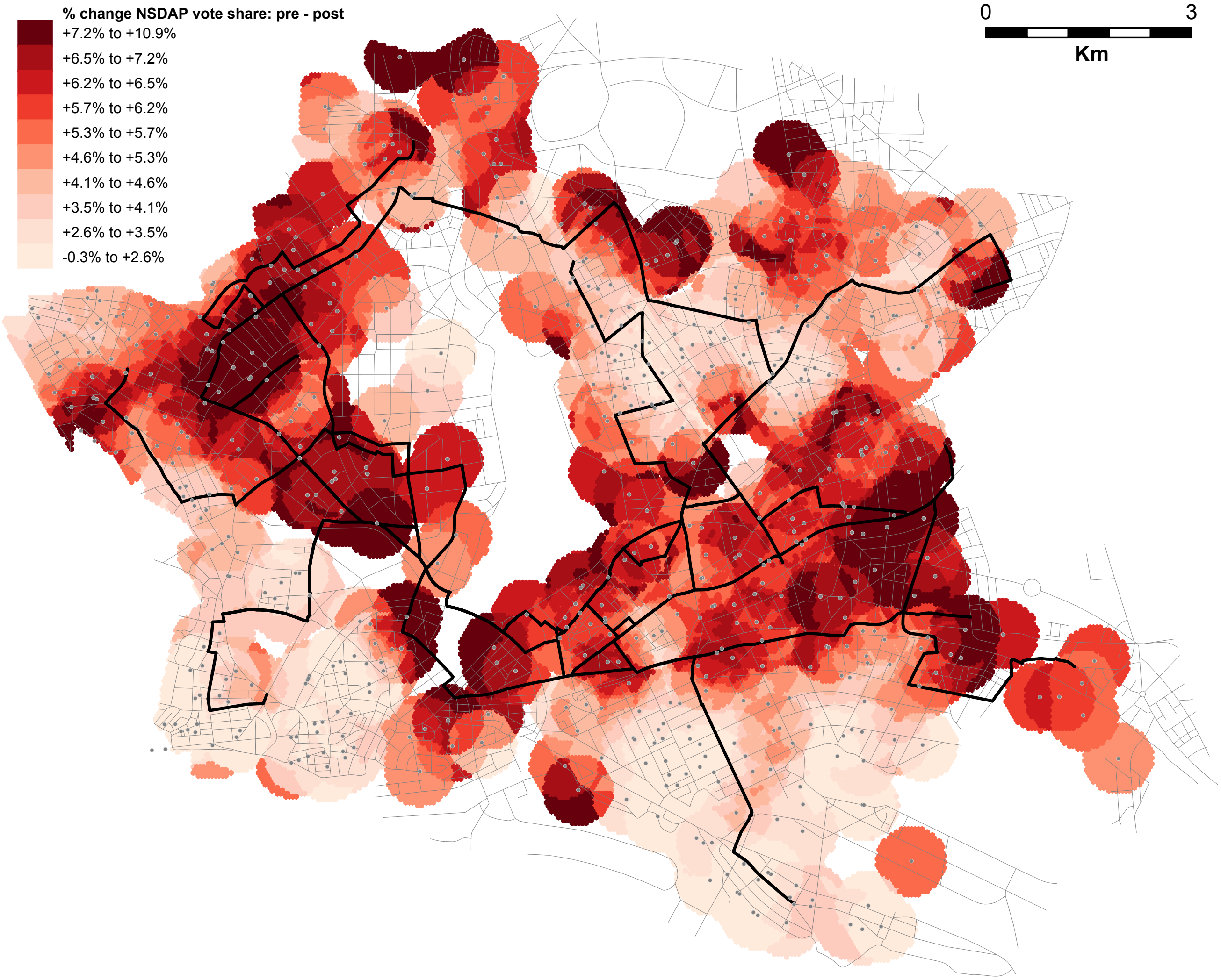



*Note*: The map shows the location of the polling stations in Hamburg (black crosses) and the addresses of the 400,000 households living in Hamburg in 1932 (blue dots). We overlay these location on the street network of Hamburg. Sources: statistical bulletin of Hamburg (Skllin, 1932a; 1932b) (voting data), 1932 Hamburg address book (Hamburger Adrebuch, 1932) (households data) and historical map of 1930-1940 Hamburg (Landesbetrieb Geoinformation und Vermessung (LGV) Hamburg, 2020) (street network).

Figure 4: Direct exposure.

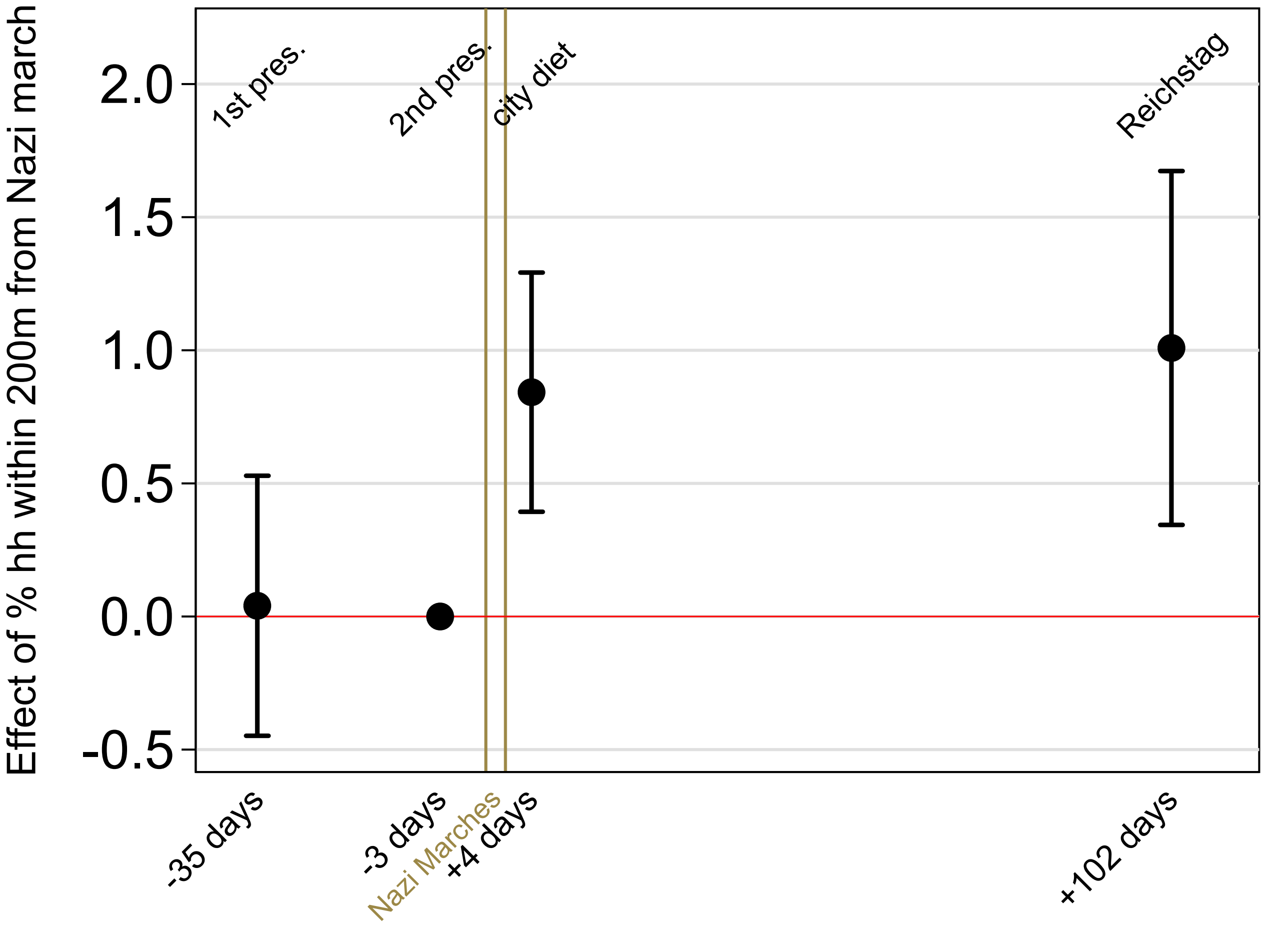

*Note*: The map shows the routes of the two Nazi marches in April 1932 (black lines) and the addresses of the 400,000 households living in Hamburg in 1932. Red dots are addresses located less than 200m from one of the marches' routes; gray dots are all other addresses. Sources: 1932 Hamburg address book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

Figure 5: 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 two after (24 April and 31 July 1932). 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 map with the routes of the two Nazi marches in April 1932 (black lines). Sources: statistical bulletin of Hamburg (Skllin, 1932a; 1932b) (voting data) and SA Hamburg documents (State Archive Hamburg, 1932a; 1932b) (Nazi marches).

Figure 6: Difference-in-differences.

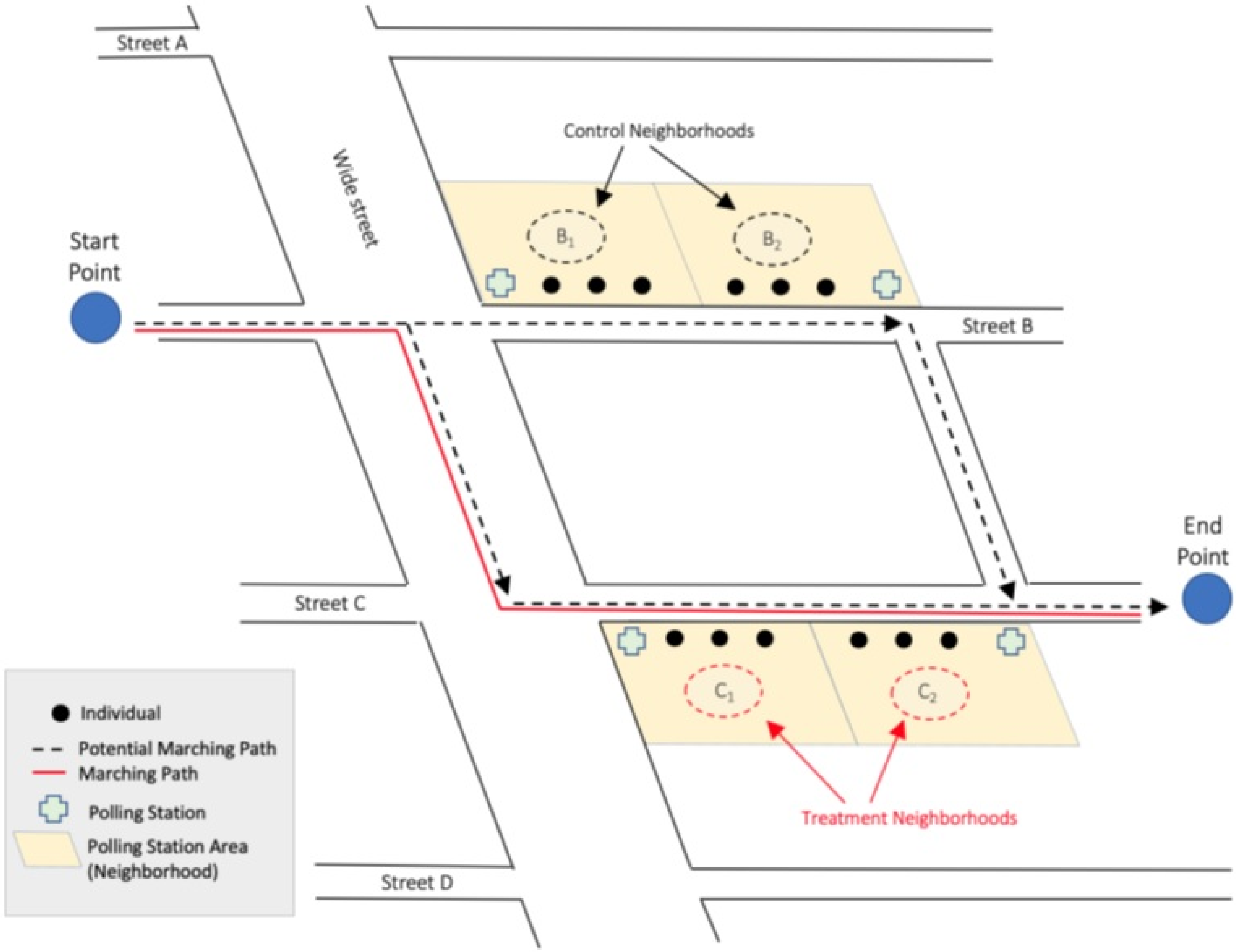



*Note*: The Figure plots the coefficients $\beta_t$ from the following regression:

$$\text{NSDAP}_{it} = \alpha_i + \delta_t + \sum_{t=13\text{Apr}}^{31\text{Jul}} \beta_t \times \text{march\_treated}_i + \sum_{t=13\text{Apr}}^{31\text{Jul}} \gamma_t X_i + u_{it}$$

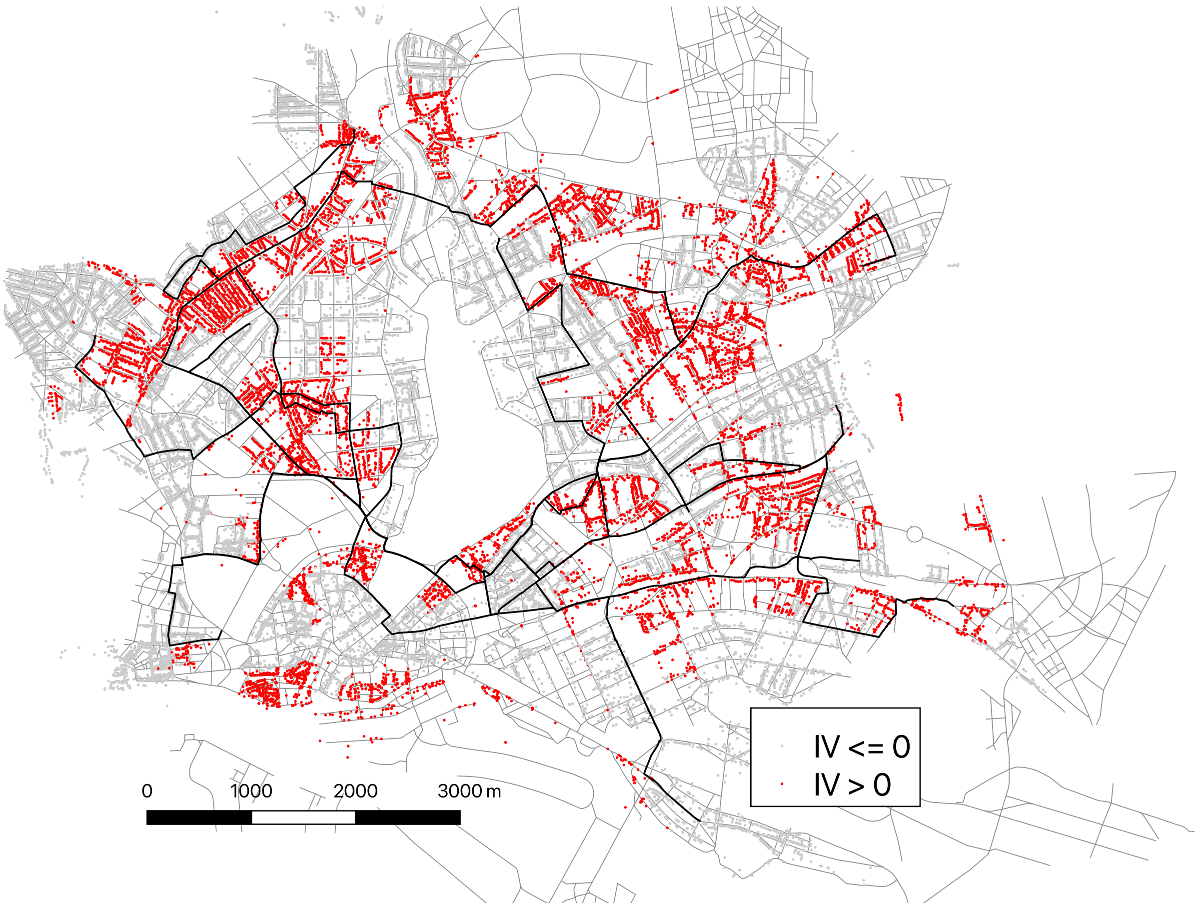
See text for a description of the variables. We estimate standard errors clustered at the polling station level and plot 95% confidence intervals as bars around the coefficients.

Figure 7: Conceptual map - direct treatment.




*Note*: The Figure shows an hypothetical map to conceptualize the construction of our instrument. See Section 4.2 for details.

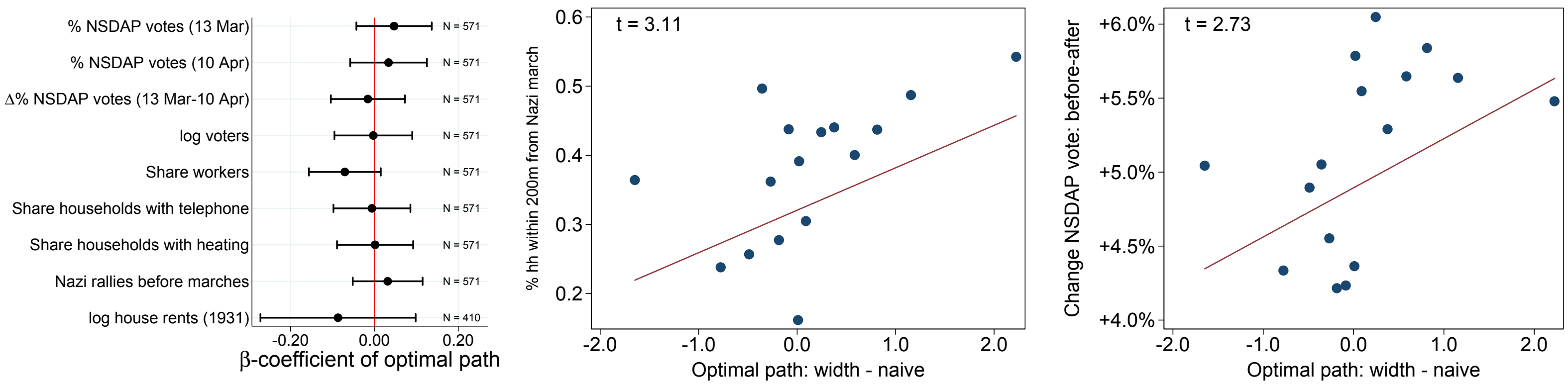
Figure 8: Instrument: street-width optimal path shocks.




*Note*: The map shows our instrument: the street width optimal path shock. To construct it, we first calculate all possible shortest distance paths connecting each of the possible 14 starting points, rallying points and end points of the marches. At the end of this process, we assign to every household a probability equal to the share of optimal paths that pass within 200m of each house. As a second step, we calculate optimal paths that give priority to wide streets. At the end of this process, we assign to every household a second probability equal to the share of width-based optimal paths passing within 200m from his house. The instrument is the difference of these probabilities. Red dots are households for which the probability of width-based optimal paths is higher than the probability of naïve optimal paths; gray dots are all other households. We overlay map with the routes of the two Nazi marches in April 1932 (black lines) and the Hamburg street network (gray lines). Sources: 1932 Hamburg address book (Hamburger Adrebuch, 1932) (households data), SA Hamburg documents (State Archive Hamburg, 1932a; 1932b) (Nazi marches) and historical map of 1930-1940 Hamburg (Landesbetrieb Geoinformation und Vermessung (LGV) Hamburg, 2020) (street network).

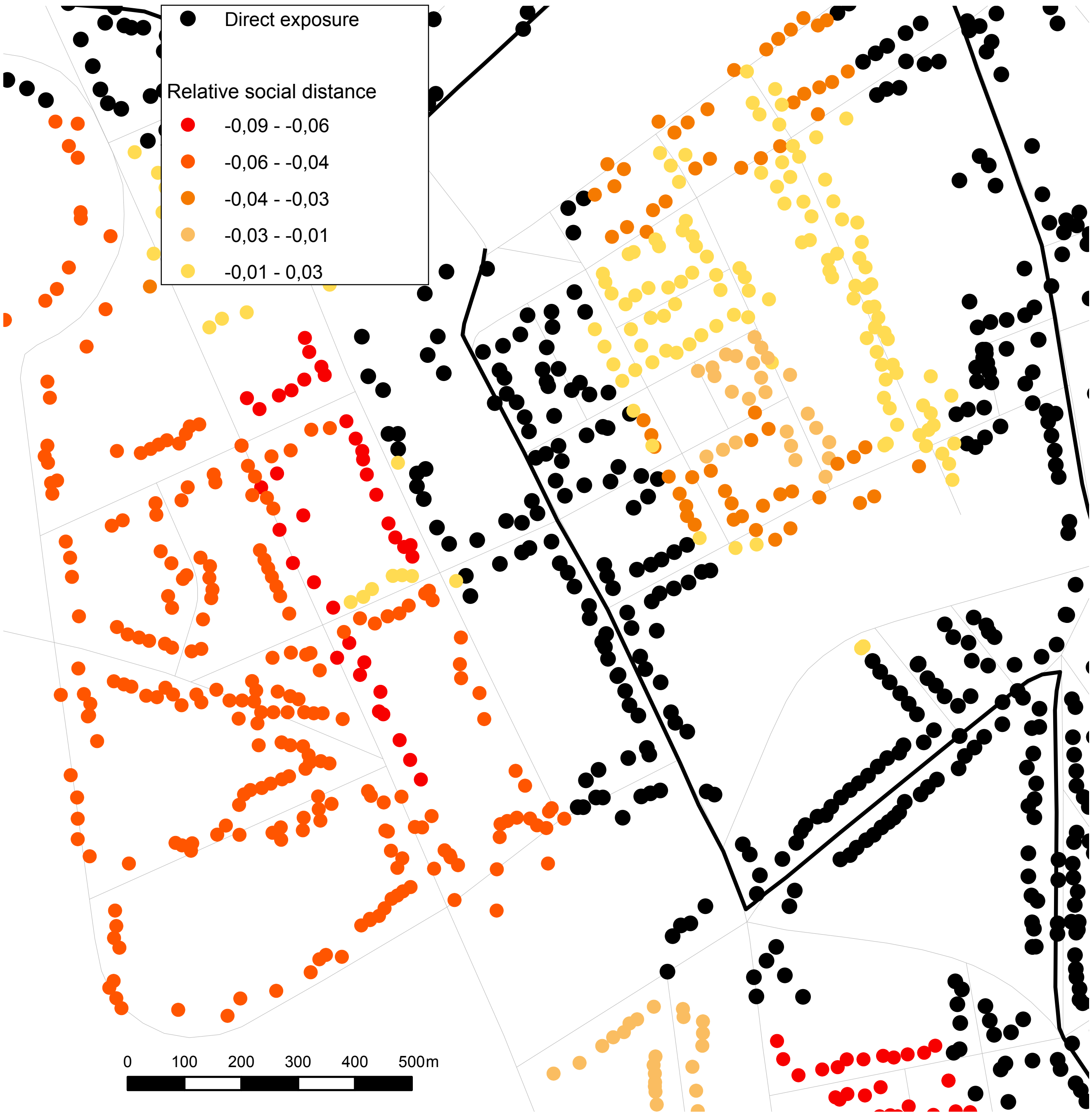
Figure 9: Identification.

(a) Balance. (b) First stage. (c) Reduced form.




*Note*: Panel (a): graph plots standardized beta coefficients of bi-variate regressions of the variables listed on the left on the street-width optimal path shock, showing balance in our sample. Bars represent 95 confidence intervals calculated from robust standard errors for all variables except for the 1931 house rent, which we observe at district level and for which we calculate clustered standard errors with the bootstrap method of Cameron, Gelbach and Miller (2008). See Appendix Table A.1, for non-standardized coefficients. Panel (b): first stage. Unconditional binscatter of street-width optimal path shock (x-axis) against the probability of having average distance to the march lower than 200m (y-axis). $t$-statistic estimated from a bivariate regression. Panel (c): reduced form. Unconditional binscatter of street-width optimal path shock (x-axis) against the change in NSDAP vote share after the marches (y-axis). Change in NSDAP vote is calculated between the two elections before the marches (13 March and 10 April 1932) and the two after (24 April and 31 July 1932). $t$-statistic estimated from a bivariate regression.

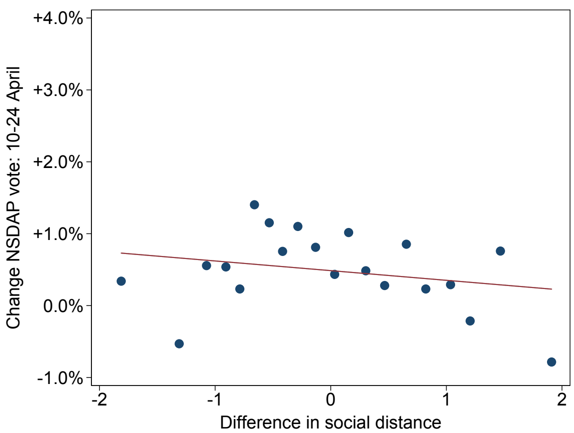
Figure 10: Indirect exposure: relative social distance to exposed households.



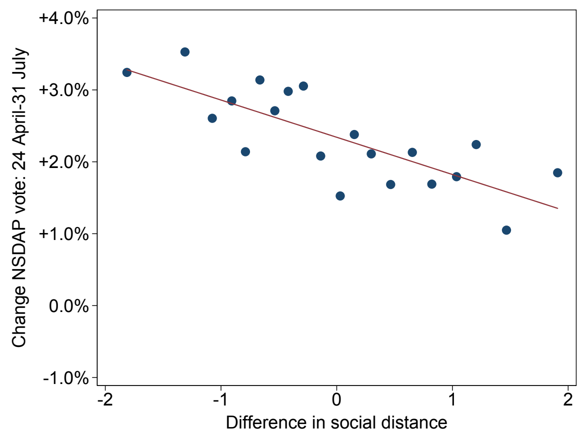

*Note*: The map provides an example of households in a local area of Hamburg. Each dot represents a unique address. Black dots indicate households that are within 200 meter of a march, i.e. direct treatment. The colors of the other dots represent our measure of social connections to directly treated households. We calculate this measure in three steps, using household-level information aggregated at the polling station level. First, for every polling station we compute the average social distance to treated polling stations. Social distance is approximated by the euclidean distance defined over 140 household-level characteristics. Second, for the same polling station, we compute the average social distance to non-treated polling stations. Third, we take the difference between the two social distances. The map plots this measure, which we assign to individual households based on the official voting lists. The map shows stark variation in social distance across households located relatively close to one another. We overlay the map with the routes of the two Nazi marches in April 1932 (black lines) and the Hamburg street network (gray lines). Sources: 1932 Hamburg address book (Hamburger Adrebuch, 1932) (households data), SA Hamburg documents (State Archive Hamburg, 1932a; 1932b) (Nazi marches) and historical map of 1930-1940 Hamburg (Landesbetrieb Geoinformation und Vermessung (LGV) Hamburg, 2020) (street network).

Figure 11: Change in NSDAP support and social distance.

(a) From 10 April to 24 April




(b) From 24 April to 31 July




*Note*: Panel (a): Binscatter of standardized difference in social distance (x-axis) against the initial change in NSDAP vote share after the marches (y-axis). Change in NSDAP vote is calculated between the last election before the marches (10 April 1932) and the first after (24 April 1932). We define treatment as dummy = 1 if more than 80% of households are located within 200m of the Nazi march. The sample only includes non-treated polling stations. We control for physical distance to the Nazi march. Panel (b): Binscatter of standardized difference in social distance (x-axis) against the change in NSDAP vote share between the two elections that happened after the marches (y-axis). Change in NSDAP vote is calculated between the first election after the marches (24 April 1932) and the second (31 July 1932). We define treatment as dummy = 1 if more than 80% of households are located within 200m of the Nazi march. The sample only includes non-treated polling stations. We control for physical distance to the Nazi march.

# Tables

Table 1: Summary statistics

| | Min | Mean | Max | St. dev. | Obs. |
|---|---|---|---|---|---|
| *Election results* | | | | | |
| Hitler vote share 13 March (pre) | 5.864 | 24.260 | 43.231 | 8.282 | 622 |
| Hitler vote share 10 April (pre) | 8.163 | 30.417 | 55.901 | 10.307 | 622 |
| NSDAP vote share 24 April (post) | 7.938 | 31.028 | 53.465 | 10.570 | 618 |
| NSDAP vote share 31 July (post) | 8.253 | 33.468 | 56.788 | 11.301 | 620 |
| *Nazi march* | | | | | |
| Share of households w/i 200m of march | 0.000 | 32.514 | 100 | 36.340 | 622 |
| Average distance to closest Nazi march (km) | 0.026 | 0.481 | 1.939 | 0.405 | 622 |
| *Demographic controls* | | | | | |
| Number of voters at polling station | 488 | 1291.299 | 1943 | 172.373 | 622 |
| Share of blue collar workers | 0.000 | 35.802 | 63.793 | 14.526 | 622 |
| Share of households with telephone | 0.000 | 12.168 | 73.165 | 12.331 | 622 |
| Share of households with heating | 0.000 | 6.011 | 75.000 | 12.281 | 622 |
| *Street controls* | | | | | |
| Distance to closest extreme point (km) | 0.069 | 1.386 | 3.855 | 0.711 | 622 |
| Distance to closest staight 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 | 0.000 | 40.926 | 100 | 29.998 | 622 |
| Share of streets in bottom tercile of width | 0.000 | 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 (Skllin, 1932a; 1932b). 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 Adrebuch, 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. Share of households with telephone, with heating, share of blue collar workers come from the 1932 Hamburg address book. 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. Number and width of streets within 200m from the polling station is calculated from the digitized street network (Landesbetrieb Geoinformation und Vermessung (LGV) Hamburg, 2020).

Table 2: Effect of NSDAP marches (17-20 April 1932): difference-in-differences.

| | % NSDAP votes | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| log distance Nazi march × post Nazi march | -0.584*** | -0.584*** | -0.585*** | -0.326*** | | | | |
| | [0.109] | [0.109] | [0.109] | [0.092] | | | | |
| % households w/i 200m from march × post Nazi march | | | | | 1.515*** | 1.514*** | 1.517*** | 0.906*** |
| | | | | | [0.277] | [0.277] | [0.278] | [0.223] |
| Election & polling station FEs | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Street controls × time trend | No | Yes | Yes | No | No | Yes | Yes | No |
| Demographic controls × time trend | No | No | Yes | No | No | No | Yes | No |
| Street and demographic controls × election FEs | No | No | No | Yes | No | No | No | Yes |
| $R^2$ | 0.976 | 0.977 | 0.977 | 0.987 | 0.976 | 0.977 | 0.977 | 0.987 |
| Mean NSDAP vote in 10 Apr '32 election | 30.417 | 30.417 | 30.417 | 30.417 | 30.417 | 30.417 | 30.417 | 30.417 |
| Observations | 2482 | 2482 | 2482 | 2482 | 2482 | 2482 | 2482 | 2482 |

*Note*: OLS estimates of equation (1) (col. 1-3 and 5-7) and (2) (col. 4 and 8). Dependent variable is the share of NSDAP votes. In all specifications we control for polling station and election fixed effects. Col. 2 and 6 include street characteristics interacted with a time trend. Col. 3 and 7 include street and demographic characteristics interacted with a time trend. Col. 4 and 8 include streets and demographic characteristics interacted with election fixed effects. Standard errors clustered at polling station level in brackets. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 3: Panel (a). First stage.

| | log distance to march | | Within 200m from march | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Optimal path: width based-naive | -0.167*** | -0.158*** | 0.065*** | 0.062** |
| | [0.054] | [0.056] | [0.024] | [0.024] |
| Street controls | Yes | Yes | Yes | Yes |
| Demographic controls | No | Yes | No | Yes |
| $R^2$ | 0.094 | 0.142 | 0.063 | 0.101 |
| Mean dep. var.: | -1.094 | -1.094 | 0.326 | 0.326 |
| F-test: excluded instrument | 9.4 | 7.8 | 7.6 | 6.4 |
| Observations | 571 | 571 | 571 | 571 |

Table 3: Panel (b). Reduced form and second stage.

| | % NSDAP vote | | | |
|---|---|---|---|---|
| | (1) RF | (2) RF | (3) 2SLS | (4) 2SLS |
| % households w/i 200m from march × post Nazi march | | | 5.413** | 5.419** |
| | | | [2.619] | [2.626] |
| Optimal path: width-naive × post Nazi march | 0.332** | 0.332** | | |
| | [0.147] | [0.147] | | |
| Election & polling station FEs | Yes | Yes | Yes | Yes |
| Street controls × time trend | Yes | Yes | Yes | Yes |
| Demographic controls × time trend | No | Yes | No | Yes |
| $R^2$ | 0.976 | 0.976 | 0.972 | 0.972 |
| Mean NSDAP vote in 10 Apr '32 election | 30.302 | 30.302 | 30.302 | 30.302 |
| Rubin-Anderson test for weak IV (p-value) | | | 0.024 | 0.024 |
| Observations | 2284 | 2284 | 2284 | 2284 |

*Note*: Panel (a): First stage estimates of equations (3a) (col. 1-2) and (3b) (col. 3-4). Col. 1-2: dependent variable is share of households within 200m from the march. See text for the construction of the instrument, the street width optimal path shift. Col. 3-4: dependent variable is the log of the average distance to Nazi marches. Col. 1 and 3 include street characteristics. Col. 2 and 4 include street and demographic characteristics. Robust standard errors in parentheses. Panel (b): OLS estimates of the reduced form equation (4) (col. 1-2) and IV estimates of equation (1) using the street width optimal path shift as instrument. Dependent variable is the share of NSDAP votes. See text for the construction of the instrument, the street width optimal path shift. In all specifications we control for polling station and election fixed effects. Col. 1 and 3 include street characteristics interacted with a time trend. Col. 2 and 4 include street and demographic characteristics interacted with a time trend. Standard errors clustered at polling station level in brackets. * p < 0.10, ** p < 0.05, *** p < 0.01.

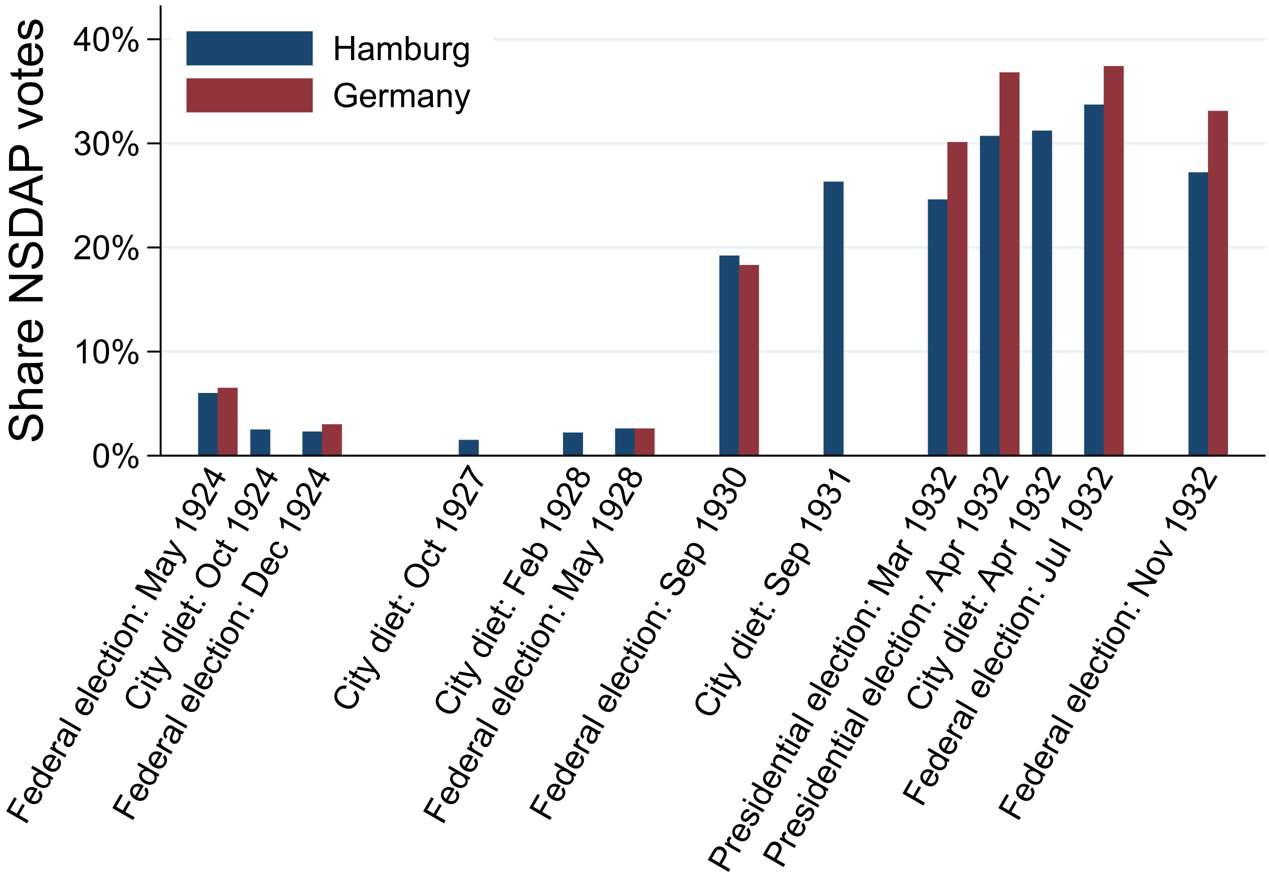
Table 4: Contagion. Effect of social distance to marches: difference-in-differences.

| | % NSDAP votes | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Difference in social distance × post Nazi march | -1.234*** | -1.234*** | -1.248*** | -1.248*** |
| | [0.100] | [0.108] | [0.100] | [0.108] |
| log distance Nazi march × post Nazi march | | 0.002 | | -0.004 |
| | | [0.194] | | [0.195] |
| Election & polling station FEs | Yes | Yes | Yes | Yes |
| Street controls × time trend | Yes | Yes | Yes | Yes |
| Demographic controls × time trend | No | No | Yes | Yes |
| $R^2$ | 0.978 | 0.978 | 0.979 | 0.979 |
| Mean NSDAP vote in 10 Apr '32 election | 28.970 | 28.970 | 28.970 | 28.970 |
| Observations | 1720 | 1720 | 1720 | 1720 |

*Note*: OLS estimates of equation (1) where we substitute the direct exposure variable (distance to the march) with the standardized difference between social distance to treated polling stations and social distance to untreated polling stations. Treatment is defined as a dummy = 1, if more than 80% of households are located within 200m of the Nazi march. Sample includes only non-treated polling stations. Dependent variable is the share of NSDAP votes. In all specifications we control for polling station and election fixed effects. Col. 1-2 include street characteristics interacted with a time trend. Col. 3-4 include street and demographic characteristics interacted with a time trend. Col. 2 and 4 include the log of the average distance to the Nazi marches. Standard errors clustered at polling station level in brackets. * p < 0.10, ** p < 0.05, *** p < 0.01.

# A Appendix Figures and Tables

Figure A.1: City and National Election Results in Hamburg, NSDAP, 1924-1932.




*Note*: The Figure reports the NSDAP vote share in Germany and Hamburg between 1924 and 1932. Sources: Germany (Falter, 1986); Hamburg (Bttner, 1982).

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

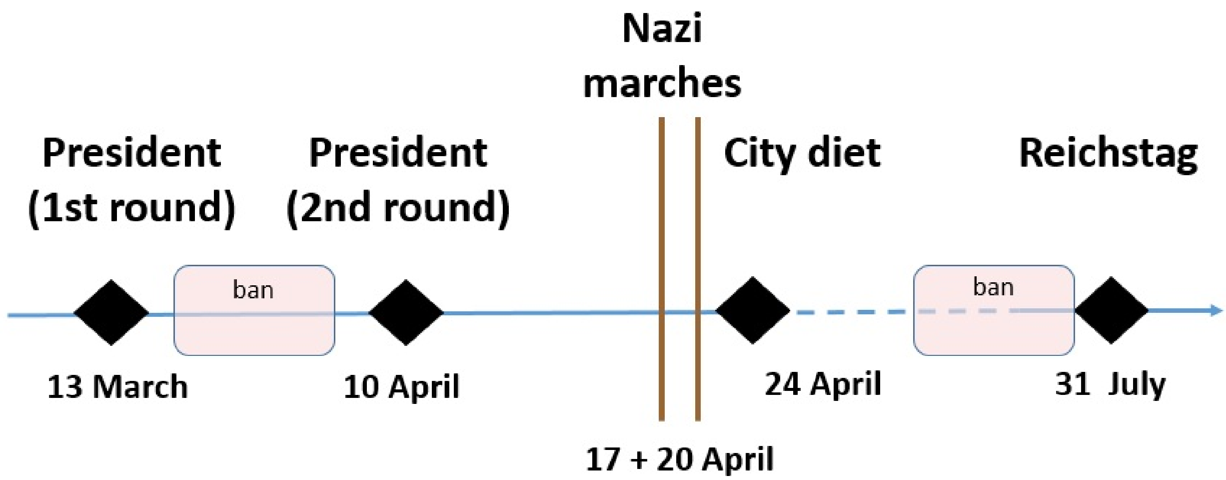
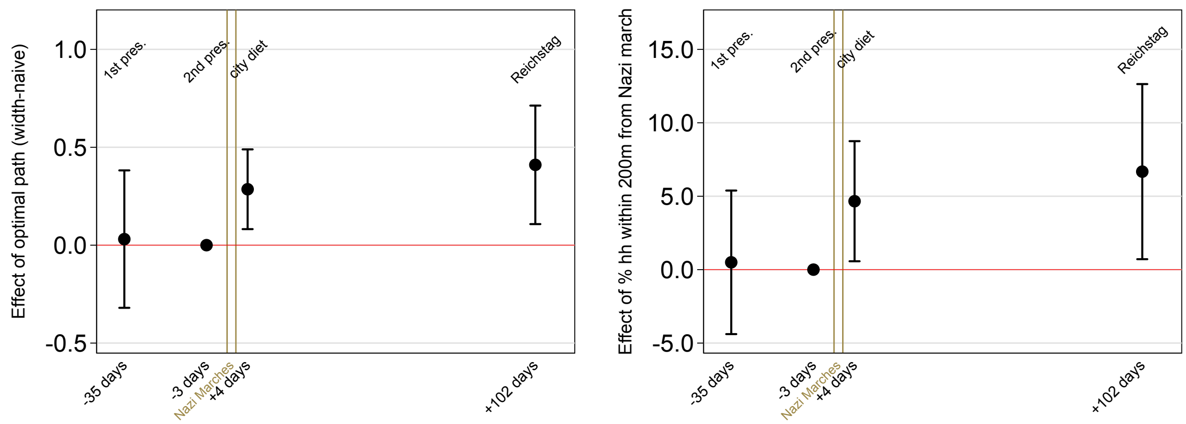


Figure A.3: Reduced form and two-stages least squares: flexible specification.

(a) Reduced form (b) Two-stages least squares




*Note*: Panel (a): flexible specification of the reduced form. The Figure plots the coefficients $\beta_t$ from the following regression:

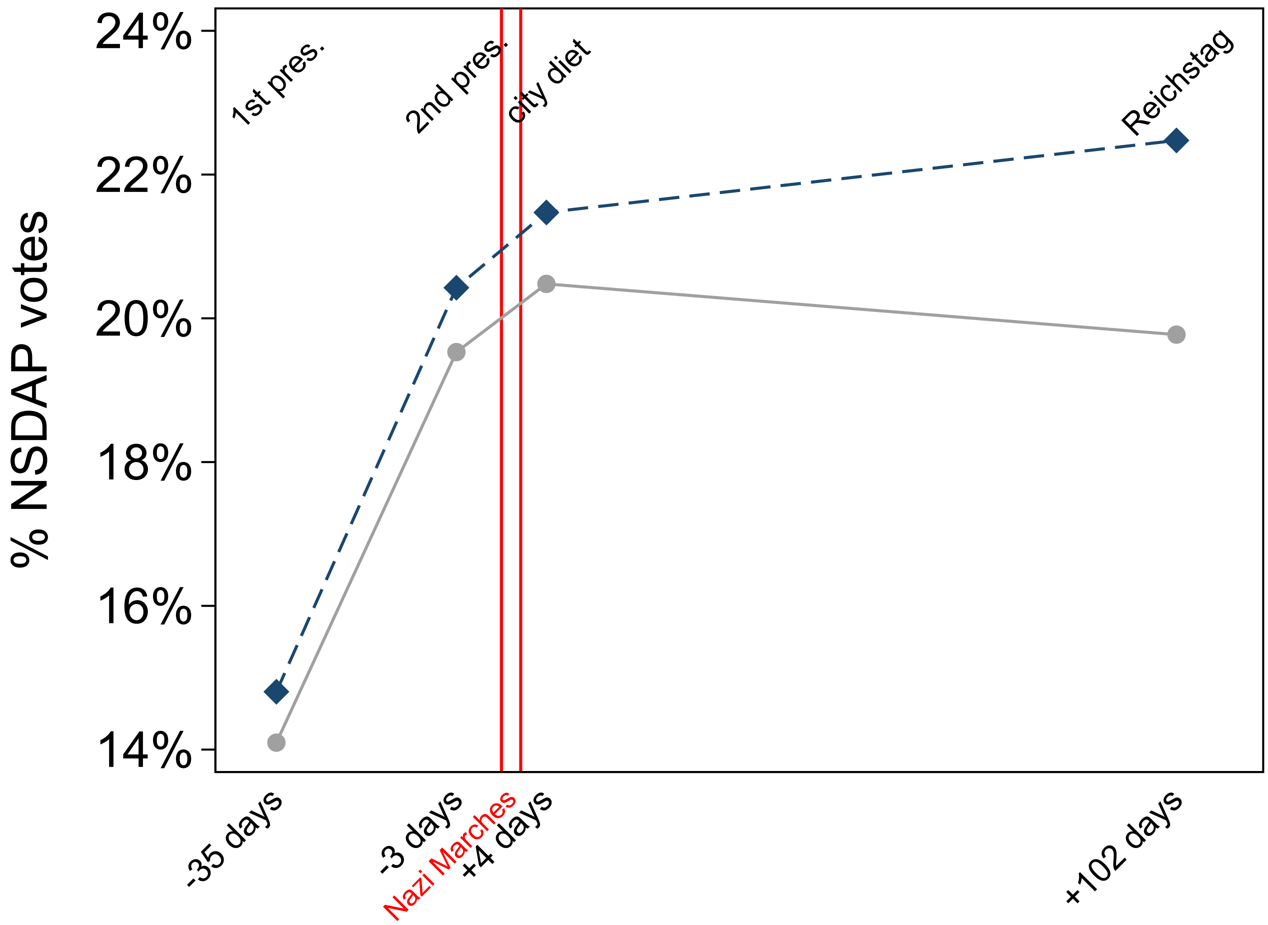
$$\text{NSDAP}_{it} = \alpha_i + \delta_t + \sum_{t=13\text{Apr}}^{31\text{Jul}} \pi_t \times \Delta\left[\text{path\_wide}_i - \text{path\_naïve}\right] + \sum_{t=13\text{Apr}}^{31\text{Jul}} \gamma_t X_i + u_{it}$$

See text for a description of the variables. We estimate standard errors clustered at the polling station level and plot 95% confidence intervals as bars around the coefficients. Panel (b): flexible specification of the IV. The Figure plots the coefficients $\beta_t$ from the following regression:

$$\text{NSDAP}_{it} = \alpha_i + \delta_t + \sum_{t=13\text{Apr}}^{31\text{Jul}} \beta_t \times \text{march\_treated}_i + \sum_{t=13\text{Apr}}^{31\text{Jul}} \gamma_t X_i + u_{it}$$

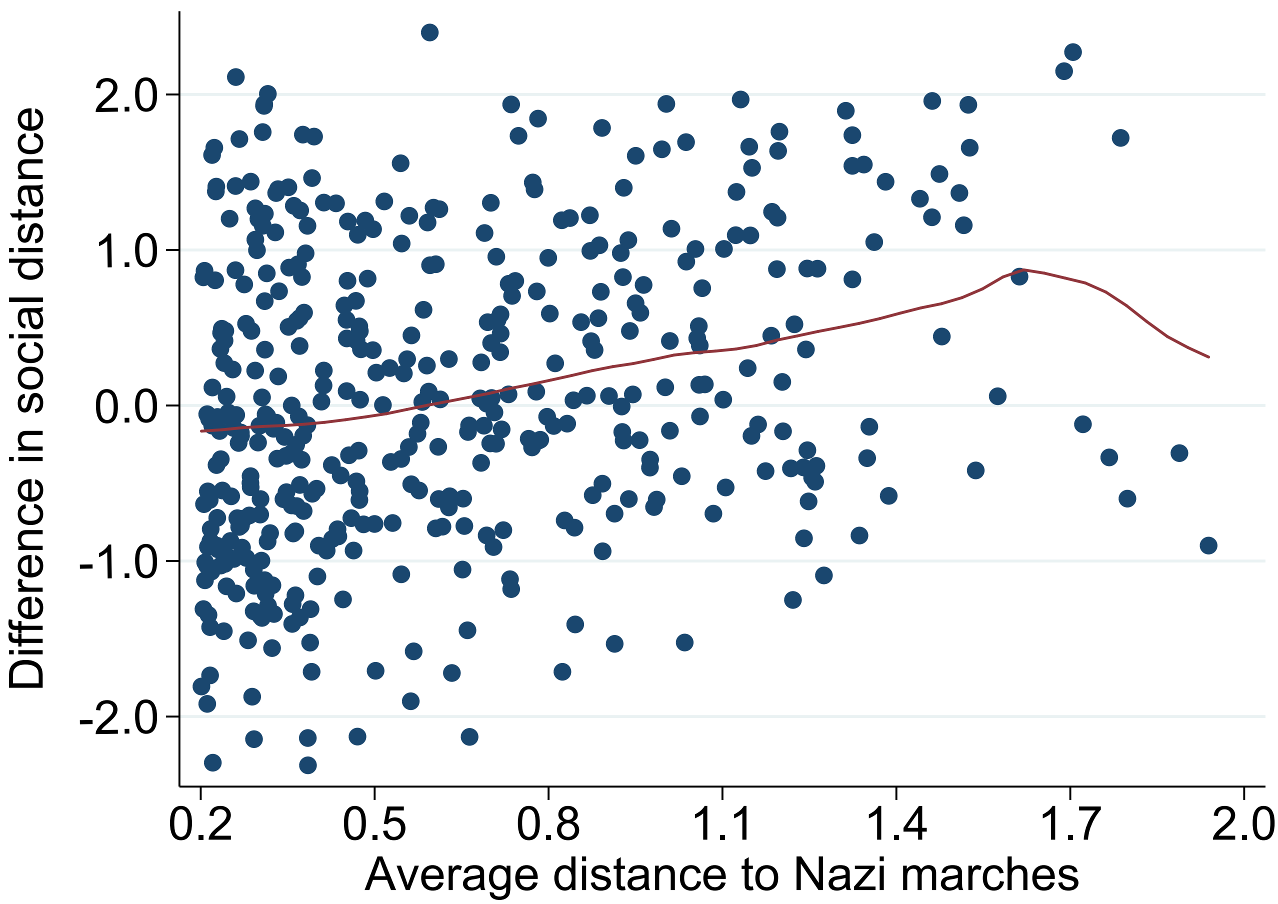
Where we instrument *march_treated* with $\Delta\left[\text{path\_wide}_i - \text{path\_naïve}\right]$. See text for a description of the variables. We estimate standard errors clustered at the polling station level and plot 95% confidence intervals as bars around the coefficients.

Figure A.4: Social distance and NSDAP voting - an example with two polling stations.




*Note*: The Figure reports the NSDAP vote share during the 4 election rounds for two polling stations: Steinbeckerstr. 30 (district Hamm in the South-East, in gray) and Krüsistr. 1 (district Barmbeck in the North-East, in blue). Both polling stations lie at more than 1 Km from the closest Nazi march (1.1 Km Steinbeckerstr. 30; 1.3 Km Krüsistr. 1). Krüsistr. 1 is closer to treated polling stations than Steinbeckerstr. 30.

Figure A.5: Physical and social distance to the marches.




*Note*: The Figure plots a scatterplot between the standardized difference in social distance (y-axis) and the average physical distance to the Nazi marches (x-axis). Treatment variable is dummy = 1 if more than 80% of households assigned to polling station are located within 200m of Nazi march. The sample only includes non-treated polling stations. We fit a local polynomial to the data.

Table A.1: Balance of instrument.

| | % NSDAP | | Δ% NSDAP | | | % household with: | | Nazi rallies | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | 13 Mar | 10 Apr | 13 Mar-10 Apr | log voters | % workers | telephone | heating | before 17 Apr | log rent 1931 |
| Optimal path: width based-naive | 0.492 | 0.438 | -0.053 | -0.000 | -1.304 | -0.095 | 0.026 | 0.010 | -0.050 |
| | [0.481] | [0.606] | [0.154] | [0.009] | [0.809] | [0.750] | [0.748] | [0.014] | [0.054] |
| Constant | 24.180*** | 30.265*** | 6.085*** | 7.155*** | 35.873*** | 12.148*** | 6.124*** | 0.069*** | 9.125*** |
| | [0.341] | [0.424] | [0.112] | [0.006] | [0.605] | [0.530] | [0.533] | [0.011] | [0.126] |
| $R^2$ | 0.002 | 0.001 | 0.000 | 0.000 | 0.005 | 0.000 | 0.000 | 0.001 | 0.008 |
| Mean DV | 24.221 | 30.302 | 6.081 | 7.154 | 35.764 | 12.141 | 6.126 | 0.070 | 9.120 |
| Observations | 571 | 571 | 571 | 571 | 571 | 571 | 571 | 571 | 410 |

*Note*: Balance of the street width optimal path shift instrument (see text for construction of instrument) relative to pre-existing characteristics. Each column reports the coefficient of a regression on the instrument. Dependent variable is listed in the column header. Standard errors in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Contagion. Effect of social distance to marches: flexible specification.

| | % NSDAP votes | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Difference in social distance × 31 July (post) | -0.724*** | -0.760*** | -0.051 | -0.074 |
| | [0.143] | [0.152] | [0.218] | [0.223] |
| Difference in social distance × 24 April (post) | -0.179 | -0.201* | -0.179 | -0.201* |
| | [0.110] | [0.117] | [0.110] | [0.117] |
| Difference in social distance × 13 March (pre) | 1.519*** | 1.493*** | 0.142 | 0.119 |
| | [0.120] | [0.125] | [0.174] | [0.178] |
| Election & polling station FEs | Yes | Yes | Yes | Yes |
| Street controls × election FEs | Yes | Yes | Yes | Yes |
| Demographic controls × election FEs | No | No | Yes | Yes |
| Log average distance to march × election FEs | No | Yes | No | Yes |
| $R^2$ | 0.982 | 0.982 | 0.986 | 0.986 |
| Mean NSDAP vote in 10 Apr '32 election | 28.970 | 28.970 | 28.970 | 28.970 |
| Observations | 1720 | 1720 | 1720 | 1720 |

*Note*: Dependent variable is the share of NSDAP votes. In all specifications we control for polling station and election fixed effects. Treatment variable is dummy = 1 if more than 80% of households assigned to polling station are located within 200m of Nazi march. The sample only includes non-treated polling stations. Col. 1 and 2 include street characteristics interacted with election fixed effects. Col. 3 and 4 include street and demographic characteristics interacted with election fixed effects. Col. 2 and 4 additionally include log of average distance to Nazi march interacted with election fixed effects. Standard errors clustered at polling station level in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Robustness. Standard errors corrected for spatial autocorrelation.

| | % NSDAP vote | | |
|---|---|---|---|
| | (1) DiD | (2) RF | (3) IV |
| Nazi march w/i 200m × post Nazi march | 1.517 | | 5.419 |
| Baseline: s.e. clustered at polling station level | [0.278]*** | | [2.626]** |
| Conley (1999) s.e.: cutoff at 200m | [0.250]*** | | [2.735]** |
| Conley (1999) s.e.: cutoff at 500m | [0.276]*** | | [3.007]* |
| Conley (1999) s.e.: cutoff at 1 Km | [0.308]*** | | [3.143]* |
| Conley (1999) s.e.: cutoff at 1.5 Km | [0.335]*** | | [3.106]* |
| Optimal path: width-naive × post Nazi march | | 0.332 | |
| Baseline: s.e. clustered at polling station level | | [0.147]** | |
| Conley (1999) s.e.: cutoff at 200m | | [0.131]** | |
| Conley (1999) s.e.: cutoff at 500m | | [0.144]** | |
| Conley (1999) s.e.: cutoff at 1 Km | | [0.153]** | |
| Conley (1999) s.e.: cutoff at 1.5 Km | | [0.159]** | |
| Observations | 2482 | 2284 | 2284 |

*Note*: 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) and 1.5km (row 5). Dependent variable is share NSDAP votes. Col. 1: OLS estimates of regression on share of households within 200m of Nazi march. Col. 2: Reduced form estimates of regression on width optimal path shift instrument (see text for construction of instrument). Col. 3: IV estimates. Clustered and corrected standard errors in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Robustness. Results excluding polling stations that changed address.

| | % NSDAP vote | | |
|---|---|---|---|
| | (1) DiD | (2) RF | (3) 2SLS |
| % households w/i 200m from march × post Nazi march | 1.471*** | | 6.167** |
| | [0.283] | | [2.761] |
| Optimal path: width-naive × post Nazi march | | 0.406*** | |
| | | [0.151] | |
| Election & polling station FEs | Yes | Yes | Yes |
| Street controls × time trend | Yes | Yes | Yes |
| Demographic controls × time trend | Yes | Yes | Yes |
| $R^2$ | 0.977 | 0.976 | 0.970 |
| Mean dep. var.: | 30.467 | 30.316 | 30.316 |
| Rubin-Anderson test for weak IV (p-value) | | | 0.007 |
| Observations | 2366 | 2180 | 2180 |

*Note*: Robustness: regressions on sub sample of polling stations with stable addresses. 29 polling stations change their address at least once between the different elections. See statistical bulletin of Hamburg (Skllin, 1932) for list of polling station addresses in each election. Dependent variable is the share of NSDAP votes. Sample in all columns excluding polling station that change their address at least once. In all specifications we control for polling station and election fixed effects, for street controls interacted with a time trend and demographic controls interacted with a time trend. Col. 1: OLS estimates of regression on share of households within 200m of Nazi march . Col. 2: Reduced form estimates of regression on width optimal path shift instrument (see text for construction of instrument). Col. 3: IV estimates. Standard errors clustered at polling station level in brackets. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.5: Nearest neighbor match: first difference results.

| | Δ % NSDAP vote (before-after) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SATT | 0.565*** | 0.551*** | 0.558*** | 0.640*** | 0.548*** | 0.672*** |
| | [0.203] | [0.206] | [0.178] | [0.187] | [0.175] | [0.186] |
| 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 from before to after the Nazi marches. 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Panel (a). Entropy matching: balance before and after re-weighting

| | Before re-weighting | | After re-weighting | |
|---|---|---|---|---|
| | Control | Treated | Control | Treated |
| log voters | 7.151 | 7.163 | 7.163 | 7.163 |
| | [0.021] | [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.6: Panel (b). Results from matching exercises.

| | % NSDAP vote | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Base | Entropy | CEM |
| >80% hh w/i 200m from march × post Nazi march | 0.766*** | 0.762*** | 0.862*** |
| | [0.194] | [0.188] | [0.204] |
| Election & polling station FEs | Yes | Yes | Yes |
| Street controls × election FEs | Yes | Yes | Yes |
| Demographic controls × election FEs | Yes | Yes | Yes |
| $R^2$ | 0.987 | 0.987 | 0.987 |
| Mean dep. var.: | 30.417 | 30.417 | 30.823 |
| Observations | 2482 | 2482 | 1896 |

*Note*: Robustness: entropy re-weighting 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 re-weighting and Coarsened Exact Matching. Dependent variable is the share of NSDAP votes. Col. 1: baseline estimates. Col. 2: estimates after entropy re-weighting. Col. 3: estimates on the sub-sample of polling stations matched by the Coarsened Exact Matching algorithm. We find exact matched 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. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A.7: Robustness. Effect of social distance to marches for different direct treatment definitions: difference-in-differences.

| | % NSDAP votes | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **Panel (a): >90% cutoff** | | | | |
| Difference in social distance (>90% def) × post Nazi march | -0.862*** | -0.798*** | -0.873*** | -0.808*** |
| | [0.089] | [0.094] | [0.089] | [0.093] |
| Mean NSDAP vote in 10 Apr '32 election | 29.920 | 29.920 | 29.920 | 29.920 |
| Observations | 2206 | 2206 | 2206 | 2206 |
| **Panel (b): >80% cutoff** | | | | |
| Difference in social distance (>80% def) × post Nazi march | -1.207*** | -1.199*** | -1.222*** | -1.213*** |
| | [0.089] | [0.096] | [0.088] | [0.095] |
| Mean NSDAP vote in 10 Apr '32 election | 29.640 | 29.640 | 29.640 | 29.640 |
| Observations | 2046 | 2046 | 2046 | 2046 |
| **Panel (c): >70% cutoff** | | | | |
| Difference in social distance (>70% def) × post Nazi march | -1.329*** | -1.343*** | -1.348*** | -1.361*** |
| | [0.090] | [0.095] | [0.090] | [0.095] |
| Mean NSDAP vote in 10 Apr '32 election | 29.292 | 29.292 | 29.292 | 29.292 |
| Observations | 1899 | 1899 | 1899 | 1899 |
| **Panel (d): >60% cutoff** | | | | |
| Difference in social distance (>60% def) × post Nazi march | -1.267*** | -1.281*** | -1.282*** | -1.295*** |
| | [0.099] | [0.105] | [0.098] | [0.105] |
| Mean NSDAP vote in 10 Apr '32 election | 28.950 | 28.950 | 28.950 | 28.950 |
| Observations | 1787 | 1787 | 1787 | 1787 |
| **Panel (e): >50% cutoff** | | | | |
| Difference in social distance (>50% def) × post Nazi march | -1.207*** | -1.220*** | -1.221*** | -1.234*** |
| | [0.106] | [0.115] | [0.106] | [0.114] |
| Mean NSDAP vote in 10 Apr '32 election | 28.878 | 28.878 | 28.878 | 28.878 |
| Observations | 1664 | 1664 | 1664 | 1664 |
| Election & polling station FEs | Yes | Yes | Yes | Yes |
| Street controls × time trend | Yes | Yes | Yes | Yes |
| Demographic controls × time trend | No | No | Yes | Yes |
| Physical distance to march × post Nazi march control | No | Yes | No | Yes |

*Note*: Robustness: changing treatment definition for the computation of social distance. OLS estimates of equation (1) where we substitute the direct exposure variable (distance to the march) with the standardized difference between social distance to treated polling stations and social distance to untreated polling stations. We define treatment as a dummy = 1, if more than a given percentage of households are located within 200m of the Nazi march: each panel report results with a different cutoff, from 90% (panel a) to 50% (panel e). Sample includes only non-treated polling stations. Dependent variable is the share of NSDAP votes. In all specifications we control for polling station and election fixed effects. Col. 1-2 include street characteristics interacted with a time trend. Col. 3-4 include street and demographic characteristics interacted with a time trend. Col. 2 and 4 include the log of the average distance to the Nazi marches. Standard errors in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.