American Economic Journal: Applied Economics 2026, 18(1): 120–159
https://doi.org/10.1257/app.20180816

Highway to Hitler†

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We show that the building of the Autobahn network in Nazi Germany boosted popular support for Adolf Hitler, helping to entrench the Nazi dictatorship. Direct local economic benefits are unlikely to explain the effect. Instead, it reflects successful propaganda: The regime portrayed the Autobahn as a symbol of recovery and the end of austerity. Support rose particularly strongly where highway construction coincided with greater radio availability and in politically unstable regions. Our findings suggest that visible infrastructure projects can raise support for autocratic regimes when voters are led to associate them with economic progress and an end to political instability. (*JEL* D72, E32, H54, N44, N94, R42, R53)

> [W]hile a decade of Weimar parliaments had produced only talk and sketches, a mere three years of National Socialism had built a thousand kilometres of traversable superhighways. … Their very existence seemed to verify the Nazi thesis that the state must be given a free hand, if it were to restore Germany to her former glory. (Shand 1984, 194)

In the last 200 years, democracy has spread around the globe. In 1816, less than 1 percent of the world population lived in democracies. In 2015, the figure stood at 58 percent. However, progress has not been a one-way street: Between 1920 and 1938, the number of democratic states in the world fell from 20 to 13, and the share of the population living in democracies declined from 20.5 percent to 14.8 percent (Boix, Miller, and Rosato 2012; Vanhanen 2010).[1] Nor is democratic fragility necessarily an issue of the past: Ever fewer young Europeans and US citizens consider it essential to live in a democracy. In the United States, their share declined from more than 70 percent among the 1930s birth cohort to about 30 percent for the 1980s cohort (Foa and Mounk 2016).[2]

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† Go to https://doi.org/10.1257/app.20180816 to visit the article page for additional materials and author disclosure statement(s).

[1] Along the same lines, Birdsall and Fukuyama (2011) observe that "political leaders in the developing world now associate efficiency and capability with autocratic political systems."

[2] The question asked is whether respondents feel "it is essential to live in a country that is governed democratically," where "feeling strongly" corresponds to a rating of 10 on a ten-point scale.

For democracy to fail, autocracy has to triumph. Authoritarian leaders rarely rule by force alone, and many rely on popular support (Egorov and Sonin 2014). How do authoritarian rulers convince voters to discard and disregard their democratic rights? The decline of democracy is often associated with (perceived) social disorder. In these circumstances, dictatorships may look appealing to the masses because of their capacity to restore order (Finer 2002; Djankov et al. 2003). As Easterly and Pennings (2016) noted, the view that economic growth is easier to engineer under strong autocratic regimes has recently gained in prominence, with China and Singapore often cited as leading examples.[3] Similarly, 30 percent of US respondents in the 2010 World Value Survey thought that it would be "good or very good to have a strong leader" that does not have to "bother with parliaments and elections" (Foa and Mounk 2016). But do shows of efficacy indeed buy support for budding dictators? It may instead be true that entrenched dictators are more effective rather than effective dictators becoming entrenched.

In this paper, we aim to identify the causal effect of effective policy implementation on autocratic consolidation. We study a concerted effort showcasing an authoritarian state's ability to accomplish ambitious goals: the construction of the Autobahn in Nazi Germany, the world's first high-speed road network. Initially, the Nazi grip on power was far from absolute. In an election that was neither free nor fair in November 1933, many voters opposed the Nazi regime—in several major cities, more than a quarter of votes were cast against the Nazi Party (NSDAP, Nationalsozialistische Deutsche Arbeiterparte).[4] By mid-1934, the regime's popularity was waning amongst the middle class; conservative elites were dismayed by Nazi lawlessness, and President Paul von Hindenburg threatened military rule.[5] And yet, by the late 1930s, the Nazi regime had become one of the most popular in German history—a "consensual dictatorship."[6] As late as 1955, almost half of all Germans opined that Hitler would have been "one of the greatest German statesmen had it not been for the war" (Möbius 2013, 257).[7]

A key turning point in the regime's fortunes came in August 1934, after President von Hindenburg's death. Hitler became both chancellor and president, concentrating singular authority in the hands of the führer.[8] This increase in power was overwhelmingly endorsed in a popular referendum. We argue that Autobahn construction

[3] See also Friedman (2009). Jones and Olken (2005) show that turnover in the leadership of autocracies leads to sharp changes in economic performance; Besley and Kudamatsu (2008) model the institutional features of autocracies that can increase their chances of success.

[4] Voting results from the Nazi period cannot be taken at face value. Intimidation was massive, and there is some (limited) evidence of fraud. Nonetheless, we argue that information about popular support can be extracted from vote shares. "Yes" votes did not necessarily reflect genuine support—but "no" votes were a clear sign of opposition. "No" votes varied importantly over time and space. Even large cities recorded substantial differences: In Aachen in 1934, for example, 24 percent voted "no"; in Nuremberg, on the other hand, only 4.6 perent voted against Hitler becoming both chancellor and president

[5] *Time* magazine in July 1934 quoted Adolf Hitler as saying, "Don't forget how people laughed at me 15 years ago when I declared that one day I would govern Germany. They laugh now, just as foolishly, when I declare that I shall remain in power!"

[6] Aly (2005) and Bajohr (2005).

[7] In another survey, some 10 percent opined that even with the war, Hitler was the greatest German statesman of all time, whose eminence would only be accepted in the future (Noelle and Neumann 1956, 135).

[8] In addition to the referendum, the wholesale murder of the storm trooper (SA) leadership and other prominent anti-Nazis in the Night of the Long Knives allowed Hitler to consolidate his powers.

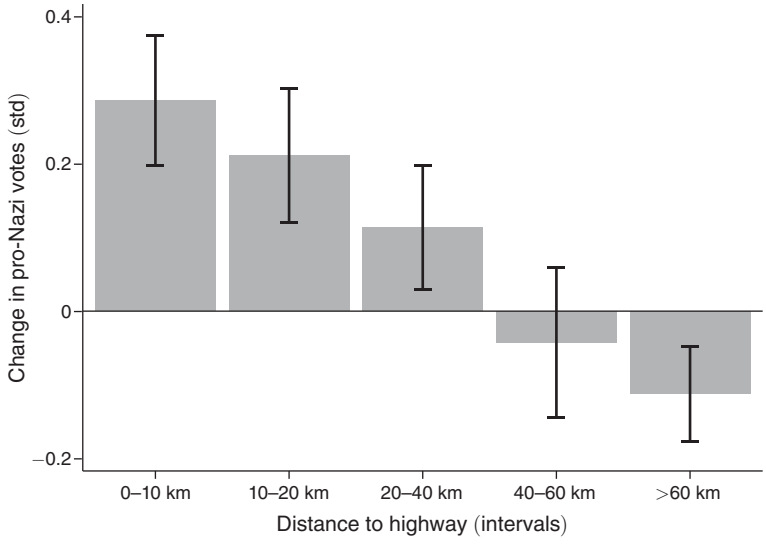

Figure 1. Change in Nazi Support, November 1933 and August 1934, by Distance to Highway

Notes: The figure shows the difference in standardized pro-Nazi votes between the November 1933 election and the August 1934 referendum, for different distance brackets to highway segments under construction (approximately corresponding to distance quintiles). Bars indicate the average change in (standardized) Nazi support; the black lines, the 95 percent confidence interval.

contributed markedly to the regime's popularity, as reflected in major support for the referendum.[9] Since the last election, held just ten months before, road construction had begun in earnest. Opposition against the regime declined significantly in locations where the new roads were being built. Figure 1 illustrates our main finding. It shows the change in support for the Nazi regime by distance to Autobahn construction.[10] The Nazis gained more support the closer that locations were to highway construction. Our results suggest persuasion rates of 8–17 percent (the share of voters previously opposed to the Nazis who then changed their minds because of Autobahn construction)—a high value compared with other studies (Della Vigna and Gentzkow 2010).[11]

Motorway planning may have followed a political lead after 1933. To deal with potential endogeneity, we construct least-cost paths (LCPs) between terminal cities connected by highways. Building costs reflect geological characteristics such as the steepness of the terrain, the number of rivers to be traversed, and so forth. We use

[9] Saiz (2006) notes a close association between dictatorships and highways but argues that this is explained by their use as instruments of repression.

[10] Since the election in November 1933 and the referendum in August 1934 are not directly comparable, we use the difference in standardized vote shares with mean zero and standard deviation one.

[11] Our results reflect local differences in voting; for the country as a whole, effects may well have been larger because people did not only react to changes at the local level—but also because they saw progress in the country as a whole.

these LCPs as an instrument for actual construction, excluding the terminal cities themselves from the analysis. Our IV results confirm the OLS estimates both in terms of magnitude and statistical significance.

What accounts for the Autobahn's success in winning electoral support for Hitler's regime? We argue that highway construction worked because it convinced voters of the Nazi regime's "competence," mainly by seeming to solve Germany's key economic problem: unemployment. Between 1933 and 1934, nationwide unemployment fell by half (Humann 2011). However, the roads' contribution to this success was limited; their construction employed relatively few workers, and a cyclical upswing had already begun to reduce unemployment before roadbuilding commenced (Ritschl 1998). Correspondingly, we find that unemployment fell at a similar rate in cities near highway construction and those farther away. Nevertheless, Josef Goebbels's propaganda effectively sold the notion that Nazi roadbuilding was reviving the German economy.[12] Thus, news that the highway was coming was enough to sway voters: We find that approved highway segments (where people knew construction would start shortly but work had not begun) had similar effects on Nazi support as highway segments already under construction.

We provide further support that propaganda and highway construction complemented each other—where radio signal strength was high and the new roads were under construction, pro-Nazi votes increased particularly strongly. In contrast, without radio coverage, the roads themselves had a negligible effect on voting behavior. One interpretation of these results is that Autobahn construction in combination with radio propaganda won "hearts and minds" of voters; another possibility is that the regime's capacity to see through a major construction project also raised the expected cost of opposition.[13] Which of these two channels dominated is not crucial for the main purpose of the referendum in 1934—showcasing almost uniform popular support for Hitler, thereby signaling the regime's popularity and stability (Evans 2005; Egorov and Sonin 2014).

Autobahn building also showcased the ability to get things done.[14] Hitler announced the plans for new roads shortly after coming to power; within nine months, he broke ground on the first stretch of motorway. Demonstrations of government competence were particularly attractive in areas where political turmoil had reigned. Weimar's federal states with more unstable government in 1919–1933

[12] In other words, the Nazi propaganda successfully attributed economic performance to its economic policies—a process that is generally challenging because of economic volatility (Buera, Monge-Naranjo, and Primiceri 2011).

[13] The latter explanation is less likely given the regime's wild popularity in later years. Raising the perceived cost of opposition would be a form of *implicit* intimidation. Explicit intimidation and fraud, on the other hand, are unlikely explanations for our finding, as we argue in Supplemental Appendix A.4 (where we perform a number of "election forensics" tests and find no evidence for a relationship between Autobahn construction and fraud). Importantly, the Nazis were already in power during the November 1933 election. This makes it less likely that cross-sectional differences in intimidation or fraud accounted for electoral success; only a *differential* increase in (explicit) intimidation or fraud in areas with Autobahn construction could contaminate our results.

[14] It also demonstrated an effective end of the austerity policies of the pre-1933 era that had been implemented by successive Weimar governments (Shand 1984).

showed systematically larger vote-winning effects of highway construction.[15] Regime propaganda also exploited the highways as powerful symbols of an energetic government overcoming "democratic gridlock" and the widely lamented disorder of the Weimar Republic (Evans 2005).

The paper proceeds as follows. We review the related literature in Section I and then explain the historical background and context of motorway building in Section II. Section III presents our data. In Section IV, we show the main empirical results. Section V presents instrumental variable results, and Section VI examines channels through which the Nazi regime's roadbuilding influenced voting. Section VII demonstrates the robustness of our findings, and Section VIII concludes.

I. Related Literature

Our work contributes to research on the political economy of regime change (Acemoglu and Robinson 2000), elections and the entrenchment of dictatorships (Egorov and Sonin 2014; Simpser 2013; Jessen and Richter 2011), the role of elections in autocracies (Gandhi and Lust-Okar 2009), and interactions between the military and old elites (Finer 1976; Acemoglu, Ticchi, and Vindigni 2010). Closely linked is work on the origins of totalitarian dictatorships, much of which emphasizes differences between normal autocracies and regimes like the Nazi dictatorship or Communist rule in Russia. Theories of "mass society" focus on industrialization and the associated rise of a large group of economically marginal individuals who have lost their traditional roots (Ortega y Gasset 1993; Arendt 1973). These in turn are said to create a fertile recruiting ground for totalitarian ideology, from both the left and the right.[16] Schmitt (1926), on the other hand, emphasized the need for an—alleged—external or internal threat for totalitarian states to consolidate.

Our research also relates to the rich literature on the electoral benefits of income transfers and infrastructure projects. There is evidence that politically motivated income transfers and federal spending can affect voting behavior (Manacorda 2011; Levitt and Snyder 1997), but aggregate patterns are often inconclusive (Stein and Bickers 1994). In a classic paper, Berman, Shapiro, and Felter (2011) examine the conditions under which an occupying force can win the "hearts and minds" of the occupied, and conclude that public service provision can have a decisive effect in reducing opposition. Also, public spending is often targeted at areas with a more informed electorate (Strömberg 2004).[17] Recent research has also generated new insights into the economic effects of major infrastructure projects. While an

[15] Speeches by conservative politicians—and not only the Nazis—frequently referred to an alleged lack of speed and decisiveness in democratic decision-making. Indeed, even before Hitler's seizure of power, plans for a new, authoritarian constitution were being proposed by many conservative politicians (cf. Stackelberg and Winkle 2013).

[16] Applications of this approach to the German context include Shirer (1960) and Stern (1972).

[17] Along the same lines, Finan and Mazzocco (2014) show that politicians with greater electoral incentives transfer more resources to areas where they expect higher political returns. Larreguy, Marshall, and Trucco (2015) show how central-government policy interventions boost support for the federal incumbent while reducing the influence of local politicians.

early literature had concluded that the invention of the railways did not matter significantly for growth (Fogel 1964), there is now ample evidence that, for example, the building of India's national railway network reduced transport costs and increased trade (Donaldson 2018). Similarly, better access to transport infrastructure in China and Prussia boosted GDP (Banerjee, Duflo, and Qian 2012; Hornung 2015)[18] and increased land values (Donaldson and Hornbeck 2016). There are also well-documented effects on urban layout (Baum-Snow 2007), city growth (Duranton and Turner 2012), and skill premia in urban areas (Michaels 2008).

Relative to the existing literature, we make a number of contributions: First, we demonstrate the political benefits of infrastructure spending on electoral outcomes, helping to entrench the Nazi dictatorship. At a crucial moment, when the Hitler regime needed to showcase its popularity, Autobahn building boosted support. We thus contribute to the literature that studies regime change in general and the rise of the Nazis in Germany more specifically (King et al. 2008; Bracher 1978). Second, we provide new evidence of the channels through which infrastructure spending can make a difference. In the Nazi case, there were real economic benefits—but they are too small to account for the overall gains in electoral support. Instead, we show that propaganda can magnify the effects of locally successful policies, convincing voters far and wide that the new regime is getting the country out of its slump. Third, we offer suggestive evidence on the conditions under which elections can boost support for an autocratic regime—a key question in the literature on voting in nondemocratic settings (Gandhi and Lust-Okar 2009). We find that roadbuilding was most effective in swaying voters in areas that had previously supported moderate parties and in Catholic areas, which were typically skeptical of the Nazis. On the other hand, in areas with high support for the communists (such as worker strongholds), highways were less effective in garnering votes. Lizzeri and Persico (2001) argue that in electoral regimes where the margin of victory matters, public goods are more likely to be provided, and pork barrel spending is lower. Our result on the Nazi regime building highways is related, but it goes further. Goebbels's propaganda emphasized that roads are public goods. This generated important synergies with actual construction, enabling the regime to show near-universal support. In this sense, the Autobahn's success in boosting pro-regime votes relied more on a perceived "competence" channel (Rogoff 1990) than on any direct economic benefits. Thus, we provide evidence in support of arguments that autocracy may be attractive to voters because of its alleged ability to "get things done" (Djankov et al. 2003).

II. Historical Background

In this section, we briefly describe motivations behind the building of the Autobahn network and its antecedents. We also discuss the nature of early Nazi elections and the growing strength of the regime.

[18] In contrast, Faber (2014) finds adverse effects of highways on GDP growth in newly connected peripheral counties in China.

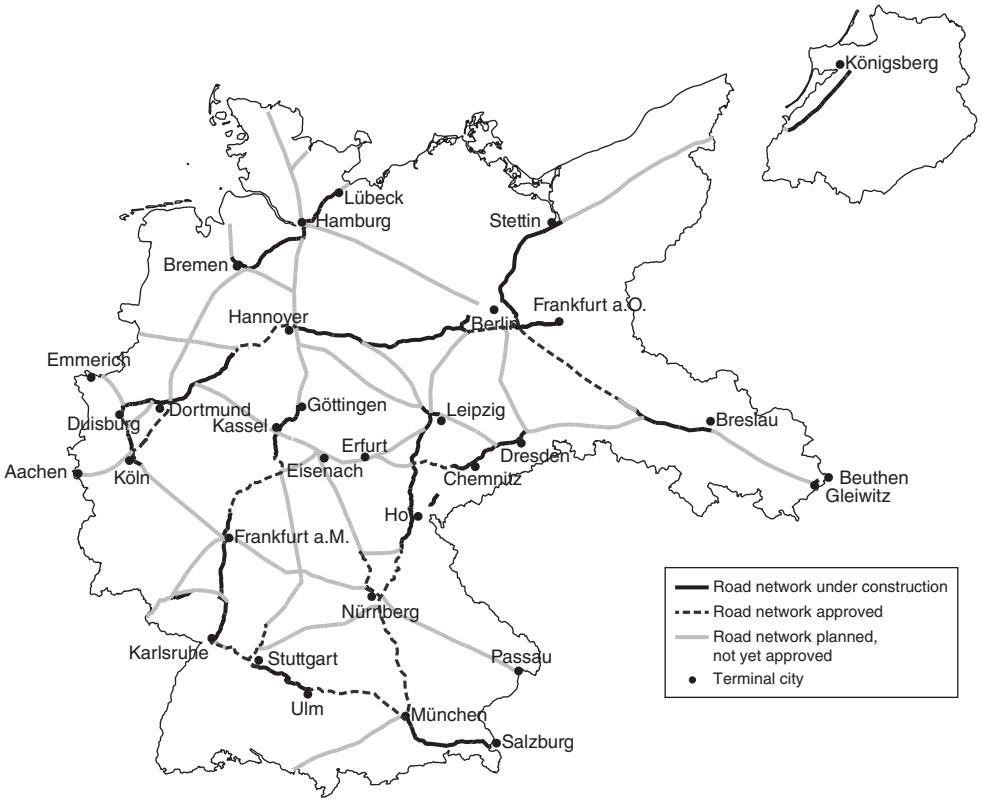

FIGURE 2. GERMAN HIGHWAY NETWORK BY 1934

Notes: Location of highway segments from Todt (1934). Map geocoded by authors.

A. *Motorway Building under the Nazis*

Roadbuilding became a government priority soon after the "seizure of power." At the Berlin Motor Show—only 11 days after becoming chancellor—Hitler presented a set of measures to boost car ownership and use, consisting of subsidies, roadbuilding, and cheaper cars.[19] By the summer of 1933, a new publicly owned company had been founded to build and operate highways Germany-wide. Plans for the network built on work by a think tank, the STUFA (Vahrenkamp 2010). In some cases, the trajectory of the actual roads was decided by Hitler himself, who insisted on scenic routes (Todt 1934).

Construction began at many points simultaneously. Figure 2 shows the 1934 highway network. Thick-black segments were under construction, double-ruled segments were approved for construction but not yet begun, and light-gray lines

[19] In the Rhineland, another—unrelated—project connected Bonn and Cologne. Konrad Adenauer, later chancellor of the Federal Republic of Germany, coordinated the building in a bid to reduce unemployment. This first highway opened in 1932. At the time, Italy had already completed the first high-speed roads reserved for car traffic.

indicate planned segments not yet approved for construction.[20] Segments under construction and trajectories approved for building had high local visibility: Actual work was taking place, and the path of the new road was officially announced and staked out. Planned segments, in contrast, were only known to the planning officials. In 22 locations, construction was under way less than a year after the start of the project. None of the highway segments were actually open for traffic by the time of the plebiscite in August 1934.

Large-scale construction only began after the election in November 1933—a fact that we exploit in our empirical analysis. Figure A.1 in the Supplemental Appendix shows employment in Autobahn construction, by month, for the period 1933–1934 (Humann 2011). Employment in November 1933 was 3,000 men, 5 percent of the level reached by August 1934. Earlier months saw even lower numbers of workers. From April 1934, at least 20,000 men were employed. In August, it reached 59,000. This was not yet the high-water mark of Autobahn employment, but it was higher than in any preceding month and equivalent to half of the all-time peak of employment (June 1936; 121,000 workers).

Together with rearmament, the Autobahn was a key part of Keynesian demand stimulus by the Hitler government. In line with the regime's propaganda, many observers took it for granted that building the new highway network reduced unemployment. John Maynard Keynes himself, in the German edition of his *General Theory*, argued that Nazi spending policies after 1933 exemplified the superiority of totalitarian regimes in implementing the "right" policies to overcome the slump (Keynes 1936).[21] Quantitative research has since established that neither military spending nor highway construction were probably responsible for Germany's recovery after 1933 (Ritschl 1998). Initially planned to employ up to 600,000 workers, motorway building never came close.[22] Instead, the rapid rise in output under Hitler is typically explained by the strength of a cyclical upswing, helped by an end to deflation and declining uncertainty.

Nazi propaganda exploited the motorway from the beginning. The regime emphasized highway construction as an integral part of its war on unemployment (*Arbeitsschlacht*).[23] Propaganda Minister Josef Goebbels ensured that work started simultaneously at many locations. This did not maximize use value but made construction work visible across the country (Shand 1984). Hitler turned the first sod of earth in September 1933. In his opening speech, he argued that the new roads would bear witness to "our [the regime's] devotion, our diligence, our ability, and our decisiveness" (Schütz and Gruber 1996). In the first month of the newly founded

[20] We digitized the September 1934 map from Todt (1934), which is the closest available to August 1934. The transition between highway segments "approved for construction" and "under construction" in Figure 2 is fluid, and even the historical maps are not completely clear about the exact timing when construction began. For example, a few smaller segments are listed as "under construction" in the May 1934 map but as "approved for construction" in the November 1934 map. We use "under construction" as our main "treatment" variable and document the robustness of results to including "approved for construction" in Section VIE. Whenever we refer to "highways" in the empirics, we mean segments that were listed as "under construction."

[21] Scholars from Karl Schiller (1936) to Richard Overy (1975) argued along similar lines.

[22] At its peak, only 121,000 Germans were working in highway construction (Humann 2011). This should be compared with a decline in unemployment from 6 million in January 1933 to 2.5 million in the summer of 1934.

[23] Literally, "battle for labor."

Autobahn company's existence, the *Völkischer Beobachter*—the leading Nazi Party paper—made construction progress front-page news no fewer than four times. Radio similarly played a prominent role—the start of construction was broadcast live to millions of listeners, including speeches by Hitler and Goebbels.

The regime celebrated the opening of each new stretch of motorway. The first segment near Frankfurt was opened by Hitler himself in May 1935, with 90,000 supporters lining the road. By 1936, some 1,000 kilometers (km) of road (out of 9,000 planned) had been finished. Each opening was covered on the radio, in the press, and by the cinema news reels (Schütz 1995).[24]

Why was highway building given priority instead of other public works programs? Roadbuilding as a make-work measure had been discussed extensively during the Great Depression, but no large-scale construction had taken place. The actual building of the highways signaled a regime change—a break with austerity (Ritschl 2003). Party propaganda told readers and listeners that the new highways proved that Germany was booming thanks to the Nazi regime. Ever since, the Autobahn project had symbolic character: "Hitler breaking new ground … the picture became an icon of the year immediately after 1933, a symbol for everything Autobahn construction seemed to stand for: energy, directness and dynamism of the national socialist movement" (Schütz and Gruber 1996, 43).

Motorway workers themselves were often skeptical of the Nazi regime. They came from among the unskilled and unemployed in big cities. Workers typically lived in barracks, with harsh discipline and low wages. Sometimes, disaffected workers even painted anti-Nazi slogans on construction lorries (Evans 2005) and went on strike.

Germany's car ownership rate in 1933 was low—approximately one-quarter of England's or France's. Most transport of goods and people took place via rail. The new regime intended to boost the German car industry by all means possible and not simply via roadbuilding. Hitler had high hopes for the automobile industry for employment and war production. A tax exemption on car purchases boosted car production. Between 1932 and 1938, the total number of cars, motorcycles, and trucks on German roads doubled (Evans 2005).

There were few military advantages to roadbuilding. Almost all troop and supply movements before and during World War II were by rail. Nonetheless, the growth of the motorcar industry expanded war manufacturing capacity. Boosting the mobility of army units was an aim of most armed forces after 1920 (van Creveld 1977). Increasing car ownership and the number of trucks in Germany benefited the army because private vehicles could be confiscated in wartime—the invasion of France in 1940 used some 15,000 trucks requisitioned from private industry (Vahrenkamp 2010).

[24] In addition, the Autobahn was also celebrated as an aesthetic innovation. The Autobahn company commissioned a number of artists to produce paintings of road segments, bridges, ramps, and construction work. A book containing reproductions of these paintings sold over 50,000 copies (Vahrenkamp 2010).

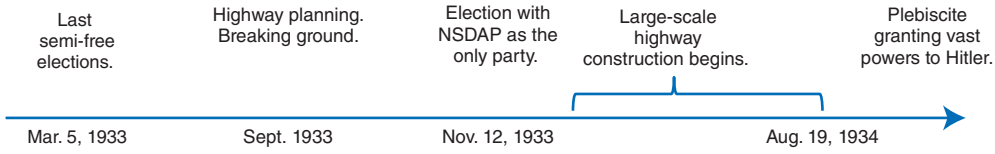

FIGURE 3. TIMELINE OF EVENT

B. *1933 Elections and the 1934 Plebiscite*

Figure 3 illustrates the timeline of elections and highway building. When Germans went to the polls in March 1933, the Hitler government had already been in power for over a month. Nonetheless, elections were still relatively fair, with limited intimidation at the polls. The Communist Party had been banned, but all other parties that had competed during the last free election in November 1932 were still on the ballot in March 1933. Despite a massive propaganda campaign (Adena et al. 2015), the NSDAP failed to win an absolute majority, receiving 44 percent of the vote.

In November 1933, the regime held fresh elections. Over the summer, all parties except the NSDAP had been banned. On average, the Nazi Party won 92 percent of the popular vote, more than doubling its vote share from March.[25] Voting in November 1933 was not free and fair; storm troopers stood guard at the voting booths. Citizens were strongly "encouraged" to vote publicly to demonstrate their support of the Nazi regime. Evans (2005, 109) observes that

> Intimidation was particularly evident during the national plebiscites and elections. … Under the Third Reich, plebiscites and elections became propaganda exercises … to provide the appearance of popular legitimacy for controversial measures.

Nonetheless, opposition was not zero. On average, 8 percent of all Germans effectively voted against the Nazi list (by spoiling their ballot papers—voting "no" was not possible in November 1933). In some areas, there was massive opposition—in the old Hanseatic city of Lübeck, for example, 40,824 voters did not vote "yes" for the NSDAP list, out of 111,911 votes cast—a proportion of 36.5 percent. Hamburg and Berlin also registered high levels of dissent, with 27 and 26 percent of voters, respectively, refusing to support the Nazi list. In other cities, less than 1 percent of votes were spoiled.[26]

In August 1934, Germans were called to the polls again to vote on the plebiscite that would make Hitler both chancellor and president. While overall support remained

[25] In parallel with the parliamentary election, voters were also asked to approve Germany's leaving the League of Nations. This proposal was wildly popular since the League of Nations was closely associated in the minds of Germans with the (hated) Versailles settlement that saddled Germany with massive reparations (Evans 2005). This referendum received 95 percent support.

[26] There are also several smaller towns where support reached 100 percent.

high, it declined slightly on average compared with November 33: 89.9 percent voted with "yes" in August 1934.[27]

C. *Crisis and Entrenchment of the Nazi Dictatorship, 1933–1934*

Once in office, the Nazi leadership quickly asserted control over the police; violence against opponents—suspected or real—was frequent (Evans 2005). Despite this ruthlessness, the regime was not firmly established during its first 18 months. In 1934, opposition to the regime increased: The most radical part of the Nazi movement, the SA, threatened a "second revolution."[28] Middle-class voters, often supporters of the NSDAP before 1933, feared wider chaos (Behnken and Rinner 1980):

> The moment was … critical for the regime. … [E]nthusiasm of the "national revolution" in 1933 had discernibly fallen off. ... The brownshirts were not the only section of the population to feel disappointed. ... Social Democratic agents reported … that people were apathetic, constantly complaining, and telling endless political jokes about the Nazi leaders. Nazi meetings were poorly attended. … The educated classes feared that the disorder caused by the stormtroopers might spill over into chaos or, worse, Bolshevism. (Evans 2005, 28–29)

By 1934, the Nazi leadership feared that the conservative members of the government might join forces with the army and overthrow the Hitler regime (Evans 2005). Paul von Hindenburg was still president and close friends with Franz von Papen, a former chancellor now serving as vice chancellor. In June 1934, von Papen gave a famous speech in Marburg in which he decried violence and lawlessness by the SA and condemned the personality cult of Hitler. Thereafter, his public appearances were often greeted with the shout "Heil Marburg." *Time* magazine concluded that Hitler was not a "real dictator."[29] At this moment, the defense minister, General Werner von Blomberg, threatened Hitler with a coup by the army if the SA was not brought to heel (Wheeler-Bennett 1964). Eventually, Hitler had both the leadership of the SA and leading conservatives murdered, claiming that the victims had been plotting a putsch.

The conflicts and threats of the summer of 1934 show that the Nazi regime was still far from its later, omnipotent position; popular support could by no means be taken for granted. Indeed, knowledgeable observers concluded that there was "evident shakiness in high Nazi places" and that "Adolf Hitler [got] the scare of his career" (*Time*, July 2, 1934). An opportunity to showcase overwhelming popular

[27] While the November 1933 election (for the Nazi Party) and the 1934 referendum (for Hitler himself) are clearly distinct, there is no obvious downward bias—right down to the end in 1945, Hitler personally was much more popular than the Nazi Party.

[28] The SA grew out of street-fighting paramilitaries; its leaders envisioned themselves as a Nazi People's Army, and many pursued dreams of a far more left-wing agenda, including wholesale nationalization of many industries. Threats to Hitler's leadership, however, were largely invented to justify the crackdown on the SA in the summer of 1934 (Evans 2005).

[29] "[I]f Adolf Hitler came home with a swelled head and hot new ideas for dictatorship from his visit to Benito Mussolini, certainly last week he was dexterously chilled and shrunk" ("Germany: Second Revolution," *Time*, July 2, 1934, https://time.com/archive/6753561/germany-second-revolution/).

support was of key importance to the regime at this juncture. It was only after Hitler became both chancellor and president as a result of the 1934 plebiscite, where a large share of the population publicly supported the führer's new powers, that the regime became deeply entrenched.

D. *Political Instability*

Germany's first attempt at democracy, the Weimar Republic, was synonymous with political instability; governments changed with alarming frequency. The perceived "chaos" of democratic governance was often contrasted with order before 1918; instability was one key reason why support for democratic rule waned. As the novelist Stefan Zweig (1942) observed:

> [I]nflation, unemployment, [and] the political crises had disturbed the German people deeply; an incredible desire for order spread amongst the German people. ... And whoever promised order … immediately had hundreds of thousands behind him.

Despite great turmoil in the country as a whole, some federal states showed relative stability, with the same prime ministers and parties in power for long stretches of time. Prussia is one notable example (Orlow 1986).

III. Data

We use voting records for more than 3,000 towns and cities, covering the entire area of Weimar Germany (Statistisches Reichsamt 1934), combined with demographic and socioeconomic information from the 1925 and 1933 censuses (Falter and Hänisch 1990). To this, we add newly coded geographical information from historical maps of the (planned and built) German motorway network, construction of which began after the summer of 1933, as well as data on vehicle ownership, radio signal strength, and the political stability of Weimar states.

A. *Highway Construction and Balancedness*

We digitize three types of Autobahn segments based on the map from Todt (1934): under construction, approved for construction (but not yet begun), and planned segments (not yet approved for construction)—see footnote 20 for detail. The general plan for the Autobahn envisaged connections between the largest cities (Figure 2). Of the 3,231 towns and cities in our sample, 1,978 were within 20 km of the planned Autobahn. A little more than a third (1,253) were farther away (Table 1). Out of the 1,978 locations close to the planned network, 1,068 saw actual construction by the summer of 1934, 54 percent of the planned total.

Some socioeconomic characteristics differed between towns close to the highway network and more distant ones. Table 2 gives an overview, showing the sample mean of several socioeconomic variables from the 1925 and 1933 German censuses in column 1, the average for cities within 20 km of the highway network (planned or built) in column 2, and the means for cities with and without actual

Table 1—Number of Towns and Cities in the Sample, Conditional on Highway Construction

	Highway under construction in 1934 (< 20 km)		
	Yes	No	Total
Part of national highway plan? (< 20 km)			
Yes	1,068	910	1,978
No	0	1,253	1,253
Total	1,068	2,163	3,231

Notes: The table reports the number of towns in our sample, depending on their proximity (20 km) to the planned highway network and highway segments under construction by 1934. A map with the location of highways is shown in Figure 2.

Table 2—Cities Characteristics (Sample Means), by Highway Plans and Construction

Variable	Full sample	Highway planned		
		All	Built	Not built
Baseline controls				
Population size 1933	12,375	16,080	22,162	8,942
Unemployment rate 1933	0.152	0.163	0.182	0.142
Additional controls				
Blue-collar share 1933	0.336	0.347	0.363	0.328
Share industrial employment	0.296	0.313	0.339	0.284
Share Catholic	0.365	0.340	0.287	0.403
Share Jewish	0.005	0.005	0.004	0.006
Initial Nazi support				
NSDAP vote share in March 1933	0.426	0.413	0.415	0.410
Number of towns and cities	3,231	1,978	1,068	910

Notes: Under "Highway planned," "All" comprises all cities within 20 km of planned, approved, or built highways in 1934, according to the highway network in Figure 2; "Not built" are those segments that were planned but not yet under construction by August 1934.

highway construction, among those near the planned network (columns 3 and 4). Cities near the planned highway network were more populous than the rest; initial unemployment (in 1933), the blue-collar share, and industrial employment were also somewhat higher, while there were fewer Catholics than in the sample overall. The share of Jewish population was the same. Comparing columns 3 and 4 shows that construction began in those parts of the planned network that were closer to larger, more industrial cities and in more Protestant areas. This gives rise to endogeneity concerns because support for the Nazis also varied with socioeconomic factors. In our empirical analysis, we address this issue in a variety of ways: by adding explicit controls and city fixed effects, entropy weighting to create a balanced sample, and the use of LCPs as an instrument for actual highway location. Importantly, preexisting support for the Nazi regime did *not* systematically affect the location of highway construction, as shown by the balanced NSDAP vote share in March 1933 in Table 2. We explore this in more detail below, showing that there were also no differential pre-trends in Nazi support before highway construction started.

B. *Elections and Plebiscites*

Our main analysis focuses on the change in the share of voters supporting the Nazi regime between the November 1933 election and the 1934 plebiscite. We digitize these data from their original, printed source (Statistisches Reichsamt 1934). As a proxy for underlying Nazi support, we also use the NSDAP vote share in the March 1933 election—after Hitler had been appointed as chancellor, but when other parties were still permitted at the polls. Data for elections between 1924 and March 1933 are from the publicly available data archive by Falter and Hänisch (1990). Figure A.2 in the Supplemental Appendix plots the share of "pro-Nazi" votes in the three elections we focus on. Since elections after March 1933 were no longer fair and free, the officially registered support for the regime at the polls surged until November 1933. Between November 1933 and August 1934, the share of pro-Nazi votes declined somewhat—if we want to disregard the fact that the nature of the vote changed too.

To make the different elections comparable, we rescale vote shares in our empirical analysis, transforming electoral "pro-Nazi" votes for each election into a standardized variable with zero mean and unit standard deviation. Our main outcome variable is NS_{broad}, which is defined as the share of "yes" votes relative to all *eligible* voters. This variable counts nonvoters as opposition to the Nazi regime—which in many cases is justified given the high pressure for turnout (see Section II). In Supplemental Appendix A.6, we show that our results also hold when using a narrow measure (NS_{narr}), which is defined as the share of "yes" votes relative to *actual* voters; it is thus unaffected by voter turnout (and by potential unobserved spatial variation in the pressure to vote).[30]

C. *Radio*

From the 1920s onward, Germany had a highly developed, government-owned system of radio stations (Bausch 1956). By the 1930s, governments regularly used radio programs to bolster support (Adena et al. 2015). There are detailed data on the number of radio subscribers in various parts of Germany and on the strength of radio signals. Since the purchase of a radio subscription may itself be a function of political preferences, we follow Adena et al. (2015) and focus on city-specific signal strength, as determined by the power and location of transmitters interacting with terrain characteristics.[31] We find that listenership increased with signal strength in a flexible nonparametric estimation (see Supplemental Appendix A.7 for detail).

[30] Note that both measures count invalid votes as opposition to the Nazi regime. In fact, the November 1933 election did not allow for a "no" vote or for votes for any other parties. Thus, conditional on voting, invalidating the ballot was the only way for voters to express their discontent with the Nazi regime. The 1934 referendum, in contrast, included an option to vote "no." This is another reason why the two elections are not directly comparable, motivating our use of standardized vote shares rather than comparing levels.

[31] We use predictions from the irregular terrain model. Ruben Enikolopov kindly provided us with city-specific signal strength data for all locations in our dataset from the implementation of the radio diffusion model in Adena et al. (2015).

D. *Political Instability of Weimar States*

To measure political instability at the state level, we follow Satyanath, Nico, and Voth (2017), who measure political stability as the first principal component of three indicators: the percentage of time that (i) the longest-serving state government was in office and (ii) the longest-serving party was in office (possibly in different coalitions), and (iii) whether a state was governed by at least one party from the "Weimar coalition." In combination, these three variables capture political turmoil at the federal state level during the Weimar period.[32]

IV. Main Empirical Results

In this section, we show that support for the Nazi regime increased significantly more in areas where the new motorways were being built. Before presenting econometric estimates, we first illustrate our main finding. Figure 4 maps changes in support for the Nazi regime between November 1933 and August 1934.[33] The darker the red on the map, the greater the (residual) electoral gains of the Nazi Party. Solid black lines are roads under construction; dashed ones, roads approved but not yet being built. On average, areas through which the new highways passed saw much greater gains in support for the Nazis than the rest. This is particularly true in East Prussia, in the north of Germany, in the west around the Ruhr, and in the area around Frankfurt. While there are areas with significant increases in support without roadbuilding (such as along the shoreline of the North Sea near Holland), they are relatively rare.

A. *Baseline Results: Change in Nazi Support, November 1933–August 1934*

In what follows, we present our baseline specifications, estimating regressions of the form

$$\Delta NS_i = \alpha + \beta D_i + \gamma X_i + \varepsilon_i, \tag{1}$$

where ΔNS_i is the change in (standardized) pro-Nazi votes between November 1933 and August 1934 in city i, D_i is its distance to the nearest highway segment under construction, X_i is a vector of controls, α is a constant, and εi is the error term. If D_i was randomly assigned, β would reflect the causal effect of motorway building on support for the Nazi regime. We present OLS results first and then discuss potential challenges to identification, followed by IV results.

In Table 3, panel A, we first show the simplest specification without controls in column 1. We find a negative and highly significant coefficient on distance to highways. To gauge its magnitude, we calculate the implied vote change (in percentage

[32] Satyanath, Nico, and Voth (2017) end their coding period before the Prussian coup d'etat in July 1932, which is often considered the beginning of the end of the Weimar democracy. We use their original measure but have also extended their coding period to January 1933 as a robustness check.

[33] We plot effects after accounting for log city population and unemployment in 1933, as well as regional fixed effects corresponding to 77 administrative districts in Weimar Germany (*Regierungsbezirke*).

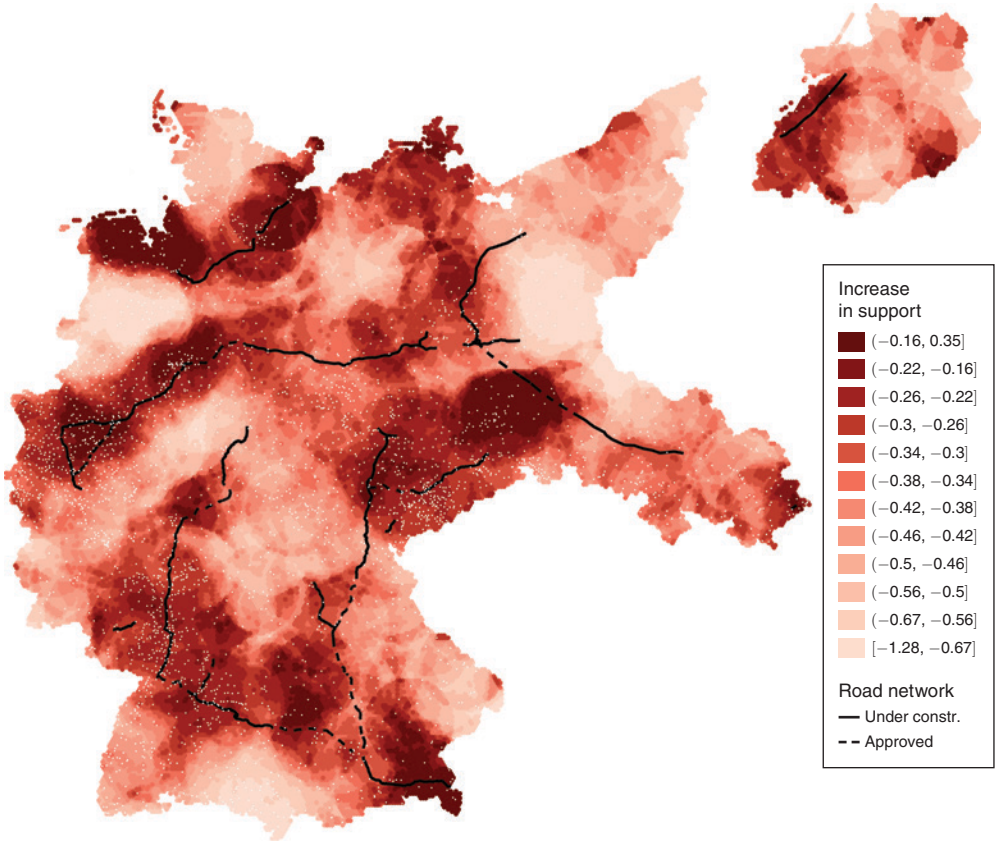

FIGURE 4. SHIFT IN FAVOR OF THE NAZI REGIME BETWEEN NOVEMBER 1933 AND AUGUST 1934

Notes: The figure shows the difference in standardized pro-Nazi votes between the November 1933 election (before highway construction had begun in earnest) and the August 1934 referendum (when large-scale highway construction had started). The figure shows residual variation after controlling for city population, unemployment, and fixed effects for 77 administrative districts (*Regierungsbezirke*). Small white dots in the figure indicate towns and cities in our dataset. We use the coordinates of these localities to draw a spatial kernel of the "increase in Nazi support" variable over a grid of hexagons 3 km wide. The kernel function is uniform, and the bandwidth is 50 km.

points) when going from a 1 to 100 km distance to highway construction.[34] This yields a vote gain of 2.8 pp. In column 2, we add our baseline controls as well as the Nazi Party vote share in the preceding November 1933 election. The coefficient on highways declines but remains highly significant, and it rises again when we add fixed effects for 77 administrative districts in column 3. Adding the latter means that we exploit only the distance to highway segments within each district, differencing out any regionally based shifts in voting patterns. Our results in column 3 thus imply that, relative to all the other towns in the same district, those closest to the new highways saw particularly large gains in Nazi support.

[34] To obtain vote differences in levels for November 1933–August 1934, we multiply the result for standardized vote differences by the standard deviation of vote differences in levels.

Table 3—Distance to Highways and Change in Nazi Support

Dependent variable: Change in standardized pro-Nazi votes, November 1933–August 1934	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. OLS results						
log(distance HW)	−0.0951	−0.0576	−0.0765	−0.0385		
	(0.0133)	(0.0121)	(0.0134)	(0.0124)		
HW within 20km					0.123	0.120
					(0.0276)	(0.0263)
Vote change 1–100km[a]	−2.8%	−1.7%	−2.3%	−1.1%	0.8%	0.8%
Pro-NSDAP votes in Nov 1933		−0.360	−0.427	−0.442	−0.359	−0.428
		(0.0157)	(0.0167)	(0.0158)	(0.0158)	(0.0168)
ln(population) in 1933		−0.0545	−0.0358	−0.0448	−0.0559	−0.0361
		(0.0144)	(0.0131)	(0.0135)	(0.0144)	(0.0132)
Unemployment rate in 1933		0.605	−0.0607	−0.111	0.674	0.0341
		(0.225)	(0.213)	(0.221)	(0.221)	(0.213)
Additional controls				✓		
District FE			✓	✓		✓
Observations	3,231	3,231	3,231	3,213	3,231	3,231
Adjusted R^2	0.013	0.186	0.468	0.552	0.186	0.466
Panel B. Accounting for spatial correlation, same controls as above[b]						
log(distance HW)	−0.0951	−0.0576	−0.0765	−0.0385		
	(0.0149)	(0.0231)	(0.0181)	(0.00532)		
HW within 20km					0.123	0.120
					(0.0331)	(0.0286)
Panel C. Change in % (nonstandardized) pro-Nazi votes, November 1933–August 1934: Persuasion rates						
log(distance HW)	−0.498	−0.410	−0.545	−0.274		
	(0.0833)	(0.0863)	(0.0957)	(0.0884)		
HW within 20km					0.873	0.857
					(0.196)	(0.188)
Vote change 1–100km[a]	−2.3%	−1.9%	−2.5%	−1.3%	0.9%	0.9%
Persuasion rate[c]	15.7%	13.0%	17.2%	8.7%	6.0%	5.9%

Notes: Robust standard errors in parentheses. "Distance HW" is the distance of a city to the nearest highway segment that was under construction by August 1934; "HW within 20km" is a dummy that takes on value one if "Distance HW" is below 20 km and zero otherwise. "Additional controls" include the share of blue-collar workers and the share of industrial employment in 1933, as well as the shares of Catholics and of Jews in 1925. "District FE" correspond to 77 *Regierungsbezirke* in Weimar Germany.

[a] In columns 1–4: Change in pro-Nazi votes (in percent) when going from 1 to 100 km distance to nearest highway segment under construction. In columns 5–6: Gain in Nazi support in cities within 20 km of highways, relative to all other cities farther than 20 km.

[b] Standard errors in panel B are adjusted for arbitrary cluster spatial correlation, using the Colella et al. (2019) algorithm. All cities with distance of less than 700 km are considered spatially contiguous and are assigned a nonzero spatial weight. The cutoff of 700 km reflects the average distance of the most important east–west (Berlin–Aachen) and north–south (Hamburg–Munich) highway connections planned by the Nazi regime. All controls are the same as in the OLS regressions in panel A.

[c] Persuasion rates are computed following Della Vigna and Gentzkow (2010), assuming that exposure to highway construction was zero beyond 100 km (columns 1–4)/beyond 20 km (columns 5–6), so that these groups form the "control group" of pro-Nazi votes absent highways. In using the formula by Della Vigna and Gentzkow (2010), we also assume that 100 percent of people "treated" by highway construction "received the message." Lower values for this share would yield higher persuasion rates.

In terms of control variables, the negative coefficient on initial pro-Nazi votes in November 1933 is significant and negative, which reflects a mechanical effect—places with close to 100 percent support could hardly gain additional votes. The coefficient on city population size is negative and significant—more populous

places saw less of an increase in Nazi support. Finally, the coefficient on unemployment in 1933 is ambiguous, switching signs and becoming insignificant when we add district fixed effects.

In column 4, we add additional socioeconomic controls (the share of blue-collar workers, of Jews, of Catholics, and of industrial workers); the significance of the distance-to-highway variable is not affected, but it declines somewhat in size. Finally, we define a dichotomous variable that takes on value one for towns or cities that were within 20 km of highways under construction, and zero otherwise. In columns 5 and 6, we use this alternative variable to repeat the specifications from columns 2 and 3.[35] We again find highly significant coefficients that suggest an increase in support by 0.12 standard deviations (corresponding to an increase in pro-Nazi votes by about 1 percentage point) if a town was close to the Autobahn. Supplemental Appendix A.2 shows that alternative cutoffs for distance to highways lead to very similar results.

In Panel B of Table 3, we allow for arbitrary spatial dependence across observations, following Colella et al. (2019).[36] The standard errors for the coefficients on distance to highway under construction are very similar to our main results in panel A of Table 3. This suggests that our results are not confounded by spatial correlation.

For completeness, panel C of Table 3 presents results using unstandardized changes in pro-Nazi votes as dependent variable. This specification relies on the (debatable) assumption that vote shares in the November 1933 election and in the August 1934 referendum are comparable. Nevertheless, we find very similar effects of highway construction on Nazi support.

B. *Persuasion Rates*

How effective were highways in winning voters for the Nazi government? And how large were electoral gains overall? We gauge the effectiveness of highway building by looking at persuasion rates—the extent to which exposure to the highway was associated with voters switching from opposition to support of the government (Della Vigna and Gentzkow 2010). Panel C of Table 3 reports our estimates of persuasion rates—the share of voters who changed their minds because of highway construction. To calculate these rates, we assume that in the log-linear specification (columns 1–4), voters in areas farther than 100 km from the highway were not affected, thus forming our "control" group. With this, using the method in Della Vigna and Gentzkow (2010), we calculate the increase in Nazi support when going from 100 km of distance to highway construction to 1 km, relative to the share of voters who were left to be persuaded to support the Nazi regime. For the dummy

[35] Note that these regressions essentially reflect a difference-in-differences specification, where "treatment" is a dummy for highway proximity. Thus, treatment effects are estimated as changes in pro-Nazi votes before versus after highway construction, in areas near versus distant from highway segments. The full difference-in-differences specification (running regressions with Nazi votes in levels for November 1933 and August 1934, with city and year fixed effects) yields almost identical results (available upon request).

[36] We consider all cities with distance less than 700 km spatially contiguous and assigned them a nonzero spatial weight. The cutoff of 700 km reflects the average distance of the most important east–west (Berlin–Aachen) and north–south (Hamburg–Munich) highway connections planned by the Nazi regime.

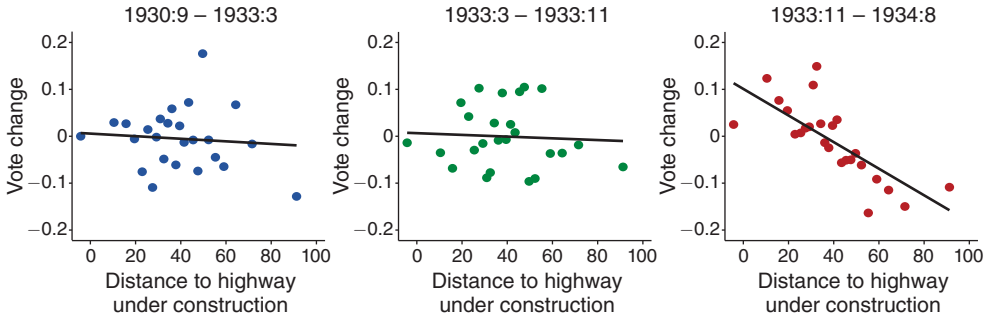

Figure 5. Change in Pro-Nazi Votes, before and after Highway Construction Began

Notes: The figure shows the difference in standardized pro-Nazi votes between the elections in September 1930 and March 1933 (left panel), between those in March 1933 and November 1933 (middle panel), and between the November 1933 election and the August 1934 referendum (right panel), as a function of distance from highway segments that were under construction by 1934 (construction began in the fall of 1933). The underlying regressions include the baseline and additional controls listed in Table 2, as well as fixed effects for 77 administrative districts (*Regierungsbezirke*). For ease of exposition, the binscatter plots group the *x*-axis into 25 equal-sized bins.

specifications in columns 5 and 6, we assume that treatment is zero beyond 20 km distance. We find substantial persuasion rates, ranging from of 8.7 to 17.2 percent for the log-linear specification and 6 percent in the dummy specification.[37] Note that these figures are a lower bound; nationwide increases in Nazi support due to highways are not captured by the persuasion rates.

C. *Voting Patterns before and after Highway Construction*

Are our baseline findings in Table 3 specific to the period November 1933–August 1934, when roadbuilding got under way in earnest—or do they reflect preexisting differences or differential trends?

To examine this question, we first add the last relatively free election of March 1933 to our analysis. We find that votes for the Nazi Party in March 1933 were not significantly correlated with distance to highways that would be built from late 1933 onward (see Table A.7 in the Supplemental Appendix for detail). Next, we examine the *change* in Nazi support before and after highway construction began on a large scale. This striking difference in effects is illustrated in Figure 5.[38] The left and middle panels serve as placebos, illustrating the change in pro-Nazi votes between September 1930 and March 1933 (before highway construction), as well as between March 1933 and November 1933, when only very little highway construction had taken place. There is essentially no relationship between distance to highways and change in Nazi support. This pattern changes dramatically after November 1933, when highway building took off: The right panel of Figure 5 shows that by August

[37] For comparison—the voting studies surveyed by Della Vigna and Gentzkow (2010) show a range of persuasion rates in elections of 2 to 20 percent, with an average of 11.9 percent.

[38] Given that regular scatterplots with every data point would become too crowded for visual interpretation, we use binscatter plots, grouping the *x*-axis into 25 equal-sized bins. To allow for a more immediate interpretation of the *x*-axis, we use distance in km rather than log-km. Results are very similar when we use logs instead.

Table 4—Panel Estimation

Dependent variable: Standardized votes for the Nazi Party						
Elections included:	March 1933, Nov 1933, Aug 1934				1924–1934	
	(1)	(2)	(3)	(4)	(5)	(6)
log(distance HW) × Aug 1934	−0.0825	−0.0647	−0.0637	−0.0483	−0.0705	−0.0882
	(0.0203)	(0.0209)	(0.0204)	(0.0225)	(0.0229)	(0.0235)
log(distance HW) × Nov 1933	0.0126	0.00118	0.00155	0.0166	0.0246	0.0257
	(0.0238)	(0.0247)	(0.0247)	(0.0239)	(0.0259)	(0.0246)
log(distance HW) × March 1933					0.0119	−0.0243
					(0.0201)	(0.0209)
log(distance HW) × Sep 1930					0.00484	−0.00458
					(0.0196)	(0.0185)
log(distance HW) × May 1928					−0.0207	
					(0.0175)	
Lagged Nazi Party votes			0.0342	0.0464		0.116
			(0.0153)	(0.0156)		(0.0141)
City FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Baseline controls × year		✓	✓	✓		✓
Additional controls × year				✓		✓
District FE × year				✓		✓
Observations	9,677	9,677	9,660	9,633	19,279	16,044
Adjusted R^2	0.459	0.464	0.464	0.672	0.349	0.563

Notes: Robust standard errors in parentheses, clustered at the city level. "Distance HW" is the distance of a city to the nearest highway segment that was under construction by August 1934. "Baseline controls" include log population and unemployment rate in 1933. "Additional controls" include the share of blue-collar workers and the share of industrial employment in 1933, as well as the shares of Catholics and of Jews in 1925. District FE correspond to 77 *Regierungsbezirke* in Weimar Germany. We use all elections in Weimar Germany for which commune-level data are available. The election in May 1924 uses the (standardized) vote share for the DVFP, which presented a joint list with Nazi candidates while the NSDAP was banned (see footnote 39).

1934, it was the areas closest to the highway that saw the biggest relative gains in Nazi support. The difference between the coefficients in March/November 1933 and August 1934 is crucial for our argument. It implies that distance from the highway only becomes a predictor of Nazi support after construction began on a large scale—after November 1933.

Next, we generalize the analysis by using panel regressions. We estimate the relationship

$$NS_{it} = \alpha_i + \delta_t + \beta D_i \times \delta_t + \gamma X_i \times \delta_t + \varepsilon_{it}, \tag{2}$$

where NS_{it} are pro-Nazi votes in city i in election t; D_i is city i's distance from the nearest highway segment under construction; X_i is a vector of city-level controls; α_i and δ_t are city and election fixed effects, respectively; and ε_{it} is the error term. Note that we interact D_i and all controls with year dummies. This allows the coefficients on distance to highways and controls to vary in each period, thereby also effectively running placebo tests for the periods before road construction began.

We present our panel results in Table 4. In columns 1–4, we pool election data on the success of the Nazi Party from the early years of dictatorship

(March 1933–August 1934). We find a negative and significant coefficient on distance to highway construction only for the August 1934 election; for all earlier elections, the interaction with the highway distance variable reveals no statistically significant or economically meaningful relationship. The nonresults for March and November 1933 also imply that Autobahn construction was not used to reward districts with strong previous support for the Nazis; in other words, "favoritism" in the sense of Burgess et al.'s (2015) finding for Kenya is probably not present in our data. These results are robust and hold when we interact our baseline controls (population and unemployment) with year dummies (column 2), when adding lagged Nazi Party votes (column 3), and when we add interactions of additional socioeconomic controls with the year dummies (column 4).

In the last two columns in Table 4, we use data from all elections with city-level data during the period 1924–1934.[39] We estimate both with fixed effects only (column 5) and with extended controls and lagged Nazi votes (column 6). Again, the 1934 referendum is the only period that shows a statistically significant relationship between Nazi support and distance to highway construction.

Overall, there is no evidence that Nazi support was either high or already growing in places where highways were (later) built (see also Table A.7 in the Supplemental Appendix). Instead, the entire effect of highway construction on electoral outcomes appears suddenly, and only for the period November 1933–August 1934.[40]

V. Identification

Given the highways' propaganda value, their trajectories may have been chosen for political reasons. In this section, we focus on the period November 1933 to August 1934 and instrument the path of actual highway construction between given city pairs with terrain characteristics that facilitated roadbuilding.

A. *IV Results: Least-Cost Paths*

Our results could be affected by endogeneity bias if the Nazis targeted areas that were more likely to increase their support for the regime even in the absence of highway construction. The Nazis could also have planned and built highways to reward (newly) loyal districts, or strong local Nazi officials may have been more successful at both attracting the highway and swaying voters. On the other hand, OLS results could also be downward biased if Nazi highway building targeted areas where it was more difficult to win new supporters. Endogeneity concerns cannot be dismissed out of hand—for example, Hitler himself intervened in the planning of the road from Munich to Salzburg, although largely on aesthetic grounds (Vahrenkamp 2010).

[39] The NSDAP was banned from the 1924 election as a result of the failed Beer Hall Putsch. Members of the banned NSDAP reconstituted themselves as a party under the label NSFP (Nationalsozialistische Freiheitsbewegung), which put forward joint lists with the DVFP (Deutschvölkische Freiheitspartei). The DVFP absorbed much of the Nazi vote in the May 1924 election (Striesow 1981), and we use its standardized vote share in the panel in 1924.

[40] In Supplemental Appendix A.2 we perform an additional analysis, restricting the sample to towns within 20 km of planned highway construction. This increases the similarity of towns and cities in our sample, excluding those that were never to be connected to the highway system. Even within this narrowly defined subsample, we find a strong relationship between actual construction and Nazi support.

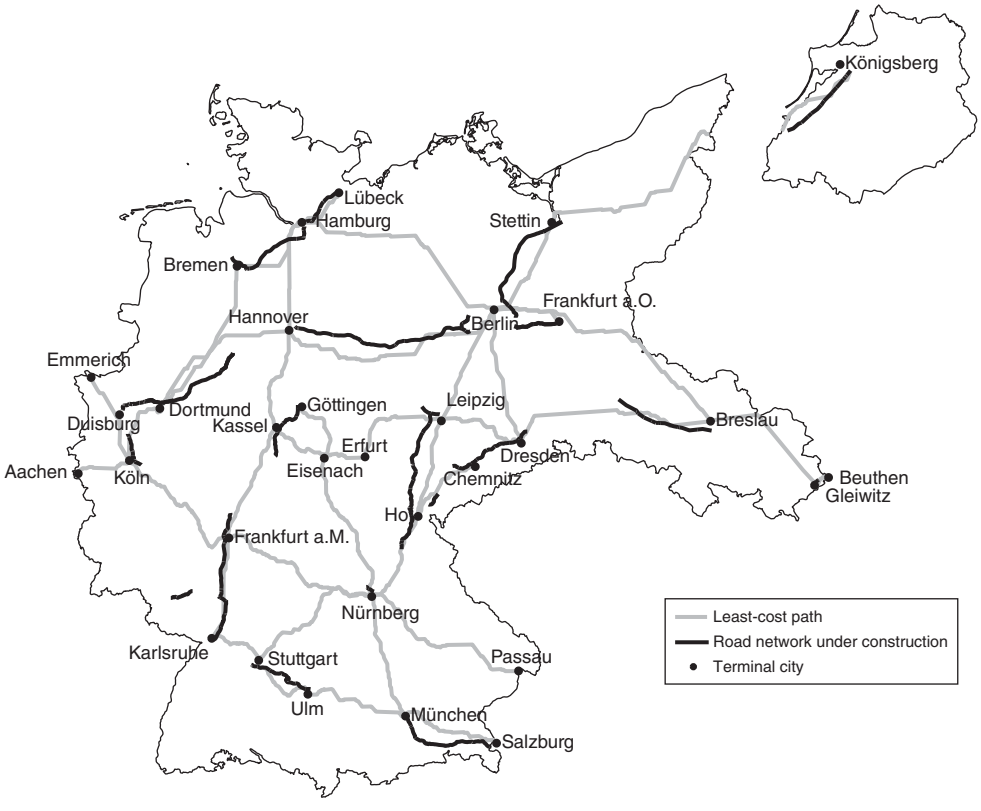

FIGURE 6. LEAST-COST PATHS AND ACTUAL HIGHWAY CONSTRUCTION

Notes: Location of highway segments from Todt (1934). LCPs between terminal cities computed by authors.

To address possible endogeneity concerns, we instrument for actual highway building with LCPs. Road construction cost is highly sensitive to the slope of the traversed terrain. We use the Cost Path tool in ArcGIS to calculate the cheapest way to connect city pairs that appear in official German publications as terminal cities that were to be connected in the first wave of highway construction.[41] Figure 6 plots LCPs and actual highway construction that began before August 1934. They overlap to a large extent. Even where the LCP does not coincide exactly with the actual motorway trajectory, differences are typically small. The only larger deviations are

[41] We compute LCPs for all 39 city-pair connections listed in Jahnke (1936). See Supplemental Appendix A.1 for details. We use data on elevation from Jarvis et al. (2008) and from Nunn and Puga (2012). Related work using geographical characteristics or earlier transport infrastructure for identification includes Baum-Snow (2007); Donaldson and Hornbeck (2013); Banerjee, Duflo, and Qian (2012); and Faber (2014). We do not use the network analysis as implemented by, for example, by (2014), who uses Kruskal's minimum spanning tree algorithm to pin down a cost minimizing network structure. As Figure 2 confirms, the Nazi building of the Autobahn did not follow a network logic, with an increasing set of cities connected to existing roads. Instead, the regime initially connected city pairs, and it started to build in multiple disconnected locations all over the country—delaying the opening of the first usable road but making the project more visible.

in North Germany, where the terrain is generally flat and small differences in cost can lead to quite different paths.

LCPs have substantial explanatory power for actual highway construction: Out of the 3,276 towns and cities in our sample, about one-half (1,602) lie within 20 km of an LCP. Of these, 1,404 (87.6 percent) also lie within 20 km of the actually planned highway network, and 914 (57.1 percent) of them saw actual construction activity by the summer of 1934. In contrast, of the 1,674 towns and cities that were more than 20 km away from LCPs, only 183 (10.9 percent) saw construction.

Our instrumental variable is the distance of each city from the LCPs. Crucially, all regressions exclude the 27 terminal cities—i.e., the end points between which LCPs are computed.[42] Before presenting our IV results, we briefly discuss their interpretation. Importantly, LCPs affect the *planning* of highways, while the electoral effects we are interested in are due to actual *construction*. Planning of highways translated into highway construction in *some* districts by 1934—depending on the timing of construction. Our IV strategy estimates the average effect of highway construction on pro-Nazi votes for those cities whose "treatment status" (proximity to highway construction) was affected by the instrument (proximity to LCPs). Using common IV terminology, we estimate the average treatment effect for "compliers" (cities where proximity to LCPs did results in construction). In contrast, cities close to LCPs where no construction occurred by 1934 ("never-takers") do not affect our estimate.

Table 5 presents our IV results. We first show results for the reduced form, regressing change in support for the Nazi Party on distance to LCPs. We find strong and significant negative coefficients, both without controls (column 1) and with the full set of controls (column 2). In column 3, we perform a placebo analysis for vote gains over the period March–November 1933, showing that distance to LCPs does not predict Nazi vote gains before highway construction began. Next, we return to our main period of analysis and demonstrate the strength of our instrument (columns 3 and 4). The first stage is powerful, with F-statistics above 500. Finally, we present the IV results in columns 5 and 6. We find highly significant coefficients on instrumented distance to highways in the second stage, with comparable—albeit larger—magnitude than our OLS estimates in Table 3.[43]

VI. Channels

Why did the Autobahn succeed in raising electoral support for the Nazis? To gain insight into possible mechanisms, we first look at one direct economic benefit of highway construction—unemployment reduction. We then analyze other explanations—in particular, whether roadbuilding "worked" because it served as a signal of government competence.

[42] Our baseline OLS results (Table 3, panel A, column 2) are identical when we exclude the terminal cities.

[43] When all controls and district fixed effects are included (column 6), the IV coefficient is about twice as large as its OLS counterpart in the same specification (column 4 in Table 3, panel A). This may be due to measurement error in the actual location of highway construction (see footnote 20). In Supplemental Appendix A.2 we perform a more restrictive IV analysis, using only connections where both terminal cities belonged to the top 20 in terms of population in 1933—i.e., cities that most sensible road planners would have planned to connect. We find strong and highly significant results that closely resemble those in Table 5. This makes it unlikely that the Nazi regime strategically picked terminal cities in order to "treat" the areas in between.

Table 5—Instrumental Variable Regressions with Least-Cost Paths

Dependent variable	Reduced form		Placebo	First stage		Second stage	
	Change in votes for the Nazi Party			log(distance to highway)		Change in votes for the Nazi Party	
	Nov'33–Aug'34		Mar–Nov'33			Nov'33–Aug'34	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(distance to LCP)	−0.0464	−0.0269	0.000172	0.405	0.333		
	(0.0103)	(0.00923)	(0.000610)	(0.0156)	(0.0152)		
log(distance HW)						−0.115	−0.0807
						(0.0255)	(0.0281)
Weak-IV robust *p*-value						[<0.001]	[0.004]
Baseline controls	✓	✓	✓	✓	✓	✓	✓
Additional controls		✓	✓		✓		✓
District FE		✓	✓		✓		✓
First-stage *F*-statistic				676.0	478.7		
Instrument partial R^2				0.253	0.200		
Observations	3,200	3,182	3,179	3,200	3,182	3,200	3,181
Adjusted R^2	0.186	0.552	0.914	0.333	0.526		

Notes: Robust standard errors in parentheses. "Distance HW" is the distance of a city to the nearest highway segment that was under construction by August 1934. "Baseline controls" include log city population and the unemployment rate in 1933, as well as the (standardized) share of pro-Nazi votes in the November 1933 election (in column 3, in the March 1933 election). "Additional controls" include the share of blue-collar workers and the share of industrial employment in 1933, as well as the shares of Catholics and of Jews in 1925. "District FE" correspond to 77 *Regierungsbezirke* in Weimar Germany.

A. *Unemployment*

As Hitler came to power, unemployment was by far the most prominent economic policy issue. Goebbels's propaganda told everyone with eyes to read and ears to hear that the country's recovery after January 1933 reflected successful Nazi policies—and no intervention was more visible throughout the country (or more talked about in the propaganda) than highway construction (Todt 1934).

Can vote gains for the Nazis be explained by employment creation due to highway construction? To answer this question, we collect data on unemployment for all 253 cities where this information is available in February 1935 (for details on data construction, see Supplemental Appendix A.3).[44] This allows us to compute the change in the unemployment rate between June 1933 and February 1935—the period with available data that is closest to our main period of interest between the November 1933 election and the referendum in August 1934. We first check whether changes in local unemployment were related to Nazi support.[45] The left

[44] We combine data on unemployment from the June 1933 census (which is available at the municipality level). The 253 cities for which unemployment figures are available in February 1935 are relatively large, accounting for 23 percent of overall unemployment in the 1933 census. On average in this subsample, the unemployment rate fell by 12.2 percentage points—from 23.0 percent in June 1933 to 10.8 percent in February 1935. However, caution is warranted when comparing unemployment rates over time since those in 1933 are from the census, while the numbers in 1935 are derived from unemployment claims filed with the Labor Ministry (see Supplemental Appendix A.3).

[45] For consistency, we account for our baseline controls in Figure 7 and in the underlying regressions presented in Supplemental Appendix A.3. The baseline controls are log city population and the initial unemployment rate in 1933, as well as the share of pro-Nazi votes in November 1933.

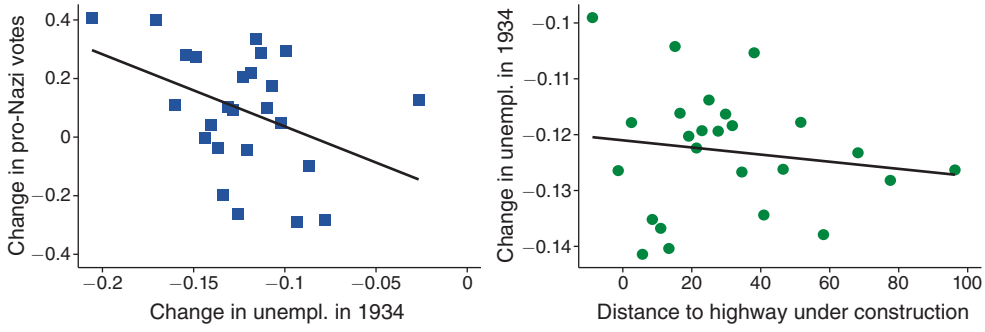

FIGURE 7. CHANGE IN PRO-NAZI VOTES, UNEMPLOYMENT AND HIGHWAYS

Notes: The figures show residual binscatter plots (with 25 equal-sized bins), after controlling for the baseline controls (log city population and the unemployment rate in 1933, as well as the share of pro-Nazi votes in the November 1933 election). The left panel shows that Nazi support increased particularly strongly between November 1933 and August 1934 in areas where local unemployment fell the most. The right panel shows that there is no relationship between unemployment and distance to highway segments under construction. The change in unemployment is computed over the period June 1933–February 1935. The underlying data are for a subsample of 253 towns and cities for which unemployment data in February 1935 are available. The corresponding regressions are reported in Supplemental Appendix A.3.

panel of Figure 7 shows that this was the case: Gains in Nazi support were greatest in areas where unemployment fell the most. The corresponding coefficient implies that a 1 pp reduction in unemployment was associated with a 0.026 std (equivalent to 0.11 pp) increase in Nazi support (see Supplemental Appendix A.3 for regression results). This corresponds to an increase in Nazi support by 0.48 pp for a 1 std drop in unemployment.

Was the decline in local unemployment in turn driven by highway construction? If this was the case, (un)employment would be a likely channel through which Autobahn construction boosted Nazi support. The right panel of Figure 7 shows that this is not the case. There is no relationship between the change in unemployment and distance to highways (controlling for initial unemployment). The corresponding coefficient is small, negative, and within a tight confidence interval around zero.[46] Similar results hold in alternative specifications (see Table A.4 in the Supplemental Appendix). While we can only show this (non)result in the subsample of 253 cities, it is nevertheless suggestive: Local job generation is unlikely to explain the effect of highways on Nazi support. In other words, pure economic voting based on improvements in the local labor market can probably not account for a sizable part of the swing toward the Nazi government.

[46]This nonresult echoes earlier arguments that doubted a role for highway construction in solving Germany's *aggregate* unemployment problem (Ritschl 2003). Construction itself was often performed by brigades that moved along with highway segments (Evans 2005), making direct local employment effects unlikely. Nevertheless, spending by construction workers or the placing of orders with local firms may have created employment around highway construction. In addition, local tourism may have benefited: Construction sites often became a popular destination for weekend trips (Eichner-Ramm 2008). Our empirical results suggest that such indirect job generation via highway construction was probably limited.

Table 6—Car Ownership

Dependent variable: Change in votes for the Nazi Party, Nov'33–Aug'34		
	Vehicle ownership relative to median	
	Below	Above
	(1)	(2)
log(distance HW)	−0.0270	−0.0393
	(0.0191)	(0.0127)
Test that coeff are equal	column 1 = column 2	
	p-value: 0.59	
Baseline controls	✓	✓
Observations	1,682	1,547
Adjusted R^2	0.130	0.412

Notes: Robust standard errors in parentheses. "Baseline controls" include log city population and the unemployment rate in 1933, as well as the (standardized) share of pro-Nazi votes in the November 1933 election.

B. *Did Vehicle Ownership Matter for the Effect of Highways?*

Direct effects could also come through vehicle ownership and the greater use value of automobiles. Germany as a whole had quite low vehicle ownership, with only 674,000 cars on the road (including buses) in 1934, plus another 984,000 motorcycles—equivalent to 10 cars and 15 motorcycles per 1,000. Any benefits from using these vehicles would have had to be anticipated in August 1934 since new roads only opened from 1935 onward. In Table 6, we stratify the sample by the density of motor vehicles (including buses, motorcycles, and cars, available at the province level from Frik 2004). There is no evidence of greater electoral gains in areas with higher vehicle ownership; the coefficients on distance to highway are very similar in both subsamples. Our results thus suggest that direct economic benefits are unlikely to account for the effect of highway construction on local Nazi support.

C. *Radio Propaganda and "Competence"*

Next, we examine another hypothesis—that highway building convinced people of the regime's competence and of their having the population's best interests at heart. As Hitler came to power, unemployment was by far the most burning economic issue. Goebbels's propaganda loudly proclaimed that the country's recovery after January 1933 was a direct result of the regime's policies—and none were more visible throughout the country (or more talked about in government propaganda) than highway construction. This suggests that places with greater exposure to propaganda should have experienced greater vote gains for the Nazis—and particularly so where the roads seemingly delivered "concrete" proof of the Nazis' claim that they were responsible for the aggregate decline in unemployment. To proxy for propaganda, we focus on Goebbels's most potent tool, radio, using city-level data radio listenership (as predicted by signal strength, driven by terrain characteristics; see Section IIIC).

Table 7—Radio Reception and Highway Building

Dependent variable: Change in (standardized) votes for the Nazi Party, Nov'33–Aug'34						
Cities in sample:	Radio reception[a]		All cities			Cities <20km from any HW[c]
	Low	High	Baseline	Spatial corr.[b]	Interactions	
	(1)	(2)	(3)	(4)	(5)	(6)
log(distance HW)	−0.0211	−0.0864	0.0932	0.0932	0.151	0.124
	(0.0210)	(0.0172)	(0.0633)	(0.0601)	(0.119)	(0.0852)
Test that coeff are equal	column 1 = column 2					
	p-value: 0.016					
Radio listeners (predicted)[d]			3.325	3.325	3.918	3.789
			(0.857)	(0.947)	(0.934)	(1.008)
log(distance HW) × radio list. (pred.)			−0.586	−0.586	−0.780	−0.683
			(0.235)	(0.242)	(0.267)	(0.305)
Baseline controls	✓	✓	✓	✓	✓	✓
log(distance HW) × baseline controls					✓	
Observations	1,566	1,566	3,132	3,132	3,132	1,926
Adjusted R^2	0.207	0.164	0.244		0.245	0.216

Notes: Robust standard errors (except for column 4) in parentheses. "Distance HW" is the distance of a city to the nearest highway segment that was under construction by August 1934. "Baseline controls" include log city population and the unemployment rate in 1933, as well as the (standardized) share of pro-Nazi votes in the November 1933 election. In columns 3 and 4, we also control for log distance to the nearest large city (more than 500,000 inhabitants) to account for the potential endogenous location of radio transmitters.

[a]Corresponds to radio signal strength below versus above median.

[b]Standard errors adjusted for arbitrary spatial correlation within 700 km, using the Colella et al. (2019) algorithm (see notes to Table 3 for detail).

[c]The sample in column 6 includes only cities within 20 km distance to the planned highway network (any highway segment that was planned, approved, or built by August 1934).

[d]Nonlinear prediction of radio listeners at the city level, as described in Supplemental Appendix A.7.

Table 7 analyzes the relationship between radio coverage, highway construction, and Nazi vote gains. To account for endogeneity in radio listenership, we follow Adena et al. (2015) and use local radio signal strength (see Section IIIC and Supplemental Appendix A.7 for detail). First, we split the sample into areas with below- and above-median radio signal strength. In the former, signal strength was too low for good radio reception, except for enthusiasts who purchased high-quality receivers. In these areas, we find only a small and insignificant relationship between distance to highways and Nazi vote gains (column 1). On the other hand, in areas with above-median signal strength (column 2), we find a strong coefficient on highway construction, which is significantly larger (in absolute value) than the coefficient in column 1, with a *p*-value of 0.016 for the difference. Next, we turn to radio listenership, predicted by signal strength.[47] We run regressions in the full sample, including interaction terms between predicted radio listenership and distance to highway construction. The results in column 3 show that (predicted) radio listenership itself is strongly positively associated with Nazi support, while the interaction

[47]The corresponding nonparametric "first-stage" regressions of radio listeners on deciles of radio signal strength are reported in Supplemental Appendix A.7. Because radio transmitters tended to be located close to large cities (Adena et al. 2015), we include—in addition to our baseline controls—the log distance to the nearest large city (with more than 500,000 inhabitants in 1933) in these regressions. The "second-stage" regressions in columns 3–6 of Table 7 also include this control variable.

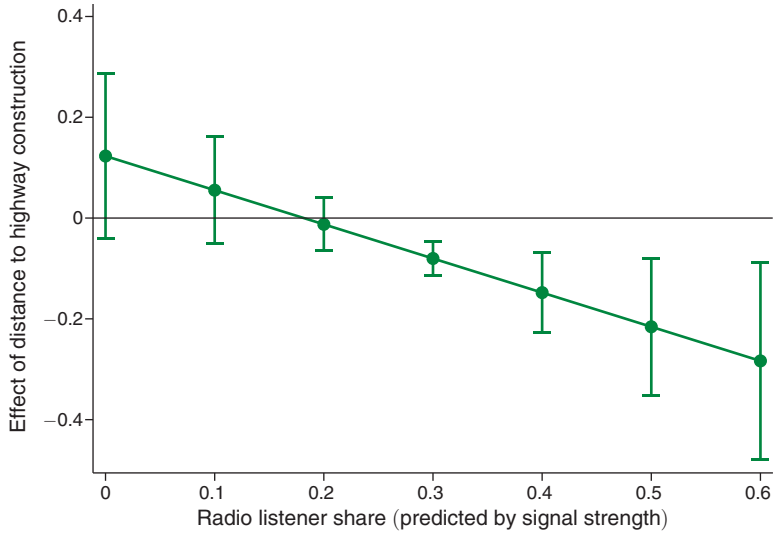

FIGURE 8. EFFECT OF DISTANCE TO HIGHWAYS ON NAZI VOTE GAIN, BY RADIO LISTENER SHARE

Notes: The figure visualizes the estimate from Table 7, column 5, showing that the effect of distance to highways under construction on pro-Nazi votes is particularly pronounced in areas with high radio listenership (as predicted by radio signal strength).

term with distance to highway construction is strongly negative. The results are almost identical when we allow for spatial correlation of the error terms (column 4). Column 5 addresses the concerns that radio signal strength may vary systematically with location attributes such as local population, which in turn could affect our interaction results. To account for this possibility, we include interactions between log(distance HW) and our baseline controls. If anything, we find that the main interaction coefficient of interest is even stronger. Overall, the interaction results suggest that Autobahn proximity had a larger effect on electoral support when combined with radio propaganda.

Are our interaction results in columns 3–5 driven by remote areas that had neither highway construction nor radio coverage? In column 6, we exclude all cities from the sample that were more than 20 km from any planned, approved, or built highway segment. In the remaining subsample, we again find a negative interaction term that is very similar in magnitude. This suggests that remote areas do not drive our results.

Figure 8 illustrates the complementarity between highways and radio: In areas with predicted listenership below 20 percent, distance to the Autobahn does not predict Nazi vote gains. The relationship becomes negative and significant for predicted listener shares of 30 percent, with a coefficient on distance of about −0.1, in line with our baseline estimate. At a listenership share of 40 percent (the upper decile in our data), the coefficient on log distance is −0.2. This corresponds to persuasion rates of about 30 percent when going from 100 km to 1 km of highway distance, which compares favorably with persuasion rates in contemporary studies (Della Vigna and Gentzkow 2010).

Table 8—Political Stability of Weimar States and Highway Building

Dependent variable: Change in (standardized) votes for the Nazi Party, Nov'33–Aug'34					
	Political stability[a]		Prussia only	All cities	
	High	Low		Stability1[b]	Stability2[b]
	(1)	(2)	(3)	(4)	(5)
log(distance HW)	−0.0310	−0.112	−0.0353	−0.0507	−0.0631
	(0.0257)	(0.0193)	(0.0176)	(0.0114)	(0.0156)
Test that coeff are equal	column 1 = column 2 *p*-value: 0.0105				
Weimar state stability[b]				−0.0935	−0.0977
				(0.0464)	(0.0494)
log distance HW × state stability				0.0188	0.0199
				(0.00798)	(0.00946)
Baseline controls	✓	✓	✓	✓	✓
Observations	711	716	1,792	3,219	3,218
Adjusted R^2	0.281	0.447	0.150	0.247	0.246

Notes: Robust standard errors in parentheses. "Distance HW" is the distance of a city to the nearest highway segment that was under construction by August 1934. "Baseline controls" include log city population and the unemployment rate in 1933, as well as the (standardized) share of pro-Nazi votes in the November 1933 election. Regressions also include dummies for the three states that were already governed by the NSDAP before January 1933: Mecklenburg-Schwerin, Oldenburg, and Thuringia.

[a]Weimar state's political stability above median (high stability) versus below median (low stability), based on the Stability1 measure. Prussia is excluded in columns 1 and 2 (see footnote 48).

[b]Stability1 is the measure for Weimar states' political stability between November 1918 and June 1932 from Satyanath, Voigtländer, and Voth (2017). Stability2 extends this measure to January 1933.

D. *Highways and Political Instability at the State Level*

Authoritarian regimes often garner support by promising a return to order and stability. German voters may have valued roadbuilding not only because of (perceived) economic benefits but because it demonstrated government effectiveness. Since the support of middle-class Germans for the Weimar Republic was undermined by political turmoil, we hypothesize that areas with more state-level instability saw larger gains in support for the Nazi regime—and all the more where the new roads were close. To test this hypothesis, we use the data on state government stability from Satyanath, Nico, and Voth (2017), as discussed in Section IIID.

Table 8 shows the regression results for political stability. We first divide our sample into three groups—above-median stability, below-median stability, and Prussia. The main reason for not including Prussia in either group is that it would dominate the sample—it accounts for more than half of all observations.[48] In the more stable states, we find only a small and insignificant effect of highway distance on Nazi support (column 1). In the low-stability areas, however, the coefficient

[48]In addition, Prussia's initial role as "a bulwark" of democracy in Weimar Germany was gutted in a coup d'etat by the central government in July 1932. Thus, when the NSDAP came to power in Berlin in January 1933, Prussia had seen a period of relatively stable government followed by a period of turmoil at the end of the Weimar Republic. Our regressions also include dummies for the three states that were already governed by the NSDAP before January 1933: Mecklenburg-Schwerin, Oldenburg, and Thuringia.

on distance is almost four times larger (column 2), and the difference in coefficient size is highly significant. Prussia shows an intermediate score (column 3), with statistically significant but smaller effects than in the highly unstable states. Column 4 in Table 8 reports a full regression specification including interaction effects between distance to highways and political stability. We find a significant and positive interaction effect, meaning that political stability curtailed the (negative) effect of distance to highways. The coefficient on Weimar state stability is negative and significant, suggesting that more stable states generally saw smaller increases in regime support.[49] The final column of Table 8 reports a robustness check, using an updated measure of the instability indicator in Satyanath, Nico, and Voth (2017), where we extend the coding from the original period of 1918–June 1932 (before the central government's coup d'etat in Prussia) to January 1933, when the NSDAP took over the central government. The coefficients and significance levels are almost unchanged.

Figure 9 illustrates the central finding from Table 8—the more stable that a federal state was before 1933, the lower the impact of road construction was on Nazi support. It seems likely that issues relating to order and government effectiveness were more salient in states where parliamentary "chaos" had been common. There, support for the Nazis grew more as a function of highway proximity than it did in stable states. This suggests that the Nazi regime scored an important symbolic victory by roadbuilding: Showcasing the rapid and highly effective implementation of a large-scale project also may have boosted support for the Nazi regime because it conveyed information about the ability and willingness to "get things done," overcoming the perceived indecisiveness of the parliamentary system.

E. *Highways Approved for Construction* (*but Not Yet Built*)

So far, we have focused on the distance to highway segments *under construction*. The map shown in Figure 2 also contains segments that were approved for building but were not yet listed as "under construction." As discussed in Section IIA, the transition between the two is fluid—approved segments likely had engineers staking out the trajectories, and the public knew that the highway was coming. In Table 9, we use the distance both to highways under construction and to approved highway segments.[50] We begin with the full sample. The coefficients on distance both to approved roads and to constructed segments are statistically significant and of similar magnitude (column 1). Both had a significantly stronger effect on Nazi support than planned (but not yet approved/constructed) segments.[51] Using the minimum of both distances also yields a negative and significant coefficient (column 3). These findings are confirmed in columns 4 and 5, where we control for distance

[49] This does not reflect convergence from lower initial support in November 33 in low-stability states—average "yes" votes for the regime were 88 percent in states with above-median stability and 90 percent in the more unstable ones. Thus, if anything, it was harder to further raise support in unstable states.

[50] The two distances are highly correlated since approved segments typically connect to those under construction. Thus, the results need to be interpreted with caution.

[51] When all types of highway segments are included together (column 2 in Table 9), the distance coefficients on both constructed and approved segments are statistically significant and negative, while the coefficient on "any HW segment" (which captures the remaining—planned—category) is small and positive.

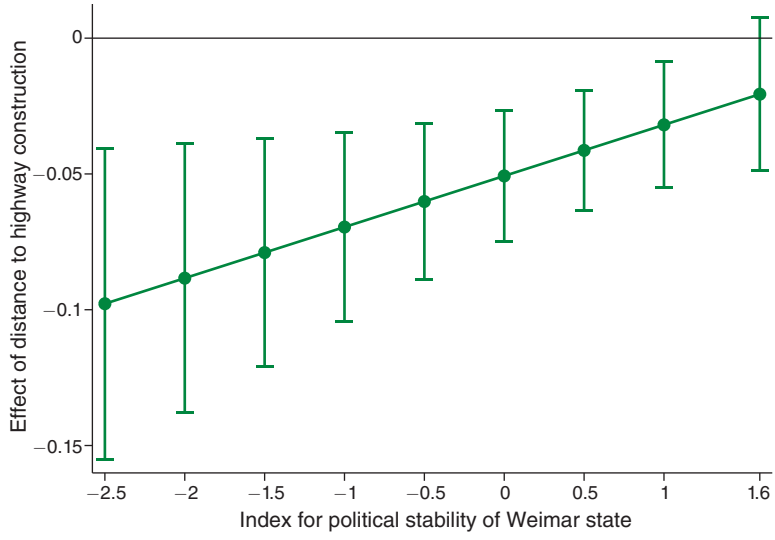

Figure 9. Effect of Distance to Highways, by Political Stability of Weimar State

Notes: The figure visualizes the estimate from Table 8, column 4, showing that the effect of distance to highways under construction on pro-Nazi votes was strongest in Weimar states with low political stability. The range of the *x*-axis corresponds to the first through the ninety-nineth percentile of the political stability measure from Satyanath, Voigtländer, and Voth (2017).

to any (planned/approved/constructed) highway and restrict the sample to cities located within 20 km of any highway segment. Overall, the evidence suggests that there are no crucial differences between highway segments under construction and those approved for construction. Once a segment was approved for construction, its trajectory was unlikely to change; for both approved segments and those under construction, the local public knew that the road was coming. In contrast, planned segments would be built at an uncertain future date, and the exact trajectory was not widely known (see Section IIA). Our findings help to shed light on the mechanism by which highways affected Nazi support. Segments that were merely approved for construction did not (yet) create any direct employment effects or other demand spillovers. Our results thus support the interpretation that highways also affected Nazi support by signaling competence in promoting economic progress and social order (and, thus, future economic gains), and not only through immediate local economic effects.

VII. Robustness

In this section, we demonstrate the robustness of our findings. We examine issues of balancedness, present results from placebo tests and different measures of distance to highways, and perform matching estimations. The majority of tables reporting robustness checks are shown in the Appendix, but their results and interpretation are summarized here.

Table 9—Highways under Construction and Those Approved for Construction

Dependent variable: Change in standardized pro-Nazi votes, Nov'33–Aug'34					
Sample includes:	All cities			Cities located <20km from any HW[a]	
	(1)	(2)	(3)	(4)	(5)
log(distance HW under construction)	−0.0292	−0.0412		−0.0398	
	(0.0132)	(0.0147)		(0.0158)	
log(distance approved HW)	−0.0316	−0.0394		−0.0259	
	(0.0148)	(0.0153)		(0.0166)	
log(distance HW approved or under construction)			−0.0440		−0.0536
			(0.0112)		(0.0152)
log(distance to any HW)[a]		0.0204		0.00944	0.0140
		(0.0110)		(0.0127)	(0.0132)
All controls	✓	✓	✓	✓	✓
District FE	✓	✓	✓	✓	✓
Observations	3,213	3,213	3,213	1,972	1,972
Adjusted R^2	0.552	0.552	0.552	0.560	0.561

Notes: Highways approved for construction are segments where people knew construction would start shortly but work had not begun. All controls include log city population, the unemployment rate in 1933, the (standardized) share of pro-Nazi votes in the November 1933 election, the share of blue-collar workers, and the share of industrial employment in 1933, as well as the shares of Catholics and of Jews in 1925. District FE correspond to 77 *Regierungsbezirke* in Weimar Germany.

[a]Distance to any highway is the distance to the nearest planned, approved, or built highway segment.

A. *Balancing the Sample*

As we discussed above, covariates are not balanced when comparing cities with and without highway construction (see Table 2). In Table 10, we address this issue by using entropy weighting to effectively create a balanced sample. This method follows Hainmueller (2012); to implement it, we use the 20 km distance threshold to define the treatment and control group. Entropy balancing reweights the control group data (cities with more than 20 km distance to highway construction) to match the mean of covariates in the "treatment group" (cities within 20 km of highway construction).[52] We confirm the magnitude and significance of our main results in the full sample (columns 1 and 2). In addition, in column 3 of Table 10, we restrict the sample to cities within 20 km of any highway. There, entropy weighting creates a balanced control group from all cities that saw approved or planned (but no actual) construction within 20 km. We obtain very similar results in this more restrictive specification.

B. *Sample Splits*

In Table 11, we provide additional results, stratifying the sample by the most important control variables. Throughout, we report *p*-values for the null that coefficients in the respective subsamples are the same. Table 11, panel A subdivides the

[52] Supplemental Appendix Table A.8 shows that entropy balancing delivers an almost perfectly balanced control group, with the mean of all correlates deviating by less than 0.1 percent from the corresponding mean in the treated group.

Table 10—Entropy Balancing

Dependent variable: Change in standardized pro-Nazi votes, Nov'33–Aug'34			
Sample includes:	All cities		Cities located <20km from any HW[a]
	(1)	(2)	(3)
HW within 20km	0.113	0.0852	0.0914
	(0.0317)	(0.0317)	(0.0393)
Baseline controls	✓	✓	✓
Additional controls		✓	✓
District FE		✓	✓
Observations	3,231	3,213	1,972
Adjusted R^2	0.005	0.242	0.259

Notes: Robust standard errors in parentheses. Regressions are estimated using entropy weighting, which creates balanced samples by reweighting the control group data (farther than 20 km from highway construction) to match the mean of covariates in the treatment group (less than 20 km from highway construction). See Hainmueller and Xu (2013) for details; Table A.8 in the Supplemental Appendix shows the means for covariates before and after rebalancing. "Baseline controls" include log city population and the unemployment rate in 1933, as well as the (standardized) share of pro-Nazi votes in the November 1933 election. "Additional controls" include all other variables listed in Table 2. "District FE" correspond to 77 *Regierungsbezirke* in Weimar Germany.

[a]Distance to any highway is the distance to the nearest planned, approved, or built highway segment.

sample by political preferences in March 1933. Where the Nazi Party was already polling strongly, the highway made less of a difference—the coefficient on distance to highway construction is significantly smaller in column 2 compared to column 1. The opposite is true for areas with substantial support for parties in the political center (SPD, Zentrum, and BVP). Here, the highway worked particularly well as a tool to change the voting behavior of the population (as shown by the significantly larger coefficient in column 4, as compared to column 3). In areas with massive Communist support, however, the highway had less of an effect—vote gains depended less on distance to the Autobahn (columns 5 and 6). This suggests that the highways were less effective in overcoming opposition at the opposite ideological extreme.[53] In other words, highway construction seems to have worked best in persuading voters in the political center—social groups that longed for a return of the order, stability, and perceived effectiveness of the state under the empire (Peukert 1993).

In panel B of Table 11, we stratify by religious composition and city size. In predominantly Catholic areas, highway building led to particularly high gains in August 1934 (columns 1 and 2). Catholics had been much more resistant to the Nazi message than Protestants until 1933, in part because they had their own party representing their interests, the Zentrum (Falter 1991), but also because the Catholic Church warned about the dangers of National Socialism (Spenkuch and Tillmann 2017). However, Catholic areas were not as fervently opposed to the Nazi regime as communists (Falter 1991). Catholic areas constitute an important part of the

[53]We find further support for this interpretation when stratifying our sample by socioeconomic characteristics that were associated with strong opposition to the Nazi regime: Areas with above-median blue-collar workers or industrial workers (the main recruiting ground for the Communist Party) also show significantly smaller effects of Autobahn construction. Note that our results are potentially subject to the ecological fallacy and thus do not necessarily reflect underlying differences in *individual* voter preferences across religious or socioeconomic dimensions.

TABLE 11—SAMPLE SPLITS

Dependent variable: Change in votes for the Nazi Party, Nov'33–Aug'34						
	NSDAP relative to median		Moderate parties relative to median		Communist Party relative to median	
	Below	Above	Below	Above	Below	Above
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Sample split by vote shares in March 1933 election						
log(distance HW)	−0.139	−0.0678	−0.0583	−0.154	−0.152	−0.0457
	(0.0239)	(0.0217)	(0.0203)	(0.0267)	(0.0299)	(0.0179)
Test that coeff are equal	column 1 = column 2 *p*-value: 0.024		column 3 = column 4 *p*-value: 0.004		column 5 = column 6 *p*-value: 0.002	
Baseline controls	✓	✓	✓	✓	✓	✓
District FE	✓	✓	✓	✓	✓	✓
Observations	1,608	1,607	1,608	1,607	1,608	1,607
Adjusted R^2	0.393	0.131	0.153	0.369	0.345	0.196
	Share of Catholics relative to 50%		Share of Jews relative to median		City population relative to median	
	Below	Above	Below	Above	Below	Above
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B. Sample split by religion and population size						
log(distance HW)	−0.0543	−0.179	−0.104	−0.111	−0.105	−0.107
	(0.0165)	(0.0361)	(0.0206)	(0.0247)	(0.0244)	(0.0212)
Test that coeff are equal	column 1 = column 2 *p*-value: 0.001		column 3 = column 4 *p*-value: 0.823		column 5 = column 6 *p*-value: 0.949	
Baseline controls	✓	✓	✓	✓	✓	✓
District FE	✓	✓	✓	✓	✓	✓
Observations	2,099	1,132	1,608	1,605	1,616	1,615
Adjusted R^2	0.133	0.336	0.346	0.265	0.312	0.289

Notes: Robust standard errors in parentheses. "Distance HW" is the distance of a city to the nearest highway segment that was under construction by August 1934. "Baseline controls" include log population and unemployment rate in 1933. "District FE" correspond to 77 *Regierungsbezirke* in Weimar Germany.

moderate voters represented in columns 3 and 4 of panel A. Thus, the results here underline that highways seem to have influenced voters closer to the political middle.[54] Jews accounted for only half a percent of the German population; there is no difference in changes in support for the Nazi Party depending on their population share (columns 3 and 4 in panel B of Table 11). There is also no difference by city size—Nazi electoral gains close to the highway were as big in small towns as in big cities (columns 5 and 6).

[54] In our baseline results, the share of Catholics in an area is strongly *negatively* related to the Nazi vote gain between November 1933 and August 1934 (column 4 in Table 3; coefficient not reported separately but available upon request). This makes it unlikely that our results are confounded by convergence of Nazi support in Catholic areas. The data suggest that this convergence had already happened between March and November 1933—over this period, Nazi vote gains are strongly positively related to the share of Catholics.

C. *Placebo Tests*

To ensure that our regressions do not pick up the effect of geographical features associated with transport infrastructure (which may have benefited disproportionately from a general revival of economic conditions), we also perform placebo regressions. In Table A.10 in the Supplemental Appendix, we use two other forms of transport in exactly the same way as the Autobahn: rivers and railways.[55] We find no consistent association between distance to these alternative means of transport and support for the Nazi Party. This makes it unlikely that the highway effects simply capture a general swing of voters toward the Nazis in locations with good communications and access to transport infrastructure.

D. *Matching Results*

To demonstrate that our results are not driven by violations of the linearity assumption, and to further address unobserved heterogeneity, we also perform nearest-neighbor matching. The results are reported in Table A.11 and discussed in more detail in the Supplemental Appendix. We match with two sets of variables—the baseline controls (log population, unemployment in 1933, and Nazi Party support in November 1933), and the extended set (which adds socioeconomic factors such as the share of Jews, of Catholics, of industrial employment, and of blue-collar workers). We use either three-neighbor matching or one neighbor, to form comparison groups with a high degree of similarity in control variables. In addition, we restrict the control group to cities from the same district (77 *Regierungsbezirke*) as the treated observation. We also experiment with defining towns and cities within either 20 km or 5 km of the highway as treated, and we restrict the range of locations from which propensity score neighbors can be drawn to cities in the vicinity of the overall planned highway network. In all specifications, we find large, significant effects. Matching estimation suggests that places "treated" with the highway show 0.1 to 0.18 standard deviations higher increases in support for the Nazi Party overall, confirming the magnitude of our OLS estimates.

E. *Additional Robustness Checks*

In Supplemental Appendix A.2, we perform additional robustness checks. Supplemental Appendix Table A.2 shows that our results hold when we restrict the sample to those cities that were near planned locations of highways—i.e., towns and cities that would eventually see highway construction according to the plans. In Supplemental Appendix Figure A.3, we show that our results are stable when we use cutoffs different from 20 km in the dichotomous specifications of "treatment" by highway construction. Supplemental Appendix Table A.3 shows that our IV results with the full set of controls are robust when we only use LCPs connecting top-20

[55] We take data on historical trajectories of canals and railways from HGIS (https://www.digihist.de/html/hgisg/index.htm)—the historical information system for Germany (Kunz and Zipf 2020). For each town, we code up distance to the nearest railway line or river.

cities in terms of population. This makes it unlikely that our IV results are driven by Nazi planners strategically picking end points with the intention to "treat" cities in the middle by highway construction.

VIII. Conclusion

Democratic reversals are common and often last for a long time. Despite their grave consequences, they are generally not well understood.[56] We focus on the entrenchment of dictatorship in a case with major implications for world history: the Hitler government's consolidation after 1933. The Nazi regime went from a relatively fragile dictatorship to a firmly entrenched regime. Our results strongly suggest that the construction of the world's first high-speed road reduced electoral opposition to the Nazis: Where the Autobahn was being built by the time of the 1934 plebiscite, electoral support for the dictatorship increased significantly.[57]

The effects are both quantitatively important and likely to be causal. Persuasion rates indicate that more than 10 percent of Germans previously opposed to the regime changed their minds as a result of highway construction.[58] We confirm our findings when we predict where roadbuilding should occur based on terrain features and the associated cost of construction. We also show that distance to the 1934 Autobahn construction is unrelated to Nazi support in prior elections, before highway construction began, and that other transport infrastructure does not have similar predictive power.

Why did motorway building reduce opposition to the regime? The Nazis lost no time claiming that Autobahn construction reduced unemployment—despite the fact that the data show no such relationship, at least not at the local level. Accordingly, the regime scored greater electoral successes near highway construction—even for highway segments that were merely approved (and known to the local public) but on which construction had not yet begun. Highways increased Nazi support all the more so where radio coverage was good, exposing locals to Nazi propaganda that exploited highways as a signal of regime competence. This suggests an important complementarity between *nationwide* propaganda and the *local* visibility of progress. Thus, while the true effect of highway building on the nationwide post-depression upturn was probably modest (Ritschl 1998), the regime succeeded in convincing the German public (and many foreign observers, including John Maynard Keynes) that the Autobahn played a crucial role in reviving the German economy.

Because building began quickly, and because it went hand-in-hand with a local decline in unemployment, the Autobahn seemingly demonstrated the new government's abilities—suggesting government "competence," along the lines of Rogoff (1990). The regime's determination and effectiveness was particularly successful in

[56] The empirical literature has identified several factors that affect the chances of a democracy surviving, such as negative economic shocks and the brevity of democratic rule (Linz and Stepan 1978; Persson and Tabellini 2009; De Bromhead, Eichengreen, and O'Rourke 2013).

[57] This finding is in contrast to earlier research on highway construction under dictatorships, which had highlighted the importance of repression and military use (Saiz 2006).

[58] The implied persuasion rate may be a lower bound because our estimates do not reflect *aggregate* vote changes. However, the opposite is also conceivable if places far away from highways felt left out and *reduced* their votes for the Nazis.

swaying voters in federal states with more political instability during the Weimar Republic. In other words, roadbuilding emphasized Nazi Germany's organizational effectiveness and economic competence, underlining its determination to make Germany "great" again.[59]

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[59] The Nazi regime routinely boasted how it would (and allegedly did) reestablish Germany's greatness. For example, in his Reichstag speech on January 30, 1939, Hitler took stock of his first six years of government, arguing that economic and political measures helped to "finally re-erect the great Reich of the German people."

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ONLINE APPENDIX

- Not for publication -

Highway to Hitler

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Appendix A.1: Background on Highway Construction and Election Results

A.1.a. Highway Planning and Construction

Figure A.1 shows the number of workers employed in highway construction between 1933 and 1938.

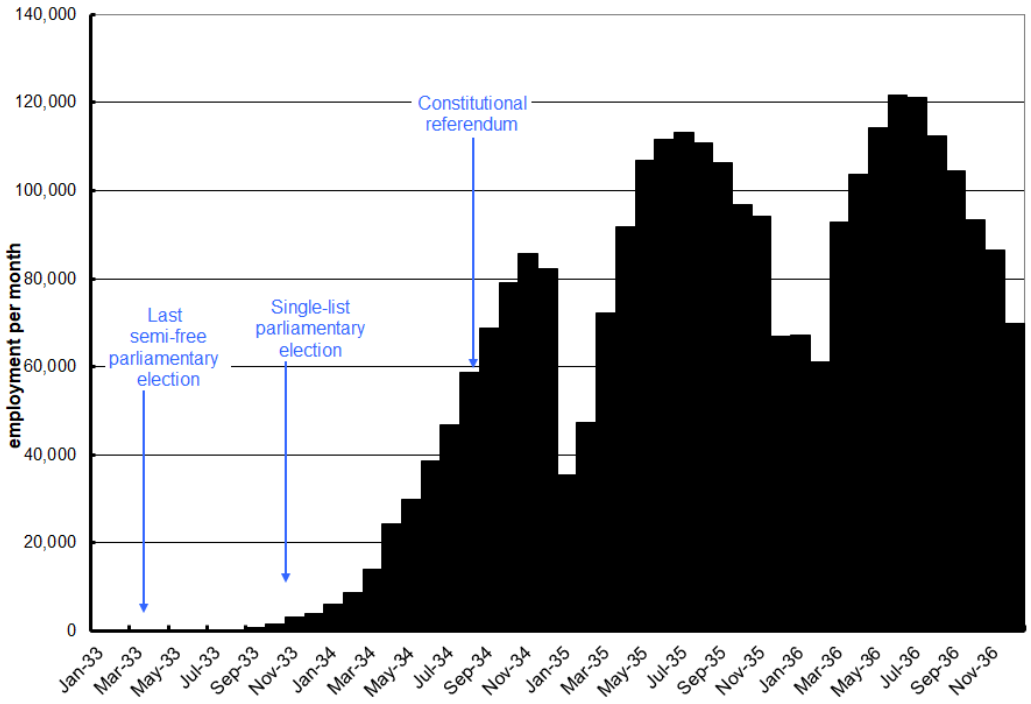

Figure A.1: Manpower used for Highway Construction

Source: Data from Humann (2011, p.83).

Table A.1 lists the 39 city pairs that were to be connected in the first wave of highway construction, according to the plans listed in Jahnke (1936). There are altogether 32 cities that were to be connected.

Table A.1: Terminal city connection pairs used to construct Least Cost Paths

	City A	City B		City A	City B
1.	Lübeck	Hamburg	21.	Berlin	Frankfurt an der Oder
2.	Hamburg	Hannover	22.	Emmerich	Duisburg
3.	Hannover	Kassel	23.	Köln	Duisburg
4.	Kassel	Frankfurt am Main	24.	Köln	Frankfurt am Main
5.	Frankfurt am Main	Karlsruhe	25.	Nürnberg	Frankfurt am Main
6.	Stettin	Berlin	26.	Nürnberg	Passau
7.	Berlin	Leipzig	27.	Hamburg	Berlin
8.	Leipzig	Nürnberg	28.	Berlin	Breslau
9.	Nürnberg	München	29.	Breslau	Gleiwitz
10.	Karlsruhe	Stuttgart	30.	Gleiwitz	Beuthen
11.	Stuttgart	Ulm	31.	Aachen	Köln
12.	Ulm	München	32.	Köln	Dortmund
13.	München	Salzburg	33.	Dortmund	Bremen
14.	Kassel	Erfurt	34.	Hamburg	Bremen
15.	Dresden	Erfurt	35.	Dresden	Berlin
16.	Dresden	Breslau	36.	Chemnitz	Hof
17.	Dortmund	Hannover	37.	Göttingen	Eisenach
18.	Berlin	Hannover	38.	Eisenach	Nürnberg
19.	West Prussia Border	Stettin	39.	Stuttgart	Nürnberg
20.	East Prussia Border	South-East Königsberg			

Source: Terminal City Connections as listed in Jahnke (1936) "1000 km Reichsautobahnen" pp. 973-974. Note that East Prussia was a separated from Germany's main territory by a Polish corridor. Thus, connecting East Prussia to the rest of Germany by a highway required two separate connections: "West Prussia Border – Stettin" and "East Prussia Border – South-East Königsberg."

A.1.a. Distribution of Pro-Nazi Vote Shares in 1933-34

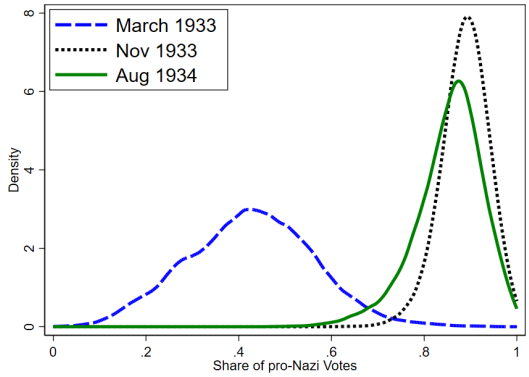

Figure A.2: Support for the Nazi Regime, 1933-34

Note: The figure shows the distribution of pro-Nazi votes across cities in Germany in the March 1933 election, the November 1933 election, and in the August 1934 referendum.

Appendix A.2 Additional Empirical Results

A.2.a. Subsample analysis – Areas with planned highways only

In the main text in Section 5, we compared locations close to actual highway segments with all other places in Germany. In the following, we focus on the subset of the data that will eventually be part of the highway network: By excluding areas that will never receive the highway, we are increasing the similarity of towns and cities in our sample. The relevant variation now arises only from differences in timing of construction – and not from selection of cities that (eventually) get highways nearby.

In Table A.2, col 1, we first add the minimum distance to any type of highway segment (planned, approved for construction, or under construction) to our specification. The corresponding coefficient is small, positive, and insignificant, while the coefficient on distance to highway under construction remains quantitatively unchanged (compared to our main results in Table 3) and statistically highly significant. Next, we limit the sample to locations within 20 km of any type of highway segment. This means that we exclude about 1,000 towns and cities in our sample. Nevertheless, the coefficient on distance to highway under construction remains large and significant with and without controls (cols 2 and 3). If we use a simple dichotomous variable for highway construction within 20 km, we find that

this is associated with pro-Nazi votes increasing by 0.23 standard deviations in the basic specification (col 4); when adding district fixed effects and all controls, it still adds 0.06 standard deviations to Nazi support (col 5). When we restrict the sample further, to those places within 5 km of the highway, we find an even bigger coefficient – an increase in Nazi support by 0.12 standard deviations, after the use of all controls and district fixed effects (col 6). The fact that coefficients continue to be large and significant even in a highly restricted subsample strengthens our confidence that it is actual roadbuilding progress that created an additional 'swing' in favor of the Nazi regime.

Table A.2: Planned vs. Built Highways
Dependent variable: Change in standardized pro-Nazi votes, Nov'33-Aug'34

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All cities	Only cities with distance<x km from any HW[#]				
		x<20km	x<20km	x<20km	x<20km	x<5km
log(distance HW under construction)	-0.0995***	-0.108***	-0.0427***			
	(0.0174)	(0.0188)	(0.0155)			
log(distance to any HW)[#]	0.00603	-0.00276	0.00526			
	(0.0152)	(0.0195)	(0.0123)			
HW under construct. within 20km				0.221***	0.0565*	
				(0.0383)	(0.0308)	
HW under construct. within 5km						0.122**
						(0.0541)
All controls			✓		✓	✓
District FE			✓		✓	✓
Observations	3,231	1,978	1,972	1,978	1,972	705
Adjusted R^2	0.013	0.018	0.560	0.017	0.559	0.565

Note: Robust standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "All controls" include log city population, the unemployment rate in 1933, the (standardized) share of pro-Nazi votes in the November 1933 election, the share of blue collar workers and the share of industrial employment in 1933, as well as the share of Catholics and of Jews in 1925. District FE correspond to 77 *Regierungsbezirke* in Weimar Germany.
[#] Distance to any highway is the distance to the nearest planned, approved, or built highway segment.

A.2.b. Alternative Cut-off Distance for Dichotomous Treatment Variable

In the text, we use a cut-off of 20 km distance to the highway to define a dichotomous treatment variable. This is clearly arbitrary. Here we shows that alternative cut-off values yield very similar results. Figure A.3 plots the coefficient on the dummy variable for highway proximity for a number of distances – 5, 10, 20, and 40 km – with and without (baseline) controls. While the results are not identical, they are always significant. The 20 km cut-off

used in the main part of the paper does not yield the biggest coefficients, demonstrating the robustness of our findings and the magnitudes involved.

A.2.c. IV Results for the "Top 20" Highway Network

In the main text we used a set of terminal cities from a Nazi-era publication as nodes for the new highway network. The nodes themselves might have been chosen so as to expose the cities *between them* to highway construction. While inherently unlikely, we nonetheless address this point by constructing an 'objective' highway network that most sensible road planners would have built.

We start with the assumption that connecting the largest 20 cities was a given. Even *if* the Nazi leadership had picked terminal cities to influence people in towns in between, it would always have built connections between the country's largest cities. We compute LCPs only for those connections listed in Jahnke (1936) where both terminal cities belong to the top-20 in terms of population in 1933. This reduces the number of city pairs from 39 to 18. In Table A.3, we repeat our IV analysis, using only these "top-20" least-cost path connections.[1] We find results that resemble those in Table 5, although the coefficient in columns 1 and 5 loses its statistical significance in this narrower specification. However, we continue to find strong and highly significant coefficients in our preferred specification with the full set of controls and district fixed effects (even columns).

[1] There is still substantial overlap between the top-20 network and 1934 building: Out of the 974 cities that lie within 20 km of the "top 20" LCPs, 880 (90.4%) saw actual construction activity within 20 km by the summer of 1934. In contrast, of the 2,257 towns and cities that were more than 20 km away from "top-20" LCPs, only 493 (21.8%) saw construction within 20 km.

Table A.3: Instrumental Variable Regressions with Least Cost Paths – Top 20 Cities Only

	(1)	(2)	(3)	(4)	(5)	(6)
	Reduced Form		First Stage		Second Stage	
Dependent Var:	Change in votes for the Nazi Party, Nov'33-Aug'34		log(distance to highway)		Change in votes for the Nazi Party, Nov'33-Aug'34	
log(distance to Least Cost Path)	-0.0145	-0.0430***	0.328***	0.306***		
	(0.0115)	(0.0136)	(0.0150)	(0.0185)		
log(distance HW)					-0.0445	-0.139***
					(0.0351)	(0.0437)
Weak-IV robust p-value					[0.206]	[0.0014]
Baseline controls	✓	✓	✓	✓	✓	✓
Additional controls		✓		✓		✓
District FE		✓		✓		✓
First Stage F-Statistic			486.1	277.4		
Instrument partial R^2			0.157	0.115		
Observations	3,215	3,197	3,215	3,197	3,215	3,195
Adjusted R^2	0.018	0.372	0.247	0.476		

Note: Robust standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "Distance HW" is the distance of a city to the nearest highway segment that was under construction by August 1934. Baseline controls include log population and unemployment rate in 1933. Additional controls include the share of blue collar workers and the share of industrial employment in 1933, as well as the share of Catholics and of Jews in 1925. District FE correspond to 77 *Regierungsbezirke* in Weimar Germany.

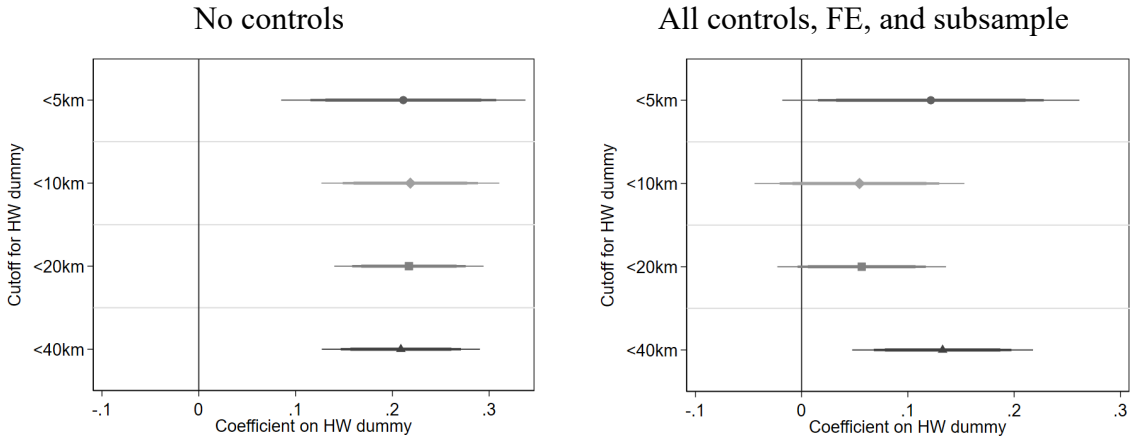

Figure A.3: Coefficients for Different Cutoffs

Note: The figure shows the coefficients of regressing the (standardized) pro-Nazi vote gain between 11/1933 and 8/1934 on a dummy for proximity to highway construction for different cutoffs (less than 5, 10, 20, or 40 km distance). The thick (medium, thin) lines correspond to the 90% (95%, 99%) confidence intervals. The left panel includes no control variables. The right panel shows our most restrictive specification, controlling for log population, unemployment rate in 1933, the (standardized) share of pro-Nazi votes in the November 1933 election, the share of blue collar workers in 1933, the share of industrial employment in 1933, the share of Catholics and of Jews in 1925, as well as district fixed effects for 77 *Regierungsbezirke* in Weimar Germany. In addition, the right panel is based on regressions for subsamples that include only cities within 5, 10, 20, or 40 km (depending on the specification) of any planned, approved, or constructed highway.

Appendix A.3 Unemployment: Data and Results

Detailed data on unemployment at the town/city level for all of our sample is only available in the 1933 census (conducted in June of that year). This makes it difficult to trace the economic effects of highway construction, because we miss a second, similarly detailed measure of unemployment post-treatment. To sidestep this issue, we use city-level reports on unemployment claims filed with the Labor Ministry. These are available for a total of 253 cities, for the end of December 1932, February 1934, and February 1935.[2] Because highway construction only got under way in earnest in the spring of 1934, the February 1934 figures are too early to reflect any (potential) effect of *Autobahn* construction on unemployment. We thus use the February 1935 figures, together with the June 1933 census, to compute the *change* in the unemployment rate over this period. For simplicity, we refer to this variable as the "change in unemployment in 1934." On average in this subsample of 253 towns and cities, the unemployment rate fell by 12.2 percentage points – from 23.0% in June 1933 to 10.8% in February 1934.

Table A.4 documents the relationship between unemployment, Nazi support, and highways. Column 1 shows that support for the Nazi regime increased particularly strongly between November 1933 and August 1934 where the decline in unemployment was more pronounced. This also holds in the subsample in column 2, where we exclude all cities with more than 200,000 inhabitants, as well as all terminal cities (i.e., those that were to be connected by highways, according to the plans). According to the point estimates, a 1 percentage point decrease in unemployment is associated with an increase in Nazi support by 0.025 standard deviations.

Next, in columns 3-6 we analyze the relationship between the change in unemployment and highway construction, controlling for initial unemployment in June 1933. We find that distance to highways is <u>not</u> systematically related to changes in unemployment (col 3). Similarly, there is no the change in unemployment did not differ for towns and cities within

[2] The data sources for the 1934 and 1935 unemployment data are: 1934 Data: Deutscher Gemeindetag (1934): Statistisches Jahrbuch Deutscher Gemeinden 29. Jahrgang (Neue Folge 8. Jahrgang), Gustav Forscher, Jena. 1935 Data: Deutscher Gemeindetag (1935): Statistisches Jahrbuch Deutscher Gemeinden 30. Jahrgang (Neue Folge 9. Jahrgang), Gustav Forscher, Jena.

20km of highway construction, as compared to all other towns (column 3). This holds also when we exclude large and terminal cities (col 4) and in a matching estimation (col 5).[3]

Table A.4: Unemployment, highways, and Nazi support

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Var:	Change in votes for the Nazi Party, Nov'33-Aug'34		Change in unemployment rate in 1934			
Note:		*a*			*a*	*a,b*
Change in unempl. rate in 1934	-2.464***	-2.280***				
	(0.834)	(0.871)				
log(distance HW)			-0.00182			
			(0.00246)			
HW within 20km				0.00183	0.00109	0.00516
				(0.00639)	(0.00641)	(0.0108)
Baseline Controls	✓	✓	✓	✓	✓	*[mv]*
Observations	256	218	256	256	218	218
Adjusted R^2	0.228	0.225	0.341	0.339	0.333	

Note: Robust standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "Distance HW" is the distance of a city to the nearest highway segment that was under construction by August 1934. "Baseline controls" include log city population and the unemployment rate in 1933, as well as the (standardized) share of pro-Nazi votes in the November 1933 election

[a] Sample excludes cities with more than 200,000 inhabitants, as well as all terminal cities that were to be connected by highways.

[b] ATT estimate from propensity score matching, with population in 1933 as matching variable, 1 nearest neighbor. "mv" indicates matching variables.

Appendix A.4 Intimidation and Manipulation: Election Forensics

One obvious concern with our data is that (changes in) votes reflect the regime's repressive activities rather than voter preferences. For example, public officials may have been under greater pressure to show that "their" districts supported the regime if the new highways passed through their constituency, leading to more intimidation at the polling station. We point to three empirical regularities that make this unlikely.

First, the modal German municipality saw a *decline* in Nazi support between November 1933 and August 1934. The differential outperformance of municipalities close to highways comes (on average) from smaller declines, and not from larger increases in support. If local party bosses forged results, it made little sense to do so and then still show declining support for the

[3] Alternatively, using the change in unemployment between February 1934 and February 1935 yields very similar results as those in Table A.4 (results available upon request).

regime. This contradicts the alternative interpretation that the party was simply in a better position to manipulate results in places with highway construction.

Second, as we show below, areas with poor radio coverage showed no effects of highway building. It was only in areas with good radio reception that highway building was associated with greater support. Differential increases in the ability to manipulate and intimidate were not dependent on the radio – local party bosses, if they profited from highway construction in terms of power, would have done so with or without radio signal strength.

Third, the Nazi regime brought intense pressure to bear on the population to vote in its favor – supporting the party and saying "yes" in the referenda. Higher turnout can, of course, be a sign of genuine support – or it can reflect intimidation. Voter turnout, in turn, affects our broad measure of Nazi support (pro-Nazi votes relative to *eligible* voters). To tackle this issue, we use an alternative, narrow measure for change in Nazi support (pro-Nazi votes relative to *actual* voters), which is unaffected by voter turnout. Table A.9 confirms our OLS, IV, and restricted sample results when using this alternative measure for Nazi support. [4] In combination, these three points make it unlikely that road construction itself led to greater intimidation of voters.

Could our results be driven by manipulation of votes after the election? We implement four tests proposed by Hicken and Mebane (2015):

1. **2BL:** Benford's Law – the empirical regularity that lower digits occur more often than higher digits in most sets of numerical data (such as the set of city population sizes of a country).[5]
2. **LastC:** Beber and Scacco (2012) point out that, without manipulation, values of the final digit of the vote count in an unmanipulated election should be distributed uniformly.
3. **C05s:** A binary variable is constructed that takes value one when the vote count for the winning party is either 0 or 5. In a variant of the Beber and Scacco argument, the expected value of this dummy should be 0.2.
4. **P05s:** This test looks at the final digit of the rounded percentage of votes for the winning party. An overabundance of zeros and fives may signal to authorities that

[4] Total turnout grew by 0.3% in places without the highway, and by 0.6% in those within 50km of highway construction. Even if everyone pressed to vote was also forced to vote for the Nazis, this cannot have accounted for more than a 0.3% gain in the yes-share. The actual gain is 1.4% in the 50km band around the highway (and if we examine the co-movement of turnout and yes-votes in general, the implied gain from pushing up turnout by 0.3% is even smaller).

[5] Previous papers using Benford's Law to detect electoral fraud include Pericchi and Torres (2011) and Mebane (2006). The method itself is controversial (Deckert et al. 2011).

vote counters have complied with their superiors and fulfilled their duty of providing fraudulent results. A mean greater than 0.2 of this variable may indicate fraud.

Figure A.4 visualizes the statistics for the four tests, using deviation from mean tests with bootstrapped confidence intervals (the corresponding numbers are shown in Table A.5). We find no systematic evidence of violations across the four tests: none of the means in the full sample ("all") differs significantly from the expected value in the absence of fraud (shown by the horizontal line in each panel of Figure A.4). We also present results for the subsamples with below- and above-median proximity to highway construction ("close" and "far", respectively). Only in one case – the 2BL test for the 1934 election – is the test statistic significantly different from the expected value under "no fraud." But even in this case, the test value does not differ significantly between the subsamples that are close vs. far from highways under construction. For all remaining election forensics tests, the statistics are tightly distributed around the expected values under "no fraud."

Hicken and Mebane (2015, p.39) argue that "an election fraud will not necessarily trigger all of the statistics and tests, but we think a genuine fraud will in general set off many of them." Given that *none* of the test results shows that locations close to the highway had more fraud, we are confident that our results are not driven by manipulation.[6]

[6] In Appendix A.3, we present results that go beyond the mean-comparison tests proposed by Hicken and Mebane (2015). For 2BL and LastC, we present chi-square tests that examine whether the whole distribution deviates from Benford's Law and uniform, respectively. The 2BL chi-square test suggests fraud overall, but there is no evidence for *differential* fraud by distance to highways. The reliability of this test, however, is questionable since it may also reflect other factors such as the grouping of voters into aggregation units (see Hicken and Mebane (2015) and the sources cited therein). The LastC chi-square test, in turn, shows no indication whatsoever for election fraud (with p-values close to one in the 1934 referendum).

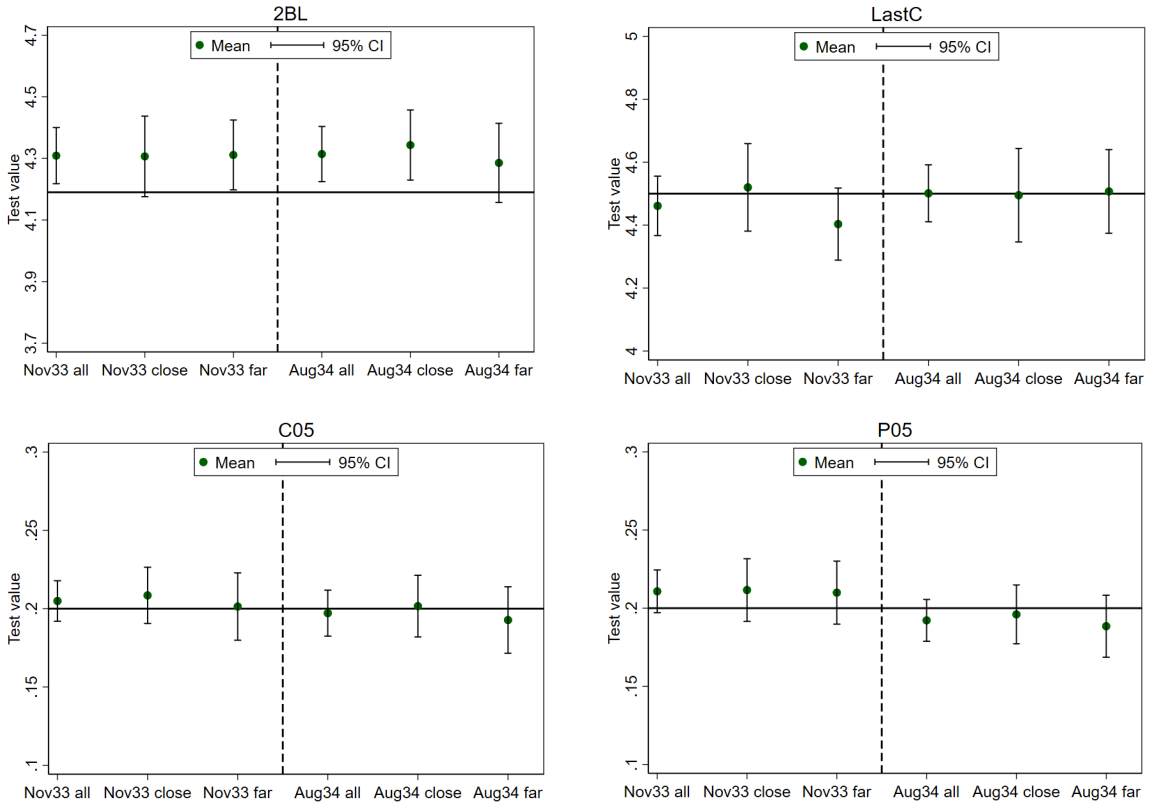

Figure A.4: Election Forensics

Note: One the x-axis, for each election (Nov'33 and Aug'34), "all" = all cities in the sample, "close"=close to highways under construction (below median-distance), "far"=above-median distance. The figure implements four tests of election fraud proposed by Hicken and Mebane (2015). For each test, the horizontal line shows the expected value under no fraud. The tests are the following: 2BL – Benford's Law, based on the second digit of each location's reported pro-Nazi votes (lower digits have a higher frequency according to Benford's Law; the expected average of 2nd digits is 4.19); LastC – analyzes the last digit of the pro-Nazi vote count (this is expected to be normally distributed, with a mean of 4.5); C05 – analyzes the proportion of the pro-Nazi vote count ending in either 0 or 5 (under a uniform distribution, this proportion should be 0.2); P05 – analyzes whether the rounded percentage of pro-Nazi votes has last digit 0 or 5 (these digits are more likely to appear if public officials want to signal that they have committed election fraud. Under a uniform distribution, the corresponding proportion should be 0.2). All statistics are based on reported town/city-level votes in favor of the NSDAP (November 1933) and of "yes" votes in the referendum in August 1934. The 95% confidence intervals are estimated using nonparametric bootstrapping. Table A.5 reports the coefficients.

We now present additional results on (potential) election fraud in November 1933 and August 1934). Table A.5 shows the detailed statistics for the four tests.

Table A.5: Election Forensics – Evidence of Manipulation

Test	Election/ Referendum	Value if no fraud	Full Sample			Distance to HW under construction: Below median			Distance to HW under construction: Above median		
			Mean	95% CI		Mean	95% CI		Mean	95% CI	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
2BL	Nov 33	4.19	4.31	4.22	4.40	4.31	4.18	4.44	4.31	4.20	4.42
	Aug 34	4.19	4.31	4.22	4.40	4.34	4.23	4.46	4.29	4.16	4.41
LastC	Nov 33	4.5	4.46	4.37	4.56	4.52	4.38	4.66	4.40	4.29	4.52
	Aug 34	4.5	4.50	4.41	4.59	4.50	4.35	4.64	4.51	4.37	4.64
C05	Nov 33	0.2	0.20	0.19	0.22	0.21	0.19	0.23	0.20	0.18	0.22
	Aug 34	0.2	0.20	0.18	0.21	0.20	0.18	0.22	0.19	0.17	0.21
P05	Nov 33	0.2	0.21	0.20	0.22	0.21	0.19	0.23	0.21	0.19	0.23
	Aug 34	0.2	0.19	0.18	0.21	0.20	0.18	0.21	0.19	0.17	0.21

Note: The table implements the following tests of election fraud proposed by Hicken and Mebane (2015): 2BL – Benford's Law, based on second digit of each location's reported pro-Nazi votes (lower digits have a higher frequency according to Benford's Law; the expected average of 2nd digits is 4.19); LastC – analyzes the last digit of the pro-Nazi vote count (this is expected to be normally distributed, with a mean of 4.5); C05 – analyzes the proportion of the pro-Nazi vote count ending in either 0 or 5 (under a uniform distribution, this proportion should be 0.2); P05 – analyzes whether the rounded percentage of pro-Nazi votes has last digit 0 or 5 (these digits are more likely to appear if public officials want to signal that they have committed election fraud. Under a uniform distribution, the corresponding proportion should be 0.2). "Value if no fraud" is the mean of the respective variable in the absence of election fraud. See Section 7.a in the paper for detail. All statistics are based on reported town/city-level votes in favor of the NSDAP (November 1933) and of "yes" votes in the referendum in August 1934. The 95% confidence intervals are estimated using nonparametric bootstrapping.

Next, we present χ^2 statistics to test Benford's Law and the "Last C" criterion described in Section 7.a. in the paper. Instead of comparing the mean in the data to the expected value under "no fraud", the χ^2 statistics examine whether the whole distribution deviates from Benford's Law and a uniform distribution, respectively. Figure A.5 illustrates Benford's Law for the two elections. The bars show the actual share of digits; the dotted line reflects the theoretical distribution. We focus on the second digit of pro-Nazi votes because vote manipulation of the first digit would be too egregious – leading to unrealistic shares of pro-Nazi votes in most cases.[7] In November 1933 and August 1934, there appear to be considerable violations of Benford's Law: the second digits 2, 3, and 4 are overrepresented. This is borne out by the χ^2 statistics and the p-values for the null of "no manipulation" shown in Table A.6 (Panel A, col 1). If we are to believe the Benford indicator, this suggests

[7] For example, changing pro-Nazi votes in a city with 1,400 voters from 1,095 to 1,295 may not raise suspicion, while changing it to 2,095 certainly would.

manipulation of votes – although manipulation by systematically rounding second digits to 2,3, or 4 appears somewhat unlikely.

Next, we examine if there is *differential* evidence for cheating for locations close to the highway. Table A.6, Panel A gives the statistical results for Benford's Law.[8] In the November 1933 election and the August 1934 referendum, we observe strong deviations from Benford's law, and thus suggestive evidence for electoral fraud. However, the χ^2 statistics are very similar for cities with above- and below-median distance to highways, suggesting that manipulation did not differ systematically with highway building. Finally, in Panel B of Table A.6. we also report χ^2 statistics for the LastC test of election fraud. Here, we find no indication whatsoever for election fraud.

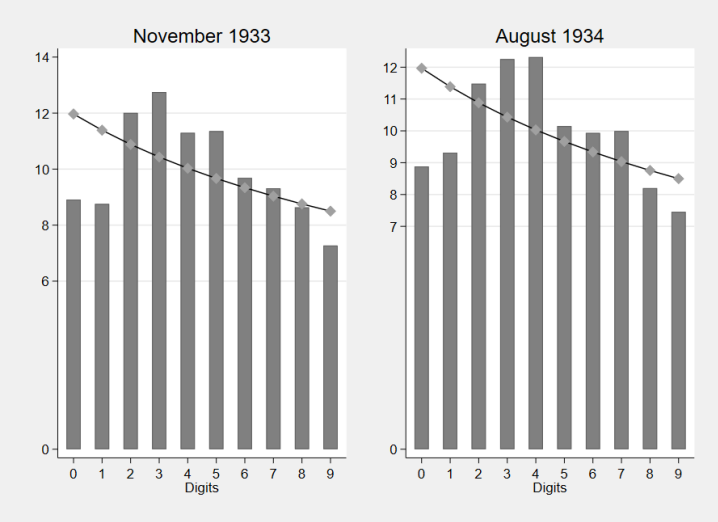

Figure A.5: Benford's Law, based on 2nd digit distributions, Nov 1933 and Aug 1934

[8] We split the sample into cities with below- and above-median distance to highway segments under construction (32 km). This ensures that the two subsamples have the same size, so that we can compare the χ^2 statistics in cols 2 and 3.

Table A.6: Benford's Law and Last C – χ^2 Tests

	(1) Full sample	(2) Distance to highway (under construction): below median	(3) above median
Panel A: Benford's Law			
November 33	86.4	50.7	45.6
p-value	*(0.000)*	*(0.000)*	*(0.000)*
August 34	76.7	48.6	45.2
p-value	*(0.000)*	*(0.000)*	*(0.000)*
Panel B: Last C			
November 33	4.68	5.72	7.06
p-value	*(0.79)*	*(0.77)*	*(0.60)*
August 34	0.63	1.73	1.26
p-value	*(0.99)*	*(0.99)*	*(0.99)*

Note: The table reports Pearson's χ^2 statistic (probability of rejection the null of no manipulation). In Panel A, this statistic is based on the second digit of the number of reported votes in favor of the NSDAP (November '33) and of yes-votes (August'34), using the *digdis* routine in STATA to examine deviations from Benford's Law. In Panel B, deviations from a uniform distribution are examined for the same elections.

Appendix A.5: Additional Figures and Tables for Main Empirical Results

In Table A.7, we focus on three elections – the last relatively free election of March 1933, the November 1933 election when voters could only support the NSDAP or not, and the 1934 plebiscite. Again, we use standardized pro-Nazi vote shares in order to compare Nazi support across the different elections and referenda. Votes for the Nazi Party in March 1933 were not significantly correlated with distance to highways that would be built from late 1933 onwards (Table A.7, col 1). In columns 2 and 3 we examine whether the Nazis gained more support in areas closer to the highway in the subsequent two elections (note that the regressions control for initial support, so that we effectively examine changes). Until November 1933, before highway construction had started on a large scale, highways were not associated with gains in support for the Nazis. It is only in the August 1934 referendum that we find a strong and significant (negative) relationship between distance to highway and pro-Nazi voting.

Table A.7: Highways and Percentage Change in Votes for the Nazi Party

Dep. variable:	(1) NSDAP vote share in March '33 (standardized)	(2) Share of pro-Nazi votes in Nov'33 (standardized)	(3) Share of pro-Nazi votes in Aug'34 (standardized)
log(distance HW)	0.0220	0.0173	-0.0576***
	(0.0157)	(0.0166)	(0.0121)
NSDAP votes March '33		0.252***	
		(0.0165)	
Pro-Nazi votes Nov'33			0.640***
			(0.0157)
Baseline controls	✓	✓	✓
Observations	3,215	3,215	3,231
Adjusted R^2	0.026	0.116	0.399

Note: Robust standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. "Baseline controls" include the log of city population and the unemployment rate in 1933. "Distance HW" is the distance of a city to the nearest highway segment that was under construction by August 1934.

Table A.8 complements our entropy balancing exercise in Table 10 in the paper; it shows that entropy balancing delivers an almost perfectly balanced control group, with the (weighted) mean of all correlates deviating by less than 0.1% from the corresponding mean in the treated group.

Table A.8: Covariates before and after Entropy Balancing

	Treatment group (<20km from HW)	Control group (>20km from HW)	
Variable	Mean	Mean before re-balancing	Mean after re-balancing
Population size 1933	8.682	8.438	8.682
Unemployment rate 1933	0.182	0.137	0.182
Blue collar share 1933	0.363	0.323	0.363
Share Industrial Empl. 1933	0.339	0.276	0.339
Share Catholic 1925	0.287	0.404	0.287
Share Jewish 1925	0.004	0.005	0.004

Note: The table shows the means for covariates in cities in the treated and control group in specification 2 in Table 10 in the paper, before and after rebalancing.

A.6: Additional Figures and Tables for Robustness Checks

Table A.9 uses our narrow measure for change in Nazi support: pro-Nazi votes relative to *actual* voters. As discussed in the main text (Section 4.b), this measure is not affected by voter turnout.

Table A.9: Narrow Definition of Pro-Nazi Votes
Dep. Var.: Narrow Definition of Change in standardized pro-Nazi votes, Nov'33-Aug'34

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		IV		Planning vs. Building	
Sample includes:	All cities		All cities		Cities located <20km from any HW[#]	
log(distance HW under construction)	-0.0888***	-0.0313**	-0.157***	-0.0671**	-0.0911***	-0.0387**
	(0.0147)	(0.0135)	(0.0265)	(0.0306)	(0.0203)	(0.0170)
log(distance to any HW)[#]					-0.00465	0.00401
					(0.0216)	(0.0139)
All controls		✓		✓		✓
District FE		✓		✓		✓
First Stage F-Statistic			787.1	471.4		
Instrument partial R^2			0.294	0.200		
Weak-IV robust p-value			[<0.001]	[0.028]		
Observations	3,203	3,185	3,172	3,153	1,965	1,959
Adjusted R^2	0.010	0.562	0.004	0.386	0.011	0.566

Note: Robust standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The narrow definition of pro-Nazi votes is unaffected by voter turnout; it is defined as the "yes" votes relative to valid votes. "All controls" include log population, unemployment rate in 1933, the (standardized) share of pro-Nazi votes in the November 1933 election, the share of blue collar workers and the share of industrial employment in 1933, as well as the share of Catholics and of Jews in 1925. District FE correspond to 77 *Regierungsbezirke* in Weimar Germany. Cols 1 and 2 replicate our main OLS specifications (corresponding to cols 1 and 4 in Table 3); cols 3 and 4 show the IV results, and cols 5 and 6 control for distance to any planned, approved, or built highway (corresponding to cols 2 and 3 in Table 5).
[#] Distance to any highway is the distance to the nearest planned, approved, or built highway segment.

Table A.10 uses the distance to railroads and canals as a placebo. In col 1, we regress standardized Nazi Party votes in November 1933 on distance to the railroad and find a small, insignificant coefficient; when we look at changes in votes between November 33 and August 1934, we again find a small negative and insignificant coefficient (col 2). When we restrict this to locations close to the highway network – to see if access to alternative transport mattered differentially where the highway was being built – we again find no effect (col 3). For distance to river (cols 4-6), we find negative, insignificant coefficients except when we look at places close to highways, when the sign changes. Overall, there is no evidence in our

placebo exercise to suggest that the highway effects simply capture a general swing of voters towards the Nazis in locations with good communications and access to transport infrastructure.

Table A.10: Placebo Regressions

	(1) Std Nazi votes, Nov'33	(2) Change in Nazi votes, Nov'33-Aug'34	(3)	(4) Std Nazi votes, Nov'33	(5) Change in Nazi votes, Nov'33-Aug'34	(6)
Cities in sample	all	all	Distance any HW<20km[#]	all	all	Distance any HW<20km[#]
log(distance to Railroad)	0.0103	-0.0124	-0.00205			
	(0.0105)	(0.00875)	(0.0114)			
log(distance to River)				-0.00869	-0.00237	0.00666
				(0.0117)	(0.00998)	(0.0117)
Controls:						
Baseline	✓	✓	✓	✓	✓	✓
District FE	✓	✓	✓	✓	✓	✓
Observations	3,231	3,231	1,978	3,231	3,231	1,978
Adjusted R^2	0.291	0.288	0.308	0.291	0.287	0.308

Note: Robust standard errors in parentheses * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[#] Distance to any highway is the distance to the nearest planned, approved, or built highway segment.

Table A.11 presents our matching results. As discussed in the text, we use either 3-neighbor-matching (cols 1-4) or the nearest neighbor only (cols 5 and 6). We also add restrictions on the range of locations from which propensity score neighbors can be drawn (col 2-6). When we restrict matches to come from the same district, we find bigger effects; and even under very strict conditions, matching on both the same district and being close to a *planned, approved or built* highway (cols 4-6), we find effects of up to 0.16 standard deviations increase in Nazi support. Under these specifications, the range of possible matches is restricted even further, to places that are both in the same district and also close to the highway network in general (including planned or approved segments). In other words, when we compare changes in votes for the Nazis in locations that are in the same *Regierungsbezirk* and also close to a planned highway, we find effects that are, if anything, even larger than in our OLS regressions (compared, in particular, to cols 5-6 in Table 3 in the paper, which also uses a 20 km cutoff dummy).

Table A.11: Matching estimation
Dependent variable: Change in votes for the Nazi Party, Nov'33-Aug'34

	(1)	(2)	(3)	(4)	(5)	(6)
	Matching with 3 nearest neighbors				1 nearest neighbor	
HW under construct. within 20km	0.0966***	0.185***	0.163***	0.153***	0.119**	
	(0.0314)	(0.0337)	(0.0379)	(0.0373)	(0.0467)	
HW under construct. within 5km						0.164***
						(0.0575)
Matching variables:						
Baseline controls	✓	✓	✓	✓	✓	✓
Additional controls				✓	✓	✓
Matching restrictions:						
within districts		✓	✓	✓	✓	✓
within 20km of any HW#				✓	✓	
within 5km of any HW#						✓
Observations	3,231	3,231	3,231	3,213	3,213	3,213

Note: The reported coefficients are average treatment effects on the treated (ATT), based on propensity score matching. Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Baseline controls are ln(city pop in 1933), unemployment rate in 1933, and the standardized vote share for the Nazi Party in the November 1933 election. Additional controls include the share of Jews in 1925, the share of Catholics in 1925, the share of blue-collar workers in 1933, and the share of industrial employment in 1933. "Districts" are the 77 *Regierungsbezirke* in Weimar Germany.
Distance to any highway is the distance to the nearest planned, approved, or built highway segment.

A.7: Signal Strength and Radio Listeners

In this section, we describe how we predict city-level radio subscribers. We use predicted rather than reported listener shares in our analysis in Section 7.c in the paper for three reasons: 1) signal strength is less subject to endogeneity concerns than reported radio ownership and subscriptions; 2) signal strength is available at the city level, allowing us to compute predicted listener shares at the city level; 3) as pointed out by Aldena et al. (2015), signal strength has the additional advantage that it proxies for the quality of radio reception.

We obtain data on city-level strength of the radio signal in 1933, based on the irregular terrain model used by Adena et al. (2015). This model takes into account the power and location of transmitters as well as geography such as mountains that block or weaken the signal. Ruben Enikolopov kindly computed city-level signal strength for us, using the coordinates of cities in our sample.

We then predict the share of radio listeners at the city-level – based on a non-parametric relationship with city-level signal strength. In particular, we use dummies for deciles of signal

strength.[9] Because we use the predicted listenership in the `second stage' in Table 7, we also include the same controls here in the `first stage.' These comprise log city population and the unemployment rate in 1933, the (standardized) share of pro-Nazi votes in the November 1933 election, and log distance to the nearest large city (more than 500,000 inhabitants). The latter accounts for the fact that transmitters tended to be located in the proximity of large cities (Adena et al., 2015). The results are reported in Table A.12 and visualized in Figure A.6.

Table A.12: Non-parametric Prediction of Radio Listenership

Dependent Variable: Share of Radio Subscribers

	Coefficients on Deciles of Radio Signal Strength								
	2	3	4	5	6	7	8	9	10
Coeff	-.00836	-.0183**	-.0140	-.0188**	.000573	0.0133	.0305**	.0332***	.0883***
StdErr	(.0065)	(.0093)	(.0092)	(.0095)	(.0099)	(0.011)	(.0125)	(.0124)	(.0243)

Notes: The table reports the coefficients for deciles of radio signal strength. Excluded category is the decile for lowest signal strength. The regression includes the same controls as those used in Table 7 in the paper: log city population and the unemployment rate in 1933, the (standardized) share of pro-Nazi votes in the November 1933 election, and log distance to the nearest large city (more than 500,000 inhabitants). The regression includes 3,132 observations, the R^2 is 0.273. Standard errors are clustered at the Kreis (county) level – i.e., the level of detail for which radio subscriber data are available.

Both the coefficients on signal strength in Table A.12 and the visualization in Figure A.6 show that for low signal strength, there is no relationship with listenership. This has technical reasons – there exists a threshold below which signal quality was insufficient to listen to the radio with standard receivers. Note that, nevertheless, listenership was about 20% in these areas. The reason for this is discussed in Adena et al. (2015, p.1906): It lies in the nature of AM transmission, which allowed people with high-quality receivers to receive (unstable) radio reception even in places with a very weak signal. While the purchase of this more expensive equipment is potentially endogenous, it does not affect our results, since our `first stage' does not predict variation in radio listenership in areas with low signal strength. Thus, the predicted number of listeners only becomes meaningful for signal strength above this threshold. As Figure A.6 shows, this threshold is at a signal strength of about 20. Median signal strength across all cities is about 23. Consequently, the cities with below-median signal

[9] Our results do not depend on using the ten deciles in the non-parametric specification (but these make it easier to report coefficients). When using 100 percentiles of signal strength instead, the results in Table 7 in the paper are almost identical.

strength in Table 7, col 1, largely belong to areas where radio reception was hardly possible without advanced equipment.

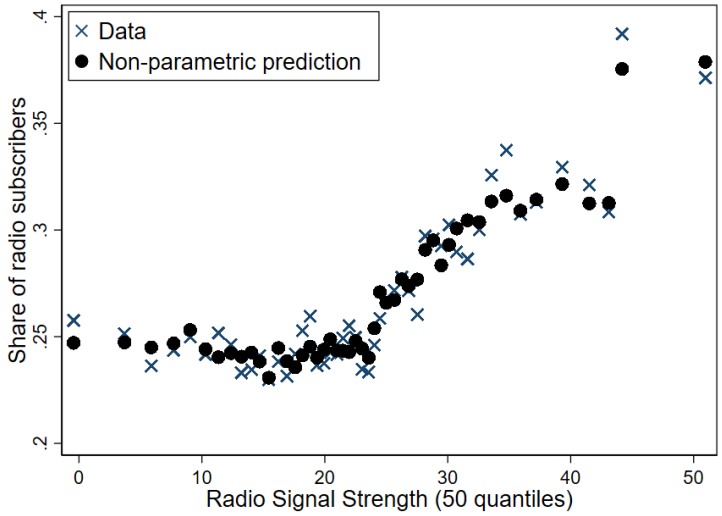

Figure A.6: Radio Signal Strength and Radio Subscribers (Data and Prediction)

Note: Data and predicted values are originally at the city level. To visualize the almost 3,000 data points, the figure groups them into 50 quantiles.