ONLINE APPENDIX

- Not for publication -

Highway to Hitler

Appendix A.1: Highway Planning and Construction

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

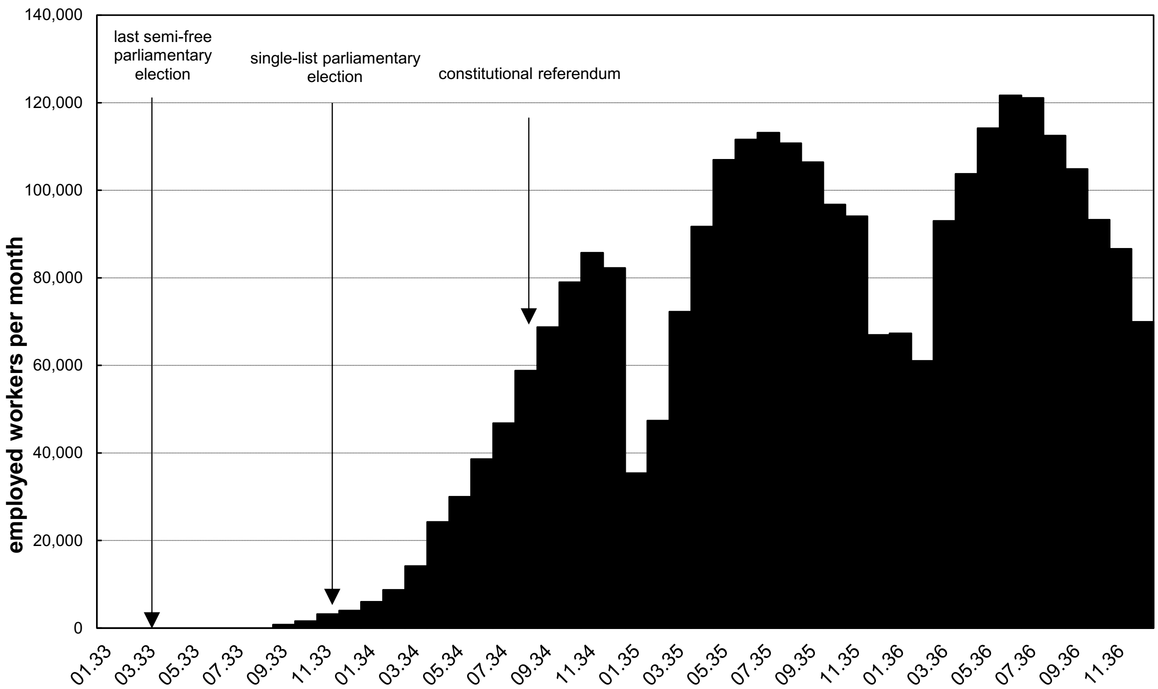

Figure A.1: Manpower used for highway construction

Source: Humann (2011)

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

Source: Terminal City Connections as listed in Jahnke (1936) "1000 km Reichsautobahnen" pp. 973-974.

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)	-0.0974***	-0.109***	-0.0503***			
	(0.0187)	(0.0201)	(0.0163)			
log(distance to any HW)#	0.0103	0.00232	0.0110			
	(0.0165)	(0.0201)	(0.0127)			
HW under construct. within 20km				0.226***	0.0578*	
				(0.0381)	(0.0306)	
HW under construct. within 5km						0.120**
						(0.0533)
All controls			✓		✓	✓
District FE			✓		✓	✓
Observations	2,797	1,799	1,788	2,002	1,979	711
Adjusted R^2	0.012	0.018	0.567	0.018	0.564	0.568

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.2 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 38 to 18. 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 38 to 18. In Table A.3, we repeat our IV analysis, using only these "top-20" least-cost path connections.[1] We find strong and highly significant results that closely resemble those in Table 5.

[1] There is still substantial overlap between the top-20 network and 1934 building: Out of the 1,052 cities that lie within 20 km of the "top 20" LCPs, and 668 (63.50%) saw actual construction activity by the summer of 1934. In contrast, of the 2,224 towns and cities that were more than 20 km away from "top-20" LCPs, only 429 (19.3%) saw construction.

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)	-0.0243**	-0.0367***	0.365***	0.335***		
	(0.0107)	(0.0126)	(0.0151)	(0.0193)		
log(distance HW)					-0.0668**	-0.109***
					(0.0293)	(0.0368)
Weak-IV robust p-value					[0.02]	[0.004]
Baseline controls	✓	✓	✓	✓	✓	✓
Additional controls		✓		✓		✓
District FE		✓		✓		✓
First Stage F-Statistic			590.6	306.9		
Instrument partial R^2			0.212	0.155		
Observations	3,215	3,197	3,215	3,197	3,215	3,195
Adjusted R^2	0.018	0.374	0.295	0.501		

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.

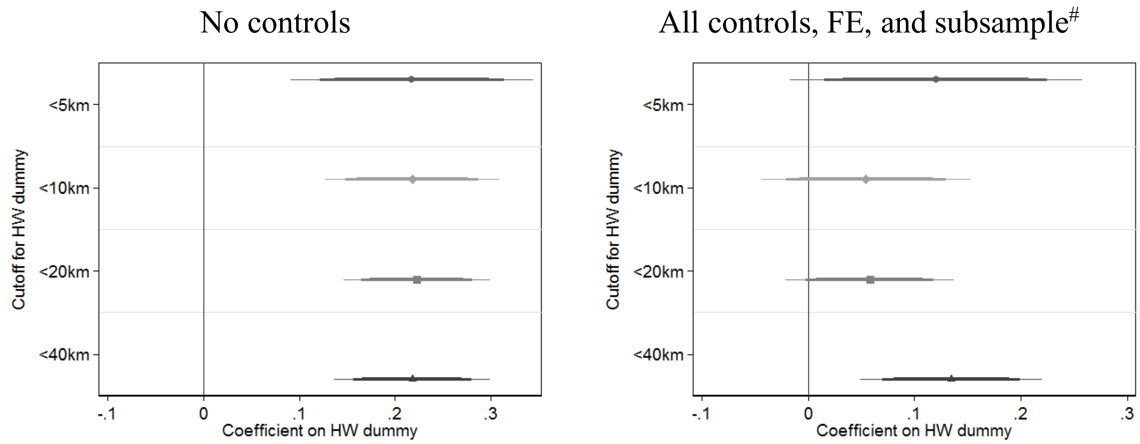

Figure A.2: Vote gain for the Nazi regime, by proximity of cities to highway (defined as less than 5, 10, 20, or 40 km distance). The figure plots the coefficient on a dummy for highway proximity, for a regression where the dependent variable is the change in (standardized) pro-Nazi votes between 11/1933 and 8/1934. 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.
Subsample includes 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. 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 0.026 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 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).[2]

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	-2.588*** (0.813)	-2.467*** (0.860)				
log(distance HW)			-0.00209 (0.00250)			
HW within 20km				0.00138 (0.00652)	0.000252 (0.00645)	0.002414 (0.01063)
Baseline Controls	✓	✓	✓	✓	✓	*[mv]*
Observations	253	218	251	251	217	217
Adjusted R^2	0.031	0.033	0.334	0.332	0.325	

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.

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[2] 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).

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 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.[3] In combination, these three points make it unlikely that road construction itself led to greater intimidation of voters.

[3] 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).

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).[4]
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 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.3 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.3). 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.[5]

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[4] 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).

[5] 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).

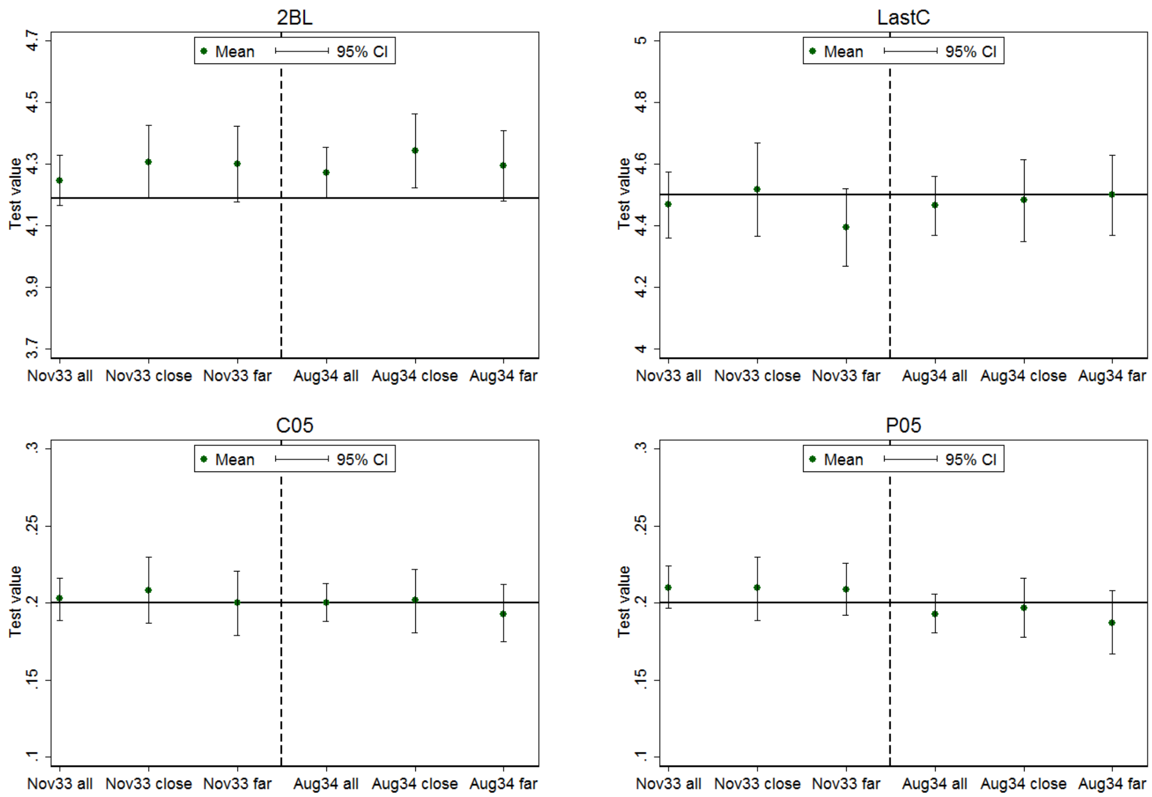

Figure A.3: Election Forensics

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			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	4.25	4.15	4.34	4.31	4.19	4.43	4.30	4.16	4.45
	Aug 34	4.19	4.27	4.17	4.37	4.34	4.19	4.49	4.29	4.15	4.44
LastC	Nov 33	4.5	4.47	4.39	4.55	4.52	4.37	4.66	4.40	4.27	4.52
	Aug 34	4.5	4.47	4.38	4.55	4.48	4.33	4.63	4.50	4.36	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	0.19	0.21	0.20	0.18	0.22	0.19	0.17	0.22
P05	Nov 33	0.2	0.21	0.20	0.23	0.21	0.19	0.23	0.21	0.19	0.23
	Aug 34	0.2	0.19	0.18	0.20	0.20	0.18	0.22	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.4 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.[6] 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

[6] 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.[7] 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.

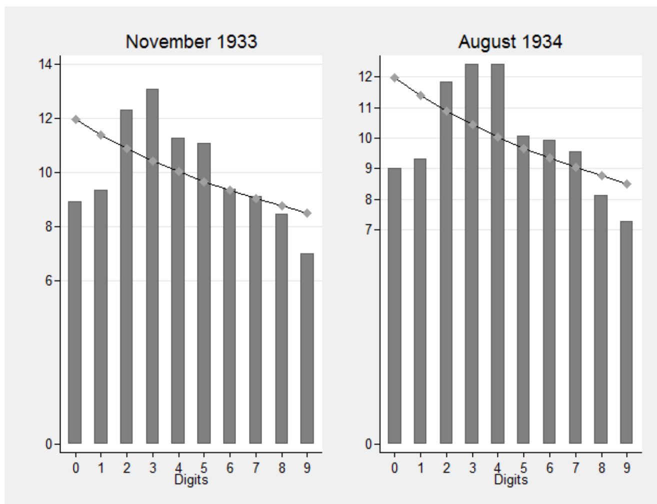

Figure A.4: Benford's Law, based on 2^{nd} digit distributions, Nov 1933 and Aug 1934

[7] 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)	(2)	(3)
	Full sample	Distance to highway (under construction): below median	above median
Panel A: Benford's Law			
November 33	91.3	50.5	47.6
p-value	*(0.000)*	*(0.000)*	*(0.000)*
August 34	86.0	49.5	47.9
p-value	*(0.000)*	*(0.000)*	*(0.000)*
Panel B: Last C			
November 33	6.4	5.6	6.9
p-value	*(0.61)*	*(0.69)*	*(0.55)*
August 34	0.9	1.9	1.3
p-value	*(0.99)*	*(0.98)*	*(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

	(1)	(2)	(3)
Dep. variable:	NSDAP vote share in March '33 (standardized)	Share of pro-Nazi votes in Nov'33 (standardized)	Share of pro-Nazi votes in Aug'34 (standardized)
log(distance HW)	0.0209	0.0180	-0.0591***
	(0.0157)	(0.0166)	(0.0121)
NSDAP votes March '33		0.251***	
		(0.0165)	
Pro-Nazi votes Nov'33			0.640***
			(0.0157)
Baseline controls	✓	✓	✓
Observations	3,230	3,218	3,234
Adjusted R^2	0.025	0.117	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	8.685347	8.439072	8.685298
Unemployment rate 1933	0.182052	0.137428	0.182045
Blue collar share 1933	0.363053	0.322727	0.363044
Share Industrial Empl. 1933	0.338796	0.276304	0.338785
Share Catholic 1925	0.284215	0.404694	0.284251
Share Jewish 1925	0.00407	0.005451	0.00407

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)	-0.0899*** (0.0146)	-0.0300** (0.0136)	-0.141*** (0.0279)	-0.0576* (0.0334)	-0.0881*** (0.0219)	-0.0435** (0.0181)
log(distance to any HW)#					-0.000415 (0.0226)	0.0102 (0.0144)
All controls		✓		✓		✓
District FE		✓		✓		✓
First Stage F-Statistic			664.6	616.9		
Instrument partial R^2			0.261	0.167		
Weak-IV robust p-value			[0.000]	[0.085]		
Observations	3,228	3,188	3,191	3,157	1,788	1,777
Adjusted R^2	0.010	0.564			0.009	0.570

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)	0.00840	-0.0113	-0.00437			
	(0.0106)	(0.00923)	(0.0123)			
log(distance to River)				-0.00718	-0.00593	0.00610
				(0.0115)	(0.00981)	(0.0119)
Controls:						
Baseline	✓	✓	✓	✓	✓	✓
District FE	✓	✓	✓	✓	✓	✓
Observations	3,306	3,306	1,985	3,306	3,306	1,985
Adjusted R^2	0.294	0.286	0.307	0.294	0.285	0.307

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 0.15 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 4-6 in Table 5 in the paper).

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	0.101*** (0.0310)	0.181*** (0.0335)	0.159*** (0.0377)	0.149*** (0.0373)	0.108** (0.0461)	
HW under construct. within 5km						0.130** (0.0530)
Matching variables:						
Baseline controls	✓	✓	✓	✓	✓	✓
Additional controls				✓	✓	✓
Matching restrictions:						
within districts		✓	✓	✓	✓	✓
within 20km of any HW#				✓	✓	
within 5km of any HW#						✓
Observations	3,234	3,234	3,234	3,216	3,216	3,216

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 in 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.[8] 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 a dummy that equals one for locations within 20km of a large city (more than 500,000 inhabitants). The results are reported in Table A.12 and visualized in Figure A.4.

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	.00144	-.0154	-.00134	.00157	.00753	.0230*	.0361**	.0552***	.0837***
StdErr	(.00947)	(.0129)	(.0132)	(.0133)	(.0137)	(.0138)	(.0151)	(.0171)	(.0225)

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 a dummy that equals one for locations within 20km of a large city (more than 500,000 inhabitants). The regression includes 2,256 observations, the R^2 is 0.253. 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.4 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.5 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

[8] 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 6 (and Figure A.3) 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.

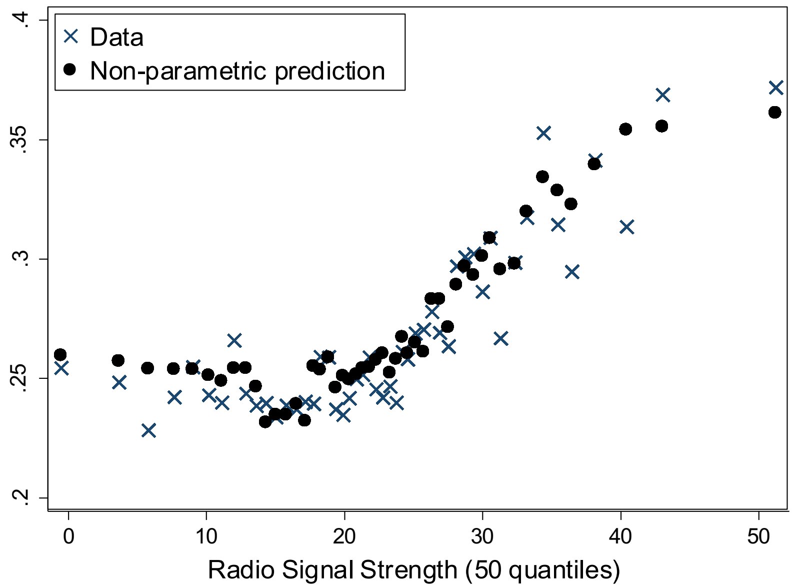

Figure A.5: Radio signal strength and radio subscribers (data and prediction)

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.