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The Mass Media of Remembering

The role of TV in coming to terms with the Nazi past*

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Abstract

By exploiting variation in transmission during the broadcasting of "Endlösung", a documentary on the Holocaust watched by approximately half of the German population in 1979, this paper studies the effects of mass media on voting behavior. At a time where many Germans still felt unaffected by their country's recent past, addressing collective and individual memory constituted a sensitive issue. Questions pertaining to the involvement of family members and the instrumental role of the masses in the Nazi regime remained unanswered. The broadcast provoked discussions of guilt and responsibility on various societal levels, triggering debates at school, at church, at work, at the dinner table, in the media and parliament. Following the transmission, the federal agency for political education (BPB) received over 450,000 requests for additional information material on the Nazi-regime. To prevent the broadcast, right-wing extremists targeted two emitters with improvised explosive devices. While one of the explosives only destroyed secondary cables and thus did not affect the transmission, the transmission of the other transmitter shut down for the whole broadcasting period. In consequence, several hundred thousands of viewers in proximity to the transmission tower did not have TV-access. This sequence of events makes it feasible to exploit variation in transmission while controlling for potential strategic motivations in the target choice. More specifically, it is possible to employ (i) a geographical regression discontinuity design, comparing municipalities around the geographical border; and (ii) a difference-in-differences Design comparing the electoral trends of municipalities with and without TV-access. While the results from the regression discontinuity are inconclusive and remain unidentified, the difference-in-differences estimates provide suggestive evidence that the voting trend of municipalities with TV-access has seen a 76 percent reduction in the trend for far-right votes in the 1980 federal elections compared to municipalities without TV-access. Though the relative size of the estimate is striking, this differential decrease translates into a mere 0.1-0.23 percentage points of the total vote.

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1 Introduction

While the effect of TV news on voting behavior has been studied extensively (e.g. DellaVigna and Kaplan, 2007; Enikolopov, Petrova and Zhuravskaya, 2011), other TV formats have received less attention. The bulk of TV airtime, however, is spent on various forms of entertainment including movies and documentaries. An exclusive focus on news might thus miss the importance of other content on viewers' attitudes and behavior. Evidence from an emerging literature suggests that non-informational entertainment indeed can have a large and lasting impact on political preferences (Durante, Pinotti and Tesei, 2018; Xiong, 2018). What role for informational entertainment? By exploiting variation in the transmission of "Endlösung", a Holocaust documentary watched by almost half of the German adult population in 1979, I study the effects of mass media on political behavior. Within this setting, I try to evaluate whether narrative information provided by the mass media (i.e. increased coverage of the Holocaust), changes voting behavior.

In expectation, untreated voters (i.e. those have not seen the TV-program) only vote on the basis of their prior, treated voters instead use the new information about the Holocaust to reevaluate the parties by exhibiting a heightened sensitivity towards the remnants of fascism within society, voting decidedly anti-fascist and thus holding accountable parties which court former Nazis and propagate anti-Semitism (first and foremost the National Democratic Party (NPD)).

The contribution of the work can be situated at the intersection of two questions. First, what is the role of mass-mediated infotainment on voting behavior? Second, do singular films have the capacity to shape collective memory? In doing so, the work might provide a perspective on how Germany evolved from a collective of former Nazis and bystanders into a mature democratic society that has virtually institutionalized a critical attitude towards its past - a past that shapes and constrains the political feasible up to the present.

The remainder of the paper is organized as follows. The first section provides a discussion of the historical context. The incident requires careful description as it helps to set the stage for assessing potential violations of the identification assumptions and clarifies the interpretation of the estimated quantities. Moreover, understanding critical determinants of far-right vote and anti-Semitic currents in the German party landscape crucially informs expected effects and the choice of covariates in the subsequent empirical analysis. After framing the origins of the incident, the paper briefly describes the data and the empirical strategy. Against this backdrop, it explores the hypothesis empirically by employing a geographic regression discontinuity and a difference-in-differences design. The paper concludes with a critical discussion of the results and points at future avenues for research.

2 Historical Context

2.1 The TV-program

In 1979, public German TV (ARD) decided to broadcast two films on the Holocaust: a documentary called "Endlösung" and an US-produced fictional melodrama called "Holocaust". At a time where a number of Germans still felt unaffected by their country's recent past, addressing collective and individual memory constituted a sensitive issue. Questions pertaining to the involvement of family members and the instrumental role of the bystanding masses in the Nazi regime remained unanswered (Lübbe, 2007). The record-breaking TV program was watched by around half of the German adult population and provoked discussions of guilt and responsibility on various societal levels; triggering debates at school, at church, at work, at the dinner table, in the media and parliament. Following the transmission, the federal agency for political education (BPB) received over 450,000 requests for additional information material on the Nazi-regime. (Märthesheimer and Frenzel, 1979)

On January 18, 1979, right-wing extremists targeted two emitters, located in two separate states - North Rhine-Westphalia (*Sender Notulln*) and Rhineland-Palatinate (*Sender Koblenz*) - with improvised explosive devices. While the explosives targeting the *Sender Notulln* only destroyed secondary cables and thus did not affect the transmission, the transmission of the *Sender Koblenz* shut down for the whole broadcasting period of 'Endlösung: Judenvernichtung', a Holocaust documentary. As a consequence, several hundreds of thousands of viewers in proximity to the transmission tower did not have TV-signal. Around 8:30 p.m. the cables at the foot of the 74m high transmission mast in Waldesch were destroyed by explosives (see Figure 4 in the Appendix for images of the transmitter). Consequently, the public television program, as well as the three public radio programs, were shut down as the transmitter in Waldesch and the subordinate downstream transmitters Bad Marienberg and Linz (as well as over 100 filling transmitters) failed. A bundle of energy cables leading from the broadcaster's headquarters in Baden-Baden to the transmission mast has been destroyed. The transmitter resumed broadcasting around 9:40 pm, although with reduced performance. Sound and image quality remained poor. Figure 1 depicts a map of the area that was affected by the transmission failure.

A similar - though unsuccessful - incident took place simultaneously at the transmitter *Sender Notulln* of the Westdeutscher Rundfunk near Münster. Soon, suspicion fell on a group of Neo-Nazis. Several right-wing papers published appeals to prevent the broadcast beforehand. In one medium, the wording read as follows: "Every decent German is called upon to prevent the broadcast of this film"[1] (cited in SWR, 2019).

[1] In German: "Jeder anständige Deutsche ist aufgefordert die Ausstrahlung dieses Films [Endlösung: Judenvernichtung] zu verhindern."

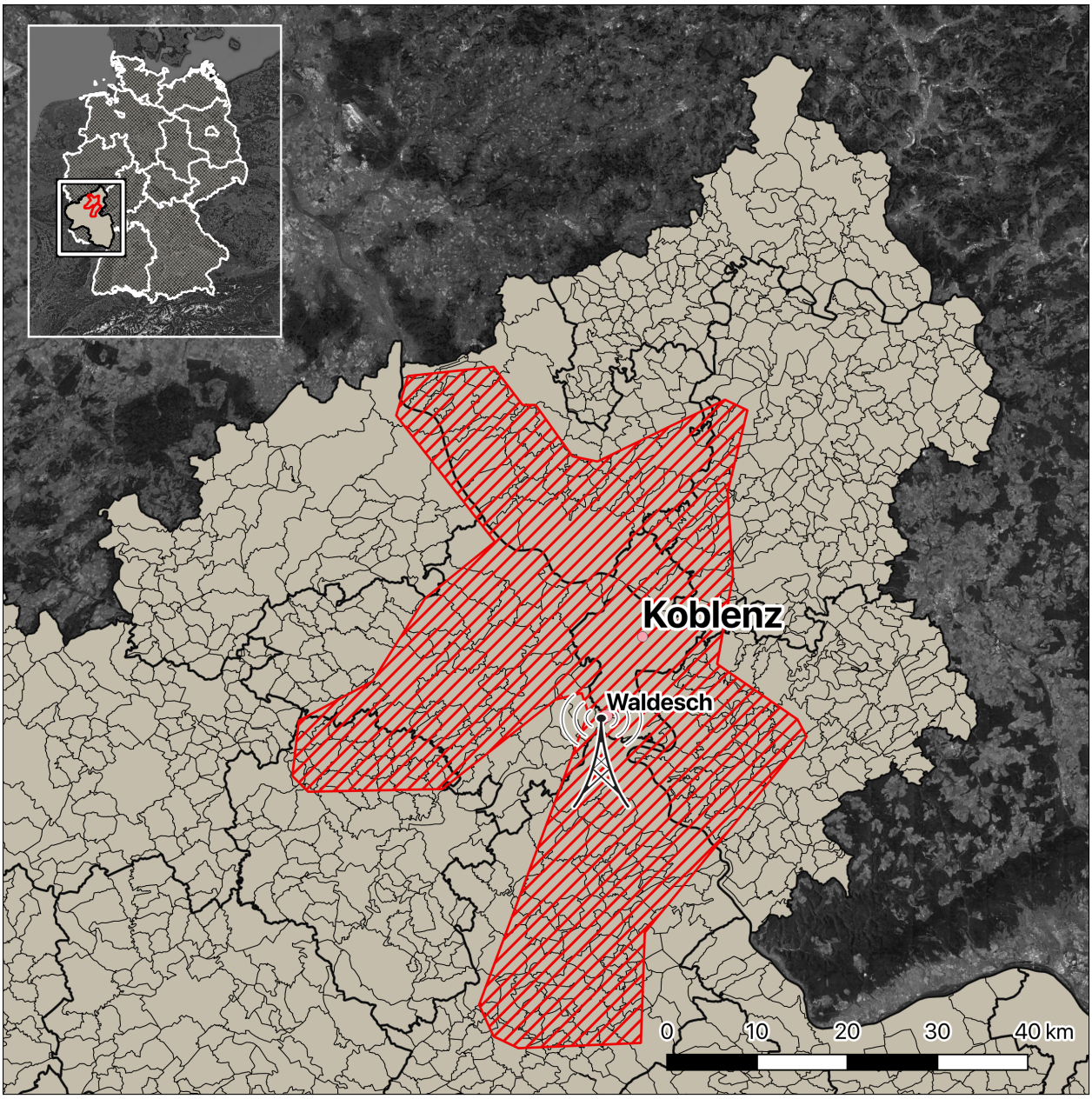

Figure 1: Map of TV-access

Note: The area affected by the attack is shaded in red, municipality boundaries are indicated in grey, bold black lines indicate district boundaries, and the outer contours reflect the state boundary of Rheinland-Pfalz. Contained within the map is an overview map of where the incident took place within Germany.

The initial suspicion was soon confirmed, the attack was traced back to a group of right-wing extremist. Eventually, a Nazi from Wiesbaden was sentenced to four years in prison.

Anecdotal evidence suggests that the entire TV-program (i.e. both the documentary and the film) had a profound impact on German society (Märthesheimer and Frenzel, 1979). Shortly after the broadcast, politicians used the images to draw historical parallels between the Holocaust and the situation of the

Indochinese refugees. Further, a survey conducted by the Marplan Offenbach Institute around the time of the broadcast suggests that the broadcast had a profound impact on attitudes towards their country's past. The share of individuals approving a "moral obligation of Germany to pay compensation and restitution" increased remarkably from 45 to 54 percent. Moreover, the statement that all adults during nazism "shared at least some guilt" was rated positively by more people subsequently (16 percent before, 22 percent afterward) (Märthesheimer and Frenzel, 1979). Lastly, while the majority of people rejected the proposal to abolish the statutory limitation on murder before the transmission, only a minority held that view after the transmission. Eventually, the proposal became law.

In this sense, this paper might also shed light on the question of how Germany evolved from a collective of former Nazis and bystanders into a mature democratic society that broadly maintains a critical attitude towards its past, by emphasizing the role of the masses in this process. Indeed, while the consensus view suggests that it is seminal historical and other scholarly work, gestures of statesmen, and grand speeches, which have contributed to the German *Vergangenheitsbewältigung* (coming to terms with the past), cultural products consumed by non-elite audiences have received less attention. These top-down approaches "ascribe nothing but a passive role to the audiences of such representational politics. The sentiments, opinions, and practices of "the masses" play no active role in these accounts" (Lüdtke, 1993).

Yet, there is something quite paradoxical in arguing that deep changes in social and political behavior (including voting behavior) come from above, since politicians themselves, along with their attitudes, are the products of changes in voting behavior and demands from the electorate. The circularity of this reasoning suggests there should be some factors exogenous to this political process which would account for changes in the social and political attitude of the public, and which would, in turn, explain political change. It thus seems plausible to believe that exposing half of the German adult population to the images of "Endlösung" might have played an instrumental role in shaping the views on the Nazi past.

Indeed, whereas the influence of scholarly work often remains circumscribed to an intellectual elite, TV, the former campfire of the nation, was the one medium capable of reaching large audiences. Moreover, while prominent episodes of the denazification process, such as the Nuremberg trials, have created opportunities for the masses to delegate the responsibility for what happened to a handful of atrocious Nazi leaders, the TV program focused on the role of the masses and the tragedies in their midst. Therefore, the show might have served as a "condensation symbol", generating a "larger interpretative framework that shapes subsequent [behavior] [...] and generates new externally provoked critical junctures, expanding the realm of political action and opening possibilities for institutional change" (Capoccia and Ziblatt, 2010, p. 948).

The documentary "Endlösung" consisted of three parts. The first part portrays the gradual eradication of Jews from public life from 1933 to 1938. The second part documents the process of economic deprivation,

covering the years from 1938 to 1941. In part three, the documentary culminates in graphic depictions of deportation, ghettos, and concentration camps - the final solution. Eye-witnesses give context to the images.

Does the aforementioned anecdotal evidence of the effect of the TV-program also stand the test of systematic examination? What were the effects on the demand for anti-fascist politics? In order to test this demand side, I intend to look at the outcomes of several elections. Fortunately, both states targeted by the attack held state elections shortly after (i.e. state elections took place in Rhineland-Palatinate March 1979 and in North Rhine-Westphalia May 1980). In addition, federal elections were held in October 1980.

I expect that individuals exposed to the program changed their evaluation of the past, as an increase in politically-relevant information might have provided individuals with a clearer picture of the crimes perpetrated under the Nazi regime (including those committed by the masses) and thus impacted the salience of anti-Semitism as an electoral issue. These increased levels of information and awareness might have altered electoral dynamics and conceivably led to higher levels of voter turnout. In short, if the TV-program had any effect, I would expect that it induced voters who have seen the TV-program to vote less fascist and thus holding parties accountable which court former Nazis and propagate anti-Semitism. This concerns first and foremost the National Democratic Party (NPD).

2.2 Anti-Semitism within the German post-war party landscape

Established to unify the fragmented far-right political landscape in 1964, anti-Semitism is one of the central tenets of the NPD's ideology. Anti-Semitism and aggressive agitation against Jews play a fundamental role in their agenda, classical anti-Semitism as propagated by the Nazi-regime has been present and explicitly articulated on every level of the party since its outset (Gensing, 2014). Constitutive for the party's agenda is a clear rejection of constitutional democracy (Stöss, 2010). The party's founding was helped by a marked economic downturn and profound structural weaknesses in manufacturing sectors (coal, iron, steel, and textiles). In the federal elections 1969, the NPD was able to mobilize a remarkable amount of votes (i.e. 1.4 million votes or 4.3 percent of the vote share), especially in municipalities which had a history with far-right voting. Indeed, the NPD performed well where the former Nazi party (NSDAP) has had its strongholds. Moreover, voters in rural medium-sized municipalities with a high proportion of Protestants were more susceptible to the party's allure, whereas practicing Catholics and trade union members hardly formed a reservoir for the right-wing extremists (see also Spenkuch and Tillmann (2018)). In addition to having little chance in Catholic-dominated areas, the NPD also had problems mobilizing voters in industrial areas which had a high union density. However, the party outperformed in districts with large numbers of workers employed under temporary contracts, which in itself was the classical clientele voting for the social democrats

(Liepelt, 1967).

Apart from the NPD, none of the other active parties at that time were outright anti-Semitic. Though every single major party had individual party members engaging in anti-Semitic behavior among their ranks, none has offered anti-Semitism a systematic platform. Historically, ties between Jews and social democrats have been strong and the SPD perceived Jews as a positive and integrative part of their political tradition (Ionescu and Salzborn, 2014). Although anti-Semitic statements by individual politicians have occurred, they were subsequently scrutinized and critically discussed within the party. Similarly, within the post-War conservative Christian-Democrats (CDU), anti-Semitism occasionally manifested itself through a perpetrator-victim reversal (e.g. chancellor Kohl's visit of a SS cemetery in 1985), and a relativization of German crimes perpetrated under the Nazi-regime. In contrast to these inclinations, the party was publicly viewed as the successor of the Christian resistance against the Nazi-regime and is still the dominant reading among party members (Ionescu and Salzborn, 2014). Lastly, the FDP occupies a special and conflicting place within this account. While formally committed to (a national variant of) Liberalism, the FDP has a contentious history with its at times far-right party base in general and anti-Semitism in particular. From attempts to recreate a neo-fascist platform in the 1950s to the formalization of a "National-Liberale Aktion" (an extreme right movement) (Leuschner, 2005), links between the FDP and the old guard of former Nazis were pervasive. According to Lösche and Walter (1996), the FDP became the most important refuge for the political rehabilitation of former Nazis in post-war Germany. While more explicit articulations were never constitutive of the party's agenda, latent anti-Semitism became ingrained in the party DNA. This latent yet deep anti-Semitism manifested itself in the context of the denazification and reparation debates in the 1980s, with the FDP rejecting to abolish the statutory limitation on murders committed during World War II (Nentwig and Walter, 2014). Notwithstanding these tendencies, the social-liberal wing of the party prevailed and dominated the party's trajectory during the 1970s. Thus, it is important to acknowledge that anti-Semitism, though a problem temporarily, was never part of the party's political self-projection, neither in- nor outwards.

3 Data

To explore the connection between watching the documentary and political behavior, I assembled data on elections, population, religion, education and (un)employment.

While electoral information on federal and state elections after 1976 are available electronically through the Statistical Office of Rhineland-Palatinate, electoral data before 1976 had to be extracted from archival sources of the Statistical Office. The original printed volumes have been digitized as scanned images. Using

these official print publications, Optical Character Recognition (OCR) was performed for each of the pages using ABBYY fine-reader software. The OCR process resulted in unformatted files full of duplicates and errors. Subsequently, these files were cleaned. Code converted the text to rows and columns. Unique entries were identified and duplicates discarded. This cleaning process was a combination of coding against problems that arose in patterns and manual proof checking.

In the same vein, I proceeded with the 1987 municipal census. The census provides information on population (i.e. total population and population by age category), education (share of students), and (un)employment (type of employment, and employment per sector) at the municipality level.

Furthermore, I digitized municipal data on religion (i.e. share of Catholics, and Protestants) from the 1970 census.

Geo-coded data in the form of geographic coordinates and shapefiles for the administrative units (i.e. municipalities, districts, and states) were sourced through the Federal Agency for Cartography and Geodesy [Bundesamt für Kartographie und Geodäsie]. I used the official "core" (a population-weighted measure of the municipality center) of these municipalities to calculate the distance to the border of the incident.

The major problem with the way I constructed the dataset is the possibility of false matches during the several merge processes. This might happen as the administrative numbers for the municipalities across the respective data sources and geo-located data are inconsistent. Further, an administrative restructuring reform in the 1970s led to widespread change. In the process of which many municipalities were either merged or separated. Two problems might occur. First, there might be false positives, i.e. an observation erroneously coded as treated (i.e. having TV-access) when in fact they were in the control group (i.e. having no TV-access). Second, if there are false negatives, i.e. to falsely code a municipality as being in the control group and thus as not having had TV-access even though the municipality, in fact, had TV-access. To the extent that false negatives are present, however, they will attenuate the estimated effect (bias toward zero). Though the error margin of the merge is relatively constrained as I use additional information to link records (i.e. the name of the municipality), there are multiple municipalities with the same or very similar names. To be confident that there are only few false positive matches, I checked them manually.

Lastly, based on an archival map from the broadcaster "Süd-West Rundfunk" and an official image of the coverage range issued by Siemens, the contracting company constructing the transmitter, I reconstructed the area covered by the transmitter Waldesch (see Figure 5 in the Appendix). Still, there are difficulties with identifying treatment and control groups properly due to uncertainty with respect to the area that was affected by the transmission failure. Again, two problems arise. First, secondary transmitters in Linz and Bad Marienberg, and over a hundred short-range radio repeaters also failed in consequence as they depended upon the provision of the signal from the transmitter in Waldesch. This leads to the risk of producing false

negatives, where municipalities which couldn't possibly receive TV signal are coded as if they were able to. False negatives will bias the estimates towards zero. Second, some regions plausibly had coverage overlaps with other transmitters. To define treatment exactly, it would be necessary to take into account the range of other transmitters nearby. Regarding the relative risk of false positives and false negatives, false negatives are more likely as what I define as no TV-access already is a conservative estimate of the coverage range of the dysfunctional transmitter. The two secondary transmitters are located either at the very border of the coverage range (i.e. Linz) or even outside of the coverage range. Analogous reasoning applies to the short-range radio repeaters. Nonetheless, this has also straightforward implications for the bandwidth choice for the geographic regression discontinuity and the plausibility of the identifying assumption, as the border - even though de facto sharp - is hard to reconstruct precisely ex-post.

4 Empirical Strategy

Because of potential reverse causality issues, it is important to use an exogenous determinant of television access to isolate the causal effect of the documentary. A naive estimate comparing individuals who watched the program with those who didn't (or those with access to those without) would suffer from an endogeneity problem (e.g. politically informed and historically interested individuals were more likely to watch the program). To circumvent this issue, I employ a geographical regression discontinuity design (RD) and a difference-in-differences design (Diff-in-Diff). Exploiting the fact that the exact areas experiencing the broadcasting failure were quasi-random gives rise to two types of discontinuities that can be used for the identification of causal effects. First, I apply a geographic regression discontinuity design based on municipalities in a close neighborhood on both sides of the treatment border in an attempt to estimate the local average treatment effect (LATE). Second, I use a difference-in-differences design comparing the electoral trends of municipalities with TV-access to those without TV-access trying to recover the average treatment effect on the treated (ATT). In the following subsections, I will outline the respective identification assumptions for both approaches and discuss potential violations.

4.1 Geographic Regression Discontinuity Design

In the RD approach, identification of the TV-show's effect comes from the change in accessibility of TV at the border of the area affected by transmission failure. In its most basic formulation, I attempt to estimate the following equation:

$$y_i = \alpha + \beta f(X_i) + \rho TV_i + \eta_i \tag{1}$$

where y_i is the vote share of the respective parties in either the state- or federal elections in municipality i. The function $f(X_i)$ is a local linear approximation using distance to the border as the forcing variable. The variable TV_i is an indicator function for whether municipality i is located within the area which was not affected by the transmission failure and thus serves as a treatment unit (i.e. individuals within these municipalities have a positive and high probability of having seen the program[2]). The coefficient ρ captures the effect of TV-accessibility on voting behavior. Finally, η_i is an error term.

For the equation to capture the causal effect, the probability of TV reception must be discontinuous at the cutoff c. Each municipality has a distance score X_i in km, indicating how far the center of the municipality is located from the closest point of the border. Units with $X_i \geq c$ are assigned to treatment (TV access) and units with $X_i < c$ are assigned to the control group (no TV access). This assignment, denoted TV_i, is defined as $TV_i = \mathbb{1}(X_i \geq c)$, where $\mathbb{1}(\cdot)$ is the indicator function. Naturally, c is normalized to 0.

Being assigned to the treatment condition, however, is not the same as receiving or complying with the treatment of interest (i.e. having watched TV). The indicator function simply specifies that households within a given municipality were theoretically able to receive the TV signal. Being assigned to receiving the signal, thus, is not the same as having seen the TV-program. This potential non-compliance introduces complications and fundamentally changes the interpretation of the estimates. Essentially, the estimated quantity is the intent-to-treat, not the effect of watching the TV program per se. To be precise, the estimate is measuring the effect of not having watched the program compared to the counterfactual of likely having watched it. Note that the estimates as such are actually exploiting quasi-random non-treatment. Actually having watched TV in areas with TV-access essentially is still non-random.

Crucial for identification, the continuity assumption states that there should be no discontinuity in potential outcomes at the cutoff. If other relevant factors vary discontinuously at the border, a discontinuity in voting behavior cannot be interpreted as the causal effect. That is, no alternative interpretation should also exhibit a discontinuity at the cutoff. Otherwise, the interpretation of the estimate remains ambiguous. Formally, this requirement can be captured as the expected outcome $\mathbb{E}[y_i(TV)|X_i = x]$ being continuous around the threshold $X_i = c$ for $TV_i = 0$ and $TV_i = 1$.

As the treatment border has no natural relevance and does not correspond to administrative borders, many concerns of other discontinuities that are important at, for example, administrative borders can be ruled out. In many cases, geographic regression discontinuity designs are sub-optimal with respect to potential compound treatments and sorting issues. Administrative borders are often a product of political considerations and historical contingencies. As such, the treatment of interest is rarely the only thing that changes when crossing the border. Further, given the historical character of these borders, individuals are

[2]By virtue of the popularity of the TV-program.

likely to sort accordingly. In our particular case, however, the treatment border is unknown to most individuals, including administrators. This renders sorting concerns as less of a problem and makes compound treatment effects unlikely.

Even though the border does not separate different institutions, there still might be discontinuities in other relevant covariates. Several issues arise which could confound the estimation. Transmitters might have been targeted strategically by right-wing terrorists. If the attackers had targeted the transmitters in politically strategic areas, then the conditional independence assumption might be violated and estimates might be biased. Conceivably, attackers targeted areas where they possessed the necessary infrastructure to execute the attacks. These areas, in turn, plausibly exhibit higher densities of right-wing cells and well-connected networks with a high appetite for action. As a result, the estimates run the risk of producing false positives as the targeted population might have been more right-wing anyway. In an alternative story, terrorists might have targeted the areas precisely because they're liberal and left-leaning. Consequently, we might wrongly conclude that the TV-program induced people to vote for right-wing parties.

The problem is aggravated by the way I employ the distance measure. Note that the RD is estimated by naively restricting the sample to observations within a certain distance range (regardless of their exact geographic location at the border). This is sub-optimal as this naive distance measure leads to units being compared which are not necessarily geographically close and thus not necessarily comparable. Ideally, we would compare geographically close units on both sides of the border.

The continuity assumption can be analyzed by looking at the baseline characteristics of places to the left and right of the geographical threshold. Crucially, the identification strategy for estimating the effect on viewers' behavior relies on the premise that viewers in locations with and without TV-access are similar on all unobserved characteristics that may drive their voting behavior once observable differences between these locations are controlled for.

A further problem concerns compliance: viewers might have circumvented the transmission failure by watching it on their neighbor's TV device. Similarly, spill-over effects might occur when individuals who watched the transmission discussed the program with individuals from the control group. If people circumvented the transmission failure by viewing it somewhere else or subsequently talking to somebody from the treatment group, this should introduce a downward bias to the estimates as the estimation treats people with access as if they had no access. Given the geographical proximity, non-compliance and spill-over were likely. Further, as the border is estimated imprecisely there is reason to doubt that the municipalities at the border were, in fact, less likely to be affected as it was also easier to capture TV from other towers when located closer to the border.

Additionally, using coarse, aggregated units, when researching individual phenomena might introduce

bias. To some extent, the municipal units are relatively arbitrary with respect to the precise spatial variation of the vote. In the end, individuals vote, not municipalities. Thus, aggregate measures might not accurately reflect individual-level phenomena unless those phenomena are spatially constant concerning the areal unit (Keele, 2015). In consequence, non-parametric RD techniques - exclusively relying on the variation at the discontinuity are not convincing as they would require precise geo-referencing (Dell, 2010). Neither precision nor size of the sample render non-parametric estimation techniques unambiguously credible. Thus, following Hahn, Todd, and Van Der Klaauw (2001), I use a local linear approximation in which I discard the units with X_i larger than some bandwidth h away from the border (i.e. 10, 20, and 30km) and subsequently apply different polynomials.

Finally, a note related to the interpretation of the estimates. The RD design provides estimates of the local average effect of having TV-access along the border. The analysis fundamentally relies on extrapolation towards the cutoff point. The effect is only identified for a small sub-population. This local average estimate could be particularly uninteresting if the sub-population is unrepresentative for the whole population. Without making further assumptions, we cannot say much about the external validity, i.e. the effect of the whole population.

4.2 Difference-in-Differences

The difference-in-differences design compares the change of vote shares within municipalities which had no TV-access to the change of vote shares among municipalities that potentially could watch the program. In its basic formulation, the difference-in-differences approach attempts to estimate the following model:

$$y_i = \alpha + \gamma D_i + \lambda Post + \sigma(Post * D_i) + \varepsilon_i \tag{2}$$

where y_i is the vote share in the election held at time t in municipality i. The variable D_i is a simple indicator for whether municipality i is located within the area which was not affected by the transmission failure and thus serves as a treatment unit (as households located within these municipalities had a high likelihood of having seen the program). The coefficient γ contains differential observed and unobserved fixed variables between treatment and control municipalities. This fixed effect prevents the estimates from being biased upward by the fact that treated municipalities generally experienced a different voting behavior in a given point in time compared to control municipalities. Finally, $Post$ is a dummy variable indicating whether the election was held after the incident. Thus, λ captures the time trend, i.e. it controls for common factors that change over time. The coefficient σ is the diff-in-diff estimate. Lastly, ε_i is an idiosyncratic error term.

For this design to provide the effect of having TV-access, the change in the municipalities without TV-

access must provide an accurate picture, on average, of how the districts with TV-access would have changed, had they not had the chance to watch the program. Formally, this parallel trend assumption is displayed in equation (4).

$$\mathbb{E}[y_i|D_i = 0] = \gamma + \lambda \tag{3}$$

This assumption may not hold, however, if voting behavior in treated municipalities has a long-term trend that differs from the trend in control municipalities. Control areas tended to be relatively urban (see next section), potentially putting downward pressure on far-right voting and resulting in a downward trend relative to control municipalities. However, these more densely populated areas may also be more receptive to a spread of political ideas that could result in a downward or upward trend relative to control municipalities. In either case, these differential trends will introduce bias to the estimate. The parallel trends assumption cannot be validated yet is defensible. In the results section, I scrutinize the estimates by subjecting them to several tests for their validity.

There are substantive reasons why the differences-in-differences design might produce more reliable estimates than the RD design. First, the uncertainty regarding the comparability of neighbors on either side of the boundary face becomes irrelevant in terms of levels. Identification, in this case, does not necessitate balancedness across control and treatment groups. For example, we can ignore the fact that Koblenz, the largest and most liberal-leaning city in the sample, was affected by the transmission failure while municipalities outside the boundary tended to be more rural. What matters are flows (i.e. trends), not levels. In this sense, the data requirements are less demanding for the diff-in-diff approach. Second, the diff-in-diff design accounts for the fact that the area suffering from the transmission failure is actually not completely random. The terrorists targeted the area for some reason. Hence, a difference in means is most likely biased. A difference in trends, however, is probably not. Third, trends might be the more interesting quantity to estimate as we're comparing the voting behavior of the same individuals over time (assuming no compositional changes). Further, complications arising in the RD design through the naive calculation of the distance to the border do not arise in the difference-in-differences approach.

On an important note, the probability of having watched the program among those who had TV-access is high, but selection into de facto watching TV still renders this sub-population non-random. This is an impediment when trying to recover the average treatment effect on the treated for watching the TV-show. The TV program might have had no or little effect on never-takers, i.e. individuals who wouldn't have watched program anyway.

Lastly, I cannot rule out the possibility that any observed effect not due the having watched the docu-

mentary bur rather a reaction of voters of simply being the victim of right-wing terrorism.

5 Results

5.1 Geographic Regression Discontinuity

Table 1 lists the summary statistics for treatment and control municipalities. The first set of columns restricts the sample to fall within the boundary; the second, the third, and fourth sets of columns restrict the sample to fall within 10, 20, and 30 km outside the boundary, respectively. Thus, the first set of columns displays the characteristics of the control group, i.e. municipalities which were affected by the transmission failure. The following sets of columns are concerned with the covariate characteristics of the treatment group. Each set contains three columns displaying mean, standard deviation, and the number of observations. The rows show information on total population size in 1979, the percentage of individuals above age 65 in 1979, the percentage of students as a share of the total population in 1987, the percentage of Catholics as a share of the total population in 1970, the percentage of unemployment rate in 1987, the percentage of civil servants as a share of the total labor force in 1987, the percentage of self-employed in 1987, the percentage of foreign workers in 1987, the share of the total labor force employed in the agricultural sector in 1987, and the share employed in manufacturing in 1987.

	Control			**Treatment**			**Treatment**			**Treatment**		
	Within Boundary			10km Outside Boundary			20km Outside Boundary			30km Outside Boundary		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N
Population	2,361	8,711	248	780.6	1,685	378	756.1	1,417	751	929.7	2,460	980
Population above 65 %	16.00	3.379	248	17.10	3.768	378	16.76	3.607	751	16.53	3.537	980
Students %	13.65	2.984	248	12.87	3.538	378	12.95	3.623	750	12.97	3.441	979
Catholics %	62.95	35.05	248	57.99	38.86	377	54.61	39.39	748	54.54	39.53	975
Unemployed %	5.402	2.512	248	5.299	2.869	378	5.350	2.739	750	5.472	3.712	979
Civil-Servants %	45.06	13.99	248	40.24	9.277	378	39.34	9.768	750	39.01	9.550	979
Self-Employed %	12.76	5.708	248	12.91	6.173	378	13.04	7.033	750	13.27	7.128	979
Foreign Workers %	1.184	2.888	248	0.618	2.164	378	0.525	1.836	750	0.550	1.821	979
Employed in Agriculture %	7.078	6.993	248	7.739	7.616	378	8.156	8.274	750	8.342	8.335	979
Employed in Manufacturing %	41.96	13.84	248	43.44	10.81	378	43.48	10.61	750	43.85	10.43	979

Table 1: Summary Statistics Covariates

Whiles some covariates appear to be statistically identical across the boundary, there are statistically significant differences in population size, the share of old people, the share of Catholics, the share of civil servants, the number of foreign workers, and the number of students between treatment and (all variants of) control municipalities. The municipalities which were affected by the transmission failure were on average more populated, younger, more Catholic, more likely to be employed by the state, and more likely to

employ foreigners; characteristics which are negatively correlated with far-right voting. These differences are certainly an artifact of Koblenz - the largest city in the sample - being part of the control group. This has straightforward implications for identification. First, without conditioning on these observed differences, the results will be biased and the estimated effect cannot unambiguously be attributed to the fact of having had TV-access. Given these observed differences, it is unlikely that I control for the whole set of unobserved characteristics that differ across the boundary. The RD results should be taken with a degree of skepticism, as the continuity assumption might is violated. Though meaningless, I report them for the sake of completeness nonetheless.

In Table 2, I report the estimates of the geographic RD for the federal elections in 1980. The table is structured as follows. The first set of columns displays the results using turnout as the dependent variable. Within this set, I restrict the sample to fall within 10, 20, and 30km distance to the boundary. I proceed correspondingly with the party vote shares. In the following four sets of columns, I report the vote shares for the Christian-Democrats (CDU), the Social-Democrats (SPD), the Liberals (FDP), and the National Democratic Party (NPD). Following Calonico, Cattaneo, Farrell, and Titiunik (2017), I report conventional, bias-corrected and robust standard errors in the first three rows of the table. Compared to conventionally estimated confidence intervals, bias-corrected confidence intervals perform better for large bandwidth choices yet have worse finite-sample properties (i.e. higher variability). To counteract this side-effect, the robust estimate rescales the bias-corrected t-statistic. In the last row, I indicate the number of observations. The same structure applies to Table 3, in which I report the estimates for the state elections in 1979.

One pattern across the tables is strikingly consistent: when estimated with robust standard errors, not a single result is statistically significant, neither in the state- nor in the federal elections.

	Turnout			CDU			SPD			FDP			NPD		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km
Conventional	0.561	0.226	0.253	2.140	4.593*	4.674*	0.041	-1.653	-1.763	-1.876	-2.752**	-2.770**	0.004	0.000	-0.010
	(0.573)	(0.444)	(0.404)	(3.384)	(2.634)	(2.399)	(2.512)	(1.968)	(1.797)	(1.445)	(1.163)	(1.076)	(0.034)	(0.027)	(0.025)
Bias-corrected	1.155**	0.774*	0.681*	2.309	2.868	5.670**	-0.443	-0.790	-2.932	-1.352	-1.689	-2.395**	-0.034	0.001	0.001
	(0.573)	(0.444)	(0.404)	(3.384)	(2.634)	(2.399)	(2.512)	(1.968)	(1.797)	(1.445)	(1.163)	(1.076)	(0.034)	(0.027)	(0.025)
Robust	1.155	0.774	0.681	2.309	2.868	5.670	-0.443	-0.790	-2.932	-1.352	-1.689	-2.395	-0.034	0.001	0.001
	(0.845)	(0.645)	(0.578)	(4.965)	(3.862)	(3.491)	(3.691)	(2.848)	(2.569)	(2.021)	(1.695)	(1.578)	(0.052)	(0.039)	(0.036)
Observations	626	995	1223	626	995	1223	626	995	1223	626	995	1223	626	995	1223

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2: Geographic Regression Discontinuity: Federal Elections 1980

	Turnout			CDU			SPD			FDP			NPD		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km
Conventional	-0.051	-0.267	0.202	2.641	5.200*	5.173**	-0.957	-2.649	-2.545	-1.580	-2.493**	-2.619**	-0.018	-0.013	-0.023
	(1.011)	(0.763)	(0.691)	(3.438)	(2.694)	(2.460)	(2.721)	(2.141)	(1.956)	(1.367)	(1.108)	(1.027)	(0.089)	(0.070)	(0.064)
Bias-corrected	0.925	0.315	0.312	2.467	3.239	6.028**	-1.564	-1.698	-4.010**	-0.773	-1.329	-1.868*	0.005	-0.033	-0.008
	(1.011)	(0.763)	(0.691)	(3.438)	(2.694)	(2.460)	(2.721)	(2.141)	(1.956)	(1.367)	(1.108)	(1.027)	(0.089)	(0.070)	(0.064)
Robust	0.925	0.315	0.312	2.467	3.239	6.028*	-1.564	-1.698	-4.010	-0.773	-1.329	-1.868	0.005	-0.033	-0.008
	(1.567)	(1.144)	(1.011)	(5.032)	(3.940)	(3.576)	(4.002)	(3.113)	(2.821)	(1.878)	(1.605)	(1.501)	(0.128)	(0.100)	(0.091)
Observations	626	995	1223	626	995	1223	626	995	1223	626	995	1223	626	995	1223

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Geographic Regression Discontinuity: State Elections 1979

Looking at the conventional and bias-corrected estimates in the first and second row, the results concerning the vote share of the FDP and the CDU gain statistical significance in some specifications. According to the conventional estimates of both the state and the federal election, the FDP's vote has been 1.5 to 2.7 percentage points smaller in the area that did not experience the transmission failure.

While higher-order polynomials (i.e. of order 4 or 5) indicate the existence of a discontinuity of the FDP vote share at the threshold, there is reason to take these results with a grain of salt. First, there is no convincing theoretical reason to believe that voting behavior to the right and the left of the cut-off follows a fourth-order polynomial. Second, when fitted to a polynomial of order 3, the alleged discontinuity disappears (see Figure 2). Third, imposing a linear polynomial on the data results in opposite signs on both sides. Why should a single determinant cause an increase in far-right voting on the one side but induce a reduction in far-right voting on the other side? Repeating this exercise with the results of the CDU's vote share warrants the same conclusion. The discontinuity disappears upon fitting different polynomials.

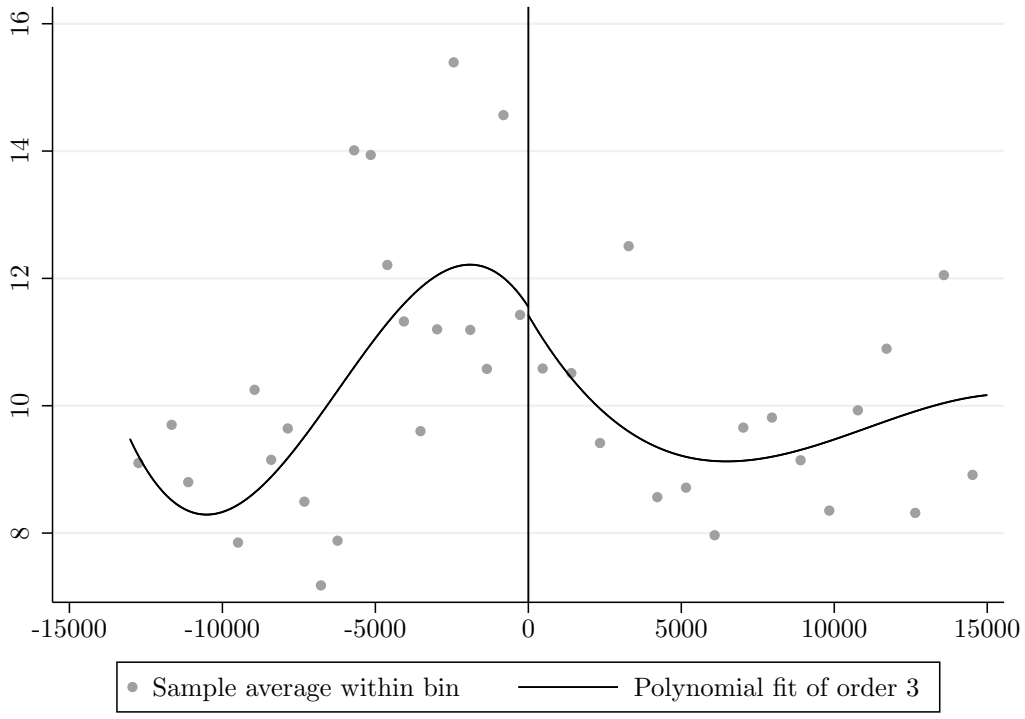

Figure 2: Mean FDP vote share in bins plotted by distance to the boundary (negative values indicate municipalities affected by the transmission failure)

Further, I redo the RD exercise with earlier electoral outcomes which are expected to be unaffected by the transmission failure. The results exhibit similar patterns as the years in question, suggesting that differences detected in some specifications can be attributed to fixed characteristics between the two areas that have

not been controlled for.

Given the evidence in favor of a violation of the continuity assumption and the problems described in section 4.1, causal claims are not warranted on the basis of the regression discontinuity design.

5.2 Difference-in-Differences

Table 4 reports the estimates of the difference-in-differences design for the federal elections in 1980. The table is structured as follows. The first set of columns displays the results using turnout as the dependent variable. Within this set, I restrict the sample to fall within 10, 20, and 30km distance to the boundary. I proceed analogously with the party vote shares. In the following four sets of columns, I report the vote shares for the Christian-Democrats (CDU), the Social-Democrats (SPD), the liberals (FDP), and the National Democratic Party (NPD). The first row shows the difference-in-difference estimate which is the coefficient for the interaction term consisting of the treatment dummy D_i and the time dummy $Post$. The second row displays the total number of observations spanning both election periods. The third column shows the number of treated observations per period. The fourth row specifies the same quantity for control observations. Lastly, the fifth row indicates the R-squared. The same structure applies to Table 5, in which I report the estimates for the state elections in 1979.[3]

One result stands out in Table 4: the trend of the NPD vote share in the federal election 1980 decreased in the municipalities which were not affected by the transmission failure compared to the control area that had no TV-access. The far-right party's vote share in the federal election experienced a statistically significant downward trend compared to municipalities which had no TV-access. Though the difference in absolute size is small (0.093 percent of the total vote), the change in the relative size is substantial (i.e. the slope is 76 percent steeper compared to the counterfactual's trend slope). This result, however, does not extend to the state elections in 1979 (see Table 5). While this does not increase the confidence in the results for the elections in 1980, it might be rationalizable as an artifact of differential voting behavior for marginalized parties in federal- versus state elections. However, I expected the effect to be the strongest in the closest election; not least because the campaign literature suggests that events closer to the election should be more influential as persuasion effects tend to fade (Gerber et al., 2011). To reiterate, anti-Semitism is one of the central tenets of the NPD's ideology. Anti-Semitism plays a fundamental role in informing the NPD's world-view and aggressive agitation against Jews is quasi habitualized. Classical anti-Semitism as propagated by the Nazi-regime is present and explicitly articulated on every level of the party (Gensing, 2014).

[3]Conventional differences-in-differences standard errors might underestimate the standard deviation (Bertrand, Duflo, Mullainathan, 2004). This can happen because conventional standard errors are agnostic with respect to serial correlation. To account for potential serial correlation and heteroskedasticity, I cluster the standard errors by municipality. Reassuringly, the results are displayed with clustered standard errors and are robust to bootstrapping.

Although it is a natural explanation to attribute the observed effect to the documentary, it might also be a reaction of voters to the fact of being deprived of TV or simply the fact of being the victim of right-wing terrorism.

	Turnout			CDU			SPD			FDP			NPD		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km
Diff-in-Diff	-0.116	-0.148	-0.066	0.076	0.220	0.275	0.560	0.405	0.589	-0.609	-0.655	-0.876	-0.093**	-0.089***	-0.093***
	(0.375)	(0.330)	(0.318)	(2.293)	(2.007)	(1.910)	(1.778)	(1.562)	(1.485)	(0.941)	(0.843)	(0.818)	(0.042)	(0.034)	(0.032)
Observations	1,252	1,998	2,456	1,252	1,998	2,456	1,252	1,998	2,456	1,252	1,998	2,456	1,252	1,998	2,456
$N^{Treated}$	378	751	980	378	751	980	378	751	980	378	751	980	378	751	980
$N^{Control}$	248	248	248	248	248	248	248	248	248	248	248	248	248	248	248
R-squared	0.057	0.051	0.047	0.020	0.012	0.012	0.009	0.003	0.003	0.024	0.022	0.021	0.053	0.048	0.050

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Difference-in-Differences: Federal Elections 1980

	Turnout			CDU			SPD			FDP			NPD		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km	<10km	<20km	<30km
Diff-in-Diff	-0.573	-0.356	-0.350	0.192	0.416	0.344	-0.229	-0.544	-0.524	-0.076	-0.070	0.005	0.015	-0.013	-0.053
	(0.700)	(0.639)	(0.620)	(2.346)	(2.067)	(1.974)	(1.865)	(1.644)	(1.564)	(0.888)	(0.810)	(0.791)	(0.085)	(0.076)	(0.075)
Observations	1,252	1,998	2,456	1,252	1,998	2,456	1,252	1,998	2,456	1,252	1,998	2,456	1,252	1,998	2,456
$N^{Treated}$	378	751	980	378	751	980	378	751	980	378	751	980	378	751	980
$N^{Control}$	248	248	248	248	248	248	248	248	248	248	248	248	248	248	248
R-squared	0.009	0.010	0.012	0.018	0.009	0.009	0.020	0.011	0.011	0.009	0.007	0.008	0.037	0.034	0.038
Standard errors in parentheses															
*** p<0.01, ** p<0.05, * p<0.1															

Table 5: Difference-in-Differences: State Elections 1979

In addition to the effect on far-right voting, I also explore the effect that exposure to the transmission failure has on overall turnout and the electoral performance of the other parliamentary parties that contested in the 1979 state- and 1980 federal elections. Employing the same difference-in-differences analysis for turnout and the vote share for each of the other parliamentary parties systematically returns null-results (columns (1) to (11)). Besides the NPD's performance, neither any other party's vote share nor turnout seems to be affected by the transmission failure. The absence of a relationship between virtually all other parties and TV-access lends credence to the reliability of the estimated effect for the NPD.

The distance from the border plays a key role in the identification strategy as it predicts which municipalities were exposed to the transmission failure and which ones were not. Implicitly, this logic assumes that being within compared to being outside the boundary is unrelated to other potential determinants of the trend in the NPD vote share between the pre- and post-treatment elections. To examine whether the estimated effect is a result of the arbitrary distance choice with which I define treatment, I focused on municipalities within different distance ranges (i.e. 10, 20, and 30km). Throughout the range, the estimates remain remarkably robust. This can be interpreted as weakening the possibility that the estimated effects are due to some distance-related time-varying confounder.

5.2.1 Parallel Trends

To the extent that there exists a time-varying correlation between the targeted area and other contemporaneous predictors of far-right voting behavior, it might be detectable through the visual inspection of electoral dynamics in previous elections. In Figure 3, I plot pre- and post-trends of the NPD vote share to validate the parallel trends assumption. Visually, the assumption holds. On average, municipalities in the treatment group experienced the same trend as municipalities in the control group before and after the election in 1980. As can be seen in Figure 3, municipalities in the targeted area tended to vote less for the NPD than in their immediate surroundings outside the boundary. This negative correlation is absorbed by the difference-in-differences approach and only would constitute a problem for identification when this correlation varies over time. The visual inspection of pre-treatment trends, a null result placebo, permutation-based inference tests, and additional hypothesis tests, however, suggests that this is not the case (see the following subsections). Further, the return to parallel trends lends confidence to the results, as it suggests that the transmission failure affected vote choice in the following election, but was not associated with other fundamental changes that would render the trend of affected and unaffected municipalities incomparable.

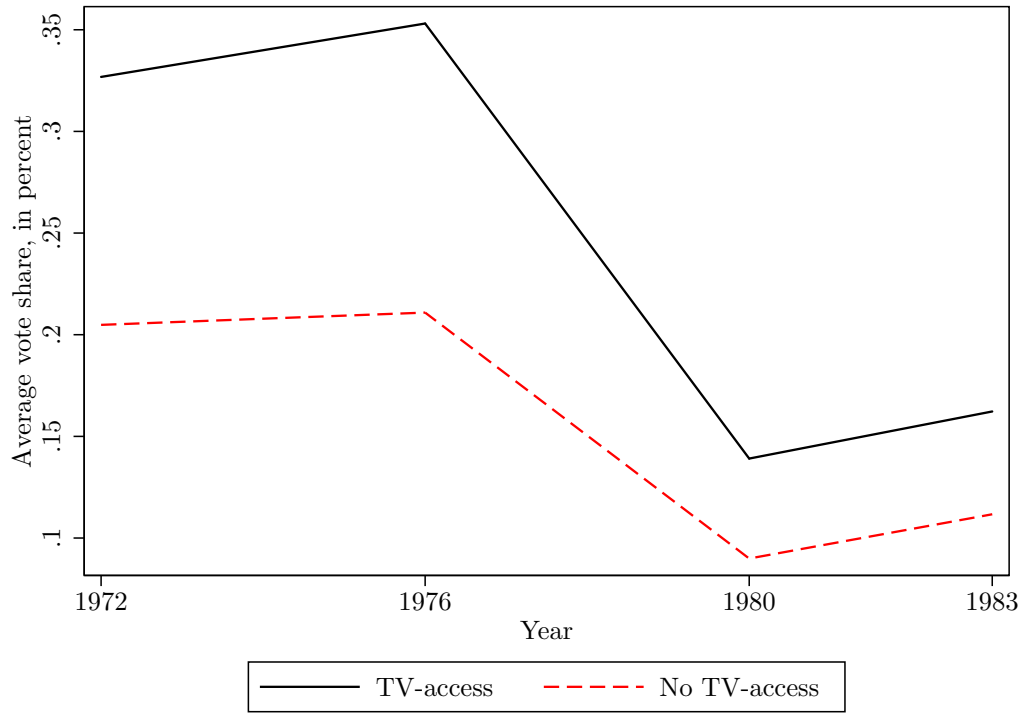

Figure 3: Mean NPD vote share in federal elections in areas with and without TV-access by years

5.2.2 Placebo

To further scrutinize the result, I use the lead (i.e. the result in the previous election) as a placebo estimator. Table 6 reports the results. The null-hypothesis that there are no systematic differences in the trends of the voting behavior between the treatment and control areas cannot be rejected (see the third row for the diff-in-diff estimate). The coefficients are close to zero and statistically insignificant. Taken together, this test further strengthens the plausibility of the parallel trends assumption yet cannot rule out the possibility of a false positive. This null placebo result is equally likely to emerge whether the estimated effect constitutes a genuine phenomenon or occurred by pure chance.

5.2.3 Boundary Variation Test

Though I cannot discriminate between the hypotheses that the result constitutes a real phenomenon or a mere false positive directly, I proceed by specifying an additional test that should hold if the estimated effect is real. In the estimates below (Table 8), I alter the treatment area in order to create different counterfactual trend, determining treatment and control more conservatively. I do this for two reasons. First, for a household located within the boundary, the closer it was to the border of the transmission failure area, the more likely it was that a household managed to see the program (either through access to neighbors

	NPD		
	(1)	(2)	(3)
	<10km	<20km	<30km
Time	0.006	0.006	0.006
	(0.035)	(0.035)	(0.035)
Treatment	0.110**	0.092**	0.122***
	(0.046)	(0.036)	(0.035)
Diff-in-Diff	-0.006	0.022	0.020
	(0.059)	(0.048)	(0.045)
Constant	0.205***	0.205***	0.205***
	(0.029)	(0.029)	(0.029)
Observations	1,249	1,992	2,450
R-squared	0.009	0.007	0.008
Robust standard errors in parentheses			
*** p<0.01, ** p<0.05, * p<0.1			

Table 6: Placebo Difference-in-Differences: Federal Elections 1976

or other transmitters). Second, the closer a household was located to the transmitter that failed, the more likely it was that the household had no TV-access. By eliminating the municipalities close to the border of the area affected by the transmission failure (on both sides), one would expect to find a stronger effect as municipalities were more likely to be compliant. The results of this test are summarized in Table 8. All columns use the NPD's vote share as the dependent variable. The first set of columns ((1) to (3)) discards all observations located within a range of 1km to either side of the boundary. Within this set, I restrict the sample to 10, 20, and 30km distance to the boundary. In the following three sets of columns, I proceed in the same way by eliminating observations within progressively larger buffer zones (i.e. 2.5, 5, and 7.5km). The first two rows contain the estimates for the time and treatment dummy, respectively. The third row shows the difference-in-difference estimate. The fourth row displays the constant. The fifth specifies the total number of observations. Lastly, the sixth row indicates the R-squared. As expected, the estimates become larger as the buffer zone within which observations are discarded becomes bigger. The estimates range from -0.082 to -0.230. The only caveat to this test is that the signal did not propagate in concentric circles from the transmitter (see Figure 1). Thus, there are areas which are relatively close to the transmitter yet received the signal from another transmitter.

Buffer Area from	-1km to 1km			-2.5km to 2.5km			-5km to 5km			-7.5km to 7.5km		
	(1) <10km	(2) <20km	(3) <30km	(4) <10km	(5) <20km	(6) <30km	(7) <10km	(8) <20km	(9) <30km	(10) <10km	(11) <20km	(12) <30km
Time	-0.121***	-0.121***	-0.121***	-0.134***	-0.134***	-0.134***	-0.110***	-0.110***	-0.110***	-0.083***	-0.083***	-0.083***
	(0.024)	(0.024)	(0.024)	(0.028)	(0.028)	(0.028)	(0.039)	(0.039)	(0.039)	(0.029)	(0.029)	(0.029)
Treatment	0.120***	0.123***	0.151***	0.114**	0.116***	0.145***	0.142**	0.137***	0.168***	0.236***	0.198***	0.230***
	(0.041)	(0.032)	(0.029)	(0.047)	(0.036)	(0.033)	(0.060)	(0.043)	(0.039)	(0.087)	(0.041)	(0.036)
Diff-in-Diff	-0.104**	-0.094***	-0.097***	-0.094*	-0.082**	-0.085**	-0.134**	-0.109**	-0.112**	-0.230**	-0.148***	-0.147***
	(0.044)	(0.036)	(0.034)	(0.051)	(0.040)	(0.037)	(0.066)	(0.049)	(0.047)	(0.090)	(0.045)	(0.041)
Constant	0.207***	0.207***	0.207***	0.217***	0.217***	0.217***	0.200***	0.200***	0.200***	0.148***	0.148***	0.148***
	(0.021)	(0.021)	(0.021)	(0.025)	(0.025)	(0.025)	(0.032)	(0.032)	(0.032)	(0.026)	(0.026)	(0.026)
Observations	1,122	1,868	2,326	916	1,662	2,120	564	1,310	1,768	268	1,014	1,472
R-squared	0.056	0.049	0.051	0.053	0.047	0.049	0.051	0.044	0.046	0.064	0.046	0.046

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Boundary variations in the difference-in-differences estimates for the NPD vote share in the federal elections 1980

5.2.4 Randomization Inference

Moreover, I perform a permutation-based test in Table 8, which helps to shed light on the statistical significance of the result from the main analysis. The test uses all units within a distance of 30km of the border and randomly applies the treatment status to the same number as the number of observations treated in the original sample. Permutations are taken at the municipality level. After each repetition, there is a set of municipalities being treated on which I subsequently employ a difference-in-differences analysis. 1000 treatment effects are estimated. The two-sided p-value of 0.042 (column (4)) of the difference-in-differences estimate (third row) indicates that it is relatively unlikely that the original result occurred due to pure chance.

	NPD				
	(1)	(2)	(3)	(4)	(5)
	Original Effect Size	c	n	p=c/n	SE
Time	-.1209677	973	1000	0.9730	0.0051
Treatment	.1421741	0	1000	0.0000	0.0000
Diff-in-Diff	-.0930118	42	1000	0.0420	0.0063
Constant	.2108871	1000	1000	1.0000	0.0000
Robust standard errors in parentheses					
*** p<0.01, ** p<0.05, * p<0.1					

Table 8: Difference-in-Differences Perumutation Test: Federal Elections 1980

5.2.5 Heterogeneity and Mechanisms

Which were the decisive municipalities driving the variation? Table 9 introduces further interaction effects. In the same vein as Freedman, Franz, and Goldstein (2004), who hypothesize that campaign ads have a differential impact conditional on pre-existing levels of information, it is conceivable that the TV-program had a stronger impact on viewers with a weaker anti-fascist tradition. Further, there is reason to suspect that the size of a municipality produced heterogeneous treatment effects. Potentially, smaller municipalities experienced higher levels of peer-pressure and thus have been more susceptible to the film. Using data on population, religion, and turnout, I explore potential heterogeneity in the decrease in NPD vote share. In column (1) to (3), I create a binary indicator for the respective variables and interact them with the treatment and time dummy variables. The first five rows are concerned with the pre-treatment period, the following five rows with the post-treatment period, and the eleventh row shows the triple differences estimate. The first row displaying "Control (A)" on the left, presents the NPD's vote share in the pre-treatment election for municipalities which were located in the area suffering from the transmission failure ("Control") and simultaneously had a small population ("A"). In contrast, the second row displaying "Control (B)" presents the NPD's vote share in the pre-treatment election for municipalities which were located in the

area suffering from the transmission failure ("Control") and simultaneously didn't have a small population ("B"). Remaining rows follow the same structure. Column (1) introduces a dummy indicating whether a municipality has less than 5000 inhabitants, serving as a rough proxy for small municipal communities. Introducing another interaction term to the diff-in-diff analysis is equivalent to estimating triple difference, which is a difference between two difference-in-differences estimators. To illustrate: the triple differences estimate (DDD) in column (1) starts with the time change in averages for municipalities with population sizes below 5000 in the area which had TV-access. Then, it nets out the average change for municipalities with population sizes below 5000 in the control municipalities that were affected by the transmission failure. Lastly, it subtracts the average change for the larger municipalities with a population size above 5000 that were not affected by the transmission failure. What we are left with is how small municipalities have differentially responded to TV-access. Thus, column (1) indicates that the treatment effect for small and large municipalities differs by -0.099 percentage points of the total vote share. A potential explanation for this result can be sought in peer-pressure, which is arguably more prevalent in smaller communities.

	(1) Small Population	(2) No Strong Catholic Tradition	(3) Low Turnout
Before			
Control (A)	0.207	0.225	0.229
Control (B)	0.250	0.201	0.203
Treated (A)	0.356	0.448	0.467
Treated (B)	0.229	0.255	0.284
Diff (T-C)	0.170***	0.170***	0.157**
	(0.044)	(0.056)	(0.057)
After			
Control (A)	0.086	0.095	0.089
Control (B)	0.130	0.086	0.091
Treated (A)	0.140	0.170	0.150
Treated (B)	0.113	0.108	0.125
Diff (T-C)	0.071**	0.053*	0.027
	(0.032)	(0.031)	(0.032)
DDD	-0.099*	-0.117*	-0.130*
	(0.054)	(0.065)	(0.070)
Observations	2,456	2,456	2,456
R-squared	0.051	0.068	0.064
Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1			

Table 9: Triple Difference-in-Differences for the NPD vote share in the federal elections 1980

In the same vein, column (2) defines not having a strong catholic tradition as being below the 50th percentile in terms of the share of Catholics within a given municipality. The DDD result suggests that municipalities which didn't constitute Catholic strongholds experienced a -0.117 percentage points sharper decrease in the NPD's vote share compared to municipalities which were inhabited by a predominantly Catholic population. This result is plausible. As the Catholic church had a long anti-fascist tradition, the

informational value of the documentary might have been limited in places that were overwhelmingly Catholic.

Column (3) investigates the effect of low turnout. Municipalities with turnout rates below the 50th percentile, exhibit a more pronounced decrease in the trend of the NPD's vote share (-0.130 percentage points). Though highly speculative, a possible interpretation could consist in comparatively low turnout rates being indicative of a relatively weaker democratic culture. If these differences, in turn, are reflective of differences in the levels of information, then we might expect that the effect was more influential in places with lower levels of political information, as the informational value was relatively greater. Note that the full causal chain of this argument is by no means supported causally by the evidence presented above. Further, the effect is not robust to defining low turnout alternatively (e.g. below the 40th percentile).

More generally, it is important to note that these three results are not identified. Triple differences necessitate a more demanding identification assumption. The parallel trends assumption has to hold for the sub-categories as well. Thus, the results are fundamentally explorative and not causal.

Further, what were the mechanisms by which the NPD's vote share has been reduced? By the logic of composition, the reduction in the NPD's vote share must have been brought about by either a commensurate reduction in the vote shares of the other parties or by simple abstention. We are confronted with two possibilities. First, the transmission may have persuaded voters who were already going to turn out to switch their vote from the NPD to another party. Second, the transmission could have demobilized individuals who would have otherwise turned out for the NPD. As the turnout variable seems largely unaffected in the original difference-in-differences estimate (Columns (1) to (3) in Table 4) it hints at the persuasion mechanism. Yet there is also no statistically significant change in another party's vote share observable. The small effect size makes it hard to directly discriminate between the two hypotheses.

6 Conclusion

In this paper, I studied the impact of TV on political behavior. Exploiting variation in transmission during the broadcasting of "Endlösung", a documentary on the Holocaust watched by approximately half of the adult German population in 1979, I document that municipalities with TV-access reduced their support for extreme-right parties more than municipalities without TV-access. Employing two identification strategies, including multiple outcomes and placebo tests, I find that in municipalities that did not experience the transmission failure, electoral support for the extreme-right party NPD decreased by approximately 0.1 percentage points, a change more than 76 percent as steep compared to the observed change in municipalities that were affected by the transmission failure. These effects are further amplified (up to 0.23 percentage points) when defining the area affected by the transmission failure more conservatively by discarding municipalities in the

immediate neighborhood of the boundary. Given the small size of the absolute change, however, I interpret the 0.1 percentage point decrease in the NPD's vote shares as a relatively modest effect. The paper showed that mere exposure to the TV-program (i.e. holding constant that a national debate ensued subsequently) was sufficient to reduce support for extreme-right parties.

While political behavior is an important outcome, the effects of TV, in general, are not confined to the political realm: other forms of (non-political) behavior such as social capital and attitudes are affected as well (Putnam, 2000; Olken, 2009; Jensen and Oster, 2009; La Ferrara et al., 2012). Anecdotal evidence suggests that the broadcast had a broader impact on societal and political engagement, reaching beyond elections. Therefore, it would be interesting to look at (i) civic capital, approximated by association density, (ii) donations for Indochinese refugees, (iii) the contemporary density of Stolpersteine (memorial plaques embedded in the street paving serving as a decentralised Holocaust memorial), and (iv) the renaming of streets where the former name has been associated with the Nazi-regime. These additional tests would also be important in judging whether the observed electoral effect can be attributed to the documentary or whether it was simply a reaction of voters to the fact of being the victim of right-wing terrorism. Voters might sanction the extreme right for being directly affected from right-wing terrorism electorally but would they then establish reading groups or donate to help Indochinese refugees?

Civic capital is a structural feature of embedded democracies. Especially formalized civic capital in the form of associations and clubs offers individuals a place to meet, coordinate their interests, exchange their ideas and build up a capacity for collective action. While valuable as such, these formative features of associations have also been linked to the stability of democracy. In political struggles, the diffusion of ideas is instrumental in delimiting the set of feasible actions (Bermeo, 1992). By way of ideational transfers, associations are important vehicles in molding and un-molding democratic culture (Bermeo, 2003; Satynath, Voth and Voigtländer, 2017). Satyanath et al. (2017), for example, document the instrumental role of associations in the rise of fascism. At the same time, a broad literature established that associations can generate trust, which is suspected to be a major determinant of economic growth (e.g. Putnam, 1993; Nunn, 2011; Algan and Cahuc, 2010). Less studied, however, are the mechanisms through which associations emerge. One contribution of future research would be to fill this gap by examining the role of mass media in the development of civil society. In the aftermath of the transmission, people across West Germany started to establish debating and research groups (Märthesheimer and Frenzel, 1979). I intend to proxy civic capital by the change in association density over time.

Further informal evidence suggests that shortly after the broadcast, politicians used the images to draw historical parallels between the Holocaust and the situation of the Indochinese refugees. In a further paper, I would like to compliment the analysis of voting behavior by looking at political engagement more broadly.

Coinciding with the broadcast, the initial reluctance towards Indochinese refugees turned into a wave of broad engagement. Many Germans donated money and household appliances via fundraising campaigns like "Helft den Vietnamesen", "Vietnam-Büro e.V" and the initiative by the German newspaper "Die Zeit", all of which surfaced in 1979. The organizations behind these actions are still active today (e.g. Ring Christlich-Demokratischer Studenten CDU, and Die Zeit). Records of individual donations might be retrievable via their archives.

Lastly, looking at the contemporary density of Stolpersteine, and the renaming of streets where the former name has been associated with the Nazi-regime, might reveal important changes in the way society responded to the TV-program.

By doing so, it might shift the focus of the German *Vergangenheitsbewältigung* away from gestures of statesmen, grand speeches, and other forms of "symbol" politics, and give back agency to where only a passive role has been ascribed before: the masses.

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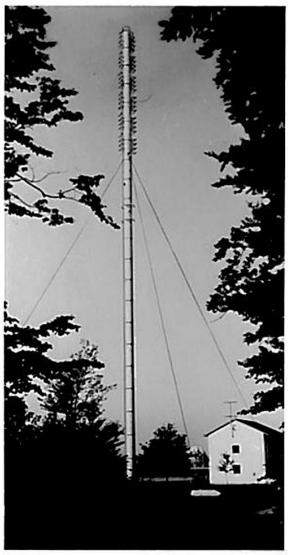
A Appendix

Koblenz.



74 m hoher Rohrmast mit Fernsehantenne,
6 x 4 Achterfelder von Siemens und
UKW-Antenne, 2-Ebenen-Würfelantenne
von Siemens darunter. (1)

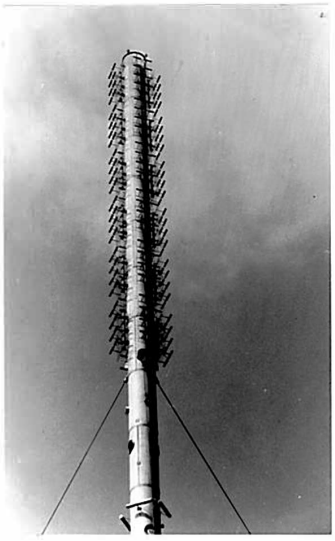
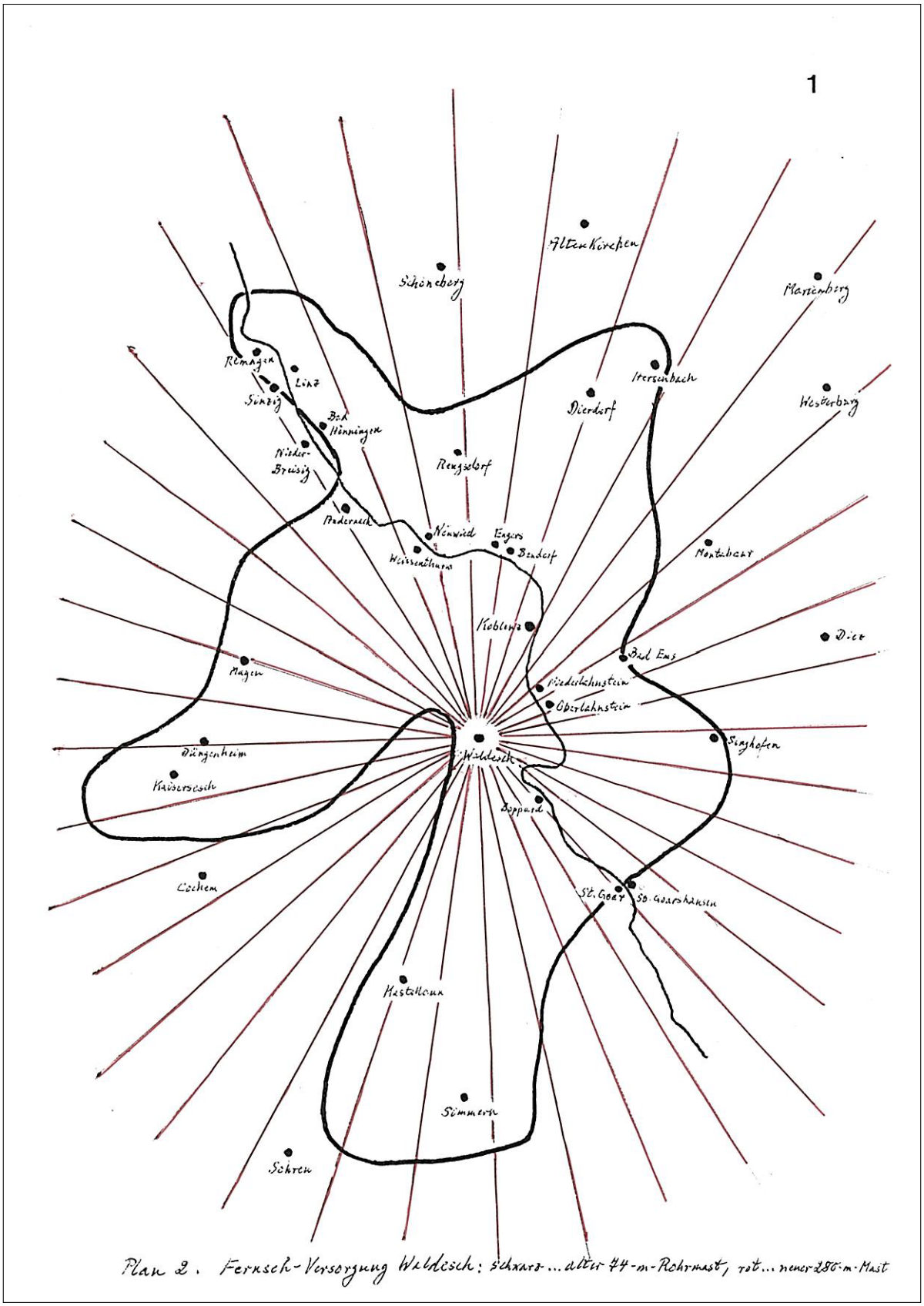


Figure 4: Images of the Sender Koblenz



Plan 2. Fernseh-Versorgung Waldesch: schwarz ... alter 74-m-Rohrmast, rot ... neuer 286-m-Mast

Figure 5: Map of the signal range provided by Sender Koblenz