A Spatial Analysis of the Impact of West German Television on Protest Mobilization During the East German Revolution

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Formal models of revolutionary collective action suggest that “informational cascades” play a crucial role in overcoming collective action problems. These models highlight how information about the aggregate level of participation in collective action conveys information about others’ political preferences, and how such informational cues allow potential participants to update their beliefs about the value of participating in collective action. In authoritarian regimes, foreign mass media are often the only credible source of information about anti-regime protests. However, very little systematic evidence exists on whether foreign media can indeed serve as a coordination device for collective action. In this paper we make use of a detailed dataset on protest events during the 1989 East German revolution and exploit the fact that West German television broadcasts could be received in most but not all parts of East Germany. Our empirical analysis does not support the widely accepted claim that West German television served as a coordination device for anti-regime protests during the East German revolution.
The recent revolutions in the Middle East notwithstanding, overthrowing an authoritarian regime is incredibly difficult. It requires ordinary citizens to overcome collective action problems and to coordinate their anti-regime behavior (Lichbach 1998). Such coordination is complicated both by the prevalence of preference falsification (Kuran 1989, 1991, 1995), which makes it difficult to accurately gauge the strength of anti-regime sentiments among the citizenry, and the absence of free mass media in authoritarian regimes. Recognizing this quandary, the United States during the Cold War attempted to reach audiences behind the Iron Curtain through radio stations such as RIAS, Voice of America, Radio Liberty, and Radio Free Europe. Some of these radio stations were explicitly designed as “surrogate” stations intended to emulate the domestic radio stations that communist countries would have had if they had been free. The U.S. government hoped that access to information not available from Eastern Europe’s state-controlled media would counteract communist propaganda, keep democratic ideas alive in Eastern Europe, and eventually facilitate the overthrow of the communist dictatorships (Quester 1990; Parta 2007).

Anecdotal evidence indeed suggests that foreign mass media served as a coordination device for anti-regime collective action during the fall of communism in Eastern Europe (Garton Ash 1990; Huntington 1991; Whitehead 1996; Schmitter 1996; Pridham 1997; O’Neil 1998). However, with the exception of a recent study by Kern (2011), there exists little systematic empirical evidence about whether foreign mass media can indeed facilitate revolutionary collective action in authoritarian regimes. In this paper, we evaluate the role played by West German television (WGTV) during the East German revolution. Because of its historical significance and the unparalleled wealth of primary sources that have become available after German reunification, the East German revolution is a prominent case in the literature on social movements and revolutionary collective action (e.g., Hirschman 1993; Lohmann 1994; Pfaff and Kim 2003; Pfaff 2006). According to this literature, WGTV played a key role during the East German revolution. In contrast to East German mass media, which first ignored the anti-regime protests that broke out over the summer of 1989 and later denounced them as the work of a few “anti-social thugs,” WGTV broadcast news about the

WGTV refers to ARD and ZDF, the two major West German public broadcasting stations. Commercial television was introduced in West Germany in the mid-1980s, but it did not broadcast to East Germany and could only be received in some areas near West Berlin. ARD and ZDF have a public service orientation and devoted a great deal of attention to politics in East Germany.
escalating political crisis in East Germany directly into East German living rooms. The literature suggests that by spreading knowledge of successful protests and the unexpected vulnerability of the East German dictatorship, WGTV was able to alter East Germans’ perceptions of political opportunity and to facilitate the activation and diffusion of protest (Kuran 1991; Opp, Voss and Gern 1993; Opp and Gern 1993; Hirschman 1993; Jarausch 1994; Grix 2000).

In this paper, we evaluate this claim that WGTV served as a coordination device for protest activities during the East German revolution. Our research design is based on a natural experiment, making use of the fact that WGTV broadcasts could be received in most but not all parts of East Germany (Kern and Hainmueller 2009). Using a detailed micro-level dataset of more than 2,700 protest events that took place between September 1989 and March 1990, we model the risk of protest events at the county-level conditional on access to WGTV and a rich set of county-level covariates. Our empirical analysis does not support the view that WGTV served as a coordination device for protests during the East German revolution.

I. The 1989 East German Revolution

Until its collapse in 1989, the German Democratic Republic (GDR) was seen as one of the most stable communist regimes in Eastern Europe. Western observers were impressed with East Germany’s economic performance and regarded it as a socialist success story (Kopstein 1997). The Socialist Unity Party of Germany (Sozialistische Einheitspartei Deutschlands, SED) ruled East Germany through an implicit social contract that rewarded political acquiescence with steadily improving living standards (Pollack 2000; Dale 2005). As the 1980s progressed, however, the East German regime became increasingly incapable of buying off its people. During the 1970s, East German economic growth had been spurred by easy access to international credit and economic aid from the Soviet Union. The 1970s oil shocks, rising interest rates, and reduced Soviet subsidies revealed the structural weaknesses of East Germany’s centrally planned economy. With its focus on heavy industry and the extensive use of factor inputs, the East German economy was ill-equipped to participate in the technological revolution that was propelling growth in capitalist economies (Stiglitz 1994). By the late 1980s, stagnation had become evident (Kopstein 1997). Even though nominal wages continued to rise, inflation and frequent consumer goods shortages left many East Germans
with the impression that their standards of living were stagnating, if not declining (Schneider 1996). Moreover, in direct comparison with West Germany, which was facilitated by the availability of WGTV and visits to and from West Germany, the gap in living standards had never been as plainly visible as in the late 1980s (Dale 2005). In short, the East German regime fell far short of delivering the material benefits expected by its citizens.

The appalling state of the environment also undermined public support for the communist regime. The East German economy was geared towards maximizing output regardless of the ecological consequences. East Germany’s large chemical industry and the use of brown coal for electrical power production, compounded by a lack of effective pollution abatement technology, led to dramatic ecological damages (Stinglwagner 1999). East Germany’s emission rates of pollutants such as particulate matter, sulfur dioxide, and nitrogen oxides were among the highest in Europe. Indeed, per capita sulfur dioxide and particulate matter emissions were more than 15 times as high as in West Germany (Buck 1996b).

East Germans had plenty of reasons to criticize the communist regime during the 1980s—it neither provided them with sufficient wealth nor a healthy environment. Yet, while they might have wanted to speak out, they could not. As in other communist countries, political dissent was not tolerated in East Germany. Lacking the opportunity to voice their concerns, many dissatisfied East Germans left their country, either legally after they had received exit visas (after a waiting period that could last up to 10 years) or illegally. Dissidents who remained could only find support within the confines of the Protestant church. A number of clergymen supported small groups of political activists concerned with issues of peace, sustainable development, the environment, and human rights by providing them with access to modest resources such as telephones, copying machines, and meeting space (Neubert 1998; Pollack 2000; Dale 2005). Some activists dismissed from their state jobs also found employment with the church. In the late 1980s, a tiny East German samizdat press emerged that attempted to create a public sphere independent of the regime.

The majority of East Germans, however, neither found the political program nor the lifestyle of these political activists attractive. Most East Germans valued higher living standards and the freedom to travel to the West more than the “socialism with a human face” that dissidents attempted to bring about (Oberschall 1996; Pollack 2000; Dale 2005). Opposition groups remained
on the fringes of popular political activity, neither organizing nor playing a key role in the protests that erupted in September and October 1989 (Opp, Voss and Gern 1993; Opp and Gern 1993). The first “Monday demonstrations” in Leipzig were predominantly led by East Germans who had applied for exit visas and were hoping that public protest would make them enough of a nuisance to the regime to be allowed to leave the country (Pollack 2000).

In contrast to Hungary and Poland, no significant impetus for reform arose from within the ruling party. Erich Honecker, General Secretary of the SED’s Central Committee since 1971, insisted on a narrow and inflexible interpretation of Marxism-Leninism and enforced strict norms of party discipline. Following Mikhail Gorbachev’s accession to the leadership of the Soviet Communist Party in 1985, relations between the GDR and the Soviet Union swiftly soured, with East Germany distancing itself from Gorbachev’s reform program. Most East Germans in contrast welcomed Gorbachev’s reforms and hoped for similar changes in East Germany (Süss 1996, 1999).

The East German regime’s situation became increasingly difficult when Hungary and Poland both effectively left the Soviet bloc at the beginning of 1989 (Roberts 1999). In January, non-communist parties were legalized in Hungary. In February, round-table talks began in Poland and in June, the Polish Communist Party relinquished its hold on power. In May, Hungary started to dismantle its border fortifications with Austria. Throughout the summer, thousands of East Germans vacationing in Hungary made use of what seemed like a once-in-a-lifetime opportunity to escape to the West. Others occupied West German embassies in Prague, Warsaw, and Budapest to force their emigration to West Germany. The East German regime eventually relented and allowed all East Germans occupying West German embassies to leave for West Germany (Zelikow and Rice 1995). The regime’s demand that the trains carrying them to West Germany had to cross East German territory so that they could be formally stripped off their citizenship and expelled from East Germany backfired, however. Many East Germans learned about this compromise solution from WGTV. Along the train route, riots broke out between the police and desperate East Germans who attempted to board the trains. The East German leadership, focused on the celebration of the 40th anniversary of the GDR on October 7, had seriously misread the public mood. While East German media remained silent about the riots, WGTV broadcast dramatic footage of the arrival of thousands of East German emigrees in West Germany. This mass exodus and the way it
was mismanaged by the East German regime significantly fueled public protests (Hirschman 1993; Lohmann 1994; Pfaff and Kim 2003).

II. Revolutionary Collective Action

Kuran (1989, 1991, 1995) has offered an elegant explanation for why revolutions, including the East German revolution, are so often unanticipated. In his view, people who dislike an authoritarian regime tend to publicly support it as long as the opposition seems weak, since the costs of siding with the seemingly unpopular opposition are higher than the psychological costs of pretending to support the secretly despised status quo. However, because of such “preference falsification,” even slight surges in the strength of the opposition can lead to “revolutionary bandwagons” in which authoritarian regimes that once appeared unshakeable quickly see their support crumble. Others have explicitly modeled “informational cascades” (Bikhchandani, Hirshleifer, and Welch 1988) in which information about the aggregate level of participation in revolutionary collective action conveys information about others’ political preferences. These informational cues then allow potential participants to update their beliefs about the value of participating in anti-regime collective action (Lohmann 1994, 2000).

The classic studies by Kuran and Lohmann, however, do not contain a detailed discussion of the channels that transmit information about the size of the opposition, as in Kuran (1989, 1991, 1995), or aggregate turnout in protest events, as in Lohmann (1994, 2000). The widely-cited article by Lohmann (1994), for example, succeeds admirably in describing how the Monday demonstrations in Leipzig helped to change public perceptions of the vulnerability of the East German regime. It also notes that mass demonstrations in Leipzig “triggered a wave of political protest throughout the GDR” (Lohmann 1994: 42). This account, however, does not explain how East Germans learned about protest events that the vast majority of them could not directly observe.

Several authors have stressed WGTV’s impact on protest during the East German revolution.\(^2\)

\(^2\)Demonstrations spread rapidly across East Germany in the fall of 1989. The first Monday demonstration in Leipzig on September 4 attracted only 800 participants (Schwabe 1999). But within little more than one month, tens of thousands of East Germans were marching through East German cities proclaiming “We are the people!” Isolated protests took place after the first Monday demonstration, but protests did not really start to spread across the country until the beginning of October. From the beginning to the end of October, protests erupted in 127 additional counties. By November 9, the day the Berlin Wall was breached, protests had taken place in 178 out of 217 East German counties.
Kuran (1991: 37) for example writes with respect to the demonstrations that took place in East Berlin during the celebrations of the 40th anniversary of the GDR that WGTV “immediately played these events back to the rest of East Germany. The scenes alerted disgruntled citizens in every corner of the country to the pervasiveness of discontent, while the government’s weak response revealed its vulnerability.” Opp and Gern (1993: 675–676) note that “the [Leipzig] Monday prayers, together with the demonstrations, contributed to the emergence of protest in other East German cities. People were informed, primarily on WGTV, about the events in Leipzig, and the expectation formed that citizens in each city would meet spontaneously on the city square for Monday demonstrations.” With regard to the exiting crisis, Hirschman (1993: 198) writes that “pictures of the exodus soon flooded the TV screens, with the result of not just causing established critics [. . . ] to sharpen their criticism but also of making activists out of long-passive average citizens.” Jarausch (1994: 44), Opp, Voss, and Gern (1993: 254–255, 260), and Grix (2000: 32–33) similarly suggest that WGTV served as a coordination device that enabled ordinary East Germans to organize anti-regime collective action by providing them with information about protest events elsewhere and increasing their awareness of both the widespread discontent and the vulnerability of the communist regime.

While these anecdotal accounts have a certain appeal, they lack strong empirical support from the wider literature. Empirical research on mass media as a facilitator of collective action has largely been limited to the impact of domestic mass media in democratic societies (Oberschall 1989; Tarrow 1989; McAdam and Rucht 1993; Soule 1997; Myers 2000; Roscigno and Danaher 2001; Oliver and Myers 2003; Andrews and Biggs 2006.) Yanagizawa-Drott (2012), one of the few works that examines the effects of domestic mass media in an authoritarian setting, finds that radio broadcasts served as a coordination device for collective violence during the Rwandan genocide. Enikolopov, Petrova, and Zhuravskaya (2011), in a paper on media effects in Russia, demonstrate a strong impact of the only national television channel independent of the government on voting for opposition parties during the 1999 parliamentary elections. Adeno et al. (2013) document the role that radio propaganda played in undermining the democratic institutions of the Weimar Republic and the rise of nazism in pre-WWII Germany.

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3A parallel research literature focuses on the effects of social media technologies such as cell phones as opposed
Most directly related to our paper are articles by Kern (2011) and Grđesić (Forthcoming) that both look at WGTV’s role as a coordination device for protest activities during the East German revolution. Kern (2011) compares counties without access to WGTV to a matched comparison group of counties with access to WGTV, but does not find any evidence that WGTV affected the speed or depth of protest diffusion. Grđesić (Forthcoming) argues, based on vector autoregressions and Granger causality tests, that WGTV news reports about East German protests “Granger caused” protests in East Germany the following day. Our paper differs from this previous research in a number of ways. First, in contrast to Kern (2011)’s analysis, which is based on differences in means after matching, we base our analysis on all 217 East German counties and directly control for an expanded set of covariates in our statistical analysis. We believe our approach is preferable because matching not only dropped 80% of East German counties from the sample but also failed to produce close covariate balance between WGTV and non-WGTV counties (see Table 1 in Kern (2011)). Our approach, while admittedly based on somewhat stronger parametric assumptions, strikes us as more suitable given the relatively small number of East German counties without access to WGTV. Second, we improve upon Kern (2011)’s measurement of the key independent variable, access to WGTV, by basing it on a radio signal propagation model (see below). Third, the question we ask is slightly different from the question asked in Kern (2011). Whereas Kern (2011) compared the speed and depth of protest diffusion across matched WGTV and non-WGTV counties, we ask whether access to WGTV increased the probability that an East German county would experience a protest event conditional on observables. Fourth, our analysis does not assume that protest events in neighboring counties are independent of each other. Our statistical model explicitly allows for spatial diffusion, something that Kern’s earlier non-parametric analysis could not accommodate. Finally, in contrast to Grđesić’s (Forthcoming) time series analysis, we believe that comparing WGTV counties to non-WGTV counties after taking differences in observable county characteristics into account is a better way of getting at WGTV’s impact on protest. When to mass media technologies such as radio and television (Warren 2014; see also Warren Forthcoming). Shapiro and Weidmann (2013), for example, argue that the availability of cell phone networks reduced collective violence in Iraq between 2004–2009 by lowering the transaction costs of cooperating with the government. Pierskalla and Hollenbach (2013) present cross-national evidence for 27 African countries that suggests that the availability of cell phone technology facilitates violent collective action.

4 We also show that our results hold when we focus on a smaller subsample of highly comparable counties similar in spirit to Kern’s (2011) matched analysis but with much better covariate balance.
rich micro-level data are available, as is the case here, it seems preferable to us to conduct the analysis at a lower level of aggregation than time series analysis, which glosses over all differences across counties and fails to exploit the naturally occurring variation in access to WGTV across East Germany.\footnote{Grđesić’s (Forthcoming) analysis includes all East German counties, whether they actually had access to WGTV or not.}

### III. Research Design

Our research design takes advantage of the fact that WGTV broadcasts could be received in most but not all parts of East Germany. Specifically the northeastern part of East Germany and the Dresden district in the southeast were by and large cut off from WGTV broadcasts due to topography and their distance from West German broadcast transmission towers. Historical maps of WGTV’s over-the-air signal strength used in previous research (Kern and Hainmueller 2009: Figures 1 and 3) allow us to distinguish between broad areas of East Germany with and without access to WGTV. However, these maps have a serious drawback in that they are not detailed enough to allow us to reliably determine the availability of WGTV at the county level (see the discussion of this point in Kern 2011). We therefore use the Longley-Rice electromagnetic signal propagation model in conjunction with terrain data and data on the location and technical characteristics of WGTV broadcast transmitters to model WGTV’s signal strength across East Germany (see Figure 1). We then discretize this continuous measure of WGTV signal strength to distinguish between East German counties with and without access to WGTV. We set the threshold value as the modeled average signal strength in the center of the city of Dresden. For a county to have access to WGTV, we require that at least 50% of the county receives signal equal to or above that threshold (see Figure 2). While the map in Figure 2 closely reproduces the overall pattern of the historical maps shown in Kern and Hainmueller (2009), our approach classifies a number of counties differently than Kern (2011).\footnote{The same approach has been used in research on media effects in economics to model the availability of radio and television signals (Ollken 2009; Enikopolov, Petrova, and Zhuravskaya 2011; DellaVigna et al. Forthcoming). See our online appendix for a detailed discussion.} In the robustness section, we show that our

\footnote{See the online appendix for details on our coding rule and its justification.}

\footnote{We code 5 counties as not having WGTV that Kern (2011) codes as having WGTV and 2 counties as having WGTV that Kern (2011) codes as not having WGTV. Our coding rules agree for 20 counties. According to our approach, the number of East German counties without WGTV is therefore 25, compared to 22 in Kern (2011).}
results are unaffected when we use Kern’s (2011) classification instead. Moreover, we also show that our results are unaffected by the exact signal strength threshold used to separate counties with WGTV from counties without WGTV.

Our identifying assumption is that access to WGTV was idiosyncratic conditional on covariates. In other words, we assume that whether a county had access to WGTV or not was uncorrelated with unobserved county characteristics related to the risk of protest, conditional on observed characteristics. Kern (2011) already noted that counties with and without WGTV differed in terms of a number of covariates, several of which are plausibly related to the risk of protest.

Table 1 shows balance statistics, comparing counties with WGTV to counties without WGTV. The first set of covariates (log(population size), population density, sector shares (industry, agriculture, crafts and construction, services and transportation), the share of skilled and unskilled labor, and the proportion of the population that holds a college degree, is female, and of working age) captures broad socioeconomic differences between counties. Other covariates capture disparities in several dimensions of quality of living that were particularly salient in East Germany (Buck 1996a, 1996b): housing quality (housing space per capita and the proportion of apartments with bathrooms, interior toilets, and modern heating), the provision of public goods (residents per medical doctor and residents per dentist), and air pollution (nitrogen oxides, sulfur dioxide, and particulate matter emissions). The district capital dummy denotes the capitals of the 15 districts (Bezirke). We also measure the percent population change over the preceding decade, which we regard as a rough proxy measure for the overall quality of living in a county.

The first two columns of Table 1 show covariate means for counties with and without access to WGTV, the third column shows differences between covariate means standardized by the square root of the average variance, the fourth column shows $p$-values from two-sample $t$-tests and the last

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9While residential mobility was very low in East Germany, we still see meaningful changes in population size across counties, with 64 counties experiencing a change of more than ±5% and 11 counties experiencing a change of more than ±10% over the 1980–88 period.
column shows \( p \)-values from bootstrapped Kolmogorov-Smirnoff tests of equality of distributions (20,000 bootstrap samples). For several of these covariates we see larger differences between the two groups of counties than what one would expect by chance alone. Clearly, our natural experiment, while providing us with variation in access to WGTV, only imperfectly approximates a randomized experiment. We will adjust for these systematic differences between counties with and without WGTV in our analyses.

Protest event data are taken from Schwabe (1999), which provides a detailed country-wide compilation of county-level protest events between September 4, 1989 and March 18, 1990, the date of the first free election in East Germany. This compilation is based on records of the East German Ministry of the Interior, which assembled daily crisis reports submitted by local police forces, the records of the Ministry of State Security, and numerous published secondary sources. Schwabe’s compilation of protest events is the most comprehensive source of protest event data available for the East German revolution, containing 2,734 occurrences of politically motivated public protests. Figure 3 shows a heat map of protest counts, while Figure 4 compares the distributions of the number of protest counts for counties with and without WGTV using violin plots. On average, counties with access to WGTV experienced about 12.9 protest events, while counties without access to WGTV experienced about 10.6 protest events. The raw data thus seem to provide some face validity for the claim that WGTV increased the likelihood of protest events. Of course, given the systematic differences between counties with and without WGTV documented in Table 1, this difference does not represent a credible estimate of the causal effect of WGTV.

A. Statistical model

Given the spatial and temporal dimensions of our data, a simple non-parametric comparison of counties with and without access to WGTV is not feasible. Any robust analysis of the impact of WGTV on the occurrence of protest during the East German revolution will need to account for both time- and space-dependency. Moreover, as we saw in the previous section, it will also have to account for systematic differences between counties with and without WGTV. We therefore rely on a spatial survival model explaining the risk and timing of protest events as a function of access to
WGTV, conditional on covariates. A critical feature of the East German revolution was that East
German counties were at risk of experiencing multiple protests over the course of the revolution,
and in fact, typically did. As a consequence, a repeated events modeling approach that accounts
for the clustered nature of the data is needed.

As Box-Steppensmeier, De Boef, and Joyce (2007) note, repeated events processes may produce
within-unit correlations in the risk of event for either of two reasons. On the one hand, there may
be event dependence, so that experiencing a previous event makes a unit more (or less) likely to ex-
perience a subsequent event. Thus, for example, event dependence would exist if a previous protest
in an East German county made subsequent protests in that county more likely, perhaps due to
protesters’ increased awareness of the East German regime’s weakness. Alternatively, heterogeneity
in risk propensity may make some counties more susceptible (in survival modeling terminology,
more frail) to multiple protests than other counties. This would be the case, for example, if cross-
county heterogeneity in deprivation made multiple protests more likely in some counties than in
others, and covariates measuring this deprivation were excluded from the structural portion of the
model.

In an important set of recent analyses, Box-Steppensmeier and De Boef (2006), Box-Steppensmeier,
De Boef, and Joyce (2007), and Box-Steppensmeier, Linn, and Smidt (Forthcoming) compare and
contrast the performance of a variety of estimators for modeling repeated events processes. The
conclusion from these studies is consistent: the conditional frailty gap time model is the pre-
ferred model among available repeated events models. In examining both bias and efficiency of
five modeling approaches (the conditional frailty gap time model, the Anderson-Gill model, the
conditional risk-set elapsed time model, the conditional risk-set gap time model, and the standard
frailty model), Box-Steppensmeier and De Boef (2006) find that the conditional frailty gap time
model performs at least as well as the alternative models in all conditions examined, and is the
only model to perform well when both event dependence and unobserved heterogeneity are present.
As a consequence, we employ the conditional frailty gap time model in this analysis.

The conditional frailty model is the preferred alternative because it is able to account simulta-
neously and directly for both event dependence and heterogeneity in repeated event processes.
It accounts for the former by specifying a conditional risk set, such that unit i is not at risk for
experiencing the $k$th event until it has experienced the preceding $k - 1$ events. Separate baseline hazards are allowed for each event by stratifying on event number. As Box-Steffensmeier, DeBoef, and Joyce (2007: 240) note, stratifying on event number accounts for event dependence, allowing for consistent coefficient estimation.

Heterogeneity is accounted for by specifying random effects, or frailty terms, for the units in the analysis. If unmodeled risk factors produce significant heterogeneity in risk propensity, the variance of the random effects will be distinguishable from zero. Thus, a probability distribution is specified for the random effects (typically either a gamma or Gaussian distribution), and the variance parameter ($\theta$) for the random effects is examined to determine whether it is distinguishable from zero. If it is, the covariates in the model do not fully capture the heterogeneity in risk propensity and the random effects are required to account for this.

The conditional frailty model can be specified in either gap time or elapsed time formulation. In the former, the time to event is the time since the unit experienced the preceding event in the repeated events process. In the latter, the time to event is the time since the unit first entered the observation period. Typically, scholars’ substantive interest is in explaining the occurrence of an event since the preceding event, and as a consequence, we employ the standard gap time modeling approach for the conditional frailty models in this paper.

An important choice in modeling repeated events processes is whether to employ a parametric model such as the Weibull or the exponential, or the Cox semi-parametric alternative. The critical distinction between these two classes of alternatives is how the baseline hazard is modeled. In the parametric alternatives, the baseline hazard is assumed to follow a specific parametric distribution and is modeled by specifying this distribution. The principal advantage of a parametric model is that it provides a direct parametric estimation of duration dependence and provides for out-of-sample prediction. The critical drawback of a parametric model is that the coefficient estimates are dependent upon the choice of parametric distribution for the baseline hazard.

Alternatively, a Cox semi-parametric model may be employed. In contrast to the parametric alternatives, the Cox model does not specify a parametric distribution for the baseline hazard. As a consequence, the baseline hazard is not directly modeled, although an empirical baseline hazard may be recovered post estimation. The advantage of the Cox model is that it is much more
flexible than its parametric counterparts and is not susceptible to erroneous inferences that may arise regarding the effects of substantive predictors as a consequence of misspecifying the underlying baseline hazard. A disadvantage of the Cox model is the absence of a (correctly chosen!) parametric model of the baseline hazard and corresponding out-of-sample predictions. In our application, we are not interested in out-of-sample prediction. Given this, we employ the much more flexible Cox semi-parametric modeling approach.

The hazard in our conditional frailty gap time model takes the form

$$\lambda_{ik}(t) = \lambda_{0k}(t - t_{k-1})e^{X_{ik}\beta + \omega_i},$$

with $k$ denoting the event number, $\lambda_{0k}$ a baseline hazard rate that varies by event number (i.e., event strata), $(t - t_{k-1})$ denoting the gap time formulation with the hazard being the risk for event $k$ occurring since the occurrence of the $(k-1)$th event, $X$ denoting the matrix of predictors including the WGTV treatment indicator, $\beta$ denoting the coefficients to be estimated, and $\omega_i$ denoting the unit-specific frailties that are assumed to be constant over time for each unit but may vary across units (Box-Steffensmeier, De Boef and Joyce 2007: 242).

The partial likelihood, conditional on the frailty terms, is then

$$L(\beta) = \prod_{i=1}^{n} \prod_{k=1}^{K} \left( \frac{e^{X_{ik}\beta + \omega_i}}{\sum_{i=1}^{n} \sum_{k=1}^{K} Y_{ik} e^{X_{ik}\beta + \omega_i}} \right)^{\delta_{ik}},$$

where again, $k$ refers to the event number or stratum, $\delta$ is a censoring indicator that equals 1 if the unit is observed and 0 if the unit is censored, and $Y_{ik}$ is an at-risk indicator that is equal to 1 if unit $i$ is at risk of experiencing event $k$ and 0 otherwise (Box-Steffensmeier, De Boef, and Joyce 2007: 242).

The conditional frailty gap time model introduced by Box-Steffensmeier and De Boef (2006) is a critical advance in estimation for repeated events models. Simultaneous to this advance in repeated events modeling, scholars have also been making advances in modeling spatial dependence in event processes. Here, scholars are interested in examining whether risk propensity among the neighbors of unit $i$ affects the risk of unit $i$ experiencing the event of interest. These advances are reflected in, among others, Banerjee, Carlin, and Gelfand (2004) and Darmofal (2009). To date, however, these advances in repeated events modeling and spatial modeling have occurred along separate tracks.
To the best of our knowledge, this is the first analysis that simultaneously accounts for spatial dependence, event dependence, and heterogeneity within a repeated events framework. It does so through a spatial conditional frailty gap time modeling framework.

We extend the conditional frailty model to include spatial dependence through the inclusion of a conditional spatial lag predictor variable. Specifically, we include a temporally lagged spatial lag variable that measures the proportion of county $i$’s neighbors that experienced a protest event on the preceding day, during the preceding week, or during the preceding 2 weeks. Thus, we allow for neighboring counties’ recent protests to affect the probability of county $i$ experiencing a protest event, conditional on covariates, access to WGTV, and the frailty terms.

IV. Results

Table 2 presents estimates from 5 Cox model specifications. The specification in column 1 contains a dummy variable for access to WGTV but no covariates or frailty terms. Column 2 adds the covariates listed in Table 1 and column 3 also adds frailty terms to account for unobserved heterogeneity across counties, as outlined above. Coefficient estimates for covariates and frailty terms are not shown. Exponentiating the coefficient estimate for WGTV gives us the estimate of WGTV’s multiplicative effect on the probability of a protest event occurring conditional on covariates and frailties. Standard errors are shown in brackets. Figure 5 graphically presents the estimates of WGTV’s multiplicative effect based on the models shown in Table 2 with 95% confidence intervals. In none of the three specifications is the impact of WGTV statistically significant at the 0.05 percent level, and only in the specification without covariates or frailties is it significant at the .10 percent level.

This null result could be due to our focus on the East German revolution in its entirety. If WGTV did have an effect on protest activities, we would expect the effect to decrease over time as East German media became more independent from the East German regime and more able to provide unbiased coverage of the mounting regime crisis (Kern 2011; Grdésić Forthcoming).

\footnote{For example, in column 3, $\exp(0.089) \approx 1.09$, for an approximate 9% increase in the probability of a protest event occurring.}
a pattern of decreasing WGTV effects would provide strong evidence for the impact of WGTV, perhaps even more convincing than if we had found a positive effect of WGTV throughout the entire East German revolution.

Specifications 4 and 5 in Table 2 look at over-time heterogeneity in the effect of WGTV. The specification in column 4 includes an interaction term between WGTV and a dummy variable for the time period before the fall of the Berlin Wall (Before 11/10). The specification in column 5 allows the effect of WGTV to vary even more finely, by including interaction terms between WGTV and dummy variables for three time periods: September 4 to October 18 (Before October 19), October 19 to October 31 (Late October), and November 1–9 (Early November). These time periods roughly correspond to periods of increased media freedom in East Germany (Kern 2011). Neither specification shows heterogeneity in the effect of WGTV. There is no evidence that WGTV had a positive effect on the probability of a protest event occurring when looking at all protest events. The data also do not support the hypothesis that WGTV’s effect was larger during the early stages of the revolution. Both of these findings are in line with Kern (2011) but run counter to Grdēsić’s (Forthcoming) result that WGTV had a positive effect on the frequency of protest events during the beginning stages of the revolution.

A. Robustness

In this section of the paper we subject our finding that access to WGTV had no discernible effect on the probability of a protest event occurring to a series of robustness checks.

We start by investigating the sensitivity of our results to spatial diffusion processes. In areas of East Germany that had access to WGTV, both WGTV and social networks could have served as coordination devices for collective action by providing East Germans with political information not available from the censored East German media. Survey data collected by Opp and his colleagues (Opp and Gern 1993; Opp, Voss, and Gern 1993) indeed show that social networks played an important role in mobilizing the Leipzig protests. In contrast, social networks were the sole providers of uncensored political information in parts of East Germany that did not have access to

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11See the non-significant interaction term in the specification in column 4. For the specification in column 5, we use a LR test to test for joint significance of the 3 interaction terms: \( \chi^2_3 = 0.52; p = 0.92 \).
WGTV. If social networks served as a substitute for WGTV when WGTV was not available, the estimated effect of WGTV would be biased downwards. We at least partially control for this possibility by including spatial lag variables that measure the extent to which protest events triggered subsequent protests in neighboring counties. This strategy does not directly capture the existence of social networks, a task for which we would need individual-level data. However, it does allow us to control for one important effect of social networks, the spatial diffusion of protest (McAdam 1986; Hechter 1987; Gould 1991).

Table 3 shows impact estimates from 5 Cox models. Specification 1 adds a temporally lagged spatial lag variable to the baseline specification in column 3 of Table 2. This spatial lag variable measures the proportion of a county’s neighboring counties that experienced a protest event on the preceding day. Specification 2 adds such a spatial lag variable for the preceding week, and specification 3 does the same for the preceding 2 weeks. Specification 4 simultaneously includes all three spatial lag variables. Across all 4 specifications, the estimated impact of WGTV remains small and statistically insignificant even though the spatial lag variables are strong, highly significant predictors of protest events (results not shown). Finally, specification 5 adds day of the week dummies to account for the fact that within each county, protests tended to take place on certain days and not others. While we indeed find very strong day of the week effects (results not shown), the inclusion of day of the week dummies does not affect our WGTV estimate. It remains small and statistically insignificant.

Table 4 evaluates the sensitivity of our results to alternative measures of access to WGTV. Here we simply present the results and refer readers to our online appendix for details on our coding rules. Specifications 1–3 in Table 4 use increasingly strict cutoff values for coding a county as having access to WGTV. Specifications 4–7 use these same cutoffs, but now we use the fraction of a county’s area that is above the respective cutoff level as a measure of access to WGTV. Finally, specification 8 uses Kern’s (2011) WGTV coding. Across all specifications, the effect of WGTV is small (positive or negative) and far from statistical significance. Our results are not affected by

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12 For example, the “Monday demonstrations” in Leipzig and many other places.
how we measure access to WGTV.

Table 5 shows that our results do not change when we focus on specific subsamples of counties or when we only estimate the effect of WGTV on time to first event. Specification 1 in Table 5 omits the capital city of East Berlin from the sample. This does not affect the WGTV estimate or its statistical significance. Specification 2 drops the non-WGTV counties in the southeast from the sample and identifies the effect of WGTV only using the non-WGTV counties in the northeast. Specification 3 does the reverse, dropping the non-WGTV counties in the northeast from the sample but keeping the non-WGTV counties in the southeast. Even though the counties in the agrarian northeast are quite different from the counties in the industrialized southeast, both control groups generate small and statistically insignificant WGTV effect estimates. Following Kern (2011), specification 4 omits all WGTV counties that border at least one non-WGTV county (22 counties in total). We drop these counties from the sample to minimize the potential for spillover effects from WGTV to non-WGTV counties, stemming from East Germans crossing county borders to watch WGTV, for example. The effect estimate remains small and statistically insignificant.

Specification 5 uses a reduced sample chosen to maximize the comparability of WGTV and non-WGTV counties given the imbalances we saw earlier in Table 1. As in specification 4, we first dropped all WGTV counties bordering non-WGTV counties. We then estimated propensity scores using a probit model with all covariates entered linearly and dropped all counties for which there was no overlap in propensity score distributions (see Figure D in the online appendix). This left us with 22 non-WGTV counties and 34 WGTV counties. We then evaluated all \( \binom{34}{22} \approx 548 \text{ million} \) samples that can be generated by choosing 22 WGTV counties (without replacement) from the pool of 34 available WGTV counties in terms of the maximum absolute standardized difference in means across covariates. The sample with the best balance between WGTV and non-WGTV counties was used to estimate specification 5. In this sample of highly comparable WGTV and non-WGTV counties, the estimated WGTV effect is tiny and statistically insignificant.

13Balance is exceptional in this sample and much better than in Kern’s (2011) matched sample. The maximum absolute standardized difference in means across all 22 covariates is just 0.12 standard deviations; the average absolute standardized difference in means across all 22 covariates is 0.06 standard deviations. In fact, balance in this sample, again measured by the maximum absolute standardized difference in means across all covariates, is
Finally, specification 6 restricts the sample to time to first event in each county, which addresses the concern that WGTV might only have facilitated the first protest event in each county. The WGTV estimate is now negative but statistically insignificant.

[Include Table 5 about here.]

To sum up, our empirical analysis did not detect any effect of WGTV on the probability of a protest event occurring. This finding is robust to variation in the time periods we looked at, spatial diffusion, the measurement of WGTV access, and the specific samples of counties used in the estimations.

V. Conclusion

Work on preference falsification (Kuran 1989, 1991, 1995) and informational cascades (Lohmann 1994, 2000) has revolutionized political scientists’ understanding of how authoritarian regimes that are detested by the vast majority of their citizens can nonetheless remain in power. This literature has demonstrated that small perturbations in publicly available information about others’ political preferences can set off “revolutionary bandwagons” in which authoritarian regimes that once appeared unshakeable rapidly see their support crumble. Much less developed in the literature is a discussion of how politically sensitive information is actually transmitted under conditions of authoritarian rule. Anecdotal evidence and an emerging quantitative literature regarding the effects of mass and social media communication technologies on collective action suggests that communication technologies can facilitate collective action, but robust empirical research is still scarce.

In this paper, we have exploited a natural experiment in communist East Germany, making use of the fact that WGTV broadcasts could be received in some parts of the country but not others. The belief that WGTV served as a coordination device that facilitated public protests is much better than one would expect if WGTV had been randomly assigned to half of these 44 counties (permutation p-value < 0.000001). Finally, we also ran Cox models using the 100 best balanced samples among the approx. 548 million possible samples. Summary balance statistics for these 100 samples are shown in Table A and effect estimates are shown in Figure E, both in the online appendix. WGTV effect estimates are generally small (and mostly negative) and statistically insignificant for all samples but one. In this one sample, WGTV has a statistically significant but negative effect on the probability of a protest event occurring.
widespread (Kuran 1991; Hirschman 1993; Jarausch 1994; Grix 2000; Grděsić Forthcoming). In contrast, our empirical evidence shows that WGTV had no effect on protest activities in East Germany. Needless to say, our case study of the impact of WGTV during the East German revolution cannot rule out the possibility that foreign mass media, or communication technologies more generally, facilitate collective action in other cases. In fact, recent work by Adeno et al. (2013) and Shapiro and Weidmann (2013) convincingly demonstrates that mass and social communication technologies can affect collective action in sometimes unexpected ways. In this sense, our paper should perhaps be read as a cautionary tale. Despite the plausibility of the link between communication technology and collective action and the accumulating cross-national evidence, detailed case studies relying on credible causal identification strategies are needed to pin down the effects, or lack of effects, of communication technologies on collective action. At this point, all we know is that some communication technologies seem to affect collective action in some cases but not others. Clearly, much more research is needed.
VI. REFERENCES


Box-Steppensmeier, Janet M., Suzanna Linn, and Corwin D. Smidt. Forthcoming. “Analyzing the robustness of semi-parametric duration models for the study of repeated events.” *Political Analysis*.


VII. Tables and Figures
Figure 1: Signal strength of West German television in East Germany as predicted by Longley-Rice radio signal propagation model
Figure 2: East German counties with and without access to West German television based on Longley-Rice radio signal propagation model and Dresden cutoff level.
Figure 3: Number of protests per county, East Germany, September 4, 1989 – March 18, 1990
Figure 4: Violin plots of the distribution of the number of protests per county, East Germany, September 4, 1989 – March 18, 1990, for counties with/without access to WGTV
Figure 5: Causal effect estimates for WGTV from models (1) – (5) in Table 2 and 95% confidence intervals.
Table 1: Balance

<table>
<thead>
<tr>
<th>Covariate</th>
<th>WGTG mean</th>
<th>non-WGTG mean</th>
<th>standardized difference</th>
<th>variance ratio</th>
<th>t-test p-value</th>
<th>KS-test p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>log(population size)</td>
<td>11.01</td>
<td>10.99</td>
<td>0.03</td>
<td>0.70</td>
<td>0.90</td>
<td>0.67</td>
</tr>
<tr>
<td>population density</td>
<td>350.24</td>
<td>474.47</td>
<td>-0.15</td>
<td>1.13</td>
<td>0.48</td>
<td>0.52</td>
</tr>
<tr>
<td>% change in pop. size</td>
<td>-0.02</td>
<td>-0.01</td>
<td>-0.07</td>
<td>0.50</td>
<td>0.76</td>
<td>0.68</td>
</tr>
<tr>
<td>district capital</td>
<td>0.07</td>
<td>0.08</td>
<td>-0.05</td>
<td>0.83</td>
<td>0.83</td>
<td>0.83</td>
</tr>
<tr>
<td>% industry</td>
<td>0.36</td>
<td>0.31</td>
<td>0.30</td>
<td>1.15</td>
<td>0.15</td>
<td>0.12</td>
</tr>
<tr>
<td>% agriculture</td>
<td>0.17</td>
<td>0.19</td>
<td>-0.11</td>
<td>0.56</td>
<td>0.64</td>
<td>0.65</td>
</tr>
<tr>
<td>% crafts and construction</td>
<td>0.09</td>
<td>0.10</td>
<td>-0.12</td>
<td>1.04</td>
<td>0.58</td>
<td>0.57</td>
</tr>
<tr>
<td>% services and transportation</td>
<td>0.36</td>
<td>0.38</td>
<td>-0.27</td>
<td>0.77</td>
<td>0.23</td>
<td>0.28</td>
</tr>
<tr>
<td>% skilled</td>
<td>0.62</td>
<td>0.61</td>
<td>0.41</td>
<td>0.82</td>
<td>0.07</td>
<td>0.31</td>
</tr>
<tr>
<td>% unskilled</td>
<td>0.13</td>
<td>0.14</td>
<td>-0.25</td>
<td>1.35</td>
<td>0.22</td>
<td>0.27</td>
</tr>
<tr>
<td>% college</td>
<td>0.20</td>
<td>0.21</td>
<td>-0.17</td>
<td>0.72</td>
<td>0.47</td>
<td>0.60</td>
</tr>
<tr>
<td>% female</td>
<td>0.52</td>
<td>0.51</td>
<td>0.52</td>
<td>0.64</td>
<td>0.03</td>
<td>0.00</td>
</tr>
<tr>
<td>% working age</td>
<td>0.64</td>
<td>0.64</td>
<td>0.05</td>
<td>0.75</td>
<td>0.81</td>
<td>0.07</td>
</tr>
<tr>
<td>housing space (m²)</td>
<td>27.34</td>
<td>26.15</td>
<td>0.57</td>
<td>0.41</td>
<td>0.03</td>
<td>0.00</td>
</tr>
<tr>
<td>% bathroom</td>
<td>0.82</td>
<td>0.79</td>
<td>0.32</td>
<td>0.57</td>
<td>0.18</td>
<td>0.03</td>
</tr>
<tr>
<td>% interior toilet</td>
<td>0.74</td>
<td>0.73</td>
<td>0.06</td>
<td>0.83</td>
<td>0.80</td>
<td>0.82</td>
</tr>
<tr>
<td>% modern heating</td>
<td>0.43</td>
<td>0.42</td>
<td>0.03</td>
<td>0.49</td>
<td>0.90</td>
<td>0.08</td>
</tr>
<tr>
<td>residents per MD</td>
<td>596.34</td>
<td>616.24</td>
<td>-0.10</td>
<td>1.05</td>
<td>0.64</td>
<td>0.08</td>
</tr>
<tr>
<td>residents per dentist</td>
<td>1549.49</td>
<td>1573.60</td>
<td>-0.08</td>
<td>0.46</td>
<td>0.75</td>
<td>0.54</td>
</tr>
<tr>
<td>nitrogen oxides (tons/km²)</td>
<td>6.24</td>
<td>7.63</td>
<td>-0.09</td>
<td>0.93</td>
<td>0.68</td>
<td>0.57</td>
</tr>
<tr>
<td>sulfur dioxide (tons/km²)</td>
<td>81.55</td>
<td>86.25</td>
<td>-0.02</td>
<td>1.33</td>
<td>0.90</td>
<td>0.33</td>
</tr>
<tr>
<td>particulate matter (tons/km²)</td>
<td>33.10</td>
<td>34.12</td>
<td>-0.02</td>
<td>2.23</td>
<td>0.93</td>
<td>0.55</td>
</tr>
</tbody>
</table>

Note: The table shows covariate balance for 217 East German counties with and without access to WGTV. The first two columns show means, the third column shows differences in means standardized by the square root of the average variance, the fourth column shows variances in the WGTV group divided by variances in the non-WGTG group, the fifth column shows p-values from two-sample t-tests, and the last column shows p-values from bootstrapped Kolmogorov-Smirnoff tests of equality of distributions.
Table 2: Effect of WGTV on probability of protest event from Cox models

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>WGTV</td>
<td>0.113*</td>
<td>0.096</td>
<td>0.089</td>
<td>0.205**</td>
<td>0.207**</td>
</tr>
<tr>
<td></td>
<td>[0.067]</td>
<td>[0.075]</td>
<td>[0.177]</td>
<td>[0.103]</td>
<td>[0.101]</td>
</tr>
<tr>
<td>Before Wall</td>
<td>1.744***</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.152]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>WGTV × Before Wall</td>
<td>−0.098</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.151]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Through October 18</td>
<td>1.734***</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.283]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Late October</td>
<td>1.832***</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.218]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Early November</td>
<td>1.702***</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.197]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>WGTV × Through October 18</td>
<td>−0.124</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.305]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>WGTV × Late October</td>
<td>−0.056</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.222]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>WGTV × Early November</td>
<td>−0.131</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>[0.202]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Covariates</th>
<th>no</th>
<th>yes</th>
<th>yes</th>
<th>yes</th>
<th>yes</th>
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<tbody>
<tr>
<td>Frailties</td>
<td>no</td>
<td>no</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
</tbody>
</table>

Number of events: 2,598, 2,598, 2,598, 2,598, 2,598
Number of observations: 40,365, 40,365, 40,365, 40,365, 40,365
Log/I-likelihood: −9,077, −8,858, −8,746, −8,586, −8,589

Note: The table shows coefficient estimates from 5 Cox model specifications, with standard errors in brackets.

* significant at 10 percent level.
** significant at 5 percent level.
*** significant at 1 percent level.
Table 3: Effect of WGTV on probability of protest event from Cox models: Accounting for spatial dependence

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>WGTV</td>
<td>0.083</td>
<td>0.062</td>
<td>0.068</td>
<td>0.070</td>
<td>0.085</td>
</tr>
<tr>
<td></td>
<td>[0.175]</td>
<td>[0.173]</td>
<td>[0.177]</td>
<td>[0.170]</td>
<td>[0.176]</td>
</tr>
<tr>
<td>Spatial lag 1 day</td>
<td>yes</td>
<td>no</td>
<td>no</td>
<td>yes</td>
<td>no</td>
</tr>
<tr>
<td>Spatial lag 1 week</td>
<td>no</td>
<td>yes</td>
<td>no</td>
<td>yes</td>
<td>no</td>
</tr>
<tr>
<td>Spatial lag 2 weeks</td>
<td>no</td>
<td>no</td>
<td>yes</td>
<td>yes</td>
<td>no</td>
</tr>
<tr>
<td>Day of week dummies</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>yes</td>
<td></td>
</tr>
<tr>
<td>Covariates</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Frailties</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Number of events</td>
<td>2,597</td>
<td>2,596</td>
<td>2,595</td>
<td>2,595</td>
<td>2,598</td>
</tr>
<tr>
<td>Number of observations</td>
<td>40,158</td>
<td>38,916</td>
<td>37,474</td>
<td>37,474</td>
<td>40,365</td>
</tr>
<tr>
<td>I-likelihood</td>
<td>-8,731</td>
<td>-8,686</td>
<td>-8,719</td>
<td>-8,676</td>
<td>-8,649</td>
</tr>
</tbody>
</table>

Note: The table shows coefficient estimates from 5 Cox model specifications, with standard errors in brackets.

* significant at 10 percent level.

** significant at 5 percent level.

*** significant at 1 percent level.
Table 4: Effect of WGTV on probability of protest event from Cox models: Alternative measures of access to WGTV

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
</tr>
</thead>
<tbody>
<tr>
<td>WGTV (−85.0dBm)</td>
<td>0.034</td>
<td>[0.174]</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>WGTV (−82.5dBm)</td>
<td>−0.018</td>
<td>[0.174]</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>WGTV (−80.0dBm)</td>
<td>−0.089</td>
<td>[0.161]</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% WGTV (Dresden)</td>
<td></td>
<td></td>
<td>0.062</td>
<td>[0.229]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% WGTV (−85.0dBm)</td>
<td></td>
<td></td>
<td>0.039</td>
<td>[0.215]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% WGTV (−82.5dBm)</td>
<td></td>
<td></td>
<td>−0.013</td>
<td>[0.202]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% WGTV (−80.0dBm)</td>
<td></td>
<td></td>
<td>−0.043</td>
<td>[0.191]</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Kern (2011) WGTV</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>−0.078</td>
<td>[0.195]</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| Covariates | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Frailties  | yes | yes | yes | yes | yes | yes | yes | yes | yes |
| Number of events | 2,598 | 2,598 | 2,598 | 2,598 | 2,598 | 2,598 | 2,598 | 2,598 | 2,598 |
| Number of observations| 40,365 | 40,365 | 40,365 | 40,365 | 40,365 | 40,365 | 40,365 | 40,365 | 40,365 |
| I-likelihood | −8,746 | −8,746 | −8,746 | −8,746 | −8,746 | −8,746 | −8,746 | −8,746 | −8,746 |

Note: The table shows coefficient estimates from 8 Cox model specifications, with standard errors in brackets.

* significant at 10 percent level.
** significant at 5 percent level.
*** significant at 1 percent level.
Table 5: Effect of WGTV on probability of protest event from Cox models: Subsets of counties or events

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>WGTV</strong></td>
<td>0.074</td>
<td>0.027</td>
<td>0.159</td>
<td>0.020</td>
<td>0.0102</td>
<td>−0.502</td>
</tr>
<tr>
<td></td>
<td>[0.180]</td>
<td>[0.258]</td>
<td>[0.241]</td>
<td>[0.191]</td>
<td>[0.141]</td>
<td>[0.335]</td>
</tr>
<tr>
<td><strong>Omit East Berlin</strong></td>
<td>yes</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>no</td>
</tr>
<tr>
<td></td>
<td>[0.180]</td>
<td>[0.258]</td>
<td>[0.241]</td>
<td>[0.191]</td>
<td>[0.141]</td>
<td>[0.335]</td>
</tr>
<tr>
<td><strong>Omit Southeast</strong></td>
<td>no</td>
<td>yes</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>no</td>
</tr>
<tr>
<td><strong>Omit Northeast</strong></td>
<td>no</td>
<td>no</td>
<td>yes</td>
<td>no</td>
<td>no</td>
<td>no</td>
</tr>
<tr>
<td><strong>Omit neighbors</strong></td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>yes</td>
<td>yes</td>
<td>no</td>
</tr>
<tr>
<td><strong>Best subset</strong></td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>yes</td>
<td>no</td>
</tr>
<tr>
<td><strong>First events only</strong></td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>yes</td>
</tr>
<tr>
<td><strong>Covariates</strong></td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td><strong>Frailties</strong></td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td><strong>Number of events</strong></td>
<td>2,521</td>
<td>2,458</td>
<td>2,476</td>
<td>2,340</td>
<td>480</td>
<td>207</td>
</tr>
<tr>
<td><strong>Number of observations</strong></td>
<td>40,170</td>
<td>38,220</td>
<td>37,830</td>
<td>36,465</td>
<td>8,385</td>
<td>11,700</td>
</tr>
<tr>
<td><strong>I-likelihood</strong></td>
<td>−8,605</td>
<td>−8,168</td>
<td>−8,184</td>
<td>−7,596</td>
<td>−908</td>
<td>−847</td>
</tr>
</tbody>
</table>

Note: The table shows coefficient estimates from 6 Cox model specifications, with standard errors in brackets.
* significant at 10 percent level.
** significant at 5 percent level.
*** significant at 1 percent level.
VIII. Online Appendix

*** Not for publication. To be posted online. ***

Contents

(1) Discussion of Irregular Terrain Model
(2) Figure A: Counties with and without access to West German television (−85dBm cutoff level)
(3) Figure B: Counties with and without access to West German television (−82.5dBm cutoff level)
(4) Figure C: Counties with and without access to West German television (−80dBm cutoff level)
(5) Figure D: Estimated propensity scores for WGTV and non-WGTV counties
(6) Table A: Summary of balance measures across 100 best subsets
(7) Figure E: Treatment effect estimates for 100 best subsets
(8) Additional references
In what follows we describe both the software and the process used to create our WGTV treatment variable based on an electromagnetic signal propagation model.

We used ArcGIS Desktop 9.3 in conjunction with the Communication System Planning Tool (CSPT) extension for ArcGIS, created by the Institute for Telecommunication Sciences at the US Department of Commerce for the US Department of Defense. CSPT provides a set of functions for modeling the spatial propagation of electromagnetic signals based on terrain data in conjunction with information about the characteristics of broadcast transmitters and radio receivers. Similar commercial software is used by mobile phone providers, for example, to create coverage maps of their services.

We modeled the geographic coverage of all main ARD and ZDF broadcast transmitters (Grundnetzsender) with a power of at least 100 kW. Broadcast transmitter data were taken from Norddeutscher Rundfunk (1989), which lists the power, frequency, and polarization of all West German broadcast transmitters in operation at the beginning of 1989. Data on broadcast transmitter latitude, longitude, altitude, and height are from Wikipedia; all latitudes and longitudes were confirmed using Google Maps’ Satellite View. We were able to include 120 out of 124 existing main broadcast transmitters. The remaining 4 transmitters had to be omitted due to missing data on antenna height, an important variable in radio signal reception. We uniformly set the radio receiver height to 10 meters above ground, following common practice (DeBolt n.d.).

The CSPT extension uses the Longley-Rice Model (also known as the Irregular Terrain Model). This model “predicts the median attenuation of a radio signal as a function of distance and the variability of the signal in time and in space” (DeBolt n.d.: 76). Since its development in 1968, the Longley-Rice model has been regularly used to model the effect of topography on radio signal reception (DeBolt n.d.; Longley and Rice 1968; National Institute of Standards and Technology 2004: 36; National Telecommunications and Information Association n.d.; Seybold 2005: 143). The Longley-Rice model can account for a number of site characteristics such as climate zone type and ground conductivity (DeBolt n.d.; Longley and Rice 1968). We kept the majority of these characteristics at their default values (DeBolt n.d.) since they either do not affect coverage area predictions or do not apply to our specific model. We used the “Single/Multiple Transmitter Coverages” function of CSPT, which “run[s] propagation analyses on each transmitter individ-
ually and then combine[s] these individual coverages into a single composite coverage of all the transmitters in the scenario” (DeBolt n.d.: 60). This procedure generated a coverage map (see Figure 1 in the paper) displaying modeled available power for 1 km × 1 km cells covering all of East Germany. Modeled available power ranges from 8.97dBm to −117dBm. However, we are not primarily interested in variation in this continuous measure of WGTV signal strength across East German counties. More important for our purposes is a binary measure: could the residents of an East German county receive WGTV broadcasts at all? Survey data as well as detailed WGTV signal strength measurements on the ground confirm that people living in Dresden could not watch WGTV in 1989 (see Kern and Hainmueller 2009). Since we do not know the technical characteristics of the various television receivers in use throughout East Germany, we use the modeled average power in the center of Dresden (−86.5 dBm) as the cutoff for access to WGTV. Consequently, East German counties that have a modeled average WGTV signal strength below −86.5dBm are coded as not having access to WGTV; all other counties are coded as having access to WGTV (see Figure 2 in the paper). We code counties as receiving signal above the cutoff if more than 50% of a county’s area receives signal equal to or greater than the cutoff value. As a robustness check, we also use three other cutoff values, −85dBm, −82.5dBm, and −80dBm, with increasingly stricter cutoffs for considering a county to have WGTV. Maps showing WGTV and non-WGTV counties based on these cutoffs are included in this online appendix (Figures A, B, and C). Moreover, in our robustness analysis we also present results for the fraction of each counties’ area that receives signal above one of the four cutoff values.

\[\text{\textsuperscript{14}The same approach has been used in previous research (DellaVigna et al. 2013).}\]
Figure A: East German counties with and without access to West German television based on Longley-Rice radio signal propagation model and cutoff level of $-85$dBm
Figure B: East German counties with and without access to West German television based on Longley-Rice radio signal propagation model and cutoff level of $-82.5\text{dBm}$
Figure C: East German counties with and without access to West German television based on Longley-Rice radio signal propagation model and cutoff level of −80dBm
Figure D: Kernel density plots of estimated propensity scores for WGTV (blue) and non-WGTV (pink) counties with areas of overlap shown in violet.
Table A: Summary of balance across 100 best subsets

<table>
<thead>
<tr>
<th>Statistic</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Function</td>
<td>min</td>
<td>avg</td>
<td>max</td>
</tr>
<tr>
<td></td>
<td>min</td>
<td>avg</td>
<td>max</td>
</tr>
<tr>
<td>t</td>
<td>0.05</td>
<td>0.07</td>
<td>0.08</td>
</tr>
<tr>
<td>max diff</td>
<td>0.12</td>
<td>0.13</td>
<td>0.17</td>
</tr>
<tr>
<td>min KS</td>
<td>0.07</td>
<td>0.07</td>
<td>0.15</td>
</tr>
<tr>
<td>KS</td>
<td>0.15</td>
<td>0.69</td>
<td>0.70</td>
</tr>
</tbody>
</table>

Note: The table shows summaries of the distribution of balance measures across the 100 best subsets. The first three columns show the minimum, average, and maximum over all 100 subsets of the average absolute standardized difference in means across all 22 covariates. The next three columns show the minimum, average, and maximum over all 100 subsets of the maximum absolute standardized difference in means across all 22 covariates. The 100 best subsets are best in terms of this statistic out of all \( \binom{22}{100} \approx 548 \) million possible subsets. The next three columns show the minimum, average, and maximum over all 100 subsets of the minimum \( p \)-value from \( t \)-tests across all 22 covariates. The final three columns show the minimum, average, and maximum over all 100 subsets of the minimum \( p \)-value from bootstrapped Kolmogorov-Smirnov tests across all 22 covariates.
Figure E: Causal effect estimates for WGTV from baseline Cox model for 100 best subsets and 95% confidence intervals. Estimates are sorted in decreasing balance as measured by the maximum absolute standardized difference in means across all 22 covariates.


National Telecommunications and Information Association. n.d. *Irregular Terrain Model (overview).*
