Opium for the Masses: How Foreign Free Media Can Stabilize Authoritarian Regimes

Kern, Holger and Hainmueller, Jens

Harvard University

August 2007

Opium for the Masses: How Foreign Free Media Can Stabilize Authoritarian Regimes

Holger Lutz Kern — Cornell University and IQSS
Jens Hainmueller — Harvard University and IQSS

This version: April 2007
First version: August 2006

This is one of several joint papers by the authors; the ordering of names reflects a principle of rotation. Both authors contributed equally to this paper. Software to implement the estimation of local average response functions is available upon request. We thank Christopher Anderson, Jake Bowers, Alexis Diamond, Andy Eggers, Justin Grimmer, Dan Hopkins, Jan Lemnitzer, Walter R. Mebane, Jr., Na'ama Nagar, Beth Simmons, Hans-Jörg Stiehler, Christopher Way, Robert Weiner, and seminar participants at Harvard and MIT for very helpful comments. Brigitte Freudenberg at the Office of the Federal Commissioner for the Records of the National Security Service of the Former German Democratic Republic was of great help in tracking down archival material. Peter Bischoff, Evelyn Brislinger, Kurt Starke, and especially Hans-Jörg Stiehler patiently answered our questions about the surveys conducted by the Central Institute for Youth Research in Leipzig. The usual disclaimer applies.

Abstract

A common claim in the democratization literature is that foreign free media undermine authoritarian rule. No reliable micro-level evidence on this topic exists, however, since independent survey research is rarely possible in authoritarian regimes and self-selection into media consumption complicates causal inferences. In this case study of the impact of West German television on political attitudes in communist East Germany, we address these problems by making use of previously secret survey data and a natural experiment. While most East Germans were able to tune in to West German broadcasts, some of them were cut off from West German television due to East Germany's topography. We exploit this plausibly exogenous variation to estimate the impact of West German television on East Germans' political attitudes using instrumental variable estimators. Contrary to conventional wisdom, East Germans who watched West German television were *more* satisfied with life in East Germany and the communist regime. To explain this surprising finding, we demonstrate that West German television's role in transmitting political information not available in the state-controlled communist media was insignificant and that television primarily served as a means of entertainment for East Germans. Archival material on the reaction of the East German regime to the availability of West German television corroborates our argument.

I. Introduction

Do global flows of information promote democracy? The vaunted "information age," the advent of global communications, and the enormous increase in transboundary flows of information make this question theoretically as well as practically important. If the answer is yes, authoritarian regimes will face a stark choice between sealing themselves off hermetically from the rest of the globe similar to North Korea or facing their demise. In the words of Thomas Friedman, "the Internet and globalization are acting like nutcrackers who open societies and empower Arab democrats with new tools."[1] Friedman's view mirrors earlier statements by President Ronald Reagan, who, shortly before the end of the Cold War, predicted that "technology will make it increasingly difficult for the state to control the information its people receive ... The Goliath of totalitarianism will be brought down by the David of the microchip."[2] To many, the subsequent demise of communist regimes in Eastern Europe and the breakup of the Soviet Union, the Evil Empire itself, confirmed the truth of Reagan's prediction. The view that transnational flows of communication played a major role in the end of communism is widespread (Whitehead 1996; Rustow 1990; Huntington 1991).

In contrast to much of the literature on democratization, this essay will demonstrate that foreign free media actually helped stabilize one of the most oppressive communist regimes in Eastern Europe, the German Democratic Republic. Unhindered by barriers of culture or language, most East Germans had access to West German television after the regime in East Berlin decided to wall off the GDR from the rest of Germany in 1961. West German television was extremely popular. Yet in contrast to what theories of democratization predict, East Germans who tuned in to West German television became more, and not less, satisfied with the East German regime. The narcotizing effect of television (Lazarsfeld and Merton 1948) served to stabilize rather than to undermine communist rule.

Our paper is structured as follows. In the next section, we review the literature on the effects of foreign free media on autocratic stability and describe our research design. The

[1] *New York Times*, July 25, 2000.
[2] Speech at London's Guildhall, June 14, 1989.

third section spells out our statistical methodology and describes the data. The fourth section presents the main results. The fifth section shows that access to West German television was linked to the number of emigration applications, another indicator of political dissatisfaction. Based on research in the archives of the former East German secret police and communist party, the sixth section shows that the East German regime itself was aware of the stabilizing effects of West German television. Section seven presents several sensitivity analyses, showing that our results are fairly insensitive to hidden bias and robust when replicated using earlier survey data and a different sample. The last section contains our conclusion.

II. Literature Review

The early literature on democratization concluded that international factors played no more than a secondary role in transitions to democracy. It viewed the dynamics of authoritarian collapse mainly as a product of strategic elite interactions (O'Donnell, Schmitter, and Whitehead 1986). The more recent literature, however, has put greater emphasis on the international context (Pridham 1991, 1997; Whitehead 1996; Linz and Stepan 1996).

One of the factors that has attracted increased attention is the role of foreign mass media. In the midst of the Cold War, Ithiel de Sola Pool (1973) had argued that the hunger for trustworthy and credible political information made foreign broadcasts the most important source of information for the peoples of Eastern Europe. Through *Radio Liberty*, *Radio Free Europe*, *Deutsche Welle*, and the *BBC*, the West attempted to reach these audiences behind the Iron Curtain. Western broadcasting policies were driven by the hope that access to free, uncensored political information would foster a more pro-democratic and pro-Western public opinion, restrain communist militarism and adventurism, and erode communist rule in the long run (Presidential Study Commission on International Radio Broadcasting 1973; Quester 1990; Lisann 1975; Nelson 1997; Puddington 2000).

The view that Western mass media played a major role in the demise of communism is widespread. One can distinguish between two different versions of this argument. One strand of the democratization literature stresses the effects of foreign mass media at crucial

moments during the transition period. Such arguments focus on instances in which Western mass media covered crucial events that took place in communist countries, playing them back to domestic audiences behind the Iron Curtain. Whitehead (1996b) and Huntington (1991: 101) for example point out that Western reporting on the political developments in Hungary and East Germany led to contagion or "snowballing," quickly triggering upheavals in neighboring countries. When Hungary decided to remove all fortifications at the Austrian border, this became public knowledge in East Germany because of West German news broadcasts. Tens of thousands of East Germans spontaneously decided to make use of what to them seemed a unique opportunity to flee to the West, thus precipitating the breakdown of the East German regime (Rice and Zelikow 1999). Later that year, West German television's coverage of mass protests taking place in Leipzig showed that protesters could demonstrate without severe repercussions, encouraging East Germans all around the country to start similar demonstrations (Lohmann 1994). *Radio Free Europe*'s coverage of these events inspired people in Czechoslovakia to also demand political reforms in November 1989 (Schmitter 1996; O'Neil 1998b; Pridham 1997).

A second strand of the literature argues that decades of Western broadcasting had a long-term impact on the stability of communist rule. Western broadcasts sapped the strength of communist regimes by giving peoples behind the Iron Curtain hope and the assurance that the Free World had not forgotten them. They provided information not available in the state-controlled media, allowing them to compare communist propaganda with uncensored information from abroad. Western broadcasting covered topics that were suppressed in the domestic media, such as the existence of dissident movements, environmental disasters and economic problems, and communist countries' involvement in foreign wars (Diamond 1993a: 421–422; Diamond 1993b; Whitehead 1996b; Sükösd 2000; Bennett 1998; Buhl 1990; Rustow 1990; Roberts 1999).

East Germany seems to make an especially convincing case for the long-term effectiveness of Western broadcasting. For decades, people in most parts of East Germany had access to West German television, which they used extensively.[3] According to Quester

[3]For example, in a survey of East German youths done in 1985, respondents on average watched more

(1990: 128), the presence of West German television lead to a "constant threat of destabilization and ferment." Rustow (1990: 77) points out that West German television invited a "constant comparison" of life styles and standards of living, undermining the legitimacy of the communist regime. Roberts (1999: 33) also draws a direct connection between Western media and the demise of the regime in East Berlin: "the availability of West German television in East Germany influenced attitudes over a long period ... compelling a reluctant regime to initiate change." For Whitehead (1996b: 6), "popular attitudes in East Germany were so powerfully influenced by messages transmitted neutrally from the West that democratization became unavoidable, whatever governments or political leaders within or without might have wished or attempted." There exists, however, surprisingly little micro-level evidence to back up such claims.

Moreover, such arguments also ignore what we know about the effects of television in democratic societies. American television in particular has been accused of undermining democracy by equating news with entertainment and through inadequate coverage of substantive political issues. Its focus on ephemeral matters such as the personalities and foibles of politicians and current opinion poll standings, its characterization of elections as "horse races," and reporters' cynical attitudes towards politicians are all credited as causes of a general disillusionment with democracy itself (Ranney 1983; Entman 1989; Lang and Lang 1984; Robinson 1976; Patterson 1993). And even though the evidence for other democratic societies is weaker (Gunther and Mughan 2000; Newton 1999), it is certainly possible that West German television had similar "videomalaise" effects on East Germans. West German television's reporting about negative aspects of West German society, including mass unemployment and the so-called "new poverty" (*Neue Armut*), both of which repeatedly made headlines throughout the 1980s, might have led East Germans to appreciate the tranquility of their own society. Even if they could not buy many of the consumer goods or travel to the exotic locations shown on West German television, they at least did not have to worry about losing their jobs or homelessness (Hesse 1988: 118–122; Meyen 2003b:

than two hours of West German television each weekday, and presumably even more on the weekends (Zentralarchiv für Empirische Sozialforschung ZA 6073).

67–68; Quester 1990: 126–129).

Alternatively, East Germans might have used television, including West German programs, primarily as a source of entertainment. In many respects, East Germany was not a very exciting place. A lack of taverns, restaurants, and night and sports clubs contributed to domesticity and boosted the importance of television as the primary form of entertainment. More so than West Germans, East Germans enjoyed watching movies and soap operas after coming home from work (Früh and Stiehler 2002: 25; Meyen 2003a: 68–73). West German television offered a way out of the gloomy socialist reality (Buhl 1990). As one scholar has put it, East Germans "collectively emigrated to West Germany" every night in front of their television screens (Wolle 1998: 71). Especially Meyen (2001, 2003b: 37–42) suggests that West German television actually stabilized the regime by allowing East Germans to flee the dullness of communist life at least for a couple of hours each day. In this paper, we will test the more conventional view that West German television undermined the East German regime together with the "videomalaise" and entertainment hypotheses, both of which would predict that West German television had a positive effect on regime stability.

One problem that has hampered micro-level research on media effects in authoritarian settings is data availability. Authoritarian regimes rarely permit independent survey research, and when they themselves conduct opinion surveys (as in East Germany), the results are not made public. Thus, with rare exceptions (e.g., Geddes and Zaller 1989), micro-level data about public opinion in authoritarian regimes are simply not available. Researchers interested in Eastern Europe have attempted to address this problem by interviewing emigrees or visitors to Western Europe. Hesse (1988, 1990) for example interviewed refugees coming to West Germany. *Radio Liberty* and *Radio Free Europe* interviewed Eastern Europeans visiting the West to estimate the effects that Western broadcasting had on political attitudes in Eastern Europe and the Soviet Union (Parta 1986; McIntosh 1986). Nevertheless, such surveys do not address the problem of self-selection into media exposure. Soviet citizens who listened to *Radio Liberty* almost certainly had different political

attitudes than non-listeners to begin with, and to attribute differences in political attitudes between these two groups to exposure to the broadcasts of *Radio Liberty* is problematic.[4]

Here, we attempt to solve these problems by making use of formerly secret survey data collected by the Zentralinstitut für Jugendforschung (Central Institute for Youth Research) that became available to researchers after German re-unification. The Central Institute was founded in 1966 to offer scientific guidance to the communist leadership on questions of youth policy. The regime however showed very little interest in its research reports. Its critical findings aroused the hostility of parts of the communist bureaucracy, and during most of its existence, researchers affiliated with the Institute were not allowed to publish any of their research.

Between 1966 and 1990, the Institute conducted several hundred surveys among East German high school and college students, apprentices, and young workers. For each survey, questionnaires had to be approved by party officials, several Departments, and the Central Office for Statistics. Surveys that were representative of the whole East German population could not be conducted for political reasons, and the range of questions that could be asked was likewise restricted. Given these constraints, the Central Institute relied on cluster sampling of young adults in schools, universities, and state-owned companies. It also implemented several surveys without official approval and sometimes surreptitiously added questions after the questionnaires had been approved (Friedrich, Förster, and Starke 1999).

Researchers at the Institute were fully aware that respondents might be reluctant to participate in the survey and answer questions about their political attitudes. In order to convince participants that survey responses were anonymous, the researchers eschewed face-to-face interviews and always handed out questionnaires in groups (i.e., in high school classes or during college lectures). Visible to all respondents, completed questionnaires were collected in sealed urns (Friedrich 1990; Niemann 1995: 27–29).

It is hard to say whether these procedural safeguards fully reassured respondents, i.e., to what extent their answers were influenced by fears of prosecution. Nonetheless, the responses recorded in the surveys we use here are fairly critical of the East German regime

[4]The extent to which visitors or emigrees are representative of the larger population is another problem.

(cf. Table 2).[5]

Even if the existence of social desirability effects cannot be completely ruled out, they probably do not pose a problem for our research design since we do not attempt to establish the objective level of regime support among East German youths. Instead, we are concerned with the effect that exposure to West German television had on political attitudes. Unless social desirability effects systematically varied between respondents who watched West German television and respondents who did not, our estimates will not be biased.[6]

Even with the use of survey data collected by the Zentralinstitut, there still remains the problem of self-selection into television consumption. People tend to expose themselves to political messages they agree with, thus making it difficult to distinguish between association and causation. If consumers of West German television had more pro-Western views that other East Germans, it could be that exposure to West German television had an impact on their political attitudes, but it is equally plausible that people dissatisfied with the communist regime tuned in to Western programs more frequently. Recent research on media effects in the United States has creatively dealt with this self-selection problem by making use of natural experiments, i.e., situations in which some exogenous social process assigns respondents to different types or levels of media consumption (Della Vigna and Kaplan 2005; Gentzkow 2006; Mondak 1995). Here, we rely on a similar strategy to estimate the effect of West German television on political attitudes in East Germany.

Not all East Germans had access to West German television (Figure 1).[7] Especially the

[5]It is also worth noting that refusals to participate in these surveys were relatively rare (personal communication (February 2, 2007) by Hans-Jörg Stiehler, who conducted several of the Central Institute's surveys during the 1980s.)

[6]One might argue that West German television provided information about how dangerous East Germany's secret police truly was, thus increasing East Germans' reluctance to divulge their true opinions. East Germans however were well aware of the vast army of informers the secret police had recruited. They knew they had to be very careful about what they said to whom. It is hard to imagine that West German television provided any information about the repressiveness of the regime that East Germans were not already aware of.

[7]West German television refers to ARD and ZDF, the two primary German public broadcasting stations, and the regional programming they offer on separate channels. Commercial television (RTL, SAT 1) was introduced in Germany in the mid 1980s, but it did not broadcast to East Germany and could only be received in some areas close to the border and near West Berlin. ARD and ZDF have a public service orientation, similar to the British BBC, whereas commercial channels focus on movies, American television series, and infotainment.

Dresden district (*Bezirk*), located in the southeast of East Germany, was largely cut off from West German broadcasts due to its distance from the German-German border and its location in the Elbe valley. The same was true for the northeastern corner of East Germany around the town of Greifswald. Respondents who lived in the Dresden district were much less likely to watch West German television than respondents who lived in other parts of East Germany. As we will explain in the next section, this naturally occurring variation in access to West German television allows us to address the self-selection problem.

III. Methodology

A. Sample

The survey data we use in the main part of our paper were collected between November 1988 and February 1989, less than one year before the fall of the Berlin Wall in November 1989.[8] This survey is one of the very few surveys conducted by the Central Institute that contains the necessary information for estimating the effect of West German television on regime support (media consumption, respondents' place of work, baseline characteristics, and attitudinal outcome variables). The sample contains young adults (average age 23 years with a standard deviation of 6 years) drawn from high schools, universities, and firms located in the metropolitan parts of seven East German districts (East Berlin, Magdeburg, Cottbus, Leipzig, Erfurt, Karl-Marx-Stadt, and Dresden). The total number of respondents is $N = 3564$.

B. Causal Inference

Our quantity of interest is the effect of exposure to West German television on stated support for the East German regime among survey respondents. In order to identify this effect we code a binary variable D that takes the value 1 for respondents who watch West German television daily, multiple time per week, once per week, or less than once per week, and 0 for respondents who never watch West German television.[9] Borrowing

[8] Zentralarchiv für Empirische Sozialforschung ZA 6008.

[9] The survey question asked respondents: "How often do you watch West German television?" Answers are coded on a five point scale ranging from "daily" to "never." We dichotomize this variable to facilitate

from the literature on causal inference in statistics (Rubin 1974, 1978, 1990; Holland 1986; Rosenbaum 2002), we refer to this variable as our "treatment indicator" and the set of respondents that are exposed to West German television as our "treatment group."

We define Y_1 and Y_0 as the potential outcomes under treatment and control, i.e., the levels of stated regime support that a respondent would have had with and without exposure to West German television. For each respondent, the "treatment" effect of West German television is defined as the difference between these two potential outcomes $Y_1 - Y_0$. The "fundamental problem of causal inference" (Holland 1986) is that for each respondent, we never observe both potential outcomes, but only her realized outcome $Y = D \cdot Y_1 + (1-D) \cdot Y_0$. In other words, for a respondent exposed to West German television, we never get to observe her counterfactual level of regime support that she would have had in the absence of television. This missing data problem renders it impossible to consistently estimate the treatment effect for any particular individual. Under certain assumptions, however, we can estimate the average treatment effect $ATE = E[Y_1 - Y_0]$ or the average treatment effect for the treated $ATT = E[Y_1 - Y_0|D = 1]$.

In order to obtain an unbiased estimate of these parameters, we need to find a suitable "control group" of respondents that is sufficiently similar in all relevant characteristics to the treatment group but not exposed to West German television. Finding such a control group is complicated in observational studies, because selection into treatment is usually associated with the potential outcomes. For example, exposure to West German television might decrease the regime support of East Germans, but it is also quite plausible that staunch communists do not watch West German television. Therefore, a naive comparison of the regime support of East Germans exposed to West German television and East Germans not exposed to West German television will generally not be informative about the effects of media consumption. We could try to make such a comparison more plausible by controlling for observable characteristics using a method of covariate adjustment (re-

the presentation of the results and to retain a clear causal interpretation of the quantity of interest. Results are substantively similar if we code as 0 respondents who watch West German television never or less than once per week or if we use the full five category scale and impose linearity.

gression or matching, for example). Such an approach would still be problematic, however, since we cannot rule out the possibility that people select into media consumption based on unobservable characteristics which we cannot control for. Instead, we will address the selection problem through the design of our study by making use of a natural experiment.

C. Causal Inference using Instrumental Variables

Instrumental variable (IV) methods are often used to recover treatment effect parameters in the presence of selection on unobservables (Heckman and Robb 1985; Imbens and Angrist 1994; Angrist, Imbens, and Rubin 1996; Heckman and Vytlacil 1999; Manski and Pepper 2000; Abadie 2003). We follow Imbens and Angrist's (1994) approach and conceptualize IV identification based on potential treatment indicators. Based on information about respondents' place of work, we code a binary instrument Z that takes the values 1 if a respondent lives in the Dresden district and 0 otherwise. The role of the instrument is to induce exogenous variation in the treatment variable. Consider the ideal experiment we would like to approximate with our observational data: We can think of respondents in the Dresden district as being "assigned" to the control group that (according to the experimental protocol) is supposed to be unexposed to West German television. Similarly, respondents living in the other districts are "assigned" to the treatment group that is supposed to be exposed to West German television.

Imbens and Angrist's (1994) approach mirrors this dependence between the treatment and the instrument by utilizing potential treatment indicators. Let D_z represent potential treatment status given $Z = z$. Then $D_0 = 0$ and $D_1 = 1$ for a particular respondent means that such an individual would not watch West German television if he or she lived in the Dresden district, but would watch West German television if living somewhere else. The treatment status indicator variable can then be expressed as $D = Z \cdot D_1 + (1 - Z) \cdot D_0$. Similar to the missing data problem for potential outcomes, we only observe Z and D (and therefore D_z for individuals with $Z = z$), but never both potential treatment indicators for the same individual. Following the terminology of Angrist, Imbens and Rubin (1996), we can now define:

- *Compliers*: $D_1 > D_0$ ($D_0 = 0$ and $D_1 = 1$). These are the individuals that follow the experimental protocol. They take the treatment when assigned to the treatment group, but do not take the treatment when assigned to the control group. In our case, compliers are East Germans who watch West German television if they do not live in the Dresden district but do not watch West German television if they live in the Dresden district.

- *Always-takers*: $D_1 = D_0 = 1$. These are the individuals that always take the treatment, regardless of whether they are assigned to the treatment or the control group (i.e., East Germans who always watch West German television).

- *Never-takers*: $D_1 = D_0 = 0$. Similarly, these are the individuals that never take the treatment, regardless of whether they are assigned to the treatment or the control group (i.e., East Germans who never watch West German television).

- *Defiers*: $D_1 < D_0$ ($D_0 = 1$ and $D_1 = 0$). These are the individuals that always violate the experimental protocol. They take the treatment when assigned to the control group and vice versa. In our case, defiers are individuals who watch West German television if they live in the Dresden district but do not watch West German television if they live anywhere else.

Since we only observe one of the potential treatment indicators (D_0, D_1), we cannot directly identify which group any particular individual belongs to.

C.1. Identification Assumptions

To fully spell out all the properties of a valid instrument we include Z in our definition of potential outcomes. Let Y_{zd} represent the potential outcome that an individual would obtain if $Z = z$ and $D = d$. For example, Y_{10} denotes the stated regime support of an individual not living in the Dresden district and not watching West German television. Clearly, if $D_1 = 1$ for such an individual, we will not be able to observe Y_{10}. Following Abadie (2003), this leads to the following nonparametric assumptions under which instru-

mental variables techniques can be used to identify treatment parameters. X represents a vector of predetermined control variables.

i. Independence of the Instrument: Conditional on X, the random vector $(Y_{00}, Y_{01}, Y_{10}, Y_{11}, D_0, D_1)$ is independent of Z.

ii. Exclusion of the Instrument: $P(Y_{1d} = Y_{0d}|X) = 1$ for $D \in \{0, 1\}$

iii. First Stage: $0 < P(Z = 1|X) < 1$ and $P(D_1 = 1|X) > P(D_0 = 1|X)$.

iv. Monotonicity: $P(D_1 \geq D_0|X) = 1$.

To what extent are these identification assumptions plausible in our application? Assumption (iv) rules out the existence of defiers and defines the partition of the population into always-takers, compliers, and never-takers. In our context, the monotonicity assumption seems unproblematic since $D_0 = 0$: most individuals living in the Dresden district simply did not have access to West German television. Substantively, it also seems implausible that there were East Germans that would have watched West German television if living in the Dresden district but who would have chosen not to do so if living anywhere else.

Assumption (iii) is also innocuous. First stage guarantees that Z and D are correlated (conditional on X).[10] Given that the residents of the Dresden district were largely cut off from West German television, not living in Dresden is highly correlated with exposure to West German television. Table 1 displays the frequency of West German television consumption in our sample. As one would expect, respondents living in the Dresden district are much less likely to be exposed to West German television than East Germans in other districts. For example, while only 10 percent of the respondents living in the Dresden district watch West German television daily or multiple times per week, 91 percent of the respondent in the other districts do.[11] The sample correlation between living in the Dresden

[10] In addition, this assumption implies that the support of X conditional on $Z = 1$ coincides with the support of X conditional on $Z = 0$.

[11] Note that there are a few respondents in the Dresden district who watch West German television on a regular basis. The reason for this is topographical variation *within* the Dresden district. We exploit this variation in a separate section below.

district (Z) and consumption of West German television (D) is 0.74. When we regress Z on D while controlling for our extensive set of covariates, the t-statistic on the West German television indicator is about 60, which indicates very strong first-stage identification.

Assumptions (i) and (ii) are more problematic. Assumption (i) states that Z is "as good as randomly assigned" once we condition on X; this assumption is often called *ignorability*.[12] Assumption (ii) implies that variation in the instrument does not change the potential outcomes other than through D. It allows us to define potential outcomes in terms of D alone, so that we have $Y_0 = Y_{00} = Y_{10}$ and $Y_1 = Y_{01} = Y_{11}$. Taken together, these assumptions guarantee that conditional on X, the only effect of the instrument on the outcome is through variation in treatment status. We assume that once we control for covariates, living in the Dresden district did not directly affect the political attitudes of East Germans.

Evidently, assumption (i) might only hold after we condition on a set of predetermined covariates X. Our research design enables us to control for confounding factors at two levels. Below, we show that the district of Dresden was very similar to other East German districts. In this sense, aggregate-level differences are controlled for "by design." In addition, our survey data contain plenty of information on individual-level characteristics that we can directly control for by including them in the estimations.

C.2. Threats to Ignorability I: District Comparison

Our identification assumptions would be called into question if the Dresden district was fundamentally different from the other East German districts in our sample. Living conditions in the Dresden district for example could have been worse than in other parts of East Germany, and therefore respondents in Dresden expressed greater dissatisfaction with life in East Germany independent of exposure to West German television.

Figure 2 shows that this possibility is rejected by the available data. The first three

[12]Note that an identification assumption involving some sort of randomization is always necessary for causal inference. For example, if we were to use matching or regression instead of instrumental variables, we would have to assume that whether respondents watched West German television or not was "as good as randomly" assigned, conditional on the included covariates.

rows compare socio-economic indicators for the districts in our sample for the year 1988, taken from East German statistical abstracts (Staatliche Zentralverwaltung für Statistik 1989). The circles denote robust standardized differences from the medians in our sample (the dashed vertical lines). Per capita consumption expenditures, divorce rates, crime rates, the percentage of children that attend kindergarten, and household sizes in Dresden closely resemble the East German average. The same is true for the number of telephone extensions and the housing space per capita, both important indicators of well-being in a country in which people had to wait several years before they were allocated a telephone extension or a new apartment. If there are differences at all, Dresdeners seem to have been a little better off than people in most other districts. The proportions of the working population employed in industry, agriculture, manufacturing, and services also do not show any significant imbalances; the Dresden district is always very close to the median line. Only the working-age population is slightly lower in Dresden than in other districts due to a relatively high percentage of retirees (numbers not shown), but it is hard to imagine how this could bias our results, especially since we are looking at a survey of young adults. Also note that East Berlin is an outlier on several dimensions. As we will see later, its exclusion does not affect our results.

What about potential differences in political attitudes *before* West German television ever became available in East Germany? The last row of Figure 2 compares turnout and vote shares for the Christian Conservatives (CDU), Liberals (LDP), and the Socialist Unity Party (SED) in the 1946 state elections in the German territories then occupied by the Soviet Union.[13] These elections were the first state elections to take place after the collapse of the Third Reich. Although the SED was massively supported by the Soviet occupation forces and the LDP and CDU were disadvantaged in many ways, the 1946 state elections were still reasonably free (Hajna 2000; Schmitt 1993). As we can see, support for the SED was not much different in the Dresden district. If at all, people in Dresden

[13]Data are taken from Broszat and Weber (1993), adjusted for redistricting, and weighted by population size. East Berlin has been excluded since West German parties ran their own candidates in East Berlin, which led to very different election results compared to the rest of East Germany.

supported the SED to a slightly larger extent than the people living in some of the other districts in our sample. The same applies to turnout and vote shares for the CDU and the LDP. There is no evidence of long-standing regional differences in political cultures and attitudes predating the rise of television that could bias our results. Of course, it would be preferable to have measures of political attitudes that are closer in time to our surveys (but not so close as to be potentially affected by television), but since the 1946 elections were the only free elections in East Germany, this is not possible.

C.3. Threats to Ignorability II: Spatial Mobility

Another threat to the validity of our instrument could be high spatial mobility among respondents. If individuals critical of the communist regime and keen on watching German television tended to move away from Dresden, the validity of our instrument would be compromised, especially since the desire to move is unobserved. The demographic controls that we will add to our extensive specification below might serve as a proxy for the desire to move, but some doubts remain. It turns out, however, that the number of individuals who moved to another county (*Kreis*) in 1988 was not higher in Dresden than in other districts (15.6 per 1,000 residents) (Staatliche Zentralverwaltung für Statistik 1989). If we assume for simplicity that this rate was constant over time, each East German moved across county borders once every 64 years, or about once per lifetime (Grundmann and Schmidt 1988).

Spatial mobility was low in the GDR in general. In East Germany's centrally planned economy, factor allocation was controlled by the state. Mobility of labor between occupations and firms was not desired since it interfered with central planning and increased costs. Across-firm labor mobility, and also spatial mobility, was therefore only about one third to one half as high as in West Germany (Grünert, Bernien, and Lutz 1997; Grünert 1996; Uunk, Mach, and Mayer 2005). Lack of housing restricted spatial mobility further (Wolle 1998: 182–88). Housing was centrally allocated and waiting lists were long. It could take several years, for example, before a newly-wed couple would be able to move in together. We therefore think it unlikely that spatial mobility could invalidate our instrument.

D. Covariates

While the use of a natural experiment lessens the possibility of confounding, some imbalances between the treatment and the control group might remain. Fortunately, the survey provides a relatively rich set of control variables that allow us to relax the ignorability assumption further.

In doing so, we face the usual tradeoff involved in the choice of covariates (Rosenbaum 2002: 76). One the one hand, we want to avoid post-treatment bias. This means that we should only adjust for variables that are measured before the instrument was assigned or for which we can be relatively certain that they are unaffected by the setting of the instrument and the treatment (Cox 1958: 4.2; Rosenbaum 1984). Since we only have cross-sectional data at our disposal, it is not immediately obvious which covariates qualify. Age, gender, and father's and mother's occupational qualification[14] are the only variables in the survey that are clearly causally prior to television consumption. We call this set of variables the limited covariate set.

On the other hand, we want to maximize the credibility of the ignorability assumption and therefore also present results for an extensive set of covariates. This set includes the variables from the limited set plus respondent's marriage status[15], living situation[16], number of children[17], highest educational attainment[18], occupational classification[19], net monthly income[20], and employment status.[21] For some of these covariates it is less clear that they are unaffected by the setting of the instrument or the treatment. As we will see, our results are virtually identical no matter which covariate set we use. Except for age (which is discretized into eight age categories), all covariates are fully factorized to avoid

[14]Occupational qualification contains 7 categories going from no occupational qualification to post-graduate degree.

[15]Four categories: single, married, divorced, and widowed.

[16]Three categories: living with partner, others, or alone.

[17]Five categories: none, one,..., more than three.

[18]Four categories: less than 8th grade, 8th grade, 10th grade, and 12th grade.

[19]Ten categories from no qualification, currently in vocational training, currently in college, etc. to post graduate degree.

[20]Nine categories ranging from 500 Mark to more than 1500 Mark.

[21]Four categories: full-time employed, part-time employed, unemployed, and still in training.

functional form assumptions. Missing data are treated as additional categories.

E. Instrumental Variable Estimators

In the absence of covariates, the most commonly used instrumental variable estimator is the Wald estimator which, as Imbens and Angrist (1994) show, identifies the local average treatment effect for compliers (LATE):

$$\alpha_{LATE} = \frac{cov(Y,Z)}{cov(D,Z)} = \frac{E[Y|Z=1] - E[Y|Z=0]}{E[D|Z=1] - E[D|Z=0]} = E[Y_1 - Y_0|D_1 > D_0] \tag{1}$$

Recall that compliers form the subpopulation composed of individuals whose treatment status is exogenously manipulated by the instrument. Specifically, when $D_0 = 0$ for most individuals, as is the case here, the IV estimator approximately identifies the effect of the treatment on the treated. In order to introduce confounders into the instrumental variable analysis, we estimate a two stage least squares (2SLS) model:

$$Y = \mu + \alpha D + X'\beta + \varepsilon, \tag{2}$$

where $E[\varepsilon|X,Z] = 0$. In this setup, α_{2SLS} and β_{2SLS} are estimated by regressing Y on D and X while using Z as an instrument.

In the absence of covariates, the 2SLS estimator reduces to the Wald estimator and thus identifies LATE. Once we condition on X, however, we have to assume a constant treatment effect in order to retain this clear causal interpretation of the 2SLS estimator (Abadie 2003).[22] Thus, despite its popularity the 2SLS estimator is somewhat restrictive. The constant treatment effect assumption may be implausible, as it requires that the treatment has the same effect for individuals with the same X characteristics. In particular, the effect of the treatment for compliers might differ from the treatment effect for the rest of the population.

In order to allow for varying treatment effects, we also implement a new class of instrumental variable estimators called local average response function for compliers (LARF),

[22]The reason is that in the case of 2SLS with covariates, the whole population contributes to variation in X, and so the estimand does not only respond to the distribution of (Y, D, X) for compliers.

which has recently been proposed by Abadie (2003). The semi-parametric LARF estimator allows for the identification of LATE conditional on X without restricting the treatment effect to be constant or to be a deterministic function of the covariates. Since this estimator has not been used in the political science literature so far, we describe the technical details in an appendix at the end of the paper. The interpretation of LARF estimates is straightforward, however. They identify the local average treatment effect for the subpopulation of compliers without imposing a constant treatment effect assumption while still allowing for the inclusion of covariates.

F. Outcome Variables

The survey includes several questions that measure support for the East German regime. Here we focus on the three questions that appear to be most relevant.[23] The three questions are worded as follows:

To what extend do you agree with the following statement:

- Q1: *I am convinced of the Leninist/Marxist worldview*[24]
- Q2: *I feel closely attached to the GDR*[25]
- Q3: *In the GDR, power is exercised in ways consistent with my views*[26]

Answer categories are *fully agree (vollkommen)*, *largely agree (mit Einschränkungen)*, *largely disagree (kaum)*, *fully disagree (überhaupt nicht)*. For each of these questions we reversed the coding so that higher numbers indicate stronger regime support.

Descriptive statistics for the three outcome variables are displayed in Table 2. All three variables are highly positively correlated, which indicates that they tap a similar underlying dimension of support for the East German regime.

[23] Results are substantively identical when other questions are used instead.
[24] German question: *"Ich bin von der marxistisch/leninistischen Weltanschauung überzeugt."*
[25] German question: *"Ich fühle mich mit der DDR eng verbunden."*
[26] German question: *"In der DDR wird die Macht in meinem Sinne ausgeübt."*

G. *Variance Estimation*

Standard errors are adjusted for the potential non-independence of observations within clusters. Clusters are the schools, firms, and universities where respondents were interviewed. There are 169 clusters in our sample. For the 2SLS models, potential within-cluster correlation is accounted for by the commonly used Eicker-Huber-White sandwich estimator. For the LARF estimator no closed-form solution for a robust variance estimator exists. Instead, we rely on a cluster-adjusted bootstrap, with clusters randomly drawn with replacement (Davidson and Hinkley 1997: 101-103; Feng, McLerran, and Grizzle 1996). 2000 bootstrap samples are used for standard errors and 15,000 bootstrap samples for confidence intervals.

IV. Results

A. *The Effect of West German Television on Regime Support*

Table 3 presents our main findings. It displays estimates of the effect of watching West German television obtained from several estimators. The first column refers to the naive difference in means. These results suggest that East German citizens who watched West German television were not significantly more critical of the communist regime. These estimates however are almost certainly confounded by selection bias. They tell us little about the effect of West German television on regime support among East Germans.

Selection bias is accounted for in column 2, which shows LATE estimates obtained from the Wald estimator. Recall that LATE represents the average causal effect of watching West German television for the subgroup of compliers for which the decision to watch West German television is exogenously manipulated by whether they live in the Dresden district or not. Once we exploit the exogenous variation in access to West German television, we find that consumption of West German television makes respondents more supportive of the East German regime. This effect is similar for all three outcome variables and significant in both statistical and substantive terms with magnitudes of about $0.15 - 0.20$ on the 4 point answer scale and p-values smaller than 0.1.

Column 3 displays 2SLS estimates (equation 2) controlling for our limited covariate set.

The effect of West German television remains robust with magnitudes between $0.20 - 0.30$ and similar precision. Considered at the mean level of regime support of around $2.7 - 3.0$, this constitutes an increase of about $7 - 11$ percent. Column 4 shows estimates of the local average response function for the same covariate set. A saturated probit model is used in the first stage to compute $\widehat{\kappa}$ (see equation 6 in the appendix for details). 2SLS and LARF estimates are very similar, suggesting that the assumption of a constant treatment effect is not critical in our application.

The last two columns show estimates of the effect of West German television for specifications in which we include the extensive covariate set. For all three responses, the positive effect of West German television is remarkably robust to the inclusion of an extensive list of additional covariates. The 2SLS and LARF estimates range around $0.20 - 0.25$ with equally strong evidence against the null of no effect.

One might argue that the results presented thus far rest upon an unwarranted linearity assumption imposed on the four answer categories of our outcome variables. Table 4 presents LARF estimates based on an ordered Probit link function (see appendix for details) that takes the ordered categorical nature of the outcome variables into account. We display simulated first differences instead of hard to interpret ordered Probit coefficient estimates. Here the complier treatment effect is the average change in the predicted probability of choosing each of the four answer categories under treatment and control.[27]

The assumption that our response categories are equally spaced turns out to be inconsequential. Ordered probit results are very similar to the results from the linear models shown in Table 3. For all three response variables, consumption of West German television on average increases (decreases) the probability of stating complete support for the communist regime (complete opposition to the communist regime) by about $0.6 - 0.13$ $(0.03 - 0.04)$. Consumption of West German television also increases (decreases) the predicted probability of largely agreeing (largely disagreeing) with the regime.

[27]First differences are computed by simulation. Model-based imputation is used to impute missing potential outcomes under treatment and control, with covariates held at their observed values and compliers identified by our $\widehat{\kappa}$ estimates.

B. Causal Pathways: Information, Videomalaise, and Entertainment

Our results so far demonstrate that consumption of West German television increased support for the East German regime. We also have more direct evidence on the causal mechanisms responsible for this effect, which will allow us to discriminate between the information, "videomalaise," and entertainment hypotheses.

Recall that the information hypothesis implied that West German television provided East Germans with critical political information that they could not obtain from the state-controlled East German media. According to this hypothesis, exposure to West German television made East Germans more critical of the communist regime, but our findings so far suggest the opposite. To further test the information hypothesis we examine whether respondents who watched West German television were more critical of East German news reporting. A subset of respondents was asked to what extend they considered themselves well informed by East German media about recent policy changes in the Soviet Union (first row of Table 5). Answers are coded on a five point scale ranging from 5 (fully) to 1 (not at all) informed.[28] At the time of the survey, West German media reported very favorably about *glasnost* and *perestroika*. East German media, in contrast, paid scant attention to the political reforms in the Soviet Union. Interestingly, exposure to West German television seems to have had a positive (although not very precisely estimated) effect on satisfaction with East German news reporting. According to the informational hypothesis, East Germans exposed to West German television should have been in a much better position to realize how slanted and ideologically driven East German media were, but this seems not to have been the case. Even if we regard the statistically insignificant effect as a null finding, the results are hard to square with the hypothesis that East Germans tuned in to West German television because it provided them with political information they could not get from the state-controlled domestic media. The best way to read this finding might be along the lines proposed by Meyen (2003b), which argued that most East

[28]German question: *"Geben Sie bitte an, wie gut Sie sich durch Presse, Funk und Fernsehen der DDR über den Prozess der Umgestaltung in der Sowjetunion informiert fühlen."* Answer categories ranged from *vollkommen* to *überhaupt nicht*.

Germans trusted neither East German nor West German news reporting, but believed the truth to be somewhere in the middle.

The same respondents were asked how attached they felt to West Germany (second row of Table 5). Answers are coded on the same five point scale.[29] While this question is not optimal, it might still allow us to test for the existence of "videomalaise" effects. If the "videomalaise" hypothesis is correct and West German news reporting about the dark side of capitalist society scared East Germans, exposure to West German television should have decreased attachment to West Germany. This is indeed what we observe, although the effect is not very large and not very precisely estimated. On average, consumption of West German television decreased expressed attachment to West Germany by about .20 on the five point scale. Although this finding is consistent with the "videomalaise" hypothesis, it would be desirable to have more detailed survey responses about specific aspects of West German society instead of this rather general question. Political constraints, however, prevented the Youth Institute from asking such questions.

The entertainment hypothesis receives the strongest support from the data. Respondents were also asked about their satisfaction with their options for recreational activities. Answers are again coded on a five point scale.[30] If West German television was mainly a means for entertainment, then exposure to West German television should have increased their level of satisfaction. As we can see in the third row of Table 5, exposure to West German television has a large positive and precisely estimated effect. The effect is twice as large as the effect we found on videomalaise, suggesting that even if videomalaise can account for parts of our overall result, the primary causal mechanism responsible for the positive effect of West German television exposure on regime support is entertainment. According to our estimates, exposure to West German television on average increases satisfaction with recreational activities by about 0.40 on a five point scale. At the mean level of satisfaction of 3.05 in our sample, this equals an increase of about 13 percent.

This result fits very well with other research on the use of television in East Germany.

[29] German question: *"Inwieweit fühlen Sie sich mit dem folgenden verbunden? Der BRD."*
[30] German question: *"Wie zufrieden sind Sie mit Ihren Möglichkeiten zur Freizeitgestaltung?"*

From detailed data on East Germans' television viewing habits, we know that they watched television, both Eastern and Western programs, primarily for its entertainment programming. Moreover, when asked what they would like to see changed about East German television programming, they demanded more entertainment and less politics (Meyen 2001, 2003a). In 1983, East German television responded to these demands by copying Western presentation styles and importing Western movies, but it was not particularly successful in competing with the more glamorous, more entertaining West German channels (Braumann 1994).

We also know that even after unification, East Germans' television viewing habits differ from those of West Germans, even though everyone now has access to the same channels. East Germans have a stronger preference for entertainment programming than West Germans. They also tune in more often to commercial channels such as SAT 1, RTL, and Pro 7. As is the case in other countries, commercial television in Germany is more entertainment-oriented than public broadcasting television and puts more emphasis on infotainment, movies, and soap operas (Früh and Stiehler 2002; Darschin and Zubayr 2000).

The available evidence thus suggests that East Germans watched West German television primarily because of the entertainment it provided. East Germans who watched West German television were happier with their lives and the political regime, simply because exposure to West German television meant a noticeable improvement in their living standards. West German television was one of the few ways in which they could escape from the bleak socialist reality at least for a couple of hours each day. Instead of fostering resistance to the communist dictatorship, the narcotizing effects of television (Lazarsfeld and Merton 1948) helped stabilize the regime.

V. Within-Dresden Variation and Emigration Applications

Is it possible to confirm these results with other indicators of regime support? A very direct measure of political dissatisfaction is the number of East Germans who applied to emigrate. The decision to file an emigration application was very costly and can thus serve as a credible signal of dissatisfaction with the communist regime. Applicants often faced

consequences such as being fired from their jobs, informal discrimination and mobbing at their workplaces, confiscation of their passports, and even criminal persecution (Mayer 2002: 177–181).

Applications to emigrate were indeed far more common in Dresden than in other districts. For the districts in our sample, Table 6 shows the number of East Germans (per 1,000 residents) who filed a new emigration application in 1988. The number of applicants in the Dresden district was more than twice as high as the average.[31]

While these figures are suggestive, it is possible to examine this issue using more disaggregate data. The Dresden district was divided into 17 counties, including the city of Dresden itself with about 500,000 residents, several counties with about $80,000 - 120,000$ residents each[32], and smaller counties with up to $80,000$ residents.[33,34]

In the mid-1980s, the Department of Postal Services, responsible for radio and television broadcasting in East Germany, conducted an investigation into the availability of West German television in the Dresden district.[35] The resulting map, submitted to the Politburo as part of a larger collection of material on West German television in East Germany, is displayed in Figure 3.

The lines that run through the map delineate counties with varying levels of access to West German television. Counties in the white parts of the map had no access to West German television. In areas shaded in light grey, residents had limited access to West German television, depending on weather conditions and exact location. In counties shaded in dark grey, residents could tune in to West German programs.

We were also able to obtain county-level data on applications to emigrate for 1985, 1986, and 1987, adjusted for population size.[36] If West German television had a positive

[31]Office of the Federal Commissioner for the Records of the National Security Service of the Former German Democratic Republic (hereafter BStU) MfS-ZKG 10734. A similar pattern existed throughout the 1980s (figures not shown).

[32]Zittau, Löbau, Riesa, Görlitz, Pirna, Meissen, Dresden Land, and Bautzen.

[33]Niesky, Grossenhain, Dipoldswalde, Sebnitz, Kamenz, Bischofswerda, and Freital.

[34]The Dresden district was comparable in size to the US state of Delaware.

[35]Federal Archive, Foundation for the Archive of the Parties and Mass Organizations of the GDR (hereafter BArch SAPMO) DY 30/J IV2/2/2317.

[36]BStU MfS BV Dresden Stellvert. OP 21; BStU MfS BV Dresden BKG-8028/8029.

effect on political satisfaction, we would expect the number of applicants to be inversely related to the extent to which residents had access to West German television. For each year, Figure 4 plots the distribution of the number of applicants for counties with access (dark grey), with partial access (light grey), and with no access to West German television.

Figure 4 shows a clear dose-response relationship for these data. In all three years, the number of applicants was highest in counties without access to West German television and lowest in counties with access. The number of applicants in counties with partial access was always in between. These differences are large in substantive terms, with the number of applicants (per 1,000 residents) in counties without access to West German television being more than twice as high as in counties with access to West German television. There is no obvious confounding factor that would account for such a pronounced difference.[37]

Randomization inference is a more formal way to assess the relationship between access to West German television and the number of applicants. Our sharp null hypothesis posits that the number of applicants is independent of access to West German television. Randomization inference randomly re-assigns counties to the "access" and "no access" groups and computes the difference in the mean number of applicants.[38] Repeating this procedure for all possible permutations of group membership provides us with a distribution of "placebo" differences that would be observed under all possible assignments of counties to the two groups.

Table 7 shows randomization inference results for two test statistics, the difference in means and the difference in average ranks for both groups of counties. The first (second) row shows results from specifications in which two ambiguous counties (Bautzen and Bischofswerda) are coded as having access (no access) to West German television. Across all models, we reject the sharp null hypothesis of no effect. This suggests that the observed difference in the per capita number of applicants is very unlikely to have arisen by chance. Although this result is certainly open to the challenge of hidden bias, it complements our

[37] We were unsuccessful in locating county-level covariates. Results do not change if we exclude the city of Dresden.

[38] We have collapsed the partial access and full access groups into one group for this test.

earlier micro-level analysis with a different measure of political dissatisfaction.[39]

Table 8 shows vote shares in the 1946 state elections for the counties in the Dresden district. We can see that across access categories, vote shares for the SED were almost identical. This provides at least some evidence that these counties were not fundamentally different from each other after World War II.

VI. Historical evidence

Another source of evidence that we can exploit is archival material on the reaction of the East German regime to the availability of West German television broadcasts. If our conclusions are correct and West German television did indeed stabilize the communist regime, East German authorities should have recognized as much.

Already in the 1960s, a large majority of East Germans, including many Communist Party members, tuned in to West German television programs.[40] While it was never illegal (except for members of the armed forces and the police) to watch West German television, the East German regime tried to suppress the popularity of West German television with campaigns of propaganda and harassment throughout the 1960s and 1970s. It considered West German television a source of ideological destabilization just like Western print media and attempted to restrict access as far as possible.[41] After the construction of the Berlin Wall in 1961, for example, Communist party youth squads tore down hundreds of roof antennas directed towards West Germany. But the extreme unpopularity of such measures assured that they were quickly abandoned (Holzweissig 2002: 49–65; Stiehler 2001: 13–16; Wolle 1998: 69–71).

The attitude of the East German regime eventually changed. It had no choice but to accept that a large majority (85–90%) of East Germans regularly tuned in to West German

[39] In order to investigate the sensitivity of this result to hidden bias, we conducted Rosenbaum's sensitivity test for unmatched groups based on the exact version of Wilcoxon's rank sum statistic (Rosenbaum 2002: 140–147). The results indicate that our finding is insensitive to small hidden biases, but sensitive to hidden biases of moderate size. More precisely, an unobserved confounder that tripled a county's odds of having access to West German television could explain the lower number of applicants in counties with access to West German television. The upper bounds on our p-values for $\Gamma = 2$, 3, and 4 are 0.046, 0.103, and 0.146.

[40] BArch SAPMO 30/IVA2/902/68.

[41] BArch SAPMO 30/IVA2/902/68.

television programs. Editorials in East German newspapers, for example, simply assumed that everyone would understand what was meant by references to the latest "capitalist agitation" on West German television. Throughout the 1980s, the East German regime also came to realize that access to West German television actually helped to keep its population content and docile (Windelen 1985; Wolle 1998: 69–71; Meyen 2001).

Around 1984, 40% of East German households were connected to community antennas (CATVs), large antenna systems connected to individual households via cable. CATVs often carried West German programs.[42] East Germans not connected to CATVs relied on smaller individual antennas to receive West German television. Depending on their specific place of residence, however, reception was often poor or impossible. In such areas, residents increasingly complained about their inability to watch "international programs," as West German television was euphemistically called, and demanded action from the state.[43] State-owned housing companies sometimes paid for the installation of CATVs, thus providing their tenants with access to West German television.[44] In other cases, East Germans joined forces in grass-roots organizations and paid for the installation of CATVs themselves. While East German authorities were suspicious of such grass-roots activities and closely monitored them, they did nothing to prevent them (Stiehler 2001: 95–96).

Since the mid-1980s, the regime was also increasingly confronted with requests by individuals and community initiatives to approve the installation of satellite dishes. Such requests came especially from parts of East Germany where West German terrestrial broadcasts could not even be received with CATVs. Dresdeners for example complained that they were discriminated against since they could not enjoy West German television in contrast to the residents of nearby towns.[45]

The sale and installation of satellite dish antennas was illegal in East Germany[46], so residents installed satellite dishes illegally imported from West Germany or Hungary. When

[42]BArch SAPMO DY 30/IV2/2.039/276: 4–12.
[43]BStU 3189/87 IV: 2–5; BStU 3189/87 III: 58–61, 156–7.
[44]BStU 3189/87 IV: 6, 62.
[45]BStU MfS BV Dresden 33349: 151, 188.
[46]BArch SAPMO DY 30/IV2/2.039/276: 36–40, 47–49.

the Department of Postal Services, charged with enforcing legal regulations pertaining to broadcasting, demanded their removal or imposed fines, residents would simply ignore them or remove the satellite dishes only to re-install them later.[47]

In a March 1988 report to the Politburo, the Department conveyed a dramatic picture of the prevailing public mood: "Demands of grass-roots organizations and individual citizens to allow the reception of West German television [with satellite dishes] are becoming more and more brazen. There are constant disputes. The whole topic is increasingly becoming a political issue, especially since these grass-roots organizations and individual citizens attempt to gain access to satellite television no matter what and fines imposed for violations of legal regulations do not have the desired impact."[48]

Just a couple of months earlier, the Department had demanded the removal of a satellite dish in Marienberg that provided 4,000 households with West German programming. This demand let to massive protests, and even local party functionaries recommended that the satellite dish should be allowed to stay since its removal would "lead to serious confrontations with thousands of citizens."[49]

In the town of Weissenberg, residents had also installed an illegal satellite dish. Pleading with state officials who demanded its removal, local party functionaries as well as the mayor pointed out that members of their community were "much more content" since the introduction of West German television. Their attitudes towards the East German regime had become "more positive" and all applications to emigrate to West Germany had been withdrawn. The dismantling of the satellite dish would cause "enormous political problems" since it would contradict the wishes of the entire town.[50]

On the basis of such reports, the Politburo decided in August 1988 to almost completely liberalize the installation of satellite dishes (Holzweissig 2002; Stiehler 2001: 95).[51] Just six months later, 203 satellite dishes were officially registered in the Dresden district, with

[47]BArch SAPMO DY 30/IV2/2.039/276: 40–43.
[48]BArch SAPMO DY 30/IV2/2.039/276: 37.
[49]BArch SAPMO DY 30/IV2/2.039/276: 41.
[50]BArch SAPMO DY 30/IV2/2.039/276: 45.
[51]BStU MfS BV Dresden 11147.

250 more applications pending.[52]

VII. Threats to Validity

In this section, we show using an earlier survey that our findings can be replicated for (1) the Dresden district and (2) Greifswald, another city also cut off from West German television. We also present the results of a formal sensitivity analysis.

A. Subsample Comparison

One way to probe the robustness of our results is to re-estimate our models for six sub-samples, each composed of respondents living in Dresden and one of the treatment districts (East Berlin, Erfurt, Karl-Marx-Stadt, Magdeburg, Leipzig, Schwerin). The idea here is to capitalize on potential variation in unobserved confounders associated with each of these districts. If there was local variation in unobserved confounders such as respondents' tendency to answer survey questions truthfully, we would expect to see considerable differences in treatment effect estimates across these sub-samples.

Table 9 demonstrates that this is not the case for any of the districts except East Berlin.[53] It displays the treatment effect estimates obtained from our LARF model with the limited covariate set for each sub-sample.[54] Treatment effect estimates are quite similar across sub-populations in magnitude and precision. They are also close to the estimates obtained for the whole sample. Such stability in estimates suggests that locally varying confounders do not constitute much of a problem.

[52]BArch SAPMO DY 30/J IV2/2/2317: 72–77. Note that satellite dishes still had to be imported from West Germany or Hungary, and that a single dish was often used to supply hundreds of households with West German television.

[53]We have already seen in the comparison of baseline characteristics that East Berlin is an outlier on several dimensions (Figure 2). It was well known that living conditions were better in East Berlin than in the rest of the country.

[54]We had to exclude Cottbus because only 66 respondents in our sample reside there.

B. Replication I: Diversity in Time

Replication is as important in observational studies as in randomized experiments. We would be more confident if our findings could be replicated in different places, times, and circumstances because peculiar biases associated with one particular test may not be present in other tests.

Unfortunately, very few surveys conducted by the Central Institute for Youth Research contain the information necessary for a replication of our results. One survey from the year 1984 however is available. It contains about 3000 respondents from several smaller East German cities (Halle, Schwerin, Dessau, Magdeburg, Bitterfeld, Greifswald) and also 950 respondents from the Dresden district.

Since we attempt to replicate our previous results, we strive to make the analysis in this part identical to previous specifications. We code our instrument as 0 for respondents from the Dresden district, and 1 otherwise. For now, we exclude Greifswald respondents from the sample, since many of them also had no access to West German television.[55] The treatment indicator is again coded from a question that asks respondents how often they watch West German television.[56] The six answer categories are (1) daily, (2) two to five times per week, (3) once per week, (4) once or twice per month, (5) less often, or (6) never. The treatment indicator is coded as 1 for respondents answering $(1-4)$ and 0 for all others. Results are substantively similar if we code respondents who answer $(1-3)$ as 1, or if we only code respondents who answer (6) as 0. The correlation between West German television consumption and access to West German television is again very strong with a correlation of about .40 and a t-statistic of about 25 after conditioning on the extensive covariate set.

We chose those survey questions as outcome variables that match the questions in the 1988/89 survey as closely as possible:

[55]The results are substantively identical if Greifswald respondents are included here.

[56]German question: *"Uber das aktuelle politische Geschehen in der Welt kann man sich auf unterschiedliche Weise informieren. Wie ist das bei Ihnen? Uber das politische Geschehen informiere ich mich durch das Fernsehen der BRD."*

- Q1: *I am proud of being a citizen of our socialist country*[57]
- Q2: *It is personally important for me to help advance socialism*[58]
- Q3: *Socialism can only succeed if workers and farmers have a firm grasp on political power under the leadership of the communist party*[59]

The 4 point answer categories are similar to the ones used in the previous survey (strongly agree to strongly disagree) and we again reverse the coding so that higher numbers indicate increased regime support. We also reproduce our two sets of covariates (limited and extensive) which contain covariates similar to the ones used in the 1989 survey.[60] As before, we discretize all variables to avoid functional form assumptions.

Results are displayed in Table 10, showing treatment effect estimates for all three outcomes and both covariate sets. Standard errors are again adjusted for clustering. To economize on space and given the earlier similarity between LARF and 2SLS results, we just present 2SLS results here.

We find that across all models exposure to West German television has a positive effect on East Germans' support for the communist regime. The magnitude is comparable to our earlier results; the effects are also estimated fairly precisely. Although the survey questions are somewhat different and the survey was conducted in different districts five years earlier, we can fully replicate our results. This bolsters our confidence that our previous effect estimates were not some statistical artifact caused by biases particular to the 1989 survey.

C. Replication II: Diversity in Control Group

The 1984 survey allows for another type of replication in which we can make use of a second control group. Another look at the map in Figure 1 reveals that a second East

[57]German question: *"Ich bin stolz, ein Bürger unseres sozialistischen Staates zu sein."*

[58]German question: *"Das hat für mein Leben Bedeutung: alles dafür einsetzen, um den Sozialismus voranzubringen."*

[59]German question: *"Der Sozialismus kann nur dann errichtet werden, wenn die Arbeiter und Bauern unter der Führung der marxistisch-leninistischen Partei die politische Macht fest in ihrer Hand halten."*

[60]The limited set of covariates contains age and gender (no information on parents' qualification is available). The extensive set contains age, gender, educational attainment, occupational qualification, marriage status, number of children, and employment status (no information on net income is available).

German region was also cut off from West German television signals. Just like Dresden, the small city of Greifswald was located so far from West German broadcast towers that most residents could not pick up the signal.

The 1984 survey contains 167 respondents from Greifswald which we will use as a second control group. The use of multiple control groups as a way to address hidden biases has a long pedigree in observational studies (Campbell 1969; Shadish, Cook, and Campbell 2002). The rationale for their use is that unobserved confounders present in the Dresden district are unlikely to also be present in Greifswald. Therefore, if we can replicate our findings with this new control group, we can rule out the possibility that our results are driven by some unobserved confounder specific to Dresden.

We conduct this replication by discarding all respondents from the Dresden district; only Greifswald respondents are assigned to the control group. We then fit the same models as in the previous section. Note that the sample size for the controls is now greatly reduced. We would like to have more respondents from Greifswald but such data are not available. Exposure to West German television and living in Greifswald are still strongly correlated at about .15; the t-statistic is about 3 when we condition on all covariates from the extensive set.

Table 11 presents the findings. Similar to our previous results, we again find a positive effect of exposure to West German television on support for the communist regime. The effects are substantively important for all outcome variables and both covariate sets. Despite the small sample size, most estimates are significant at conventional levels. Finding a similar effect from exposure to West German television for East Germans living in a different city subject to the same treatment assignment mechanism lends additional robustness to our results. It seems unlikely that hidden biases could account for such a pattern.

D. Formal Sensitivity Analysis

Another way to check the robustness of our inferences is to conduct a formal sensitivity analysis (Cornfield et al. 1959; Bross 1966; Greenhouse 1982; Schlesselman 1978; Gastwirth 1992; Rosenbaum and Rubin 1983; Rosenbaum 1987, 2002). This technique is based on the

idea that if we cannot be certain that our identifying assumptions hold, it is useful to know how severe violations would have to be to alter our substantive conclusions. Sensitivity analysis thus allows us to explicitly address the uncertainty in our assumptions.

Recall that the ignorability assumption implies zero correlation between the errors and the instrument. In order to check the sensitivity of our results to this assumption we proceed by inducing correlation between the instrument and the errors as proposed by Wand (2002). More precisely, we condition on our extensive set of covariates, the treatment variable and the instrument, and their sample covariance, and induce a correlation of magnitude ρ between these variables and a vector of simulated residuals. This vector is generated using the Metropolis-Hastings algorithm, assuming a multivariate normal distribution. This assures that despite the induced correlation between errors, instrument, and treatment variable, the relationship between the instrument and the covariates remains unchanged. The simulated residuals are subsequently added to the predicted values of the original model and the model is refitted. We thus obtain a new treatment effect estimate under the assumption of unobserved confounding of magnitude ρ and can compare this new estimate with the original treatment effect estimate.

Results are reported in Figure 5. In each of the three graphs, the vertical axis displays treatment effect estimates, with the original estimate (Table 3, column 6) marked by a dashed horizontal line. An effect estimate of zero is marked by a solid horizontal line. Each dot (connected by a fitted lowess line) represents the treatment effect estimate obtained at a specified level of ρ. Values of ρ are shown on the horizontal axis. The results suggest that our treatment effect estimates are fairly insensitive to hidden bias. For example, for our second response variable ($Y2$), ρ would have to be larger than -0.18 to change the sign of our treatment effect estimate. One would have to postulate the existence of an unobserved confounder that, conditional on the covariates we have included, is negatively correlated with both living in the Dresden district and regime support at a level of at least 0.18. Moreover, this unobserved confounder would also need to exhibit little variation across East German districts.

VIII. Discussion our our results

Our results challenge the prevailing wisdom that foreign free media threaten the stability of authoritarian regimes. We have shown, using a combination of survey data and archival material, that East Germans who watched West German television were more satisfied with the communist regime, and that East German authorities seem to have been aware of this effect as well.

We know from a well-established literature on the negative effects of television in democratic societies that it can lead to a trivialization of politics. Television equates news with entertainment, focuses on ephemeral matters such as the personalities and foibles of individual politicians, and stresses their self-interested behavior. In consequence, citizens loose interest in politics, develop cynical attitudes towards political institutions and politicians, and reduce their political participation (Ranney 1983; Entman 1989; Patterson 1993; Gunther and Mughan 2000). Wy should television have more benevolent effects in less democratic societies?

To argue that West German television stabilized the East German regime over the long run is not to deny that West German television served as a critical coordinating device during the East German revolution. In the absence of West German television, East Germans would not have learned as quickly about Hungary's decision to open its border with Austria, a decision which started the mass exodus that eventually led to the collapse of the East German regime. If East Germans had not been able to see the Monday demonstrations in Leipzig on their television screens and, more importantly, had not learned about the regime's apparent impotence to stop them, the number of protesters would not have risen as rapidly as it did, reaching hundreds of thousands in October 1989 (Lohmann 1994).

West German television accelerated the collapse of the East German communist regime, but it did not cause it. The summer and fall of 1989 were very different from earlier decades. For many years, East Germans had no reason to entertain any serious hopes for substantial political reforms, much less the breakdown of the communist dictatorship. It was only

political reforms in the Soviet Union in the wake of *glasnost* and *perestroika*, liberalization in Poland and Hungary, and, most importantly, the lack of any drastic Soviet reaction to it that convinced East Germans that the time was ripe for political protests. Without Gorbachev's decision to revoke the Brezhnev doctrine and to permit the Soviet Union's satellite regimes in Eastern Europe to go their own way, the revolutions in Eastern Europe surely would not have happened (Zubock 1998; Brown 2004; Lévesque 2004; Jarausch 1994).

Are our results generalizable to other authoritarian regimes? In many respects, East Germany is a "most likely" case (George and Bennett 2005: 120–124; Eckstein 1975) for theories that posit a negative relationship between foreign free media and the stability of authoritarian regimes. In East Germany, the potential impact of West German television was not diluted by language or cultural barriers, and almost all East Germans were able to tune in to West German television. If foreign free media indeed undermine authoritarian rule, we should have been able to observe their influence in East Germany.

Since the end of the Cold War, optimistic predictions about the impending collapse of the remaining authoritarian regimes have sometimes shifted from the purported effects of transnational television and radio to the democratizing force of the Internet. The belief that the Internet poses a fatal threat to authoritarian rule is widespread. In 1999, President George W. Bush for example admonished an audience in Phoenix, Arizona to "imagine if the internet took hold in China. Imagine how freedom would spread."[61] Yet China seems to successfully pursue a strategy of access *and* control, embracing the economic and entertainment uses of the Internet to promote development and to sustain political legitimacy while at the same strictly controlling content that is critical of the regime (Kalanthil and Boas 2003). As we have shown in our paper, foreign free media were not necessarily a force of democratization during the Cold War. Perhaps it would be prudent not to expect that much from them now either.

[61] Phoenix, Arizona, December 7, 1999.

IX. Appendix: Identification using Local Average Response Function

This appendix describes a new class of instrumental variable estimators called local average response function for compliers (LARF), which has been developed recently by Abadie (2003). The semi-parametric LARF estimator allows for the identification of LATE conditional on X without restricting the treatment effect to be constant among individuals or to be a deterministic function of the covariates. It also requires no parametric assumptions for identification while allowing the estimation of a parsimonious parameterization for the response function of interest.[62]

Following Abadie (2003), LARF is the function that describes average treatment responses for any group of compliers defined by some value for the covariates. Formally, LARF is defined as $E[Y|X, D, D_1 > D_0]$.[63] Note that in our case, $P(D_0 = 0|X) = 1$ for most individuals and therefore LARF approximates the average treatment effect on the treated conditional on X. LARF cannot be estimated directly, since only one potential treatment status is observed; compliers are not individually identified. The solution to this problem is based on an identification theorem derived by Abadie (2003) that shows that any statistical characteristic that can be defined in terms of moments of the joint distribution of (Y, D, X) is identified for compliers. Note that from assumption IV.1 it follows that

$$P(D_1 > D_0|X) = E[D|Z = 1, X] - E[D|Z = 0, X] > 0$$

i.e., the proportion of compliers in the population is identified given X and this proportion is greater than zero. Now let $g(\cdot)$ be any measurable real function of (Y, D, X) such that

[62]Notice that other attempts have been made to introduce covariates in the identification of the local average treatment effect for compliers (Angrist and Imbens 1995; Little and Yau 1998; Hirano, Imbens, Rubin, and Zhou 2000; Fröhlich 2002; Ten Have, Joffe, and Cary 2003; Vansteelandt and Goetghebeur 2003; Robins and Rotnitzky 2004).

[63]Consider the function of (D, X) that is equal to $E[Y_0|X, D_1 > D_0]$ if $D = 0$, and is equal to $E[Y_1|X, D_1 > D_0]$ if $D = 1$. Note that since $D = Z$ for compliers, and Z is ignorable conditional on X, it follows that $E[Y|X, D = 0, D_1 > D_0] = E[Y_0|X, Z = 0, D_1 > D_0] = E[Y_0|X, D_1 > D_0]$ and similarly, $E[Y|X, D = 1, D_1 > D_0] = E[Y_1|X, D_1 > D_0]$. Thus, $E[Y|X, D = 1, D_1 > D_0] - E[Y|X, D = 0, D_1 > D_0] = E[Y_1 - Y_0|X, D_1 > D_0]$.

$E|g(Y,D,X)| < \infty$. And define:

$$\kappa = 1 - \frac{D \cdot (1-Z)}{P(Z=0|X)} - \frac{(1-D) \cdot Z}{P(Z=1|X)}$$

Then,

$$E[g(Y,D,X)|D_1 > D_0] = \frac{1}{P(D_1 > D_0)} E[\kappa \cdot g(Y,D,X)]$$

Setting $g(Y,DX) = 1$ we obtain $E[\kappa] = P(D_1 > D_0)$, which shows that one can think of κ as a weighting scheme that allows us to identify expectations for compliers.[64] Informally speaking, κ is related to the probability of being a complier, based on the covariate information. Since we do not know $\tau_0 = P(Z=1|X=x)$ we have to estimate it in a first step. Here, we simply follow Abadie (2003) and rely on a fully saturated binary Probit model to guarantee that the estimate of τ_0 lies between zero and one.[65] Thus, we compute

$$\kappa_i = 1 - \frac{D_i \cdot (1-Z_i)}{1-\tau_0(X_i)} - \frac{(1-D_i) \cdot Z_i}{\tau_0(X_i)}$$

This general identification result allows for the estimation of a wide variety of LARF models. For example, one can parameterize LARF by Least Squares (LS) or specify a parametric distribution for $P(Y|X,D,D_1 > D_0)$ and estimate the parameters of LARF by Maximum Likelihood.[66] Abadie (2003) derives the asymptotic distribution of such estimators and establishes $\sqrt{N}$-consistency. Asymptotic distributions are derived under general misspecification. Therefore, the resulting standard errors are robust to misspecification.

Abadie's general identification result allows for various parameterizations of LARF. Consider some class of parametric functions $\aleph = \{h(D,X;\theta)|\theta \in \Theta \subset \mathbb{R}^m\}$. Then in the

[64] Note, however, that κ is not a proper weight as it takes negative values when D differs from Z.

[65] If X is discrete with finite support, then τ_0 is linear in a saturated model that includes indicators for all possible values of X. Just as in propensity score estimation, institutional knowledge about the nature of the instrument (assignment) can often be used to restrict the number of covariates from X that enter the function τ_0. This dimension reduction can be very important to overcome the curse of dimensionality when X is highly dimensional. In principle, τ can be estimated fully non-parametrically, but as Abadie (2003) notes, the number of observations required to attain acceptable precision increases very rapidly with the number of covariates. Another alternative is semi-parametric estimation using power series or splines.

[66] Notice that identification of the conditional distribution of $(Y|D,X)$ for compliers does not depend on the particular parametric specification adopted for LS or ML. Therefore, the estimators retain an appealing interpretation as best approximations even under misspecification of the structural model.

linear case $h(D, X; \theta) = \alpha D + X'\beta$ and thus the parameters of LARF can be expressed as:

$$(\alpha, \beta) = \text{argmin}_{\theta \in \Theta} E\big[\{Y - (\alpha D + X'\beta)\}^2 | D_1 > D_0\big] \tag{3}$$

With sample analog:

$$(\widehat{\alpha}, \widehat{\beta}) = \text{argmin}_{\theta \in \Theta} \frac{1}{N} \sum_{i=1}^{N} \kappa_i \ (Y_i - (\alpha D_i + X_i'\beta))^2 \tag{4}$$

LARF can also be estimated by maximum likelihood. We need to specify a parametric distribution for $P(Y|X, D, D_1 > D_0)$ with density $f(Y, D, X; \theta)$ for $\theta \in \Theta$ and expectation $E[Y|D, X, D_1 > D_0] = h(D, X; \theta)$. This results in the following estimator:

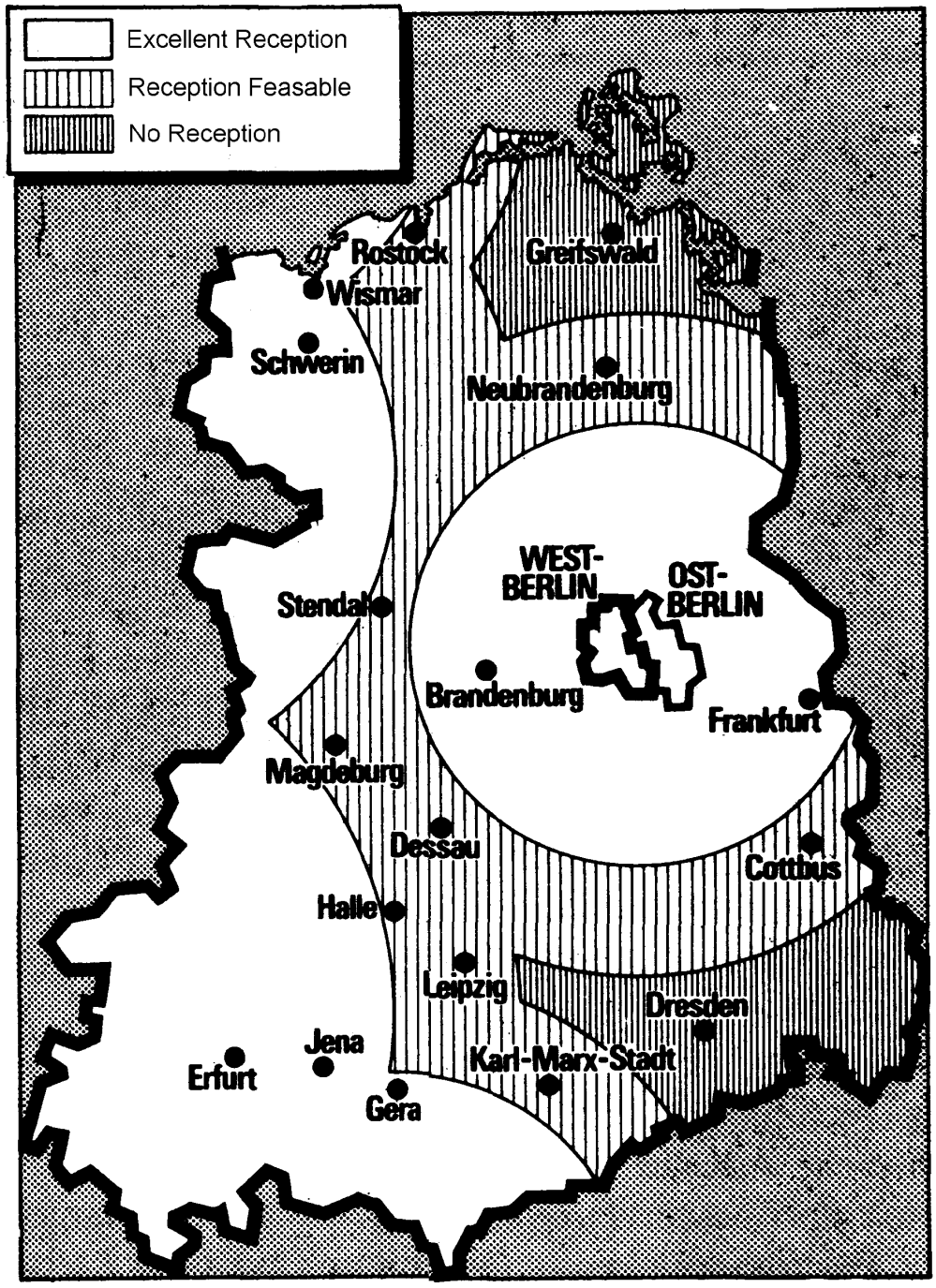
$$(\theta) = \text{argmax}_{\theta \in \Theta} E[\ln f(Y, D, X; \theta_0) | D_1 > D_0] \tag{5}$$

With sample analog:

$$(\widehat{\alpha}, \widehat{\beta}) = \text{argmin}_{\theta \in \Theta} \frac{1}{N} \sum_{i=1}^{N} \kappa_i \ \ln f(Y_i, D_i, X_i; \theta) \tag{6}$$

In our survey the response variable is categorical, with Y_i taking on the integer value associated with one of $j = (1, ..., J)$ categories. As recommended in Abadie (2003), we extent his model accordingly and specify LARF using an ordinal probit link function based on the latent continuous variable $Y_i^* \sim N(\mu_i, 1)$, with $\mu_i = \alpha D_i + X_i'\beta$. The mapping of Y_i^* to Y_i is $Y_i = j$ if $\lambda_{j-1} \leq Y_i^* \leq \lambda_j$, with thresholds $\lambda_l \in \{(\lambda_0, ..., \lambda_J) | \lambda_0 = -\infty \wedge \lambda_J = \infty \wedge \lambda_l < \lambda_{l+1}\} \ \forall \ l = (0, ..., J)$. Therefore, $P(Y_i = j) = \Phi(\lambda_j | \mu_i) - \Phi(\lambda_{j-1} | \mu_i)$ where $\Phi(\cdot)$ is the cumulative distribution function of a standard normal. An appealing feature of the ordinal Probit specification is that the estimated effect of the treatment is allowed to change with covariates.

Figure 1: Signal strength of West German television broadcasts



Source: Adapted from *Die Welt* 1973.

Figure 2: Comparison of baseline characteristics

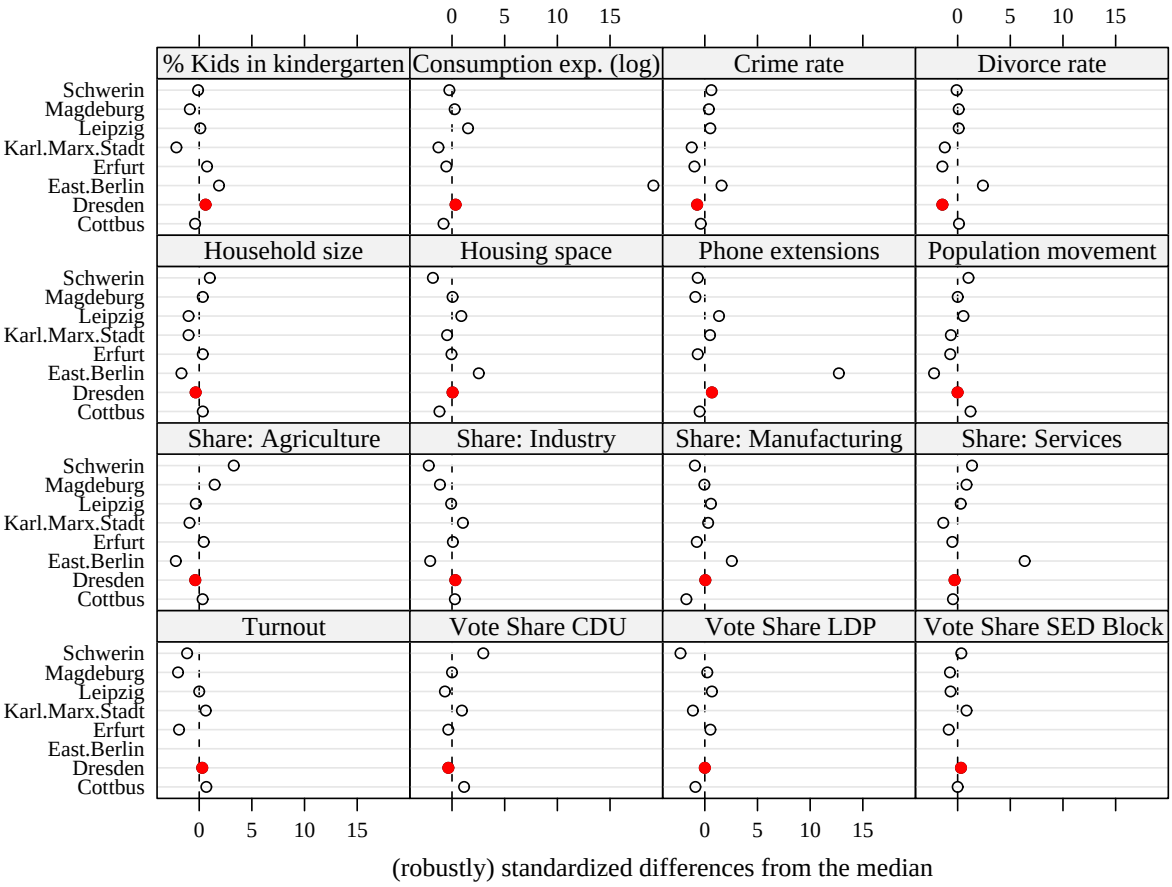
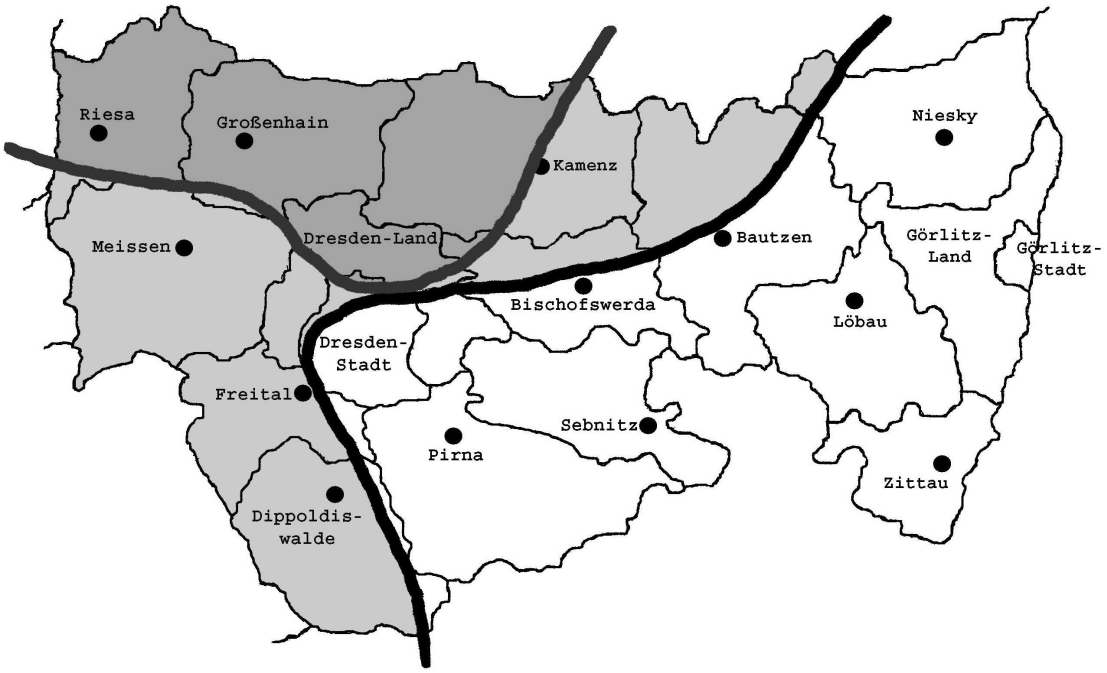
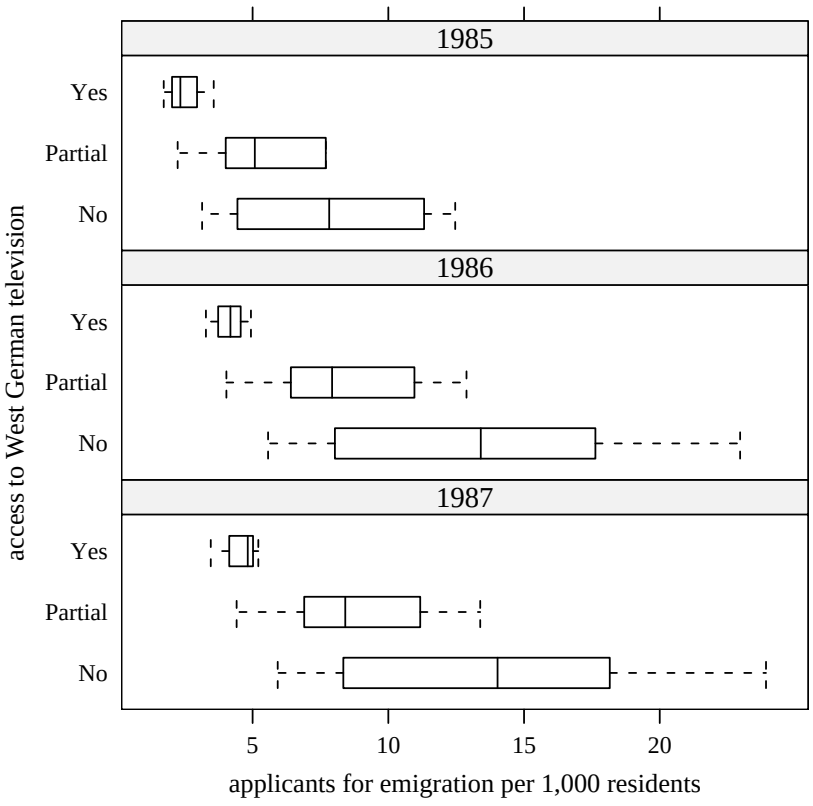


Figure 3: Applicants for emigration per capita for counties in the Dresden district and access to West German television



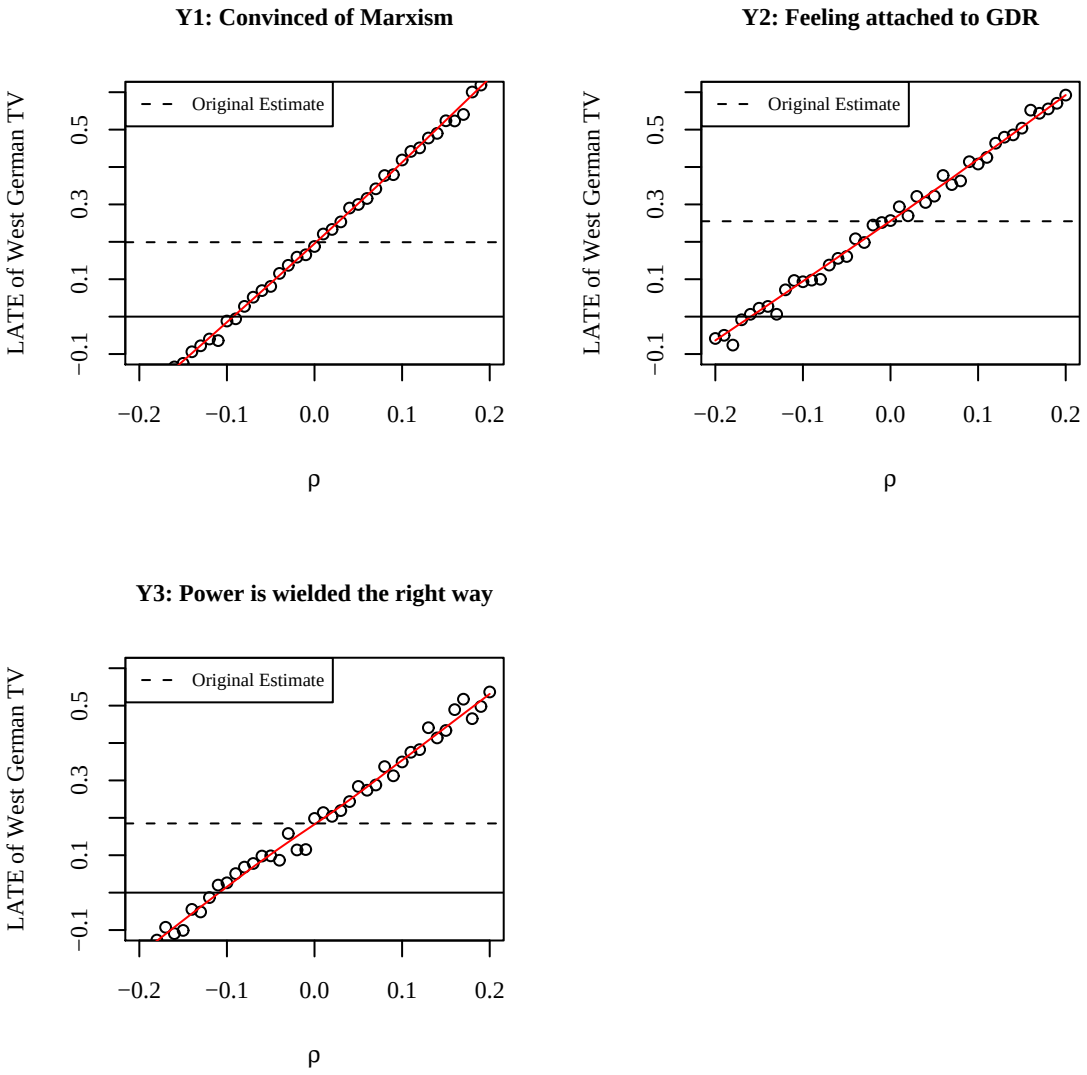
The map shows spatial patterns in access to West German television across the 17 counties in the Dresden district for 1988. White areas had no access, areas shaded in light gray had partial access, and areas shaded in dark gray had full access to West German television. Source: Adapted from original map in BArch SAPMO DY 30/J IV2/2/2317: 75.

Figure 4: Applicants for emigration per capita for counties in the Dresden district and access to West German television



Source: BStU MfS BV Dresden Stellvert. OP 21; BStU MfS BV Dresden BKG-8028/8029.

Figure 5: Sensitivity test II: Violations of the ignorability assumption



The graphs show results from a formal sensitivity analysis that examines how severe violations of the exclusion restriction would have to be (conditional on our extensive set of covariates) in order to alter the substantive conclusion for a positive treatment effect of West German television on regime support. The vertical axes display the size of the effect estimates. The horizontal axes display the size of the correlation ρ that is induced between the instrument and the error term. The dashed horizontal line indicates the original effect estimates obtained earlier under the assumption that $\rho = 0$, i.e., no correlation between instrument and error term. The results suggest that our treatment effect estimates are fairly insensitive to hidden bias. In order to change the sign of our treatment effect estimate, one would have to postulate the existence of an unobserved confounder that is, conditional on the covariates we have included, negatively correlated with both living in the Dresden district and regime support at a level of at least $0.12 - 0.18$. For comparison, the correlation between mother's and father's education in this dataset is .15.

Table 1: Consumption of West German television and living in the Dresden district

How often do you watch West German television?	Living in Dresden Count	%	Elsewhere Count	%
daily	42	5	1820	65
multiple times each week	69	9	732	26
once a week	17	2	47	2
less than once a week	108	14	101	4
never	498	64	46	2
Missing	50	6	33	1
Total	784	100	2779	100

Table 2: Outcome variables

	fully disagree	largely disagree	largely agree	fully agree	Missing	Total	Mean	SD
Q1: Convinced of Leninist/	399	850	1697	571	47	3564	2.69	0.87
Marxist world view	11	24	48	16	1	%		
Q2: Feeling closely attached to the GDR	154	600	1963	809	38	3564	2.97	0.76
	4	17	55	23	1	%		
Q3: In the GDR, power is exercised	301	874	1969	373	47	3564	2.69	0.77
in ways consistent with my views	8	25	55	10	1	%		

Table 3: The effect of West German television consumption on East Germans' regime support

Model	1	2	3	4	5	6
Estimator	Diff.	LATE	2SLS	LARF	2SLS	LARF
Covariate set	–	–	Limited	Limited	Extensive	Extensive
Y1: I am convinced of the Leninist/Marxist world view ($\mu = 2.69$; $\sigma = 0.87$)						
West German tv	−0.079	0.147	0.205	0.204	0.198	0.204
Standard error	(0.053)	(0.083)	(0.084)	(0.084)	(0.087)	(0.108)
Y2: I feel closely attached to the GDR ($\mu = 2.97$; $\sigma = 0.75$)						
West German tv	−0.013	0.217	0.258	0.255	0.256	0.251
Standard error	(0.044)	(0.067)	(0.072)	(0.075)	(0.073)	(0.090)
Y3: In the GDR, power is exercised in ways consistent with my views ($\mu = 2.68$; $\sigma = 0.77$)						
West German tv	−0.014	0.158	0.193	0.191	0.186	0.185
Standard error	(0.047)	(0.078)	(0.082)	(0.083)	(0.081)	(0.106)

Note: $N = 3441$ (3426) for models without (with) covariates. Treatment effect estimates for each estimator are shown, with robust cluster-adjusted standard errors in parentheses. Diff. is the difference in means ($\widehat{\alpha} = \bar{Y_1} - \bar{Y_0}$). LATE is the Local Average Treatment Effect for Compliers. 2SLS is the Two Stage Least Squares estimate. LARF is the estimate from a Local Average Response Function and a saturated first stage Probit specification. Note that LARF identifies LATE conditional on X. The limited covariate set includes age, gender, and father's and mother's occupational qualification. The extensive covariate set includes the limited set of covariates plus respondent's marriage status, living situation, number of children, highest educational attainment, occupational classification, net monthly income, and employment status. All covariate categories including missingness are fully discretized using indicator variables. Answer categories for the response variables are coded as 4 = fully agree, 3 = largely agree, 2 = largely disagree, and 1 = fully disagree.

Table 4: The effect of West German television consumption on East Germans' regime support: First differences from ordered Probit local average response function estimator

Predicted shift in $Pr(Y = j)$	j = 1 fully disagree	j = 2 largely disagree	j = 3 largely agree	j = 4 fully agree
Y1: I am convinced of the Leninist/Marxist world view				
West German tv	−0.038	−0.050	0.022	0.065
Upper CI bound	(−0.000)	(−0.004)	(0.155)	(0.145)
Lower CI bound	(−0.069)	(−0.267)	(0.002)	(0.008)
Y2: I feel closely attached to the GDR				
West German tv	−0.026	−0.073	−0.028	0.127
Upper CI bound	(−0.000)	(−0.013)	(0.115)	(0.261)
Lower CI bound	(−0.125)	(−0.303)	(−0.080)	(0.041)
Y3: In the GDR, power is exercised in ways consistent with my views				
West German tv	−0.036	−0.064	0.044	0.056
Upper CI bound	(−0.000)	(−0.004)	(0.180)	(0.117)
Lower CI bound	(−0.063)	(−0.264)	(0.003)	(0.004)

Note: $N = 3426$ for all models. Treatment effect estimates with upper and lower bound for robust cluster-adjusted .95 confidence interval in parentheses. Estimates are average treatment effects for compliers in first differences, i.e., the average change in the predicted $Pr(Y = j)$ for compliers under treatment and control (West German television exposure vs. no exposure). Estimates are computed using ordered Probit local average response functions with the limited covariates set (held at the observed values for all compliers) and a fully saturated first stage Probit specification.

Table 5: Videomalaise and entertainment effects

Y4: To what extend do you consider yourself well informed by the GDR media about recent policy changes in the Soviet Union? ($\mu = 3.11$; $\sigma = 1.04$)		
Covariate set	Limited	Extensive
West German tv	0.185	0.167
Standard error	(0.112)	(0.113)
N	1144	1144

Y5: Do you feel attached to West Germany? ($\mu = 2.28$; $\sigma = 1.08$)		
Covariate set	Limited	Extensive
West German tv	-0.193	-0.172
Standard error	(0.111)	(0.113)
N	1141	1141

Y6: How satisfied are you with the available options for recreational activities? ($\mu = 3.05$; $\sigma = 1.10$)		
Covariate set	Limited	Extensive
West German tv	0.395	0.405
Standard error	(0.117)	(0.110)
N	1148	1148

Note: Results are 2SLS estimates with cluster-adjusted standard errors in parentheses. The limited covariate set includes age, gender, and father's and mother's occupational qualification. The extensive covariate set includes the limited set of covariates plus respondent's marriage status, living situation, number of children, highest educational attainment, occupational classification, net monthly income, and employment status. All covariate categories including missingness are fully discretized using indicator variables. Answer categories for the response variables range from 5 = fully to 1 = not at all.

Table 6: Number of applicants for emigration in 1988

City	Applicants
Dresden	16.4
East Berlin	11.8
Karl-Marx-Stadt	9.8
Leipzig	7.9
Erfurt	5.0
Cottbus	3.5
Magdeburg	2.8
Schwerin	2.4
mean	7.45

Note: Number of applicants for emigration from East Germany per 1,000 residents in 1988. Source: BStU MfS-ZKG 10734.

Table 7: The effect of access to West German television on the number of applicants for emigration in the Dresden district

	sample mean			p-value	p-value random. inf.	
	access	no access	diff. in means	t-test	diff. in means	ranks
Ambiguous counties coded:						
access	6.2284	11.6699	−5.4415	0.0249	0.0258	0.0325
no access	5.7625	10.8230	−5.0605	0.0397	0.0394	0.0325

Note: $N = 17$. Specifications compare the average number of applicants per 1,000 residents for emigration in 1985-87 in 17 counties in the Dresden district. Two ambiguous counties (Bautzen and Bischofswerda) are either included in the group of counties with access to West German television (row 1) or the group without access (row 2). Columns 1 and 2 give the average number of applicants in the two groups. Column 3 presents the difference in means and column 4 the p-value from a t-test of no difference in group means. Columns 5 and 6 show p-values from exact randomization tests against the sharp null of no effect. Column 5 is based on the difference in means, while column 6 is based on the (more robust) difference in average ranks. All tests are two-sided.

Table 8: Vote shares in 1946 state elections for full access, partial access, and no access counties in Dresden district

access	LDP	CDU	SED
yes	23.73	23.94	52.33
partial	25.20	22.46	52.34
no	28.27	19.63	52.10

Note: Cell entries are vote shares in 1946 state elections, adjusted for redistricting and weighted by population size.

Table 9: Sensitivity test I: Effect estimates for sub-populations

	Response variable			
Treatment city	Y1	Y2	Y3	N
Schwerin	0.285	0.49	0.35	976
	(0.13)	(0.109)	(0.123)	
Magdeburg	0.322	0.399	0.223	1333
	(0.074)	(0.066)	(0.070)	
East Berlin	0.133	0.099	-0.006	1203
	(0.084)	(0.074)	(0.074)	
Leipzig	0.338	0.498	0.435	1061
	(0.10)	(0.087)	(0.086)	
Karl-Marx-Stadt	0.147	0.219	0.212	1417
	(0.075)	(0.064)	(0.065)	
Erfurt	0.137	0.151	0.123	1433
	(0.073)	(0.065)	(0.067)	

Note: Local average treatment effect estimates for compliers from Local Average Response Function estimator and a fully saturated first-stage probit specification. Standard errors are in parentheses. Note that LARF identifies LATE conditional on X. All models include the limited covariate set with covariate categories fully discretized using indicator variables. Each row presents estimates for a different treatment group, i.e., estimates for a sample with Dresden and one other district as the treatment group.

Table 10: Replication with 1984 survey

Q1: I am proud of being a citizen of our socialist country		
Covariate set	Limited	Extensive
West German tv	0.467	0.465
Standard error	(0.113)	(0.106)
N	2901	2901

Q2: It is personally important for me to help advance socialism		
Covariate set	Limited	Extensive
West German tv	0.366	0.390
Standard error	(0.128)	(0.128)
N	2897	2897

Q3: Socialism can only succeed if workers and farmers have a firm grasp on political power under the leadership of the communist party		
Covariate set	Limited	Extensive
West German tv	0.137	0.146
Standard error	(0.073)	(0.073)
N	2897	2897

Note: Results are 2SLS estimates with cluster-adjusted standard errors in parentheses. The limited covariate set includes age and gender. The extensive covariate set includes age, gender, family conditions, number of children, education, and occupational qualification. All X are fully discretized using indicator variables. Answer categories for the response variables are coded as 4 = fully agree, 3 = largely agree, 2 = largely disagree, 1 = fully disagree.

Table 11: Replication with 1984 survey, Greifwald respondents as control group

Q1: I am proud of being a citizen of our socialist country		
Covariate set	Limited	Extensive
West German tv	1.770	1.218
Standard error	(0.843)	(0.864)
N	2155	2155

Q2: It is personally important for me to help advance socialism		
Covariate set	Limited	Extensive
West German tv	1.598	1.920
Standard error	(1.564)	(1.276)
N	2154	2154

Q3: Socialism can only succeed if workers and farmers have a firm grasp on political power under the leadership of the communist party		
Covariate set	Limited	Extensive
West German tv	0.645	0.856
Standard error	(0.380)	(0.382)
N	2156	2156

Note: Results are 2SLS estimates with cluster-adjusted standard errors in parentheses. The limited covariate set includes age and gender. The extensive covariate set includes age, gender, family conditions, number of children, education, and occupational qualification. All X are fully discretized using indicator variables. Answer categories for the response variables are coded as 4 = fully agree, 3 = largely agree, 2 = largely disagree, 1 = fully disagree.

References

Abadie, A. 2003. "Semiparametric instrumental variable estimation of treatment response models," *Journal of Econometrics*, vol. 113, no. 2, 231–263.

Angrist, Joshua D. and Alan B. Krueger. 2001. "Instrumental Variables and the Search for Identification: From Supply and Demand to Natural Experiments," *Journal of Economic Perspectives*, vol. 15, no. 4, 69–85.

Angrist, J. D., Imbens, G.W. and Rubin, D.B. 1996. "Identification of Causal Effects Using Instrumental Variables," *Journal of the American Statistical Association*, vol. 91, 444–472.

Bennett, W. Lance. 1998. "The Media and Democratic Development: The Social Basis of Political Communication," in Patrick H. O'Neil (ed.), *Communicating Democracy: The Media and Political Transitions.* Lynne Rienner.

Blundell, R. and Powell, J. 2001. "Endogeneity in Nonparametric and Semiparametric Regression Models." *Cemmap Working Paper 09/01.*

Braumann, Christa. 1994. "Fernsehforschung zwischen Parteilichkeit und Objektivität," *Rundfunk und Fernsehen*, vol. 42, no. 4, 524–541.

Bross, I.D.J. 1966. "Spurious effects from an extraneous variable," *Journal of Chronic Diseases*, vol. 19, 637–647.

Broszat, Martin and Hermann Weber. 1993. *SBZ-Handbuch: Staatliche Verwaltungen, Parteien, gesellschaftliche Organisationen und ihre Führungskräfte in der Sowjetischen Besatzungszone Deutschlands 1945–1949.* 2nd ed. R. Oldenbourg.

Brown, Archie. 2004. Gorbachev and the End of the Cold War. In Richard K. Herrmann and Richard Ned Lebow (eds.) *Ending the Cold War.* Palgrave Macmillan.

Buhl, Dieter. 1990. "Window to the West: How Television from the Federal Republic Influenced Events in East Germany," Discussion paper D-5, Joan Shorenstein Barone Center, Harvard University.

Cornfield, J., Haenzel, W., Hammond, E., Lilienfeld, A., Shimkin, M., and Wynder, E. 1959. "Smoking and lung cancer: Recent evidence and a discussion of some questions," *Journal of the National Cancer Institute*, vol. 22, 173–203.

Cox, D.R. 1992. *The Planning of Experiments.* Wiley.

Darschin, Wolfgang and Camille Zubayr. 2000. "Warum sehen die Ostdeutschen anders fern als die Westdeutschen?" Media Perspektiven no 6, 249–257.

Davison, A. C. and Hinkley, D.V. 1997. *Bootstrap methods and their applications.* Cambridge University Press.

Della Vigna, Stefano and Ethan Kaplan. 2006. "The Fox News Effect: Media Bias and Voting." UC Berkeley and Stockholm University: Working paper.

de Sola Pool, Ithiel. 1974. Communication in Totalitarian Societies, in Ithiel de Sola Pool et al. (eds.) *Handbook of Communication.* Rand McNally.

Diamond, Larry (ed.) 1993a. *Political Culture and Democracy in Developing Countries.* Lynne Rienner.

Diamond, Larry. 1993b. "The Globalization of Democracy," in Robert O. Slater, Barry M. Schutz, and Steven R. Dorr (eds.) *Global Transformation and the Third World.* Lynne Rienner.

Eckstein, Harry. 1975. Case Studies in Political Science, in Fred Greenstein and Nelson Polsby (eds.) *Handbook of Political Science*, Vol. 7. Addison-Wesley.

Entman, Robert. 1989. *Democracy without citizens: Media and the Decay of American Politics.* Oxford University Press.

Feng, Z., McLeran, D. and J. Grizzle. 1996. "A comparison of statistical methods for clustered data analysis with Gaussian error." Statistics in Medicine, vol. 15, no. 16, 1793–1806.

Friedrich, Walter, Peter Förster, and Kurt Starke (eds.) 1999. *Das Zentralinstitut für Jugendforschung Leipzig 1966–1990.* Berlin: edition ost.

Friedrich, Walter. 1990. "Mentalitätswandlungen der Jugend in der DDR." *Aus Politik und Zeitgeschichte.* Beilage zur Wochenzeitung *Das Parlament*, no. 16–7, 25–37.

Fröhlich, M. 2002. "Nonparametric IV estimation of local average treatment effects with covariates." *Discussion paper no. 2019, University of St. Gallen.*

Früh, Werner and Stiehler, Hans-Jörg. 2002. *Fernsehen in Ostdeutschland.* Vistas.

Gabriel, Oscar W. and Klaus G. Troitzsch (eds.) 1993. *Wahlen in Zeiten des Umbruchs.* Peter Lang.

Gastwirth, J. L. 1992. "Methods for assessing the sensitivity of statistical comparisons used in Title VII cases to omitted variables," *Jurimetrics Journal*, 19–34.

Geddes, Barbara, and John Zaller 1989. "Sources of Popular Support for Authoritarian Regimes." *American Journal of Political Science*, vol. 33, no. 2, 319–347.

Gentzkow, Matthew. 2005. "Television and Voter Turnout." University of Chicago: Working paper

George, Alexander L. and Andrew Bennett. 2005. *Case Studies and Theory Development in the Social Sciences.* MIT Press.

George, Lisa and Joel Waldvogel 2002. "Does the New York Times Spread Ignorance and Apathy?" Michigan State University and University of Pennsylvania: Working paper.

Grünert, Holle. 1996. Das Beschäftigungssystem der DDR, in Burkart Lutz, Hildegard M. Nickel, Rudi Schmidt, and Arndt Sorge (eds.) *Arbeit, Arbeitsmarkt und Betriebe.* Opladen: Leske + Budrich.

Grünert, Holle, Maritta Bernien, and Burkart Lutz. 1997. "Das Beschäftigungssystem der DDR: Funktionsweise, Entwicklungstendenzen und Folgewirkungen" in Doris Beer, Christian Brinkmann, Axel Deeke, and sabine Schenk (eds.) *Der ostdeutsche Arbeitsmarkt in Gesamtdeutschland: Angleichung oder Auseinanderdriften?* Opladen: Leske + Budrich.

Gunther, Richard and Anthony Mughan (eds.) 2004. *Democracy and the Media.* Cambridge University Press.

Hajna, Karl-Heinz. 2000. *Die Landtagswahlen 1946 in the SBZ.* Peter Lang.

Heckmann, J. J. and Robb, R. 1985. "Alternative Methods for Evaluating the Impact of Interventions," *Journal of Econometrics*, vol. 30, no. 1–2, 239–267.

Heckmann, J. J. and Vytlacil, E. J. 1999. "Local Instrumental Variables and Latent Variable Models for Identifying and Bounding Treatment Effects," *Proceedings of the National Academy of Sciences*, vol. 96, 4730–4734.

Hesse, Kurt R. 1988. *Westmedien in der DDR: Nutzung, Image und Auswirkungen bundesrepublikanischen Hörfunks und Fernsehens.* Köln: Verlag Wissenschaft und Polititk.

Hesse, Kurt R. 1990. "Cross-Border Mass Communication from West to East Germany," *European Journal of Communication*, vol. 5, 355–371.

Holland, Paul W. 1986. "Statistics and Causal Inference," *Journal of the American Statistical Association*, vol. 81, no. 396, 945–960.

Huntington, Samuel P. 1991. *The Third Wave: Democratization in the Late Twentieth Century.* University of Oklahoma Press.

Imbens, G. W. and Angrist, J. D. 1994. "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, vol. 62, no. 4, 467–476.

Iyengar, Shanto. 1991. *Is Anyone Responsible? How Television Frames Political Issues.* University of Chicago Press.

Jarausch, Konrad H. 1994. *The Rush to German Unity.* Oxford University Press.

Kalathil, Shanti and Taylor C. Boas. 2003. *Open Networks, Closed Regimes: The Impact of the Internet on Authoritarian Rule.* Carnegie Endowment for International Peace.

Lazarsfeld, Paul F. and Robert K. Merton 1948. Mass Communication, Popular Taste and Organized Social Action, in Lyman Bryson (ed.), *Communication of Ideas,* Harper & Brothers.

Lévesque, Jacques. 2004. "The Emancipation of Eastern Europe, in Richard K. Hermann and Richard Ned Lebow (eds). *Ending the Cold War.* Palgrave Macmillan.

Lisann, Maury. 1975. *Broadcasting to the Soviet Union: International Politics and Radio.* Praeger Publishers.

Lohmann, Susanne. 1994. "The Dynamics of Informational Cascades: The Monday Demonstrations in Leipzig, East Germany, 1989–1991. *World Politics*, vol. 47, no. 1, 42–101.

Mayer, Wolfgang. 2002. *Flucht und Ausreise: Botschaftsbesetzungen als Form des Widerstands gegen die Politische Verfolgung in der DDR.* Berlin: Anita Tykve Verlag.

Manski, C. F. and Pepper, J.V. 2000. "Monotone Instrumental Variables: with an Application to the Returns to Schooling," *Econometrica*, vol. 68, no. 4, 997–1010.

Meyen, Michael. 2001. "Haben die Westmedien die DDR stabilisiert?" *Siegener Periodicum zur internationalen empirischen Literaturwissenschaft* 20 (1): 117–133.

Meyen, Michael. 2003a. *Einschalten, Umschalten, Auschalten? Das Fernsehen im DDR-Alltag.* Leipziger Universitätsverlag.

Meyen, Michael. 2003b. *Denver Clan und Neues Deutschland: Mediennutzung in der DDR.* Ch. Links Verlag.

Mondak, Jeffery J. 1995. "Media Exposure and Political Discussion in U.S. Elections," *Journal of Politics*, vol. 57, no. 1, 62–85.

Nelson, Michael. 1997. *War of the Black Heavens: The Battles of Western Broadcasting in the Cold War.* Syracuse University Press.

Newton, Kenneth. 1999. "Mass Media Effects: Mobilization or Media Malaise?" *British Journal of Political Science*, vol. 29, no. 4, 577–599.

ODonnell, Guillermo, Philippe C. Schmitter, and Laurence Whitehead (eds.) 1986. *Transitions from authoritarian rule: prospects for democracy.* Johns Hopkins University Press.

O'Neil, Patrick H. (ed.) 1998a. *Communicating Democracy: The Media and Political Transitions.* Lynne Rienner.

O'Neil, Patrick H. 1998b. Democratization and Mass Communication: What is the Link?, in Patrick H. O'Neil (ed.), *Communicating Democracy: The Media and Political Transitions.* Lynne Rienner.

Patterson, Thomas. 1993. *Out of Order.* Knopf.

Pridham, Geoffrey (ed.) 1991. *Encouraging democracy: the international context of regime transition in Southern Europe.* Leicester University Press.

Pridham, Geoffrey, Eric Herring, and George Sanford (eds.) 1997. *Building Democracy? The International Dimension of Democratization in Eastern Europe.* Leicester University Press.

Pridham, Geoffrey. 1997. "The International Dimension of democratization: theory, practice, and inter-regional comparisons," in Pridham, Geoffrey, Eric Herring, and George Sanford (ed.), Building Democracy? The International Dimension of Democratization in Eastern Europe. Leicester University Press.

Puddington, Arch. 2000. *Broadcasting Freedom: The Cold War Triumph of Radio Free Europe and Radio Liberty.* University Press of Kentucky.

Putnam, Robert D. 1995. " Tuning in, tuning out: the strange disappearance of social capital in America," *PS: Political Science & Politics,* vol. 28 , no. 4, 664–683.

Quester, George H. 1990. *The International Politics of Television.* Lexington Books.

Ranney, Austin. 1987. *Channels of Power: The Impact of Television on American Politics.* Basic Books.

Roberts, Adam. 1999. "Civil Resistance in the East European and Soviet Revolutions." Albert Einstein Institution Monograph Series no. 4.

Robins, J. and Rotnitzky, A. 2004. "Estimation of treatment effects in randomized trials with non-compliance and a dichotomous outcome using structural mean models," *Biometrika*, vol. 91, no. 4, 763–783.

Robinson, Michael J. 1976. "Public Affairs Television and the Growth of Political Malaise: The Case of 'Selling the Pentagon'," *American Political Science Review*, vol. 70, no. 2, 409–432.

Rosenbaum, Paul R. and Donald B. Rubin. 1983. "Assessing Sensitivity to an Unobserved Binary Covariate in an Observational Study with Binary Outcome," *Journal of the Royal Statistical Society, Series B*, vol. 45, no. 2, 212–218.

Rosenbaum, Paul R. 1984. "The consequences of adjustment for a concomitant variable that has been affected by the treatment," *Journal of the Royal Statistical Society Series A*, no. 147, 656–666.

Rosenbaum, P. R. 1987. "Assessing sensitivity to an unobserved binary covariate in an observational study with binary outcome," *Journal of the Royal Statistical Society Series B*, vol. 45, no. 2, 212–218.

Rosenbaum, Paul R. 2002. *Observational Studies.* 2nd edn. New York: Springer.

Rubin, Donald B. 1974. "Estimating Causal Effects of Treatments in Randomized and Non-randomized Studies," *Journal of Educational Psychology*, vol. 66, 688–701.

Rubin, Donald B. 1978. "Bayesian Inference for Causal Effects: The Role of Randomization," *Annals of Statistics*, vol. 6, no. 1, 1–26.

Rubin, Donald B. 1990. "Comment: Neyman (1923) and Causal Inference in Experiments and Observational Studies," *Statistical Science*, vol. 5, no. 4, 472–480.

Rustow, Dankwart A. 1990. "Democracy: A Global Revolution?," *Foreign Affairs*, vol. 69, no. 4, 75–91.

Schlesselmann, J. J. 1978. "Assessing the effects of confounding variables," *American Journal of Epidemiology*, vol. 108, 3–8.

Schmitt, Karl. 1993. Politische Landschaften im Umbruch: Das Gebiet der ehemaligen DDR 1928–1990, in Oscar W. Gabriel and Klaus G. Troitzsch (eds.) *Wahlen in Zeiten des Umbruchs.* Peter Lang.

Schmitter, Philippe C. 1996. The Influence of the International Context upon the Choice of National Institutions and Policies in Neo-Democracies , in Laurence Whitehead (ed.), *The International Dimensions of Democratization.* Oxford University Press.

Staatliche Zentralverwaltung für Statistik. 1989. *Statistisches Jahrbuch 1989.* Berlin: Staatsverlag der Deutschen Demokratischen Republik.

Stiehler, Hans-Jörg. 2001. *Leben ohne Westfernsehen.* Leipziger Universitätsverlag.

Süksd, Miklos. 2000. Democratic Transformation and the Mass Media in Hungary: From Stalinism to Democratic Consolidation, in Richard Gunther and Anthony Mughan (eds.), *Democracy and the Media.* Cambridge University Press.

Ten Have, T., Joffe, M., and Cary, M. 2003. "Causal logistic models for non-compliance under randomized treatment with univariate binary response," *Statistics in Medicine*, vol. 22, 1255-1283.

Vansteelandt, S. and Goetghebeur, E. 2003. "Causal inference with generalized structural mean models," *Journal of the Royal Statistical Society Series B*, vol. 65, no. 4, 817–835.

Vytlacil, E. J. 2000. "Semiparametric Identification of the Average Treatment Effect in Nonseparable Models." *Working Paper, Stanford University.*

Wand, J. 2002. "Evaluating the Consequences of Assumptions Using Simulations," *The Political Methodologist*, vol. 11, no. 1, 21–24.

Westad, Odd Arne. 2000. *Reviewing the Cold War.* Frank Cass.

Whitehead, Laurence (ed.) 1996a. *The International Dimensions of Democratization.* Oxford University Press.

Whitehead, Laurence 1996b. Three International Dimensions of Democratization, in Laurence Whitehead (ed.), *The International Dimensions of Democratization.* Oxford University Press.

Wolle, Stefan. 1998. *Die heile Welt der Diktatur. Alltag und Herrschaft in der DDR 1971–1989.* Berlin: Ch. Links Verlag.

Uunk, Wilfred, Bogdan W. Mach and Karl Ulrich Mayer. 2005. "Job Mobility in the Former East and West Germany: The Effects of State-Socialism and Labor Market Composition." *European Sociological Review* vol. 21, no. 4, 393–408.