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The Long-Term Costs of Government Surveillance: Insights from Stasi Spying in East Germany*

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Abstract. Despite the prevalence of government surveillance systems around the world, causal evidence on their social and economic consequences is lacking. Using county-level variation in the number of Stasi informers within Socialist East Germany during the 1980s and accounting for potential endogeneity, we show that more intense regional surveillance led to lower levels of trust and reduced social activity in post-reunification Germany. We also find substantial and long-lasting economic effects of Stasi spying, resulting in lower self-employment, higher unemployment and larger out-migration throughout the 1990s and 2000s. We further show that these effects are due to surveillance and not alternative mechanisms. We argue that our findings have important implications for contemporary surveillance systems.

Keywords: government surveillance, trust, social ties, East Germany
JEL codes: H11, N34, N44, P20

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1 Introduction

More than one third of the world population lives in authoritarian states that attempt to control almost all aspects of public and private life (The Economist Intelligence Unit, 2014). To these ends, those regimes install large-scale surveillance systems that infiltrate the population and generate a widespread atmosphere of suspicion reaching deep into private spheres (Arendt, 1951). Such environments of distrust are thought to have adverse economic effects, since they limit cooperation and the open exchange of ideas (Arrow, 1972, Putnam, 1995, La Porta et al., 1997, Algan and Cahuc, 2014). However, the empirical literature has not yet established a causal link between government surveillance, trust and economic performance.

In this paper, we quantify the effect of government surveillance on trust, social ties and long-run economic performance. To do so, we make use of administrative data on the large network of informers who once operated in the socialist German Democratic Republic (GDR) and link measures of regional government surveillance to post-reunification outcomes. The GDR Ministry for State Security, commonly referred to as the *Stasi*, administered a huge body of so-called *Informelle Mitarbeiter* – unofficial informers – that accounted for more than one percent of the East German population in the 1980s. The regime actually regarded its dense network of informers as the most important instrument to secure its power (Müller-Enbergs, 1996, p. 305). The informers were ordinary citizens who kept their regular jobs but also secretly gathered information within their professional and social network, thus betraying the trust of friends, neighbors and colleagues (Bruce, 2010). As the informers infiltrated private spheres, the damage done to social relations is thought to be large and persistent (Gieseke, 2014, p. 95).

To identify the long-term effects of surveillance, we exploit regional variation in the spying density across East German counties. An obvious concern is that the recruitment of spies across counties was non-random. We account for this non-randomness by adopting two different, complementary identification strategies. The first design exploits the specific administrative structure of the Stasi, whose county offices were subordinate to the respective state office. These state offices bore full responsibility to secure their territory and chose different strategies to do so, which led to different average levels of spying across states. Indeed, around 25% of the variation in the surveillance intensity across counties can be explained with state fixed effects. However, while surveillance policies varied across states, all economic and social policies were centrally decided by the politburo in East Berlin. This allows us to follow Dube et al. (2010) and use the discontinuities along state borders as a source of exogenous variation. For our second identification strategy, we follow Moser et al. (2014) and construct a county-level panel dataset covering both pre- and post-treatment years. This research design enables us to include county fixed effects to account for time-invariant confounders, say regional liberalism, that might have affected the recruitment of Stasi spies and may (still) have economic effects. Using pre-treatment data from the 1920s and early 1930s, we can also test for pre-trends in the outcome variables. Reassuringly, spying density cannot explain trends in economic performance prior to the division of Germany, which strengthens the causal interpretation of our findings.

Overall, the results of our study offer substantial evidence for negative and long-lasting effects

of government surveillance on peoples' trust, social ties and economic performance.[1] Using data from the German Socio-Economic Panel (SOEP), we find that a higher spying density leads to lower trust in strangers and stronger negative reciprocity. Both measures have been used as proxies for interpersonal trust in the literature (Glaeser et al., 2000, Dohmen et al., 2009). Individuals in counties with greater spying density also rate themselves as less sociable. These differences in beliefs and self-assessment are also reflected in individual social behavior, as we show the number of close friends to be significantly lower in counties with higher levels of surveillance. Moreover, individuals in counties with a higher informer density volunteer less frequently in associations, clubs or social services and are less engaged in local politics; both outcomes serving as common measures of social capital (Putnam, 2000, Satyanath et al., 2016). Hence, a higher density of informers among the population resulted in the undermining of trust, a withdrawal from society and an erosion of social capital.

The negative and persistent effect of a greater spying density on trust and social interactions is accompanied by negative and persistent effects on various measures of economic performance. Individual labor income and county-level self-employment rates are lower in counties with historically greater spying densities, while unemployment rates are higher. The effects are sizable. For example, our estimates imply that moving from the 75th to the 25th percentile in the intensity of surveillance would lower the long-term unemployment rate by 0.84 percentage points, which is equivalent to a 4.5 percent drop. We also document that the GDR surveillance system was a significant driver of the tremendous out-migration experienced in East Germany after reunification.

Our empirical results are robust to a number of sensitivity checks. First, we demonstrate the importance of accounting for the non-randomness of informer recruitment. The magnitudes of our estimates increase both when applying the border discontinuity design and controlling for historical confounders. This suggests that the endogeneity of the informer density biases estimates towards zero. Furthermore, we provide evidence that our effects are indeed driven by differences in the intensity of surveillance and are neither caused by local variation in socialist indoctrination nor by differences in government transfers and subsidies that were paid to East German counties after reunification in order to rebuild public infrastructure. We further test whether second-round migration and/or income effects drive our results, finding no evidence of such indirect effects. Moreover, we show that our results are not due to selective out-migration in terms of skills and age.

This is the first study to show that government surveillance has a causal negative effect on economic performance. We study the case of socialist East Germany, but our findings also have important implications with regard to contemporary forms of mass surveillance in authoritarian states. While we fully acknowledge the differences in the specifics and intensities of surveillance programs across regimes and time, a common feature of such programs is to exploit social networks to gather information (Arendt, 1951). In fact, the intrusion into private spheres was already present in Roman times when the politician and orator Cicero feared that his private letters were intercepted by the rulers (Zurcher, 2013). Given this common denominator, it is likely that contemporary government

[1] The annual number of requests for disclosure of information on Stasi activity (*Bürgeranträge*) serves as a first indication that East German citizens are still affected by Stasi spying, even 25 years after the fall of the Iron Curtain. Figure A.1 in the Appendix plots the annual number of requests filed from 1992–2012. Unfortunately, there is no regional information on these requests, which could provide an interesting outcome for our analysis.

surveillance in authoritarian states such as China or Russia exerts similar negative effects on social interactions and economic performance.[2]

Our results also relate to recent developments in surveillance strategies of democratic countries, where the threat of global terrorism and political extremism has led to the implementation of large-scale surveillance programs in recent years. Optimal policy needs to balance benefits of surveillance, i.e., security, with its costs. However, the potential costs, such as the violation of human privacy rights, social repression and the undermining of sociability and trust (Haggerty and Samatas, 2010, Anderson, 2016), are rather intangible and difficult to measure. Our study (i) demonstrates that these social costs exist, (ii) shows that they are sizable and (iii) points to additional economic costs associated with the reduction in social interactions. Importantly, these effects seem to be independent of the surveillance technology.[3] There is, for instance, ample anecdotal evidence that the revelation of the NSA secrets affected ordinary peoples' trust in the government (see, e.g., Schneier, 2013). Moreover, a substantial share of people stated that they adjusted their use of telecommunication as a consequence of the Snowden affair (Pew Research Center, 2014).

Our study is closely linked to the steadily growing literature on culture, institutions and economic performance (see Alesina and Giuliano, 2015, for a recent survey). In particular, we complement the large literature providing (mostly cross-country) evidence on the long-term positive effects of institutional quality on economic performance (Mauro, 1995, Hall and Jones, 1999, Rodrik et al., 2004, Nunn, 2008, Tabellini, 2010, Nunn and Wantchekon, 2011, Acemoglu et al., 2015). Econometrically, we refine current identification strategies to estimate causal effects of formal institutions on culture and economic outcomes by combining regional, within-country variation (Tabellini, 2010, Alesina et al., 2013) with spatial discontinuity designs (Becker et al., 2016, Fontana et al., 2016). In contrast to other studies, our identifying variation is not generated by deep, historical differences such as religion, ethnicity, or education, but induced by a rather recent, pervasive political experiment.

Our paper also speaks to the literature on trust, social capital and economic performance (see, e.g., Algan and Cahuc, 2014, Fuchs-Schündeln and Hassan, 2015, for recent surveys). Specifically, we highlight the importance of trust and social ties for economic prosperity (Knack and Keefer, 1997, Zak and Knack, 2001, Guiso et al., 2006, Algan and Cahuc, 2010, Burchardi and Hassan, 2013, Butler et al., 2016). In line with La Porta et al. (1997), our findings point to reduced entrepreneurial activity as an important channel for the economic decline. Given the intergenerational transmission of trust and reciprocity (Nunn and Wantchekon, 2011, Dohmen et al., 2012), it is likely that the East German surveillance regime will lead to a long-lasting deterioration of trust and social interactions.

Last, we contribute to the literature investigating the transformation of former countries of the Eastern bloc after the fall of the Iron Curtain (see, e.g., Shleifer, 1997). For the German case, we complement the evidence provided by Alesina and Fuchs-Schündeln (2007) and show that the East

[2] In China, the government tries to demobilize protesters by inducing pressure via the critics' social network (Deng and O'Brien, 2013). Various accounts further demonstrate that the one-party state still heavily relies on a large network of informers (see, e.g., Branigan, 2010, Jacobs and Ansfield, 2011, Yu, 2014). Likewise, Russia has been observed to re-implement surveillance strategies, in which secret informers and denunciations play an important role to control oppositional forces (Capon, 2015).

[3] While technological progress has enabled governments to intercept a substantial share of the personal communication electronically, informants still constitute an important element of the surveillance strategy. Consider, for instance, Stabile (2014) for a discussion of legal problems associated with the FBI informant recruitment, or the current case of the Orlando shooting (Lichtbaum and Apuzzo, 2016).

German regime not only affected individual preferences for redistribution, but also had long-lasting effects on trust and social behavior. In contrast to Alesina and Fuchs-Schündeln (2007), we exploit variation *within* East Germany rather than estimating the total effect of being exposed to the socialist regime by comparing East and West German individuals. There are two related papers, which also exploit the number of informers to identify post-reunification effects.[4] However, both papers do not account for the potential non-randomness in the regional spying density.[5] The first one by Jacob and Tyrell (2010) looks at the impact of surveillance on social capital, the second one focuses on personality traits (Friehe et al., 2015). Moreover, our paper also investigates long-run *economic* effects. Our negative estimates complement recent findings by Fuchs-Schündeln and Masella (2016), who show long-run negative effects of socialist education on labor market outcomes.

The remainder of this paper is organized as follows. Section 2 presents the historical background and the institutional framework of the Stasi. Section 3 describes the data, while Section 4 investigates potential determinants of the informer density across counties. Section 5 introduces our research design and explains the two different identification strategies. Results are presented in Section 6, before Section 7 concludes.

2 Historical background

After the end of World War II and Germany's liberation from the Nazi regime in 1945, the remaining German territory was occupied by and divided among the four Allied forces – the US, the UK, France and the Soviet Union. The boundaries between these zones were drawn along the territorial boundaries of 19th-century German states and provinces that were "economically well-integrated" (Wolf, 2009, p. 877) when the Nazis gained power. On July 1, 1945, roughly two months after the total and unconditional surrender of Germany, the division into the four zones became effective. With the Soviet Union and the Western allies disagreeing over Germany's political and economic future, the borders of the Soviet occupation zone soon became the official inner-German border and eventually led to a 40-year long division of the society. In May 1949, the Federal Republic of Germany was established in the three western occupation zones. Only five months later, the German Democratic Republic, a state in the spirit of "real socialism"[6] and a founding member state of the Warsaw Pact, was constituted in the Soviet ruled zone. Until the sudden and unexpected fall of the Berlin Wall on the evening of November 9, 1989 and the reunification of West and East Germany in October 1990, the GDR was an authoritarian regime under the rule of the Socialist Unity Party (SED) and its secretaries general.

In February 1950, just a few months after the constitution of the GDR, the Ministry for State Security was founded. The Stasi served as the internal (and external) intelligence agency of the socialist regime and was designed to "battle against agents, saboteurs, and diversionists [in order] to

[4] In addition, Glitz and Meyersson (2016) exploit information provided by East German foreign intelligence spies in the *West* to investigate the economic returns to industrial espionage.

[5] We show below that simple OLS estimates are upward biased. For instance, the correlation between trust – the most frequently used measure of social capital – and the spying density across counties turns out to be positive.

[6] Erich Honecker, Secretary General of the Socialist Unity Party between 1971–1989, introduced this term on a meeting of the Central Committee in May 1973 to distinguish the regimes of the Eastern bloc from Marxist theories on socialism.

preserve the full effectiveness of [the] Constitution."[7] After the unforeseen national uprising on and around June 17, 1953 had revealed the weakness of the secret security service in its infant years, the Stasi remarkably expanded its activities and soon turned into a ubiquitous institution, spying on and suppressing the entire population to ensure and preserve the regime's power (Gieseke, 2014, p. 50ff.). The key feature of the Stasi's surveillance strategy was the use of "silent" methods of repression rather than legal persecution by the police (Knabe, 1999). To these ends, the Stasi administered a dense network of unofficial informers, the regime's "main weapon against the enemy"[8], who secretly gathered detailed inside knowledge about the population. In the 1980s, the Stasi listed around 85,000 regular employees and 175,000 unofficial informers, which accounted for around 0.5 and 1.05 percent of the population, respectively.[9] With the collected intelligence at hand, the Stasi was able to draw a detailed picture of anti-socialist and dissident movements within the society and to exert an overall "disciplinary and intimidating effect" on the population (Gieseke, 2014, p. 84f.).

In order to extract information from the population, the Stasi relied on a highly decentralized administrative structure, which was at odds with the overall centralist organization of the GDR. While the main administration was located in East Berlin, the Stasi maintained state offices (*Bezirksdienststellen*) in each capital of the fifteen states, regional offices (*Kreisdienststellen*) in most of the 226 counties and offices in seven Objects of Special Interest, which were large and strategically important public companies or universities (*Objektdienststellen*).[10] State offices bore full responsibility to secure their territory and had authority over their subordinate offices in the respective counties. As a consequence, surveillance strategies differed in their intensities across GDR states. For instance, around one-third of the constantly-monitored citizens (*Personen in ständiger Überwachung*) were living in the state of Karl-Marx-Stadt (Horsch, 1997), which accounted for only 11 percent of the total population. Likewise, the state of Magdeburg accounted for 17 percent of the two million bugged telephone conversations, while this state only accounted for eight percent of the total GDR population in turn. We exploit this variation in surveillance intensities across states for identification (see Section 5.1).

The majority of informers were recruited by the regional offices and instructed to secretly collect information about individuals in their own network. Hence, it was necessary that informers pursued their normal lives, being friends, colleagues and neighbors, after recruitment. The Stasi administrated the body of informers in a highly formalized way, with cooperation being sealed in written agreements and informers being tightly led by a responsible Stasi officer (Gieseke, 2014, p. 114ff.). Informers would regularly and secretly meet with their officer, report suspicious behavior and provide personal information about individuals in their social networks. Reasons for serving as a collaborator were diverse. Some citizens agreed to cooperate due to ideological reasons, others were attracted by personal and material benefits accompanied with their cooperation. However, the

[7] According to Erich Mielke, subsequent Minister for State Security from 1957 to 1989, on January 28, 1950 in the official SED party newspaper *Neues Deutschland* as quoted in Gieseke (2014, p. 12).

[8] Directive 1/79 of the Ministry for State Security for the work with unofficial collaborators (Müller-Enbergs, 1996, p. 305).

[9] The number of regular Stasi employees was notably high when being compared to the size of other secret services in the Eastern Bloc (Gieseke, 2014, p. 72). While figures on the number of spies in other communist countries entail elements of uncertainty, other studies suggest that the level of spies in the GDR was at least as high as in other countries of the Eastern bloc in the years preceding the fall of the Iron Curtain (Albats, 1995, Harrison and Zaksauskiene, 2015).

[10] The Stasi only monitored economic activity but was not actively involved in economic production (Gieseke, 2014).

regime also urged citizens to act as unofficial informers by creating fear and pressure. In a 1967 survey of unofficial informers, 23 percent of the collaborators indicated that pressure and coercion led to recruitment (Müller-Enbergs, 2013, p. 120).

The threat of being denunciated caused an atmosphere of mistrust and suspicion within a deeply torn society (Wolle, 2009). Citizens felt the Stasi's presence like a "scratching t-shirt" (Reich, 1997, p. 28).[11] The constant surveillance had perceivable real-life consequences, ranging from students being denied the opportunity to study at the university, or teachers and factory workers being dismissed (Bruce, 2010, p. 103f.) to more serious ramifications like physical violence, abuse and sometimes even imprisonment.

3 Data

In this section, we briefly describe the various data sources collected for our empirical analysis. Section 3.1 presents information on our explanatory variable, the spying density in a county. Section 3.2 and Section 3.3 describe the data used to construct outcome measures and control variables. Detailed information on all variables are provided in Appendix Table B.1. The Data Appendix B also provides details on the harmonization of territorial county borders over time.

3.1 Stasi data

Information on the number of unofficial informers in each county is based on official Stasi records, published by the Agency of the Federal Commissioner for the Stasi Records and compiled in Müller-Enbergs (2008). Although the Stasi was able to destroy part of its files in late 1989, much information was preserved when protesters started to occupy Stasi offices across the country. In addition, numerous shredded files could be restored after reunification. Since 1991, individual Stasi records have been available for personal inspection as well as requests from researchers and the media.

Measuring surveillance intensity. Given that the Stasi saw unofficial collaborators as their main instrument of surveillance, we choose the county-level share of informers in the population as our preferred measure of the intensity of surveillance. Most regular Stasi officers were based in the headquarters in Berlin, and only 10-12 percent of them were employed at the county level. In contrast, the majority of all unofficial collaborators was attached to county offices. The Stasi differentiated between three categories of informers: (1) collaborators for political-operative penetration, homeland defense, or special operations as well as leading informers, (2) collaborators providing logistics and (3) societal collaborators, i.e., individuals publicly known as loyal to the state. We use the first category of unofficial collaborators (operative collaborators) to construct our measure of surveillance density, as those were actively involved in spying and are by far the largest and most relevant group of collaborators. If an Object of Special Interest with a separate Stasi office was located in a

[11] For more popular documentations on the impact of the Stasi, see the Academy Award winning movie "The Lives of Others" and the recent TED talk "The dark secrets of a surveillance state" given by the director of the Berlin-Hohenschönhausen Stasi prison memorial, Hubertus Knabe.

county, we add the number of unofficial collaborators attached to these object offices to the respective county's number of spies.[12] As information on the total number of collaborators are not given for each year in every county, we use the average share of informers from 1980 to 1988 as our measure of surveillance.[13] The spying density in a county was very stable across the 1980s, the within-county correlation being 0.91. For further details on our main explanatory variable, see Data Appendix B.

Variation in surveillance intensity. Figure 1 plots the spying density for each county. Today, the number of informers is known for about 90 percent of the counties for at least one year in the 1980s. The density of spies differs considerably both across and within GDR states, with the fraction of unofficial collaborators in the population ranging from 0.12 to 1.03 percent and the mean density being 0.38 percent (see Table A.2 for more detailed distributional information).[14] The median is similar to the mean (0.36 percent), and one standard deviation is equal to 0.14 informers per capita, which is more than one third of the mean spying density. In our regressions, we standardize the share of informers by dividing it by one standard deviation.

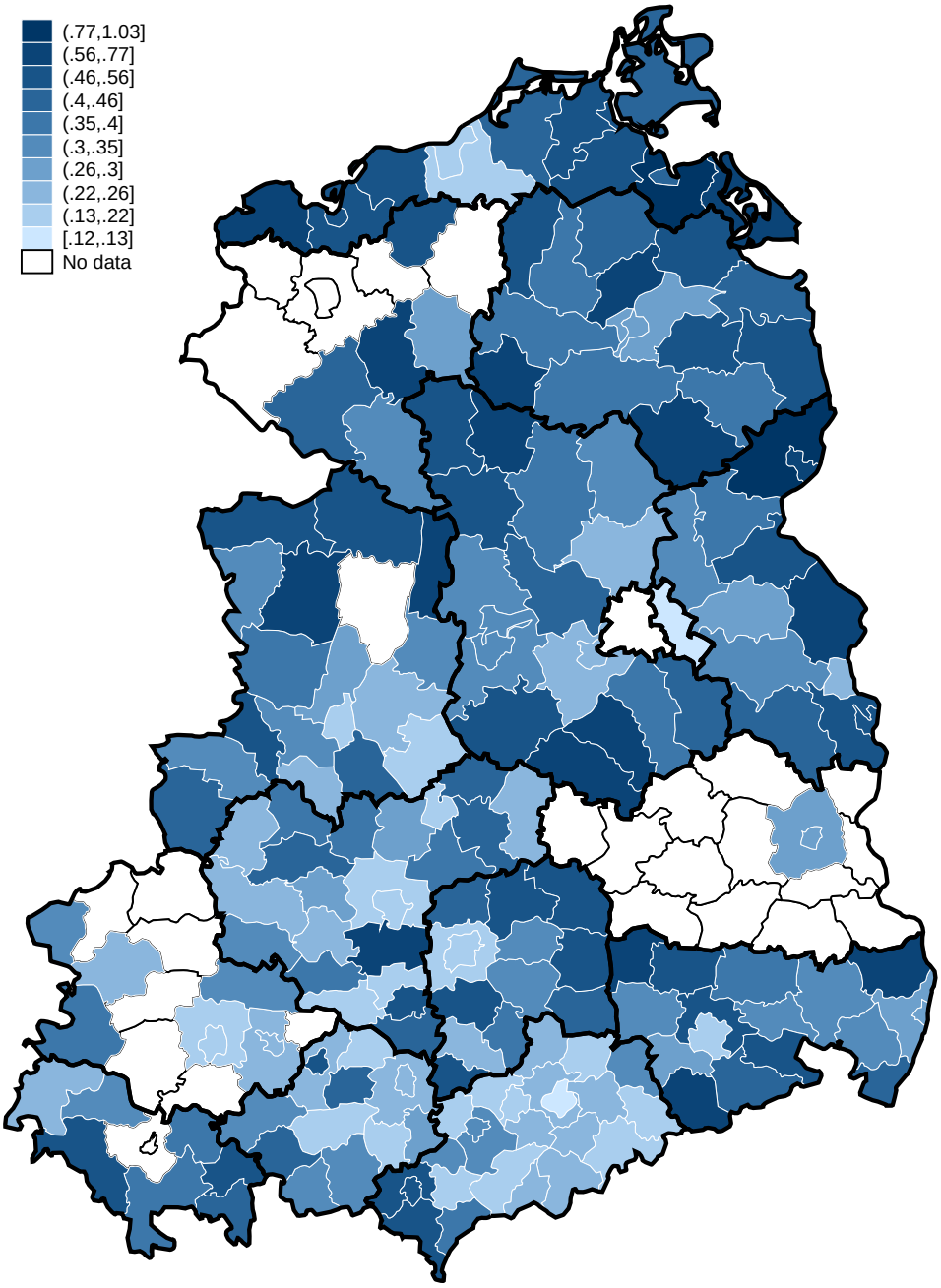
In order to identify the effects of state surveillance on trust and economic performance in the present setting, it is crucial that existing differences in the intensity of surveillance across East Germany significantly affected the population. Historical accounts suggest that the transmission occurred both consciously and unconsciously. Bruce (2010, p. 146) documents that the East German population was aware of the large number of informants at work, at restaurants, and in public places. Moreover, a large share of the population "had encountered the Stasi at one point or another in their lives, but these experiences varied greatly" (Bruce, 2010, p. 148). Given the substantial variation in the spying density, our identifying assumption is that individuals living in counties with a higher informer density were consciously or subconsciously more aware of government surveillance because they had more frequent/intense contact with the surveillance system.

Alternative measures of surveillance intensity. As discussed in Section 2, silent surveillance measures seem more appropriate to capture the repressive nature of the regime, given that the Stasi's main strategy was to scare regime opponents into terminating their activities (Bruce, 2010, p. 130). Among these silent measures, we choose the number of operative collaborators (category 1) per capita as our main regressor given their active role in spying. Moreover, data coverage is highest for this type of informant and we would lose 30 counties when basing our measure of surveillance on all three types of informers. However, as indicated by Panel A of Figure A.2 in the Appendix, this choice does not appear to be crucial as implied by the very high correlation between operative informers and the total number of collaborators ($\rho = 0.95$).

Although most official Stasi employees were based in East Berlin, the number of county officers constitutes another alternative measure of the regional intensity of surveillance. As before, Panel B of Figure A.2, however, shows that the number of regular employees and operative collaborators is highly correlated, which seems reasonable given that informers had to be administered by official

[12] In the empirical analysis, we explicitly control for the presence of such offices in Objects of Special Interest.
[13] Data from earlier years are only available for a limited number of counties.
[14] Note that these figures only relate to operative collaborators at the county level (informer category 1), which explains the lower mean in the spying density compared to the overall share of informers in the population (cf. Section 2).

Figure 1: Share of Operative Unofficial Informers at the County Level



Notes: This graph plots the county-level surveillance density measured by the average yearly share of operative unofficial informers relative to the population between 1980 and 1988. Thick black lines show the borders of the fifteen GDR states. White areas indicate missing data. *Map:* MPIDR and CGG, 2011.

employees in the respective county offices. Given the importance of unofficial informants as "the main weapon" of the Stasi, we choose the density of operative informers as our baseline explanatory variable. We find slightly smaller, but qualitatively similar effects when using the share of regular officers instead. Taking the total number of informers does not affect our results.

3.2 Individual-level data

For the empirical analysis presented below, we rely on two distinct datasets to estimate the effects of state surveillance on trust and economic performance. First, we use information from the German Socio-Economic Panel Study (SOEP), a longitudinal survey of German households (Wagner et al., 2007).[15] Established for West Germany in 1984, the survey covers respondents from the former GDR since June 1990. The SOEP contains information on the county of residence and when individuals have moved to their current home. We identify and select respondents living in East German counties in 1990 who have not changed residence in 1989 or 1990.[16] We then follow these individuals from the 1990 wave of the SOEP over time. By exploiting a variety of different waves of the survey, we are able to observe various measures of trust and social relations as well as current gross labor income (see Section 5.1 and Data Appendix B).

In order to proxy trust, we use two standard measures provided in the SOEP: trust in strangers as specified in Glaeser et al. (2000), and the negative reciprocity index proposed by Dohmen et al. (2009). To capture individual social behavior, we focus on the number of close friends and self-assessed sociability, the latter being one of the three components of the Big Five personality trait Extraversion. Last, we measure societal engagement by individuals' volunteering in clubs or social services and by their engagement in local politics. We also use monthly gross labor income out of regular employment reported in the SOEP as an individual-level measure of economic performance.

Moreover, we rely on the rich survey information to construct a set of individual control variables: gender, age, household size, marital status, level of education and learned profession. Summary statistics are presented in Table A.1; for information on the underlying survey questions, data years and exact variable definitions, see Data Appendix B.

3.3 County-level data

For the second dataset, we compiled county-level data on various measures of economic performance (self-employment, unemployment, population). We collected county-level data for two time-periods, data from the 1990s and 2000s as well as pre World War II data.[17] Both post-reunification data and historical data come from official administrative records (see Data Appendix B for details).

We further collect various county-level variables as controls. We use these to (i) explain differences in the Stasi density (cf. Section 4), and (ii) as control variables to check the sensitivity of our estimates. In total, we construct four sets of county-control variables. The first set of variables accounts for the *size* and *demographic composition* of the counties in the 1980s. Therefore, we collect information on the mean county population in the 1980s and the area of each county. Moreover, we use information on counties' demographic composition as of September 30, 1989 to construct variables indicating the share of children (population aged below 15) and the share of retirees (population aged above 64) in each county.

[15] Precisely, we use Socio-Economic Panel (SOEP), data for years 1984-2012, version 29, SOEP, 2013, doi: 10.5684/soep.v29.

[16] As discussed below, residential mobility within the GDR was highly restricted.

[17] Unfortunately, there are no annual county-level data for self-employment and unemployment for post-reunification East Germany in the years from 1990 to 1995. We filed several data requests to the various federal and state statistical offices and were informed that the information is simply not available due to the federal structure of the German statistical office system paired with the turbulences following reunification.

The second set measures the strength of the *opposition* to the regime. As mentioned in Section 2, the national uprising on and around June 17, 1953 constituted the most prominent rebellion against the regime before the large demonstrations in late 1989. The riot markedly changed the regime's awareness for internal conflicts and triggered the expansion of the Stasi informer network. We use differences in the regional intensity of the riot to proxy the strength of the opposition. Specifically, we construct three control variables: (i) an ordinal measure of the strike intensity with values "none", "strike", "demonstration", "riot", and "liberation of prisoners", (ii) a dummy variable indicating whether the regime declared a state of emergency in the county and (iii) a dummy equal to one if the Soviet military intervened in the county.

The third set of controls takes into account county differences in the industry composition. Our set of *industry* controls comprise (i) the 1989 share of employees in the industrial sector and the share in the agricultural sector, (ii) the goods value of industrial production in 1989 (in logs)[18], (iii) a dummy variable indicating whether a large enterprise from the uranium, coal, potash, oil or chemical industry was located in the county, and (iv) a measure of the relative importance of one specific industrial sector for overall industrial employment (i.e., the 1989 share of employees in a county's dominant industry sector over all industrial employees). This measure is intended to address potential concerns that important industries dominated certain regions during the GDR regime but then became unimportant after reunification.

The fourth set of controls is intended to pick up historical and potentially persistent county differences in terms of economic performance and political ideology. It will be used in the models on the individual level in the absence of pre-treatment information on the outcomes. Our *pre World War II* controls include (i) the mean Nazi and Communist vote shares in the federal elections of 1928, 1930 and the two 1932 elections to capture political extremism (Voigtländer and Voth, 2012), (ii) average electoral turnout in the same elections to proxy institutional trust, (iii) the regional share of protestants in 1925 in order to control for differences in work ethic and/or education (Becker and Wößmann, 2009), (iv) the share of self-employed in 1933 to capture regional entrepreneurial spirit and (v) the unemployment rate in 1933 to capture pre-treatment differences in economic performance.

Summary statistics for all county-level variables are presented in Table A.2; for information on the sources, data years and exact variable definitions, see Data Appendix B.

4 Explaining the informer density

In this section, we try to explain county differences in the informer density. Astonishingly, there is very little knowledge regarding the determinants of regional spying density. Some anecdotal evidence suggests that the Stasi was particularly active in regions with strategically important industry clusters. In contrast and somewhat surprisingly, previous historical research could not establish a clear correlation between the density of spying and the size of the opposition at the county level (Gieseke, 1995, p. 190). In order to shed some light on the determinants of the regional surveillance intensity, we run simple OLS regressions of the spying density on five sets of potentially important variables: (i) county size and demographic structure, (ii) county-level oppositional strength,

[18] We drop the county Plauen-Land due to missing data for this variable.

(iii) county industry composition, (iv) county-level pre World War II characteristics, and (v) GDR state-level characteristics (control sets are defined as above, see Section 3.3). We check the importance of each set of controls in explaining the county-level variation in the spying intensity as indicated by (partial) R^2 measures.

Table 1 reports the regression results. We start off by explaining the spying density with a constant and a dummy variable, which is equal to one if one of the seven offices in Objects of Special Interest, that is, an institution (company or university) of strategic importance, was located in the county.[19] In the next specification, we add variables controlling for the size and demographic structure of a county. While the spying density already accounts for differences in county population, we add the log mean county population in the 1980s and the log square meter area of the county as regressors. We find that the spying *density* decreases in the population, which could be rationalized with an economies of scale argument. In addition, we account for the demographic composition of each county by including the share of adolescents as well as the share of retirees. We find that controlling for demographic characteristics and size – in particular population – increases the explanatory power substantially, raising the overall R^2 of the model from 0.03 to 0.38.[20]

In the third column of Table 1 we add variables capturing the oppositional strength at the county level. We verify the results established by historical researchers that the intensity of the opposition to the regime does not explain much of the spying density, as revealed by the low partial R^2 measure of 0.035. In column (4), we control for the industry composition of the counties, by adding the share of industrial and agricultural employment, a dummy variable for the presence of strategic industries, a measure of the industry concentration and the value of industrial production. The partial R^2 of 0.227 indeed shows that the industrial structure is an important determinant of the spying density. However, much of the effect seems to be captured by controlling for the (population) size of a county, as the overall model fit only increases marginally.

In the fifth specification of Table 1, we add pre World War II controls, which reflect both the political orientation of a county and its 1920/1930 economic situation. Again, this set of variables can explain approximately 20 percent of the variation in the spying density, but the model fit does not improve when conditioning on the other controls. In the last and most comprehensive model, we add dummy variables for the fifteen GDR states, which non-parametrically account for differences in the local spying density due to state-level characteristics. Notably, GDR state fixed effects are an important determinant of the informer density, as can be seen from both the partial R^2 as well as the increase in the overall fit of the model.

In the most comprehensive model, we find that the spying density is higher in counties with fewer inhabitants, counties with a higher share of the working-age population and an Object of Special Interest. We also find that the intensity of surveillance is higher in counties where the Soviet military intervened in the riot of 1953, where the Nazi party received a higher vote share in the late 1920s and early 1930s and where the share of protestants is lower. In order to check the sensitivity of our

[19] As described in Section 3.1, the Stasi maintained offices in these objects, which recruited their own informers. As we add the collaborators working in these object offices to the number of informers in the respective county office, we control for offices in Objects of Special Interest with a dummy variable in all regressions below.

[20] The choice of log population seems to be very reasonable in terms of functional form. Using higher-order polynomials of population does not increase the explanatory power of the model.

Table 1: Determinants of the County-Level Informer Density

	(1)	(2)	(3)	(4)	(5)	(6)
Dummy: Object of Special Interest	1.132	1.710***	1.710***	1.718***	1.780***	1.981***
	(0.875)	(0.522)	(0.535)	(0.578)	(0.559)	(0.535)
Log mean population 1980s		-0.868***	-0.916***	-1.030***	-1.122***	-1.328***
		(0.107)	(0.115)	(0.197)	(0.237)	(0.252)
Log county size		0.125*	0.136*	0.234**	0.323***	0.206*
		(0.072)	(0.076)	(0.109)	(0.115)	(0.121)
Share of population aged above 64		-0.108**	-0.099*	-0.057	-0.102	-0.154*
		(0.052)	(0.055)	(0.068)	(0.072)	(0.088)
Share of population aged below 15		-0.025	-0.028	0.007	-0.057	-0.237**
		(0.070)	(0.073)	(0.088)	(0.094)	(0.105)
Uprising intensity 1953: Strike			0.062	0.031	0.035	-0.072
			(0.172)	(0.187)	(0.186)	(0.187)
Uprising intensity 1953: Demonstration			-0.144	-0.179	-0.240	-0.197
			(0.179)	(0.191)	(0.190)	(0.204)
Uprising intensity 1953: Riot			-0.259	-0.249	-0.322	-0.379
			(0.243)	(0.246)	(0.254)	(0.265)
Uprising intensity 1953: Prisoner liberation			-0.157	-0.220	-0.145	-0.161
			(0.241)	(0.243)	(0.246)	(0.272)
Dummy: Military intervention 1953			0.164	0.155	0.230	0.308*
			(0.156)	(0.154)	(0.168)	(0.169)
Dummy: State of emergency 1953			0.218	0.218	0.238	-0.014
			(0.146)	(0.156)	(0.174)	(0.200)
Share agricultural employment 1989				-0.018	-0.015	-0.013
				(0.016)	(0.016)	(0.014)
Share industrial employment 1989				-0.011	-0.012	-0.010
				(0.012)	(0.013)	(0.012)
Dummy: Important industries 1989				-0.096	-0.097	-0.100
				(0.160)	(0.164)	(0.156)
Industry concentration 1989				0.007	0.007	0.003
				(0.006)	(0.007)	(0.007)
Log goods value of industrial production 1989				0.022	0.048	0.092
				(0.100)	(0.102)	(0.103)
Mean electoral turnout 1928–1932					-0.035	-0.001
					(0.031)	(0.042)
Mean vote share Nazi party 1928–1932					0.008	0.040*
					(0.020)	(0.021)
Mean vote share communist party 1928–1932					-0.040**	-0.008
					(0.016)	(0.022)
Share protestants 1925					0.004	-0.016*
					(0.008)	(0.009)
Share unemployed 1933					0.038	0.014
					(0.024)	(0.025)
Share self-employed 1933					-0.044	0.031
					(0.042)	(0.061)
GDR state fixed effects	No	No	No	No	No	Yes
Observations	186	186	186	186	186	186
R^2	0.034	0.380	0.399	0.409	0.431	0.587
Adjusted R^2	0.028	0.363	0.361	0.353	0.354	0.487
Partial R^2		0.306	0.035	0.227	0.190	0.270

Notes: This table shows OLS coefficients of regressing the mean county-level informer density in the 1980s on different sets of control variables. Robust standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). For details on the sources and construction of the variables, see Appendix Table B.1.

results, we account for different sets of control variables in both research designs laid out below.[21]

[21] As noted above, we account for long-term, pre World War II differences in county characteristics in the panel data

Overall, we are able to explain around 60 percent of the variation in spying density at the county level. Importantly, different average informer densities between GDR states explain around 25 percent of the county-level variation. This is an important insight in line with the claim of historians that county offices responded to higher-ranked state offices and that decisions made at the state level indeed affected the respective county offices of the Stasi. We will exploit this institutional feature of the Stasi for identification by implementing a state border discontinuity design in Section 5.1.

5 Research designs

As shown in Section 4, we can explain roughly 60 percent of the regional variation in the spying density across counties by means of observable differences in county characteristics. In order to establish causality between the informer density and any outcome of interest, we have to make sure that remaining differences in the intensity of spying are not driven by unobserved confounders. If, for instance, the Stasi was strong in counties that have been traditionally liberal, and these counties in turn perform better in the capitalist system post-reunification, estimates would be biased. In the following subsections, we present two research designs that address potential endogeneity concerns.

Before turning to the two distinct identification strategies in Sections 5.1 and 5.2, we first argue that selection out of treatment, i.e., people moving away from counties with high levels of state surveillance, is likely to be of minor importance given the very specific institutional setting in East Germany. First, after the construction of the Berlin Wall, leaving the GDR was extremely dangerous. The regime installed land-mines along the border and instructed soldiers to shoot at citizens trying to flee. The regime also often punished those individuals who applied for emigration visas, exposing people to considerable harassment in working and private life (Kowalczuk, 2009). As a consequence, migration to West Germany was rare with only around 18,000 individuals (0.1 percent of the population) managing to leave East Germany each year, either by authorized migration (*Übersiedler*) or illegal escape (see Figure A.3 in the Appendix). Second, residential mobility *within* the GDR was highly restricted. All living space was tightly administered by the GDR authorities: in every municipality, a local housing agency (*Amt für Wohnungswesen*) decided on the allocation of all houses and flats, and assignment to a new flat was usually subject to the economic, political or social interests of the regime (Grashoff, 2011, p. 13f.). Using data on the county population and the number of informers in multiple years in the 1980s, we can directly test whether the spying density affected population size *prior* to the fall of the Berlin Wall. Reassuringly, we find no effect of the log number of informers on log population in a model including county and year fixed effects. Hence, selection out of treatment does not seem to be an issue in our setting. Third, we are able to follow individuals who moved after the fall of the Berlin Wall in our individual-level analysis using SOEP data. We assign treatment (i.e., the spying density) based on the county of residence in 1989.

5.1 Border discontinuity design

Our first identification strategy exploits the administrative structure of the Stasi. Each Stasi office at the state-level bore the responsibility to secure its territory (see, e.g., Bruce, 2010, p. 111, and Gieseke,

design by including pre-treatment outcomes and county fixed effects.

2014, p. 82). As a consequence, different GDR states administered different average levels of informer density at the county level. As shown in Table 1, about 25 percent of the county-level variation in the spying density can be explained with GDR state fixed effects. We use the resulting discontinuities along state borders as a source of exogenous variation (see, e.g., Holmes, 1998, Magruder, 2012, Agrawal, 2015, for studies applying similar research designs). We closely follow Dube et al. (2010) and limit our analysis to all contiguous counties that straddle a GDR state border.

The identifying assumption is that the county on the lower-spying side of the border is similar to the county on the higher-spying side in all other relevant characteristics. We test the smoothness of observable county characteristics at state borders within border county pairs below. Importantly, we have to make sure that there are no other policy discontinuities at state borders. This is very likely to be fulfilled, given that the GDR was a highly centralized regime. All economic and social policies were dictated by the politburo in East Berlin, and individual states had no legislative authority: "The main task of the state administrations was to execute the decision made by the central committee. This was their raison d'être."[22] In addition, our identifying assumption could be compromised if (i) informers administered by one county collected information on people located in the neighboring county within the same border county pair, or if (ii) there was a quantity-quality trade-off in terms of unofficial collaborators. Both concerns would work against us and bias our estimates towards zero.

Formally, we regress individual outcome i in county c, which is part of a border county pair b, on the spying density in county c and county pair dummies ν_b:

$$Y_{icb} = \alpha + \beta \times SPYDENS_c + \boldsymbol{X_i'}\boldsymbol{\delta} + \boldsymbol{K_c'}\boldsymbol{\phi} + \nu_b + \varepsilon_{icb}. \quad (1)$$

As outcome variables, Y_{icb}, we use trust in strangers, negative reciprocity, the number of close friends, self-assessed sociability, volunteering in clubs, participation in local politics and log gross labor income (see Section 3.2).

To assess the sensitivity of our estimates with respect to potential confounders, we include various sets of control variables, summarized in vectors $\boldsymbol{X_i}$ and $\boldsymbol{K_c}$. Vector $\boldsymbol{X_i}$ includes individual-level compositional controls, whereas vector $\boldsymbol{K_c}$ covers county-level controls, which capture differences in size, oppositional strength, industry composition and pre World War II characteristics. Reassuringly, we find that estimates are not strongly affected by the inclusion of these controls. Rather, the inclusion of county controls increases the absolute value of the coefficients, which suggests that omitted variables are likely to bias our estimates towards zero.[23] For this reason, the richest specification including all covariates is our preferred one. As most of our SOEP outcomes are observed in two survey waves (see Data Appendix B), we pool the observations and add year fixed effects to our model.[24]

We use the cross-sectional weights provided by the SOEP to make the sample representative for the whole population. If a county has several direct neighbors on the other side of the state border, we duplicate the observation and adjust sample weights. In addition, standard errors are two-way

[22] Ulrich Schlaak, Second Secretary of the SED in the state of Potsdam, cited in Niemann (2007, p. 198, own translation).

[23] This is in line with the example of regional liberalism as an omitted confounder, which should also bias estimates towards zero.

[24] We account for correlation of error terms within-individuals across waves by clustering at the 1990-county-level, which nests individuals. Results are also robust when clustering two-way at the county pair and individual level.

clustered at the county *and* county pair level. We test the robustness of our results by (i) disregarding cross-sectional weights and only accounting for duplications and (ii) by using original cross-sectional weights, not adjusting for duplicates. Results (shown in Appendix Table A.3) prove to be robust to these modifications.

Table 2: Covariate Smoothness at GDR State Borders

	Unconditional		Cond. on population	
	(1) Estimate	(2) S.E.	(3) Estimate	(1) S.E.
Log mean population 1980s	-0.219***	(0.076)		
Share of population aged below 15	0.282*	(0.155)	0.110	(0.160)
Share of population aged above 64	-0.139	(0.148)	-0.193	(0.166)
Log county size	-0.033	(0.046)	-0.055	(0.063)
Log goods value of industrial production 1989	-0.372**	(0.165)	-0.025	(0.125)
Share industrial employment 1989	-2.211	(1.348)	-0.730	(1.331)
Share agricultural employment 1989	2.630**	(1.249)	0.244	(1.220)
Share public sector employment 1989	0.287***	(0.084)	0.136	(0.084)
Dummy: Important industries 1989	-0.015	(0.056)	0.011	(0.067)
Industry concentration 1989	2.084	(1.382)	3.648**	(1.396)
Mean electoral turnout 1928–1932	-0.099	(0.279)	0.002	(0.311)
Mean vote share communist party 1928–1932	-0.477	(0.490)	-0.159	(0.469)
Mean vote share Nazi party 1928–1932	0.390	(0.510)	-0.005	(0.495)
Share protestants 1925	0.415	(0.421)	-0.274	(0.324)
Share unemployed 1933	-0.300	(0.468)	0.425	(0.395)
Share self-employed 1933	0.196	(0.242)	-0.009	(0.259)
Uprising intensity 1953: None	0.020	(0.065)	-0.027	(0.075)
Uprising intensity 1953: Strike	-0.007	(0.046)	0.008	(0.053)
Uprising intensity 1953: Demonstration	-0.059	(0.054)	-0.063	(0.064)
Uprising intensity 1953: Riot	0.040	(0.059)	0.058	(0.066)
Uprising intensity 1953: Prisoner liberation	0.006	(0.043)	0.024	(0.041)
Dummy: Military intervention 1953	0.095	(0.079)	0.127	(0.091)
Dummy: State of emergency 1953	0.113*	(0.067)	0.150*	(0.078)
Dummy: Object of Special Interest	0.063	(0.047)	0.085	(0.052)

Notes: This table summarizes the within state border county pair correlation between the informer density and several covariates. Estimates show the results from partial regressions of county-level variables on the spying density and a full set of county pair dummies. Estimates in column (1) are unconditional on log mean population in the 1980s, estimates in column (3) conditional on population. The sample includes 106 counties in 114 border county pairs. Weights are adjusted for duplications of counties that are part of multiple county pairs. Standard errors are two-way clustered at the county and border county pair level with usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). For information on all variables, see Appendix Table B.1.

Covariate smoothness. A crucial assumption in discontinuity designs is that other covariates that affect the outcome are continuous at the threshold. In our case, this implies that variables other than the spying density should be smooth at state borders within county pairs. In particular, our identification strategy would be challenged if there were *persistent* compositional or historical differences within county pairs at state borders, which are likely to have affected the recruitment of spies in the 1980s as well as post-reunification outcomes. For this reason, we provide a covariate smoothness test common in discontinuity designs. Explicitly, we regress different county-level characteristics on the spying density and a full set of county pair fixed effects. Column (1) of Table 2 reports the corresponding results for these partial regressions. In line with the findings presented in Table 1, we report that the spying density decreased with population size. Apart from that, only few

differences remain. When running the covariate smoothness test conditional on log mean population in the 1980s, most differences are even smaller and insignificant (column (3) of Table 2). Nonetheless, we control for observable differences in county characteristics in our preferred specification.

5.2 Panel data design

As discussed before, time-persistent confounders that have affected the recruitment of informers and still affect post-reunification outcomes are a potential threat to identification. Given that our individual-level measures of trust are only observed post-treatment, we cannot account for these time-persistent potential confounders by including county fixed effects. However, certain measures of economic performance can be observed pre-treatment. Using county-level outcome variables from the late 1920s and early 1930s, we apply a panel data research design in spirit of Moser et al. (2014) that allows us to include county fixed effects to account for any time-invariant confounder.[25] The panel data model reads as follows:

$$Y_{ct} = \alpha + \sum_t \beta_t \times SPYDENS_c \times \tau_t + \boldsymbol{L'_{ct}\zeta} + \rho_c + \tau_t + \varepsilon_{ct}. \quad (2)$$

Outcomes Y_{ct} are county c's self-employment rate, unemployment rate and log population in year t (see Section 3.3).[26]

We allow the effect of spying to evolve over time by interacting the time-invariant spying density $SPYDENS_c$ with year dummies τ_t. Coefficients $\beta_t, \forall t \geq 1989$ show the treatment effect after reunification. Moreover, coefficients $\beta_t, \forall t < 1989$, provide a direct test of the identifying assumption. If the surveillance levels in the 1980s had an effect on economic outcomes *prior* to World War II, this would be an indication that spies were not allocated randomly with respect to the outcome variable. Hence, we need to have flat, insignificant pre-trends to defend our identifying assumption.[27] Using pre-treatment outcomes allows us to include county fixed effects ρ_c into the regression model. These fixed effects account for persistent confounding variables such as geographic location or regional liberalism. The model is identified by relating the spying density to different adjustment paths in outcome variables relative to the initial base levels prior to the treatment. Year fixed effects τ_t account for secular trends in outcome variables over time. In our preferred specification, we allow for different regional trends by including GDR state times year fixed effects (see below).

Vector $\boldsymbol{L_{ct}}$ includes several sets of control variables that vary by specification. Any persistent time-invariant confounder is wiped out from the model by county fixed effects. We, therefore, interact time-invariant control variables with a simple post-treatment dummy variable or year dummies. The first set of controls includes county size and demographic variables. Table 1 shows that county size explains around 25 percent of the variation in the informer density. At the same time, it is

[25] Note that many (though not all) potential confounders are likely to be time-invariant by definition, since they must have affected the informer recruitment in the 1980s and outcomes in the 1990s and 2000s.

[26] We have to drop East Berlin in the panel data design, as we neither observe pre nor post-treatment outcome measures separately for East and West Berlin.

[27] We omit the spying density for the last pre-treatment year and normalize β_t to zero in the respective year. With the exception of the regression for population, our pre-treatment variables are measured prior to World War II. For unemployment, we only observe one pre-treatment year (1933). While this is sufficient to identify county fixed effects, we cannot test for pre-trends in this model specification.

likely that counties of different size, for instance rural vs. urban counties, developed differently after reunification. Secondly, it is possible that different secular regional trends are confounding our results. Thus, we additionally include GDR state times year fixed effects to the model.[28] In our richest and preferred specification, we also add the opposition and industry controls as used in Table 1 to the regression model – each of them interacted with a post-treatment dummy.

6 Results

In the following section, we present the empirical results. First, we show the effects on trust and social ties (Section 6.1). In Section 6.2, we investigate the economic consequences of government surveillance. Last, we test for alternative channels other than government surveillance and subsequently assess the role of indirect economic or (selective) out-migration effects (Section 6.3).

6.1 Effects on trust and social ties

We apply the border discontinuity design as set up in equation (1) to identify the effect of spying on measures of trust and social ties. We analyze the effects for three sets of outcome variables: (i) trust, (ii) social behavior and (iii) societal engagement. For each set, we consider two standard outcome measures. Table 3 summarizes our findings.

In terms of trust, we find that the intensity of spying significantly affects both outcomes, trust in strangers and negative reciprocity (see Panel A). Results are significant in our leanest specification (columns (1) and (4)) and also conditional on individual- and county-level controls (columns (3) and (6)). The latter specification will be our preferred one throughout the paper. For a one standard deviation increase in the spying density, the estimate in column (3) implies that trust would be around six percentage points lower, which is a large effect given an average of 14 percent. When focusing on reciprocal behavior, we also find a strongly significant and negative effect (see column (6)). Moreover, the magnitudes of our two trust effects are very similar, when we standardize the trust in strangers measure.[29]

Next, we turn to measures of social behavior, with Panel B of Table 3 providing the results. We find a significant negative effect of the spying density on the number of close friends. On average, a one standard deviation increase in the intensity of spying leads to 0.4 fewer friends. Given that the average number of close friends in the sample is 4, this implies a 10 percent drop. Likewise, we find a negative and weakly significant effect on self-assessed sociability.[30]

In Panel C of Table 3, we consider two outcomes measuring societal engagement. First, we look at the probability that an individual is volunteering in clubs or social services. The estimate in our preferred specification (column (3)) is negative but not significant. Below we show that the imprecision is due to heterogeneous county pair effects. Last, we investigate the effect on participation in local politics. In our preferred specification, a one standard deviation increase in the spying

[28] For the pre-war period, we use Prussian provinces from the time of the Weimar Republic instead of GDR states.

[29] We estimate the models using OLS to ease interpretation. Results are robust to using (ordered) probit models, see Appendix Table A.3, columns (6) and (7).

[30] In the SOEP, sociability is one of the three components of the Big Five personality trait Extraversion. We also find a significantly negative effect on the composite extraversion measure.

Table 3: The Effect of Spying on Trust and Social Ties – Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Trust	Trust in strangers			Negative reciprocity		
Spying density	-0.041**	-0.042**	-0.061***	-0.161***	-0.161***	-0.195***
	(0.018)	(0.021)	(0.018)	(0.051)	(0.048)	(0.060)
Adjusted-R^2	0.061	0.095	0.115	0.066	0.129	0.145
Number of observations	3,389	3,389	3,389	3,011	3,011	3,011
Person-Year observations	1,531	1,531	1,531	1,369	1,369	1,369
Panel B – Social behavior	Number of close friends			Sociability		
Spying density	-0.416***	-0.387***	-0.428***	-0.031	-0.050	-0.128*
	(0.156)	(0.127)	(0.146)	(0.083)	(0.078)	(0.067)
Adjusted-R^2	0.074	0.114	0.138	0.055	0.086	0.120
Number of observations	3,248	3,248	3,248	3,137	3,137	3,137
Person-Year observations	1,460	1,460	1,460	1,424	1,424	1,424
Panel C – Societal engagement	Volunteering in clubs			Participation in local politics		
Spying density	0.013	0.009	-0.028	-0.004	-0.002	-0.041**
	(0.022)	(0.018)	(0.023)	(0.019)	(0.017)	(0.017)
Adjusted-R^2	0.058	0.115	0.123	0.020	0.126	0.137
Number of observations	3,712	3,712	3,712	3,549	3,549	3,549
Person-Year observations	1,661	1,661	1,661	1,625	1,625	1,625
Individual controls		Yes	Yes		Yes	Yes
County controls			Yes			Yes

Notes: This table shows the β coefficients of the border discontinuity model laid out in equation (1) using SOEP data for a one standard deviation increase in the informer density. For better comparability, negative reciprocity is defined such that higher values indicate less negative reciprocal behavior. Mean outcomes are 0.14 for trust in strangers, 2.69 for negative reciprocity, 3.95 for the number of close friends, 3.43 for sociability, 0.24 for engagement in voluntary work, and 0.11 for engagement in local politics, respectively. All specifications include border county pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the county pair and county level with usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to county pairs for which we observe individuals in both counties along the state border. All specifications use cross-sectional weights adjusted for duplicates of counties that are part of multiple border county pairs. For detailed information on the control variables, see Data Appendix B.

density leads to a decrease in local political engagement of four percentage points. Interestingly, the effect is only significant when we condition on county control variables – the crucial variables being electoral turnout and vote shares in the 1920s and 1930s.

Identification tests. In Section 5.1, we have argued that the recruitment of informers was likely to be non-random, which led us to implement the border discontinuity design, where we identify the effects *within* neighboring counties at a state border, which are supposedly more similar than

randomly drawn counties. In the following, we provide two identification tests to underscore the importance of our identification strategy.

A first and simple test is to estimate equation (1) using a naive OLS estimator, i.e., without restricting the sample to counties at borders and ignoring border county pair fixed effects ν_b. Column (1) of Table 4 provides the results for a such a model. The estimate in column (1) of Panel A shows, for instance, a positive correlation between the spying density and trust in strangers. When restricting the sample to counties at state borders but ignoring the fixed effects ν_b (column (2)), the sign flips and we see a small but insignificant negative effect. Column (3) restates our preferred specification from Table 3 including county pair fixed effects. A similar pattern can be observed for the other measures of trust and social behavior: coefficients become more negative and more significant when moving from specification (1) to our preferred model reported in column (3).

In a second test, we try to rule out that our results are driven by long-lasting and persistent cultural differences across regions (see, e.g., Becker et al., 2016, for the Habsburg Empire). Specifically, we exploit a territorial reform that happened shortly after the foundation of the GDR. Prior to World War II, the territory of the GDR was covered by the Free States (and prior monarchies) of Prussia, Saxony, Anhalt, Mecklenburg and Thuringa. When implementing socialism, the GDR regime explicitly tried to overcome this federal structure. It limited the power of sub-national jurisdictions and established a centralist state following the example of the Soviet Union. In 1947, the Soviet occupying power dissolved the state of Prussia and formed the new administrative jurisdictions Mecklenburg, Anhalt, Brandenburg, Thuringa and Saxony. In 1952, fourteen new states (*Bezirke*) were created; East Berlin became the 15th state in 1961. The borderlines were drawn with regard to economic and military considerations, while cultural and ethnic factors played a minor role. As a result, the new state borders often separated regions, which had belonged to the same province and shared the same cultural heritage for a long time. We test whether effects of the spying density are different in county pairs that historically belonged to the same Prussian province or Free State. Column (4) of Table 4 shows the results. Reassuringly, we find either similar or stronger effects for county pairs that belonged to the same region. Thus, it seems unlikely that deep cultural differences at historical state borders drive the results of our analysis. In particular, we find a significantly negative effect for volunteering in clubs, which was insignificant in the baseline specification.

Table 4: The Effect of Spying on Trust and Social Ties – Identification Tests

	Full Sample	County Pair Sample		
	(1)	(2)	(3)	(4)
A – Trust in strangers				
Spying density	0.019	-0.005	-0.061***	
	(0.015)	(0.021)	(0.018)	
Spying density × Different Weimar Province				-0.071***
				(0.027)
Spying density × Same Weimar Province				-0.052***
				(0.020)
Person-Year observations	3,313	1,531	1,531	1,531
B – Negative reciprocity				
Spying density	-0.105**	-0.166***	-0.195***	
	(0.046)	(0.054)	(0.060)	
Spying density × Different Weimar Province				-0.147
				(0.095)
Spying density × Same Weimar Province				-0.234***
				(0.070)
Person-Year observations	2,947	1,369	1,369	1,369
C – Number of close friends				
Spying density	-0.237**	-0.118	-0.428***	
	(0.117)	(0.154)	(0.146)	
Spying density × Different Weimar Province				-0.483**
				(0.198)
Spying density × Same Weimar Province				-0.379**
				(0.176)
Person-Year observations	3,095	1,460	1,460	1,460
D – Sociability				
Spying density	-0.028	-0.090	-0.128*	
	(0.046)	(0.058)	(0.067)	
Spying density × Different Weimar Province				0.021
				(0.079)
Spying density × Same Weimar Province				-0.251***
				(0.065)
Person-Year observations	3,034	1,424	1,424	1,424
E – Volunteering in clubs				
Spying density	0.025*	-0.014	-0.028	
	(0.014)	(0.021)	(0.023)	
Spying density × Different Weimar Province				0.000
				(0.028)
Spying density × Same Weimar Province				-0.054**
				(0.023)
Person-Year observations	3,557	1,661	1,661	1,661
F – Participation in local politics				
Spying density	0.001	-0.013	-0.041**	
	(0.010)	(0.013)	(0.017)	
Spying density × Different Weimar Province				-0.017
				(0.018)
Spying density × Same Weimar Province				-0.060***
				(0.017)
Person-Year observations	3,551	1,625	1,625	1,625
County Pair Fixed Effects			Yes	Yes

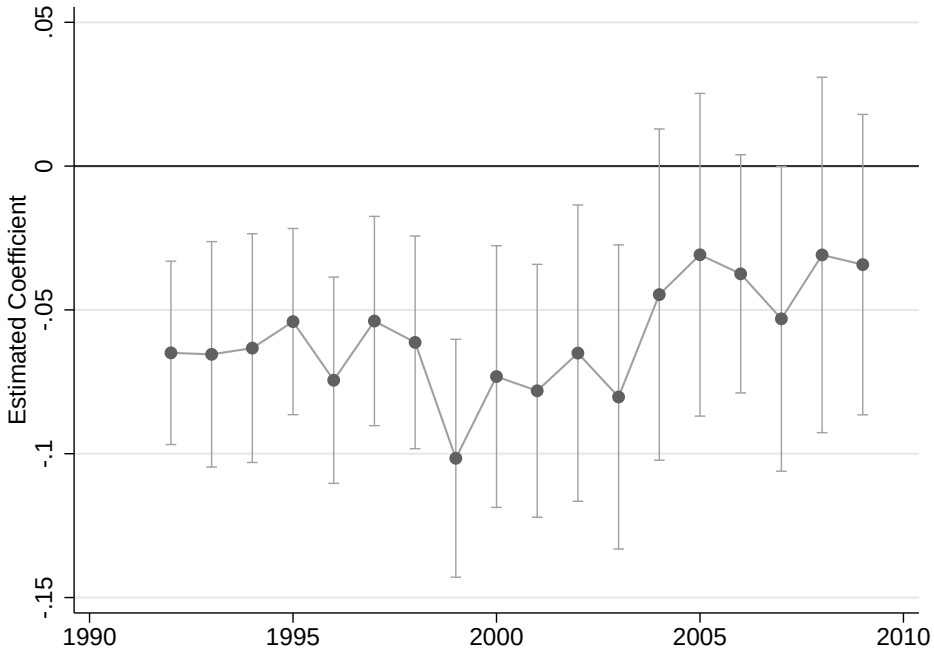
Notes: This table shows the β coefficients using different specifications on the base and on the border county pair sample for a one standard deviation increase in the informer density. Mean outcomes are 0.14 for trust in strangers, 2.69 for negative reciprocity, 3.95 for the number of close friends, 3.43 for sociability, 0.24 for engagement in voluntary work, and 0.11 for engagement in local politics, respectively. All regressions include the full set of controls (see Data Appendix B). Standard errors are two-way clustered at the county and the individual level in the full sample, and two-way clustered at the county pair and county level in the county pair sample. The usual confidence levels apply (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to county pairs for which we observe individuals in both counties along the state border. In column (1), cross-sectional weights are used. In columns (2)-(4), cross-sectional weights are adjusted for duplicates of counties that are part of multiple border county pairs.

6.2 Effects on economic performance

In the previous section, we demonstrated that a higher informer density undermined trust and led individuals to scale back their social activities. As social interactions are reduced and exchanging of ideas is less likely, we expect to observe negative economic consequences in counties with more government surveillance. We test this hypothesis in the following section.

We start with individual labor income as reported in the SOEP and apply the border discontinuity design. Unlike the measures of trust and social ties, income is reported in each wave of the SOEP, which enables us to check the evolution of the effect over time. Figure 2 shows the results of our preferred specification with a full set of individual and county control variables.[31] The figure shows a negative and persistent effect of a higher spying density on income throughout the 1990s. A one standard deviation increase in intensity of surveillance leads, on average, to a 6% decrease in labor earnings. In the mid-2000s, the effect slowly fades away.[32]

Figure 2: The Effect of Spying on Log Labor Income



Notes: The graph plots the β coefficients and corresponding 95% confidence intervals of the border discontinuity model laid our in equation (1) for a one standard deviation increase in the informer density. The informer density is interacted with year dummies. The specification includes individual and county level controls. For full regression results, see specification (5) of Appendix Table A.4.

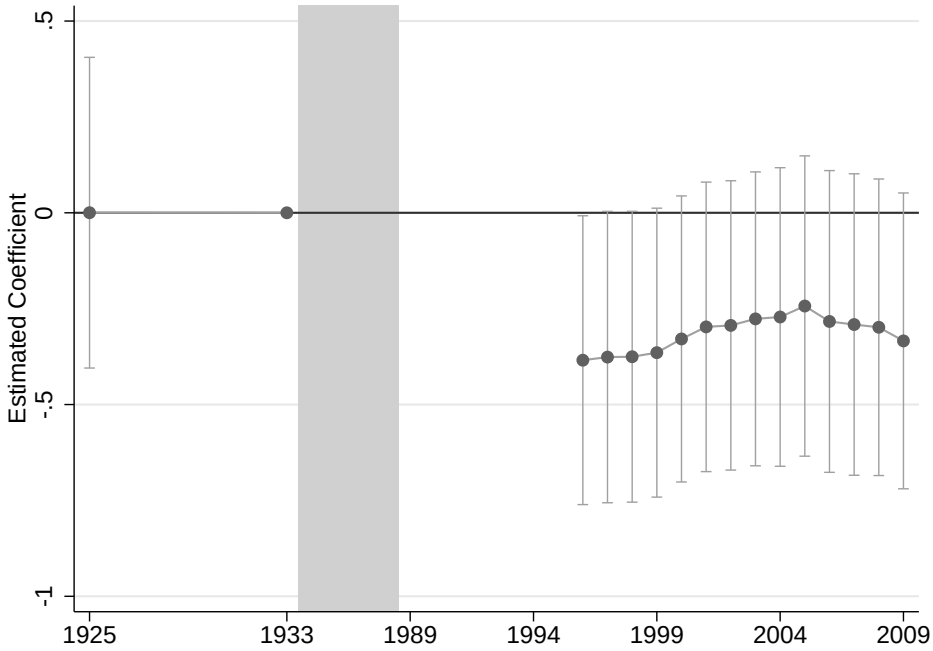
Next, we turn to county-level outcomes of economic performance and apply the panel data model as laid out in equation (2). We begin by analyzing the effect of spying on entrepreneurial activity, given that lacking trust may result in extensive monitoring of "possible malfeasance by partners,

[31] Results for specifications excluding individual and/or county controls as well as using the naive OLS estimator are shown in Table A.4 in the Appendix.

[32] We find no statistically different effects for county pairs that belonged to same or different Weimar provinces (see Figure A.4 in the Appendix).

employees, and suppliers [and] less time to devote to innovation in new products or processes" (Knack and Keefer, 1997). Indeed, many studies have shown that more trustful people are more likely to become entrepreneurs (Welter, 2012, Caliendo et al., 2014). We find a negative and quite persistent effect of the spying density on self-employment rate (see Figure 3).[33] The estimate implies that for a one standard deviation increase in the spying density, the self-employment rate would be around 0.4 percentage points lower. Figure 3 also contains information on the potential endogeneity of the intensity of surveillance. If estimates of the intensity of spying were significant prior to World War II, the allocation of spies would have responded to pre-treatment trends in self-employment rates and would thus have been endogenous in this respect. Reassuringly, we find a remarkably flat pre-trend. Moreover, full regression results show that the estimate is robust as soon as we control for state times year fixed effects (see Table A.6).

Figure 3: The Effect of Spying on Self-Employment Rates

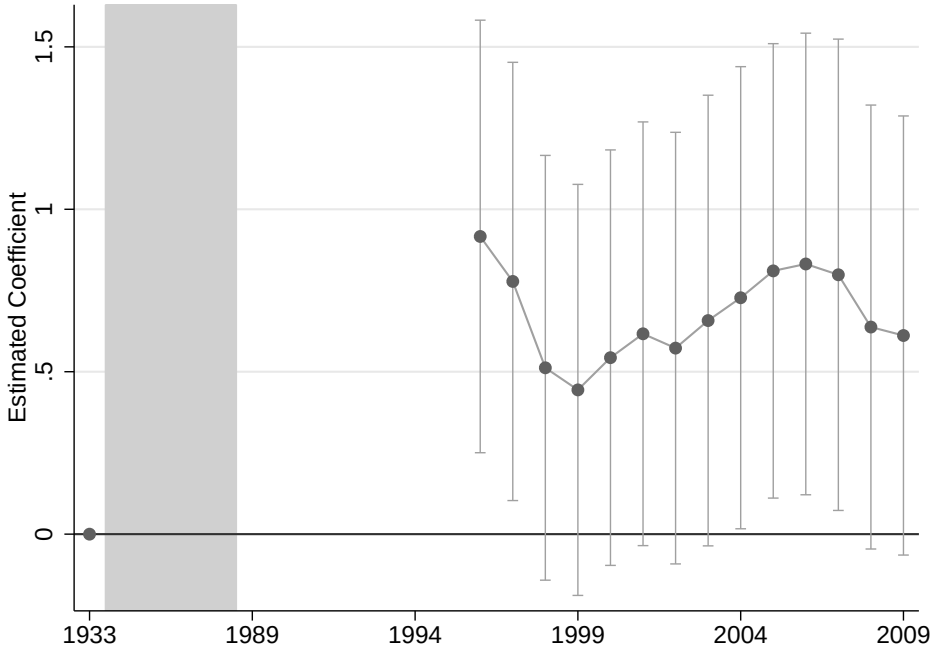


Notes: This graph plots the β_t coefficients and 95 % confidence intervals of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The unweighted average post-reunification self-employment rate across counties is 10.42 %. The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (5) in Table A.6 for details.

With entrepreneurial spirit lagging behind, we also expect negative effects on more comprehensive measures of economic performance. Ideally, we would look at the effect of spying density on GDP. Unfortunately, there is no pre World War II county-level measure available that is comparable to today's GDP. Hence, we take two other proxies for economic performance for which pre-treatment information is available: unemployment rates, and population size (Redding and Sturm, 2008). Figure 4 shows that unemployment is indeed higher in counties with a higher informer density. The

[33] As explained in Section 3.3, there is no information on self-employment and unemployment rates at the county level in the early 1990s.

Figure 4: The Effect of Spying on Unemployment Rates



Notes: This graph plots the β_t coefficients and 95 % confidence intervals of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The unweighted average post-reunification unemployment rate across counties is 18.66 %. The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (5) in Table A.7 for details.

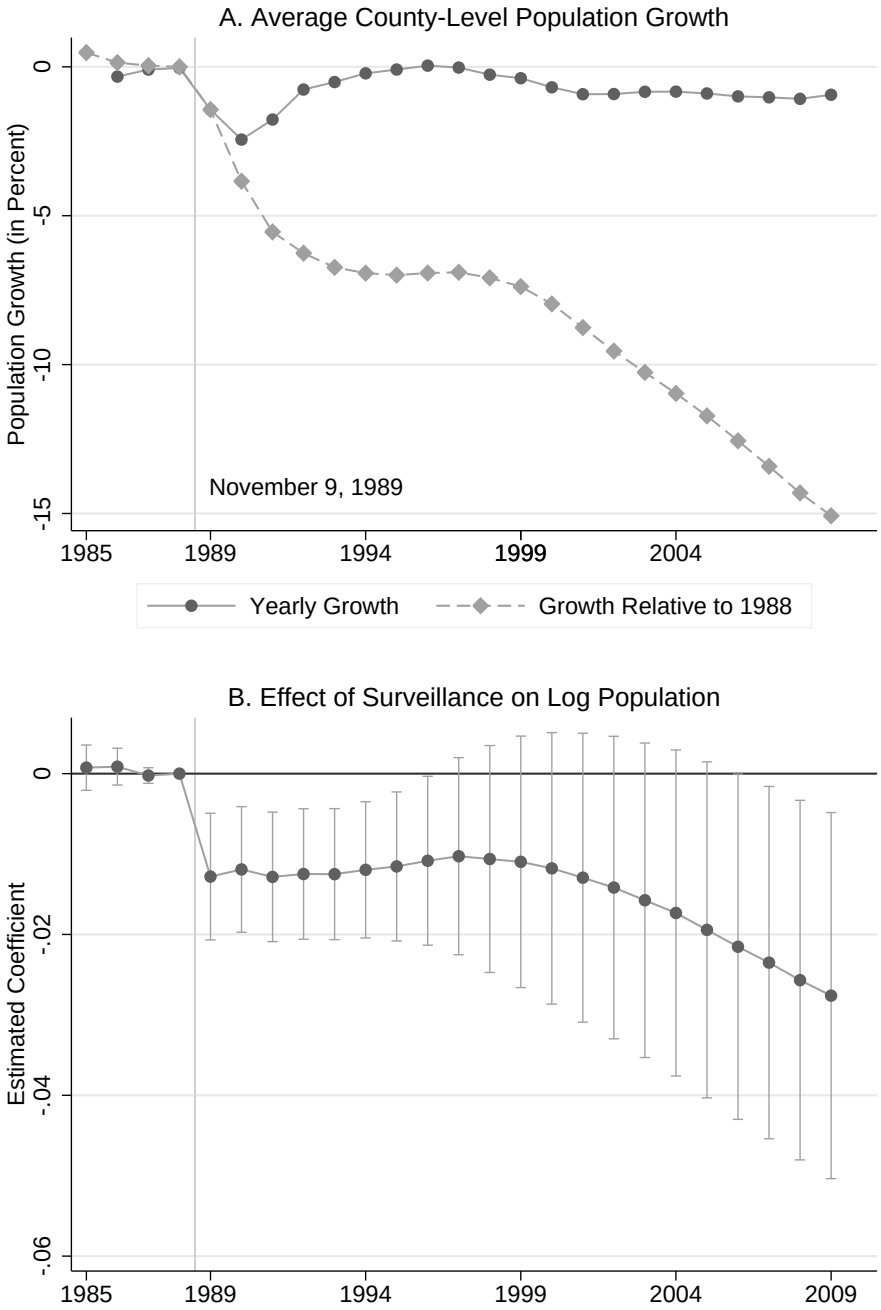
effect is persistent and oscillates around an increase of 0.6 percentage points in county unemployment for a one standard deviation increase in the informer density. Unfortunately, there is only one reliable pre-treatment observation for the unemployment rate. While we can still identify the effect of spying in our panel research design, we cannot check for pre-trends in unemployment.

Next, we investigate the effect of state surveillance on county population. We start in Panel A of Figure 5 by plotting the overall average yearly and cumulated county-level population growth rates for East Germany since the mid 1980s. The graph shows two emigration waves after the fall of the Iron Curtain – a severe and rather short one immediately after reunification (between 1989 and 1992), and a moderate and longer one starting in 1998 (cf. also Fuchs-Schündeln and Schündeln, 2009). In Panel B of Figure 5, we show that the effect of *Stasi spying* coincides with these two emigration waves.[34] First, the population in higher-spying counties sharply drops in the first post-treatment year 1989.[35] This implies that the initial emigration wave was significantly driven by people leaving higher-spying counties. For 1989, the estimates imply that a one standard deviation decrease in the spying density yields an increase in the population level of 1.2 percent. Given that the average

[34] Effects are always relative to lower spying counties. Hence, a negative population effect does not need to result in a lower number of inhabitants if population levels increased in lower spying counties. Given that populations dropped in almost all counties, the most relevant interpretation of a negative effect seems to be a faster decline in population.

[35] Population is measured on December 31, 1989, hence hardly two months after the fall of the Berlin Wall. However, many people already tried to escape from the GDR in the summer of 1989, either via Hungary and Austria or by fleeing to the West German embassies in Warsaw, Prague, and Budapest.

Figure 5: Stasi Surveillance and Population Decline



Notes: Panel A shows yearly and cumulative average population growth for East German counties from 1985 to 2009. Panel B plots the β_t coefficients and 95 % confidence intervals of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (5) in Table A.8 for details. For details on the source and construction of the variables, see Appendix Table B.1.

population loss in 1989 was 1.5 percent, this is a substantial effect. The effect of spying is flat after 1989. From 1990 to 2000, we do not see a significantly different population effect between high and

low-spying counties in addition to the initial population outflow. This implies that the population response driven by spying was immediate. In 2001, i.e., in the early years of the second emigration wave, the effect of spying on population size starts to decline again and continues to do so until 2009.

6.3 Discussion: Alternative channels and indirect effects

In the previous two sections, we demonstrated various reduced form effects of the spying density on trust, social ties and economic performance. These estimates quantify the total effect of government surveillance, which is the main focus of this study. In the following section, we take a closer look at potential mechanisms behind these overall effects. First, we corroborate that government surveillance is indeed driving our results by ruling out alternative channels such as socialist indoctrination or post-reunification subsidies. In a second step, we assess the role of indirect second-round effects. Specifically, we analyze whether negative effects on trust and social ties are simply driven by local economic or population shocks.

We start by exploring the role of alternative channels that might potentially explain the decline in trust, social ties and economic performance. As socialist indoctrination has been shown to affect individual preferences and economic outcomes (see, e.g., Alesina and Fuchs-Schündeln, 2007, Fuchs-Schündeln and Masella, 2016), it might also be a potential driver of our effects. While the mentioned studies compared East to West Germany, we have to find a measure that can pick up regional variation in the ideological penetration within the East German society. Given that the GDR regime was a one-party dictatorship, we proxy socialist indoctrination by the share of SED party members among the political and economic elites in 1988 (see Appendix Table B.1 for details on this variable). In a first step, we show that our measure of socialist indoctrination is only weakly correlated with the intensity of surveillance. Moreover, as soon as we condition on control variables, there is virtually no correlation between the two variables (see Figure A.5 in the Appendix). Second, we demonstrate that estimates are basically unchanged when including the indoctrination variable as a control (see column (2) of Tables 5 and A.5; column (6) of Tables A.6 to A.8).[36]

An alternative driver of our results might be government transfers and subsidies that were paid to East German counties after reunification in order to rebuild public infrastructure and the housing stock (*Aufbau Ost*). These subsidies, which, for instance, caused a boom in the construction sector (Paqué, 2009), might thus be correlated with the informer density via the industrial structure of a county. Although we control for the industrial structure in our baseline specification, we additionally include the annual sum of government subsidies paid to the specific counties. Results are not affected by these controls either (see column (3) of Tables 5 and A.5; column (7) of Tables A.6 to A.8).[37]

While these tests suggest that government surveillance is the driver of our results, we take a closer look at the implied mechanism, that is, surveillance affecting trust and social ties, which triggers negative economic effects. For this reason, we assess the importance of indirect effects in the remainder of this section. First, we consider the role of economic shocks, which might also lead to

[36] Column (1) of Tables 5 and A.5 replicate the main results of Tables 3 and A.4 on a sample that excludes East Berlin and the respective border county pairs. We exclude East Berlin throughout Tables 5 and A.5 because we do not have separate population data for East and West Berlin after reunification, which is necessary to control for changes in the population (see below). Note that no result is driven by the restriction of the sample.

[37] We have no information on transfers for the years 1990–1994.

lower levels of trust and social withdrawal. In order to rule out this mechanism, we first reestimate our preferred specification of equation (1), including county-level income as an additional control variable.[38] Comparing the baseline specification and the model conditional on county-level income (see columns (1) and (4) of Table 5), we find that results are almost identical.[39]

It is equally possible that negative effects on trust and social ties are driven by the substantial overall migration response. In order to capture the consequences of these population shocks, we include the county-level population growth rate as an additional control variable. Results provided in column (5) of Table 5 change only slightly.[40] We also find that effects on individual labor income are robust when additionally controlling for population growth, see Table A.5, column (4). As regards our county-level results, all economic outcomes are measured as rates, hence the estimates already account for base changes due to migration. However, we additionally check the robustness of our estimates by including log current population in order to check whether our findings are driven by agglomeration effects. Estimates, presented in columns (8) of Tables A.6 through A.7, are hardly affected.

While overall migration does not exhibit substantial second-round effects, it is still possible that results are triggered by *selective* out-migration. In the border discontinuity design, our preferred specification accounts for selective out-migration by conditioning on a large set of individual controls. Introducing these controls, which capture compositional differences within county pairs, hardly affects our results (cf. Tables 3 and A.4). This indicates that selective out-migration is not a key driver of the results, either. In addition, we provide a second, more immediate test for selective out-migration using the panel data design. Here, we directly test whether the spying density had an effect on specific population shares. In terms of skills, Figure 6 shows that, if anything, there is a small positive effect of the Stasi on the share of high-skilled employees in a county. A one standard deviation increase in the intensity of surveillance leads to an (insignificant) increase of 0.2 percentage points in the share of the high-skilled. This finding is in line with Fuchs-Schündeln and Schündeln (2009), who show that the first migration wave (1989–1992) was rather driven by individuals without a college degree or vocational training. In Figure 7, we further assess the evolution of population shares by age groups. The initial population shock of the first wave (1989–1992) seems to be driven by individuals who were 35-54 years old and had no children at that time. In contrast, with the onset of the second immigration wave starting in the late 1990s, the share of 15-34 year olds gradually starts to decline in higher-spying counties. Overall, the results on population shares do not provide strong evidence for selective out-migration. While the effect on the share of high-skilled individuals points to a negative selection out of high-spying counties, which, ceteris paribus, should be beneficial for economic outcomes, the effects by age are inconclusive but hint at a decreasing share of young individuals starting in the late 1990s. In general, the magnitudes of these effects are very small, which suggests a marginal role of indirect migration effects.

[38] We are aware that income itself is an outcome and that we are sacrificing econometric rigor with such a specification. Yet, we think that learning more about the underlying mechanisms is interesting and important, and thus justifies such a specification as a robustness test. In any case, we interpret the following results carefully and regard them as suggestive rather than definitive.

[39] Results do not change when including individual labor income rather than the mean county-level income.

[40] As mentioned above, we assign each individual the informer density of the GDR county in which she lived in 1989, which prevents selection out-of-treatment in the first place.

Table 5: The Effect of Spying on Trust and Social Ties – Exploring the mechanism

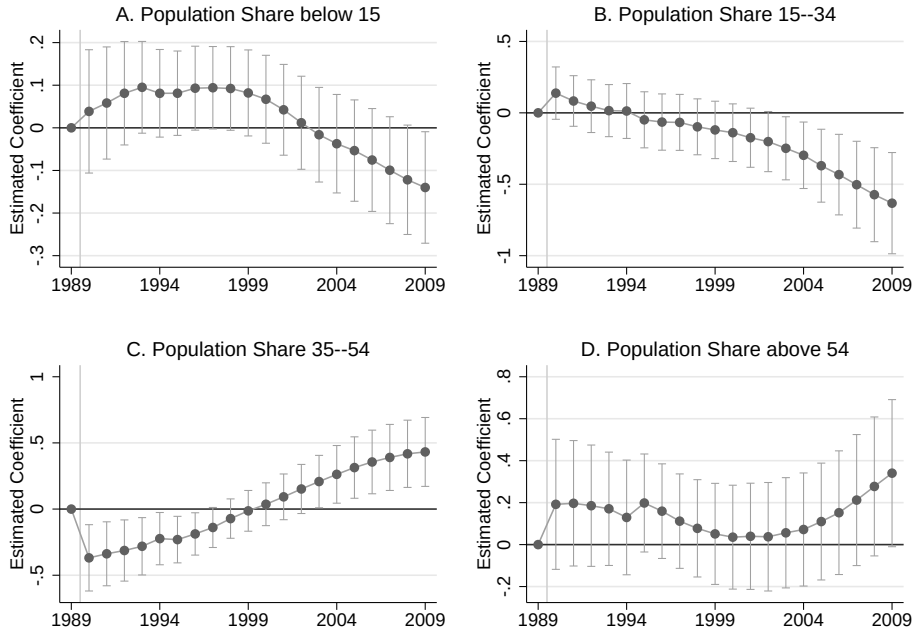
	County Pair Sample					
	(1)	(2)	(3)	(4)	(5)	(6)
A – Trust in strangers						
Spying density	-0.066***	-0.046***	-0.079***	-0.066***	-0.056***	-0.056***
	(0.019)	(0.014)	(0.021)	(0.018)	(0.021)	(0.018)
Person-Year observations	1,285	1,285	1,285	1,285	1,285	1,285
B – Negative reciprocity						
Spying density	-0.166***	-0.192***	-0.184***	-0.166***	-0.191***	-0.194***
	(0.062)	(0.056)	(0.061)	(0.062)	(0.063)	(0.068)
Person-Year observations	1,149	1,149	1,149	1,149	1,149	1,149
C – Number of close friends						
Spying density	-0.426***	-0.397***	-0.495***	-0.429***	-0.314*	-0.430***
	(0.154)	(0.149)	(0.165)	(0.152)	(0.163)	(0.164)
Person-Year observations	1,224	1,224	1,224	1,224	1,224	1,224
D – Sociability						
Spying density	-0.188***	-0.181***	-0.144**	-0.190***	-0.141**	-0.115*
	(0.060)	(0.061)	(0.059)	(0.059)	(0.063)	(0.059)
Person-Year observations	1,199	1,199	1,199	1,199	1,199	1,199
E – Volunteering in clubs						
Spying density	-0.029	-0.034	-0.034	-0.030	-0.036	-0.046*
	(0.022)	(0.022)	(0.027)	(0.022)	(0.025)	(0.027)
Person-Year observations	1,357	1,357	1,357	1,357	1,357	1,357
F – Participation in local politics						
Spying density	-0.041**	-0.042**	-0.036*	-0.043**	-0.044**	-0.042**
	(0.018)	(0.018)	(0.021)	(0.018)	(0.019)	(0.021)
Person-Year observations	1,393	1,393	1,393	1,393	1,393	1,393
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes	Yes
Share SED party members		Yes				Yes
Government transfers			Yes			Yes
Mean county-level income				Yes		Yes
Population growth					Yes	Yes

Notes: This table shows the β coefficients of the border discontinuity model laid out in equation (1) using SOEP data for a one standard deviation increase in the informer density. For better comparability, negative reciprocity is defined such that higher values indicate less negative reciprocal behavior. Mean outcomes are 0.14 for trust in strangers, 2.69 for negative reciprocity, 3.95 for the number of close friends, 3.43 for sociability, 0.24 for engagement in voluntary work, and 0.11 for engagement in local politics, respectively. All specifications include county pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the county pair and county level with usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to county pairs for which we observe individuals in both counties along the state border. All specifications use cross-sectional weights adjusted for duplicates of counties that are part of multiple county pairs. For detailed information on the control variables, see Data Appendix B.

Figure 6: The Effect of Spying on Share of High-Skilled

Notes: This graph plots the β_t coefficients and 95 % confidence intervals of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The unweighted average post-reunification high-skill share across counties is 3.98 %. The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (5) in Table A.9 for details.

Figure 7: Stasi Surveillance and Age Groups



Notes: This graph plots the β_t coefficients and 95 % confidence intervals of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. Panel A shows the effect of spying on the share of the population younger than 15, Panel B the effect on the group between 15–34, Panel C the effect on the group 35–54 and Panel D the effect on the group older than 54. The unweighted average post-reunification population shares across counties are 14.08 %, 25.35 %, 29.87 %, 30.70 %, respectively. The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (5) in Tables A.10–A.13 for details.

7 Conclusion

In this paper, we estimate the effect of state surveillance on trust, social ties and economic performance by exploiting county-level variation in the number of Stasi informers per capita in the former socialist German Democratic Republic. To account for the potentially non-random recruitment of informers across counties, we implement two different research designs. First, we exploit discontinuities at state borders arising from the administrative structure of the Ministry for State Security. Second, we set up a long-term panel including pre World War II measures of economic performance, which allows us to control for county fixed effects.

Overall, the results of our study offer substantial evidence for negative and long-lasting effects of government surveillance. We find strong and consistent evidence that a higher density of informers undermined trust and led to a withdrawal from society. In particular, more intense surveillance caused lower trust in strangers, stronger negative reciprocity, fewer close friends, lower sociability, and reduced societal engagement in clubs and local politics. Against the backdrop of this social withdrawal, we also find negative and persistent effects of government surveillance on various measures of economic performance, such as individual labor income, county-level self-employment, unemployment and population size. We provide evidence that these results are indeed caused by government surveillance and not driven by alternative factors such as socialist indoctrination.

Our findings have important implications for contemporary forms of government surveillance in authoritarian, but also in democratic countries. Given that all surveillance programs extract information from social networks, it is likely that the current systems in authoritarian states like China or Russia exert similar negative effects on social interactions and economic performance. Moreover, the social and economic costs of surveillance should also be taken into account by benevolent governments when designing the optimal surveillance policy.

Our results also add to the large literature on institutions. Following the terminology of Alesina and Giuliano (2015), our study establishes a causal link running from formal institutions to culture, as measured by trust and social behavior. We show that this effect is still visible many years after the fall of the Berlin Wall and the end of the socialist East German regime. Moreover, and in line with Tabellini (2010), we provide clear evidence that the degree of democratic governance affects economic outcomes. With both trust and economic performance being impaired by government surveillance, our findings also provide suggestive evidence in favor of a well established channel: institutions shape peoples' trust, and trust affects economic development (Algan and Cahuc, 2014). In particular, and in line with La Porta et al. (1997), our findings point to reduced entrepreneurial activity as an important channel for the observed economic decline. As for our measures of trust and social ties, these effects are persistent and detectable two decades after the end of the socialist regime. Given the intergenerational transmission of trust and beliefs (see, e.g., Nunn and Wantchekon, 2011, Dohmen et al., 2012), it seems likely that these effects will be even longer-lasting. In fact, the erosion of civic capital induced by the Stasi surveillance system could be one explanatory factor of the lacking convergence between East and West Germany.

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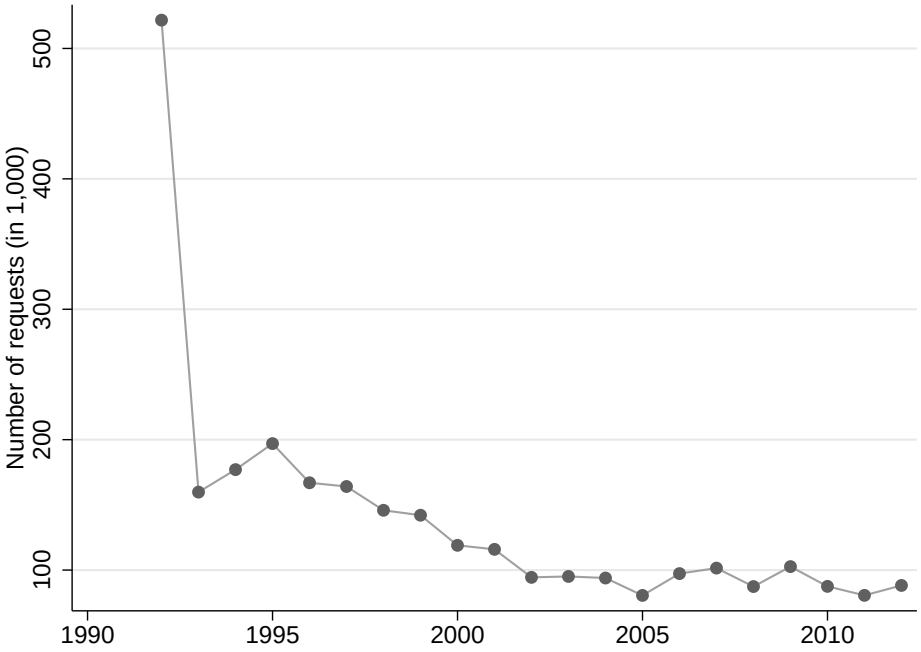
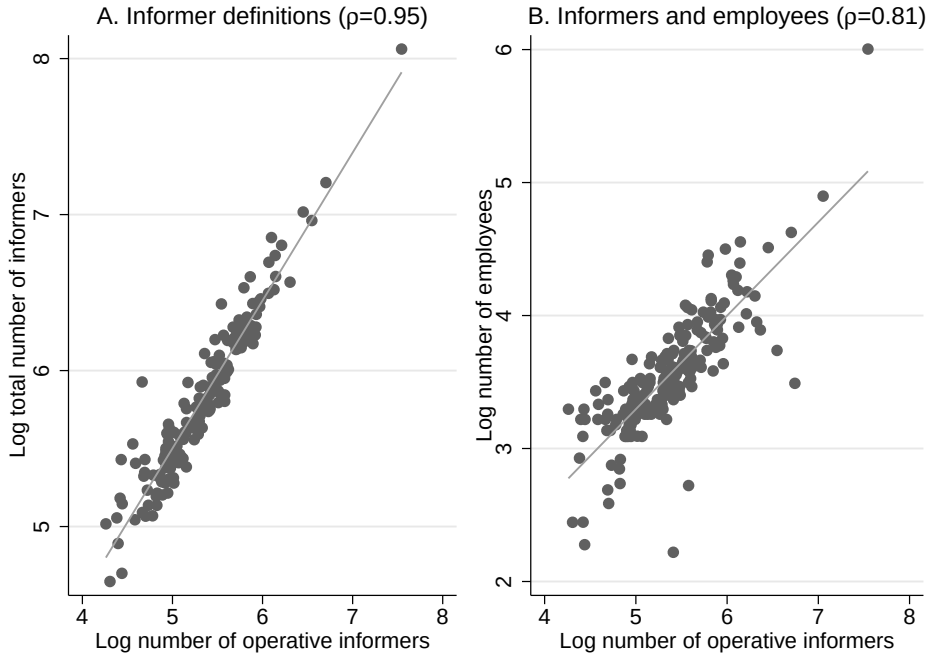
A Appendix

Figure A.1: Number of Requests for the Inspection of Stasi Files



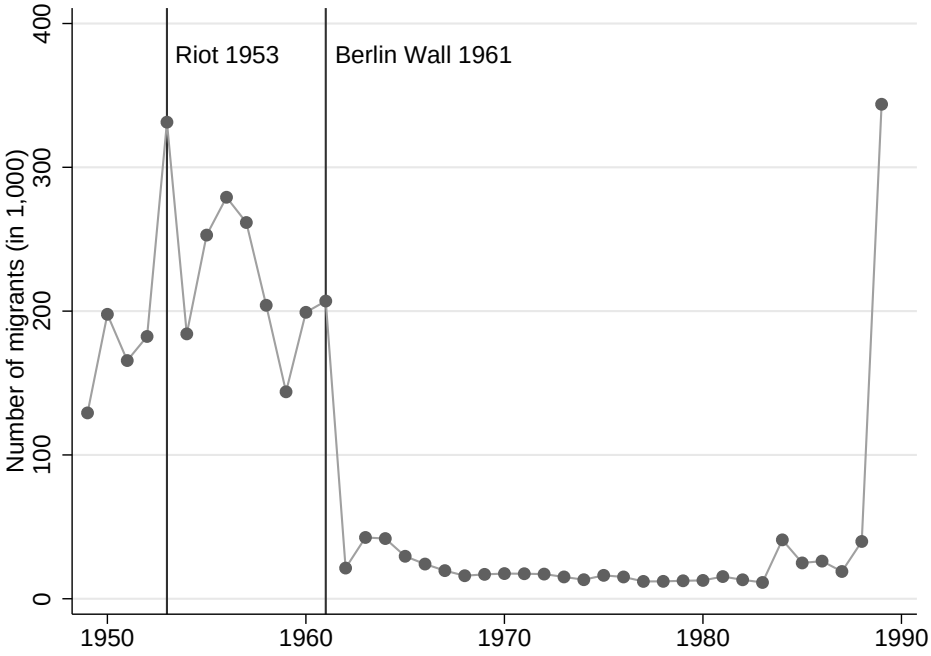
Notes: This graph plots the annual number of requests for inspection of Stasi files. It is based on data from the Agency of the Federal Commissioner for the Stasi Records.

Figure A.2: Alternative Measures of Stasi Surveillance



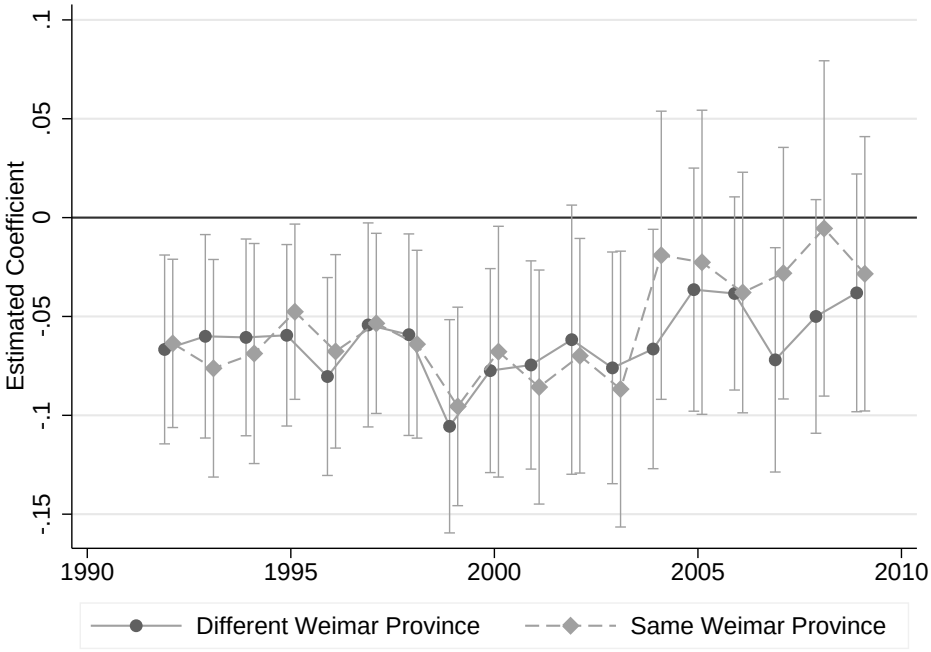
Notes: The graph plots the correlation between three different measures of Stasi surveillance: (i) the number of operative informers (unofficial collaborator category 1), which builds the base for our preferred measure of the informer density, (ii) the total number of informers in categories (1)-(3), and (iii) the number of official Stasi employees in 1982. Correlation coefficients ρ are presented in parentheses. For information on all variables, see Appendix Table B.1.

Figure A.3: Migration from East to West Germany



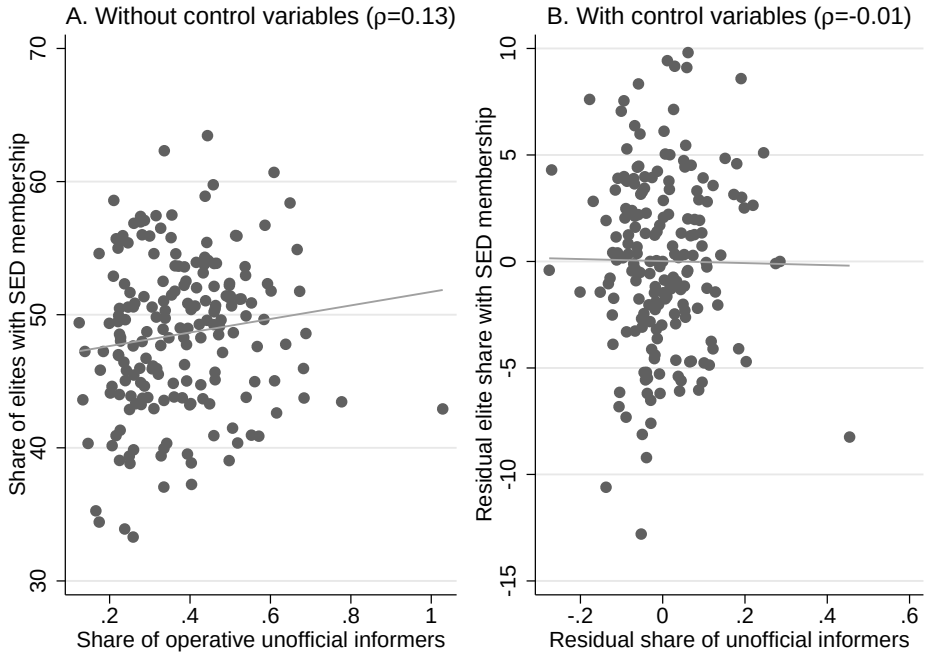
Notes: This graph shows the annual number of migrants from East to West Germany (1949-1989). It is based on data from Rühle and Holzweißig (1988), Ritter and Lapp (1997) and monthly announcements of the West German Federal Ministry for Displaced Persons, Refugees and War Victims.

Figure A.4: The Effect of Spying on Log Labor Income



Notes: The graph plots the β coefficients and corresponding 95 % confidence intervals of the border discontinuity model laid our in equation (1) for a one standard deviation increase in the informer density. The informer density is interacted with year dummies and a dummy variable indicating whether a border county pair was part of the same Weimar province. The specification includes individual and county level controls.

Figure A.5: Informer Density and Socialist Indoctrination



Notes: The graph plots the correlation between the share of operative informers in the population and the share of SED party members among political and economic executives. Panel A shows the raw correlation between both measures. Panel B depicts the correlation between the residiuals of both variables after regressing them on the full set of control variables (see Section 4). For information on all variables, see Appendix Table B.1.

Table A.1: Descriptive Statistics on SOEP Outcomes

	Mean	SD	P25	P50	P75	Min	Max	N
Dependent Variables								
Trust in strangers	0.14	0.35	0.00	0.00	0.00	0.00	1.00	3,389
Negative reciprocity	2.69	1.00	2.05	2.74	3.42	0.00	4.11	3,011
Number of close friends	3.95	2.97	2.00	3.00	5.00	0.00	15.00	3,248
Sociability	3.43	1.00	2.78	3.47	4.17	0.69	4.86	3,137
Engagement in voluntary work	0.24	0.43	0.00	0.00	0.00	0.00	1.00	3,712
Engagement in local politics	0.11	0.31	0.00	0.00	0.00	0.00	1.00	3,549
Log Labor Income	7.58	0.47	7.29	7.60	7.88	6.00	9.90	22,659
Control variables (as of 1990)								
Male	0.48	0.50	0.00	0.00	1.00	0.00	1.00	4,366
Age	42.28	16.27	29.00	40.00	54.00	17.00	95.00	4,366
Marital status							.00	
Share of Singles	0.18	0.38	0.00	0.00	0.00	0.00	1.00	4,366
Share of Married	0.69	0.46	0.00	1.00	1.00	0.00	1.00	4,366
Share of Divorced	0.13	0.33	0.00	0.00	0.00	0.00	1.00	4,366
Household Size								
One-person household	0.08	0.27	0.00	0.00	0.00	0.00	1.00	4,366
Two-person household	0.25	0.43	0.00	0.00	1.00	0.00	1.00	4,366
Three-person household	0.30	0.46	0.00	0.00	1.00	0.00	1.00	4,366
Four-person household	0.29	0.45	0.00	0.00	1.00	0.00	1.00	4,366
Five-or-more-person household	0.09	0.28	0.00	0.00	0.00	0.00	1.00	4,366
Education								
In school	0.01	0.09	0.00	0.00	0.00	0.00	1.00	4,366
Inadequately completed	0.01	0.08	0.00	0.00	0.00	0.00	1.00	4,366
General elementary school	0.07	0.26	0.00	0.00	0.00	0.00	1.00	4,366
Basic qualification	0.30	0.46	0.00	0.00	1.00	0.00	1.00	4,366
Intermediate qualification	0.04	0.20	0.00	0.00	0.00	0.00	1.00	4,366
Vocational training	0.32	0.47	0.00	0.00	1.00	0.00	1.00	4,366
General maturity certificate	0.01	0.10	0.00	0.00	0.00	0.00	1.00	4,366
Vocational certificate	0.02	0.14	0.00	0.00	0.00	0.00	1.00	4,366
Lower tertiary education	0.00	0.00	0.00	0.00	0.00	0.00	0.00	4,366
Higher tertiary education	0.22	0.42	0.00	0.00	0.00	0.00	1.00	4,366
First job								
Blue-collar worker	0.51	0.50	0.00	1.00	1.00	0.00	1.00	4,366
Self-employed	0.02	0.13	0.00	0.00	0.00	0.00	1.00	4,366
white-collar worker	0.27	0.44	0.00	0.00	1.00	0.00	1.00	4,366
Civil servant	0.00	0.06	0.00	0.00	0.00	0.00	1.00	4,366
Other	0.20	0.40	0.00	0.00	0.00	0.00	1.00	4,366

Notes: This table presents descriptives statistics on SOEP outcome variables. For information on the respective years covered, see Appendix Table B.1.

Table A.2: Descriptive Statistics on Panel Outcomes and Controls

	Mean	SD	P25	P50	P75	Min	Max	N
Self-employment rate								
1925–1933	18.56	4.17	15.29	18.09	20.92	10.88	31.79	370
1996–2009	10.42	2.01	9.00	10.30	11.90	5.00	15.40	2,590
Unemployement rate								
1933–1933	15.83	5.60	10.89	16.48	20.14	3.70	28.71	185
1996–2009	18.66	3.79	16.15	18.61	21.10	6.60	31.28	2,590
Log population								
1985–1988	11.01	0.56	10.58	10.98	11.32	9.79	13.23	740
1989–2009	10.91	0.55	10.53	10.88	11.22	9.63	13.18	3,885
Population share younger than 15								
1989–1989	19.72	1.79	18.60	19.76	20.94	15.56	24.74	185
1990–2009	14.08	3.42	11.00	13.42	16.73	8.63	24.62	3,700
Population share aged 15–34								
1989–1989	29.43	1.62	28.25	29.61	30.42	25.52	34.14	185
1990–2009	25.35	2.56	23.67	25.59	27.22	18.72	32.93	3,700
Population share aged 35–54								
1989–1989	26.21	1.38	25.31	26.19	26.96	23.06	30.32	185
1990–2009	29.87	2.56	27.75	29.58	31.86	23.96	36.60	3,700
Population share older than 54								
1989–1989	24.64	3.00	22.84	24.53	26.59	12.92	31.73	185
1990–2009	30.70	4.04	27.93	30.61	33.36	14.41	42.37	3,700
Share of high-skilled								
1989–1989	8.51	2.78	6.80	7.64	9.23	5.41	22.66	185
1995–2009	3.98	1.84	2.90	3.60	4.30	1.90	16.80	2,775
Surveillance intensity and control variables								
Spying density	0.38	0.14	0.26	0.36	0.46	0.12	1.03	185
Dummy: Object of Special Interest	0.03	0.16	0.00	0.00	0.00	0.00	1.00	185
Log mean population 1980s	11.01	0.56	10.60	10.98	11.34	9.79	13.22	185
Log county size (in sqm)	5.97	0.76	5.74	6.13	6.54	3.26	7.14	185
Share of population aged below 15	19.72	1.79	18.60	19.76	20.94	15.56	24.74	185
Share of population aged above 64	13.57	2.27	12.05	13.53	15.02	5.68	19.33	185
Uprising intensity 1953	1.36	1.37	0.00	1.00	2.00	0.00	4.00	185
Dummy: Military intervention 1953	0.48	0.50	0.00	0.00	1.00	0.00	1.00	185
Dummy: State of Emergency 1953	0.68	0.47	0.00	1.00	1.00	0.00	1.00	185
Share indust. empl. 1989	45.37	13.56	35.00	47.10	56.20	16.80	74.50	185
Share aggric. empl. 1989	17.11	12.44	7.70	14.70	25.20	0.00	51.30	185
Industry concentration 1989	38.74	11.25	31.20	35.60	44.80	19.50	74.50	185
Dummy: Important industries 1989	0.23	0.42	0.00	0.00	0.00	0.00	1.00	185
Log industrial production 1989	20.92	1.31	20.05	21.10	21.87	16.99	23.73	185
Elites with SED membership	48.55	5.78	44.62	49.00	52.33	33.30	63.45	185
Log transfers	16.92	0.67	16.47	16.81	17.24	15.63	19.91	2,773
Log investment subsidies	16.23	0.66	15.78	16.16	16.61	14.61	19.06	2,773

Notes: This table presents outcome and control variables on district variables. For detailed information on all variables, see Appendix Table B.1.

Table A.3: The Effect of Spying on Trust and Social Ties – Sensitivity Tests

	County Pair FE OLS					County Pair FE Probit	
	(1) Adj. Wts	(2) Adj. Wts	(3) Adj. Wts	(4) Cs. Wts	(5) No Wts	(6) Adj. Wts	(7) Cross. Wts
A – Trust in strangers							
Spy density	-0.041**	-0.042**	-0.061***	-0.075***	-0.030*	-0.374***	-0.405***
	(0.018)	(0.021)	(0.018)	(0.018)	(0.016)	(0.096)	(0.087)
Person-Year observations	1,531	1,531	1,531	1,531	1,531	1,531	3,389
B – Negative reciprocity							
Spy density	-0.161***	-0.161***	-0.195***	-0.211***	-0.190***	-0.209***	-0.235***
	(0.051)	(0.048)	(0.060)	(0.063)	(0.056)	(0.060)	(0.061)
Person-Year observations	1,369	1,369	1,369	1,369	1,369	1,369	1,369
C – Number of close friends							
Spy density	-0.416***	-0.387***	-0.428***	-0.469***	-0.289*	-0.181***	-0.185***
	(0.156)	(0.127)	(0.146)	(0.141)	(0.170)	(0.051)	(0.048)
Person-Year observations	1,460	1,460	1,460	1,460	1,460	1,460	3,248
D – Sociability							
Spy density	-0.031	-0.050	-0.128*	-0.070	-0.071	-0.130**	-0.057
	(0.083)	(0.078)	(0.067)	(0.062)	(0.054)	(0.062)	(0.057)
Person-Year observations	1,424	1,424	1,424	1,424	1,424	1,424	1,424
E – Voluntary work							
Spy density	0.013	0.009	-0.028	-0.026	0.003	-0.115	-0.095
	(0.022)	(0.018)	(0.023)	(0.022)	(0.020)	(0.078)	(0.078)
Person-Year observations	1,661	1,661	1,661	1,661	1,661	1,661	1,661
F – Engagement in local politics							
Spy density	-0.004	-0.002	-0.041**	-0.038**	-0.020	-0.485***	-0.485***
	(0.019)	(0.017)	(0.017)	(0.017)	(0.018)	(0.099)	(0.096)
Person-Year observations	1,625	1,625	1,625	1,625	1,625	1,625	1,625
Individual controls		Yes	Yes	Yes	Yes	Yes	Yes
County controls			Yes	Yes	Yes	Yes	Yes

Notes: This table shows β coefficients using different specifications of the border discontinuity model laid out in equation (1) for a one standard deviation increase in the informer density. For better comparability, negative reciprocity is defined such that higher values indicate less negative reciprocal behavior. Mean outcomes are 0.14 for trust in strangers, 2.69 for negative reciprocity, 3.95 for the number of close friends, 3.43 for sociability, 0.24 for engagement in voluntary work, and 0.11 for engagement in local politics, respectively. All regressions include county pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the county pair and county level in columns (1)-(5), and clustered at the county pair level in columns (6)-(7). We restrict the sample to county pairs for which we observe individuals in both counties along the state border. In columns (1)-(3) and column (6), cross-sectional weights are adjusted for duplicates of counties that are part of multiple county pairs. In columns (4) and (7), standard cross-sectional weights are used. Results presented in column (5) are without sample weights. For detailed information on the control variables, see Data Appendix B.

Table A.4: The Effect of Spying on Log Labor Income – Baseline, Identification and Sensitivity Tests

	Base Sample	County Pair Sample					
	(1) Cs. Wts	(2) Adj. Wts	(3) Adj. Wts	(4) Adj. Wts	(5) Adj. Wts	(6) Cs. Wts	(7) No Wts
Spying density × 1992	0.004	-0.011	-0.175***	-0.075***	-0.065***	-0.085***	-0.050***
	(0.014)	(0.018)	(0.029)	(0.018)	(0.016)	(0.021)	(0.017)
Spying density × 1993	0.010	-0.009	-0.119***	-0.074***	-0.065***	-0.082***	-0.059***
	(0.015)	(0.019)	(0.026)	(0.018)	(0.020)	(0.019)	(0.019)
Spying density × 1994	0.007	-0.010	-0.076***	-0.073***	-0.063***	-0.075***	-0.047**
	(0.017)	(0.020)	(0.026)	(0.020)	(0.020)	(0.019)	(0.021)
Spying density × 1995	0.021	0.002	-0.066***	-0.064***	-0.054***	-0.071***	-0.048***
	(0.014)	(0.018)	(0.023)	(0.016)	(0.016)	(0.018)	(0.017)
Spying density × 1996	0.010	-0.017	-0.062***	-0.085***	-0.074***	-0.095***	-0.058***
	(0.012)	(0.017)	(0.022)	(0.019)	(0.018)	(0.021)	(0.019)
Spying density × 1997	0.021	0.002	-0.053**	-0.064***	-0.054***	-0.069***	-0.048***
	(0.014)	(0.020)	(0.022)	(0.020)	(0.018)	(0.018)	(0.018)
Spying density × 1998	0.020	-0.006	-0.049**	-0.072***	-0.061***	-0.076***	-0.045**
	(0.015)	(0.020)	(0.022)	(0.021)	(0.019)	(0.021)	(0.017)
Spying density × 1999	0.002	-0.045**	-0.052**	-0.111***	-0.102***	-0.123***	-0.072***
	(0.016)	(0.021)	(0.023)	(0.021)	(0.021)	(0.023)	(0.021)
Spying density × 2000	0.009	-0.016	-0.046*	-0.084***	-0.073***	-0.070***	-0.057***
	(0.019)	(0.023)	(0.024)	(0.020)	(0.023)	(0.021)	(0.021)
Spying density × 2001	0.002	-0.021	-0.039	-0.088***	-0.078***	-0.082***	-0.069***
	(0.019)	(0.024)	(0.024)	(0.022)	(0.022)	(0.024)	(0.022)
Spying density × 2002	0.014	-0.007	-0.040	-0.074***	-0.065**	-0.065**	-0.066***
	(0.021)	(0.030)	(0.026)	(0.022)	(0.026)	(0.032)	(0.022)
Spying density × 2003	0.014	-0.023	-0.046*	-0.090***	-0.080***	-0.069**	-0.076***
	(0.020)	(0.031)	(0.026)	(0.023)	(0.026)	(0.028)	(0.026)
Spying density × 2004	0.034	0.009	-0.047*	-0.056**	-0.045	-0.052*	-0.061**
	(0.023)	(0.031)	(0.026)	(0.023)	(0.029)	(0.026)	(0.029)
Spying density × 2005	0.036	0.024	-0.040	-0.041*	-0.031	-0.031	-0.067**
	(0.025)	(0.032)	(0.027)	(0.023)	(0.028)	(0.025)	(0.026)
Spying density × 2006	0.019	0.020	-0.032	-0.049***	-0.037*	-0.050**	-0.061**
	(0.019)	(0.023)	(0.022)	(0.017)	(0.021)	(0.021)	(0.027)
Spying density × 2007	0.009	0.007	-0.046*	-0.059**	-0.053**	-0.074***	-0.067**
	(0.026)	(0.025)	(0.024)	(0.024)	(0.027)	(0.025)	(0.028)
Spying density × 2008	0.021	0.027	-0.043	-0.043*	-0.031	-0.043	-0.047
	(0.024)	(0.034)	(0.026)	(0.025)	(0.031)	(0.030)	(0.030)
Spying density × 2009	0.011	0.018	-0.041	-0.046**	-0.034	-0.023	-0.053*
	(0.025)	(0.029)	(0.026)	(0.020)	(0.026)	(0.027)	(0.032)
Person-Year observations	21,808	9,562	9,562	9,562	9,562	9,562	9,562
Border County Pair FE			Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes		Yes	Yes	Yes	Yes
County Controls	Yes	Yes			Yes	Yes	Yes

Notes: This table shows β coefficients using different specifications of the border discontinuity model laid out in equation (1) for a one standard deviation increase in the informer density. All regressions include a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the county and individual level in the base sample, and two-way clustered at the county pair and county level in the county pair sample. The usual confidence levels apply (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). In columns (1) and (6), cross-sectional weights are used. In columns (2)–(5), cross-sectional weights are adjusted for duplicates of counties that are part of multiple border county pairs. In columns (7), no sample weights are used. For detailed information on the control variables, see Data Appendix B.

Table A.5: The Effect of Spying on Log Labor Income - Potential Mechanisms

	County Pair Sample				
	(1)	(2)	(3)	(4)	(5)
Spying density × 1992	-0.066***	-0.066***		-0.064***	
	(0.017)	(0.017)		(0.016)	
Spying density × 1993	-0.057**	-0.057**		-0.055**	
	(0.024)	(0.024)		(0.024)	
Spying density × 1994	-0.059**	-0.059**		-0.057**	
	(0.025)	(0.025)		(0.025)	
Spying density × 1995	-0.046**	-0.046**	-0.058***	-0.044**	-0.055**
	(0.019)	(0.019)	(0.020)	(0.019)	(0.021)
Spying density × 1996	-0.066***	-0.066***	-0.077***	-0.063**	-0.074***
	(0.024)	(0.024)	(0.025)	(0.024)	(0.025)
Spying density × 1997	-0.039	-0.039	-0.050**	-0.035	-0.045*
	(0.024)	(0.024)	(0.025)	(0.025)	(0.026)
Spying density × 1998	-0.037**	-0.038**	-0.045**	-0.033*	-0.038*
	(0.018)	(0.018)	(0.020)	(0.019)	(0.022)
Spying density × 1999	-0.090***	-0.090***	-0.097***	-0.085***	-0.089***
	(0.023)	(0.024)	(0.024)	(0.024)	(0.026)
Spying density × 2000	-0.085***	-0.085***	-0.090***	-0.080***	-0.082***
	(0.025)	(0.026)	(0.025)	(0.026)	(0.028)
Spying density × 2001	-0.067**	-0.067**	-0.073**	-0.062*	-0.065*
	(0.030)	(0.030)	(0.031)	(0.032)	(0.034)
Spying density × 2002	-0.069**	-0.069**	-0.075**	-0.064*	-0.065*
	(0.034)	(0.034)	(0.034)	(0.035)	(0.035)
Spying density × 2003	-0.105***	-0.105***	-0.109***	-0.099***	-0.099**
	(0.034)	(0.035)	(0.035)	(0.036)	(0.038)
Spying density × 2004	-0.071*	-0.071*	-0.073**	-0.065*	-0.062*
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
Spying density × 2005	-0.072**	-0.072**	-0.075**	-0.065*	-0.064*
	(0.032)	(0.033)	(0.032)	(0.034)	(0.035)
Spying density × 2006	-0.027	-0.028	-0.033	-0.021	-0.022
	(0.028)	(0.028)	(0.028)	(0.029)	(0.031)
Spying density × 2007	-0.025	-0.025	-0.034	-0.018	-0.023
	(0.034)	(0.034)	(0.032)	(0.034)	(0.033)
Spying density × 2008	-0.013	-0.013	-0.023	-0.007	-0.013
	(0.047)	(0.047)	(0.046)	(0.048)	(0.047)
Spying density × 2009	-0.079**	-0.079**	-0.088**	-0.072*	-0.077**
	(0.036)	(0.036)	(0.034)	(0.037)	(0.036)
Person-Year observations	7,811	7,811	5,896	7,811	5,896
Individual controls	Yes	Yes	Yes	Yes	Yes
County controls	Yes	Yes	Yes	Yes	Yes
Share SED party members		Yes			Yes
Government transfers			Yes		Yes
Population growth				Yes	Yes

Notes: This table shows the β coefficients of the border discontinuity model laid out in equation (1) using SOEP data for a one standard deviation increase in the informer density. As population growth rates are not available for East Berlin, all corresponding county pairs are excluded. Note that (unreported) estimates based on the full county pair sample are robust to the share of SED party members and government transfers, too. All specifications include county pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the county pair and county level with usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to county pairs for which we observe individuals in both counties along the state border. All specifications use cross-sectional weights adjusted for duplicates of counties that are part of multiple county pairs. For detailed information on the control variables, see Data Appendix B.

Table A.6: The Effect of Spying on Self-Employment Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Spying density × 1925	-0.191	-0.191	0.000	0.000	0.000	0.000	0.000	0.012
	(0.189)	(0.189)	(0.205)	(0.205)	(0.205)	(0.205)	(0.205)	(0.206)
Spying density × 1996	-0.593***	-0.177	-0.458**	-0.502**	-0.384**	-0.369*	-0.379**	-0.391**
	(0.159)	(0.205)	(0.196)	(0.207)	(0.191)	(0.193)	(0.191)	(0.180)
Spying density × 1997	-0.600***	-0.184	-0.450**	-0.494**	-0.376*	-0.361*	-0.372*	-0.382**
	(0.166)	(0.212)	(0.199)	(0.209)	(0.193)	(0.194)	(0.192)	(0.182)
Spying density × 1998	-0.580***	-0.164	-0.449**	-0.493**	-0.375*	-0.360*	-0.373*	-0.382**
	(0.167)	(0.214)	(0.199)	(0.209)	(0.192)	(0.193)	(0.193)	(0.182)
Spying density × 1999	-0.564***	-0.148	-0.439**	-0.482**	-0.365*	-0.349*	-0.362*	-0.372**
	(0.171)	(0.220)	(0.199)	(0.209)	(0.191)	(0.192)	(0.191)	(0.181)
Spying density × 2000	-0.562***	-0.146	-0.403**	-0.447**	-0.329*	-0.314	-0.329*	-0.337*
	(0.175)	(0.222)	(0.196)	(0.206)	(0.189)	(0.191)	(0.189)	(0.179)
Spying density × 2001	-0.493***	-0.077	-0.372*	-0.415**	-0.297	-0.282	-0.299	-0.306*
	(0.176)	(0.223)	(0.198)	(0.208)	(0.191)	(0.192)	(0.191)	(0.182)
Spying density × 2002	-0.479***	-0.063	-0.368*	-0.411**	-0.294	-0.278	-0.292	-0.304*
	(0.177)	(0.223)	(0.199)	(0.208)	(0.191)	(0.192)	(0.191)	(0.183)
Spying density × 2003	-0.462**	-0.046	-0.351*	-0.394*	-0.277	-0.261	-0.276	-0.288
	(0.183)	(0.228)	(0.202)	(0.212)	(0.194)	(0.194)	(0.195)	(0.186)
Spying density × 2004	-0.463**	-0.047	-0.346*	-0.389*	-0.272	-0.256	-0.268	-0.285
	(0.187)	(0.231)	(0.204)	(0.214)	(0.197)	(0.197)	(0.197)	(0.189)
Spying density × 2005	-0.402**	0.014	-0.317	-0.361*	-0.243	-0.228	-0.239	-0.258
	(0.192)	(0.236)	(0.207)	(0.216)	(0.198)	(0.198)	(0.198)	(0.191)
Spying density × 2006	-0.446**	-0.030	-0.357*	-0.401*	-0.283	-0.268	-0.281	-0.301
	(0.193)	(0.235)	(0.208)	(0.217)	(0.199)	(0.199)	(0.199)	(0.192)
Spying density × 2007	-0.423**	-0.007	-0.365*	-0.409*	-0.291	-0.276	-0.294	-0.311
	(0.187)	(0.231)	(0.208)	(0.217)	(0.199)	(0.199)	(0.199)	(0.193)
Spying density × 2008	-0.369**	0.047	-0.373*	-0.416*	-0.299	-0.283	-0.301	-0.320*
	(0.185)	(0.231)	(0.205)	(0.213)	(0.196)	(0.196)	(0.195)	(0.189)
Spying density × 2009	-0.358*	0.058	-0.408**	-0.452**	-0.334*	-0.319	-0.333*	-0.357*
	(0.185)	(0.232)	(0.204)	(0.213)	(0.196)	(0.195)	(0.196)	(0.189)
Post × Object of SI x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes	Yes
Post × Elite SED members						Yes		
Post × Transfers							Yes	
Log current population								Yes
Observations	2960	2960	2960	2960	2960	2960	2958	2960
Adjusted R^2	0.877	0.886	0.920	0.921	0.926	0.927	0.926	0.927

Notes: This table shows the β_t coefficients of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The mean post-reunification self-employment rate is 10.42 %. All specifications include district and year fixed effects. Standard errors are clustered at the district level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi density times year interaction for 1933 is omitted. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq$ November 1989). Object of SI stands for Object of Special Interest. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. See Section 3.3 for the definition of control sets and Data Appendix B for detailed information on all variables used.

Table A.7: The Effect of Spying on Unemployment Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Spying density × 1996	3.092***	0.538	0.872***	0.902***	0.916***	0.912***	0.924***	0.929***
	(0.431)	(0.358)	(0.324)	(0.327)	(0.337)	(0.336)	(0.337)	(0.332)
Spying density × 1997	2.907***	0.352	0.733**	0.763**	0.778**	0.773**	0.783**	0.789**
	(0.435)	(0.365)	(0.328)	(0.332)	(0.342)	(0.340)	(0.340)	(0.336)
Spying density × 1998	2.746***	0.192	0.467	0.498	0.512	0.507	0.512	0.524
	(0.418)	(0.365)	(0.319)	(0.322)	(0.331)	(0.330)	(0.331)	(0.324)
Spying density × 1999	2.768***	0.214	0.399	0.430	0.444	0.439	0.444	0.457
	(0.402)	(0.355)	(0.306)	(0.312)	(0.321)	(0.319)	(0.320)	(0.313)
Spying density × 2000	2.886***	0.332	0.499	0.529*	0.543*	0.538*	0.540*	0.557*
	(0.402)	(0.356)	(0.308)	(0.315)	(0.324)	(0.323)	(0.323)	(0.318)
Spying density × 2001	3.044***	0.490	0.572*	0.602*	0.617*	0.612*	0.612*	0.632*
	(0.401)	(0.347)	(0.312)	(0.321)	(0.331)	(0.329)	(0.331)	(0.326)
Spying density × 2002	2.985***	0.431	0.528*	0.558*	0.573*	0.568*	0.573*	0.590*
	(0.404)	(0.347)	(0.319)	(0.329)	(0.337)	(0.335)	(0.337)	(0.331)
Spying density × 2003	3.227***	0.673*	0.613*	0.643*	0.658*	0.653*	0.658*	0.678*
	(0.438)	(0.367)	(0.337)	(0.345)	(0.352)	(0.350)	(0.352)	(0.348)
Spying density × 2004	3.349***	0.795**	0.683**	0.713**	0.728**	0.723**	0.736**	0.750**
	(0.446)	(0.376)	(0.346)	(0.354)	(0.360)	(0.359)	(0.360)	(0.357)
Spying density × 2005	3.245***	0.691*	0.766**	0.796**	0.810**	0.806**	0.820**	0.836**
	(0.426)	(0.366)	(0.338)	(0.348)	(0.354)	(0.353)	(0.353)	(0.350)
Spying density × 2006	3.333***	0.779**	0.787**	0.817**	0.832**	0.827**	0.838**	0.860**
	(0.423)	(0.370)	(0.345)	(0.354)	(0.360)	(0.358)	(0.360)	(0.359)
Spying density × 2007	3.332***	0.778**	0.754**	0.784**	0.798**	0.794**	0.797**	0.830**
	(0.416)	(0.368)	(0.351)	(0.360)	(0.368)	(0.366)	(0.369)	(0.366)
Spying density × 2008	3.141***	0.586*	0.593*	0.623*	0.638*	0.633*	0.636*	0.673*
	(0.393)	(0.351)	(0.326)	(0.336)	(0.346)	(0.344)	(0.348)	(0.344)
Spying density × 2009	2.942***	0.387	0.567*	0.597*	0.611*	0.607*	0.632*	0.650*
	(0.377)	(0.339)	(0.321)	(0.332)	(0.343)	(0.340)	(0.343)	(0.339)
Post × Object of SI x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes	Yes
Post × Elite SED members						Yes		
Post × Transfers							Yes	
Log current population								Yes
Observations	2775	2775	2775	2775	2775	2775	2773	2775
Adjusted R^2	0.602	0.722	0.829	0.835	0.836	0.836	0.836	0.838

Notes: This table shows the β_t coefficients of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The mean post-reunification unemployment rate is 18.66 %. All specifications include district and year fixed effects. Standard errors are clustered at the district level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi density times year interaction for 1933 is omitted. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq$ November 1989). Object of SI stands for Object of Special Interest. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. See Section 3.3 for the definition of control sets and Data Appendix B for detailed information on all variables used.

Table A.8: The Effect of Spying on Log Population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spying density × 1985	-0.001	-0.001	0.001	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Spying density × 1986	-0.000	-0.000	0.001	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Spying density × 1987	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Spying density × 1989	0.002***	-0.014***	-0.013***	-0.014***	-0.013***	-0.013***	-0.012**
	(0.001)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Spying density × 1990	0.004***	-0.012***	-0.012***	-0.013***	-0.012***	-0.012***	
	(0.001)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	
Spying density × 1991	0.003***	-0.013***	-0.013***	-0.014***	-0.013***	-0.013***	
	(0.001)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	
Spying density × 1992	0.004***	-0.012***	-0.012***	-0.013***	-0.012***	-0.012***	
	(0.001)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	
Spying density × 1993	0.004***	-0.012***	-0.012***	-0.013***	-0.012***	-0.012***	
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	
Spying density × 1994	0.006***	-0.010***	-0.012***	-0.013***	-0.012***	-0.012***	
	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	
Spying density × 1995	0.008***	-0.009**	-0.011**	-0.012***	-0.012**	-0.012**	-0.015***
	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)	(0.006)
Spying density × 1996	0.010***	-0.006	-0.011**	-0.012**	-0.011**	-0.011**	-0.014**
	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Spying density × 1997	0.013***	-0.004	-0.010*	-0.011*	-0.010	-0.010	-0.014**
	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Spying density × 1998	0.014***	-0.002	-0.011	-0.011*	-0.011	-0.011	-0.014*
	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Spying density × 1999	0.016***	-0.000	-0.011	-0.012	-0.011	-0.011	-0.015*
	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Spying density × 2000	0.017***	0.001	-0.012	-0.013	-0.012	-0.012	-0.016*
	(0.006)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)
Spying density × 2001	0.017**	0.001	-0.013	-0.014	-0.013	-0.013	-0.017*
	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Spying density × 2002	0.017**	0.001	-0.014	-0.015	-0.014	-0.014	-0.018*
	(0.007)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)
Spying density × 2003	0.017**	0.000	-0.016	-0.017*	-0.016	-0.016	-0.020*
	(0.007)	(0.008)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Spying density × 2004	0.016**	0.000	-0.017*	-0.018*	-0.017*	-0.017*	-0.021*
	(0.008)	(0.008)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)
Spying density × 2005	0.016*	-0.001	-0.019*	-0.020*	-0.019*	-0.019*	-0.023**
	(0.008)	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)
Spying density × 2006	0.015*	-0.001	-0.021**	-0.022**	-0.022**	-0.021*	-0.025**
	(0.008)	(0.009)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Spying density × 2007	0.015*	-0.002	-0.023**	-0.024**	-0.024**	-0.023**	-0.027**
	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Spying density × 2008	0.014	-0.003	-0.026**	-0.026**	-0.026**	-0.026**	-0.030**
	(0.009)	(0.009)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
Spying density × 2009	0.013	-0.003	-0.028**	-0.028**	-0.028**	-0.028**	-0.030**
	(0.009)	(0.010)	(0.011)	(0.011)	(0.012)	(0.012)	(0.012)
Post × Object of SI x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Post × Elite SED members						Yes	
Post × Transfers							Yes
Observations	4625	4625	4625	4625	4625	4625	3698
Adjusted R^2	0.526	0.553	0.688	0.690	0.691	0.691	0.754

Notes: This table shows the β_t coefficients of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. All specifications include district and year fixed effects. Standard errors are clustered at the district level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi density times year interaction for 1988 is omitted. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq$ November 1989). Object of SI stands for Object of Special Interest. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. See Section 3.3 for the definition of control sets and Data Appendix B for detailed information on all variables used.

Table A.9: The Effect of Spying on Share of High-Skilled

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spying density × 1995	0.276*	0.040	0.083	0.088	0.153	0.154	0.156
	(0.145)	(0.152)	(0.183)	(0.185)	(0.180)	(0.179)	(0.179)
Spying density × 1996	0.329**	0.092	0.092	0.097	0.162	0.162	0.166
	(0.146)	(0.151)	(0.182)	(0.184)	(0.178)	(0.177)	(0.177)
Spying density × 1997	0.351**	0.115	0.115	0.120	0.185	0.185	0.188
	(0.148)	(0.150)	(0.180)	(0.183)	(0.177)	(0.176)	(0.175)
Spying density × 1998	0.366**	0.130	0.136	0.141	0.205	0.206	0.208
	(0.148)	(0.149)	(0.179)	(0.181)	(0.175)	(0.174)	(0.174)
Spying density × 1999	0.360**	0.123	0.150	0.155	0.220	0.221	0.223
	(0.151)	(0.151)	(0.179)	(0.181)	(0.174)	(0.173)	(0.174)
Spying density × 2000	0.355**	0.119	0.149	0.154	0.219	0.220	0.219
	(0.150)	(0.150)	(0.178)	(0.180)	(0.173)	(0.172)	(0.172)
Spying density × 2001	0.356**	0.120	0.146	0.151	0.216	0.216	0.214
	(0.151)	(0.150)	(0.178)	(0.180)	(0.174)	(0.173)	(0.172)
Spying density × 2002	0.362**	0.126	0.144	0.149	0.214	0.214	0.213
	(0.149)	(0.149)	(0.179)	(0.181)	(0.175)	(0.174)	(0.174)
Spying density × 2003	0.372**	0.136	0.142	0.147	0.212	0.213	0.210
	(0.151)	(0.148)	(0.179)	(0.181)	(0.175)	(0.175)	(0.175)
Spying density × 2004	0.378**	0.142	0.152	0.156	0.221	0.222	0.221
	(0.151)	(0.148)	(0.180)	(0.182)	(0.176)	(0.175)	(0.175)
Spying density × 2005	0.369**	0.133	0.138	0.143	0.207	0.208	0.207
	(0.152)	(0.148)	(0.182)	(0.184)	(0.179)	(0.178)	(0.177)
Spying density × 2006	0.361**	0.125	0.135	0.140	0.205	0.206	0.202
	(0.151)	(0.148)	(0.181)	(0.183)	(0.179)	(0.178)	(0.177)
Spying density × 2007	0.338**	0.101	0.117	0.122	0.187	0.188	0.179
	(0.148)	(0.146)	(0.180)	(0.182)	(0.178)	(0.178)	(0.177)
Spying density × 2008	0.319**	0.082	0.095	0.100	0.164	0.165	0.158
	(0.147)	(0.145)	(0.181)	(0.182)	(0.179)	(0.178)	(0.178)
Spying density × 2009	0.301**	0.064	0.087	0.092	0.157	0.158	0.155
	(0.146)	(0.145)	(0.181)	(0.183)	(0.180)	(0.180)	(0.177)
Post × Object of SI x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Post × Elite SED members						Yes	
Post × Transfers							Yes
Observations	2960	2960	2960	2960	2960	2960	2958
Adjusted R^2	0.833	0.887	0.905	0.906	0.913	0.913	0.914

Notes: This table shows the β_t coefficients of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The mean post-reunification high-skill share is 3.98 %. All specifications include district and year fixed effects. Standard errors are clustered at the district level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi density times year interaction for 1989 is omitted. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq$ November 1989). Object of SI stands for Object of Special Interest. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. See Section 3.3 for the definition of control sets and Data Appendix B for detailed information on all variables used.

Table A.10: The Effect of Spying on Population Share below 15

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spying density × 1990	-0.003	0.356***	0.025	0.029	0.039	0.039	
	(0.047)	(0.064)	(0.072)	(0.074)	(0.073)	(0.073)	
Spying density × 1991	-0.022	0.338***	0.045	0.049	0.058	0.058	
	(0.048)	(0.058)	(0.065)	(0.067)	(0.067)	(0.067)	
Spying density × 1992	-0.036	0.324***	0.068	0.072	0.081	0.081	
	(0.052)	(0.053)	(0.060)	(0.062)	(0.062)	(0.062)	
Spying density × 1993	-0.071	0.289***	0.082	0.086	0.095*	0.095*	
	(0.058)	(0.048)	(0.053)	(0.055)	(0.055)	(0.055)	
Spying density × 1994	-0.114	0.246***	0.068	0.071	0.081	0.081	
	(0.069)	(0.048)	(0.051)	(0.053)	(0.052)	(0.052)	
Spying density × 1995	-0.150*	0.209***	0.068	0.072	0.081	0.081	0.086
	(0.080)	(0.050)	(0.050)	(0.052)	(0.050)	(0.050)	(0.056)
Spying density × 1996	-0.171**	0.189***	0.080	0.084	0.093*	0.093*	0.095*
	(0.084)	(0.049)	(0.050)	(0.052)	(0.050)	(0.050)	(0.055)
Spying density × 1997	-0.199**	0.160***	0.081	0.085*	0.094*	0.094*	0.097*
	(0.088)	(0.048)	(0.050)	(0.051)	(0.049)	(0.049)	(0.052)
Spying density × 1998	-0.222**	0.137***	0.079	0.083	0.092*	0.092*	0.094*
	(0.091)	(0.047)	(0.050)	(0.051)	(0.050)	(0.050)	(0.051)
Spying density × 1999	-0.258***	0.101**	0.069	0.072	0.082	0.082	0.084*
	(0.095)	(0.048)	(0.052)	(0.053)	(0.051)	(0.051)	(0.050)
Spying density × 2000	-0.302***	0.057	0.054	0.058	0.067	0.067	0.071
	(0.099)	(0.049)	(0.053)	(0.054)	(0.052)	(0.052)	(0.050)
Spying density × 2001	-0.350***	0.010	0.029	0.033	0.042	0.042	0.049
	(0.102)	(0.050)	(0.055)	(0.055)	(0.054)	(0.054)	(0.051)
Spying density × 2002	-0.405***	-0.046	-0.001	0.002	0.012	0.012	0.019
	(0.105)	(0.052)	(0.056)	(0.056)	(0.055)	(0.055)	(0.052)
Spying density × 2003	-0.453***	-0.094*	-0.030	-0.026	-0.016	-0.016	-0.007
	(0.107)	(0.052)	(0.057)	(0.057)	(0.056)	(0.056)	(0.052)
Spying density × 2004	-0.495***	-0.135**	-0.051	-0.047	-0.037	-0.037	-0.027
	(0.110)	(0.055)	(0.059)	(0.059)	(0.059)	(0.059)	(0.054)
Spying density × 2005	-0.523***	-0.164***	-0.067	-0.063	-0.053	-0.053	-0.043
	(0.112)	(0.057)	(0.061)	(0.060)	(0.060)	(0.060)	(0.055)
Spying density × 2006	-0.542***	-0.183***	-0.089	-0.085	-0.076	-0.075	-0.061
	(0.112)	(0.058)	(0.061)	(0.061)	(0.061)	(0.061)	(0.055)
Spying density × 2007	-0.562***	-0.202***	-0.113*	-0.109*	-0.099	-0.099	-0.080
	(0.114)	(0.060)	(0.063)	(0.063)	(0.064)	(0.064)	(0.058)
Spying density × 2008	-0.573***	-0.213***	-0.135**	-0.131**	-0.122*	-0.122*	-0.104*
	(0.114)	(0.062)	(0.065)	(0.065)	(0.065)	(0.065)	(0.060)
Spying density × 2009	-0.583***	-0.223***	-0.153**	-0.149**	-0.140**	-0.140**	-0.105*
	(0.114)	(0.063)	(0.066)	(0.066)	(0.066)	(0.066)	(0.060)
Post × Object of SI x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Post × Elite SED members						Yes	
Post × Transfers							Yes
Observations	3885	3885	3885	3885	3885	3885	2958
Adjusted R^2	0.969	0.974	0.985	0.985	0.985	0.985	0.990

Notes: This table shows the β_t coefficients of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The mean post-reunification population share is 14.08 %. All specifications include district and year fixed effects. Standard errors are clustered at the district level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi density times year interaction for 1989 is omitted. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq$ November 1989). Object of SI stands for Object of Special Interest. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. See Section 3.3 for the definition of control sets and Data Appendix B for detailed information on all variables used.

Table A.11: The Effect of Spying on Population Share 15–34

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spying density × 1990	-0.006	0.358***	0.169*	0.161	0.138	0.139	
	(0.068)	(0.085)	(0.095)	(0.099)	(0.093)	(0.092)	
Spying density × 1991	-0.055	0.308***	0.114	0.106	0.083	0.084	
	(0.071)	(0.084)	(0.092)	(0.097)	(0.090)	(0.089)	
Spying density × 1992	-0.081	0.283***	0.078	0.069	0.047	0.048	
	(0.076)	(0.087)	(0.096)	(0.101)	(0.094)	(0.093)	
Spying density × 1993	-0.133*	0.231***	0.047	0.038	0.016	0.017	
	(0.079)	(0.086)	(0.096)	(0.099)	(0.092)	(0.092)	
Spying density × 1994	-0.163*	0.201**	0.044	0.036	0.013	0.014	
	(0.088)	(0.086)	(0.099)	(0.104)	(0.097)	(0.098)	
Spying density × 1995	-0.259***	0.105	-0.018	-0.026	-0.049	-0.048	-0.021
	(0.090)	(0.090)	(0.101)	(0.105)	(0.099)	(0.099)	(0.113)
Spying density × 1996	-0.306***	0.057	-0.034	-0.042	-0.065	-0.063	-0.039
	(0.095)	(0.091)	(0.101)	(0.105)	(0.100)	(0.100)	(0.114)
Spying density × 1997	-0.347***	0.017	-0.036	-0.044	-0.067	-0.066	-0.039
	(0.098)	(0.092)	(0.101)	(0.105)	(0.099)	(0.099)	(0.113)
Spying density × 1998	-0.411***	-0.047	-0.067	-0.075	-0.098	-0.097	-0.071
	(0.101)	(0.094)	(0.101)	(0.105)	(0.099)	(0.099)	(0.112)
Spying density × 1999	-0.456***	-0.093	-0.089	-0.097	-0.119	-0.118	-0.092
	(0.104)	(0.095)	(0.103)	(0.107)	(0.102)	(0.101)	(0.115)
Spying density × 2000	-0.492***	-0.128	-0.108	-0.116	-0.139	-0.138	-0.106
	(0.107)	(0.097)	(0.105)	(0.108)	(0.102)	(0.102)	(0.114)
Spying density × 2001	-0.548***	-0.185*	-0.143	-0.152	-0.174*	-0.173*	-0.137
	(0.111)	(0.100)	(0.107)	(0.110)	(0.105)	(0.105)	(0.114)
Spying density × 2002	-0.594***	-0.230**	-0.170	-0.179	-0.201*	-0.200*	-0.165
	(0.115)	(0.102)	(0.110)	(0.112)	(0.107)	(0.107)	(0.115)
Spying density × 2003	-0.637***	-0.274**	-0.217*	-0.225*	-0.248**	-0.247**	-0.209*
	(0.121)	(0.106)	(0.115)	(0.117)	(0.112)	(0.112)	(0.119)
Spying density × 2004	-0.682***	-0.318***	-0.266**	-0.274**	-0.297**	-0.296**	-0.259**
	(0.125)	(0.108)	(0.121)	(0.122)	(0.118)	(0.118)	(0.124)
Spying density × 2005	-0.736***	-0.372***	-0.339**	-0.347***	-0.370***	-0.369***	-0.332**
	(0.132)	(0.113)	(0.132)	(0.133)	(0.129)	(0.130)	(0.134)
Spying density × 2006	-0.782***	-0.419***	-0.402***	-0.410***	-0.433***	-0.431***	-0.388***
	(0.139)	(0.119)	(0.146)	(0.147)	(0.143)	(0.144)	(0.146)
Spying density × 2007	-0.841***	-0.477***	-0.472***	-0.480***	-0.503***	-0.502***	-0.448***
	(0.145)	(0.124)	(0.157)	(0.157)	(0.154)	(0.155)	(0.156)
Spying density × 2008	-0.917***	-0.554***	-0.542***	-0.550***	-0.573***	-0.572***	-0.519***
	(0.154)	(0.132)	(0.171)	(0.170)	(0.167)	(0.168)	(0.165)
Spying density × 2009	-0.987***	-0.623***	-0.601***	-0.610***	-0.632***	-0.631***	-0.573***
	(0.162)	(0.139)	(0.183)	(0.183)	(0.180)	(0.181)	(0.178)
Post × Object of SI x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Post × Elite SED members						Yes	
Post × Transfers							Yes
Observations	3885	3885	3885	3885	3885	3885	2958
Adjusted R^2	0.916	0.920	0.935	0.936	0.936	0.936	0.946

Notes: This table shows the β_t coefficients of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The mean post-reunification population share is 25.35 %. All specifications include district and year fixed effects. Standard errors are clustered at the district level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi density times year interaction for 1989 is omitted. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq$ November 1989). Object of SI stands for Object of Special Interest. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. See Section 3.3 for the definition of control sets and Data Appendix B for detailed information on all variables used.

Table A.12: The Effect of Spying on Population Share 35–54

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spying density × 1990	0.111	-0.708***	-0.428***	-0.431***	-0.369***	-0.369***	
	(0.070)	(0.097)	(0.124)	(0.126)	(0.127)	(0.127)	
Spying density × 1991	0.132*	-0.687***	-0.398***	-0.400***	-0.338***	-0.338***	
	(0.072)	(0.093)	(0.120)	(0.122)	(0.123)	(0.122)	
Spying density × 1992	0.159**	-0.660***	-0.373***	-0.375***	-0.313***	-0.313***	
	(0.075)	(0.090)	(0.114)	(0.116)	(0.117)	(0.117)	
Spying density × 1993	0.208***	-0.611***	-0.341***	-0.343***	-0.281**	-0.281**	
	(0.078)	(0.084)	(0.107)	(0.109)	(0.110)	(0.110)	
Spying density × 1994	0.305***	-0.513***	-0.283***	-0.285***	-0.223**	-0.223**	
	(0.087)	(0.077)	(0.097)	(0.098)	(0.100)	(0.100)	
Spying density × 1995	0.364***	-0.455***	-0.290***	-0.293***	-0.231**	-0.231**	-0.345***
	(0.094)	(0.073)	(0.087)	(0.088)	(0.089)	(0.089)	(0.113)
Spying density × 1996	0.442***	-0.377***	-0.248***	-0.250***	-0.188**	-0.188**	-0.302***
	(0.103)	(0.072)	(0.082)	(0.083)	(0.081)	(0.081)	(0.103)
Spying density × 1997	0.527***	-0.292***	-0.199**	-0.201**	-0.139*	-0.139*	-0.254***
	(0.114)	(0.075)	(0.079)	(0.080)	(0.077)	(0.077)	(0.094)
Spying density × 1998	0.640***	-0.178**	-0.132	-0.134	-0.072	-0.072	-0.188**
	(0.126)	(0.079)	(0.080)	(0.081)	(0.075)	(0.075)	(0.088)
Spying density × 1999	0.763***	-0.055	-0.073	-0.075	-0.013	-0.013	-0.129
	(0.137)	(0.086)	(0.084)	(0.085)	(0.078)	(0.078)	(0.086)
Spying density × 2000	0.871***	0.052	-0.023	-0.025	0.036	0.036	-0.082
	(0.148)	(0.094)	(0.090)	(0.090)	(0.082)	(0.082)	(0.085)
Spying density × 2001	0.991***	0.172*	0.033	0.030	0.092	0.092	-0.027
	(0.156)	(0.101)	(0.097)	(0.097)	(0.088)	(0.088)	(0.086)
Spying density × 2002	1.081***	0.262**	0.092	0.090	0.152	0.152	0.034
	(0.164)	(0.108)	(0.103)	(0.103)	(0.094)	(0.094)	(0.089)
Spying density × 2003	1.147***	0.328***	0.148	0.146	0.208**	0.208**	0.089
	(0.169)	(0.112)	(0.110)	(0.110)	(0.100)	(0.100)	(0.093)
Spying density × 2004	1.226***	0.407***	0.203*	0.200*	0.262**	0.262**	0.145
	(0.176)	(0.119)	(0.119)	(0.119)	(0.110)	(0.111)	(0.101)
Spying density × 2005	1.288***	0.469***	0.254**	0.252**	0.314***	0.314***	0.197*
	(0.181)	(0.124)	(0.126)	(0.126)	(0.118)	(0.118)	(0.106)
Spying density × 2006	1.329***	0.511***	0.296**	0.294**	0.356***	0.356***	0.237**
	(0.184)	(0.127)	(0.131)	(0.130)	(0.122)	(0.122)	(0.110)
Spying density × 2007	1.352***	0.534***	0.330**	0.328**	0.390***	0.390***	0.267**
	(0.184)	(0.127)	(0.134)	(0.133)	(0.127)	(0.127)	(0.116)
Spying density × 2008	1.361***	0.542***	0.358***	0.356***	0.418***	0.418***	0.295**
	(0.185)	(0.128)	(0.137)	(0.136)	(0.129)	(0.129)	(0.118)
Spying density × 2009	1.353***	0.534***	0.372***	0.370***	0.432***	0.432***	0.305**
	(0.184)	(0.128)	(0.140)	(0.139)	(0.132)	(0.132)	(0.123)
Post × Object of SI x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Post × Elite SED members						Yes	
Post × Transfers							Yes
Observations	3885	3885	3885	3885	3885	3885	2958
Adjusted R^2	0.867	0.880	0.910	0.910	0.910	0.910	0.936

Notes: This table shows the β_t coefficients of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The mean post-reunification population share is 29.87%. All specifications include district and year fixed effects. Standard errors are clustered at the district level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi density times year interaction for 1989 is omitted. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq$ November 1989). Object of SI stands for Object of Special Interest. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. See Section 3.3 for the definition of control sets and Data Appendix B for detailed information on all variables used.

Table A.13: The Effect of Spying on Population Share above 54

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spying density × 1990	-0.102	-0.006	0.234	0.241	0.192	0.191	
	(0.135)	(0.135)	(0.155)	(0.157)	(0.157)	(0.158)	
Spying density × 1991	-0.055	0.040	0.239	0.246	0.197	0.196	
	(0.135)	(0.130)	(0.149)	(0.151)	(0.152)	(0.152)	
Spying density × 1992	-0.042	0.054	0.228	0.234	0.185	0.184	
	(0.137)	(0.127)	(0.144)	(0.147)	(0.147)	(0.148)	
Spying density × 1993	-0.004	0.092	0.213	0.219	0.171	0.169	
	(0.140)	(0.120)	(0.134)	(0.137)	(0.137)	(0.138)	
Spying density × 1994	-0.029	0.066	0.172	0.178	0.130	0.128	
	(0.154)	(0.118)	(0.134)	(0.139)	(0.139)	(0.140)	
Spying density × 1995	0.045	0.141	0.240**	0.247**	0.198*	0.197	0.280**
	(0.160)	(0.122)	(0.117)	(0.122)	(0.118)	(0.119)	(0.135)
Spying density × 1996	0.035	0.131	0.201*	0.208*	0.159	0.158	0.246*
	(0.168)	(0.122)	(0.115)	(0.120)	(0.114)	(0.115)	(0.129)
Spying density × 1997	0.020	0.115	0.154	0.161	0.112	0.111	0.196
	(0.177)	(0.125)	(0.117)	(0.122)	(0.114)	(0.115)	(0.123)
Spying density × 1998	-0.008	0.088	0.120	0.126	0.078	0.076	0.165
	(0.187)	(0.130)	(0.122)	(0.127)	(0.118)	(0.118)	(0.121)
Spying density × 1999	-0.049	0.047	0.093	0.100	0.051	0.050	0.138
	(0.194)	(0.135)	(0.127)	(0.132)	(0.122)	(0.122)	(0.124)
Spying density × 2000	-0.077	0.019	0.078	0.084	0.035	0.034	0.117
	(0.199)	(0.140)	(0.131)	(0.136)	(0.125)	(0.126)	(0.125)
Spying density × 2001	-0.093	0.003	0.082	0.088	0.040	0.038	0.115
	(0.204)	(0.143)	(0.134)	(0.139)	(0.128)	(0.128)	(0.126)
Spying density × 2002	-0.082	0.014	0.080	0.086	0.037	0.036	0.113
	(0.209)	(0.146)	(0.137)	(0.141)	(0.131)	(0.131)	(0.128)
Spying density × 2003	-0.056	0.039	0.098	0.105	0.056	0.055	0.127
	(0.211)	(0.147)	(0.139)	(0.143)	(0.133)	(0.134)	(0.130)
Spying density × 2004	-0.049	0.047	0.114	0.121	0.072	0.071	0.141
	(0.214)	(0.149)	(0.142)	(0.145)	(0.137)	(0.137)	(0.133)
Spying density × 2005	-0.029	0.067	0.152	0.159	0.110	0.109	0.177
	(0.215)	(0.152)	(0.146)	(0.149)	(0.141)	(0.142)	(0.139)
Spying density × 2006	-0.005	0.090	0.194	0.201	0.152	0.151	0.212
	(0.219)	(0.158)	(0.153)	(0.156)	(0.149)	(0.150)	(0.148)
Spying density × 2007	0.050	0.146	0.254	0.261	0.212	0.211	0.261
	(0.220)	(0.161)	(0.159)	(0.162)	(0.158)	(0.159)	(0.159)
Spying density × 2008	0.129	0.225	0.319*	0.326*	0.277	0.276	0.329*
	(0.226)	(0.168)	(0.168)	(0.170)	(0.168)	(0.169)	(0.167)
Spying density × 2009	0.217	0.313*	0.383**	0.389**	0.341*	0.339*	0.373**
	(0.231)	(0.174)	(0.177)	(0.179)	(0.178)	(0.179)	(0.179)
Post × Object of SI x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Post × Elite SED members						Yes	
Post × Transfers							Yes
Observations	3885	3885	3885	3885	3885	3885	2958
Adjusted R^2	0.928	0.936	0.943	0.943	0.944	0.944	0.965

Notes: This table shows the β_t coefficients of the panel data model laid out in equation (2) for a one standard deviation increase in the informer density. The mean post-reunification population share is 30.70 %. All specifications include district and year fixed effects. Standard errors are clustered at the district level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi density times year interaction for 1989 is omitted. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq$ November 1989). Object of SI stands for Object of Special Interest. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. See Section 3.3 for the definition of control sets and Data Appendix B for detailed information on all variables used.

B Data Appendix

This appendix provides additional information on the different data sets and variables used for our empirical analysis. In Section B.1, we list each variable's characteristics and source. In Section B.2, we describe the harmonization of our county-level data to the administrative-territorial structure as of October 1990.

B.1 Variable Definitions

Table B.1: Definition of Variables and Data Sources

Variable	Years	Source
Panel A – Stasi Data (see Section 3.1)		
Spying density	1980–1988	The main explanatory variable of interest, the regional spying density, is calculated as the average spying density at the county level in the period 1980–1988 (see Section 3.1 for details). Data on unofficial informers are based on official Stasi records published by the Agency of the Federal Commissioner for the Stasi Records (*Bundesbeauftragter für die Unterlagen des Staatssicherheitsdienstes der ehemaligen Deutschen Demokratischen Republik*) and compiled by Müller-Enbergs (2008). Population figures come from the Statistical Yearbooks of the GDR. Our measure of spying density covers unofficial informers for political-operative penetration, homeland defense, or special operations, as well as leading informers (*IM zur politisch-operativen Durchdringung und Sicherung des Verantwortungsbereiches, IM der Abwehr mit Feindverbindung bzw. zur unmittelbaren Bearbeitung im Verdacht der Feindtätigkeit stehender Personen, IM im besonderen Einsatz, Führungs-IM*). In cases where the Stasi held offices in Objects of Special Interest, the number of informers attached to these offices was added to the number in the respective county.
Stasi employees	1982	The number of regular Stasi employees (*Hauptamtliche Mitarbeiter*) attached to county offices in 1982 was provided by Jens Gieseke.
Panel B – Individual SOEP Data (see Section 3.2)		
Labor income	1992–2009	Information on current monthly gross labor income is provided in every wave of the SOEP for East German respondents since 1992. We calculate real income in 2010 prices using the official German CPI (*Verbraucherpreisindex*). We only consider income out of regular employment, discarding marginal employees with earnings below the administrative cut-off of 400 euros.
Negative reciprocity	2005, 2010	We use three statements on *negative* reciprocity, response options varying on a seven-point scale. We follow Dohmen et al. (2009) by combining the three questions into one single measure. The respective questions read as follows: (i) "If I suffer a serious wrong, I will take revenge as soon as possible, no matter what the cost," (ii) "If somebody puts me in a difficult position, I will do the same to him/her," and (iii) "If somebody offends me, I will offend him/her back." We standardize the reciprocity measure so that its standard deviation is equal to one. Higher values indicate *less negative* reciprocal behavior.

continued

Table B.1 continued

Variable	Years	Source
Number of close friends	2003, 2008	The underlying question reads as follows: "What would you say: How many close friends do you have?" We drop respondents that indicate implausible high numbers of close friends (more than 15), which is equivalent to 0.9% of the sample, or 19 observations.
Participation in local politics	2001, 2007	Respondents are questioned about their involvement in citizen's groups, political parties and local governments (the question reads: "Which of the following activities do you take part in during your free time?"). Response options vary on a four point scale indicating weekly, monthly, less often or no involvement at all. We construct a zero/one dummy variable indicating whether respondents are involved (weekly, monthly, less often), or not.
Sociability	2005, 2009	The statement reads as follows: "I see myself as someone who is outgoing, sociable". Response options were given on a seven-point scale to allow respondents to express different levels of conviction. We standardize the variable by dividing it by its standard deviation.
Trust in strangers	2003, 2008	The question on interpersonal trust reads as follows: "If one is dealing with strangers, it is better to be careful before one can trust them." Response options were given on a four-point scale, allowing the respondents to totally or slightly agree, or totally or slightly disagree with the given statements. Following Glaeser et al. (2000), we define a dichotomous variable by grouping the former and latter two answers.
Volunteering in clubs	2001, 2007	Respondents were questioned about their volunteering activities in clubs or social services (the question reads: "Which of the following activities do you take part in during your free time?"). Response options vary on a four point scale indicating weekly, monthly, less often or no involvement at all. We construct a zero/one dummy variable indicating whether respondents are involved (weekly, monthly, less often), or not.
Control variables		The set of control variables includes information on the respondents' age, sex, household size, marital status, education and learned profession.
Panel C – County-Level Data (see Section 3.3)		
County size	1990	The area of each East German county is reported in Rudolph (1990).
Demographics	1989	Information on age-specific population shares are obtained from infas (n.d.).
	1990–2009	Collected from the Statistical Offices of the Federal States (*Statistische Landesämter*) and the Regional Database Germany (*Regionaldatenbank Deutschland*).
Election turnout	1928–1932	We use election turnout in the federal elections in the Weimar Republic in 1928, 1930, 07/1932 and 11/1932. The data is provided in the replication data of King et al. (2008), available at the Harvard Dataverse, handle: hdl/1902.1/11193.
Industry controls	1989	Information on the goods value of production is collected from infas (n.d.). Data on the industrial composition of the workforce as of September 1989 is reported in Rudolph (1990). We further collect information from various sources whether large enterprises from the uranium, coal, potash, oil or chemical industry were located in the respective county. We construct a zero/one dummy based on this data.

continued

Table B.1 continued

Variable	Years	Source
Opposition	1953	We use cartographic statistics published by the former West German Federal Ministry of Intra-German Relations (*Bundesministerium für gesamtdeutsche Fragen*) to create two dummy variables indicating whether the regime declared a state of emergency and whether the Soviet military intervened in the particular county. In addition, the data provides an ordinal, additive measure of strike intensity ("none", "strike", "demonstration", "riot", "liberation of prisoners"). The map is available in the archives of the Federal Foundation for the Reappraisal of the SED Dictatorship (*Bundesstiftung zur Aufarbeitung der SED-Diktatur*), signature: EA 111 1889.
Political ideology	1928–1932	We proxy historic political ideology by the mean vote shares for the Communist party (*Kommunistische Partei Deutschlands, KPD*) and the Nazi party (*Nationalsozialistische Deutsche Arbeiterpartei, NSDAP*) in the federal elections in 1928, 1930, 07/1932 and 11/1932 to construct two distinct measures of political ideology. Data on Weimar Republic election results are based on King et al. (2008).
Population	1925–1933	Population figures for the Weimar Republic are obtained from King et al. (2008) and Falter and Hänisch (1990).
	1980–1989	Data collected from the Statistical Yearbooks of the German Democratic Republic (*Statistische Jahrbücher der Deutschen Demokratischen Republik*).
	1990–2009	Collected from the Regional Database Germany (*Regionaldatenbank Deutschland*), the Statistical Offices of the Federal States (*Statistische Landesämter*) and the Working Group Regional Accounts (*Arbeitskreis Volkswirtschaftliche Gesamtrechnungen der Länder*).
Religion	1925	The share of protestants in the population was published in the 1925 census of the Weimar Republic (*Volkszählung 1925*). Our data stems from King et al. (2008).
Revenues	1995–2009	Data on revenues are obtained from the Regional Database Germany (*Regionaldatenbank Deutschland*). Revenues cover federal- and state-level transfers (*allgemeine Zuweisungen und Umlagen von Bund, Land, Gemeinden/Gemeindeverbänden*) as well as investment subsidies granted to the counties (*Zuweisungen und Zuschüsse für Investitionsförderungen*).
Self-employment	1925, 1933	County-level self-employment rates come from the 1925 and 1933 censuses of the Weimar Republic (*Volks- und Berufszählung 1925 und 1933*). Data for 1925 are obtained from Falter and Hänisch (1990); data for 1933 from King et al. (2008). Note that numbers for 1925 refer to households instead of individuals and should be considered as an approximation.
	1996–2009	County-level data on the share of self-employed is available in the INKAR data base of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*Bundesinstitut für Bau-, Stadt- und Raumforschung, BBSR*).
Skills	1989	We calculate the share of high-skilled employees using administrative data from the GDR that was used for economic planning. We combine three data sets that cover around 95% of the labor force in 1989. We consider all workers with high school diplomas or university degrees as high-skilled, given that only a small share of students was admitted to high schools in the GDR. The underlying datasets were taken from the Federal Archives (*Bundesarchiv*), signatures: DC 20 MD/1 (*Zentraler Kaderdatenspeicher*), DQ 3 MD/7 (*Datenspeicher Gesellschaftliches Arbeitsvermögen*) and DR 2 MD/1 (*Arbeitskräftedatenspeicher Volksbildung*).

continued

Table B.1 continued

Variable	Years	Source
		We replicate our analysis in Section 6.3 using alternative pre-reunification data on the county-level skill distribution in large public companies provided by the Statistical Offices of the East German Länder (*Gemeinsames Statistisches Amt in Berlin, Regionalstatistische Angaben 1989 in der Gliederung nach Kreisen in den Grenzen der Länder, 1990, pp. 15–21*). The results are very similar when using this alternative measure of skills.
	1995–2009	The share of high-skilled employees in the working-age population is taken from the INKAR data base of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*Bundesinstitut für Bau-, Stadt- und Raumforschung, BBSR*).
Socialist indoctrination	1988	We proxy regional socialist indoctrination by the share of political and economic elites that were members of the Socialist Unity Party (SED). We calculate this measure using data from the Central Cadre Database (*Zentraler Kaderdatenspeicher, ZKDS*). This large administrative data set was used for planning purposes and contains information on all political and economic executives of the GDR (except for employees of the Ministry for State Security, the Ministry of National Defence and the Ministry of Internal Affairs). The data set is taken from the Federal Archives (*Bundesarchiv*), signature: DC 20 MD/1.
Unemployment	1933	County-level unemployment rates are based on the 1933 census of the Weimar Republic (*Volks- und Berufszählung 1933*), provided in King et al. (2008).
	1996–2009	Monthly county-level unemployment rates are made available from March 1996 to December 2009 by the Federal Employment Agency (*Bundesagentur für Arbeit*). We calculate yearly means from this data.

B.2 Redrawn County Boundaries and Data Harmonization

We combine county-level data from various sources and decades in this study. Since 1924, the first data year in our analysis, county borders have been redrawn multiple times. To account for these territorial changes, we harmonize all county-level data to boundaries as of October 1990. Note that this procedure only applies to county-level measures and is not necessary in the SOEP data as we select individuals based on their county of residence in 1990 and track these people over time.

The number of East German counties was gradually reduced from 216 at the time of the reunification to 87 in 2009. To keep track of changing county boundaries from 1991 onwards, we rely on detailed population weighting matrices provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*BBSR*). Based on these weighting factors, we can calculate county-level measures (election turnout, self-employment rates, population figures, etc.) assuming counterfactual county borders.

There were only minor territorial reforms during the existence of the GDR (more specifically between 1953–1990, the period for which we have data). In ten cases, neighboring counties were merged together. In five cases, bigger cities became independent from the surrounding rural county (*Stadtkreise*). We manually account for these administrative changes using detailed maps and other historical sources. When merging two counties, we always use the maximum for each of the three opposition variables (state of emergency, Soviet military intervention, strike intensity). In case new counties were constituted, we assign historical values of the emitting county to the created one.

When harmonizing data from the Weimar Republic with 1990 county boundaries, greater territorial reforms have to be taken into account. Due to the lack of adequate population weighting factors, the harmonization is based on geospatial area weighting factors as described in Goodchild and Lam (1980). We overlay the corresponding GIS shapefiles from the Weimar Republic with the shapefile from 1990 and calculate area weighting factors that allow for adjusting the historical data to county borders as of 1990. MPIDR and CGG, 2011, provide a rich set of historical shapefiles for the German territory. Given that most of our outcomes and control variables refer to people and not space, it needs to be stressed that this procedure is afflicted with some degree of imprecision. Given the long time span, the numerous territorial reforms, and the lack of population weighting factors, this procedure is, however, the most accurate harmonization procedure we can apply.