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

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Abstract. Based on official records from the former East German Ministry for State Security, we quantify the long-term costs of state surveillance on social capital and economic performance. Using county-level variation in the number of spies in the 1980s, we exploit discontinuities at state borders to show that higher levels of government surveillance led to lower levels of interpersonal and institutional trust in post-reunification Germany. Based on a second identification strategy that accounts for county fixed effects we further estimate the economic costs of spying. We find that a more intense surveillance caused lower self-employment rates, fewer patents per capita, higher unemployment rates and larger population losses throughout the 1990s and 2000s. Overall, our results suggest that the social and economic costs of East German state surveillance are large and persistent.

Keywords: spying, surveillance, social capital, trust, East Germany

JEL codes: H11, N34, N44, P26

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1 Introduction

1. SOEP (trust dohmen)
2. Gerard: Talk about informers, not about spies

More than one third of the world population lives in authoritarian states (The Economist Intelligence Unit, 2014). A unifying feature of these regimes is the need to control all aspects of public and private life at all times. For this purpose, large-scale surveillance systems are installed that monitor societal interactions, identify and silence political opponents, and establish a system of obedience by instilling fear (Arendt, 1951).¹ A large and growing literature investigating the relationship between political institutions and culture suggests that such environments of distrust should exhibit adverse economic effects (Algan and Cahuc, 2013, Alesina and Giuliano, 2015). In this paper, we aim to test this hypothesis by linking political institutions (government surveillance), culture (trust) and economic performance. We use the specificities of the surveillance system implemented in East Germany, 1950–1990, to establish exogenous variation in the intensity of government surveillance at the county level, and relate this variation to measures of interpersonal and institutional trust as well as economic outcomes such as entrepreneurship, unemployment or population growth.

The official state security service of the socialist German Democratic Republic (GDR), the Ministry for State Security, commonly referred to as the *Stasi*, administered a huge network of spies, called “unofficial informers” (*Informelle Mitarbeiter*, IM). These spies were the most important instrument to fight political enemies and secure the regime’s power. In the 1980s, more than one percent of the East German population was acquired by the Stasi as a spy. These spies were ordinary citizens who had a regular job and were recruited to secretly collect information on any societal interaction in their daily life that could be of interest to the regime, betraying friends, neighbors, colleagues and sports buddies. Historians expect the “damage done to social relations” to be “disastrous” and long-lasting (Gieseke, 2014, p. 95).

Using administrative data on the regional number of spies, we first document substantial variation in the spy density, i.e., the intensity of state surveillance, across East German counties (*Kreise*). While local variation in the spy density is necessary, it is not sufficient to identify causal effects of state surveillance on social or economic outcomes. A very obvious concern is that the allocation of spies across East German counties was non-random. Unlike other studies that use the same regional variation in the surveillance intensity (Jacob and Tyrell, 2010, Friehe et al., 2015), we explicitly account for this non-randomness by adopting two different, complementary identification strategies.²

The first design exploits the specific territorial-administrative structure of the Ministry for State Security. County offices of the Stasi were subordinate to the respective state (*Bezirk*) office. Each state office in turn bore full responsibility to secure its territory, leading different state offices to administer different average levels of spy densities. Indeed, around 25% of the variation in spy density at the

¹ We acknowledge that democratic countries usually spy on their populations as well. Apparently, there is no clear line between democracies and authoritarian states in this respect. In this paper, we are interested in the effect of surveillance on economic performance and leave definitional discussions aside. This also concerns the lively debate in political science on how to precisely define and distinguish different forms of authoritarian regimes, such as totalitarian, despotic or tyrannic systems.

² The results of our analysis suggest that simple OLS regression estimates are biased towards zero.

county level can be explained with state fixed effects. We follow Dube et al. (2010) and use the resulting discontinuities along state borders as a source of exogenous variation. Hence, identification comes from different intensities of spying within county pairs at a state border, which is induced by different state-level surveillance strategies. The identifying assumption is that county pairs are similar in all other respects. Importantly, GDR states had no legislative competence with respect to economic and social policies, as all policies were centrally decided by the politburo in East Berlin. In addition, we show that other observable characteristics are smooth at state borders.

For our second identification strategy, we follow Moser et al. (2014) and construct a county-level panel data set that covers both pre- and post-treatment years. This research design enables us to include county fixed-effects to account for time-invariant confounders, such as a regional liberalism, which might have affected the allocation of Stasi spies and may (still) affect the economic prosperity of a county. Using pre-treatment data from the 1920s and early 1930s, we can also test directly for pre-trends in the outcome variables. Reassuringly, spy density cannot explain trends in voting behavior and 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 and regional economic performance.³ Using data from the German Socio-Economic Panel (SOEP), we find that more government surveillance 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). When focusing on institutional trust, we further find peoples' intention to vote and engagement in local politics to be significantly lower in counties with a high spy density even two decades after reunification. Using county level data, we corroborate this result by showing that election turnout has been significantly lower in higher-spying counties in federal elections.

In terms of economic performance, we find a significantly negative effect of state surveillance on individual-level labor income. Using administrative county-level data, we further show that self-employment rates are significantly lower in higher-spying counties. Moreover, post-reunification unemployment is persistently higher in counties with high surveillance levels. Our estimates imply that moving from 75th to the 25th percentile in the surveillance intensity would lead to a lower long-term unemployment rate of 0.84 percentage points, which is equivalent to a 4.5 percent drop given the average unemployment level in East Germany post reunification. Last, we find significantly negative effects of the spy density on population growth: Stasi spying is a significant driver of the tremendous population decline experienced in East Germany after reunification. We find that for both out-migration waves (1989–1992, and 1998–2009, see Fuchs-Schündeln and Schündeln, 2009), population losses were relatively stronger in higher-spying counties.

Given the negative population effect of surveillance, we assess the contribution of potential migration effects to the total effects on trust and economic performance. While our baseline estimates already take into account the overall population effect, we provide further evidence on selective

³ 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.

out-migration due to higher spying intensity. Overall, we find very small effects on the skill and age composition of counties, which strongly suggests that most of our baseline findings are due to a direct effect of government surveillance on economic performance.

By linking political institutions, trust and economic performance, our paper contributes to two different strands of the literature. First, we add to the steadily growing literature on culture, institutions and economic performance (see, e.g., Tabellini, 2010, and Alesina and Giuliano, 2015, for a recent survey). Econometrically, we follow and refine current identification strategies to estimate causal effects of culture on economic outcomes. First, we follow the recent trend in using within-country variation in combination with regional fixed effects for identification rather than relying on cross-country variation (Algan and Cahuc, 2013, Alesina and Giuliano, 2015). Moreover, we exploit discontinuities at borders for identification (Becker et al., 2015). In contrast to other studies, our identifying variation is not generated by deep, historical differences (such as religion, ethnicity, education, or political institutions), but induced by a rather recent, pervasive, yet short-lived political experiment. Moreover, our outcomes are measured after the experiment has ended with the sudden and unexpected fall of the Berlin Wall in November 1989. Specifically, we add to the literature investigating the effects of the transition from socialism on individual preferences (Aghion et al., 2010). In the German context, Alesina and Fuchs-Schündeln (2007) show that ideological indoctrination in the GDR had long-term effects on individual preferences for redistribution and government intervention.

Second, this is the first study to show that state surveillance has negative effects on economic performance. Our results suggest that lower levels of trust (and/or social capital) led to worse economic outcomes (Knack and Keefer, 1997, Zak and Knack, 2001, Guiso et al., 2006, Algan and Cahuc, 2010, 2013). In particular, our results suggest that an important channel is the reduction in entrepreneurial activity, where the most direct links between trust and economic performance can be expected (La Porta et al., 1997). In the light of many governmental surveillance systems being in place today, our results thus give important indications for long-lasting negative economic performance of authoritarian states around the world. We fully acknowledge that it is difficult to compare (the effects of) surveillance systems across countries and/or time. Nevertheless, we see no reason that our qualitative results should not hold in other contexts as well. In particular, the intuitive mechanism of surveillance deteriorating trust, and lacking trust impeding economic performance should be observable in other countries as well. More generally, our findings complement the large literature providing cross-country evidence for long-term positive effects of the quality of institutions and policies on economic performance (Mauro, 1995, Hall and Jones, 1999, Rodrik et al., 2004, Nunn, 2008, Nunn and Wantchekon, 2011, Acemoglu et al., 2015).

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 spy allocation 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 lead to a 40-year long division of a society that had been highly integrated prior to its separation. 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”⁴ 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 a one-party dictatorship under the rule of the Socialist Unity Party (SED) and its secretaries general.

Over the course of its existence, the regime secured its authority by means of a large and powerful state security service. The Ministry for State Security was founded in February 1950, just a few month after the GDR was constituted, and designed to “battle against agents, saboteurs, and diversionists [in order] to preserve the full effectiveness of [the] Constitution”⁵. It soon became a ubiquitous institution, spying on and suppressing the entire population to ensure and preserve the regime's power (Gieseke, 2014, p. 50ff.). The party leaders' demand for comprehensive surveillance was reflected by the organizational structure of the Stasi. While the main administration was located in East Berlin, the Stasi also 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 (*Objektdienststellen*).⁶ Following this territorial principle, which was at odds with the overall centralist organization of the GDR, state-level offices had 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, about 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 eleven 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).

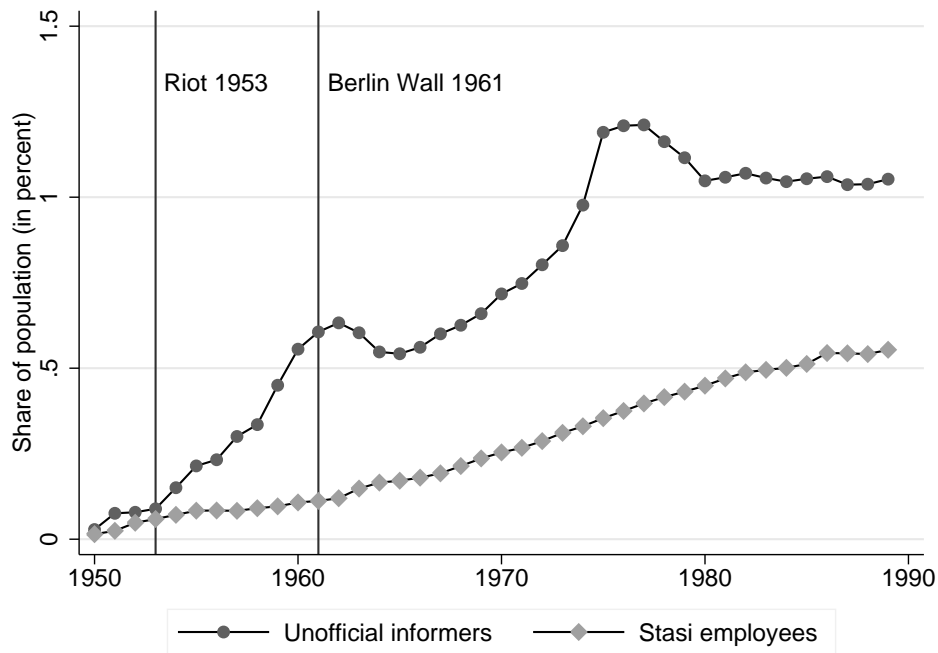
Over the four decades of its existence, the Stasi continuously expanded its competencies and duties

⁴ Erich Honecker, Secretary General of the SED between 1971–1989, introduced this term on a meeting of the Central Committee of the SED in May 1973 to distinguish the regimes of the Eastern bloc from Marxist theories on socialism.

⁵ 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).

⁶ The Stasi only monitored economic activity but was not actively involved in economic production (Gieseke, 2014).

Figure 1: Share of Stasi Employees and Informers in the GDR Population



Notes: Own calculations using data on the number of unofficial collaborators (operative and non-operative) from Müller-Enbergs (2008), information on the number of Stasi employees as reported in Gieseke (1996), and population figures from Statistical Yearbooks of the GDR.

as well as the surveillance of the population. The unforeseen national uprising on and around June 17, 1953 revealed the weakness of the secret security service in its early years and caused a subsequent transformation and expansion. The number of both official employees and unofficial collaborators continuously increased until the late 1970s and remained at a high level until the breakdown of the regime in 1989. Figure 1 plots the share of regular employees and unofficial collaborators in the population for the period of 1950 until 1989. In absolute terms, the Stasi listed 90,257 regular employees and 173,081 unofficial informants by the end of 1989, amounting to around 1.57 percent of the entire population.⁷

Figure 1 reveals that the Stasi's "main weapon against the enemy" was the dense network of unofficial collaborators.⁸ The Stasi "[...] owed its disciplinary and intimidating effect to its image among the population as an omnipresent force with unlimited powers of intervention. [...] Even the number of spontaneous denunciations under National Socialism, not to mention the downright infinitesimal number of Nazi informers, seems small when compared to the size of the IM network" (Gieseke, 2014, p. 84f.). An important and specific feature of the East German regime was the use of "silent" methods of repression rather than legal persecution through the police (Knabe, 1999). By gathering detailed inside knowledge about the population, the Stasi was able to obtain an overall

⁷ The number of regular employees of the Stasi 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).

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

picture of anti-socialist and dissident movements (Gieseke, 2014, p. 163ff.).⁹ Compared to alternative silent surveillance methods like telephone or mail monitoring, direct contact between spies and suspect individuals was the central feature of the Stasi system (Bruce, 2010, p. 120).

Spies were recruited from the population 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, neighbors or sport buddies, after recruitment. From time to time, spies would secretly meet with an official Stasi officer, report suspicious behavior and provide personal information about individuals in their social networks. The threat of being denounced 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).¹⁰ Naturally, the constant surveillance also had 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.

There were different reasons for serving as a collaborator. Some citizens agreed to cooperate due to ideological reasons, others were attracted by personal and material benefits accompanied with their cooperation. However, the regime also urged citizens to act as unofficial collaborators by creating fear and pressure. In an 1967 survey of unofficial informants, 23 percent of the spies indicated that pressure and coercion lead to recruitment (Müller-Enbergs, 2013, p. 120). Note that this survey was conducted by the Stasi itself, so underreporting to this question seems likely. The body of spies was administrated in a highly formalized way, with cooperation being sealed in written agreements and spies being tightly lead by a responsible official Stasi agent (Gieseke, 2014, p. 114ff.).

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 spy 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.3. 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 spies in each county is based on official Stasi records, published by the Agency of the Federal Commissioner for the Stasi Records (*BStU*) and compiled in Müller-Enbergs

⁹ This strategy was communicated both internally and publicly, as can be seen from teaching material used at the internal academy of the Ministry of State Security: "For political reasons it is often neither effective nor useful to respond to various forms of criminal actions with legal persecution, even though the criminal actions would justify enforcement. Enemies of the state, who try to ideologically influence and divert citizens, are far less dangerous if they are free but forced to inactivity rather than imprisoned martyrs. In such conditions, the state has to take appropriate non-persecutive measures in due time to prevent hostile activities" (see Knabe, 1999, p. 195, own translation). A similar but less technical explanation of the regime's strategy was given by Erich Mielke, the head of the Stasi, in a public speech in 1985.

¹⁰ For less scientific 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.

(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 are publicly 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 weapon of surveillance, we choose the county-level share of unofficial collaborators in the population as our main measure of the intensity of surveillance. Most regular Stasi officers were based in the headquarter in Berlin, and only 10-12 percent of them were employed at the county level. In contrast, the majority of all unofficial collaborators were attached to county offices. The Stasi differentiated between three categories of unofficial collaborators: (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 county, we add the number of unofficial collaborators attached to these object offices to the respective county's number of spies.¹¹ As information on the total number of spies are not given for each year in every county, we use the average share of spies from 1980 to 1988 as our measure of surveillance.¹² The spy 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 2 plots the spy density for each county. Today, the number of spies is known for about ninety 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 B.1 for more detailed distributional information).¹³ The median is similar to the mean (0.36 percent), and one standard deviation is equal to 0.14 spies per capita, which is more than one third of the mean spy density. In our regressions, we standardize the spy density by dividing it by one standard deviation.

Apparently, 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 the transmission occurred both consciously and unconsciously. Bruce (2010, p. 146) suggests 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 spy density, our identifying assumption is that individuals living in counties with higher spy intensity

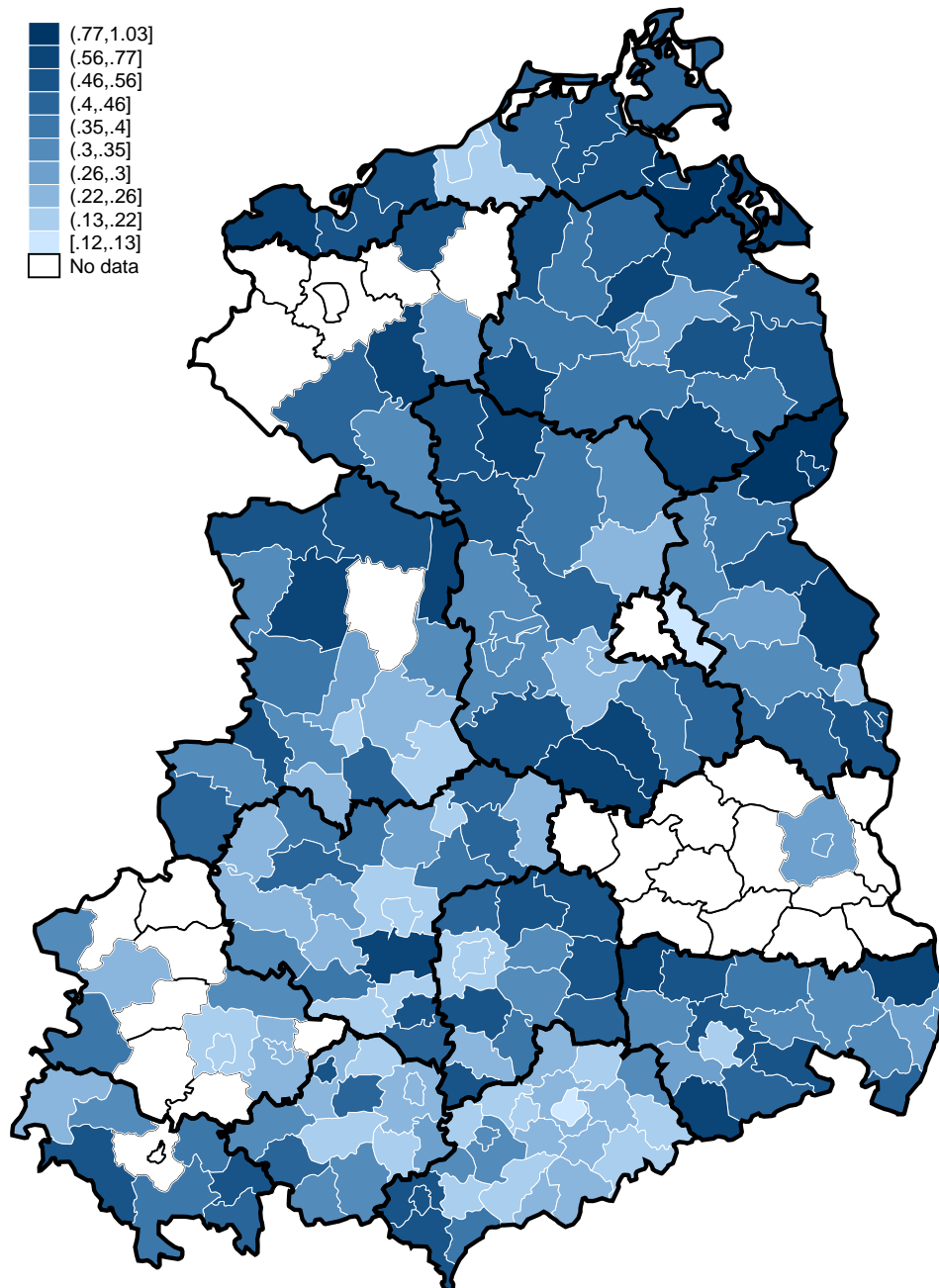
¹¹ In the empirical analysis, we explicitly control for the presence of such offices in Objects of Special Interest.

¹² Data from earlier years are only available for a limited number of counties.

¹³ Note that these figures only relate to operative collaborators at the county level (IM category I), which explains the lower mean in the spy density compared to the overall share of informers in the population (cf. Figure 1).

were consciously or subconsciously more aware of government surveillance because they had more frequent/intense contact with the surveillance system.

Figure 2: Share of Operative Unofficial Collaborators at the County Level



Notes: This graph plots the county-level surveillance density measured by the average yearly share of operative unofficial collaborators (category 1 spies) 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.

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 so that they stopped their activities (Bruce, 2010, p. 130).

Among these silent measures, we choose the number of operative spies (category 1) per capita as our main regressor given their active role in spying. Moreover, data coverage is highest for this type of spies and we would lose 30 counties when basing our measure of surveillance on all three types of spies. However, as indicated by Panel (A) of Figure A.2 in the Appendix, this choice does not appear to be crucial given the very high correlation between operative spies and the total number of spies ($\rho = 0.95$).

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 spies is highly correlated, which seems reasonable given that spies had to be administered by official employees in the county offices. Given the importance of unofficial informants as “the main weapon” of the Stasi, we choose the density of operative spies 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 spies does not affect 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). 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. 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 as well as current gross labor income (see Section 5.1 and Data Appendix B).

In order to proxy interpersonal trust, we use two standard measures provided in the SOEP: (i) trust in strangers (see, e.g., Glaeser et al., 2000), and (ii) negative reciprocity (see, e.g., Dohmen et al., 2009). To capture trust in the political system, we investigate two measures as well. First, we take the survey question about the intention to vote if federal elections were held next Sunday. The question captures stated preferences to participate in the most important German election.¹⁴ Second, we exploit the question whether individuals are engaged in local politics. We also use monthly gross labor income reported in the SOEP as an individual 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. For the underlying survey questions, data years and exact variable definitions, see Data Appendix B.

¹⁴ We also use a measure of revealed preferences, i.e., electoral turnout at the county level, below.

3.3 County-level data

For the second dataset, we compiled county-level data on various measures of economic performance (self-employment, patents, unemployment, population) as well as electoral turnout as a proxy for institutional trust. We collected county-level data for two time-periods, data from the 1990s and 2000s as well as pre World War II data.¹⁵ Post-reunification data come from official administrative records; historical data come from various sources (see Data Appendix B for details).

In addition to our outcome variables, we further collect various county-level variables. We use these to (i) explain the Stasi's allocation of spies (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 size 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 (share of population aged above 64) in each county.

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 1989. The riot markedly changed the regime's awareness for internal conflicts and triggered the expansion of the Stasi spy network (cf. Figure 1). We use differences in the regional intensity of the riot to proxy the strength of the opposition. Specifically, we construct three control variables: (i) a categorical variable measuring 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 (for details on the source and the construction of the variables, see Appendix Table B.3).

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)¹⁶, (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 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 share of Nazi and Communist votes in the federal elections of

¹⁵ 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.

¹⁶ We drop the county Plauen-Land due to missing data for this variable.

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.

4 The allocation of spies

In this section, we try to explain the mechanism that allocated spies to counties. Astonishingly, there is very little knowledge regarding the determinants of regional spy 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 allocation mechanism of spies, we run simple OLS regressions of the spy density on five sets of potentially important variables: (i) county size and demographic structure, (ii) county-level oppositional strength, (iii) county industry composition, (iv) county-level pre World War II characteristics, and (v) GDR state-level characteristics (control sets (i)-(iv) 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 spy intensity as indicated by (partial) R^2 measures.

Table 1 reports the regression results. We start off by explaining the spy 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.¹⁷ In the next specification, we add variables controlling for the *size* and demographic structure of a county. While the spy 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. The negative coefficient shows that the spy *density* is decreasing 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 0.38.¹⁸

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 spy 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

¹⁷ As described in Section 3.1, the Stasi maintained offices in these objects, which recruited their own spies. As we add the spies working in these object offices to the number of spies in the respective county office, we control for offices in Objects of Special Interest with a dummy variable in all regressions below.

¹⁸ 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: The Allocation of Stasi Spies

	(1)	(2)	(3)	(4)	(5)	(6)
Dummy: Object of Special Interest	1.132 (0.875)	1.710*** (0.522)	1.710*** (0.535)	1.718*** (0.578)	1.780*** (0.559)	1.981*** (0.535)
Log mean population 1980s		-0.868*** (0.107)	-0.916*** (0.115)	-1.030*** (0.197)	-1.122*** (0.237)	-1.328*** (0.252)
Log county size		0.125* (0.072)	0.136* (0.076)	0.234** (0.109)	0.323*** (0.115)	0.206* (0.121)
Share of population aged above 64		-0.108** (0.052)	-0.099* (0.055)	-0.057 (0.068)	-0.102 (0.072)	-0.154* (0.088)
Share of population aged below 15		-0.025 (0.070)	-0.028 (0.073)	0.007 (0.088)	-0.057 (0.094)	-0.237** (0.105)
Uprising intensity 1953: Strike			0.062 (0.172)	0.031 (0.187)	0.035 (0.186)	-0.072 (0.187)
Uprising intensity 1953: Demonstration			-0.144 (0.179)	-0.179 (0.191)	-0.240 (0.190)	-0.197 (0.204)
Uprising intensity 1953: Riot			-0.259 (0.243)	-0.249 (0.246)	-0.322 (0.254)	-0.379 (0.265)
Uprising intensity 1953: Prisoner liberation			-0.157 (0.241)	-0.220 (0.243)	-0.145 (0.246)	-0.161 (0.272)
Dummy: Military intervention 1953			0.164 (0.156)	0.155 (0.154)	0.230 (0.168)	0.308* (0.169)
Dummy: State of emergency 1953			0.218 (0.146)	0.218 (0.156)	0.238 (0.174)	-0.014 (0.200)
Share agricultural employment 1989				-0.018 (0.016)	-0.015 (0.016)	-0.013 (0.014)
Share industrial employment 1989				-0.011 (0.012)	-0.012 (0.013)	-0.010 (0.012)
Dummy: Important industries 1989				-0.096 (0.160)	-0.097 (0.164)	-0.100 (0.156)
Industry concentration 1989				0.007 (0.006)	0.007 (0.007)	0.003 (0.007)
Log goods value of industrial production 1989				0.022 (0.100)	0.048 (0.102)	0.092 (0.103)
Mean electoral turnout 1928–1932					-0.035 (0.031)	-0.001 (0.042)
Mean vote share Nazi party 1928–1932					0.008 (0.020)	0.040* (0.021)
Mean vote share communist party 1928–1932					-0.040** (0.016)	-0.008 (0.022)
Share protestants 1925					0.004 (0.008)	-0.016* (0.009)
Share unemployed 1933					0.038 (0.024)	0.014 (0.025)
Share self-employed 1933					-0.044 (0.042)	0.031 (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 the simple OLS coefficients of regressing the mean county-level spy 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 source and construction of the variables, see Appendix Table B.3.

indeed shows that the industrial structure is an important determinant of the spy density, however much of the effect seems to be already captured by controlling for the (population) size of 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 spy 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 spy density due to state-level characteristics. Notably, GDR state fixed effects are an important determinant of the spy 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 spy 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 results, we account for different sets of control variables in both research designs laid out below.¹⁹

Overall, we are able to explain around 60 percent of the variation in spy density at the county level. Importantly, different average spy 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 sixty percent of the regional variation in the spy density across counties by means of observable differences in county characteristics. In order to establish causality between the spy 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 and could not be interpreted as causal. In the following subsections, we thus present two research designs which are intended to better account for unobserved confounders and limit potential endogeneity bias.

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 refugees trying to flee. The regime also often punished those individuals who applied for emigration visas, exposing

¹⁹ As noted above, we account for long-term, pre World War II differences in county characteristics in the panel data design by including pre-treatment outcomes.

people to considerable harassment in working and private life (Kowalczyk, 2009). As a consequence, external migration 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 economic, political or social interests of the regime (Grashoff, 2011, p. 13f.). Relying on data on county population and the number of spies in multiple years in the 1980s, we can directly test whether the spy density affected population size. Reassuringly, we estimate a zero effect of the log number of spies 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 territorial-administrative structure of the Stasi and the fact that about 25 percent of the county-level variation in the spy density can be explained with GDR state fixed effects (cf. Table 1, column (6)). As the Stasi county offices were subordinate to the respective state office, different GDR states administered different average levels of spy density. 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). Technically, we closely follow Dube et al. (2010) and limit our analysis to all contiguous counties that straddle a GDR state border, thus identify the effect of surveillance on our outcome variables by comparing county pairs on either side of a state border.²⁰

The identifying assumption is that the county on the lower-spy side of the border is similar to the county on the higher-spy 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 states had no legislative competence: “The main task of the state administrations was to execute the decision made by the central committee. This was their *raison d’être*.”²¹ For identification, we exploit that the Ministry for State Security was an exception in its organizational structure, in which each state office bore the responsibility to secure its territory (see, e.g., Bruce, 2010, p. 111, and Gieseke, 2014, p. 82). Finally, our identifying assumption could be compromised if (i) spies 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 spies. Note that both of the latter concerns would work against

²⁰ If a county has several direct neighbors on the other side of the state border, we duplicate the observation and adjust sample weights; see below for more details.

²¹ Ulrich Schlaak, Second Secretary of the SED in the state of Potsdam, cited in Niemann (2007, p. 198, own translation).

us and bias our estimates toward zero.

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

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

As outcome variable, Y_{icb} , we use trust in strangers, negative reciprocity, intention to vote in elections, engagement in local politics and log individual gross 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 X_i' and K_c' . Vector X_i' includes individual-level compositional controls, whereas 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, which is in line with the findings of the covariate smoothness test reported in Table 2 below. Rather, as will be shown below, the inclusion of county controls increases the absolute value of the coefficients, which suggests that omitted variables is likely to bias our estimates towards zero.²² For this reason, the richest specification including all covariates is our preferred one.

We use the cross-sectional weights provided by the SOEP to make the sample representative for the whole population. Given that we duplicate observations in counties that neighbor multiple counties in a different state, we adjust cross-sectional weights by dividing them by the number of duplications in our baseline specification. In addition, standard errors are two-way clustered at the county pair and the individual 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.1) prove to be robust to these modifications.

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 spy 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 affect the allocation 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 spy density and our county pair fixed effects. Columns (1) and (2) of Table 2 report the corresponding results for these partial regressions. In line with the findings presented in Table 1, we report that the spy density decreased with population size. Apart from that, only few differences appear. We find that a higher spy density within a border county pair is correlated with a higher share of agricultural employment and negatively related to the value of industrial production. Moreover, the declaration of a state of emergency during the 1953 uprising was more likely in higher-spying counties.

Given the notable differences in county population, we also run the covariate smoothness test

²² Note that this is in line with the example of regional liberalism as an omitted confounder, which should also bias estimates towards zero.

conditional on log mean population in the 1980s. From columns (3) and (4) of Table 2 it can be inferred that most coefficients remain small and insignificant. Moreover, conditional on population, differences in the share of agricultural employment and the value of industrial production disappear. Conditional on population, the results, however, suggest that the industrial sector was more concentrated in higher-spying counties. Overall, despite some differences within county pairs, we conclude that covariates are smoothly distributed around state borders. Nonetheless, we control for observable differences in county characteristics in our preferred specification.

Table 2: Covariate Smoothness at GDR State Borders

	Unconditional		Cond. on population	
	(1) Estimate	S.E.	(2) Estimate	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 border county pair correlation between the spy density and several covariates. Estimates show the results from partial regressions of county-level variables on the spy 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 (2) 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.3.

5.2 Panel data design

As discussed before, time-persistent confounders that have affected the allocation of spies 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 outcomes such as measures of economic performance or political participation 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.²³ The panel data model reads as follows:

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

Outcomes Y_{ct} are county c 's election turnout, self-employment rate, number of patents per capita, unemployment rate and log population in year t (see Section 3.3).²⁴ Hence, all outcomes except for population account for population changes (see the end of Section 6.2 for a detailed discussion).

We allow the effect of spying to evolve over time by interacting the time-invariant spy density $SPYDENS_c$ with year dummies τ_t . Coefficients $\beta_t, \forall t \geq 1989$ show the treatment effect after reunification and demonstrate the potential persistence of the effect. Moreover, coefficients $\beta_t, \forall t < 1989$ (in our case mostly $t \leq 1933$) provide a direct test of the identifying assumption. If the surveillance levels in the 1980s had an effect on social capital or 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.²⁵ 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 spy 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 L_{ct} includes several sets of control variables that vary by specification. Any persistent time-invariant confounder is wiped out from the model by the county fixed effect. Given that control variables are time invariant as they are either measured pre- or during the treatment period, we have to interact them with either a simple post-treatment dummy variable or year dummies. The first set of controls includes county size and demographic controls. We know from the allocation regression presented in Table 1, that county size explains around 25 percent of the variation in spy density. At the same time, it is 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.²⁶ 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.

²³ Note that many (though not all) potential confounders are likely to be time-invariant by definition, since they must have affected the spy allocation in the 1980s and outcomes in the 1990s and 2000s.

²⁴ 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.

²⁵ We omit the spy 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 regarding regional unemployment in this model specification.

²⁶ For the pre-war period, we use Prussian provinces from the time of the Weimar Republic instead of GDR states.

Sensitivity tests. We provide several tests to assess the sensitivity of our estimates. First, we add current population to the model. Despite being a potential outcome and hence being considered as a *bad control*, we test the robustness of our results when controlling for current population size, which captures different regional adjustment paths and also accounts for selection out of counties. Second, we control for federal and state transfers as well as investment subsidies paid to East German counties after reunification, which are potential confounding variables.²⁷ Reassuringly, estimates are insensitive to the inclusion of these additional control variables (see Tables A.3-A.7 in Appendix A).

6 Results

In the following section, we present the empirical results. First, we focus on the results obtained using the border discontinuity design (Section 6.1). In Section 6.2, we apply the panel data design.

6.1 Border discontinuity results

We apply the border discontinuity design as set up in equation (1) to identify the effect of spying on measures of trust and economic performance. We start by looking at the results for interpersonal and institutional trust, which are summarized in Table 3.

In terms of interpersonal trust, we find that the intensity of spying significantly affects both outcomes, trust in strangers and negative reciprocity (see Panels A and B). Results are significant in our leanest specification (column (1)) and also conditional on individual- and county-level controls (column (3)). This specification will be our preferred throughout the paper. For a one standard deviation increase in the spy density, the estimate in column (3) implies that the probability to trust would be around six percentage points lower, which is a large effect given that the average probability is fourteen percent. When focusing on reciprocal behavior, we find an effect of similar magnitude.²⁸

Next, we turn to institutional trust, Panels C and D of Table 3 providing the results. We find a significant negative effect of the spy density on the intention to vote in elections throughout all specifications. On average, a one standard deviation increase in the intensity of spying leads to a decrease in the *intention* to attend elections of ten percentage points. Likewise, we also find a negative significant effect on engagement in local politics. In our preferred specification, a one standard deviation increase in the spy density leads to a decrease in local political engagement of four percentage points. Interestingly, the effect only becomes significant if we condition on county control variables – the crucial variables being electoral turnout and vote shares in the 1920s and 1930s.

We observe each trust measure in two SOEP waves (see Data Appendix B). For the results shown in Table 3, we pool the observations from the two waves and add wave dummies to our model.²⁹ In contrast, income variables are reported in each wave of the SOEP, so we can additionally check the evolution over time. Figure 3 shows the results of our preferred specification with a full set

²⁷ We have no information on transfers for the years 1990–1994.

²⁸ Note that we estimate the models using OLS to ease interpretation. Results are robust to using Probits, see Appendix Table A.1, columns (6) and (7).

²⁹ Results are robust when estimating the model separately for each year or interacting the trust measures with wave dummies.

Table 3: The Effect of Spying on Interpersonal and Institutional Trust

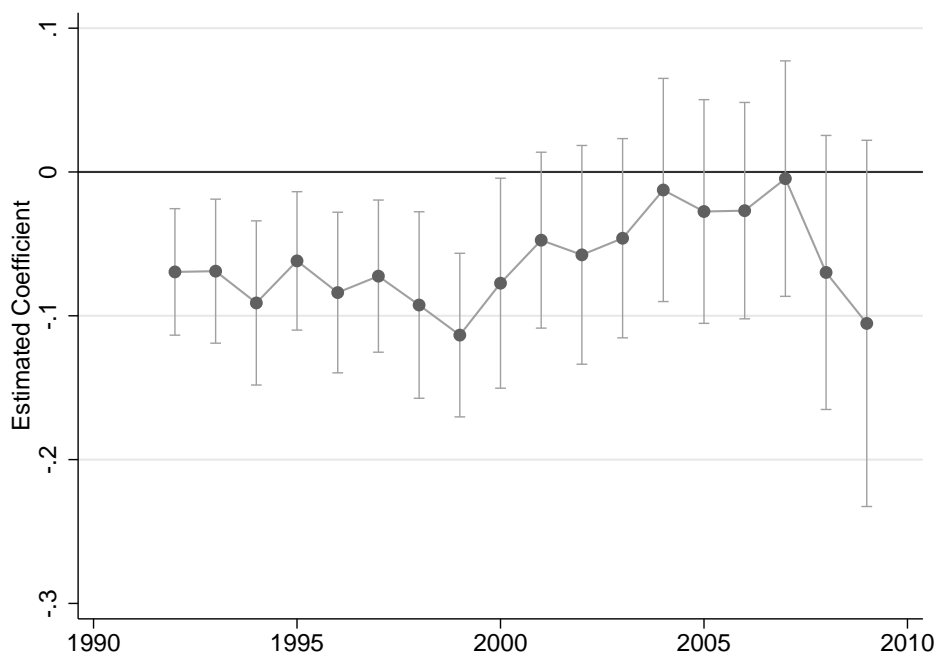
	Border County Pair Sample			No East Berlin	
	(1)	(2)	(3)	(4)	(5)
Panel A – Trust in strangers					
Spy density	-0.041** (0.020)	-0.042* (0.022)	-0.061** (0.026)	-0.066** (0.026)	-0.056** (0.028)
Adjusted- R^2	0.061	0.095	0.115	0.125	0.126
Number of observations	3,389	3,389	3,389	2,212	2,212
Panel B – Negative reciprocity					
Spy density	-0.034*** (0.012)	-0.034*** (0.012)	-0.041*** (0.015)	-0.035** (0.015)	-0.040*** (0.015)
Adjusted- R^2	0.066	0.129	0.145	0.162	0.162
Number of observations	3,011	3,011	3,011	1,969	1,969
Panel C – Intention to vote					
Spy density	-0.057* (0.031)	-0.045* (0.026)	-0.098** (0.041)	-0.102** (0.041)	-0.100** (0.041)
Adjusted- R^2	0.053	0.136	0.155	0.154	0.153
Number of observations	3,107	3,107	3,107	2,052	2,052
Panel D – Engagement in local politics					
Spy density	-0.004 (0.019)	-0.002 (0.016)	-0.041** (0.019)	-0.041** (0.020)	-0.044** (0.020)
Adjusted- R^2	0.020	0.126	0.137	0.132	0.132
Number of observations	3,549	3,549	3,549	2,259	2,259
Individual controls		Yes	Yes	Yes	Yes
County controls			Yes	Yes	Yes
Population growth					Yes

Notes: This table shows the β coefficients of the border discontinuity model laid out in equation (1) using SOEP data. For better comparability, negative reciprocity is defined such that higher values indicate less negative reciprocal behavior. 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 the individual 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.

of individual and county control variables for labor income.³⁰ The figure shows a negative and persistent effect of a higher spy density on income throughout the 1990s. A one standard deviation increase in the spy density leads to an 8% decrease in labor earnings. In the 2000s, the effect slowly fades away and becomes insignificant.

³⁰ Results for specifications excluding individual and/or county controls are shown in Table A.2 in the Appendix.

Figure 3: Effect of Spying on Labor Income



Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (1). The specification includes individual and county level controls. For full regression results, see specification (1) of Appendix Table A.2.

Identification tests. In Section 5.1, we have argued that the spy allocation was non-random, which led us to implement the border discontinuity design, in which 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 three identification tests to show the importance of implementing our research design for identification.

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 show the results for a such a model. The estimate Column (1) of Panel A shows, for instance, a positive correlation between the spy 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) shows our preferred specification from Table 3 including the fixed effects. The same pattern can be observed for the three other trust measures: coefficients become more negative and more significant when moving from specification (1) to our preferred model reported in Column (3). In the OLS specification, we only find significant negative effects of the spy density when looking at negative reciprocity, which is in line with the findings of Friehe et al. (2015). We find similar results for labor income, which are provided in Appendix Table A.2.

In a second test, we try to rule out that our results are driven by long-lasting and persistent cultural differences across regions within the GDR. For example, Becker et al. (2015) show that the historical Habsburg Empire – a composite, multi-ethnic state comprising a variety of today’s Western and

Table 4: Sensitivity Checks: OLS and Border County Pair Results

	Base Sample	County Pair Sample		
	(1)	(2)	(3)	(4)
Panel A – Trust in strangers				
Spy density	0.019 (0.015)	-0.005 (0.021)	-0.061** (0.026)	
Spy density × Different Weimar Province				-0.071** (0.036)
Spy density × Same Weimar Province				-0.052** (0.025)
Number of observations	3,313	3,389	3,389	3,389
Panel B – Negative reciprocity				
Spy density	-0.022** (0.010)	-0.035*** (0.011)	-0.041*** (0.015)	
Spy density × Different Weimar Province				-0.031 (0.022)
Spy density × Same Weimar Province				-0.049*** (0.018)
Number of observations	2,947	3,011	3,011	3,011
Panel C – Intention to vote				
Spy density	-0.012 (0.023)	-0.027 (0.030)	-0.098** (0.041)	
Spy density × Different Weimar Province				-0.103** (0.051)
Spy density × Same Weimar Province				-0.093** (0.045)
Number of observations	3,036	3,107	3,107	3,107
Panel D – Engagement in local politics				
Spy density	0.001 (0.010)	-0.013 (0.013)	-0.041** (0.019)	
Spy density × Different Weimar Province				-0.017 (0.019)
Spy density × Same Weimar Province				-0.060*** (0.021)
Number of observations	3,551	3,549	3,549	3,549
County Pair FE			Yes	Yes
Full controls	Yes	Yes	Yes	Yes

Notes: This table shows the β coefficients using different specifications for the base and border county pair sample. For better comparability, negative reciprocity is defined such that higher values indicate less negative reciprocal behavior. 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 the individual level in the base sample, and two-way clustered at the county pair and individual 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. For detailed information on the control variables, see Data Appendix B.

Eastern European countries from the 16th to 18th century that is acknowledged for its well-functioning bureaucracy – still exerts significant effects on individuals' trust in governmental institutions today. In other words, there might be other discontinuities at state borders that simultaneously drive our results.

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.³¹ 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 exploit this territorial reform to provide a second robustness check of our identifying assumption: some counties became border county pairs during the GDR and were somewhat arbitrarily separated by state borders although they belonged to the same Free State prior to World War II. If effects of the spy density on trust and economic performance can also be detected within border county pairs that historically belonged to the same Prussian province (Pomerania, Brandenburg, Saxony, Silesia, Hesse-Nassau) or Free State (Saxony, Anhalt, Mecklenburg, Thuringa), it seems unlikely that deep cultural differences at historical state borders drive the results of our analysis. Empirically, we implement this test by interacting the spy density variable with a dummy variable indicating whether both counties within a GDR state border county pair belonged to the same Prussian province or Free State. Column (4) of Table 4 shows the results. Reassuringly, we find very similar effects for border county pairs that belonged to the same Weimar province. If anything, results seem to be more significant when looking at county pairs with the same cultural heritage. We find similar results for labor income, which is shown in Figure A.4 in the Appendix.

As a last exercise, we take a closer look at the relevance of the two main components of our identification strategy: looking at neighboring counties and exploiting state-level variation. In order to test whether these elements establish identification of our model, we construct three sets of placebo border county pairs, $s = 1, 2, 3$. We randomly sample (i) county pairs that are not neighbors ($s = 1$), (ii) neighboring counties within the same state ($s = 2$), and (iii) counties that are not neighboring but are in neighboring states ($s = 3$). For each placebo set s , we run a permutation test in the spirit of Chetty et al. (2009). We randomly generate as many placebo county pairs as given in the respective regression and re-estimate equation (1). We repeat this 399 times and obtain an empirical cumulative distribution function of 400 placebo estimates for each set and outcome. From these CDFs, we obtain the non-parametric p -values for our baseline estimates, reported in column (3) of Table 3.

Results are presented in Appendix Figures A.5 and A.6. Let us first consider the outcome “trust in strangers”. We start off by looking at placebo set $s = 1$ and take purely random counties and assume that they were neighbors at a state border (Panel (A)). The probability of finding an estimate equal or smaller than our baseline coefficient of -0.061 is 1.3%. Similar to the results of the naive OLS estimator on the whole sample (see Table 4), we find that the mean placebo estimate is small and positive. This is reassuring as we basically randomly sampled counties from the full sample and included county pair fixed effects which should have no explanatory power. Next, we try to

³¹ East Berlin officially became the 15th state in 1961.

identify the importance of exploiting state-level variation. Hence, we shut down this channel and randomly sample neighboring counties within states ($s = 2$, panel (B)). We find that the probability of finding an estimate equal or smaller than our baseline results remains low at 2.8%, which highlights the importance of exploiting cross-state variation in the spy density. Last, we assess the relevance of looking at neighbors by sampling non-neighboring county pairs from neighboring states. The corresponding p -value is 3.7%, which indicates that looking at neighbors is about equally important for identification when looking at trust in strangers as an outcome variable. Panels (A)-(C) of Figure A.6 show that test results are similar for the intention to vote.

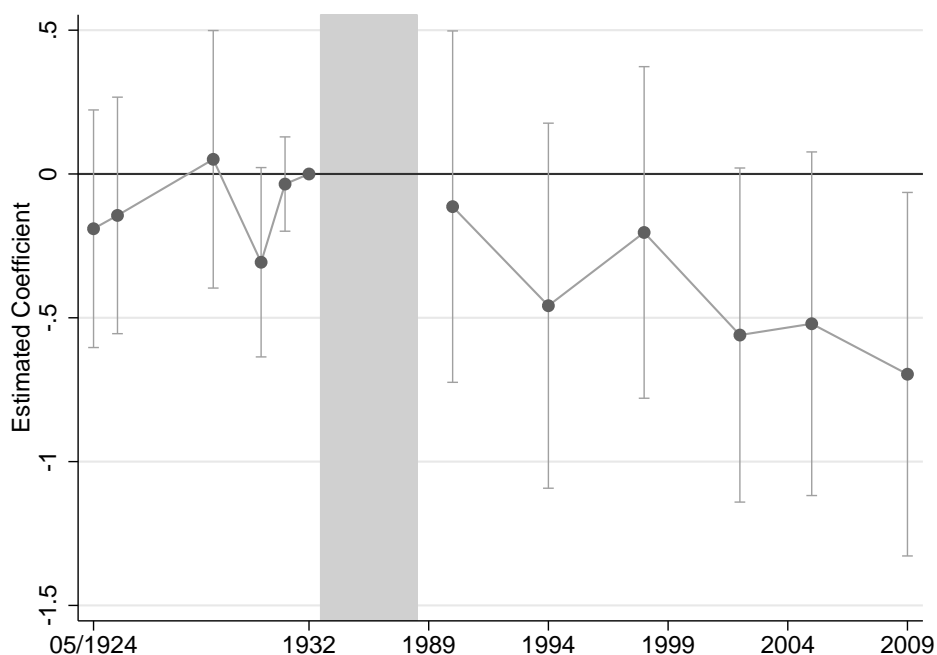
In contrast, and in line with the finding of specification (1) of Table 4, the permutation tests suggests that a significantly negative effect of spying on reciprocity can be more easily obtained by simply exploiting cross-sectional variation, the p -values for the three placebo tests being around 0.15 (panels (D)-(F) of Figure A.5). The relative contributions of isolating neighbors and looking at state-level variation to identification seem again equal. This is different when looking at the engagement in local politics (panels (E) and (F) of Figure A.6). Here, our permutation tests show that identification is likely to be achieved by comparing neighboring counties, which are supposed to be more similar to each other. Only exploiting cross-state variation in spy levels but not focusing on neighbors leads to empirical p -values that are above conventional levels ($p = 0.133$). This, in turn, stresses the importance of the assumption that within county pairs all other factors are smooth at the state border. Reassuringly, as shown above in column (4) of Table 4, the effect is stronger (and only significant) at state borders that were newly drawn after World War II and for which county pairs are arguably more similar than others.

6.2 Panel data results

In this section, we apply the panel data model as laid out in equation (2) to investigate the effects of state surveillance. Before turning to economic outcomes, we can try to corroborate the negative effect of the spy density on the intention to vote with a different research design. While the intention to vote is a soft measure capturing stated preferences, we can use administrative data on electoral turnout to check whether intentions actually translate into real political participation using county-level data on voter turnout. Figure 4 plots the corresponding β coefficients, adding the full set of control variables (i.e., county size, opposition and industry controls as well as state times year fixed effects). Table A.3 in the Appendix presents the corresponding regression results and shows that we find similar effects for leaner specifications as soon as we control for different trends by county size after reunification. Our results clearly indicate that the electoral turnout starts to decline in the 1990s for counties with a higher spy density and is significantly negative in the 2000s. For a one standard deviation increase in the spy density, average electoral turnout would be about 0.6 percentage points lower. While the effect is negative and significant, it is by about an order of a magnitude smaller than the effect on the *intention* to vote shown in Table 3. A potential explanation for the deviation could lie in the self-predicting inaccuracy of individuals, which is substantial when it comes to voting behavior (Rogers and Aida, 2013).

The figure 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

Figure 4: Effect of Surveillance on Electoral Turnout



Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (2). 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.3 for details.

would have responded to pre-treatment trends in electoral turnout and would thus have been endogenous in this respect. While we indeed find a lower turnout in the 1930 election, significant at the ten percent level, the remaining pre-treatment effects both before and after 1930 are insignificant and small. This suggests that the spy allocation was not systematically determined by pre World War II trends in institutional trust, which is crucial for establishing causality in our panel model.

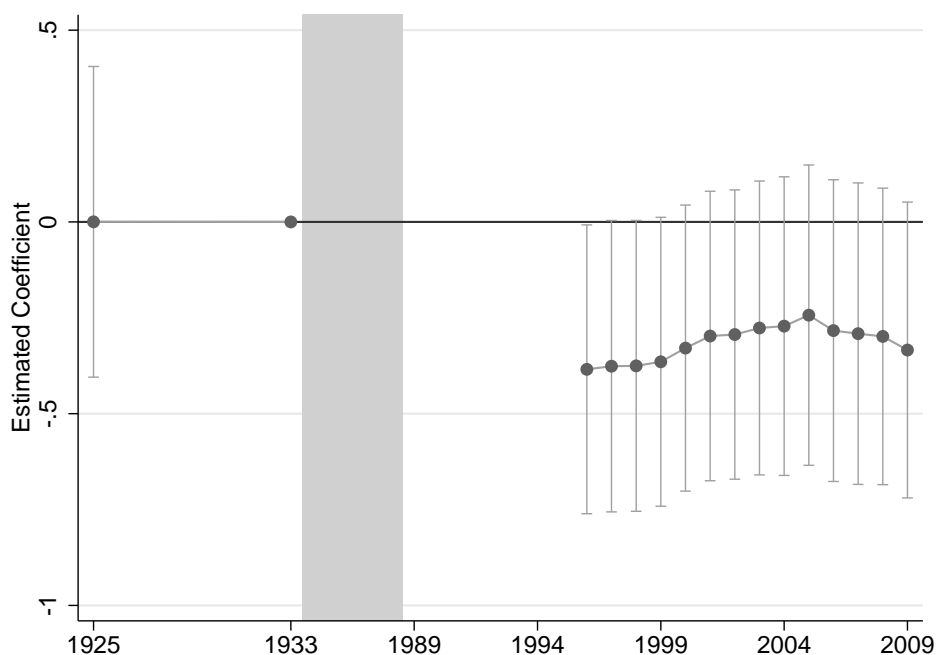
Next, we turn to economic outcomes. We begin by analyzing the effect of spying on entrepreneurial activity, given that lacking trust results in extensive monitoring of “possible malfeasance by partners, employees, and suppliers [and] less time to devote to innovation in new products or processes” (Knack and Keefer, 1997). Indeed, many studies have shown that more trustful people are more likely to become entrepreneurs (Welter, 2012, Caliendo et al., 2014). Hence, we consider two outcomes related to entrepreneurial activity, county-level self-employment rates and the number of patents per 100,000 inhabitants.

Figures 5 and 6 plot the respective regression estimates; full regression results are shown in Appendix Tables A.4 and A.5. We find that the self-employment rate is significantly lower the higher the county’s spy density.³² This negative effect is quite persistent.³³ This estimate implies that for a one standard deviation increase in the spy density, the self-employment rate would be around 0.4 percentage points lower. Reassuringly, we detect no significant pre-trend, which implies that our

³² As explained in Section 3.3, there is no information on self-employment and unemployment rates at the county level in the early 1990s.

³³ However, as shown in Appendix Table A.4, we lose precision when including county size controls.

Figure 5: Effect of Surveillance on Self-Employment Rates



Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (2). 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.4 for details.

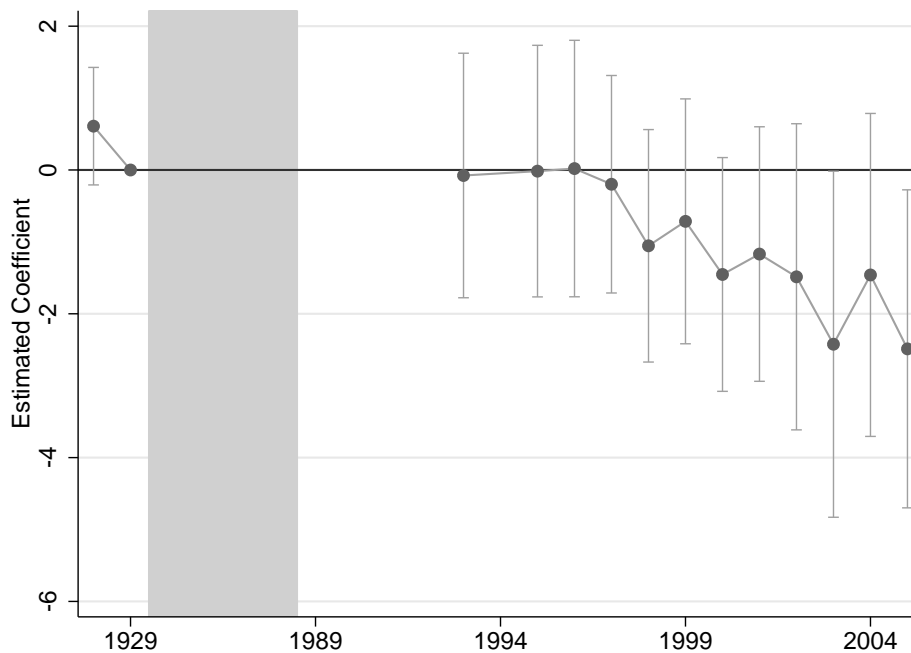
estimates are not driven by different pre-treatment trends in entrepreneurial spirit.

When looking at patents in Figure 6, we see no effect of spying on innovativeness in the first years after reunification. However, starting in 1997, the number of patents per capita in counties with a high spy density starts to drop. In 2005, the last year of our data, the point estimate implies that a one standard deviation decrease in the intensity of spying would, on average, lead to two patents more per 100,000 inhabitants, which is an increase of about 16 percent.

With entrepreneurial spirit lagging behind in counties with a high spy density, we can expect more comprehensive measures of economic performance to be lower as well. Ideally, we would look at the effect of spy 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. First, we look at the counties' unemployment rates and then at population size, which has been used as a proxy for regional growth (Redding and Sturm, 2008).

Figure 7 and Panel B of Figure 8 show the results. Figure 7 shows that unemployment is indeed higher in counties with a higher spy density. The effect is persistent and oscillates around an increase of 0.6 percentage points in county unemployment for a one standard deviation increase in the spy 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.

Figure 6: Effect of Surveillance on Patents per 100,000 Inhabitants



Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (2). 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.5 for details.

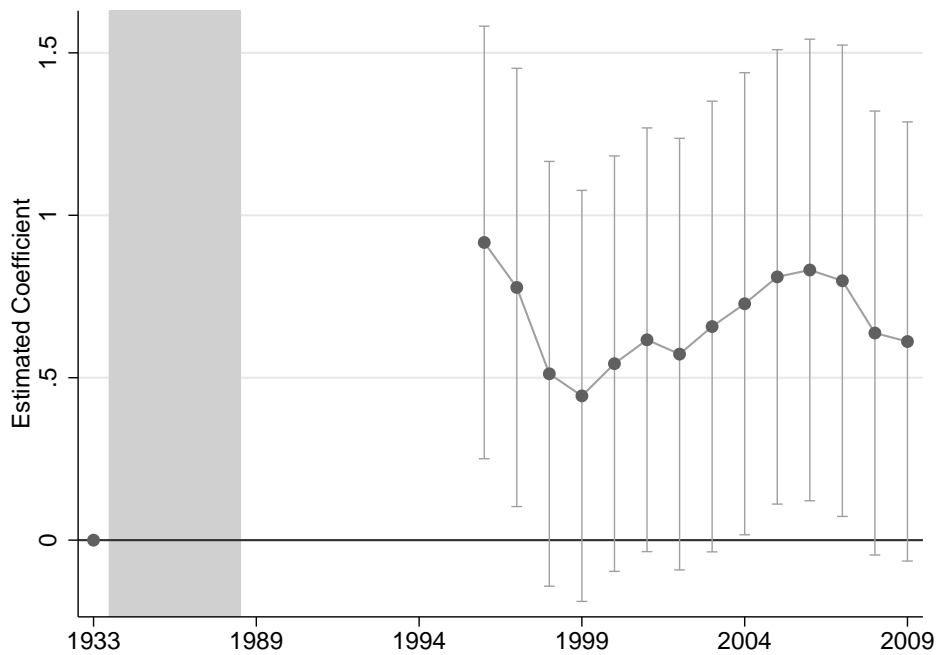
Next, we investigate the effect of state surveillance on county population. Average yearly and cumulated county-level population growth since the mid 1980s are depicted in Panel (A) of Figure 8. The graph shows two emigration waves after the fall of the Iron Curtain – a severe and rather short one immediately after reunification (between 1989 and 1992) and a moderate and longer one starting in 1998. Fuchs-Schündeln and Schündeln (2009) investigate the age, skill, and gender composition of these two migration waves in detail. They find that in the first wave it was rather the low-skilled who moved, while the second wave of migrants was driven by more educated and younger individuals.

In Panel (B) of Figure 8 and the corresponding Table A.7, we test whether these two emigration waves can be related to the intensity of Stasi spying in GDR counties. Using yearly county-level population data from 1985 to 1988 as our pre-treatment observations, we indeed find a negative population effect of spying that can be related to the two migration waves.³⁴ First, 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 spy density, yields an increase in

³⁴ Note that 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 7: Effect of Surveillance on Unemployment Rates

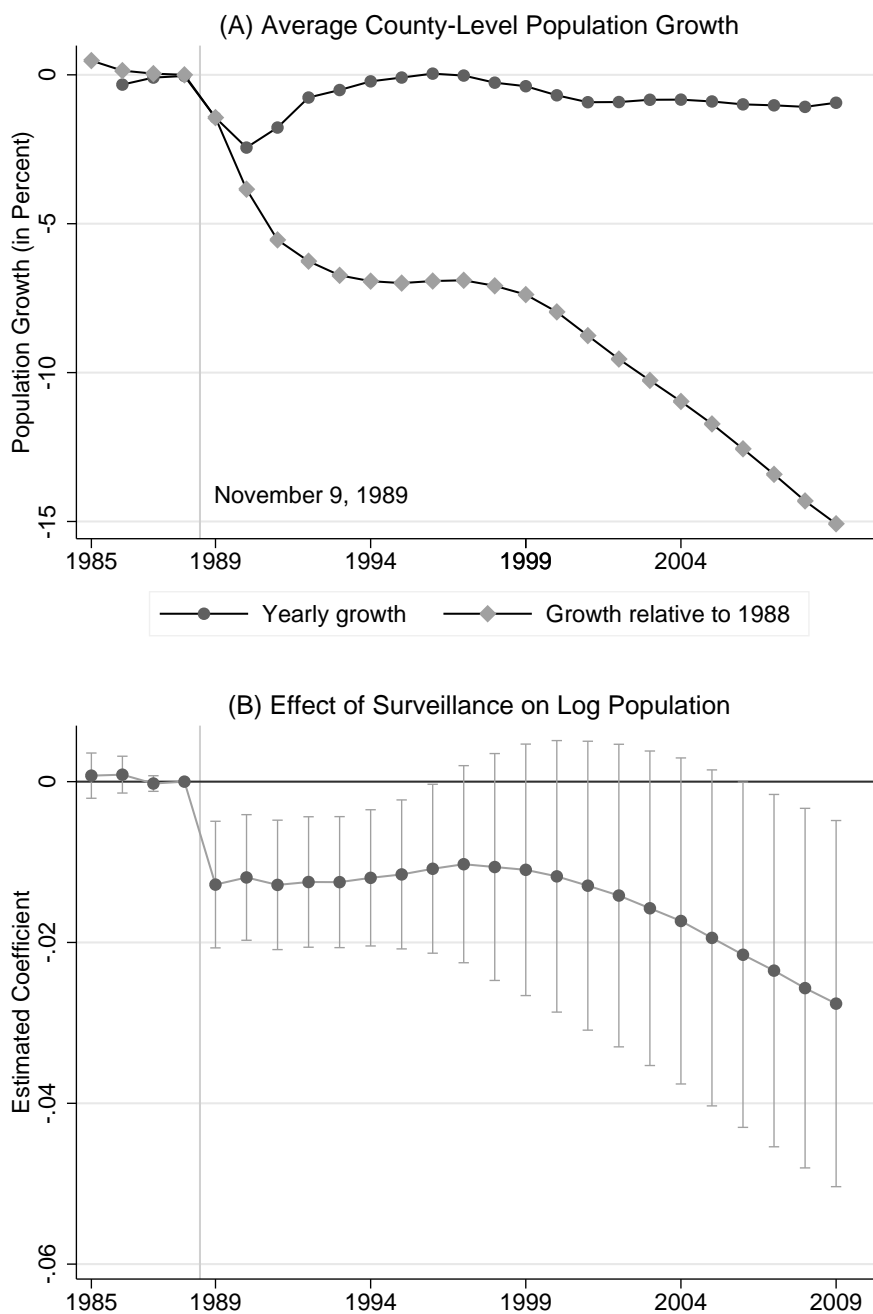


Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (2). The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (5) in Table A.6 for details.

the population level of 1.2 percent. Given that the average population loss in 1989 was 1.5 percent, this is a substantial effect. Further note that 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 intensity counties in addition to the initial population outflow. In other words, 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.

Given that the evolution of the surveillance effect on population closely mirrors the evolution of the cumulative population loss in East German counties shown in Panel (A) of Figure 8, we try to provide an upper bound of how much of the total population decline can be attributed to Stasi spying. The point estimate for the year 2009 of -0.028 implies that population levels in counties at the 25th percentile of the surveillance distribution would be about 3.9 percent higher than at the 75th percentile – everything else equal. Given the overall population decline of 15 percent, this estimate implies This number, which is clearly an upper bound, suggests that about 25 percent of the overall population could be due to people moving away from former higher-spying counties.

Figure 8: 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 point estimates and corresponding 95% confidence intervals of the spy density interacted with year dummies; see regression model (2). 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. For details on the source and construction of the variables, see Appendix Table B.3.

Migration Effect and Selection. The significant population response of spying gives rise to the question of how much of the other effects is due to government surveillance, and how much is due

to migration. While our main interest lies in the total effect, which includes (selective) out-migration, we assess the roles of direct surveillance versus indirect migration responses in the following.

In the border discontinuity design, we assign each individual the spy density of the GDR county, in which she lived in 1989. This prevents selection out-of-treatment in the first place. Moreover, we explicitly test the role of the population response by controlling for the county population growth rate since 1988. Specification (5) of Table 3 shows that estimates are hardly affected and remain significant.³⁶ In terms of selective migration, our preferred estimates are also conditional on a large set of individual control variables, which capture compositional differences within border county pairs. Introducing these controls hardly affects our results (cf. Tables 3 and A.2). This is also in line with the covariate smoothness test presented in Table 2, which shows no significant differences between high and low-spying counties.

In the panel design, all trust and economic outcomes are measured as rates, hence the estimates already account for base changes due to migration. In the absence of (i) a direct effect of surveillance, and (ii) selective out-migration, estimates for the respective rates would be zero. As we find significantly negative effects (positive for unemployment), it follows that there is either a direct effect of government surveillance on our outcomes, or selective out-migration, or both.³⁷

To better assess the potential scope of selective out-migration, we can make use of the panel data design and test whether the spy density had an effect on specific population shares. In terms of skills, Figure 9 shows that, if anything, there is a very small positive effect of the spy density on the share of high-skilled employees.³⁸ A one standard deviation increase in the spy density leads to an (insignificant) increase in the share of high-skilled of 0.2 percentage points. This finding is in line with Fuchs-Schündeln and Schündeln (2009), who show that the first migration wave (1989–1992) was negatively selected. In Figure 10, we assess the evolution of population shares by age groups. In Panel (B) of the figure, we see that the share of 15-34 year olds gradually starts to decline with the second immigration wave starting in the late 1990s. The initial population shock of the first wave (1989–1992) seems to be driven by the individuals who were 35-54 at that time and had no children (panels (C) and (A)). Last, taking all pieces of evidence together, the share of old workers is higher in the high-spying counties since they were neither affected in the first nor in the second migration wave.

Overall, the results on population shares do not provide strong evidence of selective out-migration. While the effect on the high-skilled 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

³⁶ As we do have separate population data for East and West Berlin after reunification, we had to drop East Berlin from the sample, which explains the lower number of observations. The results from the baseline specification are also robust to the exclusion of East Berlin, see specification (4) of Table 3.

³⁷ The direct response itself can be an immediate consequence of more intense surveillance and driven through an agglomeration effect if, for instance, the self-employment *rate* decreases with the population. We test for such agglomeration effects, by controlling for log current population in the regressions in which the respective rates are our left-hand side variable. As estimates remain unaffected, we can rule out this channel (see specification (6) of Tables A.3 to A.6).

³⁸ This result should be interpreted cautiously as the reliability of data on the skill distribution within the GDR is difficult to check. Although we obtained official administrative data of the working age population, the correlation of the county high-skill shares in 1989 with the shares in 1992, i.e. the first wave of the official data in unified Germany is 0.7.

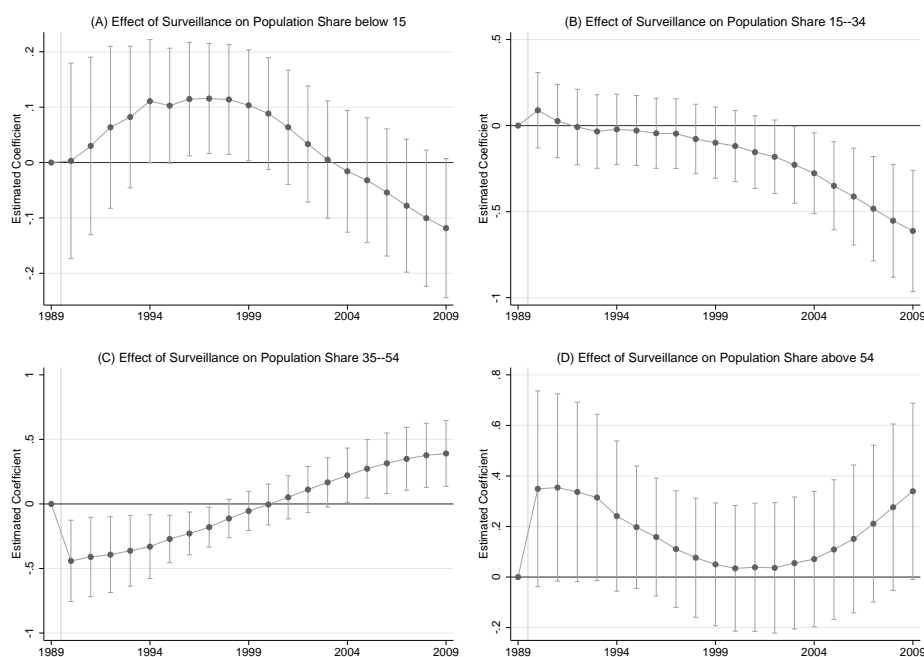
of the effects are very small, which strongly suggests that most of the effects is due to a direct effect of government surveillance on economic performance.

Figure 9: Effect of Surveillance on Share of High-Skilled



Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (2). 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.

Figure 10: Stasi Surveillance and Age Groups



Notes: Panel (A) ... 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.9–A.12 for details. For details on the source and construction of the variables, see Appendix Table B.3.

7 Conclusion

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

The results of our analysis show that more intense state surveillance had negative and long-lasting effects on both trust and economic performance. Moving from the 75th to 25th percentile of the spy density, would lead to an increase in electoral turnout of 1.0 percentage points. Moreover, it would have increased regional innovativeness and entrepreneurship through more patents per capita and higher self-employment rates. Eventually, the average unemployment rate would have been about 0.8 percentage points lower, which is equivalent to a 4.5 percent drop compared to the average in East Germany post unification. We also find that Stasi spying can explain parts up to 25 percent of the decline in population levels in East Germany.

Overall, our results imply that the East German regime did not only have a long-lasting impact on political preferences (Alesina and Fuchs-Schündeln, 2007), but also eroded institutional and interpersonal trust, which in turn exhibit long-lasting negative societal and economic effects. Our

findings demonstrate that the quality of political institutions significantly affects culture through lower levels of trust. Moreover, these effects are persistent and detectable 25 years after reunification. Given the intergenerational transmission of trust and reciprocity (Dohmen et al., 2012), it is likely that the legacy of the East German surveillance regime will even be longer.

Another mechanism that lead to long-lasting and permanent effects of the Stasi could arise due to regional divergence, which was initially induced by differences in intensities of surveillance and then had second round effects on economic performance. Along these lines, we document that differences in the spy density contributed to both migration waves, in 1989–1992 and the still on-going wave that started in 1998 (Fuchs-Schündeln and Schündeln, 2009).

In terms of external validity, it is likely that similar effects on trust and economic performance can be found in other countries of the former Warsaw pact, which relied on comparable surveillance systems (Albats, 1995, Harrison and Zaksauskiene, 2015). For these countries, it is, however, possible that the second-round effect triggered by out-migration is weaker. In the case of East Germany, the migration channel might be more pronounced as it was possibly to move within the country after the Fall of the Iron Curtain.

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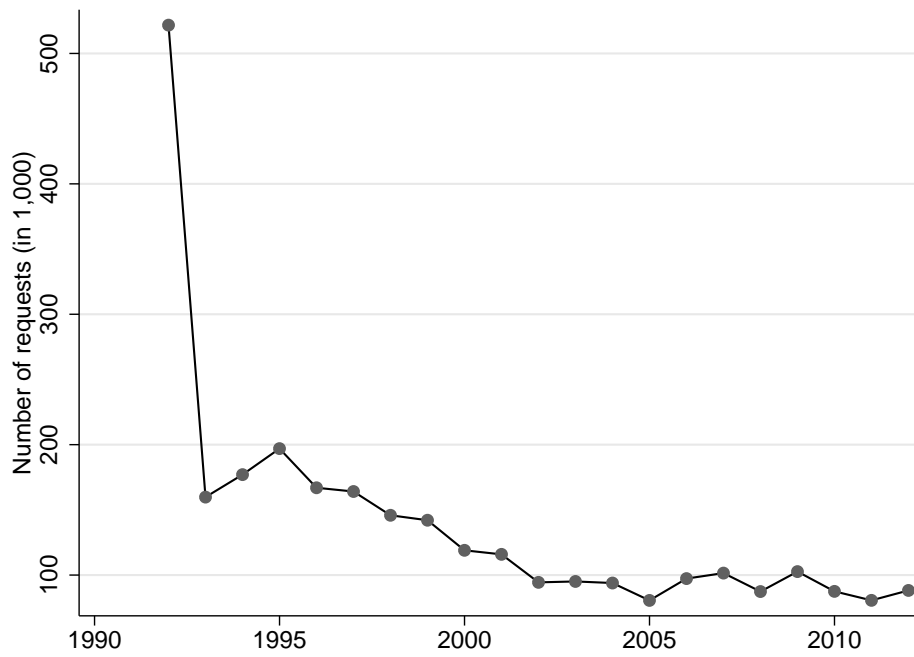
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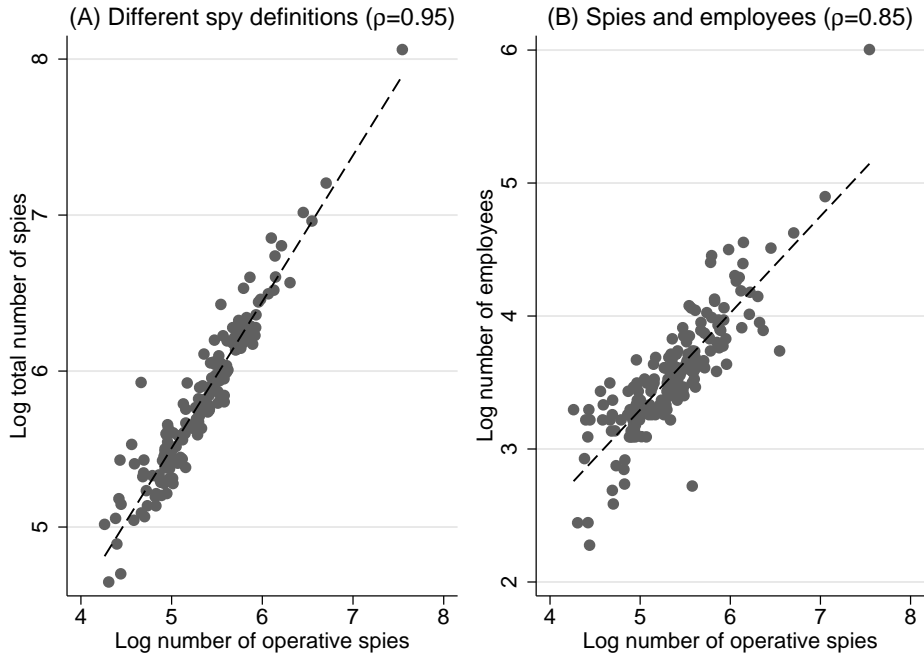
A Appendix

Figure A.1: Number of Requests for Inspection of Stasi File



Notes: Own presentation based on data provided by the Agency of the Federal Commissioner for the Stasi Records (BStU).

Figure A.2: Alternative Measures of Stasi Surveillance



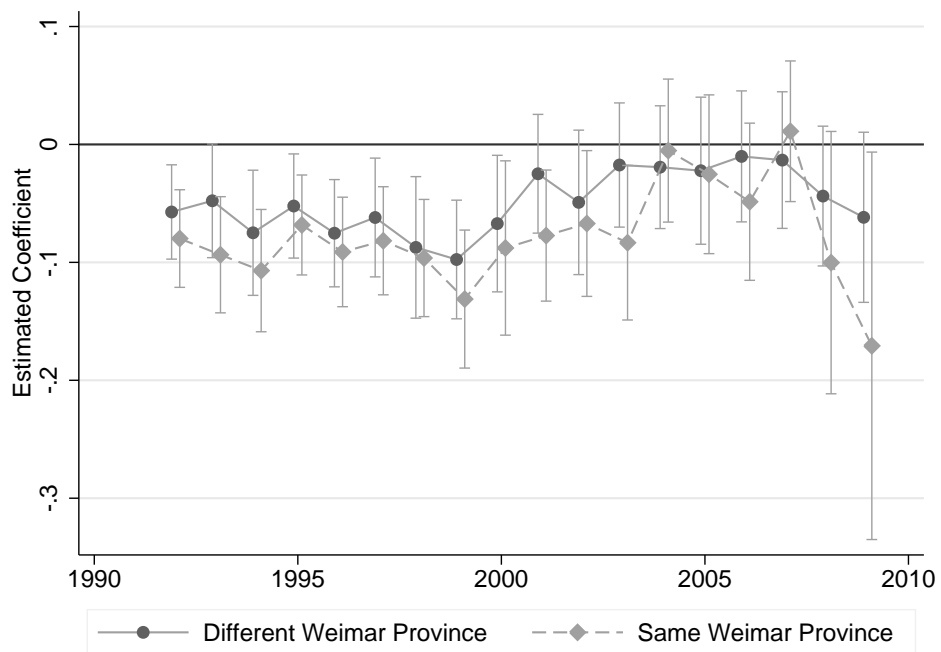
Notes: The graph plots the correlation between three different measures of Stasi surveillance: (i) the number of operative spies (unofficial collaborator category 1), which builds the base for our preferred measure of the spy density, (ii) the total number of spies in categories (1)-(3), and (iii) the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

Figure A.3: Migration from East to West Germany



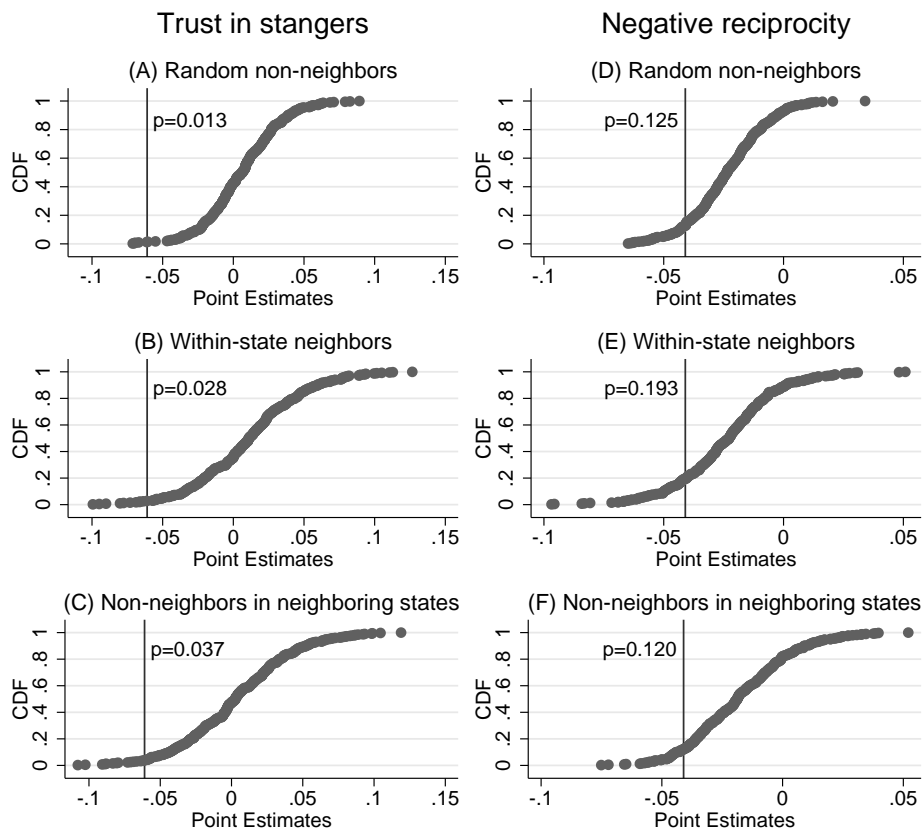
Notes: Own calculations using data from Rühle and Holzweißig (1988), Ritter and Lapp (1997) and monthly announcements of the West German Federal Ministry for Displaced Persons, Refugees and War Victims.

Figure A.4: Effect of Spying on Labor Income



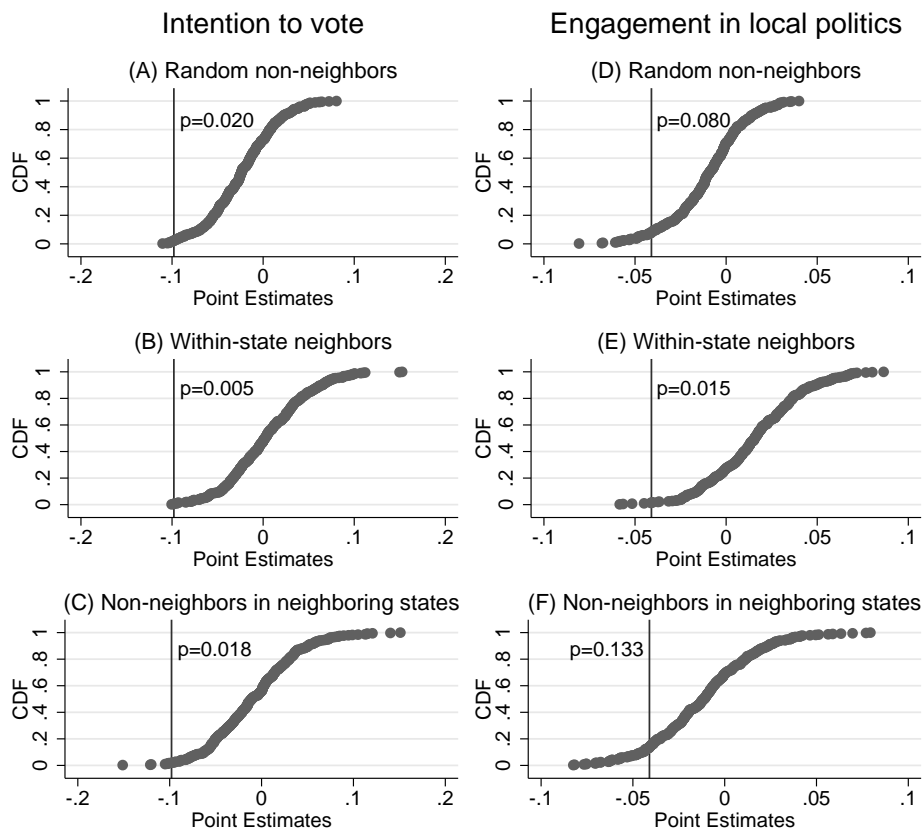
Notes: The graph plots the point estimates and corresponding 95% confidence intervals of the spy density interacted with (i) a dummy variable indicating that a border county pair was part of the same Weimar province or not and (ii) year dummies; see regression model (1). The specification includes individual and county level controls.

Figure A.5: Permutation Tests for Interpersonal Trust



Notes: The graph shows the empirical distribution of placebo effects for the two variables of interpersonal trust. The CDF is constructed from 400 placebo estimates, using our preferred specification and randomly drawn county pairs. We construct placebo county pairs by matching non-neighboring counties (Panels (A) and (D)), neighboring counties that are not at a state border (Panels (B) and (E)), and non-neighboring counties from neighboring states (Panels (C) and (F)). Vertical lines indicate our estimate as reported in Table 3.

Figure A.6: Permutation Test for Institutional Trust



Notes: The graph shows the empirical distribution of placebo effects for the two variables of institutional trust. The CDF is constructed from 400 placebo estimates, using our preferred specification and randomly drawn county pairs. We construct placebo county pairs by matching non-neighboring counties (Panels (A) and (D)), neighboring counties that are not at a state border (Panels (B) and (E)), and non-neighboring counties from neighboring states (Panels (C) and (F)). Vertical lines indicate our estimates as reported in Table 3.

Table A.1: Sensitivity Checks: Different Weights and Estimators

	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) Adj. Wts
Panel A – Trust in strangers							
Spy density	-0.041** (0.020)	-0.042* (0.022)	-0.061** (0.026)	-0.075*** (0.026)	-0.030 (0.020)	-0.227** (0.103)	-0.374*** (0.122)
Number of observations	3,389	3,389	3,389	3,389	3,389	3,389	3,389
Panel B – Negative reciprocity							
Spy density	-0.034*** (0.012)	-0.034*** (0.012)	-0.041*** (0.015)	-0.044*** (0.016)	-0.040*** (0.013)	-0.161*** (0.053)	-0.209*** (0.065)
Number of observations	3,011	3,011	3,011	3,011	3,011	3,011	3,011
Panel C – Intention to vote							
main							
Spy density	-0.057* (0.031)	-0.045* (0.026)	-0.098** (0.041)	-0.101** (0.042)	-0.065* (0.035)	-0.162** (0.080)	-0.329*** (0.104)
Number of observations	3,107	3,107	3,107	3,107	3,107	3,107	3,107
Panel D – Engagement in local politics							
Spy density	-0.004 (0.019)	-0.002 (0.016)	-0.041** (0.019)	-0.038* (0.020)	-0.020 (0.022)	-0.022 (0.091)	-0.485*** (0.113)
Number of observations	3,549	3,549	3,549	3,549	3,549	3,549	3,549
Individual controls		Yes	Yes	Yes	Yes		Yes
County controls			Yes	Yes	Yes		Yes

Notes: This table shows the β coefficients using different specifications. For better comparability, negative reciprocity is defined such that higher values indicate less negative reciprocal behavior. 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 the individual level in columns (1)-(5), and clustered at the borderpair in the Probit regressions. We restrict the sample to county pairs for which we observe individuals in both counties along the state border. In column (1)-(3), (6)-(7) cross-sectional weights are adjusted for duplicates of counties that are part of multiple border county pairs. In column (4), 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.2: The Effect of Spying on Labor Income: OLS and Border County Pair Results

	Base Sample		Border County Pair Sample				
	(1) Cs. Wts	(2) Adj. Wts	(3) Adj. Wts	(4) Adj. Wts	(5) Adj. Wts	(6) Cs. Wts	(7) No Wts
Spy density \times 1993	0.009 (0.018)	-0.030 (0.019)	-0.185*** (0.028)	-0.084*** (0.020)	-0.080*** (0.026)	-0.104*** (0.031)	-0.081*** (0.027)
Spy density \times 1994	0.005 (0.020)	-0.033 (0.022)	-0.131*** (0.028)	-0.085*** (0.021)	-0.082*** (0.027)	-0.099*** (0.030)	-0.086*** (0.027)
Spy density \times 1995	0.021 (0.017)	-0.018 (0.023)	-0.100*** (0.027)	-0.074*** (0.023)	-0.070** (0.028)	-0.096*** (0.032)	-0.075*** (0.027)
Spy density \times 1996	0.017 (0.018)	-0.032 (0.023)	-0.089*** (0.026)	-0.087*** (0.021)	-0.082*** (0.027)	-0.105*** (0.029)	-0.094*** (0.026)
Spy density \times 1997	0.019 (0.020)	-0.028 (0.026)	-0.078*** (0.025)	-0.082*** (0.024)	-0.078*** (0.027)	-0.105*** (0.030)	-0.088*** (0.026)
Spy density \times 1998	0.016 (0.021)	-0.037 (0.022)	-0.066** (0.025)	-0.090*** (0.023)	-0.085*** (0.027)	-0.109*** (0.030)	-0.096*** (0.027)
Spy density \times 1999	0.013 (0.020)	-0.029 (0.024)	-0.069** (0.028)	-0.082*** (0.025)	-0.077** (0.029)	-0.093*** (0.029)	-0.093*** (0.029)
Spy density \times 2000	0.022 (0.021)	-0.029 (0.026)	-0.071** (0.028)	-0.086*** (0.028)	-0.081*** (0.030)	-0.083*** (0.030)	-0.099*** (0.028)
Spy density \times 2001	0.032 (0.022)	-0.008 (0.028)	-0.057* (0.030)	-0.066** (0.028)	-0.061** (0.031)	-0.055* (0.031)	-0.083*** (0.029)
Spy density \times 2002	0.033 (0.026)	0.004 (0.033)	-0.056* (0.030)	-0.053 (0.033)	-0.049 (0.037)	-0.051 (0.038)	-0.085** (0.033)
Spy density \times 2003	0.039 (0.028)	0.017 (0.034)	-0.045 (0.028)	-0.040 (0.034)	-0.033 (0.037)	-0.029 (0.039)	-0.087*** (0.032)
Spy density \times 2004	0.033 (0.028)	0.004 (0.035)	-0.051* (0.029)	-0.052 (0.037)	-0.045 (0.040)	-0.043 (0.042)	-0.098*** (0.033)
Spy density \times 2005	0.021 (0.029)	0.014 (0.032)	-0.035 (0.028)	-0.043 (0.036)	-0.038 (0.039)	-0.041 (0.040)	-0.103*** (0.033)
Spy density \times 2006	0.018 (0.026)	0.005 (0.031)	-0.030 (0.027)	-0.053 (0.033)	-0.048 (0.038)	-0.055 (0.038)	-0.093** (0.036)
Spy density \times 2007	0.020 (0.026)	0.017 (0.034)	-0.041 (0.030)	-0.041 (0.040)	-0.035 (0.044)	-0.045 (0.042)	-0.081** (0.038)
Spy density \times 2008	-0.047 (0.049)	-0.032 (0.042)	-0.030 (0.032)	-0.089* (0.045)	-0.085* (0.047)	-0.072* (0.042)	-0.085** (0.035)
Spy density \times 2009	-0.089 (0.059)	-0.109** (0.049)	-0.050 (0.049)	-0.169** (0.069)	-0.164** (0.069)	-0.133** (0.052)	-0.112*** (0.035)
Number of observations	16,101	17,084	17,084	17,084	17,084	17,084	17,084
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 the β coefficients of the border discontinuity model laid out in equation (1) using different specifications for the overall and border county pair sample. 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 the individual level in the base sample (Column (1)), and two-way clustered at the county pair and individual level in the border county pair sample. The usual confidence levels apply (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to border 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)-(5) cross-sectional weights are adjusted for duplicates of counties that are part of multiple border county pairs. In column (6), standard cross-sectional weights are used. Results presented in column (7) do not include sample weights. For detailed information on the control variables, see Data Appendix B.

Table A.3: Effect of Surveillance on Electoral Turnout

	(1) Rate	(2) Rate	(3) Rate	(4) Rate	(5) Rate	(6) Rate	(7) Rate
Spy density \times 05/1924	0.049 (0.215)	0.049 (0.216)	-0.190 (0.209)	-0.190 (0.209)	-0.190 (0.209)	-0.198 (0.208)	-0.190 (0.209)
Spy density \times 12/1924	0.050 (0.202)	0.050 (0.202)	-0.144 (0.208)	-0.144 (0.208)	-0.144 (0.208)	-0.153 (0.207)	-0.144 (0.208)
Spy density \times 1928	0.334 (0.236)	0.334 (0.236)	0.051 (0.226)	0.051 (0.227)	0.051 (0.227)	0.051 (0.227)	0.051 (0.227)
Spy density \times 1930	-0.334* (0.173)	-0.334* (0.173)	-0.307* (0.166)	-0.307* (0.167)	-0.307* (0.167)	-0.306* (0.167)	-0.307* (0.167)
Spy density \times 07/1932	-0.071 (0.116)	-0.071 (0.116)	-0.035 (0.083)	-0.035 (0.083)	-0.035 (0.083)	-0.034 (0.083)	-0.035 (0.083)
Spy density \times 1990	-0.285 (0.332)	-0.818** (0.384)	-0.055 (0.302)	-0.033 (0.310)	-0.114 (0.310)	-0.091 (0.307)	
Spy density \times 1994	-0.432 (0.332)	-0.965** (0.401)	-0.399 (0.317)	-0.377 (0.322)	-0.458 (0.322)	-0.436 (0.315)	
Spy density \times 1998	-0.920*** (0.313)	-1.453*** (0.384)	-0.144 (0.281)	-0.122 (0.288)	-0.203 (0.292)	-0.183 (0.294)	-0.123 (0.294)
Spy density \times 2002	0.155 (0.309)	-0.377 (0.387)	-0.501* (0.286)	-0.479 (0.291)	-0.560* (0.294)	-0.534* (0.289)	-0.449 (0.290)
Spy density \times 2005	0.087 (0.292)	-0.446 (0.365)	-0.462 (0.290)	-0.440 (0.298)	-0.521* (0.303)	-0.485* (0.289)	-0.370 (0.304)
Spy density \times 2009	0.436 (0.333)	-0.097 (0.406)	-0.637** (0.311)	-0.615* (0.320)	-0.696** (0.320)	-0.646** (0.309)	-0.450 (0.319)
Post \times Object of SI \times Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post \times County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State \times Year FE			Yes	Yes	Yes	Yes	Yes
Post \times Opposition controls				Yes	Yes	Yes	Yes
Post \times Industry controls					Yes	Yes	Yes
Log current population						Yes	
Post \times Transfers							Yes
Observations	2220	2220	2220	2220	2220	2218	1848
Adjusted R^2	0.826	0.832	0.921	0.923	0.925	0.927	0.934

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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 November 1932 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.4: Effect of Surveillance on Self-Employment Rates

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

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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.5: Effect of Surveillance on Patents per 100,000 Inhabitants

	(1) Per Capita	(2) Per Capita	(3) Per Capita	(4) Per Capita	(5) Per Capita	(6) Per Capita	(7) Per Capita
Spy density × 1928	0.525* (0.278)	0.525* (0.279)	0.609 (0.413)	0.609 (0.414)	0.609 (0.414)	0.610 (0.415)	0.609 (0.414)
Spy density × 1993	-0.697 (0.635)	0.511 (0.856)	-0.148 (0.836)	0.012 (0.835)	-0.077 (0.862)	-0.109 (0.870)	
Spy density × 1995	-0.508 (0.771)	0.700 (0.884)	-0.087 (0.915)	0.073 (0.866)	-0.016 (0.887)	-0.046 (0.885)	-0.010 (0.884)
Spy density × 1996	-0.750 (0.806)	0.458 (0.916)	-0.051 (0.927)	0.108 (0.879)	0.020 (0.904)	-0.009 (0.910)	-0.027 (0.906)
Spy density × 1997	-0.877 (0.729)	0.330 (0.839)	-0.270 (0.774)	-0.110 (0.734)	-0.198 (0.767)	-0.226 (0.776)	-0.235 (0.770)
Spy density × 1998	-1.920** (0.778)	-0.713 (0.938)	-1.126 (0.852)	-0.966 (0.800)	-1.055 (0.819)	-1.083 (0.825)	-1.132 (0.809)
Spy density × 1999	-1.340 (0.832)	-0.132 (0.973)	-0.785 (0.896)	-0.626 (0.843)	-0.714 (0.863)	-0.744 (0.871)	-0.790 (0.854)
Spy density × 2000	-1.977** (0.834)	-0.770 (0.985)	-1.524* (0.876)	-1.364* (0.803)	-1.453* (0.824)	-1.484* (0.830)	-1.496* (0.818)
Spy density × 2001	-1.538* (0.930)	-0.331 (1.045)	-1.240 (0.917)	-1.080 (0.877)	-1.169 (0.897)	-1.202 (0.906)	-1.169 (0.884)
Spy density × 2002	-1.899* (1.120)	-0.692 (1.210)	-1.557 (1.125)	-1.397 (1.066)	-1.486 (1.079)	-1.521 (1.092)	-1.468 (1.057)
Spy density × 2003	-2.696** (1.141)	-1.489 (1.194)	-2.494* (1.289)	-2.335* (1.209)	-2.423** (1.220)	-2.462** (1.228)	-2.368** (1.190)
Spy density × 2004	-1.851 (1.125)	-0.643 (1.212)	-1.531 (1.175)	-1.371 (1.117)	-1.460 (1.138)	-1.502 (1.156)	-1.364 (1.126)
Spy density × 2005	-2.166* (1.193)	-0.959 (1.287)	-2.559** (1.163)	-2.399** (1.109)	-2.487** (1.121)	-2.534** (1.135)	-2.374** (1.106)
Post × Object of SI × 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
Log current population						Yes	
Post × Transfers							Yes
Observations	2590	2590	2590	2590	2590	2590	2405
Adjusted R ²	0.426	0.436	0.503	0.508	0.510	0.511	0.531

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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 1929 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.6: Effect of Surveillance on Unemployment Rates

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

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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: Effect of Surveillance on Log Population

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

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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.8: Effect of Surveillance on Share of High-Skilled

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density \times 1995	0.292* (0.154)	0.003 (0.166)	0.065 (0.197)	0.060 (0.198)	0.134 (0.190)	0.090 (0.178)	0.138 (0.189)
Spy density \times 1996	0.344** (0.156)	0.055 (0.165)	0.074 (0.195)	0.069 (0.197)	0.143 (0.188)	0.100 (0.176)	0.148 (0.187)
Spy density \times 1997	0.367** (0.158)	0.078 (0.164)	0.097 (0.194)	0.092 (0.196)	0.166 (0.187)	0.125 (0.173)	0.169 (0.185)
Spy density \times 1998	0.382** (0.157)	0.093 (0.163)	0.118 (0.192)	0.113 (0.194)	0.187 (0.184)	0.145 (0.170)	0.190 (0.183)
Spy density \times 1999	0.375** (0.161)	0.086 (0.165)	0.133 (0.192)	0.127 (0.194)	0.202 (0.184)	0.159 (0.168)	0.205 (0.183)
Spy density \times 2000	0.371** (0.160)	0.082 (0.164)	0.132 (0.191)	0.126 (0.193)	0.201 (0.182)	0.155 (0.166)	0.201 (0.181)
Spy density \times 2001	0.372** (0.161)	0.083 (0.164)	0.128 (0.191)	0.123 (0.193)	0.197 (0.183)	0.149 (0.166)	0.196 (0.182)
Spy density \times 2002	0.378** (0.158)	0.089 (0.162)	0.126 (0.192)	0.121 (0.194)	0.195 (0.184)	0.143 (0.167)	0.195 (0.183)
Spy density \times 2003	0.388** (0.160)	0.099 (0.162)	0.125 (0.192)	0.119 (0.194)	0.194 (0.185)	0.137 (0.167)	0.192 (0.184)
Spy density \times 2004	0.394** (0.159)	0.105 (0.161)	0.134 (0.193)	0.129 (0.195)	0.203 (0.186)	0.142 (0.167)	0.202 (0.184)
Spy density \times 2005	0.385** (0.161)	0.096 (0.162)	0.120 (0.195)	0.115 (0.197)	0.189 (0.189)	0.123 (0.169)	0.189 (0.187)
Spy density \times 2006	0.377** (0.159)	0.088 (0.161)	0.117 (0.194)	0.112 (0.197)	0.186 (0.189)	0.114 (0.168)	0.183 (0.186)
Spy density \times 2007	0.353** (0.157)	0.064 (0.159)	0.099 (0.193)	0.094 (0.195)	0.168 (0.188)	0.091 (0.167)	0.161 (0.186)
Spy density \times 2008	0.334** (0.156)	0.045 (0.159)	0.077 (0.193)	0.072 (0.196)	0.146 (0.189)	0.063 (0.167)	0.139 (0.188)
Spy density \times 2009	0.316** (0.155)	0.027 (0.159)	0.070 (0.194)	0.064 (0.196)	0.139 (0.190)	0.050 (0.168)	0.137 (0.187)
Post \times Object of SI \times Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post \times County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State \times Year FE			Yes	Yes	Yes	Yes	Yes
Post \times Opposition controls				Yes	Yes	Yes	Yes
Post \times Industry controls					Yes	Yes	Yes
Log current population						Yes	
Post \times Transfers							Yes
Observations	2960	2960	2960	2960	2960	2960	2958
Adjusted R^2	0.627	0.733	0.775	0.778	0.804	0.813	0.806

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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.9: Effect of Surveillance on Population Share below 15

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density \times 1990	-0.106 (0.073)	0.302*** (0.077)	-0.016 (0.088)	-0.008 (0.089)	0.003 (0.089)	0.051 (0.079)	
Spy density \times 1991	-0.110 (0.072)	0.298*** (0.071)	0.011 (0.080)	0.019 (0.081)	0.030 (0.081)	0.085 (0.072)	
Spy density \times 1992	-0.122* (0.072)	0.287*** (0.064)	0.045 (0.073)	0.052 (0.074)	0.064 (0.074)	0.117* (0.066)	
Spy density \times 1993	-0.149** (0.075)	0.259*** (0.057)	0.063 (0.064)	0.071 (0.065)	0.082 (0.065)	0.137** (0.059)	
Spy density \times 1994	-0.156** (0.079)	0.252*** (0.054)	0.092 (0.056)	0.100* (0.057)	0.111* (0.056)	0.166*** (0.052)	
Spy density \times 1995	-0.150* (0.080)	0.263*** (0.055)	0.083 (0.053)	0.091* (0.054)	0.103* (0.053)	0.173*** (0.052)	0.086 (0.056)
Spy density \times 1996	-0.171** (0.084)	0.243*** (0.053)	0.095* (0.052)	0.103* (0.054)	0.115** (0.052)	0.181*** (0.051)	0.095* (0.055)
Spy density \times 1997	-0.199** (0.088)	0.214*** (0.051)	0.096* (0.051)	0.104** (0.052)	0.116** (0.050)	0.179*** (0.049)	0.097* (0.052)
Spy density \times 1998	-0.222** (0.091)	0.191*** (0.050)	0.094* (0.051)	0.102* (0.052)	0.114** (0.050)	0.179*** (0.048)	0.094* (0.051)
Spy density \times 1999	-0.258*** (0.095)	0.155*** (0.050)	0.084 (0.051)	0.092* (0.052)	0.103** (0.051)	0.171*** (0.047)	0.084* (0.050)
Spy density \times 2000	-0.302*** (0.099)	0.111** (0.050)	0.069 (0.052)	0.077 (0.053)	0.088* (0.051)	0.160*** (0.046)	0.071 (0.050)
Spy density \times 2001	-0.350*** (0.102)	0.064 (0.051)	0.044 (0.053)	0.052 (0.053)	0.064 (0.052)	0.142*** (0.046)	0.049 (0.051)
Spy density \times 2002	-0.405*** (0.105)	0.008 (0.052)	0.014 (0.054)	0.022 (0.054)	0.033 (0.053)	0.118** (0.047)	0.019 (0.052)
Spy density \times 2003	-0.453*** (0.107)	-0.040 (0.052)	-0.014 (0.054)	-0.006 (0.054)	0.005 (0.054)	0.098** (0.048)	-0.007 (0.052)
Spy density \times 2004	-0.495*** (0.110)	-0.081 (0.053)	-0.035 (0.056)	-0.027 (0.056)	-0.016 (0.056)	0.086* (0.050)	-0.027 (0.054)
Spy density \times 2005	-0.523*** (0.112)	-0.110** (0.055)	-0.051 (0.057)	-0.043 (0.057)	-0.032 (0.057)	0.081 (0.052)	-0.043 (0.055)
Spy density \times 2006	-0.542*** (0.112)	-0.129** (0.056)	-0.074 (0.058)	-0.066 (0.058)	-0.054 (0.058)	0.070 (0.051)	-0.061 (0.055)
Spy density \times 2007	-0.562*** (0.114)	-0.149** (0.058)	-0.097 (0.060)	-0.089 (0.060)	-0.078 (0.061)	0.056 (0.051)	-0.080 (0.058)
Spy density \times 2008	-0.573*** (0.114)	-0.159*** (0.060)	-0.120* (0.062)	-0.112* (0.062)	-0.100 (0.062)	0.045 (0.051)	-0.104* (0.060)
Spy density \times 2009	-0.583*** (0.114)	-0.169*** (0.061)	-0.138** (0.063)	-0.130** (0.063)	-0.119* (0.064)	0.038 (0.051)	-0.105* (0.060)
Post \times Object of SI \times Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post \times County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State \times Year FE			Yes		Yes	Yes	Yes
Post \times Opposition controls				Yes	Yes	Yes	Yes
Post \times Industry controls					Yes	Yes	Yes
Log current population						Yes	
Post \times Transfers							Yes
Observations	3630	3630	3630	3630	3630	3630	2958
Adjusted R^2	0.972	0.978	0.989	0.989	0.989	0.993	0.990

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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: Effect of Surveillance on Population Share 15–34

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density × 1990	-0.100 (0.099)	0.317*** (0.095)	0.118 (0.116)	0.112 (0.119)	0.089 (0.111)	0.099 (0.111)	
Spy density × 1991	-0.162 (0.100)	0.255*** (0.095)	0.055 (0.113)	0.049 (0.116)	0.026 (0.108)	0.037 (0.108)	
Spy density × 1992	-0.202** (0.101)	0.214** (0.097)	0.021 (0.116)	0.015 (0.119)	-0.008 (0.111)	0.003 (0.111)	
Spy density × 1993	-0.243** (0.100)	0.173* (0.094)	-0.005 (0.114)	-0.011 (0.116)	-0.034 (0.108)	-0.023 (0.108)	
Spy density × 1994	-0.254** (0.101)	0.163* (0.092)	0.007 (0.107)	0.002 (0.111)	-0.022 (0.103)	-0.010 (0.103)	
Spy density × 1995	-0.259*** (0.090)	0.165* (0.094)	0.000 (0.104)	-0.005 (0.109)	-0.029 (0.103)	-0.014 (0.103)	-0.021 (0.113)
Spy density × 1996	-0.306*** (0.095)	0.118 (0.094)	-0.016 (0.105)	-0.021 (0.109)	-0.044 (0.103)	-0.031 (0.103)	-0.039 (0.114)
Spy density × 1997	-0.347*** (0.098)	0.078 (0.095)	-0.018 (0.104)	-0.023 (0.108)	-0.047 (0.103)	-0.034 (0.102)	-0.039 (0.113)
Spy density × 1998	-0.411*** (0.101)	0.014 (0.096)	-0.049 (0.104)	-0.054 (0.108)	-0.078 (0.102)	-0.064 (0.102)	-0.071 (0.112)
Spy density × 1999	-0.456*** (0.104)	-0.032 (0.098)	-0.071 (0.106)	-0.076 (0.110)	-0.099 (0.105)	-0.086 (0.104)	-0.092 (0.115)
Spy density × 2000	-0.492*** (0.107)	-0.067 (0.099)	-0.090 (0.107)	-0.095 (0.110)	-0.119 (0.105)	-0.104 (0.104)	-0.106 (0.114)
Spy density × 2001	-0.548*** (0.111)	-0.124 (0.102)	-0.125 (0.109)	-0.130 (0.112)	-0.154 (0.107)	-0.138 (0.106)	-0.137 (0.114)
Spy density × 2002	-0.594*** (0.115)	-0.169 (0.103)	-0.152 (0.111)	-0.157 (0.114)	-0.181* (0.108)	-0.164 (0.108)	-0.165 (0.115)
Spy density × 2003	-0.637*** (0.121)	-0.213** (0.108)	-0.199* (0.116)	-0.204* (0.118)	-0.228** (0.113)	-0.209* (0.113)	-0.209* (0.119)
Spy density × 2004	-0.682*** (0.125)	-0.257** (0.109)	-0.248** (0.122)	-0.253** (0.123)	-0.277** (0.119)	-0.256** (0.120)	-0.259** (0.124)
Spy density × 2005	-0.736*** (0.132)	-0.311*** (0.114)	-0.321** (0.133)	-0.326** (0.134)	-0.350*** (0.130)	-0.327** (0.131)	-0.332** (0.134)
Spy density × 2006	-0.782*** (0.139)	-0.358*** (0.120)	-0.384*** (0.146)	-0.389*** (0.146)	-0.412*** (0.143)	-0.387*** (0.144)	-0.388*** (0.146)
Spy density × 2007	-0.841*** (0.146)	-0.416*** (0.124)	-0.454*** (0.157)	-0.459*** (0.157)	-0.483*** (0.154)	-0.456*** (0.155)	-0.448*** (0.156)
Spy density × 2008	-0.917*** (0.154)	-0.493*** (0.131)	-0.524*** (0.170)	-0.529*** (0.169)	-0.553*** (0.166)	-0.523*** (0.168)	-0.519*** (0.165)
Spy density × 2009	-0.987*** (0.162)	-0.563*** (0.139)	-0.584*** (0.182)	-0.588*** (0.182)	-0.612*** (0.179)	-0.581*** (0.180)	-0.573*** (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
Log current population						Yes	
Post × Transfers							Yes
Observations	3630	3630	3630	3630	3630	3630	2958
Adjusted R ²	0.927	0.933	0.940	0.940	0.941	0.941	0.946

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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: Effect of Surveillance on Population Share 35–54

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density × 1990	0.148 (0.115)	-0.751*** (0.136)	-0.494*** (0.157)	-0.499*** (0.158)	-0.441*** (0.160)	-0.363*** (0.135)	
Spy density × 1991	0.170 (0.114)	-0.730*** (0.132)	-0.464*** (0.153)	-0.468*** (0.154)	-0.410*** (0.156)	-0.321** (0.131)	
Spy density × 1992	0.195* (0.113)	-0.704*** (0.126)	-0.445*** (0.146)	-0.450*** (0.147)	-0.392*** (0.149)	-0.305** (0.125)	
Spy density × 1993	0.236** (0.113)	-0.663*** (0.119)	-0.416*** (0.137)	-0.420*** (0.137)	-0.363*** (0.139)	-0.272** (0.119)	
Spy density × 1994	0.318*** (0.112)	-0.582*** (0.110)	-0.384*** (0.124)	-0.388*** (0.124)	-0.331*** (0.125)	-0.240** (0.114)	
Spy density × 1995	0.364*** (0.094)	-0.540*** (0.079)	-0.325*** (0.091)	-0.330*** (0.092)	-0.272*** (0.093)	-0.156 (0.096)	-0.345*** (0.113)
Spy density × 1996	0.442*** (0.103)	-0.462*** (0.077)	-0.283*** (0.084)	-0.287*** (0.085)	-0.229*** (0.084)	-0.119 (0.093)	-0.302*** (0.103)
Spy density × 1997	0.527*** (0.114)	-0.377*** (0.078)	-0.234*** (0.080)	-0.239*** (0.081)	-0.180** (0.078)	-0.076 (0.092)	-0.254*** (0.094)
Spy density × 1998	0.640*** (0.126)	-0.264*** (0.081)	-0.167** (0.080)	-0.172** (0.080)	-0.113 (0.076)	-0.006 (0.092)	-0.188** (0.088)
Spy density × 1999	0.763*** (0.137)	-0.141 (0.087)	-0.108 (0.082)	-0.113 (0.083)	-0.054 (0.077)	0.056 (0.095)	-0.129 (0.086)
Spy density × 2000	0.871*** (0.148)	-0.033 (0.094)	-0.058 (0.087)	-0.063 (0.087)	-0.004 (0.080)	0.113 (0.097)	-0.082 (0.085)
Spy density × 2001	0.991*** (0.156)	0.087 (0.100)	-0.002 (0.093)	-0.007 (0.093)	0.052 (0.085)	0.179* (0.102)	-0.027 (0.086)
Spy density × 2002	1.081*** (0.164)	0.177* (0.106)	0.057 (0.099)	0.052 (0.099)	0.111 (0.091)	0.249** (0.108)	0.034 (0.089)
Spy density × 2003	1.147*** (0.169)	0.243** (0.111)	0.113 (0.106)	0.108 (0.106)	0.167* (0.097)	0.319*** (0.114)	0.089 (0.093)
Spy density × 2004	1.226*** (0.177)	0.321*** (0.118)	0.168 (0.115)	0.163 (0.115)	0.222** (0.107)	0.387*** (0.123)	0.145 (0.101)
Spy density × 2005	1.288*** (0.181)	0.384*** (0.122)	0.219* (0.122)	0.214* (0.122)	0.273** (0.114)	0.457*** (0.131)	0.197* (0.106)
Spy density × 2006	1.329*** (0.184)	0.425*** (0.125)	0.261** (0.127)	0.257** (0.126)	0.315*** (0.119)	0.518*** (0.137)	0.237** (0.110)
Spy density × 2007	1.352*** (0.184)	0.448*** (0.126)	0.296** (0.130)	0.291** (0.129)	0.349*** (0.123)	0.569*** (0.143)	0.267** (0.116)
Spy density × 2008	1.361*** (0.185)	0.457*** (0.126)	0.323** (0.133)	0.318** (0.132)	0.377*** (0.126)	0.615*** (0.148)	0.295** (0.118)
Spy density × 2009	1.353*** (0.184)	0.449*** (0.126)	0.337** (0.137)	0.332** (0.135)	0.391*** (0.129)	0.646*** (0.152)	0.305** (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
Log current population						Yes	
Post × Transfers							Yes
Observations	3630	3630	3630	3630	3630	3630	2958
Adjusted R ²	0.877	0.892	0.914	0.914	0.915	0.932	0.936

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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: Effect of Surveillance on Population Share above 54

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density \times 1990	0.058 (0.164)	0.132 (0.165)	0.393** (0.194)	0.395** (0.193)	0.349* (0.196)	0.214 (0.158)	
Spy density \times 1991	0.103 (0.162)	0.177 (0.157)	0.398** (0.185)	0.400** (0.185)	0.354* (0.188)	0.199 (0.151)	
Spy density \times 1992	0.128 (0.160)	0.203 (0.152)	0.380** (0.178)	0.382** (0.178)	0.337* (0.180)	0.185 (0.145)	
Spy density \times 1993	0.156 (0.159)	0.231 (0.144)	0.358** (0.165)	0.360** (0.165)	0.315* (0.167)	0.158 (0.138)	
Spy density \times 1994	0.092 (0.160)	0.167 (0.138)	0.285* (0.149)	0.287* (0.149)	0.241 (0.151)	0.085 (0.133)	
Spy density \times 1995	0.045 (0.161)	0.112 (0.126)	0.242** (0.122)	0.244* (0.125)	0.197 (0.123)	-0.003 (0.128)	0.280** (0.135)
Spy density \times 1996	0.035 (0.168)	0.101 (0.126)	0.203* (0.118)	0.205* (0.122)	0.158 (0.118)	-0.031 (0.128)	0.246* (0.129)
Spy density \times 1997	0.020 (0.178)	0.086 (0.128)	0.156 (0.119)	0.157 (0.123)	0.111 (0.117)	-0.070 (0.129)	0.196 (0.123)
Spy density \times 1998	-0.008 (0.187)	0.059 (0.132)	0.121 (0.123)	0.123 (0.127)	0.077 (0.119)	-0.109 (0.132)	0.165 (0.121)
Spy density \times 1999	-0.049 (0.194)	0.017 (0.138)	0.095 (0.128)	0.097 (0.132)	0.050 (0.123)	-0.141 (0.136)	0.138 (0.124)
Spy density \times 2000	-0.077 (0.200)	-0.011 (0.142)	0.079 (0.132)	0.081 (0.136)	0.035 (0.126)	-0.169 (0.138)	0.117 (0.125)
Spy density \times 2001	-0.093 (0.204)	-0.027 (0.146)	0.083 (0.134)	0.085 (0.138)	0.039 (0.128)	-0.183 (0.140)	0.115 (0.126)
Spy density \times 2002	-0.082 (0.209)	-0.016 (0.148)	0.081 (0.137)	0.083 (0.140)	0.036 (0.131)	-0.203 (0.141)	0.113 (0.128)
Spy density \times 2003	-0.056 (0.211)	0.010 (0.148)	0.100 (0.138)	0.102 (0.141)	0.055 (0.133)	-0.208 (0.142)	0.127 (0.130)
Spy density \times 2004	-0.049 (0.214)	0.017 (0.150)	0.116 (0.141)	0.118 (0.143)	0.071 (0.136)	-0.216 (0.143)	0.141 (0.133)
Spy density \times 2005	-0.029 (0.216)	0.037 (0.153)	0.154 (0.145)	0.155 (0.147)	0.109 (0.140)	-0.210 (0.145)	0.177 (0.139)
Spy density \times 2006	-0.005 (0.219)	0.061 (0.159)	0.196 (0.153)	0.198 (0.154)	0.151 (0.148)	-0.200 (0.149)	0.212 (0.148)
Spy density \times 2007	0.050 (0.221)	0.116 (0.162)	0.256 (0.158)	0.258 (0.160)	0.211 (0.157)	-0.169 (0.155)	0.261 (0.159)
Spy density \times 2008	0.129 (0.226)	0.195 (0.168)	0.321* (0.167)	0.323* (0.168)	0.276* (0.167)	-0.137 (0.162)	0.329* (0.167)
Spy density \times 2009	0.217 (0.231)	0.283 (0.175)	0.384** (0.177)	0.386** (0.177)	0.340* (0.177)	-0.103 (0.169)	0.373** (0.179)
Post \times Object of SI \times Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post \times County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State \times Year FE			Yes	Yes	Yes	Yes	Yes
Post \times Opposition controls				Yes	Yes	Yes	Yes
Post \times Industry controls					Yes	Yes	Yes
Log current population						Yes	
Post \times Transfers							Yes
Observations	3630	3630	3630	3630	3630	3630	2958
Adjusted R^2	0.931	0.940	0.948	0.948	0.949	0.975	0.965

Notes: This table shows the β coefficients of the panel data model laid out in equation (2). 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. We present descriptive statistics for our outcome measures as well as definitions of the used control variables and detailed information on the data sources in Section B.1. In Section B.2 we describe the harmonization of the county-level data to the administrative territorial structure and county border definitions as of October 1990.

B.1 Descriptive statistics and data sources

Table B.1 shows descriptive statistics for outcome variables at the county level, Table B.2 for outcomes at the individual level. Table B.3 describes all variables used and lists the respective sources.

The sets of control variables listed in the result tables for both SOEP and panel regressions are defined as follows. *County size controls* include log county area and log mean population in the 1980s. *Opposition controls* account for the intensity of the uprising in 1953 and include uprising intensity (four dummy variables) as well as two dummy variables for state of emergency and Soviet military intervention. *Industry controls* include the goods value of industrial production, the share of employees in the industrial, agricultural or public sector, the county-level concentration of employment in the respective most important industrial sector, all measured in 1989, as well as a dummy variable equal to one if a strategically important industry (coal, oil, uranium, chemical, potash) was present in the county. *Transfers* are measured after 1994 and comprise federal and state transfers as well as investment subsidies paid to the counties. *Pre World War II controls* account for unemployment and self-employment in 1933, the share of protestants as of 1925, and the average election turnout as well as the average vote share of the Communist party and the Nazi party in the federal elections from 1928 to 1932. *Individual controls* include gender, age (and age squared), education (six dummy variables), learned profession (four dummy variables), household size (dummy variables), marital status (two dummy variables), and SOEP wave fixed effects.

Table B.1: Descriptive Statistics on Panel Outcomes and Controls

	Mean	SD	P25	P50	P75	Min	Max	N
Electoral turnout								
1923–1932	82.60	4.36	79.55	82.76	85.77	68.14	92.61	1,110
1990–2009	71.95	5.67	68.38	72.13	75.63	56.59	87.63	1,110
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
Patents per 100,000 inhabitants								
1928–1929	2.18	12.46	0.00	0.00	1.18	0.00	188.29	370
1993–2005	13.54	14.30	6.01	10.48	15.97	0.00	212.62	2,220
Unemployment 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	13.81	3.35	10.91	12.97	16.22	8.63	24.62	3,445
Population share aged 15–34								
1989–1989	29.43	1.62	28.25	29.61	30.42	25.52	34.14	185
1990–2009	25.30	2.63	23.53	25.52	27.29	18.72	32.93	3,445
Population share aged 35–54								
1989–1989	26.21	1.38	25.31	26.19	26.96	23.06	30.32	185
1990–2009	30.07	2.52	27.95	29.94	32.05	24.20	36.60	3,445
Population share older than 54								
1989–1989	24.64	3.00	22.84	24.53	26.59	12.92	31.73	185
1990–2009	30.82	4.12	27.99	30.82	33.59	14.41	42.37	3,445
Share of high-skilled								
1989–1989	6.54	2.72	4.78	5.72	7.27	3.24	20.71	185
1995–2009	3.98	1.84	2.90	3.60	4.30	1.90	16.80	2,775
Surveillance intensity and control variables								
Spy 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
Uprising intensity 1953	1.36	1.37	0.00	1.00	2.00	0.00	4.00	185
Dummy: State of Emergency 1953	0.48	0.50	0.00	0.00	1.00	0.00	1.00	185
Dummy: Military intervention 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
Dummy: Important industries 1989	0.23	0.42	0.00	0.00	0.00	0.00	1.00	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.3.

Table B.2: Descriptive Statistics on SOEP Outcomes

	Mean	SD	Min	Max	N
Dummy: Trust in strangers	0.14	0.35	0.00	1.00	3,389
× Born before 1940	0.18	0.38	0.00	1.00	895
× Born 1940–1961	0.14	0.34	0.00	1.00	1,867
× Born after 1961	0.11	0.31	0.00	1.00	627
× Stayed in county	0.15	0.36	0.00	1.00	2,713
× Moved	0.11	0.31	0.00	1.00	676
Negative reciprocity	9.22	4.23	3.00	21.00	3,014
× Born before 1940	8.80	4.41	3.00	21.00	754
× Born 1940–1961	9.10	4.11	3.00	21.00	1,673
× Born after 1961	10.09	4.25	3.00	21.00	587
× Stayed in county	9.26	4.20	3.00	21.00	2,443
× Moved	9.03	4.40	3.00	21.00	571
Dummy: Attend elections	0.71	0.45	0.00	1.00	3,116
× Born before 1940	0.80	0.40	0.00	1.00	789
× Born 1940–1961	0.69	0.46	0.00	1.00	1,732
× Born after 1961	0.65	0.48	0.00	1.00	595
× Stayed in county	0.69	0.46	0.00	1.00	2,484
× Moved	0.77	0.42	0.00	1.00	632
Dummy: Engagement in local politics	0.11	0.31	0.00	1.00	3,563
× Born before 1940	0.13	0.33	0.00	1.00	926
× Born 1940–1961	0.12	0.32	0.00	1.00	1,959
× Born after 1961	0.06	0.24	0.00	1.00	678
× Stayed in county	0.11	0.32	0.00	1.00	2,890
× Moved	0.09	0.29	0.00	1.00	673
Log monthly gross labor income	7.52	0.66	4.09	9.52	1,773
× Born before 1940	5.81	0.44	5.03	6.92	15
× Born 1940–1961	7.52	0.66	4.09	9.21	1,215
× Born after 1961	7.56	0.62	5.01	9.52	543
× Stayed in county	7.50	0.65	4.09	9.52	1,358
× Moved	7.57	0.72	5.01	9.21	415

Notes: This table presents descriptive statistics on SOEP outcome variables. For information on the respective years covered, see Appendix Table B.3.

Table B.3: Data Sources and Variable Construction

Variable	Years	Source
Panel A – Stasi Data (see Section 3.1)		
Spy density	1980–1988	<p>The main explanatory variable of interest, regional spy density, is calculated as the average spy density at the county level in the period 1980–1988 (see Section 3.1 for details). Data on spies, called unofficial collaborators, are based on official Stasi records published by the Agency of the Federal Commissioner for the Stasi Records (<i>Bundesbeauftragter für die Unterlagen des Staatssicherheitsdienstes der ehemaligen Deutschen Demokratischen Republik, BStU</i>) and compiled by Müller-Enbergs (2008). Population figures come from the Statistical Yearbooks of the GDR.</p> <p>Our measure of spy density covers unofficial collaborators for political-operative penetration, homeland defense, or special operations, as well as leading informers (<i>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</i>). In cases where Stasi held offices in Objects of Special Interest, the number of spies attached to these offices was added to the number of spies in the respective county.</p>
Stasi employees	1982	The number of regular Stasi employees (<i>Hauptamtliche Mitarbeiter</i>) attached to county offices in 1982 was provided by Jens Gieseke.
Panel B – Individual SOEP Data (see Section 3.2)		
Attend elections	2005, 2009	The question exploited reads as follows: “If the next election to the German ‘Bundestag’ were next Sunday, would you vote?”. Response options were given on a five-point scale to allow respondents to express different levels of conviction (not) to vote (“in no case”, “probably not”, “possibly”, “probably”, “in any case”). We construct a zero/one dummy grouping the former three and the latter two response options.
Engagement 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 at all.
Labor income	2003, 2008	Information on current monthly gross labor income is provided in every wave for East German respondents since 1992. As we aim to identify the direct relationship between surveillance, trust, and economic performance, we focus on those two waves in which both trust and wages can be observed.

continued

Table B.3 continued

Variable	Years	Source
Negative reciprocity	reci- 2005, 2010	We use three statements on <i>negative</i> 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 it is defined in the interval between 0 and 1, with higher values indicating less negative reciprocal behavior.
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. To simplify interpretation of our estimates we group the first and latter two answers.
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)		
Demographic composition	1989	The share of the population aged below 15 and the share of the population aged above 64 as of September 30, 1989 are obtained from infas (1990).
	1990–2009	Collected from the Statistical Offices of the Federal States (<i>Statistische Landesämter</i>) and the Regional Database Germany (<i>Regionaldatenbank Deutschland</i>).
Election turnout	1924–1932	We use election turnout in the federal elections in the Weimar Republic in 05/1924, 12/1924, 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.
	1990–2009	Data on regional election turnout in the federal elections in 1990, 1994, 1998, 2002, 2005 and 2009 are provided by the Federal Returning Officer (<i>Bundeswahlleiter</i>).
Industry controls	1989	Information on the goods value of production is collected from infas (1990). 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.
Opposition	1953	We use cartographic statistics published by the former West German Federal Ministry of Intra-German Relations (<i>Bundesministerium für gesamtdeutsche Fragen</i>) 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 (<i>Bundesstiftung zur Aufarbeitung der SED-Diktatur</i>), signature: EA 111 1889.

continued

Table B.3 continued

Variable	Years	Source
Patents	1928–1929	We approximate county-level patent filings in 1928 and 1929 with data on high-value patents provided by Jochen Streb. High-value patents are defined as patents with a life span of at least ten years (Streb et al., 2006).
	1993–2005	Information on post reunification patent filings come from the German Patent and Trade Mark Office (<i>Deutsches Patent- und Markenamt</i>). Yearly data are provided for 1995–2005; for 1992–1994 the aggregated number of patents is given. We assign the average number of patents to the year of 1993.
Political ideology	1928–1932	We proxy historic political ideology by the mean vote shares of the Communist party (<i>Kommunistische Partei Deutschlands, KPD</i>) and the Nazi party (<i>Nationalsozialistische Deutsche Arbeiterpartei, NSDAP</i>) 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 (<i>Statistische Jahrbücher der Deutschen Demokratischen Republik</i>).
	1990–2009	Collected from the Regional Database Germany (<i>Regionaldatenbank Deutschland</i>), the Statistical Offices of the Federal States (<i>Statistische Landesämter</i>) and the Working Group Regional Accounts (<i>Arbeitskreis Volkswirtschaftliche Gesamtrechnungen der Länder</i>).
Religion	1925	The share of protestants in the population was published in the 1925 census of the Weimar Republic (<i>Volkszählung 1925</i>). Our data stems from King et al. (2008).
Revenues	1995–2009	Data on revenues are obtained from the Regional Database Germany (<i>Regionaldatenbank Deutschland</i>). Revenues cover monetary transfers from the federal and state level (<i>allgemeine Zuweisungen und Umlagen von Bund, Land, Gemeinden/Gemeindeverbänden</i>) as well as investment subsidies granted to the counties (<i>Zuweisungen und Zuschüsse für Investitionsförderungen</i>).
Self-employment	1925, 1933	County-level self-employment rates from the 1925 and 1933 censuses of the Weimar Republic (<i>Volks- und Berufszählung 1925 und 1933</i>). 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 and should be considered as an approximation of the self-employment rate.
	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 (<i>Bundesinstitut für Bau-, Stadt- und Raumforschung, BBSR</i>).
Skills	1989	We calculate the share of high-skilled employees in a county using a large administrative data set from the GDR (<i>Datenspeicher Gesellschaftliches Arbeitsvermögen des Staatssekretariat für Arbeit und Löhne der DDR</i>) that covers roughly 85 % of the labor force (Dietz and Rudolph, 1990). Our measure of high-skilled employees includes all workers with high school diplomas or university degrees as only a small share of students was admitted to high schools in the GDR. The data set is available in the Federal Archives (<i>Bundesarchiv</i>), signature: DQ 3-MD/7 (see Rathje, 1996, for a documentation).
	1995–2009	The share of high-skilled relative to the working age population is available in the INKAR data base of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (<i>Bundesinstitut für Bau-, Stadt- und Raumforschung, BBSR</i>).

continued

Table B.3 continued

Variable	Years	Source
Unemployment	1933	County-level unemployment rates are based on the 1933 census of the Weimar Republic (<i>Volks- und Berufszählung 1933</i>), 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 (<i>Bundesagentur für Arbeit</i>). We calculate yearly means from this data.

B.2 Redrawn County Borders and Data Harmonization

We combine county-level data from various sources and decades in this study. Since 1925, the first year in our data set, county borders have been redrawn multiple times. To account for these territorial changes, we harmonize all county-level data to borders as of October 1990.

The Federal Institute for Research on Building, Urban Affairs and Spatial Development (*BBSR*) provides population and area weighting factors for all county border reforms from 1991 onwards to harmonize the data. We rely on population weights because population shares yield the most accurate harmonization of different border definitions with regard to our outcomes, which are mainly driven by people, not space. The outlined procedure is important as the number of East German counties was gradually reduced from 216 at the time of the German reunification to 87 in 2009 (the boroughs of East Berlin counting as one single county). Of course, this harmonization is only valid when looking at county-level aggregates and not individual data. The panel dimension of the SOEP, however, allows us to identify individuals' county of residence prior to the fall of the Berlin Wall.

Unfortunately, there are no administrative weighting factors available for the harmonization of county borders prior to reunification. However, there were only minor territorial reforms between 1953 and 1990, the period we cover with our GDR data. In ten cases, neighboring counties were merged together. In five cases, bigger cities became independent from the surrounding 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 riot 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 county borders as of 1990, considerable administrative 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 (Goodchild and Lam, 1980). We merge the corresponding shapefiles from the Weimar Republic with the shapefile for 1990 to determine weighting factors that allow to adjust the historical data to the 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.