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ABSTRACT

The Economic Costs of Mass Surveillance: Insights from Stasi Spying in East Germany*

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 spy density in the 1980s, we exploit discontinuities at state borders to show that higher levels of Stasi surveillance led to lower levels of social capital as measured by interpersonal and institutional trust in post-reunification Germany. We estimate the economic costs of spying by applying a second identification strategy that accounts for county fixed effects. We find that a higher spy density 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 state surveillance are large and persistent.

JEL Classification: H11, N34, N44, P26

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

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1 Introduction

Many countries monitor their citizens using secret surveillance systems. According to the Democracy Index 2012, published by the Economist Intelligence Unit, 37 percent of the world population lives in authoritarian states. A key feature of these regimes is the aim to control all aspects of public and private life at all times. In order to establish and maintain control over the population, large-scale surveillance systems are installed that constantly monitor societal interactions, identify and silence political opponents, and establish a system of obedience by instilling fear (Arendt, 1951).[1] The theory of social capital predicts an unambiguously negative effect of surveillance on economic performance. Spying on the population destroys interpersonal and institutional trust, i.e., social capital. As all economic transactions involve an element of trust between trading partners, government surveillance should exhibit adverse economic effects (Arrow, 1972, Putnam, 1995).

Despite the prevalence of surveillance systems around the world, there is no empirical evidence on the effect of spying on social capital and economic performance. This is most likely due to the fact that it is challenging to establish a credible research design. The empirical challenge is to find random variation in surveillance intensities while keeping other policies affecting trust and economic performance constant. The common trend requirement with regard to other policies makes cross-country settings basically inviable as isolating the effect of spying from the authoritarian policy mix seems impossible. For credible single-country research designs, two conditions have to be met: (i) there should be observable variation in surveillance density (regionally or over time) and (ii) the variation in the intensity of the treatment has to have at least a random component.

In this paper, we aim to overcome the empirical challenges and estimate the effect of state surveillance on economic outcomes by using official data on the regional number of spies in the former socialist German Democratic Republic (GDR). We argue that the surveillance system implemented by the GDR regime from 1950 to 1990 was a setting that fulfills both conditions for a valid research design. The official state security service of the GDR, the Ministry for State Security (*Ministerium für Staatssicherheit*), commonly referred to as the Stasi, administered a huge network of spies called "unofficial collaborators" (*Informelle Mitarbeiter*, IM). These spies were ordinary people, recruited to secretly collect information on any societal interaction in their daily life that could be of interest to the regime. We use the substantial regional variation in the intensity of spying across GDR counties (*Kreise*) to estimate the effect of surveillance on long-term *post-regime* outcomes of social capital and economic performance, measured in the 1990s and 2000s, after the fall of the Iron Curtain and Germany's reunification.[2]

Given that condition (i) is fulfilled, the remaining challenge for identification is to establish

[1] We acknowledge that democratic countries usually spy on their populations as well. Thus, it is obvious that 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] An earlier study by Jacob and Tyrell (2010), and a recent paper by Friehe et al. (2015), which has been conducted simultaneously to and independently of our study, present cross-sectional OLS regressions showing that Stasi spying is negatively associated with some measures of personality traits and social capital – assuming the regional spy density to be random. We demonstrate below that the number of Stasi spies in a county was to a large part driven by state level decisions and county characteristics.

exogenous variation in the intensity of spying. Although historians and scholars from related disciplines have not yet identified an obvious regional allocation pattern of spies, it is a priori unlikely that the spy allocation was purely random. While we demonstrate that endogeneity is likely to drive estimates towards zero, yielding a lower bound, we implement two different research designs to overcome doubts on our identification.

The first design exploits the specific territorial-administrative structure of the Stasi. About 25 percent of the variation in the spy density at the county level can be explained with GDR state (*Bezirk*) fixed effects. As the Stasi county offices were subordinate to the state office, different GDR states administered different average levels of spy densities across states. We use the resulting discontinuities along state borders as the source of exogenous variation and limit our analysis to all contiguous county pairs that straddle a GDR state border (see Dube et al., 2010, for an application of this identification strategy in the case of minimum wages). Hence, identification comes from different intensities of spying, induced by different state surveillance strategies, within county pairs on either side of a state border. The identifying assumption is that border pair counties are similar in all other respects. An advantageous feature of our border discontinuity setting is that many of the GDR state borders do not exist anymore as GDR states were merged into much larger federal states after reunification. Indeed, around fifty percent of the counties straddling a former GDR state border in our sample are nowadays part of the same federal state.

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 also affect the economic prosperity of a county. Using pre-treatment data from the 1920s and early 1930s, this design enables us to directly test for pre-trends in the outcome variables. Reassuringly, Stasi density has no explanatory power for social capital and economic performance *prior* to the division of Germany, which strengthens the causal interpretation of our findings. Similarly, controlling for a large set of historical pre-treatment variables that account for persistent regional differences in economic potential, political ideology and social capital does not affect the estimates of our border county pair research design qualitatively.

Overall, we find a negative and long-lasting effect of spying on both social capital and 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). In line with evidence on the shaping of trust levels (Sutter and Kocher, 2007), the negative effect on interpersonal trust is strongest for the cohort who spent their entire childhood in the GDR. Looking at institutional trust, we find that the intention to vote and engagement in local politics is significantly lower in counties with a high spy density even two decades after reunification. Using county level data, we find that election turnout has been significantly lower in higher-spying counties in federal elections from 2002 onwards.

[3] A first indication that surveillance by the Stasi still affects the lives of individuals even ten or twenty years after reunification is by observing the number of requests for disclosure of information on Stasi activity (*Bürgeranträge*) filed each year. Since the year 2000, roughly 90,000 requests have been filed each year. Unfortunately, there is no regional information on these requests, which could provide an interesting outcome.

In terms of economic performance, we find a negative significant effect of the spy density on labor income as reported in the SOEP. Using administrative county-level data, we further show that self-employment rates and the number of patents per capita are significantly lower in higher-spying counties. Moreover, post-reunification unemployment is persistently higher in counties with high surveillance levels. Our estimates imply that abolishing state surveillance would, on average, have reduced the long-term unemployment rate by 1.8 percentage points, which is equivalent to a ten percent drop given the average unemployment level in East Germany. Last, we find significantly negative effects of the spy density on population: Stasi spying appears to be an important 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.

Overall, our paper contributes to the large literature documenting a long-term positive effect of the quality of political institutions (oftentimes used as measures of social capital) on economic performance using cross-country research designs (Mauro, 1995, Knack and Keefer, 1997, Hall and Jones, 1999, Sobel, 2002, Rodrik et al., 2004, Nunn, 2008, Nunn and Wantchekon, 2011, Acemoglu et al., 2015). We add to this strand of the literature in several ways. First, we study the impact of a certain element of (lacking) democracy – state surveillance – on economic performance. Second, we do this in a within-country setting, which is close to a natural experiment and therefore allows for a clean identification of our effect. Third, by using surveillance as our source of variation and rich survey data, we are able to directly link variations in social capital (trust) to changes in economic performance. In fact, using two-stage least squares and spy density as an instrument, we are able to show that higher trust has a positive and causal effect on income. Fourth, the persistence of the adverse economic effects documents the long-term costs of eroding social capital and the transmission of trust across generations (Guiso et al., 2006, Algan and Cahuc, 2010, Tabellini, 2010, Becker et al., 2015). Lastly, our study is related to the influential study by Alesina and Fuchs-Schündeln (2007). While their study shows that ideological indoctrination in the GDR had long-term effects on individual preferences, we show that the same is true for the type of governance used to strengthen the power of the regime.

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. Section 4 introduces our research design and explains the two different identification strategies. Results are presented in Section 5, before Section 6 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 had largely disappeared by then (Wolf, 2009). 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 member state of the Warsaw pact, was founded 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 in October 1990, the GDR was a one-party dictatorship under the rule of the Socialist Unity Party (SED) and its secretaries general.

The GDR 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 offices in each capital of the fifteen states (*Bezirksdienststellen*), regional offices in most of the 226 counties (*Kreisdienststellen*) and offices in seven Objects of Special Interest, which were large and strategically important public companies (*Objektdienststellen*). Following this territorial principle, 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. We exploit this variation in surveillance intensities across states for identification (see Section 4.2).

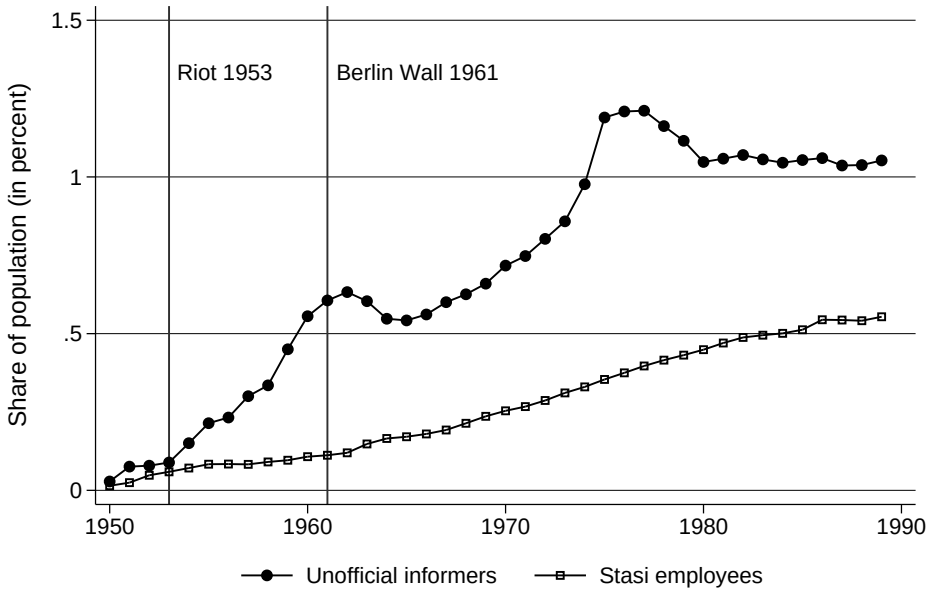
Over the four decades of its existence, the Stasi continuously expanded its competencies and duties as well as the surveillance of the population. The unforeseen national uprising on and around June 17, 1953 revealed the weakness of the secret security service in its early years and caused a subsequent 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.[6]

[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] Note that 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). Although figures on the number of spies in other communist countries during times of the Iron Curtain entail elements of uncertainty, the level of surveillance was comparable to the Soviet Union.

Figure 1: Share of Regular Stasi Employees and Unofficial Collaborators in the GDR Population



Notes: Own calculations using data on the number of unofficial collaborators 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.

The Stasi's most important tool of surveillance and suppression, and its "main weapon against the enemy"[7] was the dense network of spies called unofficial collaborators. Spies were recruited from the population and instructed to secretly collect information about individuals in their own social network. Being friends, colleagues, neighbors or sport buddies of the individuals they spied on, collaborators were able to provide valuable personal information that complemented the Stasi's knowledge of the population and helped creating an overall picture about anti-socialist and dissident movements and hence guaranteed surveillance of the society's everyday life (Gieseke, 2014, p. 163 ff.). At the same time, the threat of being denunciated and the concealed presence of the state security caused an atmosphere of mistrust and suspicion (Wolle, 2009, Gieseke, 2014).[8]

Reasons for serving as a collaborator were manifold. Some citizens agreed to cooperate due to ideological reasons, others were intrigued 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. 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. 114 ff).

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

[8] Historians argue about the degree of mistrust and suspicion caused by the regime's monitoring. E.g., Wolle (2009) characterizes the society as deeply torn, whereas Gieseke (2014) argues that the Stasi was barely able to intrude into and guide people's family life. Spies were yet oftentimes in close contact with the spied-upon person and citizens felt the Stasi's presence like a "scratching t-shirt" (Reich, 1997, p. 28). For less scientific documentations about the impact of the Stasi, see the Academy Award winning film "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.

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 border changes over time.

3.1 Spy 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 (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.

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 types of unofficial collaborators: (i) collaborators for political-operative penetration, homeland defense, or special operations as well as leading informers, (ii) collaborators providing logistics and (iii) societal collaborators, i.e., individuals publicly known as loyal to the state. We use the first category of unofficial 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 unofficial collaborators attached to these object offices to the county's number of spies.[9] 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. For further details on our main explanatory variable, see Data Appendix B.

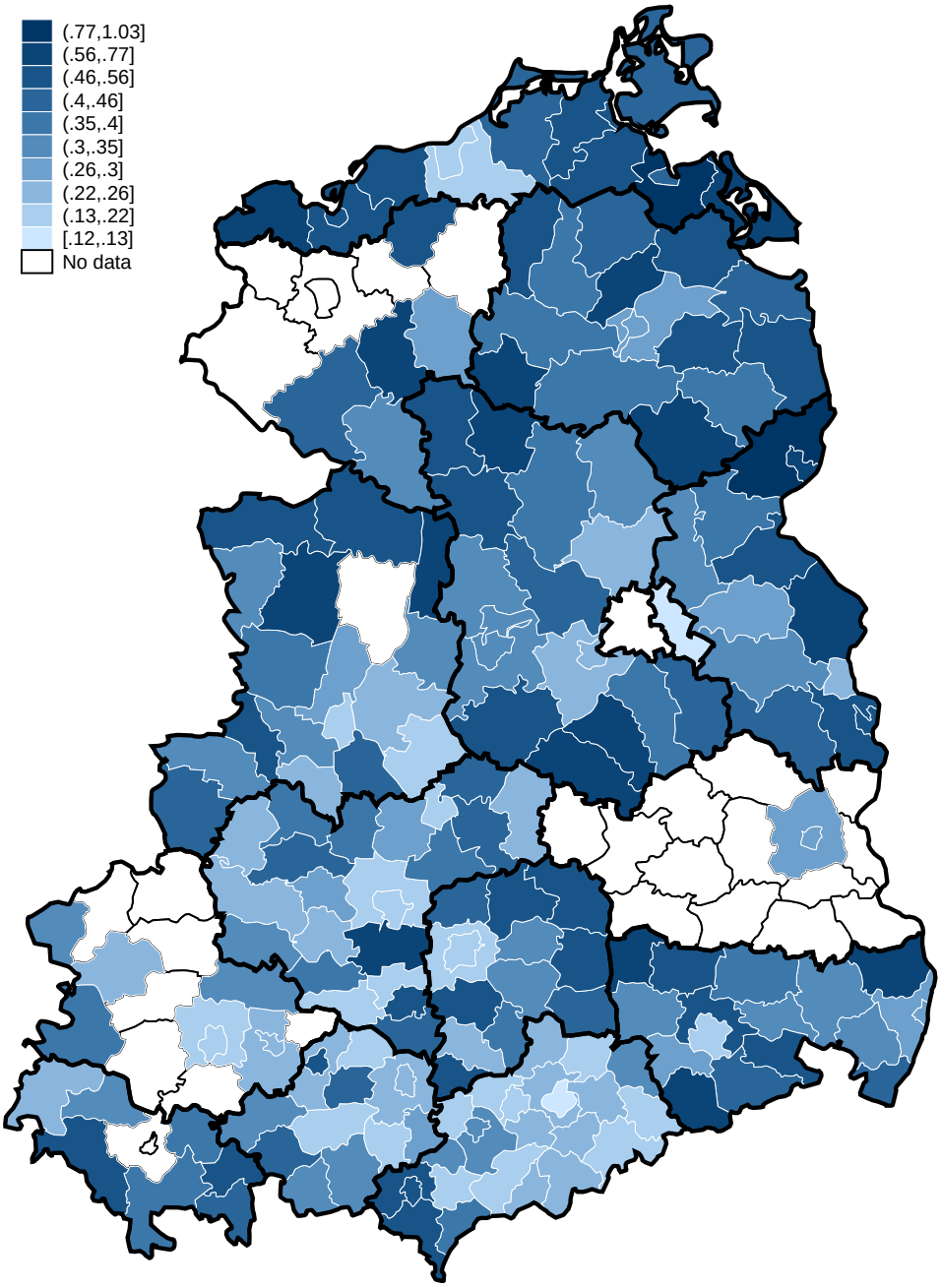
Figure 2 plots the density of unofficial collaborators for each county. Today, the number of spies is known for about ninety percent of the counties for at least one year in the 1980s. The density of spies differs considerably both across and within GDR states, with the fraction of unofficial collaborators in the population ranging from 0.12 to 1.03 percent and the mean density being 0.38 percent. The median is similar to the mean (0.36 percent), one standard deviation refers to 0.14 spies per capita.

3.2 Individual level data

For the empirical analysis presented below, we rely on two distinct datasets to estimate the effect of state surveillance on social capital and economic performance. First, we use information from the

[9] In the empirical analysis, we explicitly control for the presence of such offices in Objects of Special Interest in the county.

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



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

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. In order to determine the individuals' county of residence prior to the fall of the Berlin Wall and the end of the regime in November 1989, we limit the analysis to East German respondents in the wave of 1990 and follow these individuals over time. Information on moves after the fall of the Berlin Wall but prior to the interview as well as the county of residence in 1990 allow

us to identify the individuals' respective counties of residence prior to November 1989. By exploiting a variety of different waves of the survey we are able to observe various measures of social capital as well as current gross labor income (see Section 4.2 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 the stated preferences to participate in the most important election. We also use a measure of revealed preferences, i.e., electoral turnout, below. Second, we exploit the question whether individuals are engaged in local politics. Apart from these variables measuring social capital, we also take reported monthly gross labor income as a measure of individual economic performance. Moreover, we use the rich information of the SOEP 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.

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 social capital. We collected county-level data for two time-periods, data from the 1990s and 2000s as well as pre-World War II data. 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 that we use as control variables to check the sensitivity of our estimates. These control variables are used in both individual and county level models.

In total, we construct three sets of control variables. The first set measures the strengths 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 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 second set of controls takes into account that the Stasi tried to protect certain firms in the industrial sectors. Hence, our *industry* controls comprise (i) the share of employees in the industrial sector in 1989 and (ii) a dummy variable indicating whether a large enterprise from the uranium, coal, potash, oil or chemical industry was located in the county.

The third 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 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 Research designs

We present two research designs to identify the causal effect of spying on social capital and economic performance. First, we lay out a very simple linear model as a benchmark and discuss potential threats to identification and likely biases (Section 4.1). Based on this discussion, we propose two empirical approaches intended to overcome endogeneity problems (Sections 4.2 and 4.3).

4.1 Linear model

To identify the long-term effects of surveillance, we regress various measures of social capital and economic performance on our measure of surveillance intensity. The simplest model takes the following form

$$Y_j = \alpha + \beta SPYDENS_c + \boldsymbol{V}_j'\boldsymbol{\xi} + \varepsilon_j, \tag{1}$$

where Y_j measures an outcome that may either vary at the individual or county level, $j \in [i, c]$. Our main regressor is the spy density in county c, defined as the average number of spies per capita in each county of the GDR in the 1980s.[10] Vector $\boldsymbol{V}_j$ may contain control variables. In this simple model, identification comes from cross-sectional variation in the intensity of surveillance across GDR counties (see Jacob and Tyrell, 2010, and Friehe et al., 2015, for empirical applications of this model). Two main threats to identification are obvious: (i) selection out of treatment and (ii) omitted variable bias. We discuss these concerns in turn.

Selection out of treatment. If people moved away from counties with a high spying density, we would face a selection problem that could bias our estimates. However, the authoritarian regime controlled and limited external and internal migration in a very strict way, making residential sorting a secondary concern.

First, leaving the East German territory without permission was illegal throughout the existence of the GDR. Refugees could be sentenced to lengthy terms of imprisonment. However, about three million citizens had escaped to West Germany up until the early 1960s, which was the main reason for the construction of the Berlin Wall and the expansion of border fortifications in August 1961. Consequently, the large-scale installation of land-mines at the borderland and the regime's order for soldiers to shoot at refugees trying to pass the border led to a sharp drop in the number of refugees. The regime also often punished those individuals who applied for emigration visas, exposing people

[10] Results do not change when using the density of specific years as a regressor.

to considerable harassment in working and private life (Kowalczuk, 2009). Between 1962 and 1988, around 18,000 individuals (0.1 percent of the population) managed to leave East Germany each year, either by authorized migration (*Übersiedler*) or illegal escape (see Panel (a) in Figure A.1 in the Appendix). The share of refugees on the total number of migrants was around one third and thus even smaller.

Second, due to considerable housing shortages, residential mobility within the GDR was highly restricted. All living space was administered by the GDR authorities: In every municipality, a local housing agency (*Amt für Wohnungswesen*) decided on the allocation of all houses and flats, whether privately, cooperatively or publicly owned. Every individual looking for a new apartment had to file an application at the local housing agency. Processing times often lasted several years and assignment to a new flat was usually subject to economic, political or social interests of the regime (Grashoff, 2011, p. 13f.). From 1975 to 1988, the average number of yearly applications was 755,000, constituting around 4.5 applications per 100 citizens (Steiner, 2006).[11] Panel (b) of Figure A.1 in the Appendix shows the extent of residential mobility in the GDR. Mobility of East German citizens had been considerably lower compared to mobility in West Germany. Having data on county population and the number of spies in multiple years in the 1980s, we can directly test whether the spy density affected the population. 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.

Confounding variables. The second, more serious threat to identification are regional confounders that have affected the allocation of Stasi spies in the 1980s and that affect our outcomes of interest after the fall of the Iron Curtain. Astonishingly, there is very little knowledge on what determined the regional spy density. There is some anecdotal evidence that the Stasi was particularly active in regions with strategically important industry clusters. In contrast, and a bit surprisingly, previous research could not establish a clear correlation between the size of the Stasi and the size of the opposition at the county level (Gieseke, 1995, p. 190).

Before looking at the effects of spying on social capital and economic outcomes, we try to explain the regional variation in the spy density, which will be our treatment variable later on. We run a simple OLS regression of the spy density on four sets of potential explanatory variables and check the explanatory power of the model as indicated by the R^2 measure. Table 1 shows the results, while full regression outputs are shown in Appendix Table A.1. 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 "Objects of Special Interest", that is, a large public company of strategic importance, was located in the county.[12] In the next specification, we add dummy variables for the fifteen GDR states. The R^2 measure in column (2) shows that around 25 percent of the county-level variation can be explained by differences across GDR states. This is suggestive evidence in line with the claim of historians that county offices

[11] Some citizens tried to elude the governmental allocation by illegal and unseen movements into dilapidated flats. There are no official records about the actual number of illegal squatters. Estimates for the city of Rostock show that the share of squatters within the population was small, amounting to 0.28 percent in early 1990 (Grashoff, 2011, p. 76).

[12] 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 objects to the number of spies in the respective county offices, we control for "Objects of Special Interest" with a dummy variable in all regressions below.

responded to higher-ranked state offices and that decisions made at the state level indeed affected county-level outcomes. We will exploit this feature in our border discontinuity design presented in Section 4.2.

Table 1: The Allocation of Stasi Spies

	(1)	(2)	(3)	(4)	(5)	(6)
GDR state FE	No	Yes	Yes	Yes	Yes	Yes
County size controls	No	No	Yes	Yes	Yes	Yes
Opposition controls	No	No	No	Yes	Yes	Yes
Industry controls	No	No	No	No	Yes	Yes
Pre World War II controls	No	No	No	No	No	Yes
Observations	187	187	187	187	187	187
R^2	0.033	0.298	0.529	0.540	0.545	0.561
Adjusted R^2	0.028	0.237	0.481	0.475	0.474	0.473

Notes: This table demonstrates the power of different sets of county-level control variables in explaining the county spy density using a simple OLS regression. Every specification includes a constant and a dummy for Objects of Special Interest (*Objektdienststellen*). Full regression results are shown in Appendix Table A.1.

In the third specification of Table 1, we add variables controlling for the *size* of the 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 a regressors. We find that controlling for size – and in particular population – increases the explanatory power substantially, raising the R^2 to 0.53. Moreover, results show that the spy density is decreasing in the population (cf. Appendix Table A.1), which could be rationalized with an economies of scale argument. Overall, column (3) suggests that it might be important to control for county size when identifying the effect of the Stasi on our outcomes. We test this assertion below.

In columns (4) to (6) we sequentially add opposition, industrial and pre World War II controls (see Section 3.3). In total, neither of the three sets of control variables adds much to the explanatory power of the model. Nevertheless, we will test the sensitivity of all our results to the inclusion of the control variable sets for both research designs.

Unobserved confounders and potential bias. While controlling for observable potential confounders may demonstrate the robustness of our estimates, it is impossible to prove that there are no unobservable variables biasing our results. However, we can try to assess the direction of the potential bias. Let us assume that there is a systematic confounding variable Z_c, such as capitalist spirit or strive for freedom, that varies at the county level. Given that measures of liberal attitudes are usually positively correlated with social capital and economic performance in democratic countries, it is likely that an unobserved confounder with a positive (negative) correlation with the spy density also has a positive (negative) correlation with our outcomes. With this claim in mind, we study the potential endogeneity bias more formally. We rewrite the error term of equation (1) as $\varepsilon_j = \gamma Z_c + \eta_j$, with γ being the effect of the unobserved capitalist spirit on Y_j and η_j being noise. In such a case, the

OLS estimate would be given by:

$$\begin{aligned}\beta^{OLS} &= \frac{Cov(SPYDENS_c, \varepsilon_j)}{Var(SPYDENS_c)} \\ &= \beta + \gamma \frac{Cov(SPYDENS_c, Z_c)}{Var(SPYDENS_c)} + \underbrace{\frac{Cov(SPYDENS_c, \eta_j)}{Var(SPYDENS_c)}}_{=0}.\end{aligned}$$

If, as argued above, the effect of capitalist spirit on the outcome γ and the covariance between capitalist spirit and the spying density $Cov(SPYDENS_c, Z_c)$ have the same sign, and if, as suggested by the theory of social capital, $\beta < 0$, the estimate β^{OLS} will be biased towards zero and underestimate the negative effect of spying on our outcomes.

In the following subsections, we present two research designs which are intended to better account for unobserved confounders and limit the potential endogeneity bias.

4.2 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 (2)). As the Stasi county offices are subordinate to the state office, different GDR states administered different average levels of spy densities across states. We use the resulting discontinuities along state borders as a source of exogenous variation. We follow Dube et al. (2010) and limit our analysis to all contiguous counties that straddle a GDR state border, thus identifying the effect of spy density on our outcome variables by comparing county pairs on either side of a state border.[13] 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. While such an assumption can be quite strong in similar border research designs, it might be less critical in our case given that we focus on post-GDR outcomes and many GDR state borders do not exist anymore. In fact, after reunification the fifteen GDR states merged into six federal states, and around half of the counties straddling a GDR border in our sample belong to the same federal state in post-reunification Germany.

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

$$Y_{icb} = \alpha + \beta SPYDENS_c + \boldsymbol{X_i'\delta} + \boldsymbol{K_c'\phi} + \nu_b + \varepsilon_{icb}. \tag{2}$$

As outcome variable, Y_{icb}, we use trust in strangers, extent of negative reciprocity, intention to vote in elections, engagement in local politics and individual gross income (see Section 3.2).

The identifying assumption in the border discontinuity design is that counties on either side of a border differ systematically in their spy density since they belonged to different GDR states. Apart from that, there should be no systematic difference between the counties straddling a former state

[13] If a county has several direct neighbors on the other side of the state border, we duplicate the observation. See below for a discussion.

border. However, there might be *persistent* compositional or historical differences within county-border pairs which affected the spy allocation in the 1980s as well as the post-reunification outcomes. For that reason, we add two sets of control variables as a sensitivity check. First, vector $\boldsymbol{X_i}$ accounts for compositional differences in the population and includes individual information provided by the SOEP on age, gender, marital status, education and learned profession. Second, vector $\boldsymbol{K_c}$ controls for potential county-level differences within a border pair. It is important to understand that in order to invalidate our research design, these differences must (i) have influenced the spy allocation in the 1980s and (ii) affect outcome variables after reunification, making these factors *time-persistent* per definition. As a consequence, we include the county size, opposition, industry and pre-World War II controls that we use above to explain the variation in spy density (cf. Table 1). In addition, we add a dummy variable indicating whether an Object of Special Interest was present in the county since we add the spies attached to this object to the county-level spies (see Sections 3.1 and 4.1).

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 through the number of duplications in our baseline specification and cluster standard errors at the border 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.2) prove to be robust to these modifications.

Table 2 provides a test of the validity of our research design by checking whether counties straddling a state border are indeed similar. Based on the GDR state average spy density, we assign one county in a border pair to either the higher- or the lower-spy state side. Table 2 shows the differences between higher and lower-spying side counties in terms of the spy density and all other control variables used in regression equation (2). We also test whether the differences are statistically significant using a t-test.[14] We find that the spy density is indeed significantly higher in counties located in higher-spying GDR states. Apart from population, all other control variables seem to be well balanced between the higher and lower spy density side. The fact that the county population is slightly higher on the lower-spy side is in line with the results from Table A.1: the spy density was lower in cities. For that reason, we control for population and county size in all specifications.

4.3 Panel data design

In Section 4.1, we discussed that time-persistent confounders that have affected the spy allocation and are still affecting post-reunification outcomes are a potential threat to identification. Given that the social capital measures obtained from the SOEP 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 following Moser et al. (2014) that allows us to include county fixed effects to account for any time-invariant confounder.[15] The panel data model reads as

[14] Note that we use the same weights as in the regression.

[15] Note that many (though not all) potential confounders are likely to be time-invariant by definition, since they must

Table 2: Descriptive Statistics in the Border Pair Sample

			Mean by spy density		Difference	
	Mean	SD	Low-density	High-density	Δ	*p*-value
Spy density	0.36	0.13	0.34	0.38	-0.04	0.04
County variables						
Log mean population 1980s	11.14	0.72	11.23	11.04	0.19	0.13
Log county size	6.14	0.52	6.10	6.19	-0.10	0.28
Dummy: Object of Special Interest	0.03	0.17	0.01	0.04	-0.03	0.31
Share indust. employment 1989	45.70	12.15	45.85	45.55	0.30	0.89
Dummy: Important industries 1989	0.25	0.44	0.19	0.31	-0.12	0.11
Mean electoral turnout 1928–1932	84.10	3.64	83.69	84.52	-0.83	0.19
Mean vote share KPD 1928–1932	15.45	6.83	15.71	15.20	0.52	0.66
Mean vote share NSDAP 1928–1932	25.36	3.84	25.54	25.17	0.36	0.59
Share self-employed 1933	15.75	2.52	15.92	15.57	0.35	0.43
Share protestants 1925	91.77	3.85	91.23	92.30	-1.07	0.11
Share unemployed 1933	16.80	5.45	17.16	16.44	0.72	0.45
Uprising intensity 1953: None	0.29	0.46	0.27	0.31	-0.04	0.57
Uprising intensity 1953: Strike	0.25	0.44	0.24	0.27	-0.03	0.69
Uprising intensity 1953: Demonstration	0.13	0.34	0.12	0.15	-0.03	0.62
Uprising intensity 1953: Riot	0.25	0.43	0.31	0.18	0.13	0.07
Uprising intensity 1953: Prisoner liberation	0.07	0.26	0.06	0.09	-0.03	0.51
Dummy: State of emergency 1953	0.75	0.43	0.79	0.72	0.07	0.32
Dummy: Military intervention 1953	0.57	0.50	0.54	0.60	-0.06	0.49
Individual characteristics (in 1990)						
Male (in percent)	46.56	49.89	45.89	47.52	-1.64	0.29
Age	46.58	18.72	46.75	46.32	0.44	0.80
Household size	2.72	1.16	2.67	2.80	-0.13	0.20
Share of singles	21.08	40.79	22.66	18.80	3.86	0.23
Share of married	59.29	49.13	56.92	62.71	-5.79	0.11
Other marital status	19.63	39.72	20.42	18.49	1.94	0.45
Share of low-skilled	45.45	49.80	42.92	49.11	-6.19	0.33
Share of medium-skilled	34.42	47.52	34.34	34.52	-0.18	0.94
Share of high-skilled	20.13	40.10	22.74	16.37	6.36	0.20
Learned profession: Blue-collar worker	51.50	49.98	49.41	54.51	-5.10	0.16
Learned profession: Self-employed	2.62	15.98	3.24	1.73	1.51	0.15
Learned profession: White-collar worker	23.51	42.41	25.08	21.23	3.85	0.35
Learned profession: Civil servant	0.25	5.01	0.09	0.49	-0.40	0.25
Learned profession: Other/unknown	22.12	41.51	22.18	22.04	0.14	0.96

Notes: The contiguous border pair sample covers 134 counties. Lower-spying and higher-spying counties are determined by means of the population-weighted GDR state average of the county-level spy density in the border pair sample. Lower-spying counties include 1,131 individuals, higher-spying counties 748 individuals. Descriptive statistics on individual characteristics are based on the 1990 wave of the SOEP data and calculated using cross-sectional weights, adjusted for duplications of counties that are part of multiple border pairs. The corresponding *p*-values are based on OLS regressions of individual characteristics on an indicator variable for lower-/higher spy density counties, clustering standard errors at the county and person level. For information on all variables, see Appendix Table B.3.

follows:

$$Y_{ct} = \alpha + \sum_t \beta_t SPYDENS_c \times \tau_t + \boldsymbol{L}'_{\boldsymbol{ct}}\boldsymbol{\zeta} + \rho_c + \tau_t + \varepsilon_{ct}. \quad (3)$$

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).

have affected the spy allocation in the 1980s and outcomes measure in the 1990s and 2000s.

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 $\beta_t, \forall t < 1989$ 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 fall of the Iron Curtain, 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.[16]

Year fixed effects τ_t account for trends in outcome variables over time. In our preferred specification, we even allow for heterogeneous and flexible trends by region (see below). County fixed effects ρ_c account for persistent confounding variables such as geographic location or regional liberalism. Note that identification in this panel model is somewhat more subtle than in the standard case since the Stasi density is constant across the panel and identification cannot be within-county as a consequence. Instead, the model is identified by exploiting cross-sectional variation in post-treatment adjustment paths. The interactions of the spy density with the year dummies thus capture the potential relationship between state surveillance in the 1980s and different adjustment paths after reunification relative to the initial base levels prior to the treatment.

Although we account for county fixed effects, we test the robustness of our results and include several sets of control variables, which are captured in $\boldsymbol{L_{ct}}$. In a first specification, and as done above, we control for the presence of an Object of Special Interest in county c by interacting a dummy variable with year dummies after the treatment ($t \geq 1989$). Second, we account for both county size and regional trends. Clearly, rural and urban jurisdiction are likely to show different economic developments in the 1990s and 2000s independent of the Stasi density. The same is true for certain regions. Given that both population and GDR states explain up to 50 percent of the spy density variation (cf. Table 1), it is crucial to account for both regional and county-size trends. Concretely, we add GDR state times year fixed effects to the model[17] as well as size controls (log mean population in the 1980s and log county area) interacted with a dummy indicating the post-treatment period. In our richest and preferred specification, we also add the opposition and industry controls used in Table (cf. Table 1)—each of them interacted with a post-treatment dummy. Lastly, we apply two other sensitivity checks: First, we add current population to the model, given that it is a potential outcome that might affect regional adjustment paths. Second, we control for federal and state transfers as well as investment subsidies paid to East German counties after reunification.

5 Results

In the following section, we present the empirical results. First, we focus on the effect of the spy density on measures of interpersonal and institutional trust (Section 5.1). In Section 5.2, we investigate how governmental surveillance affects economic performance. Last, we test the theoretical

[16] 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.

[17] For the pre-war periods, we use German states and Prussian provinces from the time of the Weimar Republic.

mechanism between government surveillance, social capital, and economic performance using the spy density as an instrument for trust (see Section 5.3).

5.1 Effects of surveillance on social capital

We apply the border discontinuity design (see equation (2)) to identify the effect of spying on measures of interpersonal and institutional trust. For each outcome, we estimate three specifications of the model: (i) only controlling for border pair fixed effects, (ii) adding individual characteristics to pick up compositional differences in the population, and (iii) additionally including county-level controls to capture differences in county size, oppositional strength, industry composition and persistent political ideology and economic performance (as captured by the pre World War II controls).

Panel A of Table 3 presents the results for our measures of interpersonal trust. We find that spying significantly affects both of our outcomes, trust in strangers and negative reciprocity. Results are significant in our leanest specification and also conditional on individual- and county-level controls, the latter specification being our preferred one.[18] For a one standard deviation increase in the spy density (see Table 2), the estimate in column (3) implies that the probability to trust would be around four percentage points lower, which is a large effect given that the average probability is fourteen percent. When focusing on reciprocity, we find that a one standard deviation increase in the spy density raises negative reciprocity by 0.7 points, the mean level of reciprocity being 9.2 points.

In Panel B of Table 3, we test for heterogeneous effects by age. We interact our main regressor with cohort dummies for individuals born (i) before 1940, (ii) between 1940 and 1961, and (iii) after 1961. Psychological and economic research has shown that trust is shaped during adolescence and does not change much after the age of 21 (Sutter and Kocher, 2007). With the Berlin Wall being built in 1961, this implies that the youngest cohort in our analysis was fully socialized in the GDR and should have been influenced most by Stasi spying. The second cohort, born between 1940 and 1960, was predominantly socialized in the immediate aftermath of World War II and during the first years of the GDR. In contrast to the youngest cohort, these respondents (or their families) had the chance to move out of the GDR prior to the construction of the Berlin Wall in 1961. Lastly, people born before 1940 experienced World War II and reached adulthood prior to 1961. Interestingly, and in line with our expectations, we find that the negative effect of spying on trust is strongest – and statistically significant – for the youngest cohort. When focusing on negative reciprocity, we find the medium cohort to be most affected, although point estimates for all three cohorts are not statistically different from each other. In Panel C, we split the sample by individuals' moving decision after reunification. We will discuss these effects in Section 5.2, when looking at the population effect of state surveillance.

Next, we turn to institutional trust with Table 4 providing the results. We find a significant negative effect of the spy density on the intention to vote in elections throughout all specifications. This effect is driven by the medium cohort. On average, a one standard deviation increase in the intensity of

[18] Given that trust in strangers is a binary outcome and negative reciprocity is measured on a 21 point scale, we estimate equation (2) using Ordinary Least Squares to ease interpretation. We find similar results when estimating a binary probit model for trust and an ordered probit model for reciprocity.

Table 3: Effect of Spying on Interpersonal Trust

	Trust in strangers			Negative reciprocity		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Baseline effects						
Spy density	-0.293**	-0.279*	-0.319*	5.120***	4.912***	5.283***
	(0.141)	(0.154)	(0.184)	(1.803)	(1.698)	(1.747)
Adjusted-R^2	0.061	0.090	0.106	0.063	0.130	0.142
Panel B – Heterogeneous effects by age						
Spy density × Born before 1940	-0.241	-0.191	-0.254	4.286*	3.421	4.424*
	(0.191)	(0.219)	(0.241)	(2.346)	(2.550)	(2.288)
Spy density × Born 1940–1961	-0.245	-0.237	-0.285	6.530***	5.812***	6.444***
	(0.172)	(0.176)	(0.205)	(2.054)	(2.028)	(2.167)
Spy density × Born after 1961	-0.604**	-0.631***	-0.604***	4.201	3.527	4.008
	(0.255)	(0.226)	(0.222)	(3.241)	(3.205)	(3.549)
Adjusted-R^2	0.068	0.094	0.108	0.082	0.134	0.146
Panel C – Heterogeneous effects by mobility						
Spy density × Stayed in county	-0.321**	-0.318**	-0.338*	5.325***	4.865***	5.311***
	(0.137)	(0.153)	(0.188)	(1.781)	(1.734)	(1.811)
Spy density × Moved	-0.197	-0.125	-0.189	3.839	4.866	4.968
	(0.236)	(0.229)	(0.237)	(4.101)	(3.668)	(3.655)
Adjusted-R^2	0.063	0.092	0.108	0.063	0.129	0.142
Individual controls		Yes	Yes		Yes	Yes
County size controls			Yes			Yes
Opposition controls			Yes			Yes
Pre World War II controls			Yes			Yes
Industry controls			Yes			Yes
Number of observations	3,389	3,389	3,389	3,014	3,014	3,014

Notes: This table shows the β coefficients of the border pair model laid out in equation (2) using SOEP data. All specifications include border pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the border pair and the individual level with usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to border pairs for which we observe individuals in both counties along the border. All specifications use cross-sectional weights adjusted for duplicates of counties that are part of multiple border pairs. For detailed information on the control variables, see Data Appendix B.

spying leads to a decrease in the *intention* to attend elections of seven percentage points. In contrast, when looking at engagement in local politics, the young and the old cohorts seem to be negatively affected, while the overall average effect is negative but statistically insignificant.

While the intention to vote is a soft measure of institutional trust capturing stated preferences, we can use administrative data on electoral turnout to check whether intentions actually translate into real political participation. Given that county-level data on voter turnout are available since the 1920s, we apply our panel data model (see equation (3)), which allows us to control for time-invariant political preferences and historical differences in social capital by adding county fixed effects.

Figure 3 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

Table 4: Effect of Spying on Institutional Trust

	Attend elections			Engagement in local politics		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Baseline effects						
Spy density	-0.434*	-0.335*	-0.537**	-0.040	-0.027	-0.195
	(0.222)	(0.186)	(0.252)	(0.131)	(0.116)	(0.123)
Adjusted-R^2	0.053	0.137	0.146	0.020	0.125	0.134
Panel B – Heterogeneous effects by age						
Spy density × Born before 1940	-0.268	-0.084	-0.292	-0.385**	-0.246	-0.424***
	(0.243)	(0.209)	(0.264)	(0.160)	(0.155)	(0.158)
Spy density × Born 1940–1961	-0.622***	-0.597**	-0.800***	0.135	0.140	-0.016
	(0.239)	(0.240)	(0.287)	(0.151)	(0.131)	(0.137)
Spy density × Born after 1961	-0.163	-0.023	-0.271	-0.022	-0.092	-0.259*
	(0.313)	(0.298)	(0.336)	(0.163)	(0.142)	(0.147)
Adjusted-R^2	0.079	0.145	0.153	0.035	0.132	0.141
Panel C – Heterogeneous effects by mobility						
Spy density × Stayed in county	-0.404*	-0.314	-0.529**	-0.057	-0.051	-0.227*
	(0.229)	(0.193)	(0.247)	(0.134)	(0.117)	(0.125)
Spy density × Moved	-0.572*	-0.339	-0.508	0.043	0.103	-0.002
	(0.334)	(0.311)	(0.375)	(0.169)	(0.167)	(0.161)
Adjusted-R^2	0.053	0.138	0.149	0.020	0.126	0.135
Individual controls		Yes	Yes		Yes	Yes
County size controls			Yes			Yes
Opposition controls			Yes			Yes
Pre World War II controls			Yes			Yes
Industry controls			Yes			Yes
Number of observations	3,116	3,116	3,116	3,563	3,563	3,563

Notes: This table shows the β coefficients of the border pair model laid out in equation (2) using SOEP data. All specifications include border pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the border pair and the individual level with usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to border pairs for which we observe individuals in both counties along the border. All specifications use cross-sectional weights adjusted for duplicates of counties that are part of multiple border pairs. For detailed information on the control variables, see Data Appendix B.

Appendix presents the corresponding regression results and shows that we find similar effects for leaner specifications as soon as we control for different trends in 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. In the 2000s, voter turnout is about 4.8 percentage points lower relative to low-spying counties. For a one standard deviation increase in the spy density, average electoral turnout would be about 0.7 percentage points lower.

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 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

Figure 3: Effect of Spying 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 (3). 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 (6) in Table A.3 for details.

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.

5.2 Effects of surveillance on economic performance

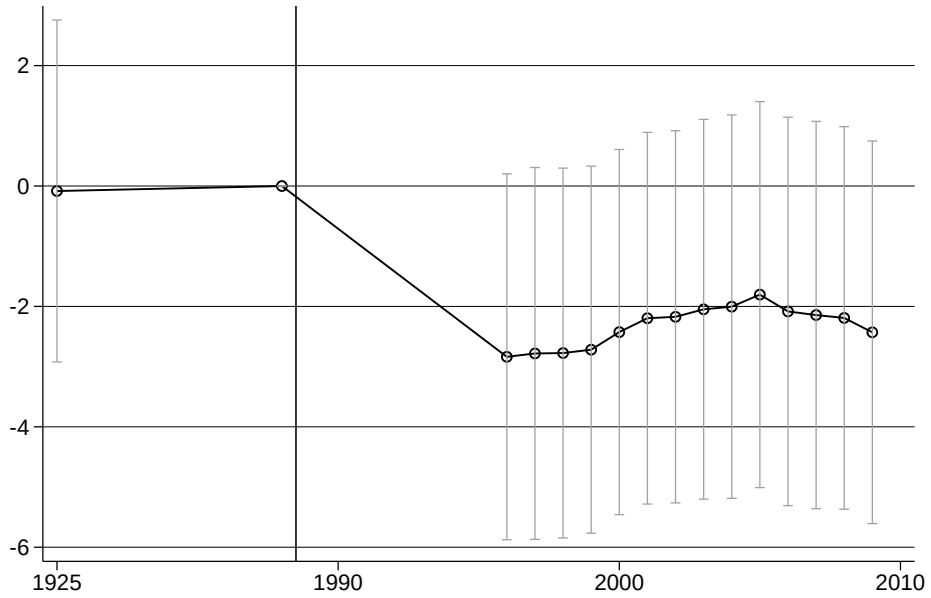
Theoretically, we expect government surveillance to deteriorate social capital, which in turn leads to lower economic performance. While we have demonstrated the first part of this mechanism in the previous section, we now turn to the economic effects of state surveillance. First, we look at the direct effect of spying on economic outcomes, hence we estimate reduced form effects.

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 4 and 5 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 (at the ten percent level) the higher the county's spy density. This negative effect is quite persistent and varies around -2.5 percentage points.[19] This estimate implies that for a one standard deviation

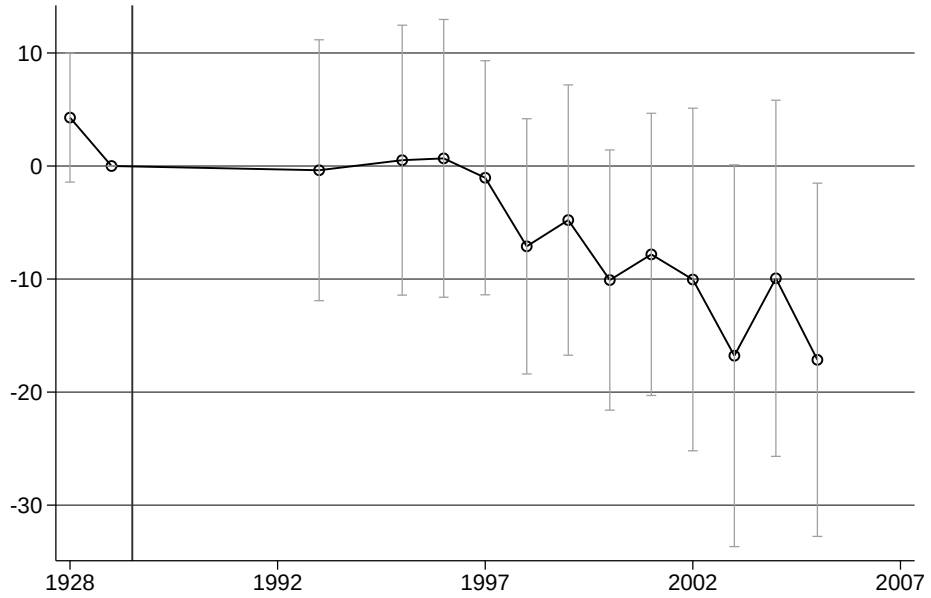
[19] However, as shown in Appendix Table A.4, we lose precision when including county size controls.

Figure 4: Effect of Spying 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 (3). 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 (6) in Table A.4 for details.

Figure 5: Effect of Spying 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 (3). 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 (6) in Table A.5 for details.

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

When looking at patents in Figure 5, 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 is around -17, which implies that a one standard deviation decrease in the intensity of spying would, on average, lead to 2.4 patents more per 100,000 inhabitants, which is an increase of about twenty 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 6: Effect of Spying 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 (3). 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 (6) in Table A.6 for details.

Figures 6 and 8 as well as Appendix Tables A.6 and A.7 show the results. Figure 6 shows that unemployment is indeed higher in counties with a high spy density. The effect is persistent and oscillates around 4.7 percentage points. A one standard deviation increase in the spy density leads to an increase in the unemployment rate of 0.7 points. Unfortunately, there is only one reliable pre-treatment observation for the unemployment rate. While we can still identify the effect of spying in our panel research design, we cannot check for pre-trends in unemployment.

Next, we investigate the effect of state surveillance on county population. Average yearly and cumulated county-level population growth since the mid 1980s are depicted in Figure 7. The graph shows two emigration waves after the fall of the Iron Curtain – a severe and temporary one immediately after reunification (between 1989 and 1992) and a moderate and persistent 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 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.[20] First, population in higher spying counties sharply drops in the first post-treatment year 1989.[21] This implies that the initial emigration wave was significantly driven by people leaving high-spy counties. For a one standard deviation decrease in the spy density, the population would be 0.9 percent higher. Given that the average population loss in 1989 was 1.5 percent, this is a substantial effect.

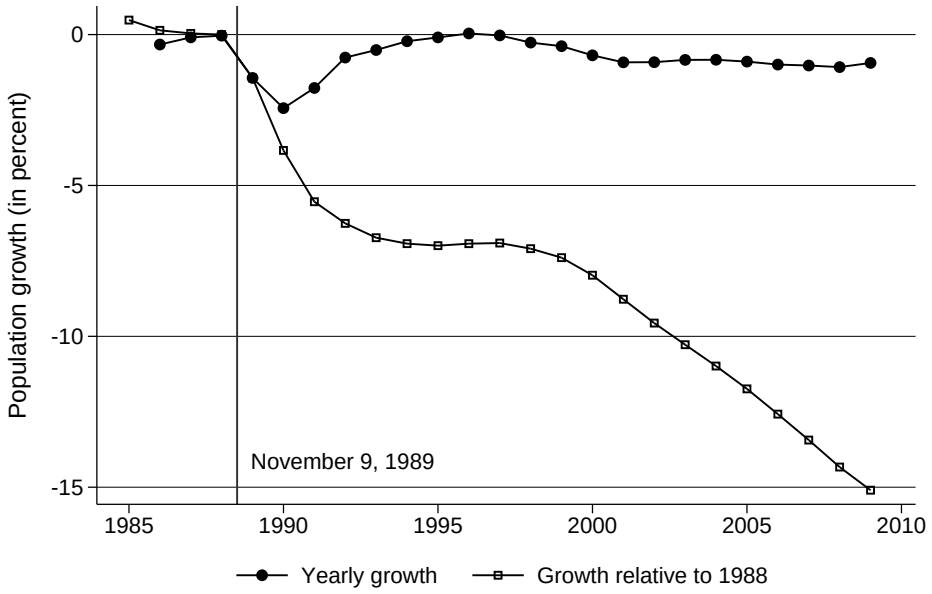
Further note that the effect of spying is flat after 1989. From 1990 to 1997, we do not see a significantly different population effect between high and low spy counties in addition to the initial population outflow. In other words, the population response driven by spying was immediate. In 1998, i.e., the first year 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 overall population loss in 2009 for East German counties was fifteen percent (relative to 1988, see Figure 7), we use back-of-the-envelope calculations to assess how much of this decline can be attributed to spying. Given that the mean spy density is 0.38, the point estimate for the year 2009 of -0.169 implies that the population would, on average, be 6.6 percent higher in the absence of any spying. Hence, about forty percent of the overall decline can be explained by people moving away from former high-spying counties.

The strong population effect of spying gives rise to the question of how much of our effects on social capital and other economic outcomes are driven by selection out of high-spying counties. For the panel estimates, we show that results for all outcomes are robust to the inclusion of the current population as a control variable, acknowledging that this only accounts for the population drop but not for potential differences in the composition of emigrants. Moreover, we can re-assess the timing of the effects, bearing in mind that the first wave of migrants was rather negatively selected in terms of education, while the second wave was positively selected (Fuchs-Schündeln and Schündeln, 2009). For unemployment and self-employed, we find the strongest negative effects in 1996 and 1997, hence prior to the second migration wave. Given that the stayers were positively selected in the first wave, it is possible that the true effect of spying is even more negative than estimated. In terms of patents, it is interesting to see that the decline actually starts with the beginning of the second migration

[20] 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.

[21] 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: Average County-Level Population Growth in East Germany



Notes: The graph shows yearly and cumulative average population growth for East German counties from 1985 to 2009. Cumulative growth is measured relative to the year 1988.

Figure 8: Effect of Spying on Log Population

Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (3). 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 (6) in Table A.7 for details.

wave. Hence, it is possible that the effect of spying on patents is of second-order and triggered by the emigration of young and highly educated people.

In terms of social capital, we can go a bit further in assessing the potential selection effect. First, note that Table 2 indicates that the initial level in terms of education and learned occupation was not statistically different between higher and lower spy density counties. Second, we largely estimate the intention-to-treat effect by assigning individuals the spy density of the county they lived in during the GDR. Unfortunately, we cannot observe individuals who moved to the West in the period from 1989 to June 1990, the month when the 1990 survey was conducted. Given the immediate population response in higher spying counties in 1989, it seems fair to assume that people who moved immediately after the fall of the Wall (or even before) were particularly affected by spying. Hence, we expect our intention to treat effects of spying on social capital to be slightly underestimated.

As a last robustness check, we interact the spy density with a dummy variable indicating whether that individual moved out of the 1989 county of residence; see Panels C of Tables 3 and 4. Our results show no significantly different effects between movers and stayers.[22] This suggests that the compositions of movers is not different from stayers in terms of social capital and our findings are not driven by selection of movers.

Table 5: Effect of Spying on Monthly Gross Labor Income

	Reduced form			2SLS	
	(1)	(2)	(3)	(4)	(5)
Dependent variable	Income	Income	Income	Trust	Income
Spy density	-1.043*	-0.776*	-0.915**	-0.744***	
	(0.560)	(0.423)	(0.416)	(0.245)	
Trust in strangers					1.354*
					(0.725)
Individual controls		Yes	Yes	Yes	Yes
County size controls			Yes	Yes	Yes
Opposition controls			Yes	Yes	Yes
Pre World War II controls			Yes	Yes	Yes
Industry controls			Yes	Yes	Yes
Number of observations	1,773	1,773	1,773	1,743	1,743
Adjusted-R^2	0.084	0.313	0.341	0.134	
F-Test				9.237	

Notes: This table shows the β coefficients of the border pair model laid out in equation (2) using SOEP data. All specifications include border pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the border pair and the individual level with usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to border pairs for which we observe individuals in both counties along the border. All specifications use cross-sectional weights adjusted for duplicates of counties that are part of multiple border pairs. For detailed information on the control variables, see Data Appendix B.

5.3 Linking surveillance, social capital and economic performance

In the previous two sections, we provided evidence of negative effects of spying on social capital and economic performance. In a last step, we aim at documenting the theoretical mechanism between government surveillance, social capital, and economic performance. We use gross labor income reported in the SOEP as our measure of economic performance. First, we estimate the reduced

[22] Given that the group of movers is much smaller, we do not obtain statistically significant effects for them.

form effect of spying on income (see Table 5, columns (1) to (3)). As expected, we find a negative significant effect of the spy density on log gross income of -0.915. The effect implies that a one standard deviation increase in the spy density leads to a gross income loss of twelve percent. To test the suggested channel with surveillance affecting trust, and trust affecting income, we run a two-stage least squares regression of income on trust using the spy density as an instrument. Column (4) of Table 5 shows the first stage result of the regression.[23] The F-test of the first-stage regression is 9.2, which suggests that the instrument has reasonable power. The second stage results are presented in column (5). Using the change in trust induced by a one standard deviation *decrease* in surveillance, the first stage implies a ten percent increase in the probability to trust strangers, which in turn increases gross income by 15.1 percent.

6 Conclusion

In this paper, we estimate the effect of state mass surveillance on social capital 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, or Stasi. For the second research design, we set up a long-term panel with pre World War II measures of social capital and economic performance. This allows us to control for county fixed effects and identify the effect of cross-sectional spy density variation through different adjustments paths after the breakdown of the regime and the reunification. Moreover, we are able to inspect potential pre-treatment trends in outcome variables, which would invalidate our identifying assumption.

The results of our analysis show that more intense state surveillance had negative and long-lasting effects on both social capital and economic performance. Our estimates imply that an abolishment of all spying activities would have led to an increase in electoral turnout of 1.8 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 1.8 percentage points lower, which is equivalent to a ten percent drop compared to the average in East Germany. We also find that Stasi spying can explain a large part of the decline in population levels in East Germany. Hence, we show that the former East German regime did not only have a long-lasting impact on political preferences (Alesina and Fuchs-Schündeln, 2007), but it also eroded institutional and interpersonal trust, which in turn has long-lasting negative effects on the society and the economy.

[23] As found in Table 3, spying has a significantly negative effect on trust. Note that the point estimate is twice as high and more significant despite the smaller sample. This can be explained by the fact the 2SLS sample is restricted to individuals with positive labor income who are, on average, younger compared to the full estimation sample. In fact, the magnitude of the first-stage estimate is comparable to the one found for the youngest cohort.

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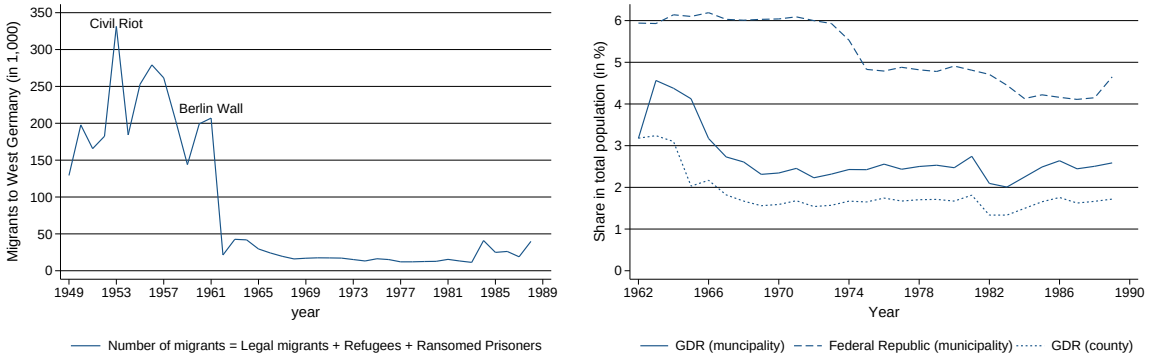
A Appendix

Figure A.1: Migration in the German Democratic Republic



(a) External Migration (b) Internal Migration

Notes: Panel (a) is based on 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. Panel (b) is based on own calculations using data from the Statistical Yearbooks of the German Democratic Republic and the Federal Statistical Office of Germany.

Table A.2: Robustness Checks

	Border pairs (OLS)					Border pairs (Probit)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Weights	Adj.	Adj.	Adj.	Cross	None	Adj.	Adj.
Panel A – Trust in strangers							
Spy density	-0.293**	-0.279*	-0.319*	-0.422**	-0.088	-1.611**	-1.689**
	(0.141)	(0.154)	(0.184)	(0.191)	(0.129)	(0.734)	(0.695)
Observations	3,389	3,389	3,389	3,389	3,389	3,389	3,389
Adjusted-R^2	0.061	0.090	0.106	0.078	0.076		
Pseudo-R^2						0.088	0.155
							continued
Individual controls		Yes	Yes	Yes	Yes		Yes
County size controls			Yes	Yes	Yes		Yes
Opposition controls			Yes	Yes	Yes		Yes
Pre World War II controls			Yes	Yes	Yes		Yes
Industry controls			Yes	Yes	Yes		Yes

Table A.2 continued

	Border pairs (OLS)					Border pairs (Probit)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Weights	Adj.	Adj.	Adj.	Cross	None	Adj.	Adj.
Panel B – Negative reciprocity							
Spy density	5.120***	4.912***	5.283***	5.877***	5.439***	1.161***	1.203***
	(1.803)	(1.698)	(1.747)	(1.878)	(1.849)	(0.376)	(0.350)
Observations	3,014	3,014	3,014	3,014	3,014	3,014	3,014
Adjusted-R^2	0.063	0.130	0.142	0.146	0.125		
Pseudo-R^2						0.015	0.034
Panel C – Attend elections							
Spy density	-0.434*	-0.335*	-0.537**	-0.578**	-0.297	-1.226**	-1.753**
	(0.222)	(0.186)	(0.252)	(0.256)	(0.209)	(0.560)	(0.697)
Observations	3,116	3,116	3,116	3,116	3,116	3,116	3,116
Adjusted-R^2	0.053	0.137	0.146	0.135	0.102		
Pseudo-R^2						0.058	0.152
Panel D – Engagement in local politics							
Spy density	-0.040	-0.027	-0.195	-0.180	-0.083	-0.203	-2.109***
	(0.131)	(0.116)	(0.123)	(0.127)	(0.141)	(0.647)	(0.603)
Observations	3,563	3,563	3,563	3,563	3,563	3,563	3,563
Adjusted-R^2	0.020	0.125	0.134	0.115	0.100		
Pseudo-R^2						0.062	0.250
Panel E – Monthly gross labor income							
Spy density	-1.043*	-0.776*	-0.915**	-0.671	-0.666*		
	(0.560)	(0.423)	(0.416)	(0.470)	(0.388)		
Observations	1,773	1,773	1,773	1,773	1,773		
Adjusted-R^2	0.084	0.313	0.341	0.325	0.266		
Pseudo-R^2							
							continued
Individual controls		Yes	Yes	Yes	Yes		Yes
County size controls			Yes	Yes	Yes		Yes
Opposition controls			Yes	Yes	Yes		Yes
Pre World War II controls			Yes	Yes	Yes		Yes
Industry controls			Yes	Yes	Yes		Yes

Table A.2 continued

	Border pairs (OLS)					Border pairs (Probit)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Weights	Adj.	Adj.	Adj.	Cross	None	Adj.	Adj.
Individual controls		Yes	Yes	Yes	Yes		Yes
County size controls			Yes	Yes	Yes		Yes
Opposition controls			Yes	Yes	Yes		Yes
Pre World War II controls			Yes	Yes	Yes		Yes
Industry controls			Yes	Yes	Yes		Yes

Notes: This table shows the β coefficients of the border pair model laid out in equation (2) using SOEP data. All specifications include border pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the border pair and the individual level in specifications (1)-(5) and one-way clustered at the border pair level only in specifications (6)-(7). We use the usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to border pairs for which we observe individuals in both counties along the border. Columns (1)-(3) and (6)-(7) present estimates using cross-sectional weights, adjusted for duplications of counties that are part of multiple border pairs. Estimates in column (4) use unadjusted cross-sectional weights, column (5) shows unweighted regression results but adjusts for duplicates. Specifications (6)-(7) present ordered probit results if negative reciprocity is the outcome variable. For detailed information on the control variables, see Data Appendix B.

Table A.1: The Allocation of Stasi Spies – Full Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)
Dummy: Object of Special Interest	0.160	0.172	0.261***	0.267***	0.265***	0.272***
	(0.124)	(0.114)	(0.067)	(0.069)	(0.070)	(0.076)
Log mean population 1980s			-0.134***	-0.144***	-0.148***	-0.135***
			(0.015)	(0.016)	(0.016)	(0.021)
Log county size			-0.006	-0.002	0.001	0.003
			(0.011)	(0.012)	(0.012)	(0.014)
Uprising intensity 1953: Strike				0.004	-0.001	-0.005
				(0.024)	(0.026)	(0.027)
Uprising intensity 1953: Demonstration				0.002	-0.006	-0.014
				(0.026)	(0.027)	(0.031)
Uprising intensity 1953: Riot				-0.030	-0.035	-0.041
				(0.037)	(0.038)	(0.038)
Uprising intensity 1953: Prisoner liberation				0.002	-0.005	-0.015
				(0.034)	(0.035)	(0.038)
Dummy: Military intervention 1953				0.037	0.037	0.045*
				(0.023)	(0.023)	(0.024)
Dummy: State of emergency 1953				-0.014	-0.009	-0.009
				(0.026)	(0.027)	(0.029)
Share indust. employment 1989					0.001	0.001
					(0.001)	(0.001)
Dummy: Important industries 1989					-0.003	-0.011
					(0.022)	(0.022)
Mean electoral turnout 1928–1932						0.001
						(0.006)
Mean vote share NSDAP 1928–1932						0.006**
						(0.003)
Mean vote share KPD 1928–1932						-0.000
						(0.003)
Share protestants 1925						-0.001
						(0.001)
Share unemployed 1933						0.001
						(0.004)
Share self-employed 1933						0.004
						(0.009)
GDR state FE	No	Yes	Yes	Yes	Yes	Yes
Observations	187	187	187	187	187	187
R^2	0.033	0.298	0.529	0.540	0.545	0.561
Adjusted R^2	0.028	0.237	0.481	0.475	0.474	0.473

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. For details on the source and construction of the variables, see Appendix Table B.3.

Table A.3: Effect of Spying on Electoral Turnout

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Spy density × 05/1924	0.327	0.327	0.327	-1.305	-1.305	-1.305	-1.343	-1.305
	(1.513)	(1.515)	(1.516)	(1.460)	(1.462)	(1.463)	(1.453)	(1.462)
Spy density × 12/1924	0.341	0.341	0.341	-0.972	-0.972	-0.972	-1.022	-0.972
	(1.418)	(1.420)	(1.421)	(1.452)	(1.454)	(1.455)	(1.442)	(1.454)
Spy density × 1928	2.316	2.316	2.316	0.354	0.354	0.354	0.357	0.354
	(1.659)	(1.661)	(1.662)	(1.581)	(1.583)	(1.584)	(1.584)	(1.583)
Spy density × 1930	-2.379*	-2.379*	-2.379*	-2.192*	-2.192*	-2.192*	-2.183*	-2.192*
	(1.217)	(1.219)	(1.219)	(1.164)	(1.166)	(1.166)	(1.166)	(1.165)
Spy density × 07/1932	-0.479	-0.479	-0.479	-0.239	-0.239	-0.239	-0.231	-0.239
	(0.809)	(0.811)	(0.811)	(0.580)	(0.580)	(0.581)	(0.581)	(0.580)
Spy density × 1990	-2.367	-1.888	-5.534**	-0.554	-0.745	-0.480	-0.510	
	(2.290)	(2.340)	(2.647)	(2.101)	(2.166)	(2.162)	(2.166)	
Spy density × 1994	-2.753	-2.894	-6.540**	-2.710	-2.901	-2.635	-2.667	
	(2.298)	(2.340)	(2.740)	(2.212)	(2.278)	(2.256)	(2.223)	
Spy density × 1998	-6.205***	-6.505***	-10.151***	-1.585	-1.776	-1.511	-1.558	-0.866
	(2.228)	(2.202)	(2.640)	(1.975)	(2.037)	(2.036)	(2.072)	(2.071)
Spy density × 2002	0.638	0.963	-2.683	-3.988*	-4.179**	-3.914*	-3.919*	-3.082
	(2.109)	(2.170)	(2.641)	(2.041)	(2.092)	(2.086)	(2.047)	(2.087)
Spy density × 2005	0.167	0.500	-3.147	-3.726*	-3.917*	-3.652*	-3.592*	-2.643
	(1.993)	(2.053)	(2.513)	(2.067)	(2.124)	(2.134)	(2.038)	(2.166)
Spy density × 2009	2.666	2.924	-0.723	-4.878**	-5.069**	-4.804**	-4.644**	-2.971
	(2.264)	(2.335)	(2.779)	(2.190)	(2.268)	(2.269)	(2.201)	(2.273)
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	2232	2232	2232	2232	2232	2232	2230	1858
Adjusted R^2	0.826	0.826	0.829	0.919	0.920	0.921	0.923	0.930

Notes: This table shows the β coefficients of the panel data model laid out in equation (3). 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 share-year interaction for November 1932 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object of SI stands for Object of Special Interest. County size controls include log county area and log mean 1980s population. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix B.

Table A.4: Effect of Spying on Self-Employment Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Spy density × 1925	-1.353	-1.353	-1.353	-0.083	-0.083	-0.083	0.023	-0.083
	(1.326)	(1.330)	(1.330)	(1.437)	(1.439)	(1.439)	(1.441)	(1.440)
Spy density × 1996	-3.694***	-4.155***	-2.938*	-2.188	-2.468	-2.837*	-2.769*	-2.773*
	(1.220)	(1.117)	(1.512)	(1.455)	(1.517)	(1.540)	(1.421)	(1.532)
Spy density × 1997	-3.751***	-4.197***	-2.979*	-2.132	-2.412	-2.782*	-2.708*	-2.733*
	(1.259)	(1.164)	(1.558)	(1.485)	(1.544)	(1.565)	(1.454)	(1.557)
Spy density × 1998	-3.626***	-4.061***	-2.843*	-2.125	-2.405	-2.774*	-2.704*	-2.755*
	(1.259)	(1.170)	(1.571)	(1.479)	(1.536)	(1.557)	(1.453)	(1.556)
Spy density × 1999	-3.534***	-3.949***	-2.731*	-2.070	-2.349	-2.719*	-2.652*	-2.705*
	(1.264)	(1.203)	(1.607)	(1.474)	(1.529)	(1.545)	(1.445)	(1.541)
Spy density × 2000	-3.504***	-3.914***	-2.697	-1.777	-2.056	-2.426	-2.366	-2.433
	(1.294)	(1.229)	(1.633)	(1.460)	(1.517)	(1.538)	(1.437)	(1.532)
Spy density × 2001	-3.060**	-3.431***	-2.213	-1.546	-1.826	-2.196	-2.145	-2.218
	(1.301)	(1.234)	(1.633)	(1.489)	(1.545)	(1.565)	(1.469)	(1.558)
Spy density × 2002	-2.973**	-3.332***	-2.115	-1.523	-1.803	-2.173	-2.133	-2.160
	(1.320)	(1.241)	(1.629)	(1.493)	(1.547)	(1.567)	(1.477)	(1.555)
Spy density × 2003	-2.859**	-3.214**	-1.996	-1.399	-1.678	-2.048	-2.022	-2.041
	(1.366)	(1.286)	(1.669)	(1.528)	(1.582)	(1.599)	(1.513)	(1.594)
Spy density × 2004	-2.851**	-3.213**	-1.995	-1.355	-1.635	-2.004	-1.992	-1.946
	(1.397)	(1.317)	(1.691)	(1.541)	(1.597)	(1.614)	(1.526)	(1.600)
Spy density × 2005	-2.475*	-2.779**	-1.562	-1.154	-1.434	-1.804	-1.810	-1.728
	(1.407)	(1.348)	(1.716)	(1.558)	(1.611)	(1.625)	(1.540)	(1.614)
Spy density × 2006	-2.799**	-3.086**	-1.868	-1.434	-1.714	-2.083	-2.107	-2.021
	(1.416)	(1.354)	(1.715)	(1.571)	(1.620)	(1.635)	(1.558)	(1.627)
Spy density × 2007	-2.623*	-2.935**	-1.717	-1.494	-1.774	-2.143	-2.184	-2.132
	(1.397)	(1.311)	(1.676)	(1.562)	(1.612)	(1.630)	(1.560)	(1.619)
Spy density × 2008	-2.305*	-2.560**	-1.342	-1.542	-1.822	-2.192	-2.251	-2.186
	(1.354)	(1.297)	(1.668)	(1.542)	(1.593)	(1.611)	(1.539)	(1.601)
Spy density × 2009	-2.248*	-2.484*	-1.266	-1.781	-2.061	-2.430	-2.507	-2.413
	(1.348)	(1.299)	(1.676)	(1.539)	(1.593)	(1.611)	(1.541)	(1.612)
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	2976	2976	2976	2976	2976	2976	2976	2974
Adjusted R^2	0.877	0.877	0.878	0.915	0.916	0.917	0.920	0.918

Notes: This table shows the β coefficients of the panel data model laid out in equation (3). 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 share-year interaction for 1933 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object of SI stands for Object of Special Interest. County size controls include log county area and log mean 1980s population. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix B.

Table A.5: Effect of Spying on Patents per 100,000 Inhabitants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Spy density × 1928	3.685*	3.685*	3.685*	4.285	4.285	4.285	4.289	4.285
	(1.947)	(1.952)	(1.953)	(2.887)	(2.891)	(2.892)	(2.895)	(2.893)
Spy density × 1993	-5.397	-4.891	-0.411	-2.008	-0.966	-0.373	-0.396	
	(3.968)	(4.463)	(5.511)	(5.898)	(5.834)	(5.850)	(6.050)	
Spy density × 1995	-5.673	-3.361	1.118	-1.118	-0.076	0.516	0.507	0.714
	(3.930)	(5.416)	(5.857)	(6.659)	(6.296)	(6.051)	(6.132)	(6.097)
Spy density × 1996	-6.545	-5.109	-0.629	-0.958	0.084	0.677	0.678	0.519
	(4.366)	(5.668)	(6.146)	(6.825)	(6.446)	(6.228)	(6.340)	(6.302)
Spy density × 1997	-6.956*	-6.052	-1.572	-2.673	-1.631	-1.038	-1.029	-1.134
	(4.074)	(5.128)	(5.462)	(5.722)	(5.411)	(5.249)	(5.378)	(5.332)
Spy density × 1998	-13.326***	-13.433**	-8.954	-8.741	-7.699	-7.106	-7.102	-7.578
	(4.577)	(5.471)	(6.239)	(6.380)	(5.974)	(5.722)	(5.803)	(5.678)
Spy density × 1999	-9.876**	-9.350	-4.870	-6.416	-5.374	-4.781	-4.782	-5.261
	(4.725)	(5.848)	(6.576)	(6.708)	(6.284)	(6.061)	(6.183)	(6.036)
Spy density × 2000	-13.226***	-13.858**	-9.378	-11.724*	-10.682*	-10.089*	-10.102*	-10.335*
	(5.032)	(5.862)	(6.603)	(6.572)	(6.052)	(5.833)	(5.925)	(5.832)
Spy density × 2001	-9.948*	-10.716	-6.236	-9.451	-8.409	-7.816	-7.846	-7.741
	(5.593)	(6.538)	(7.167)	(6.868)	(6.515)	(6.327)	(6.456)	(6.282)
Spy density × 2002	-10.802	-13.265*	-8.785	-11.676	-10.633	-10.041	-10.088	-9.837
	(6.556)	(7.870)	(8.427)	(8.366)	(7.911)	(7.681)	(7.751)	(7.561)
Spy density × 2003	-15.861**	-18.940**	-14.460*	-18.417*	-17.375*	-16.782*	-16.852*	-16.317*
	(6.109)	(8.013)	(8.282)	(9.498)	(8.928)	(8.560)	(8.602)	(8.391)
Spy density × 2004	-9.422	-12.959	-8.479	-11.568	-10.526	-9.934	-10.027	-9.194
	(6.665)	(7.905)	(8.410)	(8.674)	(8.220)	(7.984)	(8.091)	(7.935)
Spy density × 2005	-10.239	-15.291*	-10.811	-18.778**	-17.736**	-17.143**	-17.267**	-16.280**
	(6.992)	(8.377)	(8.877)	(8.548)	(8.114)	(7.921)	(7.987)	(7.855)
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	2604	2604	2604	2604	2604	2604	2604	2418
Adjusted R^2	0.391	0.427	0.430	0.504	0.509	0.511	0.513	0.532

Notes: This table shows the β coefficients of the panel data model laid out in equation (3). 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 share-year interaction for 1929 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object of SI stands for Object of Special Interest. County size controls include log county area and log mean 1980s population. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix B.

Table A.6: Effect of Spying on Unemployment Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Spy density × 1996	19.852***	21.075***	10.195***	4.287*	4.734*	4.838*	4.741**	4.916**
	(3.054)	(3.016)	(3.296)	(2.453)	(2.510)	(2.466)	(2.327)	(2.465)
Spy density × 1997	19.187***	20.269***	9.388***	4.377*	4.824*	4.928*	4.822**	4.990**
	(3.071)	(3.058)	(3.303)	(2.515)	(2.564)	(2.524)	(2.387)	(2.517)
Spy density × 1998	18.163***	19.101***	8.220**	2.440	2.887	2.991	2.891	3.042
	(2.988)	(2.940)	(3.242)	(2.448)	(2.494)	(2.465)	(2.315)	(2.461)
Spy density × 1999	18.245***	19.261***	8.380***	1.977	2.424	2.528	2.433	2.574
	(2.891)	(2.825)	(3.189)	(2.345)	(2.409)	(2.379)	(2.223)	(2.377)
Spy density × 2000	19.055***	20.078***	9.197***	2.650	3.097	3.202	3.119	3.221
	(2.895)	(2.827)	(3.215)	(2.379)	(2.451)	(2.423)	(2.279)	(2.418)
Spy density × 2001	19.968***	21.204***	10.324***	3.188	3.635	3.739	3.675	3.734
	(2.916)	(2.825)	(3.193)	(2.416)	(2.496)	(2.464)	(2.335)	(2.466)
Spy density × 2002	19.610***	20.811***	9.931***	2.932	3.379	3.483	3.438	3.504
	(2.893)	(2.841)	(3.185)	(2.459)	(2.544)	(2.515)	(2.367)	(2.519)
Spy density × 2003	21.232***	22.497***	11.617***	3.529	3.976	4.081	4.061	4.086
	(3.121)	(3.081)	(3.397)	(2.586)	(2.660)	(2.629)	(2.506)	(2.629)
Spy density × 2004	22.038***	23.330***	12.450***	3.975	4.422	4.526*	4.531*	4.567*
	(3.154)	(3.137)	(3.458)	(2.655)	(2.732)	(2.704)	(2.584)	(2.701)
Spy density × 2005	21.394***	22.625***	11.744***	4.584*	5.031*	5.136*	5.175**	5.187*
	(2.926)	(2.999)	(3.357)	(2.585)	(2.674)	(2.648)	(2.528)	(2.638)
Spy density × 2006	21.777***	23.246***	12.366***	4.719*	5.166*	5.270*	5.342**	5.298**
	(3.014)	(2.972)	(3.381)	(2.642)	(2.714)	(2.689)	(2.594)	(2.683)
Spy density × 2007	21.949***	23.237***	12.357***	4.504*	4.951*	5.055*	5.159*	5.025*
	(2.971)	(2.927)	(3.379)	(2.670)	(2.743)	(2.722)	(2.626)	(2.727)
Spy density × 2008	20.640***	21.914***	11.034***	3.448	3.895	3.999	4.138*	3.969
	(2.872)	(2.766)	(3.249)	(2.494)	(2.574)	(2.553)	(2.447)	(2.561)
Spy density × 2009	19.308***	20.537***	9.657***	3.264	3.711	3.816	3.985*	3.916
	(2.727)	(2.653)	(3.118)	(2.460)	(2.547)	(2.523)	(2.404)	(2.531)
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	2790	2790	2790	2790	2790	2790	2790	2788
Adjusted R^2	0.603	0.609	0.656	0.823	0.829	0.829	0.837	0.829

Notes: This table shows the β coefficients of the panel data model laid out in equation (3). 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 share-year interaction for 1933 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object of SI stands for Object of Special Interest. County size controls include log county area and log mean 1980s population. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix B.

Table A.7: Effect of Spying on Log Population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density × 1985	-0.009	-0.009	-0.009	0.003	0.003	0.003	0.003
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
Spy density × 1986	-0.003	-0.003	-0.003	0.004	0.004	0.004	0.004
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Spy density × 1987	-0.002	-0.002	-0.002	-0.001	-0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Spy density × 1989	0.013**	0.016***	-0.063**	-0.069**	-0.070**	-0.067**	
	(0.006)	(0.005)	(0.025)	(0.027)	(0.028)	(0.029)	
Spy density × 1990	0.024***	0.029***	-0.050**	-0.062**	-0.064**	-0.060**	
	(0.009)	(0.007)	(0.024)	(0.027)	(0.028)	(0.028)	
Spy density × 1991	0.019*	0.023***	-0.056**	-0.068**	-0.070**	-0.066**	
	(0.010)	(0.008)	(0.024)	(0.028)	(0.028)	(0.028)	
Spy density × 1992	0.024**	0.028***	-0.051**	-0.065**	-0.067**	-0.064**	
	(0.011)	(0.009)	(0.024)	(0.027)	(0.027)	(0.028)	
Spy density × 1993	0.027**	0.031***	-0.048**	-0.065**	-0.067**	-0.064**	
	(0.013)	(0.011)	(0.024)	(0.026)	(0.027)	(0.027)	
Spy density × 1994	0.036**	0.041***	-0.037	-0.062**	-0.063**	-0.060**	
	(0.017)	(0.015)	(0.025)	(0.025)	(0.026)	(0.026)	
Spy density × 1995	0.049**	0.055***	-0.024	-0.059**	-0.060**	-0.057**	-0.083**
	(0.021)	(0.019)	(0.027)	(0.026)	(0.027)	(0.028)	(0.033)
Spy density × 1996	0.065**	0.071***	-0.008	-0.054*	-0.056*	-0.052*	-0.082**
	(0.026)	(0.025)	(0.031)	(0.029)	(0.030)	(0.031)	(0.035)
Spy density × 1997	0.081**	0.088***	0.009	-0.050	-0.052	-0.048	-0.078**
	(0.032)	(0.030)	(0.035)	(0.034)	(0.036)	(0.036)	(0.039)
Spy density × 1998	0.092**	0.101***	0.022	-0.052	-0.054	-0.051	-0.084*
	(0.038)	(0.036)	(0.039)	(0.041)	(0.042)	(0.043)	(0.044)
Spy density × 1999	0.104**	0.112***	0.033	-0.055	-0.057	-0.053	-0.086*
	(0.042)	(0.041)	(0.043)	(0.046)	(0.048)	(0.048)	(0.049)
Spy density × 2000	0.110**	0.118***	0.039	-0.060	-0.062	-0.059	-0.090*
	(0.045)	(0.044)	(0.046)	(0.050)	(0.052)	(0.052)	(0.052)
Spy density × 2001	0.112**	0.119**	0.040	-0.068	-0.070	-0.067	-0.096*
	(0.048)	(0.048)	(0.049)	(0.054)	(0.056)	(0.056)	(0.055)
Spy density × 2002	0.112**	0.118**	0.039	-0.077	-0.078	-0.075	-0.103*
	(0.051)	(0.050)	(0.052)	(0.057)	(0.059)	(0.059)	(0.058)
Spy density × 2003	0.112**	0.117**	0.038	-0.088	-0.089	-0.086	-0.111*
	(0.052)	(0.053)	(0.054)	(0.060)	(0.061)	(0.062)	(0.061)
Spy density × 2004	0.110**	0.114**	0.035	-0.099	-0.100	-0.097	-0.119*
	(0.054)	(0.055)	(0.056)	(0.062)	(0.064)	(0.064)	(0.063)
Spy density × 2005	0.108*	0.110*	0.031	-0.113*	-0.115*	-0.112*	-0.133**
	(0.055)	(0.057)	(0.058)	(0.064)	(0.066)	(0.066)	(0.066)
Spy density × 2006	0.105*	0.106*	0.027	-0.128*	-0.130*	-0.126*	-0.145**
	(0.057)	(0.059)	(0.060)	(0.067)	(0.068)	(0.069)	(0.067)
Spy density × 2007	0.102*	0.101*	0.023	-0.142**	-0.143**	-0.140**	-0.156**
	(0.059)	(0.061)	(0.062)	(0.068)	(0.070)	(0.070)	(0.069)
Spy density × 2008	0.097	0.095	0.016	-0.157**	-0.158**	-0.155**	-0.172**
	(0.060)	(0.062)	(0.064)	(0.070)	(0.071)	(0.072)	(0.071)
Spy density × 2009	0.093	0.090	0.011	-0.170**	-0.172**	-0.169**	-0.177**
	(0.061)	(0.064)	(0.066)	(0.071)	(0.073)	(0.073)	(0.073)
Post × Object of SI x Year FE		Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls			Yes	Yes	Yes	Yes	Yes
State × Year FE				Yes	Yes	Yes	Yes
Post × Opposition controls					Yes	Yes	Yes
Post × Industry controls						Yes	Yes
Post × Transfers							Yes
Observations	4650	4650	4650	4650	4650	4650	3532
Adjusted R^2	0.529	0.528	0.548	0.703	0.703	0.704	0.799

Notes: This table shows the β coefficients of the panel data model laid out in equation (3). 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 share-year interaction for 1988 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object of SI stands for Object of Special Interest. County size controls include log county area. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix B.

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 industrial employment share in September 1989 and 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	Min	Max	N
Electoral turnout	77.3	7.4	56.6	92.6	2,232
Self-employment rate	11.4	3.6	5.0	31.8	2,976
Patents per 100,000 inhabitants	11.9	14.6	0.0	212.6	2,604
Unemployement rate	18.4	4.0	3.7	31.3	2,790
Log population	10.9	0.6	9.6	13.2	4,650
Stasi share	0.4	0.1	0.1	1.0	186
Dummy: Object of Special Interest	0.0	0.2	0.0	1.0	186
Log mean population 1980s	11.0	0.6	9.8	13.2	186
Log county size (in sqm)	6.0	0.8	3.3	7.1	186
Uprising intensity 1953	1.4	1.4	0.0	4.0	186
Dummy: State of Emergency 1953	0.5	0.5	0.0	1.0	186
Dummy: Military intervention 1953	0.7	0.5	0.0	1.0	186
Share indust. employment 1989	45.3	13.6	16.8	74.5	186
Dummy: Important industries 1989	0.2	0.4	0.0	1.0	186
Log transfers	16.9	0.7	15.6	19.9	2,788
Log investment subsidies	16.2	0.7	14.6	19.1	2,788

Notes: This table presents outcome and control variables on district variables. For information on the respective years covered, 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 descriptives 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	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 (*Bundesbeauftragter für die Unterlagen des Staatssicherheitsdienstes der ehemaligen Deutschen Demokratischen Republik, BStU*) and compiled by Müller-Enbergs (2008). Population figures come from the Statistical Yearbooks of the GDR.

continued

Table B.3 continued

Variable	Years	Source
		Our measure of spy density covers unofficial collaborators for political-operative penetration, homeland defense, or special operations, as well as leading informers (*IM zur politisch-operativen Durchdringung und Sicherung des Verantwortungsbereiches, IM der Abwehr mit Feindverbindung bzw. zur unmittelbaren Bearbeitung im Verdacht der Feindtätigkeit stehender Personen, IM im besonderen Einsatz, Führungs-IM*). In cases where 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.
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. Note that information on individuals' engagement in local politics is provided in additional waves as well. We choose the years of 2001 and 2007 to cover similar points in time with all of our four measures of social capital.
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.
Reciprocity	2005, 2010	We use three statements on negative reciprocity, response options varying on a seven-point scale. We follow Dohmen et al. (2009) by combining the three questions into one single measure. The respective questions read as follows: (i) "If I suffer a serious wrong, I will take revenge as soon as possible, no matter what the cost," (ii) "If somebody puts me in a difficult position, I will do the same to him/her," and (iii) "If somebody offends me, I will offend him/her back."
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. As different measures of social capital are measured in various waves of the survey; samples slightly differ for the outcome variables of interest.

continued

Table B.3 continued

Variable	Years	Source
Panel C – County-level data (see Section 3.3)		
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 (*Bundeswahlleiter*).
Industry controls	1989	Industry composition is measured by means of the share of employees in the industrial sector as of September 1989, 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.
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 re-unification patent filings come from the German Patent and Trade Mark Office (*Deutsches Patent- und Markenamt*). 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 (*Kommunistische Partei Deutschlands, KPD*) and the Nazi party (*Nationalsozialistische Deutsche Arbeiterpartei, NSDAP*) in the federal elections in 1928, 1930, 07/1932 and 11/1932 to construct two distinct measures of political ideology. Data on Weimar Republic election results are based on King et al. (2008).
Population	1925–1933	Population figures for the Weimar Republic are obtained from King et al. (2008) and Falter and Hänisch (1990).
	1980–1989	Data collected from the Statistical Yearbooks of the German Democratic Republic (*Statistische Jahrbücher der Deutschen Demokratischen Republik*).
	1990–2009	Collected from the Regional Database Germany (*Regionaldatenbank Deutschland*), the Statistical Offices of the Federal States (*Statistische Landesämter*) and the Working Group Regional Accounts (*Arbeitskreis Volkswirtschaftliche Gesamtrechnungen der Länder*).
Religion	1925	The share of protestants in the population was published in the 1925 census of the Weimar Republic (*Volkszählung 1925*). Our data stems from King et al. (2008).
Revenues	1995–2009	Data on revenues are obtained from the Regional Database Germany (*Regionaldatenbank Deutschland*). Revenues cover monetary transfers from the federal and state level (*allgemeine Zuweisungen und Umlagen von Bund, Land, Gemeinden/Gemeindeverbänden*) as well as investment subsidies granted to the counties (*Zuweisungen und Zuschüsse für Investitionsförderungen*).

continued

Table B.3 continued

Variable	Years	Source
Self-employment	1925, 1933	County-level self-employment rates from the 1925 and 1933 censuses of the Weimar Republic (*Volks- und Berufszählung 1925 und 1933*). Data for 1925 are obtained from Falter and Hänisch (1990); data for 1933 from King et al. (2008). Note that numbers for 1925 refer to households 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 (*Bundesinstitut für Bau-, Stadt- und Raumforschung, BBSR*).
Unemployment	1933	County-level unemployment rates are based on the 1933 census of the Weimar Republic (*Volks- und Berufszählung 1933*), provided in King et al. (2008).
	1996–2009	Monthly county-level unemployment rates are made available from March 1996 to December 2009 by the Federal Employment Agency (*Bundesagentur für Arbeit*). We calculate yearly means from this data.
Uprising	1953	We use cartographic statistics published by the former West German Federal Ministry of Intra-German Relations (*Bundesministerium für gesamtdeutsche Fragen*) to create two dummy variables indicating whether the regime declared a state of emergency and whether the Soviet military intervened in the particular county. In addition, the data provides an ordinal, additive measure of strike intensity ("none", "strike", "demonstration", "riot", "liberation of prisoners"). The map is available in the archives of the Federal Foundation for the Reappraisal of the SED Dictatorship (*Bundesstiftung zur Aufarbeitung der SED-Diktatur*), signature: EA 111 1889.

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. 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.