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Abstract

We investigate the long-run effects of government surveillance on civic capital and economic performance, studying the case of the Stasi in East Germany. Exploiting regional variation in the number of spies and administrative features of the system, we combine a border discontinuity design with an instrumental variables strategy to estimate the long-term, post-reunification effect of government surveillance. We find that a higher spying density led to persistently lower levels of interpersonal and institutional trust in post-reunification Germany. We also find substantial and long-lasting economic effects of Stasi surveillance, resulting in lower income, higher exposure to unemployment, and lower self-employment.

JEL Code: H11, N34, N44, P20

Keywords: Civic capital, government surveillance, trust, economic performance, East Germany

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1 Introduction

Autocracies have been the dominant form of government in human history. Despite substantial shifts towards more democratic political institutions in recent decades, autocratic regimes still rule in more than a quarter of the countries worldwide (see Figure B.1), accounting for more than one third of the world population (The Economist Intelligence Unit, 2014). A common, defining characteristic of autocracies is the repression of oppositional movements to ensure political stability and avoid revolution (Gerschewski, 2013, Marshall et al., 2017). While regimes differ in their mix of repressive measures, all need to extract precise information about oppositional movements within the population. To this end, they operate large-scale state surveillance systems that monitor the population (Davenport, 2005). These repressive surveillance measures, reaching deep into private lives, may in turn affect individual social behavior, creating a widespread atmosphere of suspicion and distrust towards fellow citizens and state institutions, and thereby transform civil society (Arendt, 1951). While qualitative historical research and numerous media contributions support this mechanism, there is no systematic empirical evidence documenting the detrimental effects of such repressive measures on society.[1]

In this paper, we intend to advance our understanding of the legacy of repression by studying the case of the socialist German Democratic Republic (GDR). The GDR was an autocratic state, whose repressive policies were explicitly built upon silent methods of surveillance rather than overt persecution and violence (Knabe, 1999). As a result, the regime implemented one of the largest and densest surveillance networks of all time. The Ministry for State Security, commonly referred to as the *Stasi*, administered a huge body of so-called *Inoffizielle Mitarbeiter*, unofficial informers. These informers accounted for around one percent of the East German population in the 1980s and were regarded as the regime's most important instrument to secure power (Müller-Enbergs, 1996, p. 305). Informers were ordinary citizens who kept their regular jobs but secretly gathered information within their professional and social network, thus betraying the trust of friends, neighbors, and colleagues (Bruce, 2010). A large body of historical research deems the effects of the surveillance apparatus as devastating, having shattered interpersonal trust with long-lasting consequences: "The oppressive effects of the constant threat of Stasi surveillance [...] can scarcely be overstated. It led to perpetual insecurity in personal relationships, and was to leave a difficult legacy for post-reunification Germany" (Fulbrook, 2009, p. 221).[2]

We put these claims to a test by estimating the long-term, post-reunification effects of state surveillance on society after the fall of the GDR regime. Using administrative data on the ubiquitous network of informers, we construct a measure of local surveillance intensity and exploit administrative features of the Stasi to set-up a quasi-experimental research design. To operationalize the effect of surveillance on social and cooperative behavior, we choose standard measures of interpersonal and institutional trust that have been seen as key components in broader measures of civic capital (Knack

[1] A limited number of papers studies determinants of state repression, mostly focusing on specific configurations of political institutions that lead to repression (Collier and Rohner, 2008, Besley and Persson, 2009). Similarly, studies in other social sciences primarily analyze political and societal factors that lead regimes to use coercive power; only few studies investigate micro-level effects of repression on groups such as environmental activists (see Davenport, 2007, Earl, 2011, for surveys in political science and sociology, respectively).

[2] See also Gieseke (2014, p. 95) or Childs and Popplewell (1996, p. 111).

and Keefer, 1997, Guiso et al., 2008, 2010).[3] While the general nature of autocratic repression suggests deteriorations in individuals' trust in institutions, the specific use of informers within social networks makes interpersonal trust another well-suited measure to proxy the social costs of surveillance. Last, we additionally test whether Stasi surveillance had an effect on measures of economic performance such as income and unemployment, as civic capital has been shown to be positively associated with economic outcomes (see Algan and Cahuc, 2014, Fuchs-Schündeln and Hassan, 2016, for surveys, and the more detailed discussion below).

Our empirical strategy explicitly addresses the concern that recruitment of informers across space might not have been random by combining a border discontinuity design with an instrumental variables approach that takes advantage of the specific administrative structure of the surveillance state. Stasi district offices bore full responsibility for securing their territory and supervising the respective subordinate county offices, which caused surveillance intensities to differ substantially across GDR districts (Engelmann and Schumann, 1995). Important for our identification strategy, this structure was at odds with the fully centralized political system of the GDR, which followed the Leninist principle of Democratic Centralism in allocating all political powers and legislative competencies to the level of the central government (Bartsch, 1991, Niemann, 2007). This set-up allows us to use discontinuities in surveillance intensities along district borders as a source of exogenous variation. Using the leave-out average surveillance intensity at the district level as an instrument, we further isolate the part of the variation in the county-level spying density that is explained by differences in surveillance strategies across districts.

Overall, the results of our study offer substantial evidence for negative and long-lasting effects of government surveillance on civic capital and economic performance. Using data from the German Socio-Economic Panel (SOEP), we find that a higher spying density leads to lower trust in strangers and stronger negative reciprocity, two standard measures predicting cooperative behavior (Glaeser et al., 2000, Dohmen et al., 2009). In terms of magnitude, we find that a one standard deviation increase in the spying density – equal to more than one third of the average surveillance intensity – decreases trust (reciprocal behavior) by 0.1 (0.18) of a standard deviation. We further observe negative effects on political participation as measured by individuals' intention to attend elections, political interest and engagement (Putnam, 1993, Guiso et al., 2010). The effects on civic capital are accompanied by negative and persistent effects on measures of economic performance. A one standard deviation increase in the surveillance intensity reduces monthly individual income by 84 EUR and increases the overall time spent in unemployment by 1.4 percentage points. Moreover, we find negative effects on self-employment, with entrepreneurial spirit being one likely channel linking trust and economic performance (Knack and Keefer, 1997). Importantly, we corroborate these estimates using administrative wage and turnout data at the regional level. Investigating the dynamics of our effects, we further find effects on civic capital to precede economic effects, which is in line with our theoretical priors that reductions in civic capital lead to worse economic outcomes.

[3] We follow Guiso et al. (2010) and focus on civic capital defined as "those persistent and shared beliefs and values that help a group overcome the free rider problem in the pursuit of socially valuable activities". Using this conceptualization has two major advantages. First, it addresses the critique of too elastic definitions of social capital in the literature. Second, and important in the context of our study, it highlights the importance of interpersonal and institutional trust, which are likely to be affected by the Stasi regime and their specific surveillance technique. We use trust and civic capital interchangeably throughout the paper.

Our empirical results become stronger when tightening identification and moving from cross-sectional OLS estimates to the border design-IV specification. In line with this finding, we further show that effects are larger at district borders separating counties that had been part of the same province during the time of the Weimar Republic and share the same cultural heritage. In addition, we provide a wide range of tests to demonstrate the robustness of our results with respect to (i) different measures of surveillance such as political arrests, (ii) alternative definitions of the instrument, (iii) different specifications of the border design, and (iv) alternative ways to draw inference. Moreover, we rule out alternative mechanisms that may explain our economics effects, such as surveillance-induced differences in risk aversion, personality traits unrelated to trust, or preferences for redistribution. Last, we take a closer look at the channels behind our economic effects, providing evidence that differences in educational attainment are a main driver. We link these differences to reductions in civic capital, corroborating the prediction that higher levels of trust should lead to higher investments in (human) capital (Goldin and Katz, 1999).

Our study is closely linked to the steadily growing literature on the relationship between institutions, culture, and economic performance (see Algan and Cahuc, 2014, Alesina and Giuliano, 2015, Fuchs-Schündeln and Hassan, 2016, for recent surveys and Section 3 for a more detailed discussion of the literature). We show that rather short-lived political institutions can have persistent, long-term effects on important economic preferences and – more generally – cultural traits. Our findings complement other studies that use variation in deep, cultural differences such as religion, ethnicity, or education to explain contemporaneous differences in economic preferences, beliefs, and values (Tabellini, 2010, Alesina et al., 2013). In addition, we also provide evidence documenting the long-term positive effects of institutional quality on economic performance (La Porta et al., 1997) and recent evidence showing that too little (but also too much) individual trust leads to negative economic outcomes (Butler et al., 2016). Econometrically, we refine current identification strategies used in the literature to estimate causal effects of formal institutions on culture and economic outcomes by combining within-country variation with spatial border designs (Becker et al., 2016, Fontana et al., 2017). We further break new ground by studying the long-term effects of repression in autocratic regimes, in our context state surveillance, on social behavior and economic performance. Thereby, we complement studies on the macro level that show positive effects of democracy on growth (Rodrik and Wacziarg, 2005, Acemoglu et al., 2019). In particular, and similar to Nunn and Wantchekon (2011) in the case of slave trade in Africa, we show that the GDR surveillance state destroyed civic capital and led to mistrust towards others and the political class.

Moreover, we contribute to the literature investigating the transformation and legacy of countries of the former Eastern Bloc after the fall of the Iron Curtain (see, e.g., Shleifer, 1997). While evidence on the social and economic consequences of the fall of Communism in Central and Eastern Europe is mixed (cf. the discussion in Alesina and Giuliano, 2015), our paper complements evidence that features of these regimes had long-lasting social and economic effects. Looking at the German case, we show that the East German regime did not only affect individual preferences for redistribution as demonstrated by Alesina and Fuchs-Schündeln (2007), but also had long-lasting effects on economic preferences and performance. In line with Fuchs-Schündeln and Masella (2016), we further document that contemporaneous differences in labor market outcomes can be attributed to

features of the socialist regime. Last, our paper is related to two other studies which investigate the effects of Stasi surveillance.[4] Importantly, our analysis is related to earlier work by Jacob and Tyrell (2010) who were the first to investigate the relationship between surveillance, social capital, and economic backwardness in East Germany. In our paper, we try to contribute to this work by implementing a quasi-experimental research design that is able to establish a causal link between government surveillance, civic capital, and economic performance. Moreover, we take a closer look at the underlying mechanisms driving the effects. A second paper by Friehe et al. (2015), pursued simultaneously but independently from our project, investigates the effects of Stasi surveillance on personality traits. While both studies document negative effects of government surveillance, which can be partly reconciled with our findings, we suggest a novel identification strategy which explicitly addresses the non-randomness of the county-level surveillance density. Going beyond cross-sectional correlations, we demonstrate that ignoring the endogeneity of the regional surveillance intensity can lead to a non-negligible bias in the estimates.

The remainder of this paper is organized as follows. Section 2 presents the historical background, the institutional details of the Stasi surveillance system, and our measure of the regional surveillance intensity. In light of the GDR surveillance state, Section 3 lays out our conceptual framework, combining theoretical predictions with empirical insights from the literature on trust and economic performance. Section 4 describes the data used in the empirical analysis and introduces our research design. Results are presented in Section 5. Section 6 concludes.

2 The GDR Surveillance State

After Germany's unconditional surrender and the end of World War II in May 1945, the country's territory west of the Oder-Neisse line was divided among the four Allied Forces – the US, the UK, France, and the Soviet Union. While the Western forces soon established the principles of democracy and free markets in their respective zones, the Soviet Union implemented a socialist regime in the eastern part of the country. In May 1949, the ideological division of the nation was institutionalized when the Federal Republic of Germany was established on the territory of the three western zones. Five months later, the German Democratic Republic (GDR) was constituted in the Soviet ruled zone, which eventually led to a 40 year long division of the country.

In the early years, the GDR was under constant internal pressure. Dissatisfaction with working conditions and the implementation of socialism culminated in the People's Uprising on and around June 17, 1953, when an unexpected wave of strikes and demonstrations hit the country. Moreover, from 1949 to 1961, roughly 2.7 million citizens (around 20 percent of the population) managed to leave the country by authorized migration or illegal border crossing (see Figure B.2). Securing the inner-German border in 1952 was not sufficient to stop this exodus, as people were still able to escape to the West relatively easily via the divided city of Berlin. Eventually, the regime stopped the substantial population loss by building the Berlin Wall in 1961, and ordering soldiers to shoot at every person trying to illegally cross the inner-German border. Between 1962 and 1988 only around

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

0.1 percent of the population managed to emigrate on an annual basis (6-7 percent of which were illegal border crossings to the West).

Throughout most of the 1960s and 1970s, East and West Germany increasingly grew apart in their social and cultural patterns, leading to a situation of relative political stability. East Germans "felt they had to try to work with socialism, and to confront and make the best of the constraints within which they had to operate" (Fulbrook, 2009, p. 174). In the late, 1970s dissident tendencies resurfaced and became stronger throughout the 1980s, leading to the fall of the Berlin Wall on the evening of November 9, 1989. This event marked the beginning of the dissolution of the GDR, which officially ended with the reunification of West and East Germany in October 1990.

The Principle of Democratic Centralism. Throughout its existence, the GDR was an autocracy under the rule of the Socialist Unity Party (SED) and its secretaries general. Its organization closely followed the Soviet example of a highly centralized state, with all political power being held by the Politburo in East Berlin. Importantly, the GDR followed the Leninist principle of Democratic Centralism, which stipulated that all local authorities were subordinate to the administration at the central level in order to secure uniformity of governance (Bartsch, 1991). To this end, the regime quickly abolished existing decentralized political institutions from the times of the Weimar Republic and eliminated the power of subnational entities. In a first step, the Soviet occupying forces formed the five intermediate jurisdictions Mecklenburg, Anhalt, Brandenburg, Thuringia, and Saxony, which were eventually abolished in 1952 and replaced by 15 administrative districts (*Bezirke*).[5] Districts were deprived of all legislative powers: "In lieu of a state that showed rudimentary features of a federal structure, a unity state with a uniform administration from the top to the smallest municipality was implemented" (Mampel, 1982, p. 1123, own translation). "The legislative competence was exclusively allocated to the central level: local authorities – districts, counties, or municipalities – had the responsibility to locally implement the directives coming from the central level" (Kotsch and Engler, 2017, p. 35, own translation). Using a direct quote from the district official Ulrich Schlaak, Second Secretary of the SED in the district of Potsdam: "The only task [of districts] was to execute the decisions made by the central committee. This was their raison d'être" (as cited in Niemann, 2007, p. 198, own translation). This is a key feature of our identification strategy described in Subsection 4.2.

The decision on how to delineate districts was the results of a complex and eventually quite unsystematic process. The overarching goal of the regime was to curb the political and economic influence of the former Weimar provinces by establishing spatial economic equality – a key feature of the Leninist organization of the state (Ostwald, 1989, Kotsch and Engler, 2017). District boundaries were created to re-establish the "proportionality" of regional economic activity, in particular with respect to the distribution of productive forces, a cornerstone of the Socialist and Communist ideology (Schmidt-Renner, 1953). According to an internal note by Hans Warnke, a government official in the Ministry of Internal Affairs, from 1952, the following – potentially conflicting – additional factors played a role in this process: the external borders (land and sea) were to be administered by as few districts as possible; district capitals were to be easily accessible from all counties (without

[5] Initially, 14 districts were created. In 1961, East Berlin was declared a district of its own.

being forced to pass the old province capital); certain industries, such as agriculture, energy/mining, or textile, were to be clustered in certain districts (reprinted in Werner et al., 2017). Overall, the entire process was unsystematic and turbulent – with last minute changes being made in certain regions such as Brandenburg. As a consequence, the goal of separating districts due to economic considerations was rarely achieved (Kotsch and Engler, 2017).

Districts were immediately dissolved after reunification and replaced by five federal states. This happened "noiselessly and without any consequences" as the districts had always been considered as administrative, artificial artifacts which had never "shaped an own identity" among the population of the GDR (Neitmann, 2017).

The Ministry for State Security. In February 1950, just a few months after the proclamation of the GDR, the Ministry for State Security, generally known as the Stasi, was founded. It served as the internal (and external) intelligence agency of the regime. Its official mission was to "battle against agents, saboteurs, and diversionists [in order] to preserve the full effectiveness of [the] Constitution."[6] Soon after its foundation and the unforeseen national uprising against the regime in June 1953, the Stasi substantially expanded its activities and turned into an ubiquitous institution, spying on and suppressing the entire population to ensure and preserve the regime's power (Gieseke, 2014, p. 50ff.).

The key feature of the Stasi's surveillance strategy was the use of "silent" methods of repression rather than legal persecution by the police (Knabe, 1999). To this end, the Stasi administered a dense network of unofficial informers, the regime's "main weapon against the enemy"[7], who secretly gathered detailed inside knowledge about the population. "Informers were seen as an excellent way of preventing trouble before it started [...]" (Childs and Popplewell, 1996, p. 83). In the 1980s, the Stasi listed around 85,000 regular employees and 142,000 unofficial informers, which accounted for around 0.5 and 0.84 percent of the population, respectively.[8]

The organizational structure of the Stasi differed markedly from the otherwise highly centralized political system. Having been decentralized from the very beginning, responsibilities of the Stasi's regional offices were further increased during the mid-1950s to extract information from the society in a more efficient manner (Naimark, 1994, Engelmann and Schumann, 1995). In line with this strategy, Stasi district offices (*Bezirksdienststellen*) bore full responsibility for securing their territory and were independent in how to achieve this goal (Gill and Schröter, 1991, Gieseke, 2014).[9] As a consequence of the decentralized structure, surveillance strategies differed substantially across GDR districts. Overall, district differences account for more than a quarter of the variation in the informer density across counties.[10] This institutional feature is the key attribute we build our identification

[6] 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).

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

[8] The number of regular Stasi employees was notably high compared to the size of other secret services in the Eastern Bloc (see, e.g., Albats, 1995, Gieseke, 2014, p. 72, and Harrison and Zaksauskiene, 2015).

[9] The Minister of State Security in Berlin hardly influenced the activities and directives governed by the heads of the district offices (Gill and Schröter, 1991). Moreover, according to various accounts, the Politburo did not exert any control over the Ministry of State Security from the mid-1950s onwards (see Childs and Popplewell, 1996, p. 67).

[10] Similarly, there were sharp differences in other domains of the surveillance system. For instance, around one third of the constantly-monitored citizens (*Personen in ständiger Überwachung*) were living in the district of Karl-Marx-Stadt (Horsch, 1997), which accounted for only eleven percent of the total population. Likewise, 17 percent of the two million bugged telephone conversations were tapped in the district of Magdeburg, which only made up eight percent of the population.

strategy on (cf. Subsection 4.2).

While many historical accounts acknowledge the considerable differences in surveillance intensities across districts, only a few discuss potential reasons for th heterogeneity. "[The different intensities] do not of course tell us why there were relatively more IM in Cottbus than in Magdeburg, Postsdam or Berlin. Was it due to the zealousness of the Stasi officers in that district or were there other factors involved? Cottbus [a district with a considerably high spying density] was a frontier district with Poland, but so were Frankfurt/Oder and Dresden." (Childs and Popplewell, 1996, p. 85). Following these different historical accounts, we can loosely separate "hard" from "soft" factors as drivers of district-level differences in surveillance intensities. The former ones include population size, the presence of strategically important firms and/or industries as well as the strength of the political opposition (Horsch, 1997, Müller-Enbergs, 2008). Besides these systematic drivers, soft and arguably more random determinants, such as the district leadership's effort, zeal, or loyalty to the regime, are acknowledged as potential drivers of different surveillance intensities across districts (Gill and Schröter, 1991, Childs and Popplewell, 1996, Müller-Enbergs, 2008). We discuss the implications for our identification strategy in Subsection 4.3, paragraph "Correlated District Discontinuities".

Unofficial Informers. Each district office had full authority over the county offices (*Kreisdienststellen*) and on-site offices (*Objektdienststellen*) within their territory.[11] In total, there were 209 county offices, which executed the commands and orders from their respective district office and recruited and administered Stasi informers. These informers were instructed to secretly collect information about individuals in their own network. It was thus necessary for informers to pursue their normal lives as friends, colleagues, and neighbors. To report suspicious behavior, informers secretly met with their responsible Stasi officer on a regular basis.

The process of informer recruitment was almost exclusively demand-driven as informers were selected by a Stasi official. Individuals that approached the Stasi to volunteer were generally not accepted (Müller-Enbergs, 1996). Reasons for cooperating with the Stasi were diverse. Some citizens complied for ideological reasons, others were attracted by personal benefits (e.g., with regard to their regular job, see Müller-Enbergs, 2013). Only in very rare cases, citizens were compelled to act as unofficial informers (Fulbrook, 2005, p. 242f.).

With the collected intelligence at hand, the Stasi was able to draw a detailed picture of anti-socialist and dissident movements within the society and to exert an overall "disciplinary and intimidating effect" on the population (Gieseke, 2014, p. 84f.). Numerous historical accounts suggest that the population was aware of the large network of informers: according to Bruce (2010), the vast majority of citizens had direct contact with the Stasi multiple times throughout their lives; Reich (1997) describes that citizens felt the Stasi's presence like a "scratching T-shirt"; Fulbrook (1995) states that friendships inevitably had a shadow of distance and doubt; Wolle (2009) writes that the threat of being denounced caused an atmosphere of mistrust and suspicion within a deeply torn society. "The very knowledge that the Stasi was there and watching served to atomize society, preventing independent discussion in all but the smallest groups" (Popplewell, 1992). The consequence was "the

[11] On-site offices were separate entities in seven strategically important public companies or universities. The Stasi only monitored economic activity but was not actively involved in production (Gieseke, 2014).

breakdown of the bonds of trust between officers and men, lawyers and clients, doctors and patients, teachers and students, pastors and their communities, friends and neighbors, family members and even lovers" (Childs and Popplewell, 1996, p. 111). The preferred method of the Stasi was "to build up and propagate distorted stories with enough kernel of truth to sow suspicion and discredit the individual" (Fulbrook, 1995, p. 54), eventually destroying relationships, reputations, and careers.

The gathered intelligence also served as a basis for further actions by the regime, such as arrests and imprisonments for political reasons or the use of physical violence (see below for more details). Moreover, historical evidence shows that the spying activities led to other forms of non-persecutive, yet perceivable and important real-life consequences: among others, students suspected of anti-regime behavior/attitudes were denied the opportunity to study at the university, employees and workers were not promoted or even dismissed (Bruce, 2010, p. 103f.).[12] Importantly, the regime did not only sanction direct dissident behavior, but also followed the principle of collective punishment. As a consequence, family members of regime critics or dissidents regularly got into trouble as well.

Measuring Government Surveillance. As the Stasi saw unofficial collaborators as their main instrument, we choose the county-level share of informers in the population as our preferred measure of government surveillance. Although the Stasi was able to destroy parts of its files in late 1989, much of the information was preserved when protesters started to occupy Stasi offices across the country. In addition, numerous shredded files have been restored since reunification by the Stasi Records Agency (BStU) – a government agency established in 1990/1991 to safe-keep and secure the Stasi Records and to provide citizens, researchers, and media access to these files. Our data on the number of unofficial informers in each county are based on these official records. Most of the data have been compiled in Müller-Enbergs (2008). Until today, the Stasi Records Agency keeps restoring old files and releasing new data and information. Hence, we were able to extend the information in Müller-Enbergs (2008) with additional data for previously unobserved counties which we collected from the archives of the Agency.[13] Overall, this allows us to observe the spying density for around 92 percent of the counties at least once in the 1980s.

The Stasi officially differentiated operative collaborators (*IM1*) from collaborators providing logistics (*IM2*).[14] Our baseline measure of the county-level spying density is based on the number of operative collaborators as these informers were actively involved in spying, constituted the largest and most relevant group of collaborators, and exhibit the best data coverage across counties.[15] If an on-site office was located in a county, we add the respective number of informers to the county total and explicitly control for the presence of these on-site offices in the econometric analysis. As

[12] For more popular representations of the impact of the Stasi, see the Academy Award-winning movie "The Lives of Others" and the TED talk "The Dark Secrets of a Surveillance State" given by the former director of the Berlin-Hohenschönhausen Stasi prison memorial, Hubertus Knabe.

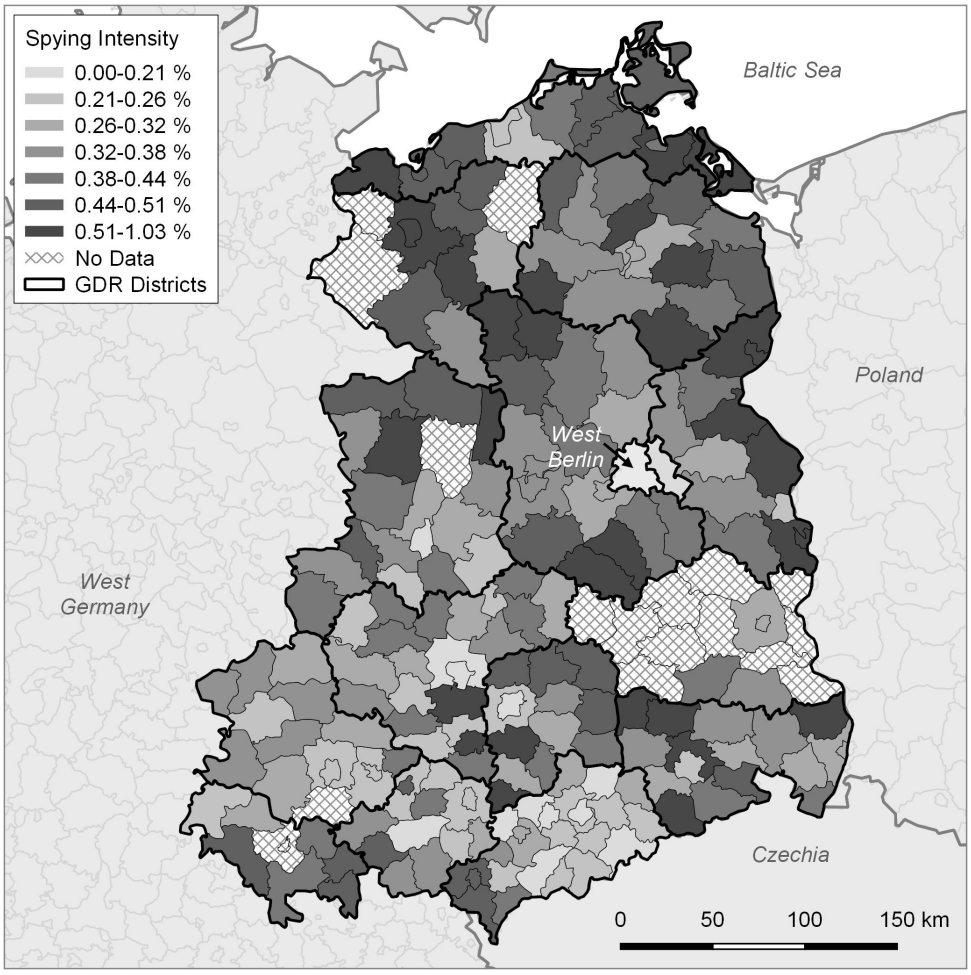
[13] The available data is exhaustive. The BStU recovers all available documents for one county office before moving to the next one. Pre-1980 data are only available for a limited number of counties.

[14] In some of the Stasi's informer accounts, there is a third category called "societal collaborators". These individuals were publicly known to be loyal to the regime and usually not involved in spying. Rather, these collaborators were asked to actively and openly support the Stasi and the regime (Kowalczuk, 2013). In this sense, they were less secret than official Stasi employees who oftentimes disguised their connections to the regime.

[15] Nonetheless, we show that results are robust when combining both categories, hence using the total number of spies as our main regressor.

information on the total number of collaborators is not given for each year in every county, we use the average spying density from 1980 to 1988 as our measure of surveillance. The spying density was stable over the 1980s, the within-county correlation being 0.91. For further details on our main explanatory variable, see Data Appendix A. As operative informers were the central weapon of the surveillance system, this measure is arguably the best proxy to pick up the effect of the Stasi as a whole. By definition, this overall effect also comprises the specific *modus operandi* of the Stasi, i.e., using informers within social networks. We discuss and test the quality of our measure of surveillance in Subsections 4.3 and 5.2.

Figure 1: Spying Intensity across Counties



Notes: This figure shows the county-level surveillance density measured by the average yearly share of operative unofficial informers relative to the population between 1980 and 1988.
Source: See Data Appendix A. *Maps:* MPIDR and CGG (2011) and @EuroGeographics.

Figure 1 plots the regional variation of surveillance intensity, darker colors indicating higher spying densities. The surveillance intensity differs considerably both across and within GDR districts. The share of operative informers in a county ranges from 0.12 to 1.03 percent, the mean density being 0.38 percent (see Appendix Table A.2 for more detailed distributional information). The median is similar to the mean (0.36 percent), and one standard deviation is equal to 0.14 informers per capita. In our regressions, we standardize the share of informers by dividing it by one standard deviation in

the respective sample.

3 Conceptual Framework and Related Literature

Autocratic regimes generally secure their power by establishing a system of obedience through the creation of fear and the constant threat of denunciation (Arendt, 1951). In the example of the GDR and its ubiquitous surveillance state, the aforementioned historical accounts reiterate this mechanism, suggesting that the Stasi had a strong impact on people's social behavior as informers intruded deep into the private spheres of the East German population (see, e.g., Fulbrook, 2009). Given the historical context, it thus seems plausible that the repressive political environment shaped citizens' attitudes towards political institutions and affected the way citizens cooperated with and trusted each other.

Against this background, we study the effect of the surveillance state on civic capital, defined as "those persistent and shared beliefs and values that help a group overcome the free rider problem in the pursuit of socially valuable activities" (Guiso et al., 2010). This definition emphasizes "values and beliefs, which are shared by a community and persistent over time, often passed on to its member through intergenerational transmissions, formal education, or socialization" (Guiso et al., 2010). We focus on civic capital for three reasons. First, the definition narrows the concept of social capital, which sometimes lacks precision (Solow, 1995), to norms and beliefs that help a community solve collective action problems. These norms can be observed at the individual level by measures of trust and cooperative behavior, as well as by cooperative behavior in the socio-political context, for instance political engagement (Guiso et al., 2010). We select our main outcomes along these lines (cf. Subsection 4.1). Second and relatedly, the concept fits our historical setting well: the ample qualitative evidence discussed in Section 2 suggests that Stasi surveillance shattered individual trust, citizens' confidence in state institutions and political leadership, and led to a withdrawal from society. Third, higher levels of civic capital have a direct economic payoff and can be incorporated in standard economic models (Tabellini, 2008, Guiso et al., 2008).

Interpersonal trust as one key element of civic capital has long been seen as an important economic preference which shapes economic outcomes (Almlund et al., 2011), given that every economic transaction involves an element of mutual confidence (Arrow, 1972). Among others, the importance of trust for economic performance becomes apparent when thinking about transactions that involve future payments or imperfect monitoring of performance, e.g., in an employer-employee relationship (Knack and Keefer, 1997). This role of trust as an "economic primitive" has been well documented in various studies in behavioral economics, which demonstrate that trust fosters reciprocal behavior and cooperation, and thereby leads to more efficient economic outcomes (see, e.g., Berg et al., 1995, Dohmen et al., 2009). In addition to the direct effects of interpersonal trust and cooperative norms on economic performance, trust may also indirectly shape economic activity through the political process. As argued by Knack and Keefer (1997), less cooperative, more self-interested individuals are less likely to vote, and thus monitor politicians to a lesser extent. This could, in turn, result in a lower quality of economic policies, which eventually triggers negative effects on economic performance. Consequently, electoral turnout is a widely used outcome-based measure

of social capital (see, e.g., Putnam, 1993, Guiso et al., 2004, 2010). Note that in the context of our setting, the theoretical predictions on the effect of surveillance on institutional trust, and in particular turnout, are ambiguous. On the one hand, it is possible that individuals lost trust in the political class, independent of the ideology. On the other hand, it might well be the case that surveillance increased individuals' dissatisfaction with the Socialist regime and had a positive effect on electoral participation post reunification, for instance, to prevent another Socialist episode. Eventually, the effect on turnout and political engagement is thus an empirical question.

Building on previous evidence, the negative effects of Stasi spying on individual trust/cooperative norms and – potentially – institutional trust are further expected to be accompanied by negative economic effects. Various studies have investigated and confirmed the link between trust and economic performance in other contexts (see Algan and Cahuc, 2014, for a detailed survey of the literature). Knack and Keefer (1997) and Zak and Knack (2001) document a positive correlation between trust and economic indicators across countries. In two related papers, Nunn (2008) and Nunn and Wantchekon (2011) show that transatlantic and Indian Ocean slave trade still has a causal and persistently negative effect on current trust levels and economic performance in Africa. Algan and Cahuc (2010) isolate the trust that US descendants inherited from their forebears who had immigrated from different countries at different dates and show that variation in inherited trust impacts economic growth in the respective countries of origin. In a series of papers, Guiso et al. (2006, 2009) exploit variation in deep cultural aspects, such as religious affiliation, to explain trust levels, which in turn have real economic effects. Similarly, Tabellini (2010) exploits variation in literacy and (the quality of) political institutions at the end of the 19th century to explain trust levels and regional economic development across European countries in the 1990s. In a more recent study, Butler et al. (2016) demonstrate this relationship at the individual level, showing that too little (but also too much) individual trust exhibits negative effects on individual income.

4 Data and Research Design

In the following, we first describe the data used for the empirical analysis (Subsection 4.1). In Subsection 4.2, we develop our research design and set up the empirical model. In Subsection 4.3, we provide an extensive discussion of potential challenges to identification and a set of identification tests to corroborate our empirical strategy.

4.1 Data

To estimate the effect of surveillance on trust and economic performance, we use the German Socio-Economic Panel Study, a longitudinal survey of German households (Wagner et al., 2007, SOEP, 2015). Established for West Germany in 1984, the survey covers respondents from the former GDR since June 1990. We focus on all East German respondents (below retirement age of 65) in this first wave and follow them over time. This allows us to assign treatment (i.e., the spying density) based on the respondents' county of residence in the year before the fall of the Berlin Wall and observe respondents even when they changed residence post reunification.

Main Outcomes. We proxy individual trust and cooperative behavior by the following variables provided in the SOEP: trust in strangers [measured in 2003 and 2008] (Glaeser et al., 2000, Naef and Schupp, 2009); reciprocal behavior [2005, 2010] (Dohmen et al., 2009); the intention to attend elections [2005, 2009] and general political interest [1990 to 2010] as an alternative more frequently measured proxy for voting behavior (Putnam, 2000, Guiso et al., 2004, Rodenburger, 2018); political engagement [1990–2010, with gaps] (Guiso et al., 2010). We further focus on three measures of economic performance. First, we use log mean income between 1991–2010. Second, we calculate the total unemployment duration for each individual over this period. Third, we calculate the individual probability to become self-employed within the survey period. Besides these main measures of civic capital and economic performance, we use a range of other outcome variables to test for alternative channels (cf. Subsection 5.3) and analyze potential underlying mechanisms (cf. Subsection 5.4). See Appendix Tables A.1 and A.2 for more information on each outcome variable.

Controls. In our empirical model, we include control variables at the individual and county level (vectors $\boldsymbol{X_i}$ and $\boldsymbol{H_c}$ in equation (1) introduced below). All specifications control for the respondents' age and gender as well as the presence of an on-site office in a given county. We abstain from controlling for additional covariates at the individual level such as marital status, household size, or education as these variables might have been shaped by state surveillance. We investigate the effects of Stasi surveillance on education in Subsection 5.4. At the county-level, we construct three sets of control variables. First, we account for the *size and demographic composition* of the counties in the 1980s. The corresponding set of controls comprises (i) a county's surface area (in logs), (ii) the log mean county population 1980–1988, (iii) the share of children and pensioners as of September 30, 1989, and (iv) whether the county is rural or urban (*Stadt-/Landkreis*).[16] Second, we account for differences in the *sectoral composition* of counties. The set of industry controls comprises (i) the respective shares of employees in the agricultural, energy/mining and textile industry as of September 1989, i.e., the industries that played a major role when the regime decided on how to draw the new district borders in 1952 (cf. Section 2), (ii) the employment share of cooperative members, and (iii) the goods value of industrial production in 1989 (in logs). Third, we control for historical/pre-determined and potentially persistent county differences in terms of economic performance and political ideology. The set of *historical controls* comprises (i) the regional strength of the opposition as proxied by the intensity of the uprising in June 1953 (cf. Section 2); (ii) the electoral turnout as well as the Nazi and Communist vote shares in the federal election of March 1933 to measure institutional trust and the level of political extremism (Voigtländer and Voth, 2012), (iii) the regional share of Jews and Protestants in 1925 in order to control for religious differences (Becker and Wößmann, 2009), and (iv) the unemployment rate, the share of white-collar and the share of self-employed workers in 1933 as proxies for persistent productivity differences across local labor markets.

Summary Statistics. Summary statistics for all outcomes and controls on the individual and county level are presented in Table A.2 in the Appendix.

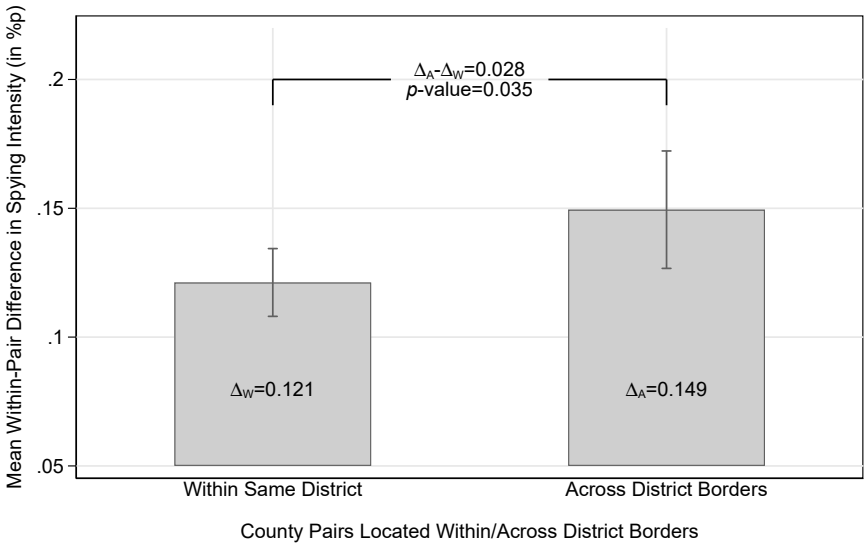
[16] Controlling for surface area and population accounts for population density. The rural/urban dummy is intended to pick up additional differences between independent cities (*Stadtkreise*) and so-called rural counties (*Landkreise*) that consist of many municipalities, typically with one larger city, which is the capital of the respective county.

4.2 Research Design

Our identification strategy exploits the administrative structure of the Stasi, where district offices bore the full responsibility for securing their territory and administered different average levels of the informer density at the county level. As a result, district fixed effects explain more than a quarter of the county-level variation in the spying density. We harness the resulting discontinuities along district borders as a source of exogenous variation and set up a border design to derive causal effects. Intuitively, we compare county pairs that straddle a district border and use differences in the spying density within these pairs to identify the effect of government surveillance on our respective outcomes (see, e.g., Holmes, 1998, Dube et al., 2010, for studies applying similar research designs).[17]

A precondition for the validity of this design is that there is meaningful variation in the policy variable within county pairs at district borders. Figure 2 visualizes this variation by plotting the average differences in the spying density among neighboring counties, contrasting within and across-district variation. The figure shows that differences are significantly larger in county pairs that straddle a district border.

Figure 2: Differences in Spying Intensity Within/Across Districts



Notes: This figure plots the average difference in the share of operative unofficial informers at the county level within (i) county pairs from the same district and (ii) county pairs divided by district borders. Additionally, we test for the difference between both estimates being zero and depict the corresponding *p*-value. County pairs are weighted by the average county-level population. Standard errors are clustered at the county-pair level, vertical bars show 95 % confidence intervals. See Data Appendix A for detailed information on all variables.

Technically, we limit the analysis to contiguous counties that straddle a GDR district border.[18] In case of multiple neighbors, we only consider the neighboring county that shares the longest border

[17] Our border design is related but different from a spatial regression discontinuity design (RDD) as, e.g., used in Dell (2010), where a two-dimensional discontinuity (longitude and latitude) is exploited and a well-defined treatment border is approached. In our setting, the border design is preferable since there are many treatment borders, such that there is not much of a hinterland that can be used to approach the border. Figure 1 shows that the hinterland of one district border is oftentimes the border region of another treatment border.

[18] We discard East Berlin from our analysis as it was both a county and district on its own.

with the respective county and discard pairs with very short common borderlines (less than 2 km). We show below that results are robust to alternative county pair definitions (see Subsection 5.2 and Appendix Table C.2.2). On this sample, we regress outcome Y (see Subsection 4.1 for a detailed list of outcomes) of individual i in county c, which is part of a border county pair b and situated in the former Weimar province p and district d, on the spying density in county c and county-pair dummies ν_b. Including only the subscript for the level at which the respective variables vary, we formally estimate the following equation:

$$Y_i = \alpha + \beta \cdot SPYDENS_c + \boldsymbol{X_i}\boldsymbol{\delta} + \boldsymbol{H_c}\boldsymbol{\phi} + \nu_b + \mu_p + \varepsilon_i. \quad (1)$$

In addition to county-pair fixed effects, our preferred model includes sets of covariates at the individual and county level, denoted $\boldsymbol{X_i}$ and $\boldsymbol{H_c}$, respectively. At the individual level, we control for the age and gender of the respondents. County-level covariates account for the above mentioned systematic factors determining the surveillance strategy and include controls for differences in size and demography, industrial composition, as well as pre-treatment differences in terms of economic performance and political ideology (see Subsection 4.1 for a detailed description of control variables). We also include a set of dummy variables indicating pre-World War II provinces from the Weimar Republic, denoted μ_p, which accounts for long-term cultural differences, e.g., between Prussia and Saxony, in a non-parametric way (see Figure B.3 for a mapping of GDR districts into provinces from the times of the Weimar Republic). In addition, all regressions include a dummy for the presence of an on-site office (cf. Section 2). The error term is denoted by ε_i.

As discussed in Subsection 4.1, we observe some of the civic capital outcomes in two waves only (trust, reciprocity, and attend elections).[19] In these cases, we follow Alesina and Fuchs-Schündeln (2007) and pool observations from both waves and add year fixed effects – results are robust when allowing for differential treatment effects by survey wave or taking the mean outcome over time, see Appendix Table C.2.1. In contrast, political interest, engagement in politics, and all economic outcomes are observed over (almost) the full sample period from 1990 to 2010. For those variables, we use the mean value over time. In addition, we present a dynamic specification, in which we investigate the effect of Stasi surveillance over time, interacting all control variables and fixed effects given in equation (1) with year dummies.

In our baseline specification, standard errors are two-way clustered at the county-pair and county-in-1990 level to (i) allow for shocks affecting county pairs, and (ii) account for the duplication of some counties in our preferred specification that leads to multiple person-years observations in our sample. We provide a more detailed discussion of alternative ways to calculate standard errors, such as clustering at the district level, and demonstrate the robustness of our inference in Subsection 5.2.

4.3 Identification

Equation (1) describes a standard border design that exploits variation within county pairs. The identifying assumption is that a given county on the lower-spying side of a district border is similar

[19] This is also true for some outcomes used in the sensitivity checks such as risk aversion, the Big Five personality traits, or preferences for redistribution.

to its neighboring county on the higher-spying side in all relevant characteristics except for the spying density. If this is fulfilled, the remaining source of systematic variation is induced by district-level differences in surveillance strategies, and our estimates capture the causal effect of the spying density. However, several endogeneity concerns arise in this context, which could invalidate the design.

Within-Pair Confounders. One prime concern for identification is that unobservable confounders within county pairs might drive parts of the county-level spying density. We address this potential omitted variable bias problem in two ways. First, we explicitly use the fact that districts held full responsibility for securing their territory and guiding the respective county offices. Based on this insight, we strengthen identification by combining the border design with an instrumental variables (IV) approach. Using the district-level leave-out-average spying density as an instrument for the county-level density, we isolate the district-level variation in the county-level spying density and use it for identification within county pairs at district borders. The corresponding first-stage equation for individual i is then given by:

$$SPYDENS_c = \tilde{\alpha} + \tilde{\zeta} \cdot \frac{1}{|\mathbb{C}^d_{-c}|} \sum_{k \in \mathbb{C}^d_{-c}} SPYDENS_k + \boldsymbol{X_i}\tilde{\boldsymbol{\delta}} + \boldsymbol{H_c}\tilde{\boldsymbol{\phi}} + \tilde{\nu}_b + \tilde{\mu}_p + \nu_i, \tag{2}$$

where county c's district-level leave-out-average spying density is defined as the mean spying density in district d, excluding county c's contribution to this mean. Instead of the leave-out average density, we also estimate equation (2) using the simple district average spying density $\frac{1}{|\mathbb{C}^d|}\sum_{k \in \mathbb{C}^d} SPYDENS_k$.

Second, we directly test whether observable county-specific characteristics differ at district borders within pairs. Applying a standard covariate smoothness test for discontinuity designs as suggested by Lee and Lemieux (2010), we separately regress each county-level control variable as defined in Subsection 4.1 on the county-level spying density. The coefficient provides a direct test of whether the respective covariate is unrelated to the informer density. Appendix Table C.1.1 shows step-by-step how our identification strategy is able to balance covariates. In column (1), we investigate the smoothness of covariates in the full sample of all East German counties covered by the SOEP and see that the spying density is significantly correlated with most of our covariates. In column (2), we restrict the sample to counties at district borders but do not include county-pair fixed effects. Hence, we still compare counties which are far away from each other and might differ in many other dimensions than the spying intensity. Again, we detect systematic differences in observables and the joint F-test of all estimated coefficients being zero, reported at the bottom of Table C.1.1, is rejected. In specification (3), we eventually begin to restrict comparisons of distant counties by introducing Weimar province fixed effects, which substantially improves the balance of our sample, yet a few significant differences persist. In a last step, we implement our border design by introducing county-pair fixed effects, explicitly testing the smoothness of covariates within county pairs at district borders. In specification (4), none of the control variables turns out to be significant, which suggests that our research design is able to balance the sample.

Reverse Causality. A related concern is reverse causality. Differences in the county-level spying density might have been due to historical but still-prevailing differences in trust or economic

performance across counties. While our instrumental variables strategy – exploiting variation in the spying density due to differences in district-level surveillance strategies – addresses reverse causality concerns, we can additionally conduct a direct test for reverse causality in our border design. Using county-level proxies from the 1920s and 1930s for our set of civic capital and economic performance outcomes, we run our empirical model described in equation (1) at the county level.[20] Table 1 provides the corresponding results. Overall, surveillance intensity cannot explain differences in pre-treatment outcomes within county pairs, irrespective of using control variables from the times of the GDR or not.

Table 1: The Effect of Spying on Historical Outcomes

	Share Protest. (1)	Share Jews (2)	Voter Turnout (3)	Extreme Vote (4)	Unemp-loyment (5)	Self-Employ. (6)	White Collar (7)
Panel A – Without Control Variables							
County-Level Spying Density	0.003	0.217	-0.057	-0.001	0.161	0.083	0.178
	(0.138)	(0.209)	(0.201)	(0.171)	(0.219)	(0.173)	(0.205)
Number of Observations	102	102	102	102	102	102	102
Adjusted *R*-Squared	0.611	0.931	0.904	0.768	0.923	0.918	0.771
Panel B – Including GDR Control Variables							
County-Level Spying Density	-0.115	0.168	-0.047	0.006	0.143	0.076	0.048
	(0.263)	(0.197)	(0.172)	(0.211)	(0.183)	(0.166)	(0.165)
Number of Observations	102	102	102	102	102	102	102
Adjusted *R*-Squared	0.759	0.969	0.957	0.857	0.969	0.963	0.887

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on different measures of local civic capital and economic performance before the existence of the GDR (in the 1920s and 1930s). The underlying econometric model is described in equation (1), estimated at the county level. Each specification includes dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, and county-pair fixed effects. Panel A presents results in the absence of any additional covariates. Panel B displays the corresponding results when controlling for the size and demographic/industrial composition of counties in the 1980s, as well as the strength of the riot in June 1953 (see Section 4.1 for details). All outcome variables are standardized. All specifications are based on the sample of contiguous county pairs that straddle a GDR district border and are covered in the SOEP. Population weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Correlated District Discontinuities. Another threat arises due to district discontinuities that might be systematically correlated with the district-level spying density. Our IV design would not tackle this type of endogeneity because the unobserved confounder would operate at the same level as the instrument. As long as we observe the potential district-level confounders at the *county level*, we can, however, test for smoothness within county pairs and control for systematic differences if necessary. As discussed in Section 2, the overarching goal of the regime when delineating districts was to establish regional economic equality in productive forces. While this does not necessarily lead to discontinuities within county pairs at district borders, we test for such differences using county-level

[20] Using election data from March 1933, we observe electoral turnout and vote shares for the extreme right (the Nazi party, NSDAP) and the extreme left (Communist party, KPD). We proxy interpersonal trust with the share of Protestants and Jews in 1925, two religious groups that have been shown to exhibit higher levels of trust compared to Catholics (Guiso et al., 2003). We also observe the county-level unemployment rate and the share of self-employed in the population as of 1933. Last, we use the share of white-collar workers in 1933 as a potential proxy for economic development.

industrial output and the number of workers as measures of economic activity. Table C.1.1 shows that these variables are smooth in our border design. The other relevant factor which might induce a discontinuity was the regime's goal to create industry clusters in certain districts. Using detailed information on the industrial composition of the workforce, we show that sector-specific worker shares are smooth within county pairs at district borders (cf. Appendix Figure B.4). Nevertheless, we control for employment shares in the agriculture, energy/mining, and textile industry – those three sectors for which clusters were to be formed (see Section 2 and Werner et al., 2017) – in our regressions. Moreover, we discussed a list of hard, district-level factors, such as population size, oppositional strength, and again industrial composition, that have been assumed to influence the district-level surveillance strategy (see Section 2). Table C.1.1 shows that these potential confounders are smooth within county pairs at district borders. Last, other correlated district policies might threaten identification. Importantly and as extensively discussed in Section 2, districts had no legislative competencies. From the very beginning, the GDR followed the Leninist principle of Democratic Centralism, in which all legislative power accrued to the central level (Schulze, 1991). In this respect, the Council of Ministers, as the chief executive body of the GDR, ensured that all decisions made by the Central Committee were unconditionally implemented and executed at lower regional levels.

Selection Out of Treatment. Selection effects pre and post-reunification could further invalidate our research design. While out-migration was very limited after the construction of the Berlin Wall (cf. Section 2), residential mobility *within* the GDR was also highly restricted as all living space was tightly administered and allocated by municipal housing agencies (Grashoff, 2011, p. 13f.). Post reunification, we assign treatment based on the county of residence in 1989 and follow individuals over time, also when they change residency. In Subsection 5.4, we investigate the decision to move after reunification as one potential channel driving our effects on civic capital and economic outcomes. Results show that surveillance-induced mobility responses are of little importance.

Measurement Error. Last, our proxy of surveillance intensity may not translate into differences in people's awareness of the Stasi. While we cannot directly test for differences in awareness during the times of GDR, we can do so post reunification. Since 1992, any citizen has been able to file a request to view her or his personal Stasi file. We acquired official data on the total number of individual requests for disclosure (see Figure B.5 for the evolution of these requests over time) at the district level from the Stasi Records Agency; unfortunately, county-level information is not available. As shown in Panel A of Appendix Figure B.6, we find a positive correlation between the per-capita number of individual requests in a district and the corresponding district-level spying density. However, as this finding is not derived from our border design model, we cannot attribute any causal interpretation to it. For example, it might be true that the observed correlation is driven by district-level differences in anti-regime attitudes that positively affected the spying density and the number of file requests. We test this argument in Panel B of Appendix Figure B.6, where we plot the respective correlation when controlling for the district-level number of exit visa applications as of December 31, 1988 and the date the district experienced the first protest during the peaceful revolution of 1989 – two measures of

anti-regime sentiment (Kern and Hainmueller, 2009, Grdešić, 2014). Controlling for these two proxies leads to a stronger positive correlation between the spying density and the number of disclosure requests, a finding we interpret as additional suggestive evidence that citizens perceived differences in surveillance intensities.

Moreover, we could face measurement error in the main regressor if (i) informers recruited by one county collected information on individuals located in the neighboring county within the same county pair, or (ii) there was a quantity-quality trade-off in terms of unofficial collaborators. While we cannot rule out these mechanisms, both would work against finding large effects and bias our estimates towards zero.

Sign of Bias. While it would be interesting to formulate a clear *ex ante* hypothesis about the sign of the endogeneity bias, the nature of endogeneity concerns discussed above prevents us from doing so. For instance, when looking at reverse causality, the direction of bias depends on whether the Stasi allocated more or less spies to counties with historically low levels of trust. If low regional trust implies non-conformity with the political system, the Stasi may have allocated more spies to low-trusting counties and simple OLS regressions would provide an overestimate of the effect of surveillance on trust. However, if low trust implies that regions were less economically vibrant and less in favor of free markets *ceteris paribus*, the Stasi might have allocated less spies to the specific counties and we would underestimate effects. Hence, a prediction of the sign of the bias is *ex ante* ambiguous. The same holds true for other endogeneity concerns discussed in Subsection 4.3 such that the direction of bias remains an empirical question. Our step-wise implementation of the research design (cf. Subsection 5.1) suggests *ex post* that endogeneity leads to a downward bias of our estimates.

5 Empirical Results

In the following, we present our empirical findings. Subsection 5.1 presents the main results. In Subsection 5.2, we provide a range of tests to demonstrate the robustness of our effects. In Subsection 5.3, we test whether alternative mechanisms may drive (parts of) our results. Last, we investigate the channels behind our baseline effects in Subsection 5.4.

5.1 Main Results

In this subsection, we analyze the effect of spying on our measures of civic capital and economic performance, applying the border design and combining it with our instrumental variables approach as set up in equations (1) and (2). Tables 2 and 3 summarize our main findings. In order to demonstrate the relevance of our identification strategy, we implement the research design step-by-step, starting in column (1) with the naive OLS correlation for all counties and ending with the border-IV design specification in column (6). The latter specification will be our preferred one throughout the rest of the paper.

Overall, Table 2 shows significantly negative effects of surveillance on our measures of civic capital. We find that a one standard deviation increase in the spying density reduces individuals' trust in

strangers by 0.098 of a standard deviation (Panel A, column (6)), and reciprocal behavior by 0.183 of a standard deviation (Panel B). Panel C further shows that a one standard deviation increase in the spying density decreases individuals' probability to attend elections by 0.109 of a standard deviation, corresponding to a decrease of 4.5 percentage points (or 5.6 percent relative to the mean). Likewise, a standard deviation increase in the spying density lowers individuals' overall political interest and political engagement by 0.261 and 0.181 of a standard deviation, respectively (cf. Panels D and E).

Table 3 summarizes the main results for our measures of economic performance. Panel A shows that a one standard deviation increase in the spying intensity increases individual unemployment duration by 1.4 percentage points. We show below that the probability of unemployment is significantly affected, too (see Figure 3). Panel B further shows that more intense government surveillance decreases individuals' probability to become self-employed by 1.6 percentage points, a finding in line with evidence that trust is an important asset for entrepreneurs (Knack and Keefer, 1997). Last, we present the effect of government surveillance on labor/self-employment income conditional on working in Panel C of Table 3. We find that a one standard deviation increase in the spying density decreases monthly income by 0.056 log points (or 84 EUR) on average. Comparing this estimate to evidence from the returns to schooling literature, our result suggests that a one standard deviation increase in Stasi surveillance had the same effect on income as 0.6 years less of schooling (cf. Card, 1999).[21] We show below that educational attainment is a key driver of the economic effects (see Subsection 5.4).

Overall, our results indicate that Stasi surveillance affected individuals' economic performance at both the extensive and the intensive margin, i.e., conditional on working. In Appendix Table C.4.1, we provide some additional evidence for this pattern. We show that effects on income are larger when not conditioning on employment (compare columns (1) and (2) to column (3)). Moreover, while Stasi surveillance has no effect on individuals' choice of being in the labor force (column (4)), we find that a higher spying density has significantly negative effects on the probability of being employed (columns (5)-(7)). Last, column (8) suggests that there is also a negative effect of surveillance on working hours: a one standard deviation increase in the spying density tends to decrease working time conditional on employment by 0.251 hours (0.7 %). Note that the average effect on working hours is not significant at conventional levels; however, we find significant results for individuals born between 1960 and 1973 (not reported).

[21] The survey by Card (1999) suggests that the OLS coefficient on the returns to schooling is about 0.1 log points and close to estimates obtained when applying quasi-experimental research designs. We confirm the survey's OLS results using the SOEP, finding a returns to schooling estimate of about 0.1 for West Germany.

Table 2: The Effect of Spying on Civic Capital

	All Counties	Border County-Pair Sample				
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Trust in Strangers						
County-Level Spying Density	0.066**	0.057	-0.040	-0.091***		-0.098***
	(0.032)	(0.038)	(0.028)	(0.023)		(0.034)
District-Level Spying Density					-0.094**	
					(0.038)	
Number of Observations	3,175	1,795	1,795	1,795	1,795	1,795
Adjusted R-Squared	0.008	0.031	0.117	0.149	0.147	0.149
Kleibergen-Paap F-Statistic						12.03
Panel B – Reciprocal Behavior						
County-Level Spying Density	-0.067*	-0.098**	-0.109***	-0.085**		-0.183**
	(0.034)	(0.045)	(0.038)	(0.032)		(0.069)
District-Level Spying Density					-0.178***	
					(0.044)	
Number of Observations	2,835	1,588	1,588	1,588	1,588	1,588
Adjusted R-Squared	0.053	0.065	0.141	0.185	0.187	0.181
Kleibergen-Paap F-Statistic						15.40
Panel C – Attend Elections						
County-Level Spying Density	-0.009	-0.081**	-0.067***	-0.087***		-0.109**
	(0.031)	(0.036)	(0.024)	(0.032)		(0.052)
District-Level Spying Density					-0.107**	
					(0.044)	
Number of Observations	2,828	1,583	1,583	1,583	1,583	1,583
Adjusted R-Squared	0.014	0.048	0.105	0.122	0.121	0.121
Kleibergen-Paap F-Statistic						14.68
Panel D – Political Interest						
County-Level Spying Density	-0.091***	-0.078*	-0.120***	-0.179***		-0.261***
	(0.028)	(0.045)	(0.035)	(0.026)		(0.069)
District-Level Spying Density					-0.270***	
					(0.043)	
Number of Observations	2,914	1,736	1,736	1,736	1,736	1,736
Adjusted R-Squared	0.036	0.047	0.113	0.152	0.149	0.149
Kleibergen-Paap F-Statistic						19.12
Panel E – Political Engagement						
County-Level Spying Density	0.051*	0.008	-0.066**	-0.096***		-0.181***
	(0.028)	(0.041)	(0.029)	(0.022)		(0.047)
District-Level Spying Density					-0.188***	
					(0.034)	
Number of Observations	2,914	1,736	1,736	1,736	1,736	1,736
Adjusted R-Squared	0.019	0.043	0.102	0.124	0.126	0.121
Kleibergen-Paap F-Statistic						19.12
Border County-Pair Fixed Effects			Yes	Yes	Yes	Yes
County-Level Control Variables				Yes	Yes	Yes

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on different measures of individual civic capital (see panels). The underlying econometric model is described in equations (1) and (2). In column (1), we present simple correlations between the county-level spying density and the corresponding outcome when using the full sample of counties. In columns (2)-(6), we limit the sample to contiguous county pairs that straddle a GDR district border. Column (2) shows the corresponding simple correlations for this sample. In columns (3) and (4), we present results based on our border design. In columns (5) and (6), we combine the border design with our instrumental variables strategy. Column (5) presents the reduced-form effect of the instrument, the leave-out average district-level spying density. Column (6) shows the respective second-stage results. All outcome variables are standardized. All specifications include dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, and control variables for the individuals' age and gender (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table 3: The Effect of Spying on Economic Performance

	All Counties	Border County-Pair Sample				
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Unemployment Duration						
County-Level Spying Density	0.005	0.002	0.004	0.008*		0.014***
	(0.004)	(0.009)	(0.006)	(0.005)		(0.005)
District-Level Spying Density					0.014**	
					(0.006)	
Number of Observations	2,880	1,719	1,719	1,719	1,719	1,719
Adjusted *R*-Squared	0.041	0.049	0.135	0.161	0.161	0.161
Kleibergen-Paap *F*-Statistic						20.81
Panel B – Self-Employment Probability						
County-Level Spying Density	0.000	-0.001	-0.008*	-0.008**		-0.016**
	(0.005)	(0.008)	(0.004)	(0.004)		(0.007)
District-Level Spying Density					-0.016***	
					(0.005)	
Number of Observations	2,724	1,611	1,611	1,611	1,611	1,611
Adjusted *R*-Squared	0.014	0.025	0.080	0.094	0.094	0.093
Kleibergen-Paap *F*-Statistic						18.76
Panel C – Log Mean Income						
County-Level Spying Density	-0.041***	-0.015	-0.030**	-0.044***		-0.056***
	(0.014)	(0.017)	(0.011)	(0.013)		(0.019)
District-Level Spying Density					-0.055**	
					(0.026)	
Number of Observations	2,517	1,482	1,482	1,482	1,482	1,482
Adjusted *R*-Squared	0.163	0.184	0.234	0.253	0.251	0.253
Kleibergen-Paap *F*-Statistic						16.80
Border County-Pair Fixed Effects			Yes	Yes	Yes	Yes
County-Level Control Variables				Yes	Yes	Yes

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on different measures of individual economic performance (see panels). The underlying econometric model is described in equations (1) and (2). In column (1), we present simple correlations between the county-level spying density and the corresponding outcome when using the full sample of counties. In columns (2)-(6), we limit the sample to contiguous county pairs that straddle a GDR district border. Column (2) shows the corresponding simple correlations for this sample. In columns (3) and (4), we present results based on our border design. In columns (5) and (6), we combine the border design with our instrumental variables strategy. Column (5) presents the reduced-form effect of the instrument, the leave-out average district-level spying density. Column (6) shows the respective second-stage results. All specifications include dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, and control variables for the individuals' age and gender (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Identification. Our results become stronger when implementing the identification strategy step-by-step. While columns (1) and (2) of Tables 2 and 3 provide naive raw correlations between our measure of government surveillance and the respective outcomes in the full and border pair sample, we start tightening identification when including county-pair fixed effects in column (3) and exploiting differences in the spying density *within* county pairs at district borders only.[22] Specification (4) shows the results of the standard border discontinuity model as described in equation (1), including our set of county-level controls. In a last step, we set up our preferred empirical model by combining the border design with an IV approach, taking the leave-out average district-level surveillance intensity as an instrument. We report the corresponding reduced form as well as the 2SLS estimates in columns (5) and (6). Overall, the instrument proves to be reasonably strong with first-stage

[22] While estimates generally become larger (in absolute terms) when moving from simple correlations to our county-pair design, estimates flip sign in three of our eight outcomes. In light of the various identification challenges discussed in Subsection 4.3, this demonstrates that simple correlations may be quite misleading in our setting.

F-statistics exceeding 10 for all outcomes and second-stage estimates being statistically significant.[23] For some outcomes, instrumenting the county-level spying density leads to significantly larger estimates than in the pure border design. This suggests that the additional IV approach further reduces biases due to endogeneity in the county-level spying density. Interestingly, the reduction of the bias for civic capital and economic outcomes goes in the same direction, which would be in line with a story that the Stasi allocated more spies to counties with relatively higher levels of civic capital and economic potential (see Subsection 4.3, "Sign of Bias").

In the Appendix, we additionally visualize the mechanics behind our identification strategy. For example, in Panel A of Figure B.7, we first plot the raw correlation between trust and the spying density at the county level, which is mildly positive as reflected in column (1) of Table 2. Restricting the sample to border counties and including county-pair fixed effects, as in specification (4), changes the sign of the correlation (Panel B). The change of sign demonstrates that that simple correlations are not informative for inferring causality and can be quite misleading (see Subsection 4.3). Panel C depicts the relationship in our 2SLS specification, which is more negative and tighter. Figures B.8-B.14 show similar patterns for our other outcomes. A second important insight we take from these graphs is that outliers do not drive our results and – if anything – tend to bias estimates towards zero.

Last, we run an additional identification test that exploits the fact that a substantial share of the new district borders were drawn through former Weimar provinces, separating regions with the same cultural heritage (cf. Figure B.3). If unobserved cultural differences existed across all county pairs, they should be smaller at ahistorical borders, which, in turn, should tighten identification. Estimating an interaction model that differentiates effects between pairs at historical and newly drawn borders, we find that estimates are more precise for county pairs which share the same cultural heritage (see Table C.1.2).

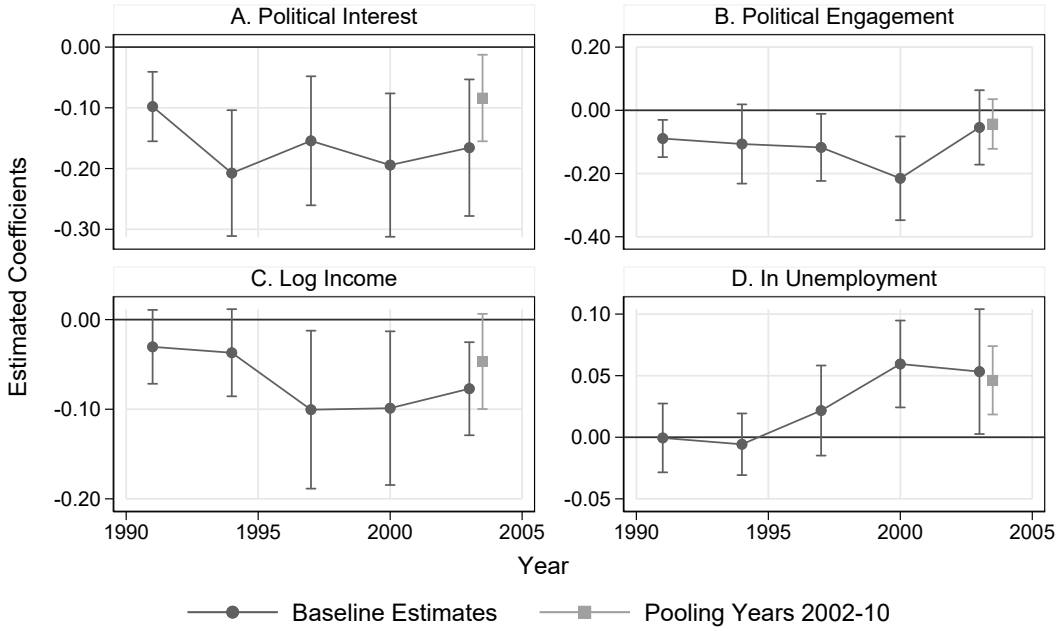
Dynamics and Persistence. We observe some of our civic capital and economic variables on an (almost) annual basis (cf. Subsection 4.1). This enables us to investigate the dynamics behind the average effects reported in Tables 2 and 3. In our main specification, we form three-year bins – 1990 to 1992, 1993 to 1995, etc. – and estimate the IV specification separately for these groups of years, interacting all control variables, including the county-pair fixed effects, with year dummies. We pool years for two reasons: (i) to smooth outcomes and account for mean reversion, which is particularly helpful for our economic outcomes, and (ii) to increase statistical power by down-weighting potential outliers, which make estimates imprecise in the smaller yearly samples. In the Appendix, we show that yearly estimates look very similar but are a little more bumpy and less precise, see Figure B.15.

Panels A and B of Figure 3 show that effects on the two measures of civic capital – political interest and political engagement – are statistically significantly negative immediately after reunification, while effects on unemployment and income become significant by the mid-1990s (see Panels (C) and (D)). This pattern is in line with our theoretical prior that lower levels of civic capital eventually lead to worse economic outcomes in a market economy. From the mid-1990s onwards, effects for all four outcomes are relatively stable until the early years of the new century. In the course of the 2000s, the

[23] We find similar effects when using the overall district average instead of the leave-out-average as an instrument (cf. Appendix Table C.1.3).

problem of smaller annual samples becomes more severe as individuals drop out due to retirement or death. By 2005, the number of individuals is less than half compared to 1990. We address this natural attrition in two ways: first, we simply exclude years 2005–2010 from the analysis and report the coefficient for the years 2002–2004 as our last dynamic estimate (black dot); secondly, we pool years 2002–2010 and report the corresponding coefficient (gray square). Overall, we detect some reversion for our outcomes – in particular, when taking into account the years from 2005 to 2010. However, when pooling the respective years, economic effects are still statistically significant in the late 2000s.

Figure 3: The Effect of Spying – Dynamics Over Time



Notes: This figure shows the effect of a one standard deviation increase in surveillance intensity on different measures of individual civic capital and economic performance for different periods of our sample. Estimates are based on our IV specification and obtained from separate regressions, pooling data over three year periods (1990–92, 1993–95, etc.). Dark circles show our baseline estimates for the period between 1990 and 2004 (excluding the years 2005–10); light squares report alternative estimates for the last period, pooling the years 2002–10 instead. In all regressions, we interact the set of county-pair fixed effects, the dummies for historical provinces of the Weimar Republic, the dummy variable indicating the presence of a Stasi on-site office, and our full set of controls (as described in Section 4.1) with year dummies. Outcomes in Panels A and B are standardized. Cross-sectional weights are adjusted for multiple person-year observations and the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level (vertical bars indicate 95 % confidence intervals). See Data Appendix A for detailed information on all variables.

To test the dynamics and persistence of our effects in more detail, we further make use of regional, administrative data that does not suffer from attrition and re-estimate our border-IV model at the local rather than the individual level. In terms of civic capital, we use county-level data on voter turnout in the two federal elections in 1990 (the last *Volkskammerwahl* as the only free election in the GDR and the first *Bundestagswahl* in reunited Germany) to see whether we detect effects on civic capital immediately after the fall of the Berlin Wall. In addition, we look at voter turnout statistics

at the municipal level for the federal election in 2009. To measure local economic performance, we use social security data at the municipal level and construct measures of local wage levels and unemployment (see Data Appendix A for details). While the collected data offer very precise information on local voter turnout and economic performance, they come at the cost that we cannot assign treatment based on individuals' county of residence in 1989. Consequently, these estimates do not account for (potentially selective) migration after reunification.

Table C.2.3 presents the corresponding results. In Panel A and B, we contrast individual and local-level estimates. Overall, effects are comparable and do not systematically deviate in any direction – although effects on our local measures of wages and unemployment are a bit smaller. Panel C further confirms that effects of surveillance on civic capital (turnout) are detectable immediately after reunification and smaller in the late 2000s – also see our discussion in the next paragraph. Overall, we consider this immediate effect of government surveillance on civic capital as evidence that the Stasi's activities shattered the trust of individuals during the time of the GDR and that our effects are not due to the revelation of the extent of Stasi surveillance post reunification. In terms of economic performance, Panel C further corroborates our survey data results by indicating that effects on unemployment and wages appear with a lag. Moreover, effects on economic performance are still sizable and statistically significant at the end of the 2000s.

Last, we return to our survey data and investigate differential effects of spying by age groups to explore whether the effect of surveillance might eventually vanish. We extend the baseline sample (birth cohorts until 1973) and add the children of our respondents (birth cohorts 1974 and later) to the analysis. This cohort only spent parts of their childhood under the regime, such that effects of surveillance might differ be smaller due to lower exposure. Panel A of Table C.4.2 shows that effects are indeed smaller across outcomes (although statistically significant in most cases) for the children generation (born 1974 or later). In line with the exposure hypothesis, our findings suggest that negative effects of Stasi surveillance on civic capital might be even smaller for the generation born after reunification.[24]

5.2 Sensitivity Checks

We next provide a range of robustness checks to make sure that (the significance of) our baseline estimates do(es) not depend on modeling choices.

Other Measures of Government Surveillance. While the number of operative informers is arguably the most natural measure of surveillance intensity (cf. Section 2), we show in Appendix Table C.2.4 that results remain robust when using alternative definitions of our measure of government surveillance, such as the total number of informers or when additionally including the number of official Stasi employees (columns (2) and (3)). Moreover, while informers were seen as the main weapon of the Stasi, the collected evidence occasionally led to more visible actions of the regime, such as arrests. Therefore, we further test whether local differences in political (or total) arrests rather than differences in surveillance drive our effects on trust and economics performance. To this end, we

[24] Unfortunately, we cannot dig deeper and rigorously test for intergenerational effects as we only observe very few children, who were born after 1989 and responded to civic capital questions in the survey.

acquired official micro data on detained individuals in East Germany for the years 1984 to 1988 (see Appendix Table A.1 for more details). While it is not straightforward to distill the number of political arrests from the data in light of non-exclusive, partially inconsistent, and potentially biased categorizations of criminal offenses (see Schröder and Wilke, 1998, for a critical discussion), we make a modest effort to come up with a county-level measure of politically motivated arrests per capita. We find that effects are basically unchanged when controlling for these measures in our baseline model, see columns (5) and (6) of Table C.2.4. This finding is backed with the slightly positive, yet overall rather unsystematic (conditional) correlation between the county-level number of (political) arrests and the respective spying density (Figure B.17). The result is also in line with our interpretation of the historical evidence that the large network of informers served as the regime's most important measure to ensure political stability and oppress oppositional movements before they even started (Childs and Popplewell, 1996).

Inference. Standard errors of our baseline results are two-way clustered at the county-pair and county-in-1990 level. As discussed above, we choose this default to account for common shocks within county pairs as well as the duplication of certain counties. One-way clustering at either the county-pair or county-in-1990 level yields very similar standard errors. Moreover, two-way clustering at the person and county-pair level does not affect inference. As parts of the identifying variation are induced by differences in surveillance strategies across districts, we further cluster standard errors at the county-pair and district level in one specification. Due to the small number of districts/clusters ($N = 14$), we further implement this specification by means of the standard percentile-t Wild cluster bootstrap approach as proposed by Cameron et al. (2008). We implement the Wild bootstrap for reduced-form estimates only as we are not aware of any procedure that accounts for the few-cluster problem in an IV setting. As an alternative test, we further conduct randomization inference to overcome possible accuracy problems when using conventional clustering methods to draw inference (Kempthorne, 1955, Young, 2019). Following Fouka and Voth (2016), we perform 2,999 random permutations of the dependent variable and re-estimate model (1) for each permutation. We combine these with the original, non-permuted estimate to calculate the empirical p-values. Table C.2.5 demonstrates that inference is robust across the different tests; the only notable exception being the effect on self-employment, for which we find p-values slightly above 0.1 when using the Wild cluster bootstrap and the randomization inference test.

County Pairs. As mentioned in Subsection 4.2, there are various ways to define the county pair estimation sample in case of multiple neighbors across one or more district borders. Our baseline specification is as follows: for a given county, we only consider the neighboring county that shares the longest border. This practice still leads to the duplication of counties if a given border is not the longest one for both counties within a pair or a rather large county spans over two or more counties on the other side of a district border (see Appendix Figure B.16 for an example). We account for the duplications of counties by clustering standard errors at the county-pair and county level, and dividing individual weights by the number of duplications (see preceding and following paragraphs). We also provide estimates based on (i) an extensive set of county pairs, duplicating each county

according to all its available neighbors across district borders, and (ii) a specification without any duplicates, dropping the smallest pairs in case of duplications. Columns (1) to (3) of Table C.2.2 shows that results are not affected by the definition of county pairs.

Weighting. In line with the recommendations of the SOEP, we use survey weights in all our baseline regressions to correct for biases due to the over-sampling of low-income households and potential attrition due to unemployment as stressed in Solon et al. (2015).[25] Columns (4) and (5) of Appendix Table C.2.2 show that estimates are similar when (i) using individual weights that are not adjusted for the duplication of counties and (ii) not using survey weights.

5.3 Alternative Mechanisms

Throughout the paper, we assume that Stasi surveillance reduced individuals' civic capital, which in turn affected economic performance. In this subsection, we test whether alternative mechanisms may (partly) account for the observed effects.

The Effect of Socialism. We first investigate whether local differences in socialist indoctrination rather than state surveillance may account for the observed differences in civic capital and economic performance. Two important studies document that East Germans' exposure to socialism had long-lasting effects on political attitudes (Alesina and Fuchs-Schündeln, 2007) and real labor market outcomes through education (Fuchs-Schündeln and Masella, 2016). To test this alternative mechanism, we proxy regional socialist indoctrination by the share of the political and economic elites who were members of the regime party (Socialist Unity Party, SED) and add this variable as a control. Results remain unchanged (cf. column (4) of Table C.2.4), which is in line with the rather unsystematic correlation between the spying density and our proxy of local differences in socialist indoctrination (see Figure B.18).

Distance to West Germany. Next, we investigate whether differences in a county's distance to the inner German border might drive our results. One may be concerned that counties closer to the border (within a county pair) had systematically higher surveillance intensities. Moreover, it may well be the case that individuals' geographic proximity to the West had a direct effect on civic capital (e.g., due to the extended visitors program that facilitated visits of West Germans in selected counties (Stegmann, 2018)) and post-reunification economic activity (e.g., due to better access to West German markets (Redding and Sturm, 2008)). Table C.3.1 shows that our estimates are robust to including various distance measures.

Risk Aversion and Personality Traits. In addition to civic capital, the Stasi may have also affected individuals' risk preferences, which may in turn account for (parts of) the observed differences in economic performance as individuals' preferences for risk taking have been shown to positively correlate with wage growth and the returns to education (Shaw, 1996). However, column (1) of Table C.3.2 shows that risk aversion is unaffected by government surveillance. Similarly, the Stasi

[25] Government surveillance itself does not significantly affect panel attrition.

may have changed personality traits, which could be driving (parts of) the economic effects. Among others, Borghans et al. (2008) show that personality traits predict economic outcomes such as educational attainment and wages. To test this potential alternative mechanism, we estimate the effect of spying on the Big Five personality traits "Extraversion", "Neuroticism", "Conscientiousness", "Openness", and "Agreeablness". However, as indicated in columns (2)-(6) of Table C.3.2, only one — "Agreeableness" — of the Big Five personality traits is significantly negatively affected by a higher spying density. We interpret this result in favor of our hypothesis that Stasi surveillance affected civic capital since trust and altruism are two of the six dimensions that constitute the measure of "Agreeableness" in the SOEP (Gerlitz and Schupp, 2005).

Preferences for Redistribution and Political Preferences. Last, we analyze whether the observed economic effects are (partly) due to surveillance-induced differences in preferences for redistribution or general party preferences, which may have been affected by the surveillance state as well. Alesina and Fuchs-Schündeln (2007) show that East Germans generally express higher preferences for state intervention than West Germans and link these differences to the socialist system. We test whether our economic effects are at least partly due to surveillance-induced differences in preferences for redistribution within East Germany, but do not find any statistically significant effect (see columns (1) to (6) of Table C.3.3). Relatedly, the influence of the Stasi might also be reflected in people's preferences for extreme parties, which, in turn, may be associated with negative economic outcomes. We test for effects on extreme voting behavior in columns (7) to (9) of Table C.3.3. We find a marginally significant effect on overall political extremism, operationalized by individuals' stated preferences for either the far-left or the far-right political spectrum. When decomposing the effect into preferences for either the extreme left or extreme right, we find similar effects at both ends of the distribution – however, we cannot rule out that effects are zero. We take this finding as further suggestive evidence that the surveillance state led to distrust in the political system, which is also reflected in a move away from moderate political views.

5.4 Underlying Channels

In the following subsection, we look at channels behind the overall economic effects documented in Table 3 and aim at corroborating our hypothesis that civic capital is the main driver of the economic effects observed.

Migration. First, we look at the role of migration after reunification, which could have affected both civic capital and economic performance. In the context of the GDR, this channel is particularly interesting as many people left East Germany and migrated to the West after 1990. In Panel A, column (1) of Appendix Table C.4.3, we show that Stasi surveillance had no significant impact on individuals' probability to leave the pre-reunification county of residence. Neither do we find differential effects when allowing for heterogeneity by education, gender or age (not shown). In Panel B of this table we further show that effects on civic capital and economic performance are similar when allowing for heterogeneous effects for individuals that moved from or stayed in the 1989 county of residence after reunification. However, given that any mobility response post

reunification may in itself be driven by the spying density, these findings should not be interpreted causally. We rather take these findings as suggestive evidence that selection effects are not driving our results. In line with this interpretation, our estimates are unaffected when additionally controlling for county-level population changes since 1988, see Panel C of Appendix Table C.4.3. Last, we test whether the spying density of the current (rather than the 1989) county of residence is able to explain effects on civic capital and economic performance for movers within East Germany. Results in Panel D suggest that this is not the case.

De-constructing the Economic Effect. Next, we investigate the effects of Stasi surveillance on educational attainment. As displayed in Table 4, we find that educational outcomes are negatively affected by more intense surveillance.[26] A one standard deviation increase in the spying density decreases individuals' years of education by 0.28 years on average. In line with this finding, the probability of having some vocational training or a university degree decreases with more intense surveillance (the latter effect being slightly insignificant at conventional levels). Assuming an additional year of schooling to yield an increase in income of around 0.1 log points (see above), surveillance-induced reductions in education can account for a decrease in income of about 0.03 log points, which is roughly half of the estimated income coefficient (0.056).

Importantly, the Stasi could have systematically affected educational attainment in two ways. First, there might have been a direct link since the regime denied allegedly oppositional citizens access to universities or apprenticeships (Bruce, 2010). Second, there may have been an indirect channel as social capital has been shown to be a "handmaiden" of human capital investments (Goldin and Katz, 1999). To infer the relevance of both channels, we allow for differential treatment effects by birth cohorts. If the reductions in educational attainment were merely due to direct expulsions by the Stasi, effects should be weakest for the youngest of our three cohorts (individuals born 1960–1973 and aged 16 to 29 in 1989) as they could have more easily invested in additional education after reunification than older cohorts. In contrast, we find that the effects for this cohort are – if anything – stronger than for older individuals (cf. Panel B of Table 4). Along with the finding that effects on education become zero once we condition on civic capital (see below), we conclude that the indirect channel seems to be of significant relevance.

Next, we investigate whether surveillance affected the type of occupation(s) individuals held after reunification. Estimates in column (4) of Table 4 indicate that this was the case: individuals exposed to a higher spying density were less likely to work in the job they were trained for after reunification. Along with the results on reduced occupational prestige (column (5)), a possible interpretation of these findings is that individuals exposed to a higher spying density were downgraded in terms of their occupations, possibly because of lower levels of civic capital.

In a final step, we directly assess the role of civic capital for our reduced form effects of spying on education, occupational choice, and our three measures of economic performance. Sacrificing some econometric rigor[27], we estimate the effects of government surveillance on these five outcomes while

[26] We find no differential effects of spying on civic capital or economic performance by individuals' level of education (see Panel C of Table C.4.2).

[27] We would need additional instruments to cleanly attribute the observed effects on economic performance to (a specific measure of) civic capital.

Table 4: The Effect of Spying on Education & Job Characteristics

	Years of Education (1)	Vocational Education (2)	University Degree (3)	In Job As Trained For (4)	Occup. Prestige (5)
Panel A – Average Effects					
County-Level Spying Density	-0.280***	-0.029***	-0.034	-0.056***	-0.119***
	(0.092)	(0.010)	(0.021)	(0.016)	(0.041)
Number of Observations	1,736	1,736	1,736	1,467	1,483
Adjusted R-Squared	0.162	0.202	0.109	0.103	0.137
Kleibergen-Paap F-Statistic	19.12	19.12	19.12	16.75	16.87
Panel B – Effects by Age					
District-Level Spying Density					
× Born Before 1945	-0.204	-0.033**	-0.011	-0.052**	-0.080
	(0.141)	(0.013)	(0.028)	(0.023)	(0.073)
× Born 1945–1959	-0.299**	-0.028**	-0.043	-0.061***	-0.132**
	(0.140)	(0.011)	(0.027)	(0.022)	(0.056)
× Born 1960–1973	-0.408***	-0.033***	-0.062**	-0.060**	-0.161***
	(0.125)	(0.011)	(0.026)	(0.023)	(0.049)
Number of Observations	1,736	1,736	1,736	1,467	1,483
Adjusted R-Squared	0.173	0.209	0.122	0.105	0.142
Border County-Pair Fixed Effects	Yes	Yes	Yes	Yes	Yes
County-Level Control Variables	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on different measures of education and job characteristics (see columns). The underlying econometric model is described in equations (1) and (2). In Panel A, we present average effects for the five outcomes, in Panel B we show heterogeneous effects by age groups. Outcomes in column (5) are standardized. All estimates are based on our sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

controlling for our measures of civic capital. Column (1) of Table 5 reveals that the effects on years of schooling are basically zero once we condition on our measures of trust. Similarly, estimates in column (2) show that the surveillance-induced effect on occupational prestige becomes small and insignificant when conditioning on civic capital. The same is true for income and unemployment duration, with coefficients decreasing and becoming statistically insignificant in Panel C. Overall, we take these findings as suggestive evidence that the surveillance-induced reductions in civic capital are an important driver of our economic effects, which is in line with our theoretical priors and the dynamic pattern displayed in Figure 3.

Table 5: The Effect of Spying on Economic Performance Conditional on Civic Capital

	Years of Education (1)	Occup. Prestige (2)	Unemploy. Duration (3)	Self-Emp. Probability (4)	Log Mean Income (5)
Panel A – Baseline Effects					
County-Level Spying Density	-0.280***	-0.119***	0.014***	-0.016**	-0.056***
	(0.092)	(0.041)	(0.005)	(0.007)	(0.019)
Number of Observations	1,736	1,483	1,719	1,611	1,482
Adjusted R-Squared	0.162	0.137	0.161	0.093	0.253
Kleibergen-Paap F-Statistic	19.12	16.87	20.81	18.76	16.80
Panel B – Reduced Sample					
County-Level Spying Density	-0.177	-0.107**	0.013*	-0.001	-0.057**
	(0.109)	(0.042)	(0.007)	(0.008)	(0.026)
Number of Observations	947	843	939	890	841
Adjusted R-Squared	0.189	0.206	0.219	0.145	0.328
Kleibergen-Paap F-Statistic	13.13	27.13	17.66	15.26	26.62
Panel C – Conditional on Civic Capital					
County-Level Spying Density	-0.032	-0.055	0.005	0.003	-0.042
	(0.104)	(0.042)	(0.007)	(0.007)	(0.025)
Number of Observations	947	843	939	890	841
Adjusted R-Squared	0.273	0.293	0.255	0.160	0.375
Kleibergen-Paap F-Statistic	12.71	26.57	17.12	14.75	26.13
Border County-Pair Fixed Effects	Yes	Yes	Yes	Yes	Yes
County-Level Control Variables	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on different measures of education, job characteristics, and economic performance (see columns). The underlying econometric model is described in equations (1) and (2). In Panel A, we present baseline effects from Tables 3 and 4. Panel B shows results when estimating the same model using the subsample of individuals for which we observe all five measures of civic capital (see Table 2). In Panel C, we additionally control for our five measures of civic capital. Outcomes in column (2) are standardized. All estimates are based on our sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

6 Conclusion

In this paper, we investigate the effect of state surveillance on civic capital and economic performance. We study the case of the former socialist German Democratic Republic that implemented one of the largest surveillance systems of all time and exploit county-level variation in the density of Stasi informers. To account for the non-random recruitment of informers across counties, we harness the specific institutional features of the East German surveillance state and combine a border research design with an instrumental variables approach.

Overall, the results of our study offer substantial evidence for negative and long-lasting effects of government surveillance. We find strong and consistent evidence that a higher density of informers negatively affects civic capital by undermining individuals' interpersonal trust, cooperative behavior, and political engagement. We further find negative and persistent effects of government surveillance on measures of economic performance, such as the probability of employment or self-employment and income (un)conditional on employment. Moreover, we show that reduced educational attainment can explain large parts of the negative economic effects, which is in line with the theoretical prediction that individuals with lower trust/civic capital invest less in (human) capital.

The magnitudes of our effects are meaningful. Translated into monetary terms, a one standard deviation increase in the spying density decreases monthly gross income by 108 EUR (84 EUR conditional on working). We can use these estimates to make a careful back-of-the-envelop (and out-of-sample) calculation to predict the overall contribution of the Stasi to the prevailing income gap between East and West Germany. To this end, we infer from our data that counterfactually abolishing the Stasi is equal to a decrease of 2.84 standard deviations in the spying density on average. The East-West Gap in GDP (wages) over the period 1991–2010 is 72 % (39 %).[28] Taking our estimates at face value, the Stasi can account for up to around 50 % of the un(conditional) East-West gap in economic performance.[29]

Our results add to the literature on institutions, trust, and economic performance (see, e.g., Alesina and Giuliano, 2015, for a survey). First, our study establishes a causal link between formal institutions (surveillance) and culture (trust). Second, and in line with Tabellini (2010), we provide evidence that the degree of democratic governance affects economic outcomes. Third, with both trust and economic performance being impaired by government surveillance, our findings also provide suggestive evidence in favor of a well-established channel: institutions shape people's trust, and trust affects economic development (Algan and Cahuc, 2014). In this respect, we, fourth, add to our understanding of the effects of repression in autocratic regimes, which generally make use of large-scale surveillance systems. Last, we show that our effects are persistent and still detectable two decades after the end of the socialist regime. However, it seems that the legacy of the Stasi may eventually fade out as the children of our sampled citizens (born between 1974–1990) exhibit smaller effects than the parent generation. This implies that the negative effects of Stasi surveillance on trust are at least not transmitted one to one to the next generation (see, e.g., Nunn and Wantchekon, 2011, Dohmen et al., 2012, for studies on the intergenerational transmission of trust and beliefs). Whether the legacy of Stasi surveillance *will* eventually fade out remains an open question that has to be investigated in future research; a partial answer could be given once children born after 1990 turn adults and information about their trust levels and economic performance becomes available.

Another important question is how our findings translate to other (contemporary) forms of mass surveillance in autocratic states given that surveillance strategies have changed over the last decades and nowadays rely arguably more on technology than individual informers.[30] It is likely that this shift towards electronic surveillance modes renders the findings for interpersonal trust within the social network less important. At the same time, it seems plausible that trust in institutions could still be affected by modern forms of surveillance. After the revelation of the NSA wiretapping and the Snowden affair, for example, anecdotal evidence suggests that citizens did not know which communication companies to trust (see, e.g., Schneier, 2013). Moreover, a large share of people stated that they had adjusted their use of telecommunications as a consequence of the affair (Pew Research Center, 2014). The Snowden affair further points to another conceptual issue when generalizing our

[28] We take the East-West gap in GDP from the Working Group Regional Accounts of the Statistical Offices and derive the corresponding gap in wages from the SOEP.

[29] Without Stasi surveillance, the East-West gap in income would be lower by factor $0.44 = (\exp(-0.056 * -2.84) - 1)/0.39$.

[30] Nevertheless, contemporaneous regimes still make use of informers to control their citizens. Various accounts state that China still heavily relies on a large network of informers (see, e.g., Branigan, 2010, Jacobs and Ansfield, 2011, Yu, 2014). Likewise, Russia has been observed to re-implement surveillance strategies in which secret informers and denunciations play an important role in controlling opposition forces (Capon, 2015).

findings – the question of whether effects of government surveillance are different in a democracy. Both democratic and autocratic regimes would justify surveillance with the need to secure the stability of the system – hence with benevolent motives, while the (perceived) degree of benevolence is, of course, highly subjective. Separating negative and positive aspects of surveillance is notoriously difficult, and researchers will most likely only be able to assess the net effect of surveillance. The findings of this study show that the net effect of government surveillance on trust and economic performance was negative in the case of socialist East Germany. Net effects of state surveillance in other systems and at different times may vary and should be studied case-by-case.

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A Appendix – Data Appendix

For our analysis, we draw upon a variety of datasets and historical sources that we detail below.

Information on our explanatory variable of interest, the mean county-level spying density in the 1980s, were in large part taken from Müller-Enbergs (2008). As the Federal Commission for the Stasi Records (BStU) keeps restoring files for previously missing counties, we were able to expand this dataset with information for ten additional county offices from the Stasi archives in Berlin via a special request to utilize Stasi files for research (*Antrag auf Akteneinsicht zu Forschungszwecken*). Data on the county-level number of regular Stasi employees were generously provided by the historian Jens Gieseke, who had worked at the BStU in Berlin. Finally, to calculate the spying density in a given county, we obtained yearly population data from the Statistical Yearbooks of the GDR, which are accessible online (`http://www.digizeitschriften.de/en/dms/toc/?PID=PPN514402644`).

Individual-level measures of civic capital and economic performance were taken from the German Socio-Economic Panel (SOEP). We focus on East German respondents covered in the 1990 wave of the survey and follow these individuals over time until 2010. We assign treatment based on the county of residence in 1989, i.e., prior to the fall of the Iron Curtain. Information on respondents' county of residence is accessible via remote computing (SOEPremote), see Knies and Spiess (2007) for details.

Measures of civic capital and economic performance at the regional level stem from multiple sources. Information on voter turnout at the county and municipality level were obtained from the Federal Returning Officer, the Federal Statistical Office, and `http://www.wahlrecht.de`. Data on local unemployment rates and wage levels base on information provided by the Institute for Employment Research (IAB) and the Federal Employment Agency (BA).

Our sets of county-level controls comprise measures of demographic, industrial, and historical differences across GDR counties, both from before World War II and during the time of the socialist regime (cf. Subsection 4.1). Data on the demographic and industrial structure of counties in the 1980s were taken from Rudolph (1990) and infas (n.d.); the latter source was kindly provided by Davide Cantoni. Information on regional differences (in terms of economic performance, religious composition, and political attitudes) before World War II were obtained from King et al. (2008). The underlying dataset is accessible via Gary King's website (`http://gking.harvard.edu/data`). Information on county-level differences in the intensity of the People's Uprising against the SED regime in 1953 base on cartographic statistics published by the the former West German Federal Ministry of Intra-German Relations (*Bundesministerium für gesamtdeutsche Fragen*).

To test for differences in the awareness of Stasi surveillance and rule out alternative mechanisms driving our effects, we further collected district-level data on the number of requests for the disclosure of an individual's own Stasi file post reunification (from the BStU), county-level data on the number of (political) arrests in the 1980s (from the Federal Archives), as well as data on the county-level share of political and economic elites who were members of the SED (from the Federal Archives) to proxy regional socialist indoctrination. Last, we calculated the minimum crow-fly distance and travel time from every county to the inner-German border (using openrouteservice API and OpenStreetMap).

Table A.1 defines all variables used in our analysis and details their corresponding source. Descriptive statistics for all variables are presented in Table A.2. In Subsection A.1, we describe our procedure to harmonize historical and current county-level data to boundaries as of October 1990.

Table A.1: Definition of Variables and Data Sources

Variable	Years	Source
Panel A – Stasi Data (See Section 2)		
Spying Density	1980–1988 (counties)	We take the average spying density over the period 1980–1988 as our main explanatory variable (see Section 2 for details). Data on the county-level number of unofficial informers are taken from official Stasi records that were in large part compiled by Müller-Enbergs (2008). As the Stasi Records Agency keeps restoring files and releasing new data on previously missing counties, we collected additional information from the Stasi archives and expanded the dataset with information for ten previously missing county offices (*Akteneinsicht zu Forschungszwecken, BStU Tgb. 15582/15Z, 15561/15Z, 15164/15Z.*). We divide the county-level number of unofficial informers by the average county-level population to arrive at our measure of the spying density. Annual population figures were taken from the Statistical Yearbooks of the GDR. Our baseline measure of spying density covers unofficial informers for political-operative penetration, homeland defense, or special operations, as well as leading informers (*IM zur politisch-operativen Durchdringung und Sicherung des Verantwortungsbereiches, IM der Abwehr mit Feindverbindung bzw. zur unmittelbaren Bearbeitung im Verdacht der Feindtätigkeit stehender Personen, IM im besonderen Einsatz, Führungs-IM*). In cases where the Stasi held additional on-site offices (*Objektdienststellen*) in counties, the number of informers attached to these offices was added to the number of spies in the respective county.
Stasi Employees	1982 (counties)	The number of regular Stasi employees (*Hauptamtliche Mitarbeiter*) in each county was provided by Jens Gieseke.
Disclosure Requests	1992–2016 (districts)	Annual district-level data on the number of disclosure requests were provided by the Stasi Records Agency in response to a Freedom of Information request. There is no information for East Berlin, data for the districts of Frankfurt/Oder and Cottbus is aggregated. We divide the district-level number of yearly disclosure requests by a district's average population between 1980 and 1988 to account for district-level differences in (population) size.
	1992–2012 (federal)	Annual county-level data on the number of disclosure requests are published in the Yearbooks of the Agency of the Federal Commissioner for the Stasi Records (BStU).
Panel B – Individual SOEP Data (See Subsection 4.1)		
Attend Elections	2005, 2009, 2010	We measure individuals' voting intentions by combining two questions from the survey. First, we take information from 2005 and 2009, where individuals were asked about their intention to attend the next election for the German parliament. Response options were given on a five-point scale to allow individuals to express varying degrees of conviction (not) to vote. We create a dummy variable turning one if respondents indicated that they will "probably" or "in any case" attend the upcoming elections. Second, in 2010, individuals were asked whether they voted in the 2009 elections for German parliament. We combine these information to arrive at our measure of voting intentions and discard all individuals not eligible to vote.

continued

Table A.1 continued

Variable	Years	Source
Educational Attainment	1990–2010	We measure educational attainment threefold. First, by means of individuals' total years of schooling, a variable provided by the SOEP. Second, we create a dummy variable whether an individual obtained at least some vocational degree. Last, we create a dummy variable turning one if an individual holds a university degree.
Employment	1991–2010	Respondents were asked about their labor market status in every wave of the survey. For the working-age population, we create dummy variables indicating whether respondents are in the labor force and employed, respectively.
Income	1990–2010	We observe information on monthly gross income (from dependent employment or self-employment) in every wave of the SOEP for East German respondents. We account for inflation by calculating real income in 2010 prices using the official East German CPI (*Verbraucherpreisindex*), see Vortmann et al. (2013) for details. When analyzing the average post-reunification effect, we calculate the mean for every individual over the period of 1991–2010 and drop the bottom and top 1 % of the income distribution. We look at the 1990 effect on income when analyzing the dynamics of our effects.
Job Type	1991–2010	We use two measures to assess the type of job(s) held by individuals. First, we calculate the average prestige score of the job(s) held by each individual over the period from 1991 to 2010. Occupational prestige is measured by means of the Magnitude Prestige Scale (MPS) and provided/calculated by the SOEP. Second, individuals were asked whether they hold a job in the occupation they were initially trained for (yes/no). We take the average value of this dummy variable over the period 1991–2010 to proxy occupational transitions.
Moving	1990–2010	We measure mobility by creating a dummy variable that turns one if an individual moved out of its county of residence in 1990 at any point during the survey period. Information on individuals' county of residence is accessible via SOEPremote and available in two distinct ways: (i) according to the county border definitions in a given survey year (we use this variable from the 1990 wave to assign treatment), and (ii) according to a harmonized county border definition as of 2012. We use the latter to measure individual mobility. Note that the applied procedure slightly underestimates cross-county migration because counties as of 2012 in East Germany are generally larger and may thus contain (parts of) multiple counties as of 1990.
Personality Traits	2005, 2009	We measure individuals' personality traits by means of the Big Five: "Neuroticism", "Extraversion", "Openness", "Agreeablness", and "Conscientousness". Every measure is based on three different survey questions on individuals' personality inventory. We combine these three questions via a principal components analysis to arrive at our personality traits measures.
Political Engagement	1990–2010	Respondents were asked (in almost every wave) whether they participate in public initiatives, political parties, or local government. Response options were given on a four point scale, allowing for different levels of political engagement: "every day", "every week", "less frequently", "never". We recode the variable to test whether individuals engage in politics at all (combining the former three response options).

continued

Table A.1 continued

Variable	Years	Source
Political Extremism	2005, 2009	Respondents were asked to state their political orientation. The underlying question reads: "In politics, people often talk about 'left' and 'right' when describing different political views. When you think about your own political view, how would you rate them on the scale below?" Response options were given on a eleven-point scale, allowing for a different placement along the political spectrum (left to right). We consider respondents to be politically extreme in case they ended up in the upper or lower four percent of the distribution. We further consider left- and right-wing extremism in separate regressions.
Political Interest	1990–2010	Respondents were asked about their overall interest in politics. Response options were given on a four point scale to allow for varying degrees of political interest: "very much", "much", "not so much", "not at all". We merge the former three answers to arrive at a zero/one dummy variable.
Preferences for Redistribution	1997, 2002	Respondents were asked about their preferred role of the state regarding different areas of social security. The question reads as follows: "At present, a multitude of social services are provided not only by the state but also by private free market enterprises, organizations, associations, or private citizens. What is your opinion on this? Who should be responsible for (i) financial security in case of unemployment, (ii) financial security in case of illness, (iii) financial security of families, (iv) financial security for old-age, (v) financial security for persons needing care." Response options were given on a five point scale, ranging from "only private forces", "mostly private forces", "state and private forces", "mostly the state", to "only the state".
Reciprocal Behavior	2005, 2010	We use six questions on positive and negative reciprocity to combine them into one single measure (taking the simple mean). Response options on each statement varied on a seven-point scale and we recode responses on the three statements indicating negative reciprocity such that higher values indicate more positive reciprocal behavior.
Risk Aversion	2004, 2006	Individuals were asked about their personal willingness to take risks. Response options were given on a 11-point scale, with higher values indicating a higher willingness to take risk. Note that we present estimates using the waves 2004 and 2006. Questions on risk aversion enter the survey in 2008, 2009 and 2010, too. Results are very similar when taking the mean over all five years.
Self-Employment Probability	1991–2010	Detailed information on individuals' type of employment is given in every wave of the survey. The dataset distinguishes between self-employed farmers, free-lance professionals, solo self-employed, and self-employed individuals with coworkers. We focus on the latter two categories and calculate the probability of self-employment conditional on employment in every wave.
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. We recoded the original variable to a dummy that turns one if respondents indicated to slightly or totally disagree.

continued

Table A.1 continued

Variable	Years	Source
Unemployment	1990–2010	In every year, respondents were asked to indicate whether they were unemployed at the time of the interview and state the total number of months spent in (registered) unemployment in the 12 months preceding the interview. We take the unemployment dummy variable when looking at the dynamic nature of our effects. When analyzing the mean post-reunification effect, we calculate the average number of months in unemployment per year over the period 1991–2010.
Working Hours	1991–2010	Respondents were asked about their contractual weekly working hours. We calculate average working hours for each individual during the sampling period.
Control Variables		The set of control variables includes the respondents' age (and age squared) and gender.
Panel C – Regional Data (See Section 4.1)		
Arrests	1984–1988 (counties)	Our county-level measures of (political) arrests per capita are based on official micro data on the universe of detained and imprisoned persons between 1984 and 1988 as recorded by the former Ministry of Internal Affairs (*Projekt "NRC" / Personenerfassung, Strafgefangenen- und Verhaftetendateien des Ministeriums des Innern, Teilkomplex 4*). The corresponding dataset was obtained from the Federal Archives of Germany (*Bundesarchiv*), signature: DO 1 MD / 003. Our two measures are calculated by taking the average number of (political) arrests per capita over this period. We code the following arrests as political: *Staatsfeindliche Verbindungen, Staatsfeindlicher Menschenhandel, Staatsfeindliche Hetze, Staatsfeindliche Gruppenbildung, Verleitung zu asozialer Lebensweise, Verbreitung von Schund- und Schmutzerzeugnissen, Widerstand gegen staatliche Maßnahmen, Ungesetzlicher Grenzübertritt, Beeinträchtigung staatlicher oder gesellschaftlicher Tätigkeit, Rowdytum, Rowdytum – schwere Fälle, Zusammenrottung, Vereinsbildung zur Verfolgung gesetzwidriger Ziele, Ungesetzliche Verbindungsaufnahme, Staatsverleumdung, Missachtung staatlicher und gesellschaftlicher Symbole, Gefährdung der öffentlichen Ordnung durch asoziales Verhalten, Gesetz über die Staatsgrenze der DDR*. We code political arrests following Schröder and Wilke (1998).
County Size	1990 (counties)	Information on a county's surface area was taken from Rudolph (1990).
Demographics	12/1989 (counties)	Data on age-specific population shares (the share of youths below 18 and the share of retired above age 65) were taken from the material in infas (n.d.).
Distance		We calculate two distinct measures of a county's distance to the West German border. First, we calculate the shortest crow-fly distance of a county's centroid to the former inner-German border. Second, we calculate the *current* travel time by car from a county's capital to the inner German border (using the openrouteservice API based on OpenStreetMap). Moreover, we proxy distance to the West by means of two dummy variables: (i) a dummy turning one if the county is situated at the inner-German border, and (ii) a dummy variable indicating whether a county was affected by the extended visitors program in 1972 that facilitated visits from the West in some regions.

continued

Table A.1 continued

Variable	Years	Source
Exit Visa Applications	1988 (districts)	We collected information on the district-level number of exit visa applications as filed by December 31, 1988 from the archives of the Stasi Records Agency (*Akteneinsicht zu Forschungszwecken*, BStU Tgb. 003665/19Z, signature: BStU MfS-ZAIG 27884, p. 358). To derive our measure of local differences in anti-regime attitudes, we divide the total number of exit visa applications (*insgesamt gegenwärtig vorhandene um Übersiedlung nach der BRD und Westberlin Ersuchende – einschließlich bereits vor dem 1. Januar 1988 gesteller Ersuchen, soweit von diesen Personen noch entsprechende Aktivitäten dazu erfolgen*) by the district's average population between 1980 and 1988.
Industry Controls	09/1989 (counties)	Information on the goods value of production is collected from infas (n.d.). Data on the industrial composition of the workforce as of September 1989 is reported in Rudolph (1990).
Opposition	1953 (counties)	We use cartographic statistics published by the former West German Federal Ministry of Intra-German Relations (*Bundesministerium für gesamtdeutsche Fragen*) to construct our measure of regional opposition to the regime. The map was taken from the archives of the Federal Foundation for the Reappraisal of the SED Dictatorship (*Bundesstiftung zur Aufarbeitung der SED-Diktatur*, signature: EA 111 1889).
	1989 (districts)	We proxy district-level differences in the strength of the opposition in 1989 by the date the first protest took place in the corresponding district during the Peaceful Revolution. To this end, we rely on information provided in Grdešić (2014). His "event catalog for the revolution in East Germany" is based on Schwabe (1999).
Political Ideology	1933 (counties)	We proxy historical differences in political ideology by the vote shares for the Communist party (*Kommunistische Partei Deutschlands, KPD*) and the Nazi party (*Nationalsozialistische Deutsche Arbeiterpartei, NSDAP*) in the federal election in March 1933 to construct two distinct measures of political ideology. Data were taken from King et al. (2008).
Population	1980–1989 (counties)	County-level population data were collected from the Statistical Yearbooks of the German Democratic Republic (*Statistische Jahrbücher der Deutschen Demokratischen Republik*).
Religious Composition	1925 (counties)	Information on the county-level share of Protestants and Jews was taken from King et al. (2008).
Socialist Indoctrination	1988 (counties)	We proxy regional socialist indoctrination by the share of the political and economic elite who were members of the Socialist Unity Party (SED). We calculate this measure using data from the Central Cadre Database (*Zentraler Kaderdatenspeicher, ZKDS*). This large administrative dataset was used for planning purposes and contains information on all political and economic executives of the GDR (except for employees of the Ministry for State Security, the Ministry of National Defence and the Ministry of Internal Affairs). The dataset was obtained from the Federal Archives (*Bundesarchiv*), signature: DC 20 MD / 1.
Unemployment	1998–2010 (municipalities)	We measure regional unemployment by calculating the municipality-level unemployment rate as the number of registered unemployed over the sum of unemployed and employed workers in a given year. Unemployment statistics for all municipalities are provided by the Federal Employment Agency (*Bundesagentur für Arbeit*). Data on the number of employed workers (subject to social security contributions) per municipality and year were provided by the IAB.

continued

Table A.1 continued

Variable	Years	Source
Voter Turnout	1933 (counties)	Information on county-level voter turnout in the March 1933 federal election were taken from King et al. (2008).
	1990 (counties)	We measure voter turnout in the last election for the *Volkskammer* in March 1990 – the only free election in the history of the GDR – and in the first federal election for German parliament after reunfication in December 1990 (*Bundestagswahl*). Data for the former election are based on information from the electoral commission of the GDR (*Wahlkommission der DDR*) accessible via `http://www.wahlrecht.de`; data on the latter election stem from the Federal Returning Officer (*Bundeswahlleiter*).
	2009 (municipalities)	We obtained data on municipality-level voter turnout in the 2009 federal election from the Federal Statistical Office (*Statistisches Bundesamt*) via `http://www.regionalstatistik.de`.
Wages	1992–2010 (municipalities)	We obtained annual data on average daily wages at the municipality level over the period 1992–2010 from the Institute for Employment Research (IAB).
Workforce Composition	1933 (counties)	Information on the local composition of the workforce (the share of white-collar workers, the share of self-employed, the share of unemployed) were taken from King et al. (2008).
Panel D – Other Data Sources		
Autocracies	1800–2016 (global)	We follow the Polity IV Project (Marshall et al., 2017) and consider a country as autocratic in a given year if the *Polity2* index takes a negative value (on a range from -10 [strongly autocratic] to +10 [strongly democratic]). A similar definition is used by, e.g., Besley and Kudamatsu (2008).
Out-Migration	1951–1989 (federal)	Annual data on overall out-migration was taken from the Federal Statistical Office (Statistisches Bundesamt, 1993).
	1961–1989 (federal)	Annual information on the number of successful illegal border crossings of East German citizens to the West (*vollendetes ungesetzliches Verlassen der DDR nach nichtsozialistischen Staaten bzw. West Berlin*) were reported by the Ministry for State Security and are available in the archives of the Stasi Records Agency (*Akteneinsicht zu Forschungszwecken*, BStU Tgb. 003665/19Z, signature: BStU MfS-ZAIG 27884, p. 8).

Notes: This table provides details on the definition and sources for all variables used.

Table A.2: Descriptive Statistics

	Mean (1)	SD (2)	P25 (3)	P50 (4)	P75 (5)	Min (6)	Max (7)	N (8)
Panel A – Spying Intensity								
County-Level Spying Density	0.37	0.13	0.26	0.35	0.45	0.12	0.78	148
District-Level Spying Density	0.37	0.07	0.30	0.38	0.42	0.27	0.50	147
District-Level Spying Density (Leave-Out)	0.37	0.07	0.31	0.38	0.43	0.26	0.52	147
Panel B – Main Dependent Variables								
Trust in Strangers (2003, 2008)	0.12	0.32	0.00	0.00	0.00	0.00	1.00	3,175
Reciprocal Behavior (2005, 2010)	5.36	0.81	4.83	5.33	6.00	2.50	7.00	2,835
Attend Elections (2005, 2010)	0.79	0.41	1.00	1.00	1.00	0.00	1.00	2,828
Political Interest (1991–2010)	0.86	0.23	0.81	1.00	1.00	0.00	1.00	2,914
Political Engagement (1991–2010)	0.38	0.49	0.00	0.00	1.00	0.00	1.00	2,914
Unemployment Duration (1991–2010)	0.10	0.17	0.00	0.00	0.15	0.00	0.94	2,880
Self-Employment Probability (1991–2010)	0.05	0.19	0.00	0.00	0.00	0.00	1.00	2,724
Log Mean Income (1991–2010)	7.34	0.48	7.08	7.36	7.64	5.27	8.59	2,517
Panel C – Other Dependent Variables								
Years of Education (1990–2010)	12.24	2.29	10.50	11.50	13.00	7.00	18.00	2,913
Vocational Degree or Higher (1990–2010)	0.95	0.21	1.00	1.00	1.00	0.00	1.00	2,913
University Degree (1990–2010)	0.26	0.44	0.00	0.00	1.00	0.00	1.00	2,913
In Job as Trained For (1991–2010)	0.56	0.42	0.07	0.64	1.00	0.00	1.00	2,477
Average Occupational Prestige (1991–2010)	0.26	0.11	0.18	0.24	0.30	0.14	1.00	2,509
Average Working Hours (1991–2010)	37.50	5.83	37.05	40.00	40.00	3.00	60.00	2,403
Risk Aversion (2004, 2006)	4.59	2.09	3.00	5.00	6.00	0.00	10.00	3,263
Big Five Traits: Conscientiousness (2005, 2009)	0.18	1.22	-0.49	0.46	1.10	-6.83	1.62	2,899
Big Five Traits: Extraversion (2005, 2009)	-0.02	1.28	-0.90	-0.00	0.88	-4.51	2.56	2,910
Big Five Traits: Neuroticism (2005, 2009)	0.13	1.20	-0.69	0.10	0.93	-3.25	3.40	2,914
Big Five Traits: Openness (2005, 2009)	-0.04	1.26	-0.82	-0.02	0.81	-3.80	2.93	2,906
Big Five Traits: Agreeableness (2005, 2009)	0.01	1.24	-0.82	0.10	0.96	-4.73	2.09	2,908
Redistribution Preferences: Total (1997, 2002)	3.73	0.60	3.40	3.80	4.00	1.00	5.00	4,139
Redistribution Preferences: Family (1997, 2002)	3.55	0.85	3.00	3.00	4.00	1.00	5.00	4,123
Redistribution Preferences: Unemployment (1997, 2002)	4.13	0.77	4.00	4.00	5.00	1.00	5.00	4,118
Redistribution Preferences: Care (1997, 2002)	3.69	0.78	3.00	4.00	4.00	1.00	5.00	4,132
Redistribution Preferences: Sick (1997, 2002)	3.64	0.78	3.00	4.00	4.00	1.00	5.00	4,118
Redistribution Preferences: Old Age (1997, 2002)	3.67	0.85	3.00	4.00	4.00	1.00	5.00	4,132
Extreme Left/Right Voter (2005, 2009)	0.08	0.27	0.00	0.00	0.00	0.00	1.00	2,895
Extreme Voter: Right Wing (2005, 2009)	0.04	0.20	0.00	0.00	0.00	0.00	1.00	2,895
Extreme Voter: Left Wing (2005, 2009)	0.04	0.20	0.00	0.00	0.00	0.00	1.00	2,946
Moved Residency (1990–2010)	0.26	0.44	0.00	0.00	1.00	0.00	1.00	2,913

continued

Table A.2 continued

	Mean (1)	SD (2)	P25 (3)	P50 (4)	P75 (5)	Min (6)	Max (7)	N (8)
Panel D – Control Variables								
Age (in 1990)	39.16	12.81	29.00	38.00	50.00	17.00	64.00	2,914
Male	0.49	0.50	0.00	0.00	1.00	0.00	1.00	2,914
Stasi On-Site Office	0.03	0.16	0.00	0.00	0.00	0.00	1.00	148
Log Mean Population (1980–1988)	11.13	0.55	10.73	11.09	11.41	9.94	13.23	148
Log County Size	5.92	0.80	5.57	6.12	6.52	3.26	7.14	148
City County	0.16	0.37	0.00	0.00	0.00	0.00	1.00	148
Share of Population Aged Under 15 (1989)	19.64	1.76	18.68	19.57	20.74	15.56	24.74	148
Share of Population Aged 65 and Above (1989)	13.48	2.29	12.13	13.53	15.00	5.68	19.33	148
Log Industrial Output (1989)	21.11	1.33	20.37	21.30	22.03	16.99	23.73	148
Share Agricultural Employment (09/1989)	15.06	11.79	6.25	13.10	21.10	0.00	50.00	148
Employment Share Energy Industry (09/1989)	2.08	6.09	0.20	0.30	1.30	0.00	45.39	148
Employment Share Textile and Clothing (09/1989)	2.92	6.19	0.00	0.20	1.85	0.00	27.87	148
Share of Cooperative Workers (09/1989)	12.91	8.90	5.97	11.01	18.16	1.20	35.91	148
Uprising 1953: Strike, Demonstration, Riot	0.66	0.48	0.00	1.00	1.00	0.00	1.00	148
Electoral Turnout (1933)	90.21	2.09	88.67	90.41	91.85	84.44	94.13	148
Vote Share Nazi Party NSDAP (1933)	48.80	6.17	45.00	48.28	52.59	24.75	63.77	148
Vote Share Communist Party KPD (1933)	13.05	5.47	9.12	12.55	16.46	2.59	31.32	148
Share Protestants (1925)	91.35	7.68	90.69	92.76	94.23	16.44	97.10	148
Share Jews (1925)	0.17	0.17	0.07	0.12	0.22	0.01	1.32	148
Share of White Collar Workers (1933)	7.26	2.33	5.69	6.87	8.24	3.31	15.60	148
Self-Employment Rate (1933)	16.03	2.39	14.42	15.83	17.77	11.09	22.56	148
Unemployment Rate (1933)	16.17	5.28	12.59	16.49	20.06	3.70	28.71	148
Panel E – Robustness Checks								
County-Level Spying Density (IM1 + IM2)	0.46	0.15	0.34	0.44	0.56	0.16	0.94	142
County-Level Spying Density (IM1, IM2 + HM)	0.52	0.17	0.38	0.50	0.64	0.18	1.04	142
Share of Elites with SED Membership	48.89	5.67	45.07	49.55	52.92	33.14	62.56	148
Total Arrests per Capita (per 1,000)	1.51	0.70	1.03	1.44	1.97	0.29	5.88	148
Political Arrests per Capita (per 1,000)	0.58	0.30	0.35	0.53	0.77	0.07	2.32	148
Log Distance to West German Border	4.20	0.88	3.74	4.41	4.89	1.61	5.43	148
Log Travel Time to West German Border	0.07	0.63	-0.37	0.16	0.55	-1.65	1.09	148
County Straddles West German Border	0.11	0.32	0.00	0.00	0.00	0.00	1.00	148
Part of Extended Visitors Program	0.26	0.44	0.00	0.00	1.00	0.00	1.00	148
Population Growth Since 1988	-0.09	0.08	-0.13	-0.08	-0.05	-0.27	0.24	148

Notes: This table presents descriptive statistics for all variables used.

A.1 Redrawn County Boundaries and Data Harmonization

We combine county-level data from various sources and decades in this study. Since 1925, the first year in our analysis, borders have been redrawn multiple times. To account for these territorial changes, we harmonize all county-level data to boundaries as of reunification in October 1990. The same is true for our municipality-level data covering the period 1992–2010. Below, we describe the harmonization procedure in more detail. Note that this procedure only applies to regional data. It is not necessary to harmonize the survey data as we focus on East German respondents in the 1990 wave of the survey, observe their county residence in 1989 (before reunification) and track these people over time.

We harmonize data from the times of the Weimar Republic, i.e., before World War II, via geospatial area weighting factors as described in Goodchild and Lam (1980). To this end, we overlay the corresponding GIS shapefiles from the Weimar Republic with the shapefile from 1990 and calculate area weighting factors that allow for an adjustment of the historical data to county borders as of 1990. MPIDR and CGG (2011) provide a rich set of historical shapefiles for the German territory. Given that most of our outcomes and control variables refer to people and not space, it needs to be stressed that this procedure is subject to some degree of imprecision. However, given the long time span, the numerous territorial reforms, and the lack of population weighting factors, this procedure is the most accurate harmonization strategy we can apply.

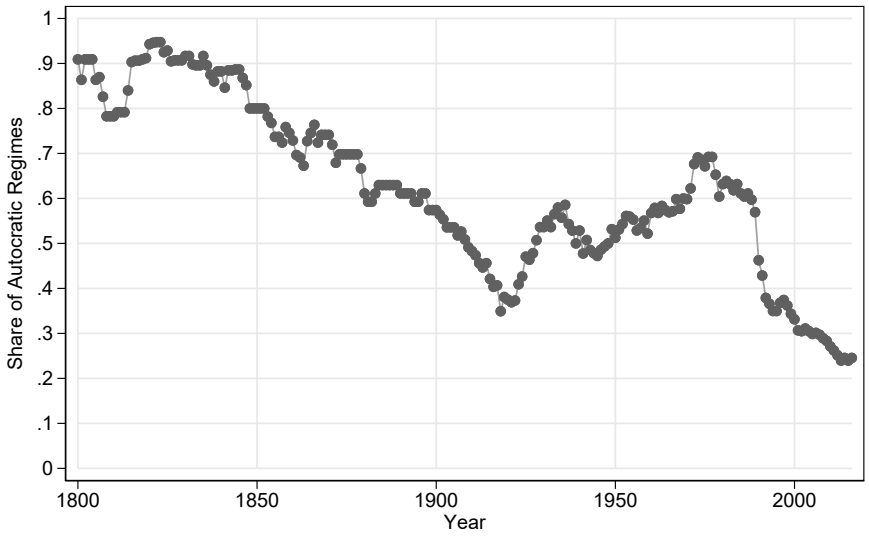
As regards data from the time of the GDR, we have to account for minor territorial reforms only. In ten cases, neighboring counties were merged. In five cases, bigger cities became independent counties (*Stadtkreise*) from the surrounding rural county. We manually account for these administrative changes using detailed maps from MPIDR and CGG (2011) and information from Wikipedia. In cases where new counties were constituted, we assign historical values of the emitting county to the created one.

Last, when using municipality-level data from the period 1992–2010, we harmonize municipal border as of December 31, 2016 using administrative crosswalks provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*BBSR*), and match municipalities to counties in 1990 by overlaying the corresponding GIS shapefiles and using the geographic point coordinates of a municipality.

B Appendix – Additional Figures

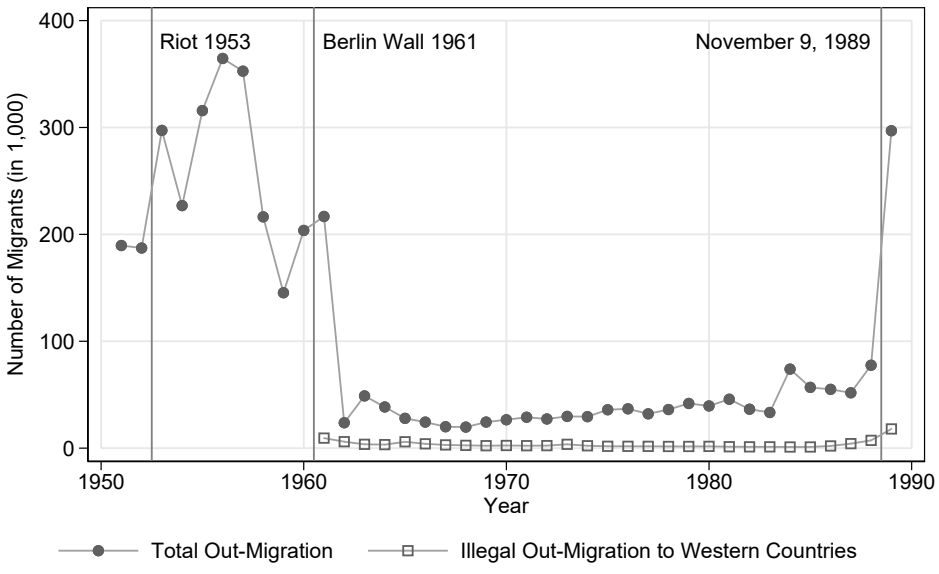
Appendix B provides additional figures referred to in the manuscript.

Figure B.1: Share of Autocracies over Time



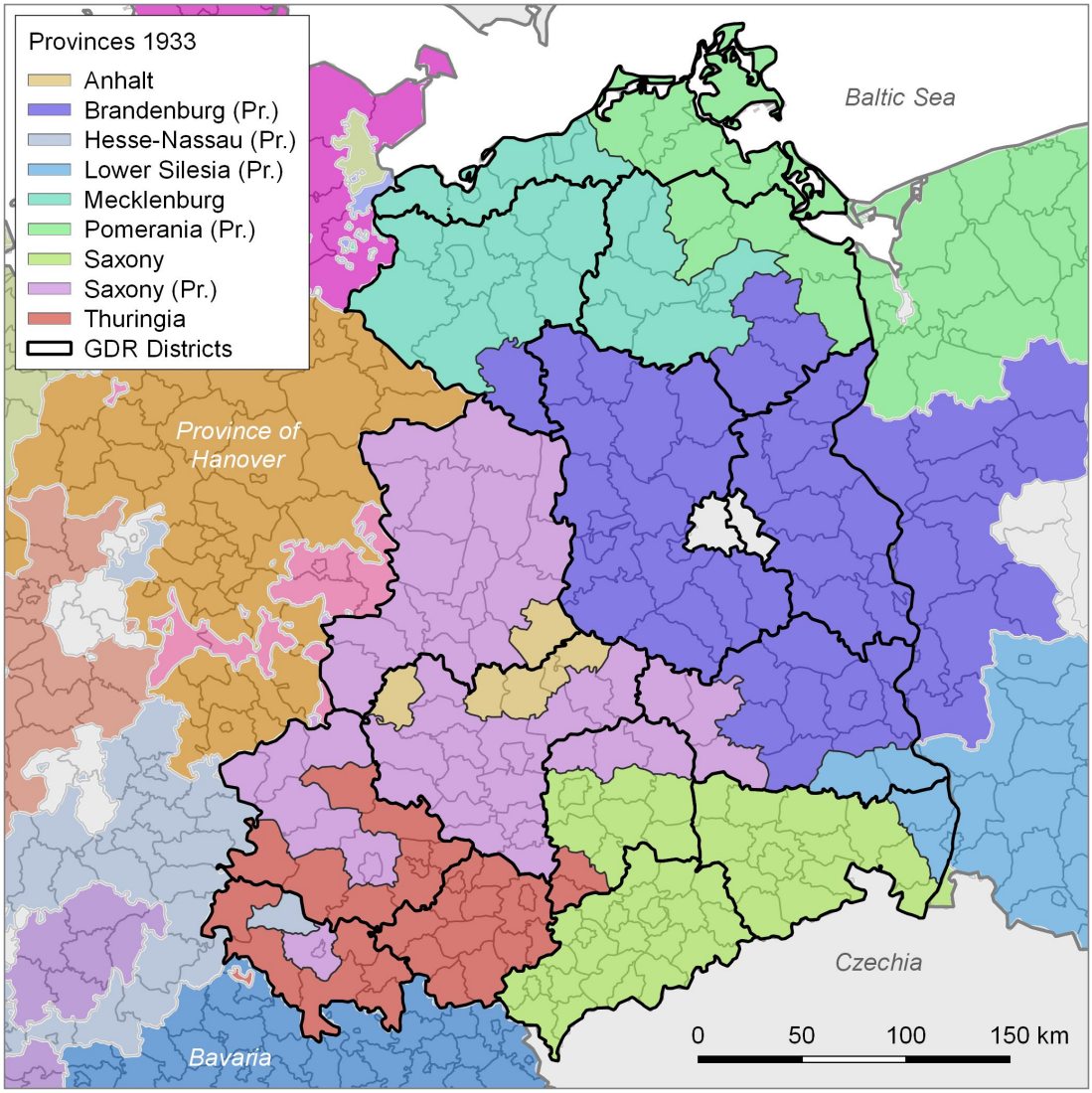
Notes: This figure plots the global prevalence of autocratic regimes between 1800 and 2010 as defined in the Polity IV database. See Data Appendix A for detailed information on all variables.

Figure B.2: Out-Migration from East Germany



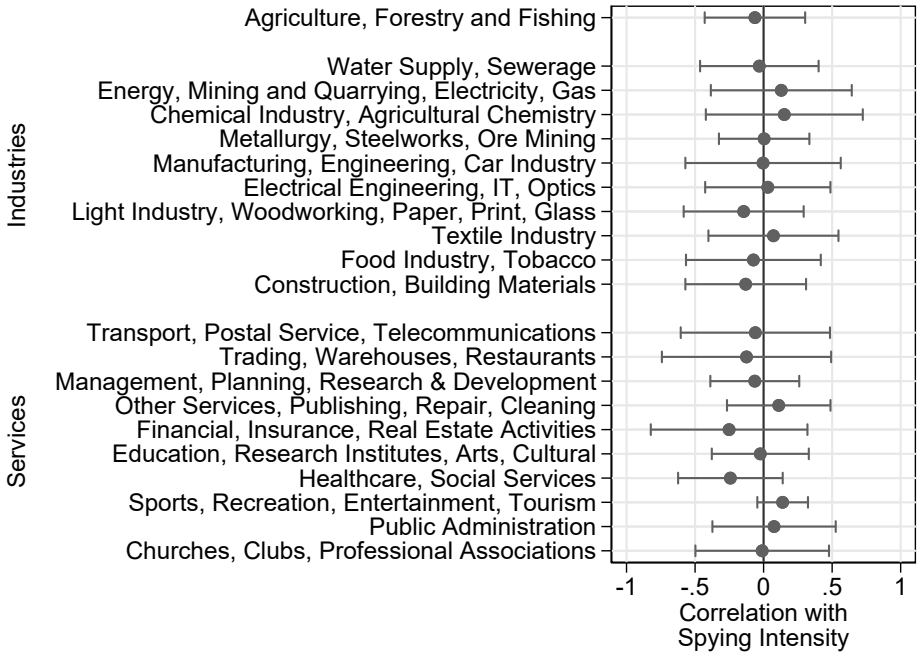
Notes: This graph shows the annual pattern of out-migration from East Germany over the period 1951–1989. It shows the number of East Germans (in 1,000) who migrated out of the country (circles) as well as the annual number of successful illegal border crossings from East Germany to the West (hollow squares, in 1,000). The three vertical lines indicate (from left to right) the People's Uprising on and around June 17, 1953, the construction of the Berlin Wall on August 13, 1961, and the fall of the Berlin Wall on November 9, 1989. See Data Appendix A for detailed information on all variables.

Figure B.3: GDR Districts and Provinces of the Weimar Republic



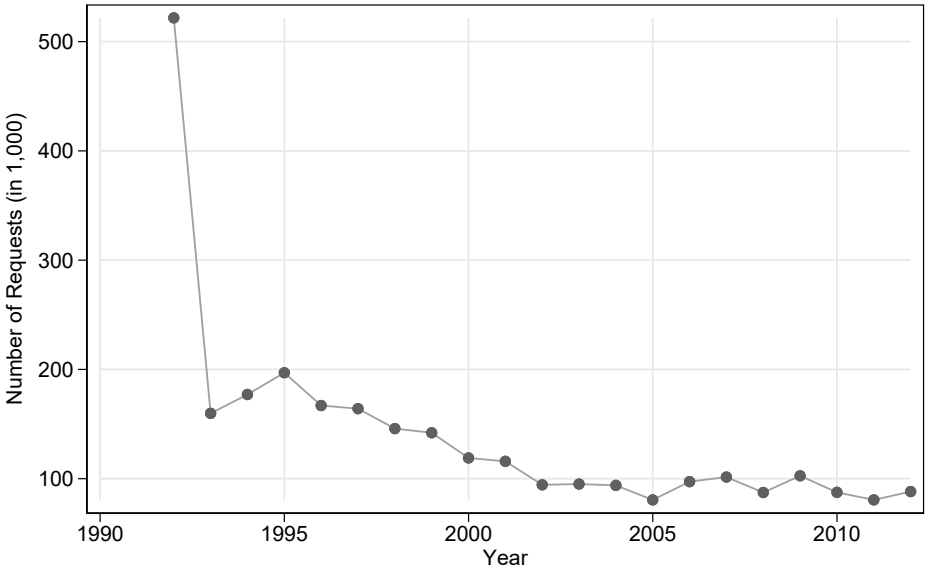
Notes: This figure shows GDR district borders and historical borders of the states of the Weimar Republic and the Prussian provinces as of 1933. *Maps:* MPIDR and CGG (2011) and @EuroGeographics.

Figure B.4: Smoothness of Industrial Composition



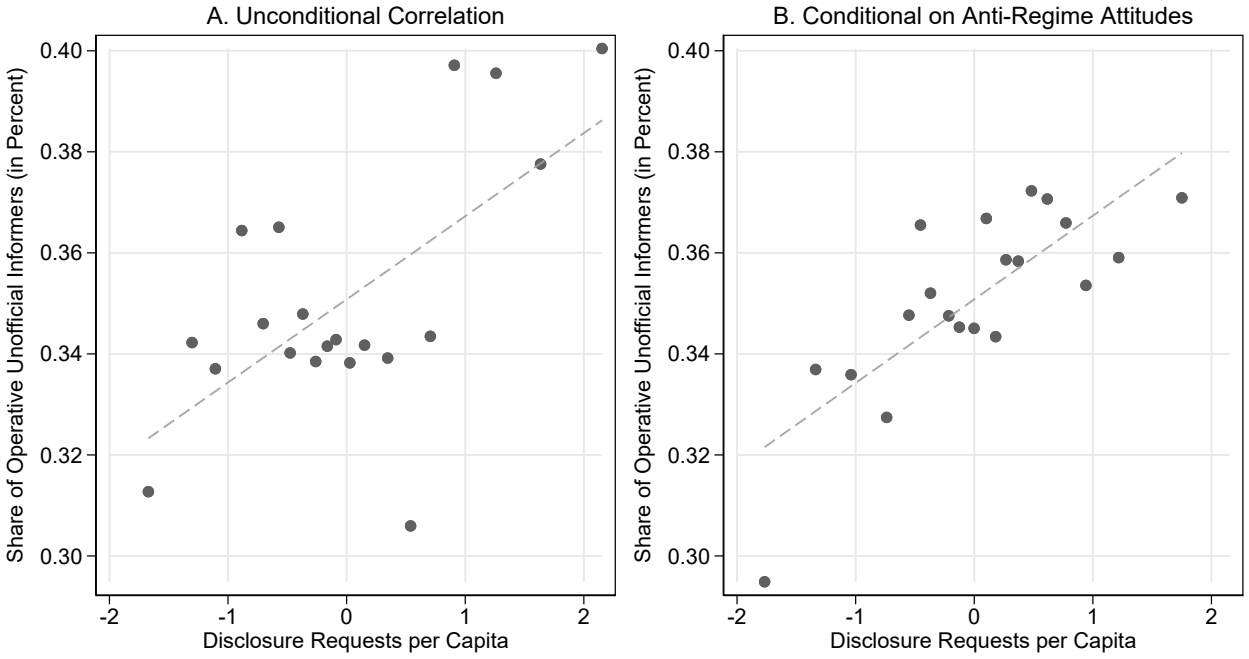
Notes: This figure tests the smoothness of county-level employment shares in various industries at district borders. Each coefficient is estimated separately by regressing the respective employment share on the spying density, the set of county-pair fixed effects as well as dummy variables for the historical provinces of the Weimar Republic. All outcome variables are standardized. Population weights are adjusted for the duplication of counties that are part of multiple county pairs. Standard errors are two-way clustered at the county and county-pair level (horizontal bars indicate 95 % confidence intervals). See Data Appendix A for detailed information on all variables.

Figure B.5: Number of Requests for the Inspection of Stasi Files



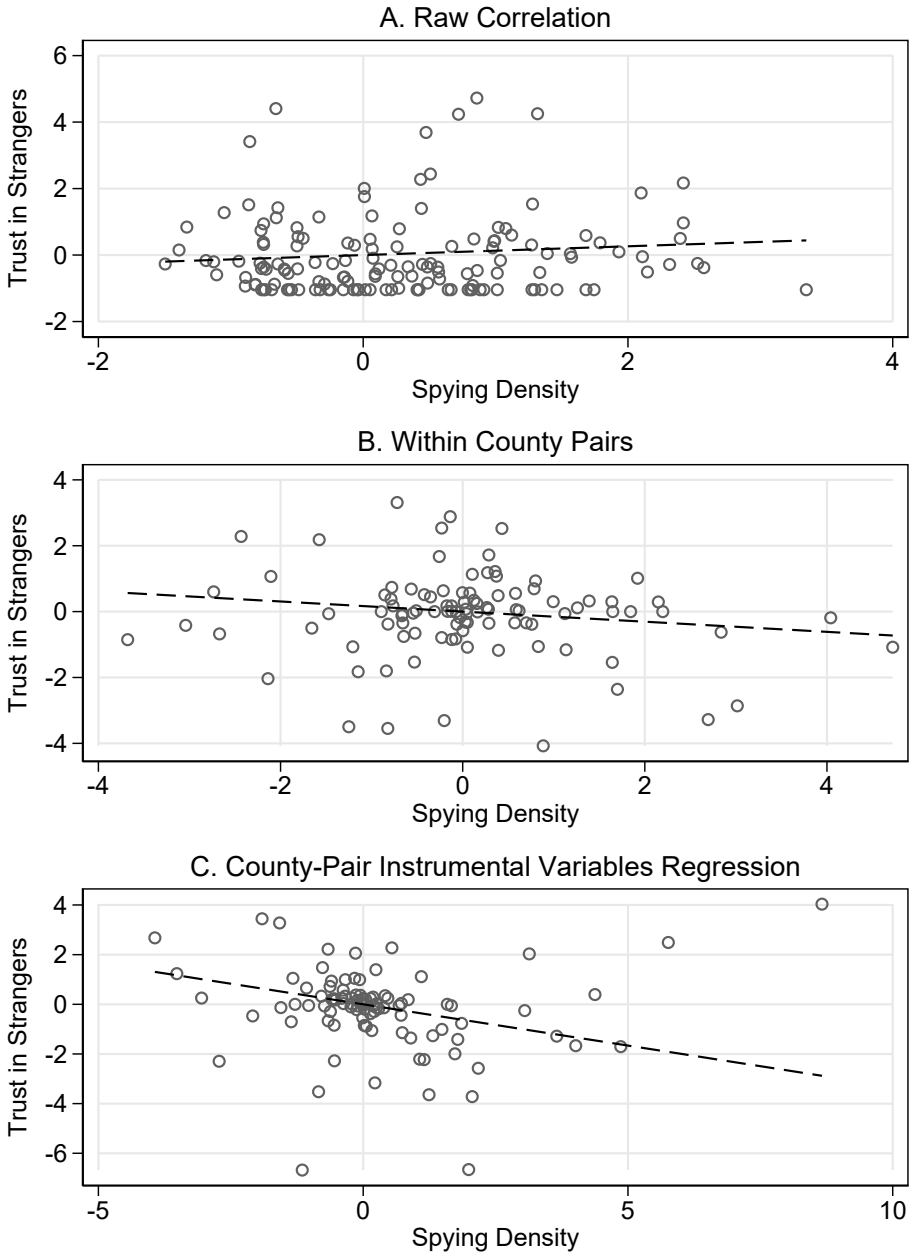
Notes: This graph plots the annual number of requests to inspect Stasi files. See Data Appendix A for detailed information on all variables.

Figure B.6: Regional Disclosure Requests and the Number of Informers



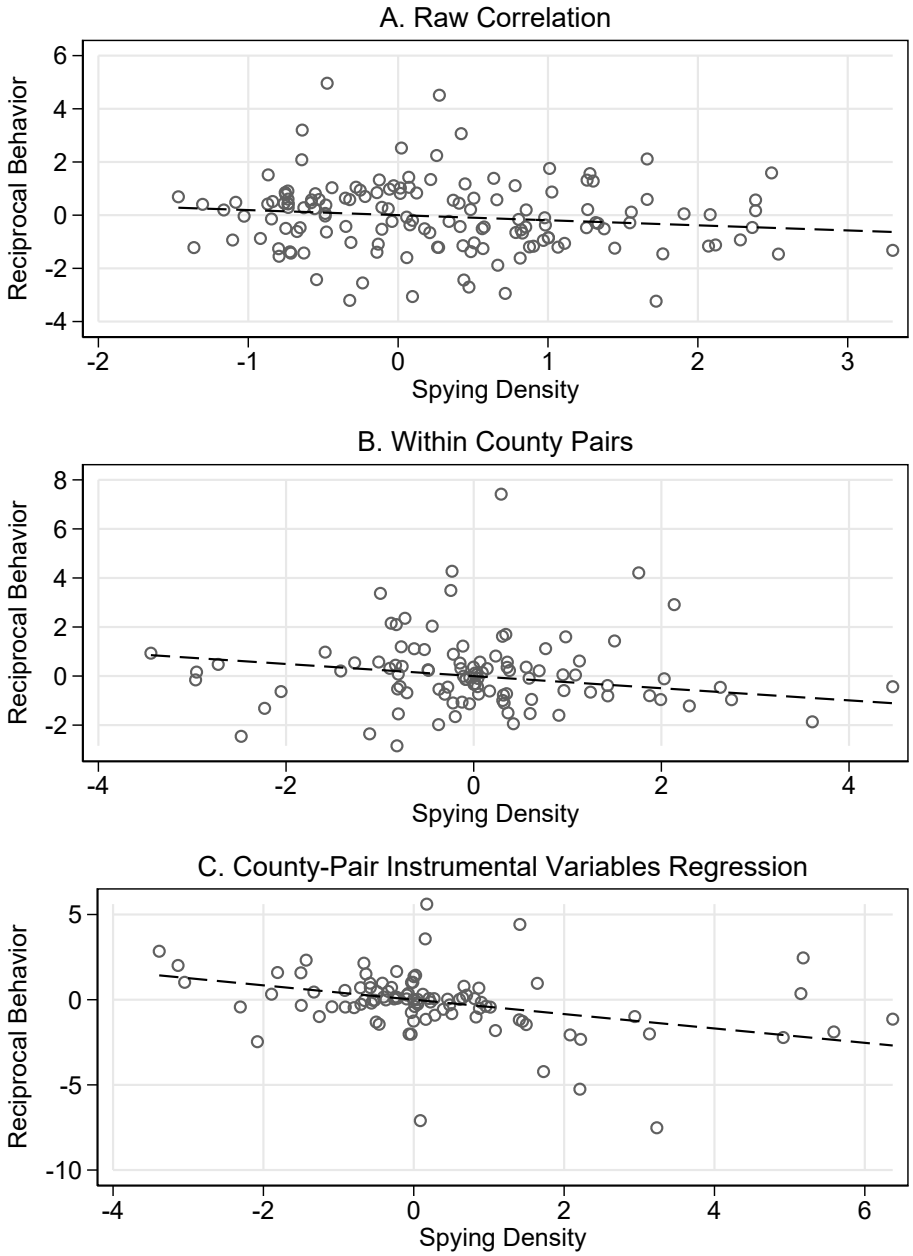
Notes: The two binned scatter plots show the district-level correlation between the annual number of disclosure requests per capita between 1992–2016 and the average number of operative Stasi informers in the 1980s relative to a district's average population between 1980 and 1988. Panel A shows the raw correlation between both variables, while Panel B plots the corresponding correlation when controlling for two measures of anti-regime attitudes: (i) the number of exit visa applications per capita as of December 31, 1988, and (ii) the date the first protest took place in a given district during the Peaceful Revolution in 1989. We standardize the number of disclosure requests per capita within each year. See Data Appendix A for detailed information on all variables.

Figure B.7: Stasi Spying versus Trust in Strangers



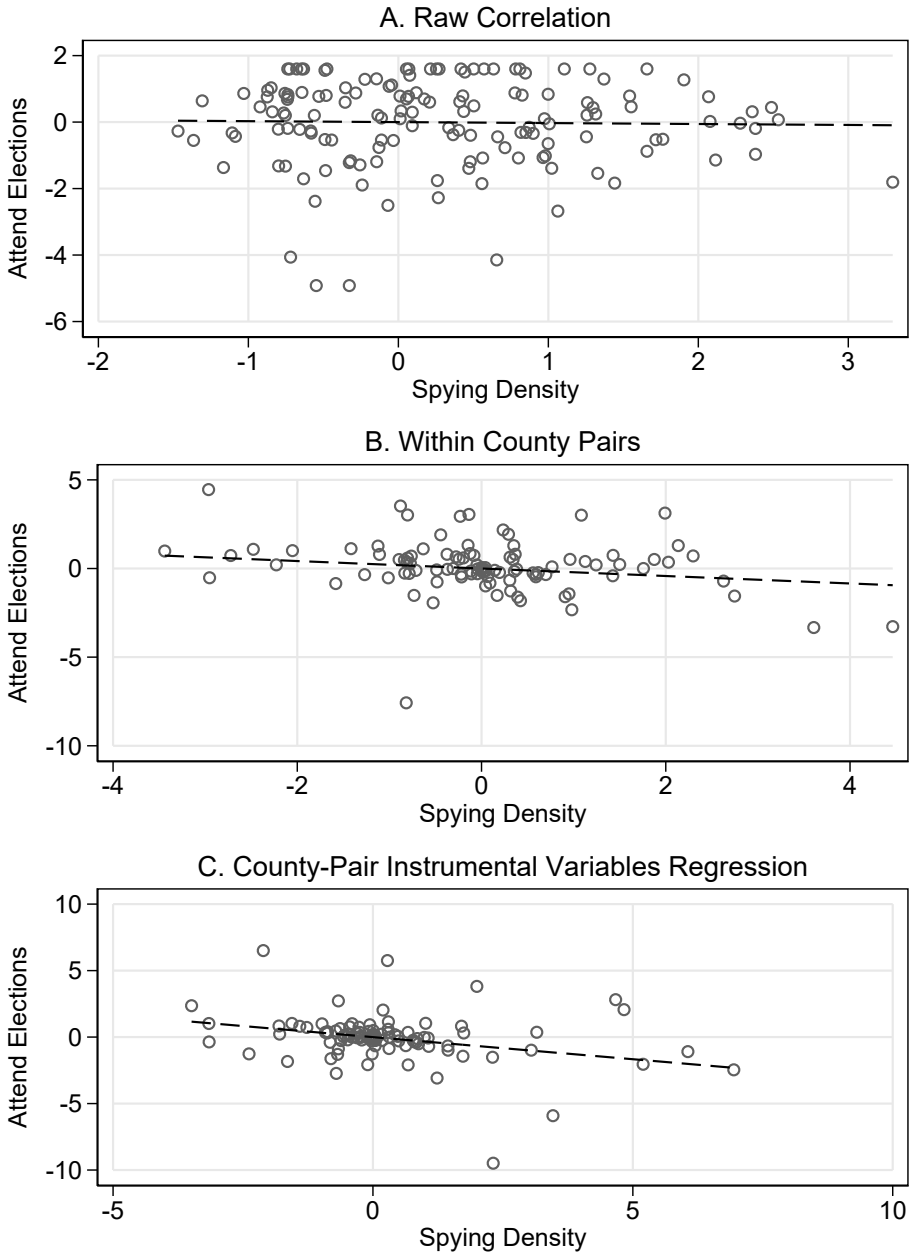
Notes: This scatter plot illustrates the county-level relation between the number of unofficial informers per capita and the share of individuals who trust in strangers. Each circle represent a single county. Panel A depicts the raw correlation in the full sample using all counties. Panel B shows the correlation within county pairs at GDR district borders, thus accounting for county-pair fixed effects. Panel C visualizes the relationship when instrumenting the county-level spying density with the leave-out average distict-level intensity, controlling for covariates (see Section 4.1 for details) and accounting for county-pair as well as Weimar province fixed effects (see column (6) in Tables 2 and 3). Variables have been standardized and centered around zero for better comparability. See Data Appendix A for detailed information on all variables.

Figure B.8: Stasi Spying versus Reciprocal Behavior



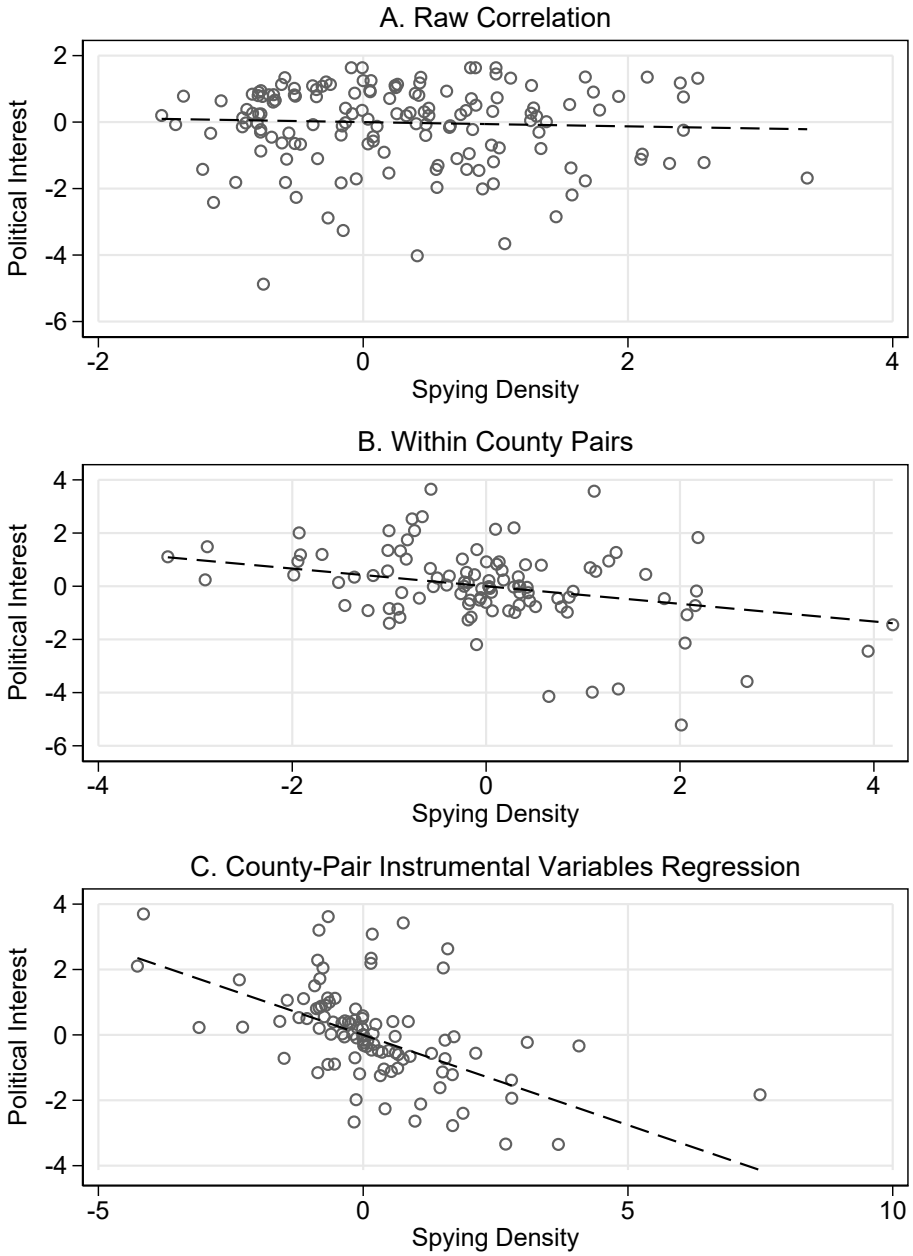
Notes: This scatter plot illustrates the county-level relation between the number of unofficial informers per capita and individuals' mean reciprocal behavior. Each circle represent a single county. Panel A depicts the raw correlation in the full sample using all counties. Panel B shows the correlation within county pairs at GDR district borders, thus accounting for county-pair fixed effects. Panel C visualizes the relationship when instrumenting the county-level spying density with the leave-out average distict-level intensity, controlling for covariates (see Section 4.1 for details) and accounting for county-pair as well as Weimar province fixed effects (see column (6) in Tables 2 and 3). Variables have been standardized and centered around zero for better comparability. See Data Appendix A for detailed information on all variables.

Figure B.9: Stasi Spying versus Attend Elections



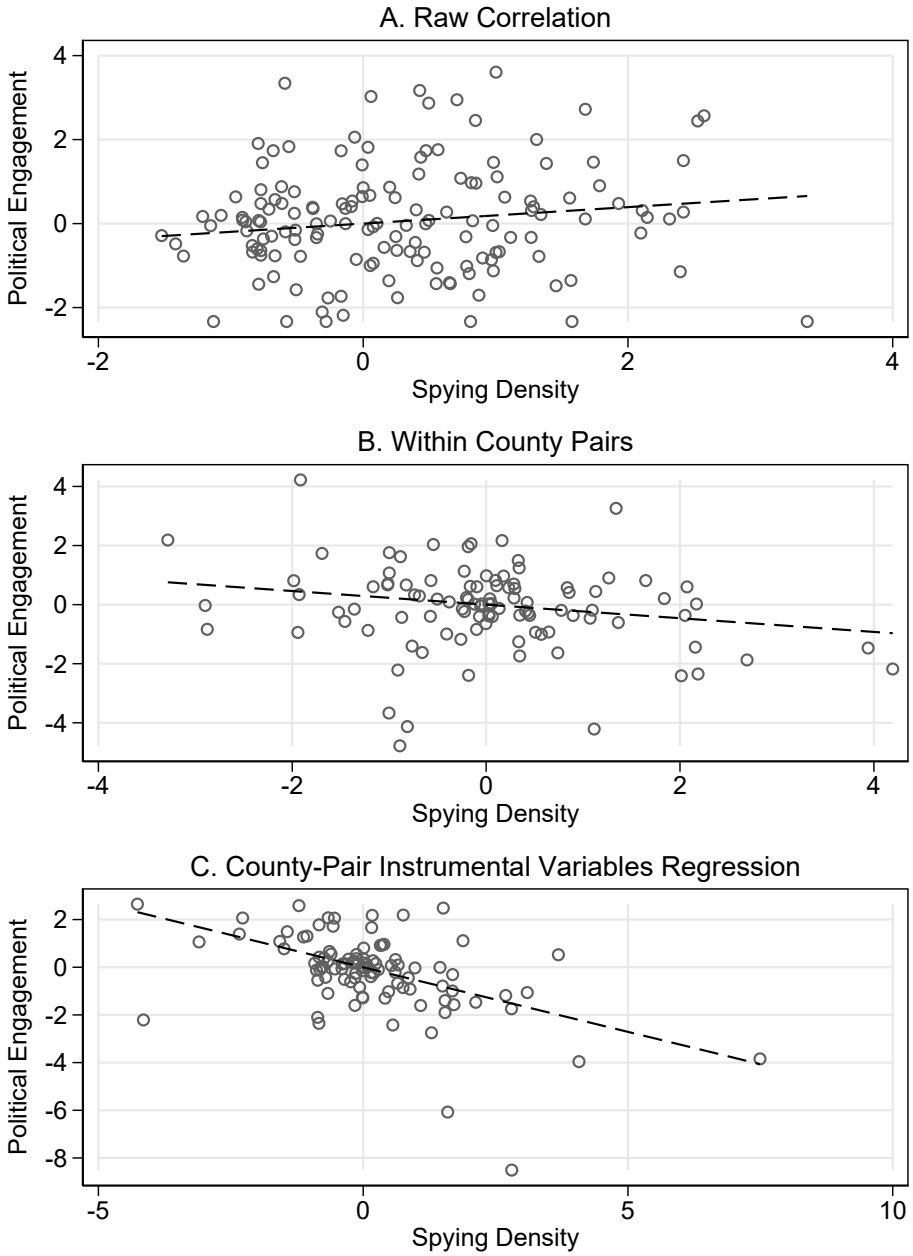
Notes: This scatter plot illustrates the county-level relation between the number of unofficial informers per capita and average intention to attend elections. Each circle represent a single county. Panel A depicts the raw correlation in the full sample using all counties. Panel B shows the correlation within county pairs at GDR district borders, thus accounting for county-pair fixed effects. Panel C visualizes the relationship when instrumenting the county-level spying density with the leave-out average distict-level intensity, controlling for covariates (see Section 4.1 for details) and accounting for county-pair as well as Weimar province fixed effects (see column (6) in Tables 2 and 3). Variables have been standardized and centered around zero for better comparability. See Data Appendix A for detailed information on all variables.

Figure B.10: Stasi Spying versus Political Interest



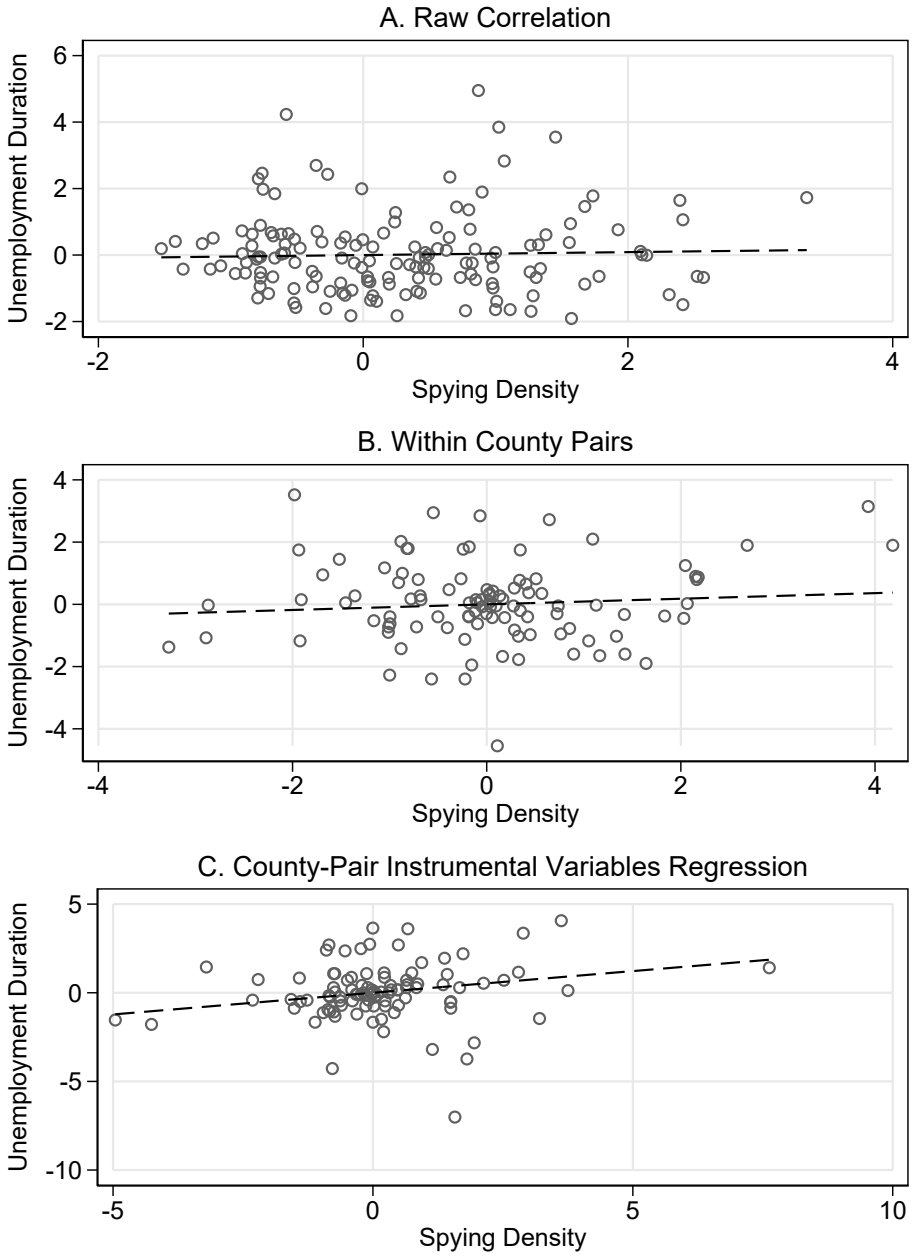
Notes: This scatter plot illustrates the county-level relation between the number of unofficial informers per capita and average probability to be interested in politics. Each circle represent a single county. Panel A depicts the raw correlation in the full sample using all counties. Panel B shows the correlation within county pairs at GDR district borders, thus accounting for county-pair fixed effects. Panel C visualizes the relationship when instrumenting the county-level spying density with the leave-out average distict-level intensity, controlling for covariates (see Section 4.1 for details) and accounting for county-pair as well as Weimar province fixed effects (see column (6) in Tables 2 and 3). Variables have been standardized and centered around zero for better comparability. See Data Appendix A for detailed information on all variables.

Figure B.11: Stasi Spying versus Political Engagement



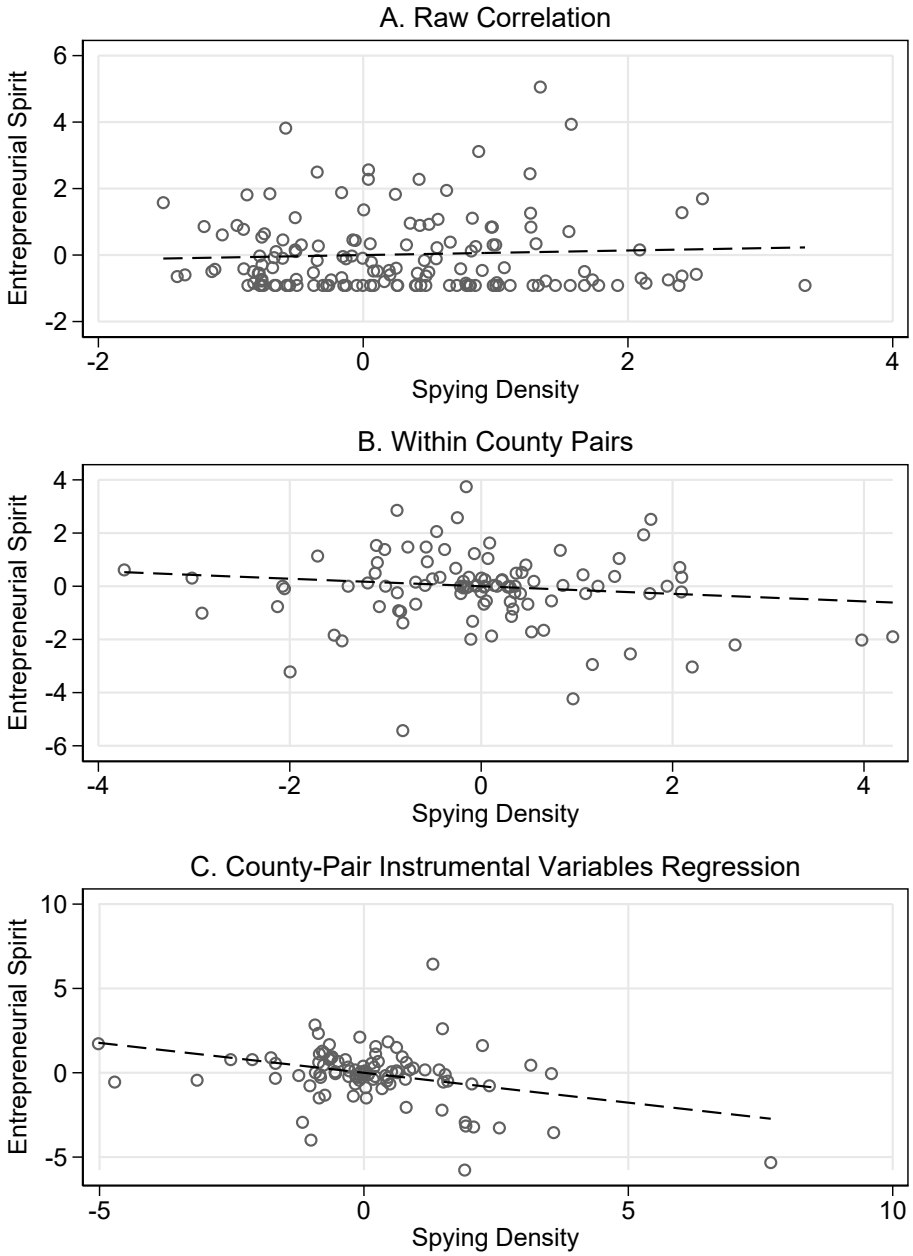
Notes: This scatter plot illustrates the county-level relation between the number of unofficial informers per capita and the average probability to be engaged in politics. Each circle represent a single county. Panel A depicts the raw correlation in the full sample using all counties. Panel B shows the correlation within county pairs at GDR district borders, thus accounting for county-pair fixed effects. Panel C visualizes the relationship when instrumenting the county-level spying density with the leave-out average distict-level intensity, controlling for covariates (see Section 4.1 for details) and accounting for county-pair as well as Weimar province fixed effects (see column (6) in Tables 2 and 3). Variables have been standardized and centered around zero for better comparability. See Data Appendix A for detailed information on all variables.

Figure B.12: Stasi Spying versus Unemployment Duration



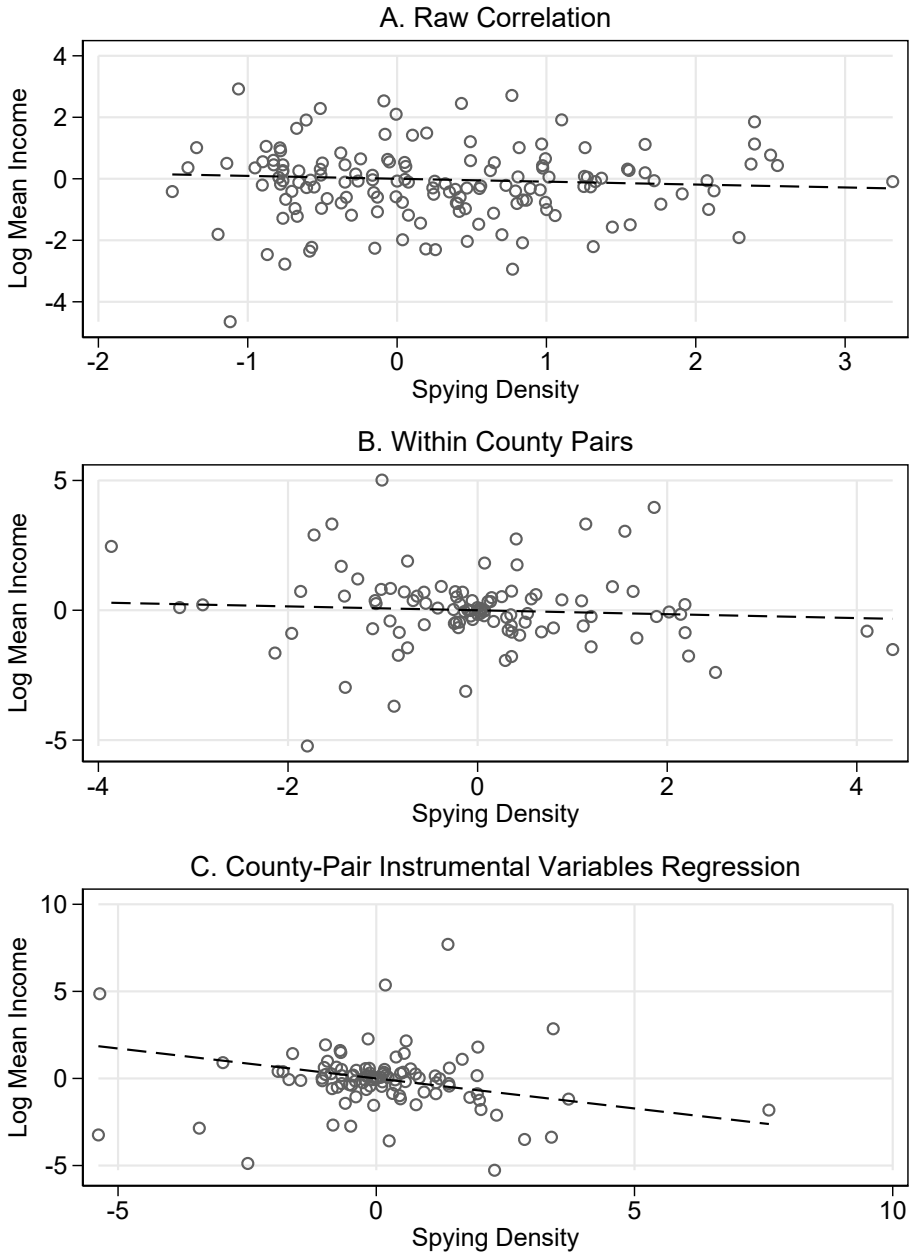
Notes: This scatter plot illustrates the county-level relation between the number of unofficial informers per capita and the average unemployment duration. Each circle represent a single county. Panel A depicts the raw correlation in the full sample using all counties. Panel B shows the correlation within county pairs at GDR district borders, thus accounting for county-pair fixed effects. Panel C visualizes the relationship when instrumenting the county-level spying density with the leave-out average distict-level intensity, controlling for covariates (see Section 4.1 for details) and accounting for county-pair as well as Weimar province fixed effects (see column (6) in Tables 2 and 3). Variables have been standardized and centered around zero for better comparability. See Data Appendix A for detailed information on all variables.

Figure B.13: Stasi Spying versus Entrepreneurial Spirit



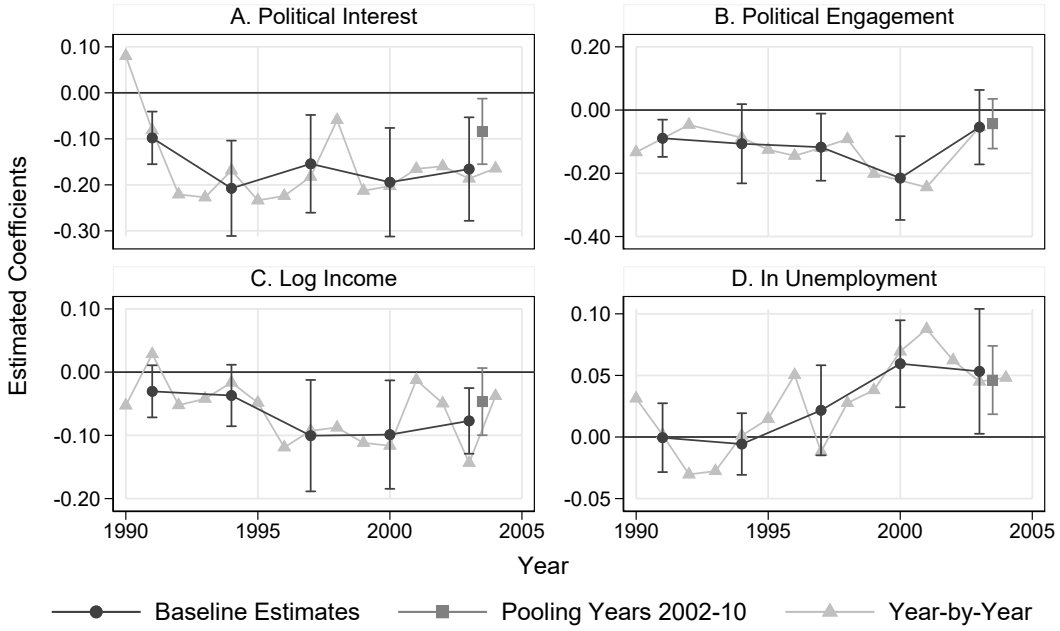
Notes: This scatter plot illustrates the county-level relation between the number of unofficial informers per capita and the average self-employment probability. Each circle represent a single county. Panel A depicts the raw correlation in the full sample using all counties. Panel B shows the correlation within county pairs at GDR district borders, thus accounting for county-pair fixed effects. Panel C visualizes the relationship when instrumenting the county-level spying density with the leave-out average distict-level intensity, controlling for covariates (see Section 4.1 for details) and accounting for county-pair as well as Weimar province fixed effects (see column (6) in Tables 2 and 3). Variables have been standardized and centered around zero for better comparability. See Data Appendix A for detailed information on all variables.

Figure B.14: Stasi Spying versus Log Mean Income



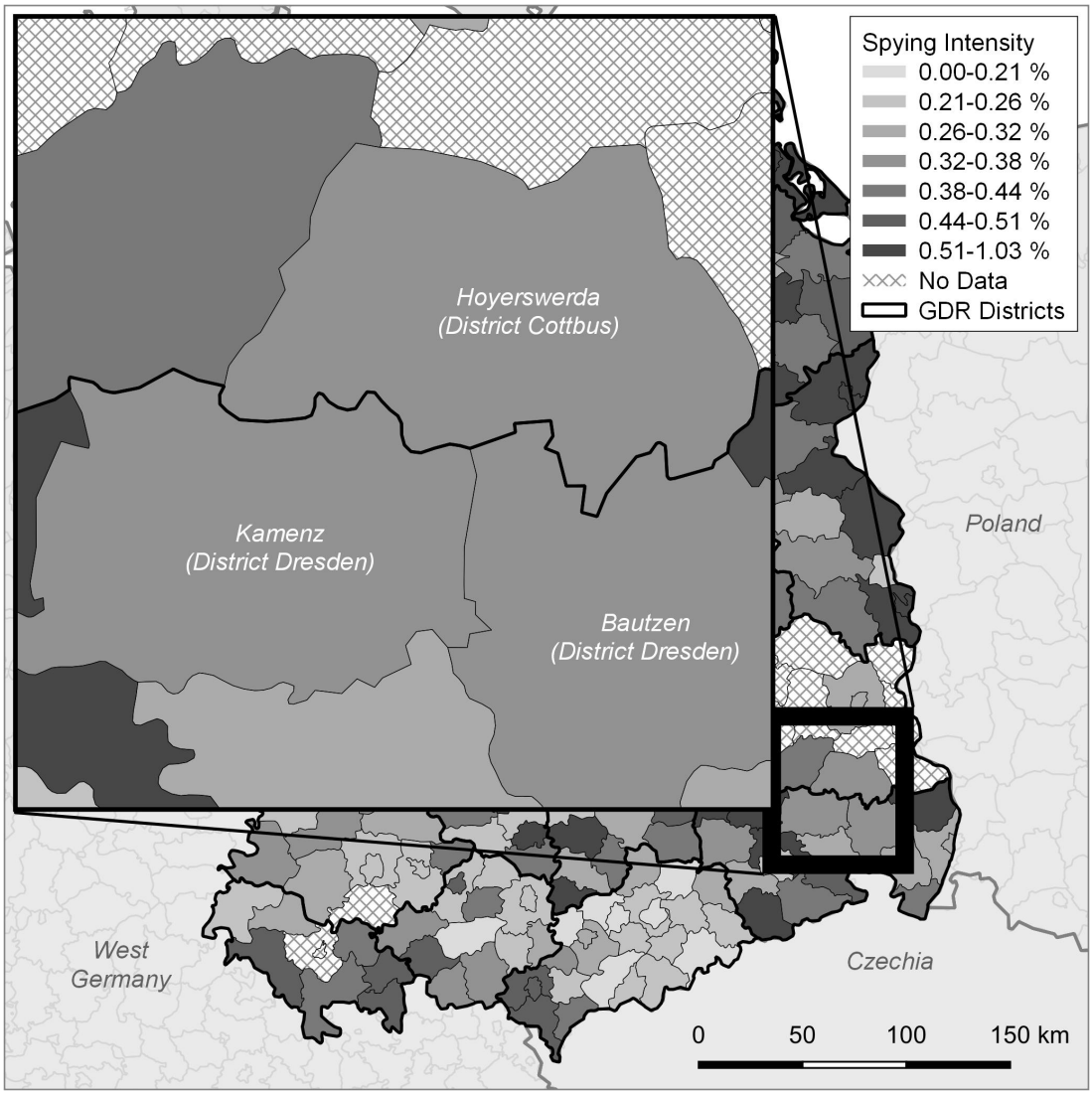
Notes: This scatter plot illustrates the county-level relation between the number of unofficial informers per capita and the average income. Each circle represent a single county. Panel A depicts the raw correlation in the full sample using all counties. Panel B shows the correlation within county pairs at GDR district borders, thus accounting for county-pair fixed effects. Panel C visualizes the relationship when instrumenting the county-level spying density with the leave-out average distict-level intensity, controlling for covariates (see Section 4.1 for details) and accounting for county-pair as well as Weimar province fixed effects (see column (6) in Tables 2 and 3). Variables have been standardized and centered around zero for better comparability. See Data Appendix A for detailed information on all variables.

Figure B.15: The Effect of Spying – Dynamics Over Time (Robustness)



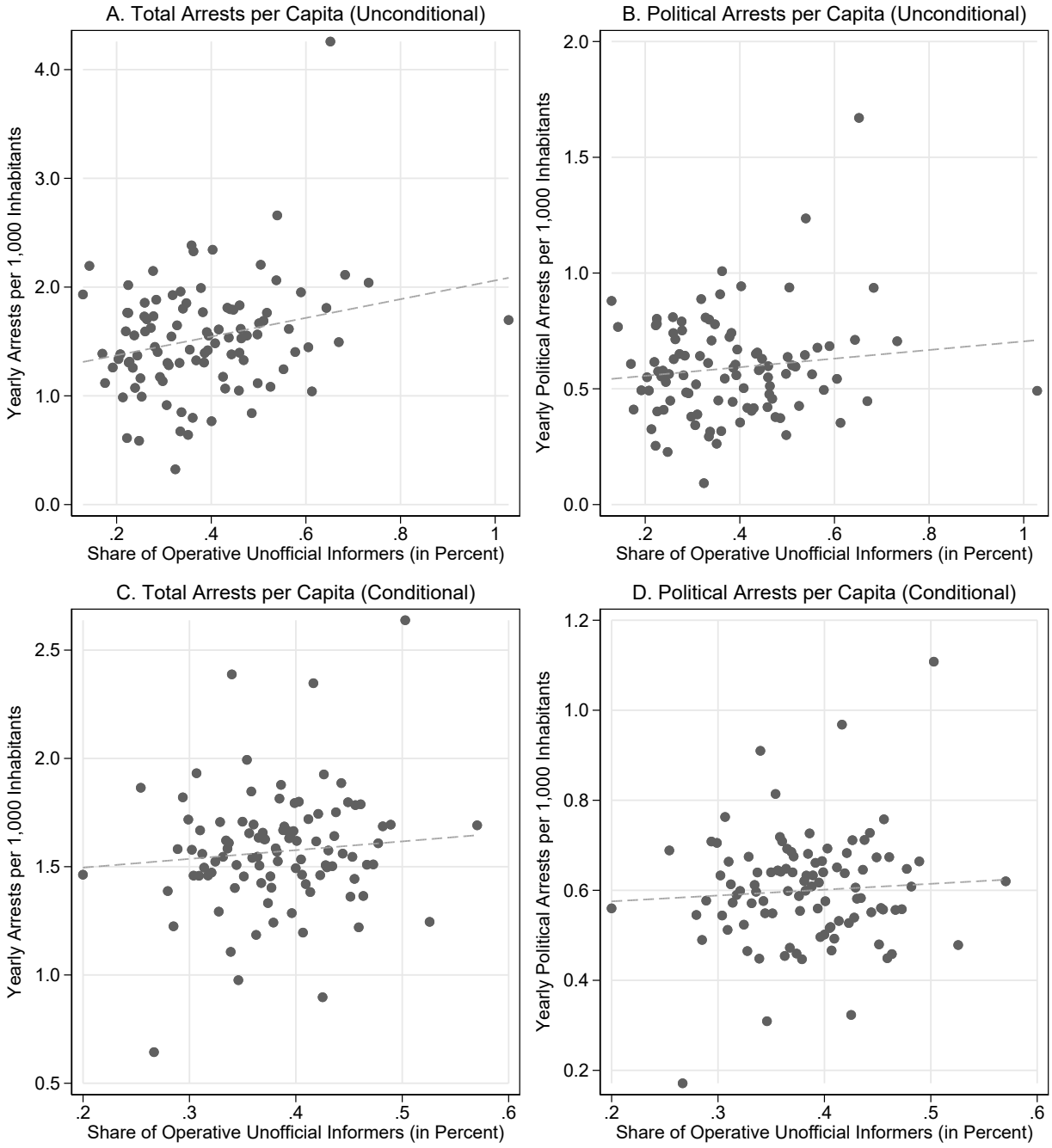
Notes: This figure shows the effect of a one standard deviation increase in surveillance intensity on different measures of individual civic capital and economic performance for different periods of our sample. Estimates are based on our IV specification. Dark circles show our baseline estimates for the period between 1990 and 2004 (excluding the years 2005–10), where we pool data over three year periods (1990–92, 1993–95, etc.); light squares report alternative estimates for the last period, pooling the years 2002–10 instead. Moreover, light triangles show the corresponding results when estimating the effects year-by-year. In all regressions, we interact the set of county-pair fixed effects, the dummies for historical provinces of the Weimar Republic, the dummy variable indicating the presence of a Stasi on-site office, and our full set of controls (as described in Section 4.1) with year dummies. Outcomes in Panels A and B are standardized. Cross-sectional weights are adjusted for multiple person-year observations and the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level (vertical bars indicate 95 % confidence intervals). See Data Appendix A for detailed information on all variables.

Figure B.16: County Duplication at GDR District Borders



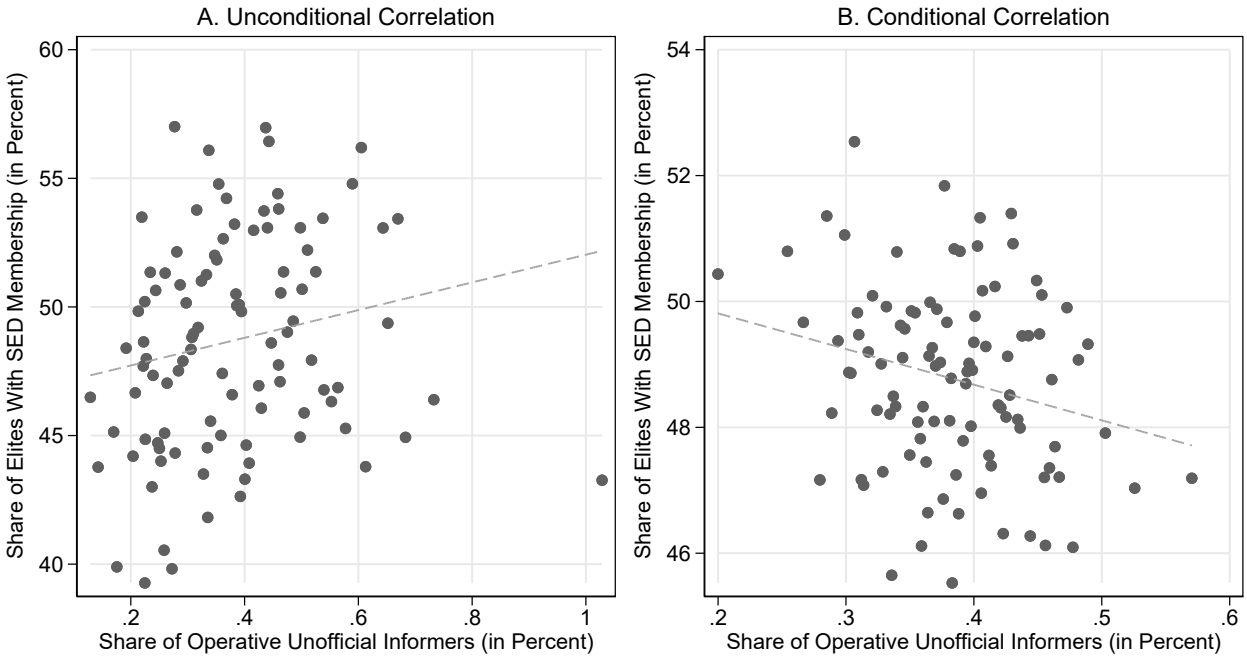
Notes: This figure shows an example of county duplication in the longest-border county-pair sample. Counties Kamenz and Bautzen both share the longest district border with Hoyerswerda County in the north. Hoyerswerda will thus be duplicated and appear in multiple county pairs. Gray tones indicate the county-level surveillance density measured by the average yearly share of operative unofficial informers relative to the population between 1980 and 1988.
Source: See Data Appendix A. *Maps:* MPIDR and CGG (2011) and @EuroGeographics.

Figure B.17: Surveillance Intensity and Arrests



Notes: This graph plots the correlation between the county-level spying density and the number of (political) arrests per capita using binned scatter plots. Panels A and C depict the correlation between the spying density and the *total* number of arrests, Panels B and D the corresponding correlation with the number of *political* arrests. Panels A and B show unconditional correlations when using the full sample of counties, Panels C and D plot the respective correlations when limiting the sample to contiguous county pairs that straddle a GDR district border and accounting for county-pair fixed effects, dummies for historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, and the full set of control variables as defined in Section 4.1. See Data Appendix A for detailed information on all variables.

Figure B.18: Informer Density and Socialist Indoctrination



Notes: The two binned scatter plots show the correlation between the county-level spying density and the intensity of socialist indoctrination – as measured by the county-level share of elites who were members of the SED. Panel A depicts the unconditional correlation using the full sample of counties, Panel B the corresponding correlation when limiting the sample to contiguous county pairs that straddle a GDR district border and conditioning on the set of county-pair fixed effects, dummies for historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, and county-level covariates as defined in Section 4.1. See Data Appendix A for detailed information on all variables.

C Appendix – Additional Results

Appendix C presents additional regression results referred to in the manuscript. In Subsection C.1, we present the results of different identification checks, estimates of various robustness checks are presented in Subsection C.2. In Subsection C.3, we plot results analyzing potential, alternative mechanisms driving the observed (economic) effects. Last, Subsection C.4 presents regression results investigating the channels behind the observed effects on civic capital and economic performance.

C.1 Identification Checks

Table C.1.1: Covariate Smoothness at GDR District Borders

	All Counties	Border County Pair Sample		
	(1)	(2)	(3)	(4)
Log Mean Population 1980–1988	-0.588***	-0.316***	-0.269**	-0.137
	(0.132)	(0.113)	(0.119)	(0.232)
Log County Size	0.300***	0.199*	0.028	-0.054
	(0.092)	(0.112)	(0.078)	(0.209)
City County	-0.387***	-0.174	-0.085	0.012
	(0.122)	(0.170)	(0.076)	(0.019)
Share of Population Aged under 15, 1989	0.353***	0.302**	0.131	-0.105
	(0.098)	(0.122)	(0.108)	(0.178)
Share of Population Aged over 64, 1989	-0.200**	-0.235**	-0.084	0.093
	(0.095)	(0.110)	(0.114)	(0.258)
Log Industrial Output 1989	-0.429***	-0.253	-0.086	-0.078
	(0.118)	(0.152)	(0.134)	(0.227)
Share Agricultural Employment 09/1989	0.417***	0.263*	0.089	-0.066
	(0.098)	(0.137)	(0.125)	(0.198)
Employment Share Energy Industry 09/1989	0.120	0.158	0.177	0.110
	(0.095)	(0.136)	(0.175)	(0.256)
Employment Share Textile and Clothing 09/1989	-0.160**	-0.205*	-0.169	0.076
	(0.065)	(0.115)	(0.120)	(0.282)
Share of Cooperative Workers 09/1989	0.404***	0.271**	0.115	-0.109
	(0.097)	(0.128)	(0.120)	(0.200)
Uprising 1953: Strike, Demonstration, Riot	-0.130*	-0.087	-0.064	0.175
	(0.076)	(0.098)	(0.093)	(0.207)
Electoral Turnout 1933	-0.260**	-0.197	-0.020	-0.075
	(0.108)	(0.132)	(0.093)	(0.189)
Vote Share Nazi Party (NSDAP) 1933	0.387***	0.214**	0.122	-0.036
	(0.108)	(0.102)	(0.105)	(0.201)
Vote Share Communist Party (KPD) 1933	-0.437***	-0.232*	-0.143	0.050
	(0.117)	(0.122)	(0.119)	(0.145)
Share Protestants 1925	0.172***	0.184***	0.215***	-0.001
	(0.053)	(0.068)	(0.079)	(0.128)
Share Jews 1925	-0.417**	-0.093	-0.068	0.225
	(0.210)	(0.136)	(0.097)	(0.193)
Share of White Collar Workers 1933	-0.448***	-0.129	-0.040	0.194
	(0.140)	(0.118)	(0.117)	(0.181)
Self-Employment Rate 1933	0.451***	0.130	0.119	0.074
	(0.094)	(0.117)	(0.114)	(0.157)
Unemployment Rate 1933	-0.555***	-0.298***	-0.106	0.122
	(0.103)	(0.110)	(0.097)	(0.217)
Weimar Province Fixed Effects			Yes	Yes
County-Pair Fixed Effects				Yes
Counties	148	78	78	78
County Pairs		51	51	51
Joint *F*-Test	7.883	4.316	2.835	1.240
p-value	0.000	0.000	0.002	0.265

Notes: This table presents the results of our covariate smoothness test. In column (1), we separately regress each covariate on the spying density using the full set of counties in the SOEP. Specification (2) is based on our border county-pair sample. Column (3) adds the set of Weimar Province fixed effects to control for persistent differences across Weimar Provinces. In column (4), we further include border county-pair fixed effects, identification is thus only within county pairs at district borders. All variables have been standardized in the respective sample. Population weights are adjusted for duplications of counties that are part of multiple county pairs. Standard errors are two-way clustered at the county and county-pair level. Significance levels are * $p < .1$, ** $p < .05$, *** $p < .01$. The reported *F*-test statistics and the corresponding *p*-values test the null hypothesis of all coefficients being jointly equal to zero in a stacked regression (Lee and Lemieux, 2010). See Data Appendix A for detailed information on all variables.

Table C.1.2: The Effect of Spying by Weimar Provinces

	Trust in Strangers (1)	Reciprocal Behavior (2)	Attend Elections (3)	Political Interest (4)	Political Engagem. (5)	Unemploy. Duration (6)	Self-Emp. Probability (7)	Log Mean Income (8)
District-Level Spying Density								
× Different Weimar Province	-0.142**	-0.017	-0.108	-0.215**	-0.317***	0.014	-0.014*	-0.078***
	(0.057)	(0.093)	(0.078)	(0.092)	(0.056)	(0.013)	(0.007)	(0.028)
× Same Weimar Province	-0.092**	-0.178***	-0.107**	-0.273***	-0.180***	0.014**	-0.016***	-0.053*
	(0.038)	(0.047)	(0.044)	(0.045)	(0.037)	(0.006)	(0.005)	(0.027)
Number of Observations	1,795	1,588	1,583	1,736	1,736	1,719	1,611	1,482

Notes: This table shows the effect of a one standard deviation increase in the county-level spying density on individual civic capital and economic performance when allowing for heterogeneous treatment effects for county pairs with similar and different cultural heritage. The underlying econometric model is described in equation (1), where the county-level spying density is fully interacted with a dummy variable indicating whether two counties in a pair belonged to the same state or Prussian province of the Weimar Republic before World War II. The spying density and outcomes in columns (1)–(5) have been standardized. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include border county-pair fixed effects, dummies for historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for individuals' age and gender, indicators for each subpopulation, as well as county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for duplicates of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table C.1.3: Instrumental Variables Results

	Leave-Out District Mean			District-Level Average		
	First Stage (1)	Reduced Form (2)	Second Stage (3)	First Stage (4)	Reduced Form (5)	Second Stage (6)
Panel A – Trust in Strangers ($N = 1,795$)						
District-Level Spying Density	0.953***	-0.094**		0.975***	-0.094***	
	(0.275)	(0.038)		(0.149)	(0.030)	
County-Level Spying Density			-0.098***			-0.096***
			(0.034)			(0.028)
Weak Instrument F-Statistic	12.03			42.73		
Underidentification p-Value	0.065			0.036		
Panel B – Reciprocal Behavior ($N = 1,588$)						
District-Level Spying Density	0.976***	-0.178***		0.973***	-0.151***	
	(0.249)	(0.044)		(0.147)	(0.040)	
County-Level Spying Density			-0.183**			-0.156***
			(0.069)			(0.053)
Weak Instrument F-Statistic	15.40			43.92		
Underidentification p-Value	0.050			0.031		
Panel C – Attend Elections ($N = 1,583$)						
District-Level Spying Density	0.981***	-0.107**		0.981***	-0.106***	
	(0.256)	(0.044)		(0.145)	(0.039)	
County-Level Spying Density			-0.109**			-0.108**
			(0.052)			(0.046)
Weak Instrument F-Statistic	14.68			45.91		
Underidentification p-Value	0.061			0.038		
Panel D – Political Interest ($N = 1,736$)						
District-Level Spying Density	1.036***	-0.270***		1.075***	-0.244***	
	(0.237)	(0.043)		(0.141)	(0.038)	
County-Level Spying Density			-0.261***			-0.227***
			(0.069)			(0.044)
Weak Instrument F-Statistic	19.12			58.12		
Underidentification p-Value	0.030			0.012		
Panel E – Political Engagement ($N = 1,736$)						
District-Level Spying Density	1.036***	-0.188***		1.075***	-0.168***	
	(0.237)	(0.034)		(0.141)	(0.027)	
County-Level Spying Density			-0.181***			-0.156***
			(0.047)			(0.034)
Weak Instrument F-Statistic	19.12			58.12		
Underidentification p-Value	0.030			0.012		
Panel F – Unemployment Duration ($N = 1,719$)						
District-Level Spying Density	1.059***	0.014**		1.089***	0.012**	
	(0.232)	(0.006)		(0.138)	(0.005)	
County-Level Spying Density			0.014***			0.011***
			(0.005)			(0.004)
Weak Instrument F-Statistic	20.81			62.11		
Underidentification p-Value	0.028			0.012		
Panel G – Self-Employment Probability ($N = 1,611$)						
District-Level Spying Density	1.016***	-0.016***		1.067***	-0.014***	
	(0.235)	(0.005)		(0.146)	(0.005)	
County-Level Spying Density			-0.016**			-0.014**
			(0.007)			(0.005)
Weak Instrument F-Statistic	18.76			53.02		
Underidentification p-Value	0.023			0.009		
Panel H – Log Mean Income ($N = 1,482$)						
District-Level Spying Density	0.986***	-0.056**		1.032***	-0.053**	
	(0.241)	(0.026)		(0.153)	(0.021)	
County-Level Spying Density			-0.056***			-0.051***
			(0.019)			(0.016)
Weak Instrument F-Statistic	16.80			45.40		
Underidentification p-Value	0.027			0.011		

Notes: This table provides additional results on our instrumental variables strategy laid out in equations (1) and (2). Column (1) provides results from the first stage of our IV specification, column (2) the corresponding reduced-form coefficient, column (3) the second-stage effect. In addition to the proposed leave-out instrument, we further use the district-level average to instrument the county-level spying density. The corresponding results are provided in columns (4) to (6). Outcomes in Panels A–E are standardized. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

C.2 Robustness Checks

Table C.2.1: Effects by Wave & Mean Effects over Time

	Trust in Strangers		Reciprocal Behavior		Attend Elections	
	Mean (1)	By Wave (2)	Mean (3)	By Wave (4)	Mean (5)	By Wave (6)
District-Level Spying Density	-0.099**		-0.155***		-0.126***	
	(0.044)		(0.054)		(0.046)	
× First Wave		-0.077*		-0.201***		-0.104**
		(0.042)		(0.047)		(0.045)
× Second Wave		-0.116***		-0.149***		-0.111**
		(0.040)		(0.045)		(0.051)
Number of Observations	1,031	1,795	934	1,588	932	1,583

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on different measures of civic capital (see columns) when allowing for differential treatment effects by survey wave or taking the mean outcome over time. The underlying econometric model is described in equations (1) and (2), using the leave-out instrument as our main regressor. Estimates in columns (1), (3), and (5) show the corresponding effects when taking the mean outcome for each individual over time (rather than pooling the information from two waves of the survey). Columns (2), (4), and (6) in turn show the results when interacting the spying density with wave fixed effects. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table C.2.2: Varying Sample Definition & Weighting Procedures

	Baseline Effect (1)	All Pairs (2)	Without Duplic. (3)	Simple Weights (4)	Without Weights (5)
Panel A – Trust in Strangers					
County-Level Spying Density	-0.098***	-0.098*	-0.058*	-0.094**	-0.065**
	(0.034)	(0.055)	(0.030)	(0.041)	(0.030)
Number of Observations	1,795	2,402	1,201	1,795	1,795
Kleibergen-Paap *F*-Statistic	12.03	3.79	93.52	8.56	25.06
Panel B – Reciprocal Behavior					
County-Level Spying Density	-0.183**	-0.189*	-0.181***	-0.228**	-0.171***
	(0.069)	(0.107)	(0.028)	(0.087)	(0.057)
Number of Observations	1,588	2,116	1,058	1,588	1,588
Kleibergen-Paap *F*-Statistic	15.40	5.18	140.17	11.98	28.38
Panel C – Attend Elections					
County-Level Spying Density	-0.109**	-0.129	-0.063*	-0.127**	-0.076**
	(0.052)	(0.094)	(0.032)	(0.062)	(0.038)
Number of Observations	1,583	2,111	1,055	1,583	1,583
Kleibergen-Paap *F*-Statistic	14.68	4.66	131.15	10.90	27.82
Panel D – Political Interest					
County-Level Spying Density	-0.261***	-0.283**	-0.210***	-0.305***	-0.161***
	(0.069)	(0.120)	(0.015)	(0.089)	(0.038)
Number of Observations	1,736	2,281	1,130	1,736	1,736
Kleibergen-Paap *F*-Statistic	19.12	5.67	69.00	16.12	24.22
Panel E – Political Engagement					
County-Level Spying Density	-0.181***	-0.066	-0.117***	-0.200***	-0.081**
	(0.047)	(0.058)	(0.029)	(0.053)	(0.033)
Number of Observations	1,736	2,281	1,130	1,736	1,736
Kleibergen-Paap *F*-Statistic	19.12	5.67	69.00	16.12	24.22
Panel F – Unemployment Duration					
County-Level Spying Density	0.014***	0.016*	0.009	0.015**	0.016***
	(0.005)	(0.009)	(0.006)	(0.006)	(0.005)
Number of Observations	1,719	2,249	1,118	1,719	1,719
Kleibergen-Paap *F*-Statistic	20.81	6.43	67.59	17.33	25.49
Panel G – Self-Employment Probability					
County-Level Spying Density	-0.016**	-0.025**	-0.015**	-0.019**	-0.016***
	(0.007)	(0.012)	(0.007)	(0.008)	(0.006)
Number of Observations	1,611	2,112	1,042	1,611	1,611
Kleibergen-Paap *F*-Statistic	18.76	6.71	65.42	16.57	23.66
Panel H – Log Mean Income					
County-Level Spying Density	-0.056***	-0.044	-0.049**	-0.053**	-0.042*
	(0.019)	(0.034)	(0.023)	(0.021)	(0.022)
Number of Observations	1,482	1,952	958	1,482	1,482
Kleibergen-Paap *F*-Statistic	16.80	5.65	79.35	15.42	21.73

Notes: This table shows the effects of a one standard deviation increase in state surveillance on civic capital and economic performance when using varying definitions of our county-pair sample and different weights. Estimates in column (1) print our baseline results shown in column (6) of Tables 2 and 3. These estimates are based on our preferred county-pair sample, survey weights being adjusted for the duplication of counties. Column (2) shows the corresponding results when using all county duplicates (excluding the smallest 1 %) and weighting county pairs by the length of their border. In turn, estimates in column (3) are based on a sample that excludes all county duplicates, dropping the smallest pairs. Estimates in column (4) base on our baseline definition of the county-pair sample, but survey weights are not adjusted for the duplication of counties. In column (5), no weights are used. The spying density, the instrument, and outcomes in Panels A–E have been standardized. The underlying econometric model is described in equations (1) and (2), using the leave-out district-level spying density as an instrument for county-level surveillance. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include border county-pair fixed effects, dummies for historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for individuals' age and gender, as well as county-level control variables (see Section 4.1 for details). Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table C.2.3: The Effects of Spying Using Administrative Data

	Voter Turnout (1)	Log Wage (2)	Unemp. Rate (3)
Panel A – Average Effects on SOEP Data			
District-Level Spying Density	-0.107**	-0.131**	0.084**
	(0.044)	(0.061)	(0.034)
Number of Observations	1,583	1,482	1,719
Adjusted R-Squared	0.121	0.251	0.161
Panel B – Average Effects on Administrative Data			
District-Level Spying Density	-0.166***	-0.072***	0.068*
	(0.048)	(0.027)	(0.039)
Number of Observations	3,505	56,284	38,158
Adjusted R-Squared	0.019	0.002	0.002
Panel C – Effects Over Time on Administrative Data			
District-Level Spying Density			
× Year 1990	-0.193***		
	(0.072)		
× Year 1992		-0.042**	
		(0.020)	
× Year 1998			0.025
			(0.042)
× Year 2009	-0.109**		
	(0.052)		
× Year 2010		-0.121***	0.093***
		(0.036)	(0.033)
Number of Observations	3,505	5,961	5,887
Adjusted R-Squared	0.020	0.003	0.002

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on different measures of local civic capital and economic performance using administrative data. The underlying econometric model is described in equations (1) and (2), using the leave-out instrument as our main regressor. To ease comparison across datasets, Panel A replicates our baseline estimates using the SOEP data and standardizing outcomes. Panel B presents average effects over time when using the administrative data, Panel C shows effects separately for the first and the last year of observation in the corresponding administrative datasets. Voter turnout is observed in March and December 1990 as well as September 2009; average daily wages are observed from 1992 to 2010 on a yearly basis, annual local unemployment rates during the period 1998–2010. We match municipalities to counties in 1990 using geographic coordinates provided by the German Federal Agency for Cartography and Geodesy. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border. In all regressions, we interact the set of county-pair fixed effects, the dummy variables for the historical provinces of the Weimar Republic, the dummy variable indicating the presence of a Stasi on-site office, and our set of control variables (see Section 4.1 for details) with year dummies. Observations are weighted by the 1990 population in column (1) and the number of workers in 1992 in columns (2) and (3), respectively. Weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table C.2.4: Alternative Measures of Spying & Controlling for County-Level Confounders

	Baseline Effect (1)	Spying IM1 + IM2 (2)	Spying IM1 IM2 + HM (3)	Cond. on Indoctri. (4)	Cond. on Pol. Arrests (5)	Cond. on All Arrests (6)
Panel A – Trust in Strangers						
County-Level Spying Density	-0.098***	-0.137**	-0.126*	-0.087***	-0.089***	-0.088***
	(0.034)	(0.052)	(0.064)	(0.029)	(0.029)	(0.028)
Number of Observations	1,795	1,549	1,549	1,795	1,795	1,795
Kleibergen-Paap *F*-Statistic	12.03	16.06	6.87	10.35	12.89	12.70
Panel B – Reciprocal Behavior						
County-Level Spying Density	-0.183**	-0.174***	-0.187***	-0.189**	-0.170***	-0.172***
	(0.069)	(0.050)	(0.059)	(0.073)	(0.057)	(0.058)
Number of Observations	1,588	1,368	1,368	1,588	1,588	1,588
Kleibergen-Paap *F*-Statistic	15.40	29.89	15.70	13.63	16.18	16.11
Panel C – Attend Elections						
County-Level Spying Density	-0.109**	-0.127**	-0.111**	-0.102**	-0.113**	-0.111**
	(0.052)	(0.048)	(0.046)	(0.048)	(0.054)	(0.052)
Number of Observations	1,583	1,363	1,363	1,583	1,583	1,583
Kleibergen-Paap *F*-Statistic	14.68	28.81	14.60	12.77	15.63	15.50
Panel D – Political Interest						
County-Level Spying Density	-0.261***	-0.234***	-0.256***	-0.265***	-0.253***	-0.256***
	(0.069)	(0.042)	(0.056)	(0.071)	(0.063)	(0.065)
Number of Observations	1,736	1,519	1,519	1,736	1,736	1,736
Kleibergen-Paap *F*-Statistic	19.12	29.62	16.55	17.41	20.88	21.15
Panel E – Political Engagement						
County-Level Spying Density	-0.181***	-0.133***	-0.113**	-0.173***	-0.187***	-0.185***
	(0.047)	(0.046)	(0.050)	(0.046)	(0.048)	(0.048)
Number of Observations	1,736	1,519	1,519	1,736	1,736	1,736
Kleibergen-Paap *F*-Statistic	19.12	29.62	16.55	17.41	20.88	21.15
Panel F – Unemployment Duration						
County-Level Spying Density	0.014***	0.014***	0.015**	0.012**	0.012**	0.013**
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
Number of Observations	1,719	1,506	1,506	1,719	1,719	1,719
Kleibergen-Paap *F*-Statistic	20.81	30.82	17.06	18.95	22.93	23.28
Panel G – Self-Employment Probability						
County-Level Spying Density	-0.016**	-0.021***	-0.022**	-0.015**	-0.014**	-0.014**
	(0.007)	(0.008)	(0.008)	(0.007)	(0.006)	(0.006)
Number of Observations	1,611	1,411	1,411	1,611	1,611	1,611
Kleibergen-Paap *F*-Statistic	18.76	25.30	13.91	17.04	20.10	20.40
Panel H – Log Mean Income						
County-Level Spying Density	-0.056***	-0.046*	-0.046*	-0.056***	-0.053**	-0.054***
	(0.019)	(0.023)	(0.027)	(0.019)	(0.020)	(0.020)
Number of Observations	1,482	1,295	1,295	1,482	1,482	1,482
Kleibergen-Paap *F*-Statistic	16.80	18.09	10.34	15.08	17.72	18.06

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on measures of individual civic capital and economic performance when using different measures of government surveillance and accounting for alternative channels. All estimates are based on our instrumental variables specification as defined in equations (1) and (2). Estimates in column (1) re-print our baseline results shown in column (6) of Tables 2 and 3. Column (2) shows the corresponding results when adding the county-level number of informers who provided logistics (IM2) to our measure of state surveillance. In column (3), the spying density bases on the total number of informers (IM1 and IM2) and the number of official Stasi employees (HM). Column (4) shows the results for our baseline specification when controlling for differences in the socialist indoctrination of counties. In columns (5) and (6), we control for the county-level number of (political) arrests per capita, respectively. Outcome variables in Panels A–E are standardized. All estimates are based on our sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table C.2.5: Inference

	Trust in Strangers (1)	Reciprocal Behavior (2)	Attend Elections (3)	Political Interest (4)	Political Engagem. (5)	Unemploy. Duration (6)	Self-Emp. Probability (7)	Log Mean Income (8)
Panel A – Reduced Form								
Baseline Estimate	-0.094	-0.178	-0.107	-0.270	-0.188	0.014	-0.016	-0.055
Cluster on County-Pair and County Level	(0.038)	(0.044)	(0.044)	(0.043)	(0.034)	(0.006)	(0.005)	(0.026)
	[0.018]	[0.000]	[0.018]	[0.000]	[0.000]	[0.018]	[0.004]	[0.039]
Alternative Cluster Definitions								
Cluster on County-Pair Level	(0.039)	(0.045)	(0.044)	(0.040)	(0.034)	(0.006)	(0.006)	(0.025)
	[0.019]	[0.000]	[0.019]	[0.000]	[0.000]	[0.024]	[0.005]	[0.034]
Cluster on County Level	(0.032)	(0.037)	(0.038)	(0.037)	(0.028)	(0.005)	(0.004)	(0.021)
	[0.004]	[0.000]	[0.006]	[0.000]	[0.000]	[0.002]	[0.000]	[0.009]
Cluster on County-Pair and District Level	(0.031)	(0.046)	(0.046)	(0.046)	(0.035)	(0.005)	(0.006)	(0.026)
	[0.010]	[0.002]	[0.037]	[0.000]	[0.000]	[0.021]	[0.014]	[0.050]
Cluster on Person and County-Pair Level	(0.038)	(0.046)	(0.045)	(0.045)	(0.034)	(0.007)	(0.005)	(0.026)
	[0.018]	[0.000]	[0.022]	[0.000]	[0.000]	[0.052]	[0.001]	[0.036]
Wild Cluster Bootstrap-t (H_0 imposed)								
Cluster on County-Pair and District Level	[0.010]	[0.040]	[0.174]	[0.016]	[0.000]	[0.095]	[0.141]	[0.085]
Randomization Inference								
Cumulative Distribution of Estimates	[0.098]	[0.007]	[0.070]	[0.000]	[0.002]	[0.090]	[0.103]	[0.037]
Panel B – Instrumental Variables								
Baseline Estimate	-0.098	-0.183	-0.109	-0.261	-0.181	0.014	-0.016	-0.056
Cluster on County-Pair and County Level	(0.034)	(0.069)	(0.052)	(0.069)	(0.047)	(0.005)	(0.007)	(0.019)
	[0.006]	[0.011]	[0.040]	[0.000]	[0.000]	[0.006]	[0.019]	[0.004]
Alternative Cluster Definitions								
Cluster on County-Pair Level	(0.035)	(0.069)	(0.052)	(0.067)	(0.045)	(0.005)	(0.007)	(0.019)
	[0.007]	[0.010]	[0.040]	[0.000]	[0.000]	[0.017]	[0.021]	[0.005]
Cluster on County Level	(0.029)	(0.053)	(0.044)	(0.053)	(0.039)	(0.004)	(0.005)	(0.015)
	[0.001]	[0.001]	[0.014]	[0.000]	[0.000]	[0.000]	[0.003]	[0.000]
Cluster on County-Pair and District Level	(0.033)	(0.075)	(0.057)	(0.065)	(0.049)	(0.005)	(0.007)	(0.021)
	[0.010]	[0.029]	[0.079]	[0.002]	[0.003]	[0.016]	[0.037]	[0.017]
Cluster on Person and County-Pair Level	(0.033)	(0.070)	(0.052)	(0.070)	(0.045)	(0.006)	(0.006)	(0.019)
	[0.005]	[0.012]	[0.043]	[0.000]	[0.000]	[0.041]	[0.012]	[0.005]

Notes: This table presents robustness checks on inference for our baseline reduced-form and 2SLS IV estimates presented in columns (5) and (6) of Tables 2 and 3. First, we re-print the corresponding baseline point estimate along with our preferred standard errors, allowing for two-way clustering at the county-pair and the county level, and the corresponding p-values [in square brackets]. Below, we report the corresponding standard errors and p-values for alternative ways of clustering: (i) at the county-pair level, (ii) at the county level, (iii) at the county-pair and district level, and (iv) at the person and county-pair level. We further provide results of two additional test for our reduced-form estimates: (i) the empirical p-values from a wild cluster percentile-t bootstrap test with H_0 imposed (see Cameron et al., 2008), and (ii) p-values based on randomization inference in the spirit of Fouka and Voth (2016). Empirical p-values from both strategies are based on 2,999 replications and our baseline estimate. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. See Data Appendix A for detailed information on all variables.

C.3 Alternative Mechanisms

Table C.3.1: Controlling for Distance to West Germany

	Baseline Effect (1)	Distance To West (2)	Travel Time (3)	West Border (4)	Visit Program (5)
Panel A – Trust in Strangers					
County-Level Spying Density	-0.098***	-0.070*	-0.079**	-0.098***	-0.094***
	(0.034)	(0.037)	(0.037)	(0.034)	(0.033)
Number of Observations	1,795	1,795	1,795	1,795	1,795
Kleibergen-Paap *F*-Statistic	12.03	11.23	12.55	11.47	13.54
Panel B – Reciprocal Behavior					
County-Level Spying Density	-0.183**	-0.156**	-0.183***	-0.183***	-0.178***
	(0.069)	(0.069)	(0.068)	(0.066)	(0.065)
Number of Observations	1,588	1,588	1,588	1,588	1,588
Kleibergen-Paap *F*-Statistic	15.40	14.15	16.81	14.87	17.13
Panel C – Attend Elections					
County-Level Spying Density	-0.109**	-0.106**	-0.126**	-0.109**	-0.104**
	(0.052)	(0.051)	(0.051)	(0.052)	(0.047)
Number of Observations	1,583	1,583	1,583	1,583	1,583
Kleibergen-Paap *F*-Statistic	14.68	13.50	15.70	14.24	16.56
Panel D – Political Interest					
County-Level Spying Density	-0.261***	-0.309***	-0.299***	-0.259***	-0.260***
	(0.069)	(0.078)	(0.072)	(0.066)	(0.068)
Number of Observations	1,736	1,736	1,736	1,736	1,736
Kleibergen-Paap *F*-Statistic	19.12	17.91	20.14	16.38	19.69
Panel E – Political Engagement					
County-Level Spying Density	-0.181***	-0.179***	-0.191***	-0.183***	-0.178***
	(0.047)	(0.047)	(0.053)	(0.052)	(0.046)
Number of Observations	1,736	1,736	1,736	1,736	1,736
Kleibergen-Paap *F*-Statistic	19.12	17.91	20.14	16.38	19.69
Panel F – Unemployment Duration					
County-Level Spying Density	0.014***	0.016***	0.014**	0.014***	0.014***
	(0.005)	(0.005)	(0.006)	(0.004)	(0.005)
Number of Observations	1,719	1,719	1,719	1,719	1,719
Kleibergen-Paap *F*-Statistic	20.81	19.60	22.13	17.90	21.01
Panel G – Self-Employment Probability					
County-Level Spying Density	-0.016**	-0.018**	-0.019***	-0.016***	-0.016**
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)
Number of Observations	1,611	1,611	1,611	1,611	1,611
Kleibergen-Paap *F*-Statistic	18.76	17.72	19.97	16.19	18.90
Panel H – Log Mean Income					
County-Level Spying Density	-0.056***	-0.051***	-0.057***	-0.056***	-0.056***
	(0.019)	(0.019)	(0.019)	(0.018)	(0.018)
Number of Observations	1,482	1,482	1,482	1,482	1,482
Kleibergen-Paap *F*-Statistic	16.80	16.17	18.17	15.03	16.81

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on different measures of individual civic capital and economic performance (see panels) when controlling for the distance to West Germany. All estimates are based on our instrumental variables specification as defined in equations (1) and (2). Column (1) re-prints our baseline effect as in Column (6) of Tables 2 and 3. In column (2), we control for a counties' crow-fly distance (in logs) to the inner-German border. Column (3) presents results when using current log travel time by car as a control variable. In column (4), we add a dummy variable that equals one if a county straddles the inner-German border. Last, column (5) includes a dummy variable indicating whether a county was affected by the extended visitors program implemented in 1972 (Stegmann, 2018). All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table C.3.2: The Effect of Spying on Risk Aversion and Personality Traits

		Big Five Personality Traits				
	Risk Aversion (1)	Extra-version (2)	Neuro-ticism (3)	Conscien-tiousness (4)	Open-ness (5)	Agree-ableness (6)
County-Level Spying Density	0.013	0.033	-0.096	-0.084	-0.034	-0.275***
	(0.086)	(0.071)	(0.073)	(0.052)	(0.055)	(0.074)
Number of Observations	1,874	1,650	1,653	1,642	1,650	1,647
Adjusted *R*-Squared	0.104	0.185	0.164	0.159	0.171	0.142
Kleibergen-Paap *F*-Statistic	14.26	13.25	13.09	13.52	13.53	13.34

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on individual risk aversion and different personality traits. All estimates are based on our instrumental variables specification as defined in equations (1) and (2). Outcome variables are standardized. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different set of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table C.3.3: The Effect of Spying on Political Preferences

	Preferences for Restribution						Political Extremism		
	Total (1)	Family (2)	Unempl. (3)	Sick (4)	Old (5)	Care (6)	Total (7)	Right (8)	Left (9)
County-Level Spying Density	0.000	0.017	0.014	-0.012	0.003	-0.036	0.095*	0.091	0.053
	(0.065)	(0.057)	(0.067)	(0.057)	(0.057)	(0.036)	(0.057)	(0.084)	(0.032)
Number of Observations	2,402	2,391	2,387	2,388	2,394	2,395	1,633	1,564	1,555
Adjusted *R*-Squared	0.191	0.149	0.137	0.140	0.142	0.137	0.139	0.154	0.110
Kleibergen-Paap *F*-Statistic	16.03	16.02	16.03	16.01	16.01	16.04	13.15	12.31	13.36

Notes: This table shows the effect of a one standard deviation increase in surveillance intensity on different measures of individual political preferences. All estimates are based on our instrumental variables specification as defined in equations (1) and (2). Outcome variables are standardized. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

C.4 Underlying Channels

Table C.4.1: The Effect of Spying on Income and Employment

	Log Average Income			Employment Indicators				
	In Work. Age Pop. (1)	In Labor Force (2)	In Em. ployment (3)	In Labor Force (4)	Unemploy. Duration (5)	In Em. ployment (6)	Self-Emp. Probability (7)	Working Hours (8)
County-Level Spying Density	-0.122***	-0.108***	-0.056***	-0.006	0.017***	-0.017*	-0.016**	-0.251
	(0.035)	(0.034)	(0.019)	(0.007)	(0.006)	(0.009)	(0.007)	(0.321)
Number of Observations	1,482	1,482	1,482	1,736	1,699	1,736	1,611	1,411
Adjusted R-Squared	0.348	0.252	0.253	0.579	0.152	0.484	0.093	0.242
Kleibergen-Paap F-Statistic	16.80	16.80	16.80	19.12	20.54	19.12	18.76	16.40

Notes: This table shows the effect of a one standard deviation increase in state surveillance on different measures of income and employment. All outcomes correspond to individual averages over the sampling period. In column (1), we recode missing earnings as zero earnings for individuals who were part of the working-age population but not employed in a given year and take the (log) average income over the survey period as our outcome. In column (2), we recode missings as zero earnings for those individuals who were part of the labor force in a given year. In column (3), we calculate simple average earnings over all non-missing observations. In column (4), we look at the effect of spying on individuals' labor force attachment. Column (5) presents the effect of surveillance on average unemployment duration, column (6) the effect on an dummy variable indicating employment in a given year. In column (7) we use the self-employment probability over the survey period as an outcome. Column (8) shows results using average contractual working hours as left-hand side variable. All estimates are based on our instrumental variables specification as defined in equations (1) and (2). All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table C.4.2: The Effect of Spying – Heterogeneous Effects

	Trust in Strangers (1)	Reciprocal Behavior (2)	Attend Elections (3)	Political Interest (4)	Political Engagem. (5)	Unemploy. Duration (6)	Self-Emp. Probability (7)	Log Mean Income (8)
Panel A – Effects by Age								
District-Level Spying Density								
× Born Before 1945	-0.101**	-0.144***	-0.120*	-0.238***	-0.179***	0.016***	-0.006	-0.053**
	(0.047)	(0.049)	(0.069)	(0.049)	(0.045)	(0.006)	(0.009)	(0.026)
× Born 1945–1959	-0.036	-0.176***	-0.124**	-0.202***	-0.170***	0.015**	-0.011	-0.091***
	(0.047)	(0.056)	(0.056)	(0.053)	(0.039)	(0.007)	(0.010)	(0.028)
× Born 1960–1973	-0.094**	-0.137***	-0.067	-0.216***	-0.203***	0.017***	-0.038***	-0.087***
	(0.039)	(0.051)	(0.055)	(0.052)	(0.037)	(0.005)	(0.010)	(0.023)
× Born 1974 or Later	-0.062	-0.116**	-0.050	-0.128*	-0.184***	0.011	-0.023*	-0.033
	(0.040)	(0.044)	(0.063)	(0.070)	(0.043)	(0.008)	(0.013)	(0.030)
Number of Observations	2,248	1,994	1,982	2,177	2,164	2,160	1,988	1,855
Adjusted R-Squared	0.128	0.195	0.117	0.162	0.120	0.152	0.115	0.403
Panel B – Effects by Gender								
District-Level Spying Density								
× Female	-0.113**	-0.144***	-0.125**	-0.252***	-0.184***	0.013*	-0.018***	-0.057**
	(0.042)	(0.048)	(0.048)	(0.043)	(0.037)	(0.006)	(0.006)	(0.026)
× Male	-0.076**	-0.215***	-0.088*	-0.287***	-0.192***	0.016**	-0.015***	-0.054*
	(0.037)	(0.045)	(0.051)	(0.046)	(0.034)	(0.007)	(0.006)	(0.028)
Number of Observations	1,795	1,588	1,583	1,736	1,736	1,719	1,611	1,482
Adjusted R-Squared	0.149	0.192	0.122	0.150	0.126	0.161	0.095	0.251
Panel C – Effects by Education								
District-Level Spying Density								
× Low/Medium Education	-0.103**	-0.187***	-0.113**	-0.271***	-0.174***	0.013**	-0.019***	-0.058**
	(0.042)	(0.046)	(0.043)	(0.041)	(0.036)	(0.006)	(0.006)	(0.022)
× High Education	-0.051	-0.161***	-0.082	-0.222***	-0.165***	0.011*	-0.008	-0.046*
	(0.040)	(0.049)	(0.049)	(0.041)	(0.041)	(0.006)	(0.007)	(0.026)
Number of Observations	1,795	1,588	1,583	1,736	1,736	1,719	1,611	1,482
Adjusted R-Squared	0.151	0.199	0.136	0.171	0.167	0.177	0.098	0.341

Notes: This table shows the effect of a one standard deviation increase in state surveillance on different measures of individual civic capital and economic performance when allowing for heterogeneous effects by gender and education. The underlying econometric model is described in equations (1) and (2), using the leave-out instrument as our main regressor. Panel A shows heterogeneous effects by age cohorts. In Panel B, we allow for different effects of state surveillance for women and men. Panel C shows differential effects by education. Outcomes in columns (1) to (5) are standardized. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender as well as the different sets of county-level control variables (see Section 4.1 for details).Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.

Table C.4.3: Analyzing the Role of (Selective) Migration

	Moved County (1)	Trust in Strangers (2)	Reciprocal Behavior (3)	Attend Elections (4)	Political Interest (5)	Political Engagem. (6)	Unemploy. Duration (7)	Self-Emp. Probability (8)	Log Mean Income (9)
Panel A – Baseline Effects									
County-Level Spying Density	-0.029	-0.098***	-0.183**	-0.109**	-0.261***	-0.181***	0.014***	-0.016**	-0.056***
	(0.020)	(0.034)	(0.069)	(0.052)	(0.069)	(0.047)	(0.005)	(0.007)	(0.019)
Number of Observations	1,735	1,795	1,588	1,583	1,736	1,736	1,719	1,611	1,482
Adjusted *R*-Squared	0.363	0.149	0.181	0.121	0.149	0.121	0.161	0.093	0.253
Kleibergen-Paap *F*-Statistic	19.09	12.03	15.40	14.68	19.12	19.12	20.81	18.76	16.80
Panel B – Effects By Moving									
District-Level Spying Density									
× Stayed		-0.089**	-0.186***	-0.102**	-0.268***	-0.187***	0.014**	-0.015**	-0.049**
		(0.041)	(0.046)	(0.046)	(0.043)	(0.035)	(0.006)	(0.006)	(0.024)
× Moved		-0.061	-0.153***	-0.136**	-0.246***	-0.204***	0.021**	-0.025***	-0.050*
		(0.058)	(0.054)	(0.054)	(0.056)	(0.053)	(0.009)	(0.009)	(0.026)
Number of Observations		1,795	1,588	1,583	1,736	1,736	1,719	1,611	1,482
Adjusted *R*-Squared		0.148	0.188	0.121	0.151	0.126	0.163	0.096	0.263
Panel C – Accounting for Population Changes									
County-Level Spying Density		-0.097***	-0.180**	-0.112**	-0.256***	-0.180***	-0.056***	0.016***	-0.037***
		(0.034)	(0.069)	(0.052)	(0.058)	(0.050)	(0.019)	(0.005)	(0.009)
Number of Observations		1,795	1,588	1,583	1,736	1,736	1,719	1,611	1,482
Adjusted *R*-Squared		0.150	0.181	0.121	0.149	0.121	0.154	0.091	0.253
Kleibergen-Paap *F*-Statistic		12.03	15.40	14.68	19.12	19.12	20.81	18.76	16.80
Panel D – Spying Current County									
District-Level Spying Density		-0.089**	-0.188***	-0.098**	-0.270***	-0.187***	0.015**	-0.016***	-0.054**
		(0.038)	(0.046)	(0.044)	(0.043)	(0.035)	(0.006)	(0.005)	(0.026)
Moved × Spying Current County		0.019	-0.039	0.033	-0.008	0.018	0.011	0.004	0.021
		(0.045)	(0.046)	(0.051)	(0.035)	(0.042)	(0.008)	(0.008)	(0.017)
Number of Observations		1,795	1,588	1,583	1,736	1,736	1,719	1,611	1,482
Adjusted *R*-Squared		0.148	0.188	0.121	0.149	0.126	0.164	0.095	0.253

Notes: This table shows the effect of a one standard deviation increase in state surveillance on different measures of individual civic capital and economic performance when accounting for individual migration decisions and overall changes in local populations. Moreover, Panel A shows the direct effect of spying on individuals' decision to move (and replicates our baseline results from column (6) of Tables 2 and 3). When analyzing indivdiuals' migration decision, we look at the overall out-migration effect, including migration within East Germany. We find very similar effects when only looking at migration to West Germany. In Panel B, we allow for heterogeneous effects for individuals who stayed in the 1990 county of residence and those who changed residence during the sample period. Panel C presents results when controlling for county-level population changes since 1988. In Panel D, we add the spying density in the respondents' current county of residence as an additional control - for those individuals who moved across county borders within East Germany. The underlying econometric model is described in equations (1) and (2). Estimates in Panels A and C are based on our instrumental variables specification, in Panels B and D the leave-out instrument serves as our main regressor. Outcome variables in columns (2)-(6) are standardized. All estimates are based on the sample of contiguous county pairs that straddle a GDR district border and include county-pair fixed effects, dummy variables for the historical provinces of the Weimar Republic, a dummy variable indicating the presence of a Stasi on-site office, control variables for the individuals' age and gender, as well as the different sets of county-level control variables (see Section 4.1 for details). Cross-sectional weights are adjusted for the duplication of counties that are part of multiple pairs. Standard errors are two-way clustered at the county-pair and the county level. Significance levels are * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. See Data Appendix A for detailed information on all variables.