THE LONG-TERM COSTS OF GOVERNMENT SURVEILLANCE: INSIGHTS FROM STASI SPYING IN EAST GERMANY

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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 variable 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: H11, N34, N44, P20)

Teaching Slides
A set of Teaching Slides to accompany this article are available online as Supplementary Data.

The editor in charge of this paper was Paola Giuliano.

Acknowledgments: We would like to thank Paola Giuliano and four anonymous referees for their valuable comments and suggestions. We are grateful to Jens Gieseke for sharing county-level data on official employees of the Ministry for State Security, and Davide Cantoni for sharing regional GDR data with us. Moreover, we would like to thank Felix Bierbrauer, Pierre Cahuc, Davide Cantoni, Antonio Ciccone, Arnaud Chevalier, Ernesto Dal Bó, Denvil Duncan, Frederico Finan, Corrado Giulietti, Yuriy Gorodnichenko, Emanuel Hansen, Mark Harrison, Johannes Hermle, Paul Hufe, David Jaeger, Pat Kline, Michael Krause, Ulrike Malmendier, Andreas Peichl, Gerard Pfann, Martin Peitz, Nico Pestel, Anna Raute, Derek Stemple, Jochen Streb, Uwe Sunde, Nico Voigtländer, Johannes Voget, Fabian Waldinger, Felix Weinhardt, Ludger Wößmann, Noam Yuchtman as well as conference participants at IIPF, SOLE, VfS, ASSA, EEA, and seminar participants at IZA Bonn, ZEW Mannheim, BeNA Berlin, U Mannheim, U Münster, U Bonn, CREST, Paris School of Economics, UC Berkeley, U Düsseldorf, CReAM, U Maastricht, IAB Nuremberg, U Bochum, and TU Munich for helpful comments and suggestions. Tim Bayer, Felix Pöge, and Georgios Tassoukis provided outstanding research assistance. We would also like to thank the SOEPremote team at DIW Berlin for their continuous support.

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Journal of the European Economic Association 2021 19(2):741–789 DOI: 10.1093/jeea/jvaa009

1. Introduction

Autocracies have been the dominant form of government in human history. Despite substantial shifts toward more democratic political institutions in recent decades, autocratic regimes still rule in more than a quarter of the countries worldwide (see Online Appendix Figure C.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, Gurr, and Jaggers 2017). Although 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 toward 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 1% 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]

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 microlevel effects of repression on groups such as environmental activists (see Davenport 2007 and Earl 2011, for surveys in political science and sociology, respectively).

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

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 and Keefer 1997; Guiso, Sapienza, and Zingales 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, Chap. 2, and Fuchs-Schündeln and Hassan 2016, Chap. 12, 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, Chap. 4; 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

3. We follow Guiso, Sapienza, and Zingales (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.

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, Sapienza, and Zingales 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 and increases the time spent in unemployment by five days per year on average.

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.

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 stronger 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 economic 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 as well as 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, Giuliano, and Nunn 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 relate to recent evidence showing that too little (but also too much) individual trust leads to negative economic outcomes (Butler, Giuliano, and Guiso 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, Nannicini, and Tabellini 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 toward others and the political system.

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). Although 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 that 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, Pannenberg, and Wedow (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 that explicitly addresses the nonrandomness 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.

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

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 United States, the United Kingdom, 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% of the population) managed to leave the country by authorized migration or illegal border crossing (see Figure C.2 in the Online Appendix). 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 0.1% of the population managed to emigrate on an annual basis (6%–7% 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 Section 4.2.

The decision on how to delineate districts was the result 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 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, Kotsch, and Engler 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 that 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

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

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).

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% 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 strategy on (cf. Section 4.2).

Although many historical accounts acknowledge the considerable differences in surveillance intensities across districts, only a few discuss potential reasons for the 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"

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; 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 onward (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 11% of the total population. Likewise, 17% of the two million bugged telephone conversations were tapped in the district of Magdeburg, which only made up 8% of the population.

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 Section 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 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

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).

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. Moreover, historical evidence shows that the spying activities led to other forms of nonpersecutive, 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 that we collected from the archives of the Agency. Overall, this allows us to observe the spying density for around 92% of the counties at least once in the 1980s.[13]

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,

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.

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 information on the total number of collaborators is not given for each year in every county, we use the average spying density between 1980 and 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 Online Appendix B. 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, that is, using informers within social networks. We discuss and test the quality of our measure of surveillance in Sections 4.3 and 5.2.

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%, the mean density being 0.38% (see Online Appendix Table B.2 for more detailed distributional information). The median is similar to the mean (0.36%), 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 toward 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, Sapienza, and Zingales 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, Sapienza, and Zingales 2010). We focus on civic capital for three reasons. First, the

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

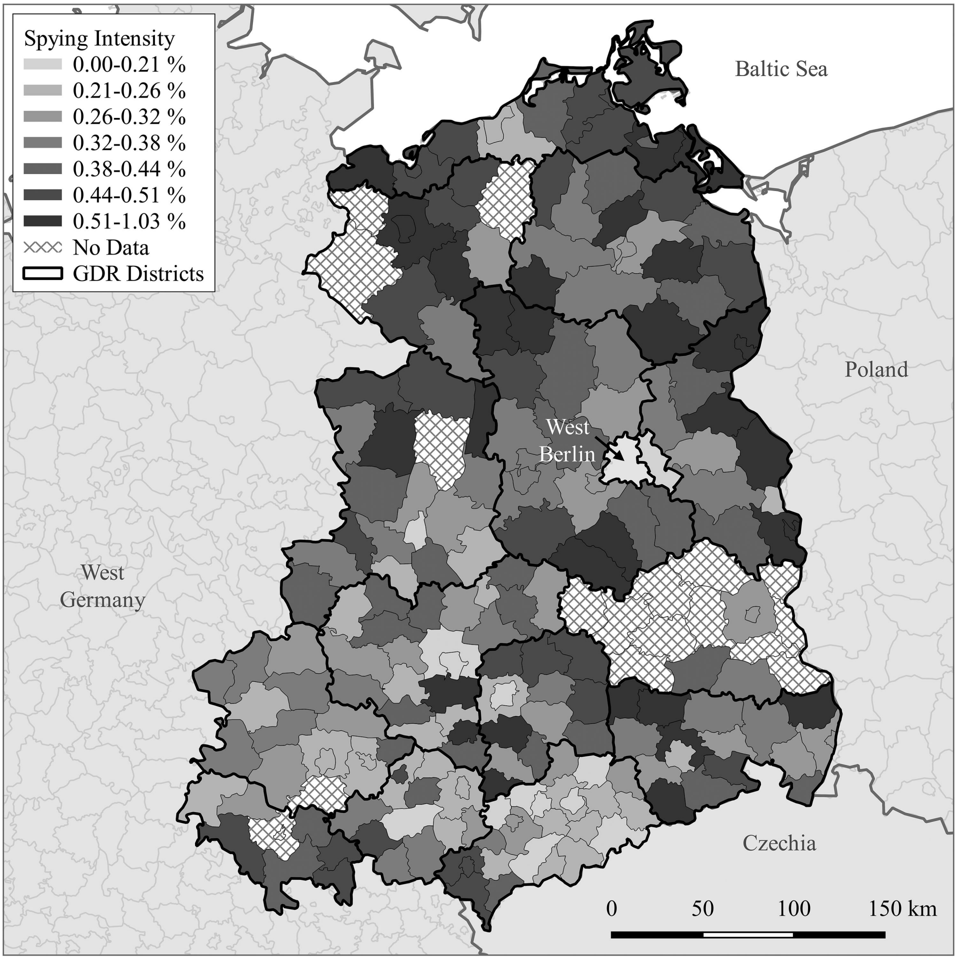

FIGURE 1. Spying intensity across counties. This figure shows the county-level surveillance density measured by the average yearly share of operative unofficial informers relative to the population between 1980 and 1988. Source: See Online Appendix B. *Maps:* MPIDR and CGG (2011) and @EuroGeographics.

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 sociopolitical context, for instance political engagement (Guiso, Sapienza, and Zingales 2010). We select our main outcomes along these lines (cf. Section 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, Sapienza, and Zingales 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, for example, 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, Dickhaut, and McCabe 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, Sapienza, and Zingales 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 politics, 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, Sapienza, and Zingales (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 rates 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, Giuliano, and Guiso (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 (Section 4.1). In Section 4.2, we develop our research design and set up the empirical model. In Section 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, Frick, and Schupp 2007; Socio-Economic Panel (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 [1990–2010] as an alternative more frequently measured proxy for voting behavior (Putnam 2000; Guiso, Sapienza, and Zingales 2004; Rodenburger 2018); political engagement [1990–2010, with gaps] (Guiso, Sapienza, and Zingales 2010). We further focus on three measures of economic performance. First, we use log mean income between 1991 and 2010. Second, we calculate the total unemployment duration for each individual over this period, defined as the number of months in unemployment relative to the total number of months in the sample period. Third, we derive individuals' time spent in self-employment; analogously defined as the number of years with an episode of self-employment relative to the total number of years in self-employment or regular employment in the sample period.[16] Besides these main measures of civic capital and economic performance, we use a range of other outcome variables to test for alternative channels (cf. Section 5.3) and analyze potential underlying mechanisms

16. There is no information on the months in self-employment per year in the SOEP.

(cf. Section 5.4). See Online Appendix Tables B.1 and B.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 in Section 4.2). 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 Section 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 shares of children and pensioners as of September 30, 1989, and (iv) whether the county is rural or urban (*Stadt-/Landkreis*).[17] 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, that is, 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/predetermined 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 Woessmann 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 B.2 in the Online Appendix.

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

17. 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.

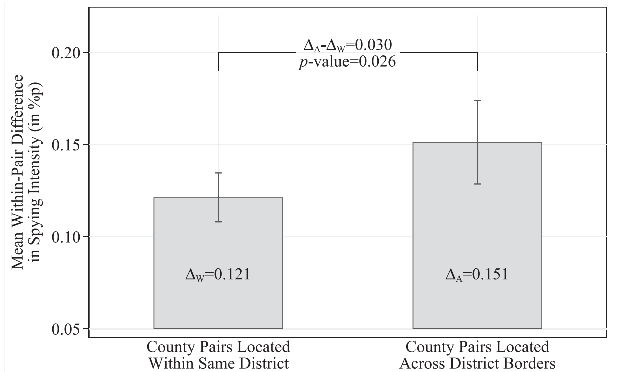

FIGURE 2. Differences in spying intensity within/across districts. This figure plots the average difference in the share of operative unofficial informers at the county level within (i) 336 county pairs from the same district and (ii) 122 county pairs divided by district borders. Differences in the average spying intensity between both groups are statistically significantly different from zero (see 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 Online Appendix B for detailed information on all variables.

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 neighboring counties at different sides of a district border and use differences in the spying density within these county pairs to identify the effect of government surveillance on our respective outcomes (see, e.g., Holmes 1998; Dube, Lester, and Reich 2010, for studies applying similar research designs).[18]

A precondition for the validity of this design is that there is meaningful variation in the policy variable within county pairs at district borders. To test this, we identify all possible neighboring county pairs within the GDR (discarding pairs with very short common borderlines of less than 2 km) and calculate the mean within-pair difference in the spying density.[19] Figure 2 visualizes the policy-induced variation in the border design by comparing the average within-pair differences in the spying density of county pairs that straddle a district border to pairs within districts. The figure shows that differences are significantly larger in county pairs that straddle a district border.

18. Our border design is related but different from a spatial regression discontinuity design (RDD) as, for example, 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.

19. We discard the city of East Berlin from our analysis because it was a district on its own in the 1980s and we cannot separate East and West Berlin post reunification. We show that results are robust to alternative county pair definitions (see Section 5.2 and Online Appendix Table D.7).

In the econometric model, we limit the analysis to contiguous county pairs on different sides of a GDR district border (again discarding pairs with very short common borderlines). In case of multiple neighbors on the other side of a district border, we only consider the neighboring county that shares the longest border with the respective county. On this sample, we regress outcome Y (see Section 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, 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:

$$Y_i = \alpha + \beta \cdot SPYDENS_c + \boldsymbol{X_i}\delta + \boldsymbol{H_c}\varphi + \nu_b + \mu_p + \varepsilon_i. \tag{1}$$

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 previously 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 Section 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, for example, between Prussia and Saxony, in a nonparametric way (see Appendix Figure A.1 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 Section 4.1, we observe some of the civic capital outcomes in two waves only (trust, reciprocity, and attend elections).[20] 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 Online Appendix Table D.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-year 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 Section 5.2.

20. 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.

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 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 in district d is then given by

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

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 Section 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 A.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 that 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 Appendix Table A.1, is rejected. In column (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 column (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. Although 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.[21] 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.

Correlated District Discontinuities. The main remaining threat to identification arises from district-level 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. Although this does not necessarily lead to discontinuities within county pairs at district borders, we test for such differences using county-level industrial output and the number of workers as measures of economic activity. Appendix Table A.1 shows that these variables are smooth in our border design. The other relevant factor that 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 A.2). Nevertheless, we control for employment

21. 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, Sapienza, and Zingales 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.

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)
No. obs.	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)
No. obs.	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. See Online Appendix B for detailed information on all variables.

shares in the agriculture, energy/mining, and textile industry—those three sectors for which clusters were to be formed (see Section 2 and Werner, Kotsch, and Engler 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). Appendix Table A.1 shows that these potential confounders are smooth within county pairs at district borders.

Historical accounts further suggest that soft factors, such as the personality of the Stasi's district leadership, led to differences in the intensity of surveillance across districts (see the discussion in Section 2). We exploit the resulting variation, assuming that differences in the district leaderships' effort or loyalty affected individuals' civic capital and economic performance only due to differences in surveillance intensities. Although this assumption is ultimately untestable, we argue that it is likely to hold as the Stasi operated secretly and was not involved in economic production or the political process (Gieseke 2014).

Moreover, 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, Chap. 2). 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 Section 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. Although 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 C.3 in the Online Appendix for the evolution of these requests over time) at the district level from the Stasi Records Agency; unfortunately, county-level information are not available. As shown in Panel A of Online Appendix Figure C.4, 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 requests. We test this argument in Panel B of Online Appendix Figure C.4, 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. Although we cannot rule out these mechanisms, both would work against finding large effects and bias our estimates toward zero.

Sign of Bias. Although it would be interesting to formulate a clear ex ante hypothesis about the sign of the endogeneity bias, the nature of endogeneity concerns discussed previously 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 nonconformity 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 simple OLS 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 Section 4.3 such that the direction of bias remains an empirical question. Our step-wise implementation of the research design (cf. Section 5.1) suggests ex post that endogeneity leads to a downward bias of estimates.

5. Empirical Results

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

5.1. Main Results

In this section, 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% 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 or five days

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)	
No. obs.	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)	
No. obs.	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)	
No. obs.	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)	
No. obs.	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)	
No. obs.	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. See Online Appendix B for detailed information on all variables. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

per year on average. We show in Figure 3 that the probability of unemployment is significantly affected, too. Panel B further shows that more intense government surveillance decreases individuals' time in self-employment (conditional on working) 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) 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).[22] We show in Section 5.4 that educational attainment is a key driver of the economic effects.

Overall, our results indicate that Stasi surveillance affected individuals' economic performance at both the extensive and the intensive margin, that is, conditional on working. In Appendix Table A.2, 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).

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.[23] Column (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

22. 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.

23. 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 Section 4.3, this demonstrates that simple correlations may be quite misleading in our setting.

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)	
No. obs.	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						
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)	
No. obs.	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)	
No. obs.	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. See Online Appendix B for detailed information on all variables. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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 *F*-statistics exceeding 10 for all outcomes and second-stage estimates being statistically significant.[24] 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 Section 4.3, "Sign of Bias").

In the appendix, we additionally visualize the mechanics behind our identification strategy. For example, in Panel A of Online Appendix Figure C.5, 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 column (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 Section 4.3). Panel C depicts the relationship in our 2SLS specification, which is more negative and tighter. Figures C.6– C.12 in the Online Appendix 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 toward 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. Appendix Figure A.1). 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 that share the same cultural heritage (see Online Appendix Table D.3).

Dynamics and Persistence. We observe some of our civic capital and economic variables on an (almost) annual basis (cf. Section 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

24. We find similar effects when using the overall district average instead of the leave-out-average as an instrument (cf. Online Appendix Table D.2).

yearly samples. In the Online Appendix, we show that yearly estimates look very similar but are a little more bumpy and less precise, see Figure C.13 in the Online Appendix.

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, whereas 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 onward, effects for all four outcomes are relatively stable until the early years of the new century. In the course of the 2000s, the 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.

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 Online Appendix B for details). Although 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.

Appendix Table A.3 presents the corresponding results. In Panels 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

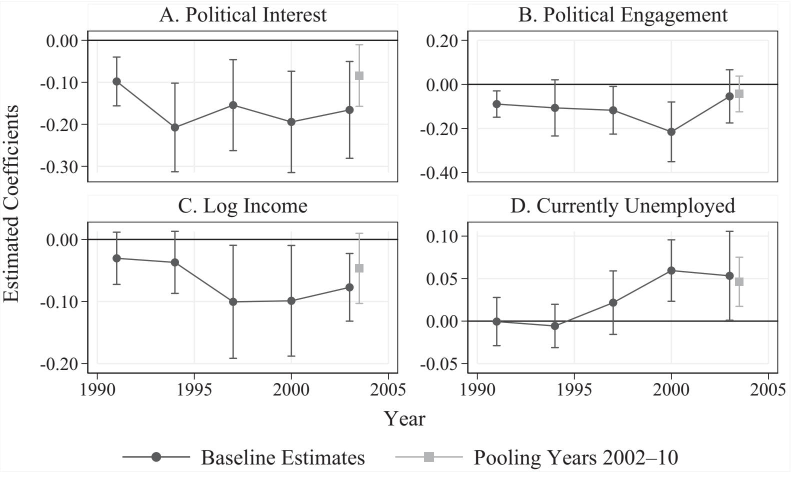

FIGURE 3. The effect of spying—dynamics over time. This figure shows the effect of a one standard deviation increase in surveillance intensity on different measures of individual civic capital and economic performance for different periods of our sample. Estimates are based on our IV specification and 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 2002–2010 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). Source: See Online Appendix B for detailed information on all variables.

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. These respondents only spent parts of their childhood under the regime, such that effects of surveillance might be smaller due to lower exposure. Panel A of Online Appendix Table D.4 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.[25]

25. 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.

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. Although the number of operative informers is arguably the most natural measure of surveillance intensity (cf. Section 2), we show in Online Appendix Table D.5 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, although 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 acquired official microdata on detained individuals in East Germany for the years from 1984 to 1988 (see Online Appendix Table B.1 for more details). Although it is not straightforward to distill the number of political arrests from the data in light of nonexclusive, 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 Online Appendix Table D.5. 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 C.14 in the Online Appendix). 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 previously, 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, Gelbach, and Miller (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, nonpermuted estimate to calculate the empirical *p*-values. Online Appendix Table D.6 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 Section 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 Online Appendix Figure C.15 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)–(3) of Online Appendix Table D.7 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, Haider, and Wooldridge (2015).[26] Columns (4) and (5) of Online Appendix Table D.7 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

26. Government surveillance itself does not significantly affect panel attrition.

and Fuchs-Schündeln 2007) and 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 Online Appendix Table D.5), which is in line with the rather unsystematic correlation between the spying density and our proxy of local differences in socialist indoctrination (see Figure C.16 in the Online Appendix).

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, see Stegmann 2018) and post-reunification economic activity (e.g., due to better access to West German markets, see Redding and Sturm 2008). Table D.8 in the Online Appendix 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 Appendix Table A.4 shows that risk aversion is unaffected by government surveillance. Similarly, the Stasi 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 "Agreeableness". However, as indicated in columns (2)–(6) of Appendix Table A.4, only one of the Big Five personality traits—"Agreeableness"—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)–(6) of Appendix Table A.5). 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)–(9) of Appendix Table A.5. 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 reductions in civic capital can explain parts 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 Table D.9 in the Online Appendix, 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 Online Appendix Table D.9. 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.

Deconstructing 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.[27] A one

27. We find no differential effects of spying on civic capital or economic performance by individuals' level of education (see Panel C of Table D.4 in the Online Appendix).

TABLE 4. The effect of spying on education and 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)
No. obs.	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)
No. obs.	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. See Online Appendix B for detailed information on all variables. $^{**}p < 0.05$; $^{***}p < 0.01$.

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 the previous section), 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 16–29 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), which suggests that the indirect channel was important. Of course, this assertion assumes that cohorts only differed in their opportunities to invest in education after reunification, which is a strong assumption in light of the substantial age differences across our cohorts. Moreover, the finding does not suggest that the direct channel was irrelevant but rather implies that the indirect one was at play, too.

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[28], we estimate the effects of government surveillance on these five outcomes while controlling for our measures of civic capital. Columns (1) and (2) of Table 5 reveal that effects on education and occupational prestige become smaller when conditioning on trust, which is another suggestive piece of evidence that the indirect channel—Stasi surveillance lowering civic capital and reduced civic capital impeding educational investment—is relevant. As previously mentioned, this does not rule out any direct effect of surveillance on individuals' educational attainment for given levels of civic capital. The fact that coefficients in Panel C are still different from zero (although not statistically significant) hints at the fact that the direct channel is important as well. A similar argument holds true for our effects on income and unemployment duration, where coefficients also decrease and become statistically insignificant once conditioning on civic capital but are not entirely explained away. Overall, we thus take these findings as suggestive evidence that the surveillance-induced reductions in civic capital are one driver of the sizable economic effects, which is in line with our theoretical priors and the dynamic pattern displayed in Figure 3.

28. We control for an outcome, which gives rise to the well-known bad control problem. We would need additional instruments to cleanly attribute the observed effects on economic performance to (a specific measure of) civic capital.

TABLE 5. The effect of spying on economic performance conditional on civic capital.

	Years of education (1)	Occup. prestige (2)	Unemploy. duration (3)	Self-employment (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)
No. obs.	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)
No. obs.	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)
No. obs.	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. See Online Appendix B for detailed information on all variables. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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 nonrandom 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 roughly half of the negative economic effects. We also find evidence for the theoretical prediction that individuals with lower trust/civic capital invest less in (human) capital and experience negative economic effects.

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 (€84 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%).[29] Taking our estimates at face value, the Stasi can account for up to around 50% of the East-West gap in economic performance.[30]

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 the 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 and 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

29. 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.

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

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.[31] It is likely that this shift toward 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 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, whereas 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.

31. 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).

Appendix A: Additional Material

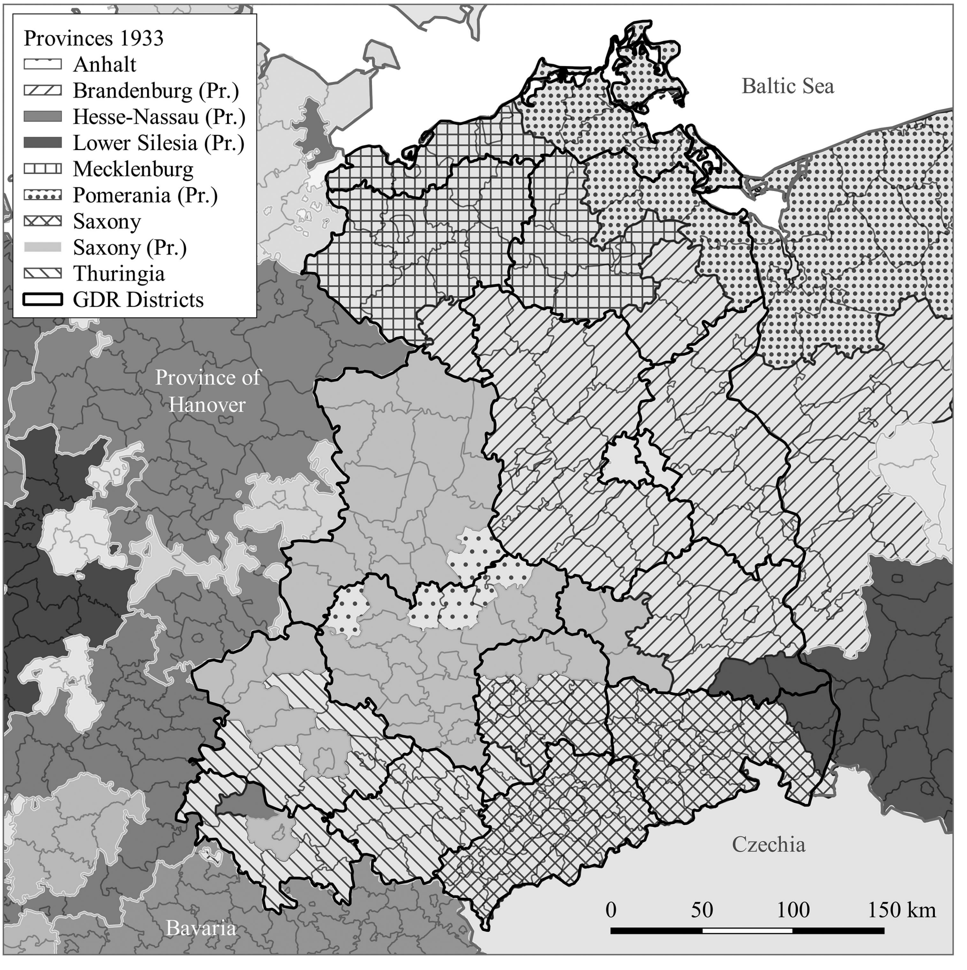

FIGURE A.1. GDR districts and provinces of the Weimar Republic. This figure shows GDR district borders and historical borders of the states of the Weimar Republic and the Prussian provinces as of 1933. *Maps:* MPIDR and CGG (2011) and @EuroGeographics.

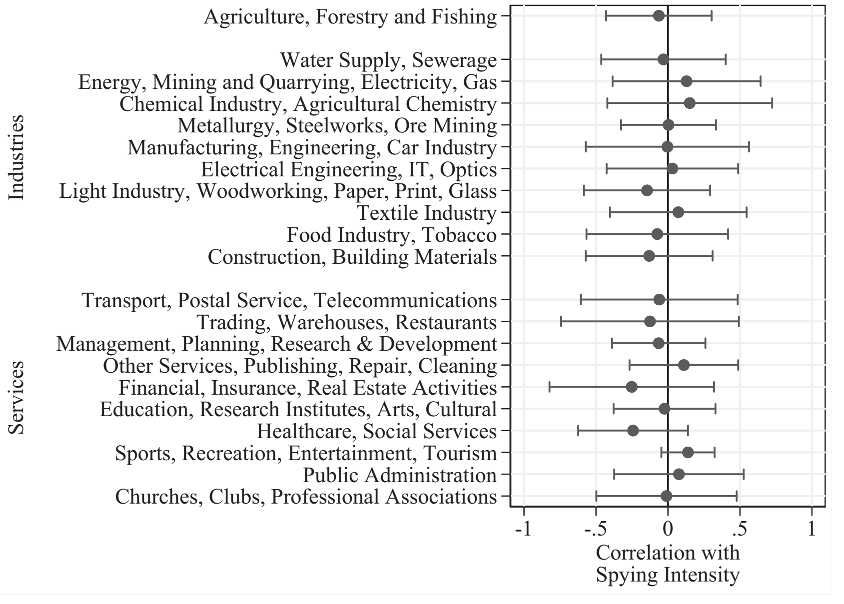

FIGURE A.2. Smoothness of industrial composition. This figure tests the smoothness of county-level employment shares in various industries at district borders. Each coefficient is estimated separately by regressing the respective employment share on the spying density, the set of county-pair fixed effects 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 Online Appendix B for detailed information on all variables.

TABLE A.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. 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 Online Appendix B for detailed information on all variables. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE A.2. 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- employment (7)	Working hours (8)
County-level spying density	−0.122***	−0.108***	−0.056***	−0.006	0.014***	−0.017*	−0.016**	−0.251
	(0.035)	(0.034)	(0.019)	(0.007)	(0.005)	(0.009)	(0.007)	(0.321)
No. obs.	1,482	1,482	1,482	1,736	1,719	1,736	1,611	1,411
Adjusted R-squared	0.348	0.252	0.253	0.579	0.161	0.484	0.093	0.242
Kleibergen–Paap F-statistic	16.80	16.80	16.80	19.12	20.81	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 nonmissing 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. See Online Appendix B for detailed information on all variables. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE A.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)
No. obs.	1,583	1,482	1,719
Adjusted *R*-squared	0.121	0.251	0.161
Panel B: Average effects on administrative sata			
District-level spying sensity	– 0.166***	–0.072**	0.068*
	(0.051)	(0.028)	(0.039)
No. obs.	3,515	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**		
	(0.076)		
× Year 1992		–0.042**	
		(0.020)	
× Year 1998			0.025
			(0.043)
× Year 2009	–0.109**		
	(0.055)		
× Year 2010		– 0.121***	0.093***
		(0.037)	(0.034)
No. obs.	3,515	5,961	5,887
Adjusted *R*-squared	0.020	0.004	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. See Online Appendix B for detailed information on all variables. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

TABLE A.4. 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)
No. obs.	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. See Online Appendix B for detailed information on all variables. $^{***}p < 0.01$.

TABLE A.5. The effect of spying on political preferences

	Preferences for redistribution						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)
No. obs.	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. See Online Appendix B for detailed information on all variables. $^{*}p < 0.1$.

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Supplementary Data

Supplementary data are available at *JEEA* online.