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Police reorganization and crime: Evidence from police station closures

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Police reorganization and crime: Evidence from police station closures

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Abstract

Does the administrative organization of police affect crime? In answering this question, we focus on the reorganization of local police agencies. Specifically, we study the effects police force reallocation via station closures has on local crime. We do this by exploiting a quasi-experiment where a reform substantially reduced the number of police stations. Combining a matching strategy with an event-study design, we find no effects on total theft. Police station closures, however, open up tempting opportunities for criminals in car theft and burglary in residential properties. We can rule out that our effects arise from incapacitation, crime displacement, or changes in employment of local police forces. Our results suggest that criminals are less deterred after police station closures and use the opportunity to steal more costly goods.

JEL Classification: K42, R53

Keywords: Crime, Policing, Crime Deterrence, Police Station Closures

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1 Introduction

A substantial amount of literature in economics and criminology deals with the question whether more police leads to less crime. While there are discernible differences about the magnitude of the exact elasticities of additional police forces across various types of crime (e.g. Levitt, 1997; Evans and Owens, 2007; Lin, 2009; Chalfin and McCrary, 2017*b*), there is a consensus that more police is indeed effective in terms of crime reduction (Chalfin and McCrary, 2017*b*).

Unlike prior studies, the present paper studies the crime effects of a yet overlooked aspect of police presence: does the administrative organization of police forces affect for local crime?[1] Specifically, we look at the effects of the reduction of the number of local police agencies, i.e. the closure of local police stations, on crime. For this purpose, we exploit quasi–experimental variation from a large-scale police reorganization reform in Germany that resulted in the closure of about 40% of existing local police stations.

Police forces are typically structured in various types of organizations which are “an ubiquitous aspect of the landscape of criminal justice” (King, 2014). Most police organizations, however, are small local law enforcement agencies that deal with crime clearance at the municipal or county level. For convenience, we refer to them as local police stations. Policy makers frequently reform police forces for budgetary reasons and, in order to improve efficiency of policing. These attempts often result in a centralization of police forces as well as a reduction in the number of local police agencies (Fyfe et al., 2013).[2]

Shutting down entire police agencies in the course of police centralization can affect crime in various ways. For instance, it may have negative effects on crime through increased distances to nearby forces. Thus, citizens’ access to police services is reduced and travel distance of police forces increase in the case of crime notification. Moreover, station closures likely lead to a lower presence of police forces, and criminals may find it less costly to engage in crime as their (perceived) risk of detection decreases. However, local forces may be more efficient if closures of small and less productive stations lead to larger and more professional police services. Hence, the effect of police station closures on crime and its mechanisms is ultimately an empirical question.

We provide new causal evidence of the effects that eliminating entire local police agencies has on crime. We also study the impact of local police station closures with a focus on theft crimes. Thieves often have monetary incentives. The opportunity-of-crime literature argues that the situational context is important for theft and burglary (Felson and Clarke, 1998; Clarke, 2012). Closing down police organizations may provide such an opportunity for property crimes due to a

[1]While we are the first to study related effects on crime, prior studies analyzed economies of scale of police agencies (Ostrom et al., 1973; Simper and Weyman-Jones, 2008; Fegley and Growette Bostaph, 2018; Mendel et al., 2017; Maher, 2015), and focussed on the determinants of police organizations (King, 2009; Brunet, 2015; King, 2014). According to Gruenewald et al. (2018), police consolidations enjoy wide support among police officers.

[2]For instance, consolidation waves of police stations led to substantial shut-downs of local stations in the UK (Metropolitan Police, 2016), Switzerland (Aargauer Zeitung, 2017), Belgium (Vereniging van Vlaamse Steden en Gemeenten, 2017), New Zealand (New Zealand Parliament, 2017), Finland (Haraholma and Houtsonen, 2013), Austria (Bundesministerium für Inneres, 2014), Denmark, Scotland, the Netherlands (Mendel et al., 2017) as well as in the US. The US department of Justice reports, for example, the shut-down of 175 (1.4% of) local police agencies between 2008 and 2013 (US Department of Justice, 2011, 2015).

salient reduction of police presence. Arguably, the existence of a *salient* local police organization represents a parameter for the expected value of getting caught and changes the expected benefits of crime (Becker, 1968). Thus, reorganizing local police agencies may generate a change in crime opportunity. We also focus on theft and burglary since in our set-up local agencies are not responsible for most violent crimes. We study violent crime effects as a placebo exercise.

The reorganization reform took place in the German state of Baden–Wuerttemberg. Prior to the reform, the state had a very decentralized system of law enforcement with 574 local police stations (*Polizeiposten*).[3] In 2004, the state government decided to close more than 200 police stations for efficiency reasons while retaining the affected local police forces on payroll. Interestingly, the reform did not involve layoffs but reallocated local police forces only to nearby stations. This allows us to study the effect of administrative reorganization of police forces on crime.

Causal inference about police station closures regarding crime outcomes is challenging since it is unlikely that these closures are implemented at random by the state government. Instead, policy makers may attempt to close stations in low crime areas and target efforts at crime hotspots (Braga et al., 2014) or shut-down policing in less problematic areas.[4] Selection would, in turn, bias our estimates of police station closures in a simple before–after comparison of reported crimes. In order to estimate causal effects of station closures on crime, we use a combined approach of matching and event-study methods to account for these endogeneity concerns. Matching allows us to retrieve similar counterfactuals for municipalities that undergo station closures by using various municipal-level characteristics on demographics, local labour markets as well as local government accounts. Event studies also allow us to trace treatment effect dynamics after closure events and, importantly, falsify the identifying assumption of common trends of criminal activity in municipalities undergoing station closures and those that do not before the reform.

Our results suggest that police station closures do not affect overall theft. However, we find substantial changes in the way local criminals conduct theft crimes. We observe significant and persistent increases in the number of reported crimes for car theft and burglary in residential buildings. Documented effects on crime rates come typically with high monetary rewards. For instance, car theft and residential burglary were worth 9 or 3 times as much as an average theft case in our sample period. Criminals, however, do not engage in more crime with respect to other theft categories, which are typically of lower monetary value, including other vehicle theft (moped or bicycles) or burglary in commercial buildings. We argue that station closures provide a salient signal to potential offenders that police presence has reduced in affected areas. This is consistent with decreased perceived risk of sanctions and an increase of expected returns, especially for goods with high monetary rewards. Closing local police agencies thus leads to lower crime deterrence and opens up tempting opportunities to steal goods with high monetary rewards.

Our findings are robust to various sensitivity checks. Importantly, our event-study estimates indicate that effects are not driven by pre-existing trends that differ across groups. Results are also

[3] This amounts to 5,4 stations per 100,000 residents for a population of 10,6 million (similar to the US state Ohio).

[4] For instance, crime is less likely in areas with more favorable demographics or labour market characteristics, e.g. comparably low unemployment (Entorf and Spengler, 2000), wages (Machin and Meghir, 2004) or shares of foreigners (Bell et al., 2013).

robust to various alternative control groups such as conditioning control group localities to have at least one police station, or using municipalities of the neighboring state of Hesse. The set-up also allows us to use violent crimes as a placebo test for the reform intervention since violent crimes are not part of the primary domain of local police agencies but are part of the jurisdiction of the state criminal police. Reassuringly, we do not find effects of station closures on violent crimes. We also find supportive evidence using official suspect statistics where we show that numbers of suspects are increasing along with reported crime cases for car theft and residential burglary but not for other types of theft. Moreover, labour market conditions do not significantly deteriorate after station closures, which could have compromised our findings. Our findings are also robust to potential changes in police employment. First, trends in police employment were parallel for regions which were affected by closures and those that were not. Hence, police station closures did not lead to layoffs but to reallocations of police forces within the same precinct. Second, also explicitly controlling for changes in employment does not invalidate our results.

We thoroughly test mechanisms through which police station closures affect theft crimes. Broadly speaking, there are two ways by which (a lack of) policing can affect crime: deterrence and incapacitation. While incapacitation prevents crime by taking criminals off the streets through arrest or detention, crime may be deterred through policies that change economic incentives of potential offenders to engage in crime (Chalfin and McCrary, 2017*b*; Nagin, 2013). Police forces may thus deter crime by changing the expected costs and benefits of criminal actions. Closing entire police stations can arguably affect crime through both channels. In analyzing detection rates, we do not find changes in the effectiveness of local police forces. Hence, we can rule out that the ability of police to apprehend, i.e. incapacitation, as an explanation for higher theft.

Instead, our findings are rather driven by lower crime deterrence: police station closures send a salient signal of reduced state presence to potential offenders. The first piece of evidence that economic incentives of criminal subjects play a large role in our set-up is the fact that our results of more high value theft are partially driven by more unequal local income distributions. This suggests that more valuable property crimes are deterred less, and the respective criminals use the opportunity to steal more costly goods. The inspection of heterogenous station closures also adds further supporting evidence to the deterrence channel. First, closing smaller stations seem to drive our baseline effects, which may imply that the closure of the police building itself represents a salient signal, irrespective of the size of reallocated police forces. Second, we also find that closing more successful stations, i.e. stations with relatively high detection rates, increases residential burglary and contributes to our findings. Closing these stations likely leads to a reduction in the perceived risks of detection for potential offenders.

We can also rule out that we observe higher theft in the course of station closures due to crime displacement from surrounding municipalities. Using various neighborhood definitions of treated municipalities with matched control municipalities in the neighboring state of Hesse, we are not able to find lower reported crimes for car theft and residential burglary. Hence, we can argue that higher crime rates in closure municipalities are not caused by crime displacement. On the contrary, we find that car theft diffuses to adjacent municipalities. Furthermore, we find no crime spillovers for neither residential burglary nor for other theft categories. We also rule out that increased distances drive our results on car theft and residential burglary.

Finally, we use simple back-of-the-envelope calculations and information on the average size of insurance claims across theft categories to quantify the direct costs of police station closures. Direct costs are moderate in size with about 0.5% of pre-reform police spending but appear to be permanent since thieves shift their attention to more costly goods. Given a lack of layoffs and better detection rates, reform benefits are not obvious and unlikely to cover the costs of reform.

This paper makes several contributions to the literature. First, we contribute to papers on police deployment and crime by studying the reallocation of police forces through the closure of local police agencies across different towns. Prior papers used, for instance, exogenous variation in police force allocation from large scale terror threats (Klick and Tabarrok, 2005; Draca et al., 2011; Di Tella and Schargrodsky, 2004), find that place-based increases in policing reduce crime through more crime deterrence. We find similar effects for theft and residential burglary. According to Blanes i Vidal and Mastrobuoni (2018), however, increasing patrols in normal times does not curb crime. Weisburd (2016) also finds only modest effects when using irregular patrols to identify deterrence from police presence, whereas Mastrobuoni (2019) exploits disrupted police shifts and finds in turn lower clearance rates. Moreover, Bindler and Hjalmarsson, 2018 study the effect of the creation of the first professional police forces of the London Metropolitan Police and find that policing reduces both property and violent crimes. Related to their study, our set-up reveals insights into the extensive margin of policing but we focus on the abolishment of local agencies as an important phenomenon in recent years. Rather than no policing, station closures lead instead to a switch from direct policing to contracting in affected areas.[5]

Second, we speak more broadly to the crime deterrence literature. Various ways have been identified through which policing can deter crime, ranging from simply adding more manpower and resources (e.g., Chalfin and McCrary, 2017*a*; Machin and Marie, 2011; Mello, 2019) to the use of various police tactics, including rapid response to calls for service (Weisburd, 2016), problem-oriented targeting (Kennedy et al., 2001) or proactive and disorder policing (Kubrin et al., 2010). We relate in particular to the literature on hot-spot policing which shows that targeting defined geographical areas can effectively reduce crime (e.g. Weisburd and Green, 1995; Rosenfeld et al., 2014; Braga and Bond, 2008; Braga et al., 2014; Blattman et al., 2017). We add to the literature by studying the crime effects of administrative reorganizations of police agencies as a means of place-based crime control. Reallocating police forces through police agency closures amounts to a permanent redeployment of police forces.

Third, we also inform a small theoretical literature on the optimal allocation of police forces. For instance, Fu and Wolpin (2017) estimate a structural model of crime for metropolitan areas in the US and employ their estimations to evaluate several targeting schemes that allocate federally-sponsored additional police across cities. The authors find that decentralized decisions on resource

[5]While most existing studies study more intense policing, our study instead uses a permanent reduction in local police presence. Also Shi (2009) and Poutvaara and Priks (2009) exploit negative shocks to police presence to study related crime effects. Moreover, Heaton et al. (2016) as well as MacDonald et al. (2016) use variation in private policing around university campuses in Chicago and Pennsylvania, respectively, instead of public policing to study deterrence.

allocation are more efficient than centralized actions. Indeed, we find that police station closures, as a measure of centralization of police agencies across towns, increase theft. We find that centralization (closures) of stations with high quality partially drive our results. Galiani et al. (2018) provide an analysis of optimal police force allocation within cities.

Fourth, we also add to the opportunity of crime literature (e.g. Clarke, 2012 and Felson and Clarke, 1998). We argue that reduced police presence through police station closures opens up tempting crime opportunities for goods with high monetary rewards, namely car theft and residential burglary. We thus relate to recent literature which finds that higher prices of property goods make them more attractive for thieves (Draca et al., 2018; Kirchmaier et al., 2018; Harbaugh et al., 2013). Our findings suggest that these high value goods are particularly suspeptible to a reduction in place-based crime control via police station closures. Again, this speaks to the role of economic incentives in deterring crime (Draca and Machin, 2015).

The paper is structured as follows. Section 2 describes local law enforcement in Baden–Wuerttemberg and the reform of local police agencies. Section 3 outlines our data and provides summary statistics. We highlight our identification strategy in Section 4. Sections 5 and 6 show our empirical results and discuss underlying mechanisms as well as costs and benefits of the reform, respectively. Section 7 concludes.

2 Institutional background

2.1 Organizational structure of local law enforcement

With the exception of some federal police duties, such as border control, asylum legislation or aviation security, law enforcement in Germany is predominantly managed at the state-level. However, there are large disparities across federal states regarding the effectiveness and organization of local law enforcement. Baden-Wuerttemberg has some of the lowest numbers of crime cases per capita among the German states (Bundesministerium des Inneren, 2017) and the most decentralized and fragmented system of local law enforcement (Landtag Baden-Wuerttemberg, 2004).

According to Supplementary Appendix Figure A.1, state policing covers both law enforcement and other areas, such as state criminal police. Criminal police, however, addresses criminal cases with special demands for crime clearance and a high degree of severity or hardship which demands specialized expertise and investigation efforts, such as murder, sexual assault, and organized crime. The present study focusses on organizational changes in local law enforcement (the so called *Schutzpolizei*). State-wide law enforcement is organized by the Ministry of Interior (MI) in four sublayers. First, four state police departments *(Landespolizeidirektionen)* control and organize police laws and guidelines for their respective jurisdictions.[6] These units function as an intermediate layer and were integrated into administrative areas *(Regierungsbezirke)* in 2005. Second, each of the state police departments are divided into local police departments or presidiums which usually comprise a county or a county free city. Altogether, there were 38 presidiums

[6]Including the Landespolizeidirektion Stuttgart II which was renamed as presidium Stuttgart in 2005. Unlike other presidiums, presidium Stuttgart is a direct subordinate to the MI of Baden-Wuerttemberg.

in our sample period. Third, police departments or presidiums comprise several precincts that deal with local criminal cases in a citizen-oriented manner. In our sample period there are more than 170 precincts. This study, however, focuses on the fourth layer, the so called police stations *(Polizeistationen)*. They are the primary means of contact for residents with police forces, and are thus both a preventive and corrective arm of the executive branch. From police stations (and precincts in extension) officers go on patrols, offer consultation, and act as a point of crime notification for residents. Local police stations cover all crimes which are in their jurisdiction but the vast majority of violent crimes is dealt with by the criminal police. Only certain types of violent crimes are in the domain of local police stations. Broadly speaking, they cover cases which do not need specialized forces for crime clearance. For instance, homicide cases are not in the domain of local police stations but of criminal police and other specialized forces. There are also some restrictions on the authority of local police stations if investigations are related to organized crime or if the related case is expected to be tedious and comprehensive. Then again, more specialized forces are involved for those. Stations record evidence for crimes in their jurisdiction, process evidence, and perform the final processing of crime cases. There were more than 570 local police stations in Baden-Wuerttemberg in 2003, which, on average, translates to around one station for every second municipality in the state. The police reform of 2004 reduced the number of police stations to about 370 stations.

2.2 Background of the 2004 police station reform

Inspired by a recommendation of the state audit court of Baden-Wuerttemberg to optimize local law enforcement through the closure of police stations in the presidium of Mannheim, the state government decided to reform the highly decentralized and fragmented police organization throughout the state (Audit Court Baden-Wuerttemberg, 2002). On 21 October 2003, the state government of Baden-Wuerttemberg announced to optimize police structures as part of a larger structural reform of the public sector (Innenministerium Baden-Wuerttemberg, 2012*b*). Hence the MI instructed the presidiums and departments in 2003 to review their respective police stations in order to improve local law enforcement efficiency by considering the following criteria: *(i)* Police stations should not have less than 4 employees to provide reliable and professional local enforcement. Thus, the focus was on 302 stations with less than 4 officers. Other criteria were to *(ii)* increase widely low workloads of police forces, *(iii)* maintain small distances to nearby police stations, *(iv)* avoid increases in the number of residents per police officer and to *(v)* improve local hot spot policing.

Based on these criteria, on 15 January 2004, the presidiums and departments submitted their propositions for local law enforcement reorganizations, including the respective candidates for station closures and the receiving stations to which the affected officers should be reallocated (Landtag Baden-Wuerttemberg, 2004). Initially, the MI only intended to cut 100 police stations. However, after the review of local police stations and recommendations made by the presidiums, it announced in March 2004 to close about 220 of its 570 stations and that these closures should not have any significant consequences for public safety (Schwäbische Zeitung, 2004). According to contemporary witnesses, the stark increase in the number of closures came as a surprise. The presidiums were obliged to make their decisions transparent to the local population and local policy makers but did not require their approval (Gäubote, 2003). Since police station closures and reorganization of police staff were not a political issues but merely bureaucratic ones, no approval from the state parliament was needed (Landtag Baden-Wuerttemberg, 2004).

Police station closures were implemented in a piece-meal fashion, since ongoing rental contracts had to be considered and new real estate for enlarged police stations had to be found (Landtag Baden-Wuerttemberg, 2004).[7] Hence, police stations closed from 2004 onwards, although at a decreasing rate (see Panel A of Figure 1). Most closure events occurred in the years of 2004 and 2005 with 77 and 67 police closures, respectively.[8] Altogether, the number of police stations dropped from 574 in 2003 to 367 in 2011.[9]

According to Panel B of Figure 1 the average number of police officers per station increased from about 4 to roughly 5.5 (Panel B of Figure 1, solid line). In line with the priority of closing police stations with less than 4 police officers, the reform substantially decreased the number of these smaller stations. After the reform, however, there were still 67 stations with less than 4 assigned officers. These stations remained because of special local circumstances, distance to the other police stations, or differences in local crime levels (Steinmauern Gemeindeanzeiger, 2012).

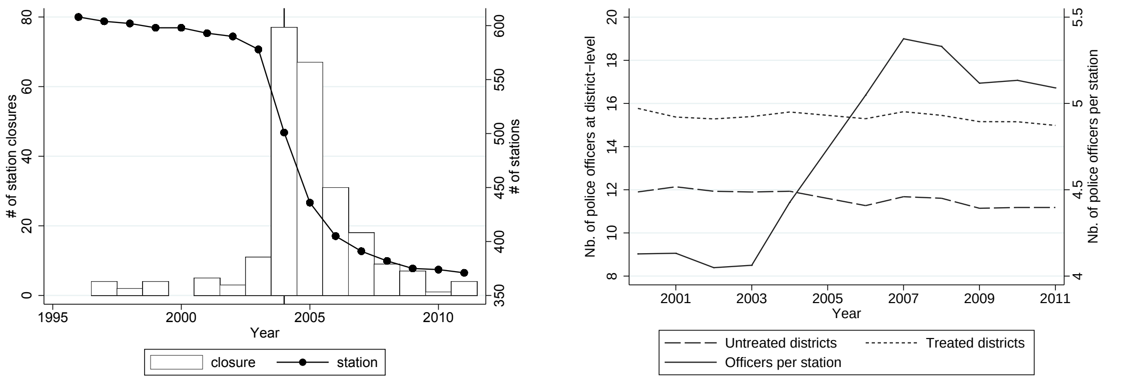
The reorganization of local law enforcement was motivated by efficiency arguments, i.e. the reduction of operating costs in the long run, as well as with improved usage of equipment, infrastructure and personnel to cope with increased use of technology (Landtag Baden-Wuerttemberg, 2003). It was also argued that fewer but larger stations should increase flexibility of police forces (e.g. with longer opening hours of prevailing stations)[10], increased presence at locations with higher crime incidence, and improved professionalism. Reassuringly for our design, the number of police officers was not changed in the course of the reform. According to Panel B of Figure 1 employment of police officers was parallel for regions which were affected by closures and those that were not. Treated districts are districts with at least one station closure during the event window. For both types of districts we observe slightly negative but parallel trends, indicating that the reform kept the number of officers constant. The figure aggregates police staff of local stations and their respective precincts to the precinct-level, which is the immediate superordinate tier of local stations police. The number of precincts were not affected by the reorganization reform. Hence, the reform did not lead to employment losses from affected stations but rather to employment shifts within the same police precinct. Moreover, police staff from closed stations were still, given the needs of everyday operation, by and large responsible for their old jurisdiction, even though they were transferred to a nearby police station as their new place of work (Landtag Baden-Wuerttemberg, 2005; Amtsblatt Eichstetten – Eichstetter Nachrichten, 2004).

[7] For example, it was decided that the station Ulm-Jungingen was closed and its officers were to be allocated to Dornstadt station but the actual closure had to wait until new real estate facilities opened up in 2006 to accommodate the new police officers (Schwäbische Zeitung, 2006).

[8] Earlier closures from 1990 to 2003 were more scarce and not due to state-wide reform. For instance, 5 smaller stations in Mannheim just closed before the reform (Audit Court Baden-Wuerttemberg, 2002).

[9] Supplementary Appendix Figure A.2 illustrates the spatial allocation of local police stations around the reform.

[10] Opening hours increased by 7.5% on average for remaining police stations (Landtag Baden-Wuerttemberg, 2005).



(A) Timing of police station closures *(B) Number of police officers*

Notes: Panel (A) plots the number of police stations closures (left axis) over the time period between 1995 and 2011. We observe 235 closing events in total in that period. The solid line (right axis) shows the total number of police stations at the end of each respective year. Panel (B) plots the number of police officers in the years 2000 to 2011. The left y-axis shows the number of police officers for treated (dotted line) and untreated districts (dashed line) and the solid line (right axis) depicts the average number of police officers per station. Note that we lack information for the year 2005. We impute the number of police officers linearily for that year.

Figure 1: Timing of police station closures and employment of police officers

Hence, these officers still patrolled their previous pre-closure town but had to travel farther from their new assigned workplace to their old jurisdiction after the reform.[11] The transferred officers also offered consultation hours in their old jurisdiction, but, had to travel farther in case of crime notification by local residents.[12]

According to the head of the state police labour union, Rüdiger Seidenspinner, station closures made it increasingly harder to be in line with prescribed intervention times of 15 minutes after notification by residents (Stuttgarter Nachrichten, 2009). This is particularly true for rural areas.

3 Data and descriptive statistics

Police station closures. We draw detailed municipal level data from various sources. First, we gather data on police station locations in Baden-Wuerttemberg for the period 1990-2011. A list of all police stations as well as suspected targets of police station closures is available for the advent of the reorganization law (Landtag Baden-Wuerttemberg, 2004). We hand-collect other information, such as the date of closure as well as the type of reorganization, i.e. which stations were integrated into which prevailing stations. We exploit various web-based sources of local

[11] Note that keeping the same officers responsible for their old jurisdiction preserved valuable local knowledge about criminal suspects, residents and the place (Landtag Baden-Wuerttemberg, 2005).

[12] The reform was accompanied by other changes to the police organization (see Innenministerium Baden-Wuerttemberg, 2012*b*). First, all four state police departments but one were integrated into administrative districts. State police department Stuttgart II became subordinate to the MI. Second, special police forces such as the highway and water police departments were integrated into police presidiums and local departments. Third, the economic control service *(Wirtschaftskontrolldienst)*, responsible for food control, was transferred from police duty to the county level. Lastly, police working hours were prolonged to 41 hours. Importantly, these changes are similar for municipalities with or without police closures and should thus not bias our empirical results on crime effects from police closures.

newspapers or called local experts in town halls or in currently existing police stations as no central database on local policing is available. After all, we use 2000 to 2011 as our sample period which centers around the 2004 reform and avoids including a possibly confounding reorganization policy of upper tier police forces (Innenministerium Baden-Wuerttemberg, 2012*a*).[13]

We complement the data on police station closures with administrative data on local police employment from the State Ministry of Interior.[14] The data is available for the years 2000 to 2011 and measures the job positions planned in the budget of the state police for each existing police station. We aggregate the number of police forces in a given year at the precinct level, which is the immediate superordinate layer of local police administration.

Crime data. We also use rich crime information from the State Criminal Office (*Landeskriminalamt*) in Baden-Wuerttemberg. Our crime data covers detailed information at the municipal-year level with respect to reported crime cases and detection rates, as well as monetary damage incurred for detailed crime categories. Monetary damages from crimes committed reflect insurance claims and are based on market values. The data is available from 2003 onwards and measures monetary damages in real euros based on 2010 prices at the municipality-year level.

We focus on property crimes through theft, especially with respect to vehicle theft and burglary. Vehicle theft comprises car theft, moped theft, and bicycle theft. Burglary can be related to buildings with residential or commercial usage. Residential burglary accounts for theft combined with breaking and entering into residential apartments, cellars, or frames (unfinished residential buildings). Commercial burglary is instead related to burglary in buildings of commercial usage, including burglary from financial institutions and postal services, offices, hotels and restaurants as well as businesses and shops. Vehicle theft and burglary jointly account for about 60% of theft in 2010.[15] Table 1 provides summary statistics across all municipalities over time.

Municipality characteristics. In addition, we exploit municipal-level variation in various sociodemographic characteristics and labour market information. In our empirical analysis we use these variables to match municipalities that experienced closures with non-affected but comparable municipalities, based on the distribution of their pre-reform characteristics across these variables. We gather data on the demographic structure of the municipalities, such as age, skill level, female population share, share of foreigners in the population as well as municipal revenues and expenditures (income and commercial revenue, public safety and law and order expenditures, and the public deficit) from the Statistical Office of Baden-Wuerttemberg. Moreover, we draw administrative information of local labour market indicators from the Institute of Employment Research (IAB), including average real daily wage, the unemployment rate and the share of individuals in active

[13]This reform did not change the number of precincts and police stations but reduced the number of state police departments and presidiums. It, however, also aimed at long term savings in infrastructure, personnel and equipment.

[14]Note that the data does not represent actual employment but budgeted jobs. These numbers may differ due to leaves of absence with respect to early retirement, parental leave, sickness or changes due to educational reasons. Moreover, the numbers do not reflect full-time equivalent positions but refer to the number of jobs only.

[15]Other observable theft categories in official crime statistics comprise theft of guns, theft in/from ATMs and theft of antiques/art or religuous goods as well as theft related to cashless payment methods.

Table 1: Summary statistics around the police reform in 2004

	2000 (1)	2002 (2)	2004 (3)	2006 (4)	2008 (5)	2010 (6)
Panel A: Reported crime cases per 100,000 inhabitants						
Total theft	1,183.433	1,269.470	1,275.155	1,125.682	1,088.539	1,017.866
Car theft	20.226	21.865	18.411	16.148	10.699	8.088
Motorbike theft	22.935	19.796	25.881	28.597	25.382	20.947
Bicycle theft	133.658	146.926	153.463	146.504	138.707	112.552
Residential burglary	78.974	78.467	77.228	65.937	62.159	65.905
Commercial burglary	541.148	543.056	555.603	440.147	398.763	399.866
Panel B: Detection rates						
Total theft	0.279	0.284	0.288	0.274	0.269	0.293
Car theft	0.424	0.442	0.454	0.478	0.380	0.404
Motorbike theft	0.243	0.241	0.245	0.224	0.263	0.270
Bicycle theft	0.076	0.093	0.098	0.090	0.087	0.093
Residential burglary	0.181	0.230	0.185	0.170	0.180	0.153
Commercial burglary	0.555	0.581	0.571	0.522	0.576	0.557
Panel C: Monetary damage per case						
Total theft			930.3	925.9	956.8	989.2
Car theft			6,085.6	7,618.1	7,828.5	8,538.4
Motorbike theft			1,448.9	1,080.0	1,228.6	1,249.4
Bicycle theft			494.1	342.7	343.4	373.3
Residential burglary			2,218.8	1,973.7	2,484.0	2,679.9
Commercial burglary			1,068.7	1,211.6	861.4	905.2
Panel D: Police information						
# Police station	0.544	0.536	0.455	0.368	0.347	0.340
Panel E: Municipality characteristics						
Population density	311.032	315.324	317.439	318.348	318.596	318.527
Population share < 15	0.186	0.182	0.173	0.165	0.157	0.151
Population share > 65	0.150	0.159	0.169	0.181	0.186	0.189
Unskilled	0.211	0.204	0.193	0.189	0.188	0.182
Skilled	0.709	0.711	0.718	0.718	0.712	0.712
High-skilled	0.080	0.085	0.089	0.093	0.099	0.106
Female	0.465	0.470	0.473	0.474	0.474	0.478
Foreigners	0.072	0.072	0.069	0.071	0.071	0.072
Real daily wage	186.709	202.651	227.154	198.324	194.117	201.356
Unemployment rate	0.035	0.042	0.058	0.049	0.035	0.041
ALMP	0.086	0.095	0.108	0.120	0.183	0.187
Agriculture	0.014	0.015	0.014	0.015	0.013	0.015
Production	0.340	0.325	0.315	0.313	0.312	0.301
Salary	0.125	0.136	0.137	0.138	0.143	0.144
Sale	0.063	0.062	0.065	0.066	0.067	0.067
Clerical	0.232	0.237	0.237	0.237	0.232	0.230
Service	0.226	0.225	0.233	0.232	0.232	0.243
Public safety expenditures	48.865	52.449	49.620	54.160	57.988	65.896
Law and order expenditures	16.885	17.493	17.231	18.182	19.273	20.588
Public deficit	-7.145	12.633	10.931	-27.332	-38.674	36.662

Notes: The table refers to the full sample containing all municipality-year pairs. Panel A shows reported crime cases per 100,000 inhabitants. Panels B and C report the number of detected cases and the monetary damage per reported crime case. Monetary damage statistics are measured in real 2010 euros and represent the market value of the goods. Panel D and E show the number of police stations and municipal-level characteristics, respectively..

labour market programs.[16] Based on wage data, we also construct several measures of income

[16] This administrative data set covers a 2% random draw of the universe of all individuals who have at least one entry in their social security records since 1975 in West Germany and starting from 1992 in East Germany. The data covers approximately 80% of the German workforce and provides panel information on individual employment biographies. Self-employed workers, civil servants, and individuals doing their military service are not included. We focus on entries from Baden-Wuerttemberg and Hesse, respectively. For detailed information see for e.g. Oberschachtsiek et al. (2008).

inequality across municipal-year pairs such as a locality being in the top or bottom wage quartile. The IAB also provides the local occupational structure for 1-digit occupations. Both local labour market conditions and the local economic structure are arguably important confounders to the effects of police station closures on local criminal activity (Entorf and Spengler, 2000).

Adding data from federal state of Hesse. While our main empirical analysis relies on a comparison of municipalities with and without police station closures within the state of Baden–Wuerttemberg, we add similar data from the neighboring state of Hesse to improve our results on several avenues. First, we use Hesse municipalities to establish an alternative control group for non-reformer municipalities in Baden-Wuerttemberg. Second, we use Hesse municipalities to study potential spillover effects of police station closures on local crime in Baden-Wuerttemberg.

Summary statistics. Table 1 outlines descriptive statistics on various theft crimes, the number of police stations as well as municipal-characteristics over time. Municipal crime data includes reported crime cases per 100,000 inhabitants, detection rates as well as reported insurance claims for overall theft, car, bicycle and motorbike theft, and residential and commercial burglary.

The data suggests that there is substantial variation across all of these dimensions of criminal activity. Overall, Baden-Wuerttemberg is among the states with the lowest crime incidence rates in Germany (Bundesministerium des Inneren, 2017) with only 5,390 overall reported cases per 100,000 residents in 2016. Relative to other European countries, theft rates in municipalities used in this study are relatively low with around 1,018 reported cases per 100,000 residents in 2010 (as compared to the EU28 average of 1,583 cases, see Eurostat, 2018). Note that reported cases of car, bicycle and motorbike theft also include unauthorized usage by persons other than the owner. Theft crime underwent a strong and persistent decline from the mid-2000s to 2010. This is observable in all sub-categories except car theft, which already started to decline somewhat earlier. Monetary damage per case also varies strongly for different property crimes. Specifically, residential burglary and car theft in particular have high monetary values, being on average 7 or 23 times the monetary value of bicycle theft (see values of Panel C for 2010 in Table 1).[17]

4 Empirical strategy

In our empirical strategy we combine matching with an event study approach. This allows us to trace criminal activity in municipalities after a police station closure compared to matched control localities in a flexible manner (see Gathmann et al., 2018 for a similar procedure). We first present the matching approach and then show the identification strategy for our generalized difference-in-differences and event-study design.

[17] Also detection rates differ across crime categories. For instance, total theft has lower detection rates than average crimes with 28% versus 58%, respectively. Bicycle theft typically has detection rates below 10%, whereas car theft is cleared in 4 out of 10 cases. Burglary in commercial properties has higher odds of detection than burglary in residential buildings with about 56% and 15% in 2010, respectively. For comparison, violent crimes or crimes against life have relatively high detection rates of 92% and 96%, respectively.

4.1 Matching procedure

The main econometric challenge we face with our control group approach is that treated and control units might differ systematically. Supplementary Appendix Table B.1 shows simple difference of means tests for municipalities with a police station closure between 2004 and 2008 (column 1), and all other municipalities (column 4) measured before the treatment event. For instance, municipalities experiencing police closures have on average less unskilled residents, more public deficits, and lower unemployment rates than all other municipalities. Hence, a treated municipality might be on a different crime trajectory not only because of the police station closure but also due to other confounding factors at the municipal level.

We construct counterfactuals by matching a similar control municipality to each treated municipality affected by the police reform starting in 2004. Our baseline matching variables cover the demographic structure of the region (population density, age, skill level, gender and share of foreigners) and local labour market indicators (average real daily wage, unemployment rate, share of individuals in active labour market programs and the 1-digit occupational structure). We also include public deficit and expenditure information of local governments (spending on public safety as well as law and order). For the chosen variables matching is done based on the figures reported for one year as well as four years prior to police station closures in order to capture differences in levels and potential pre-trends of the selected confounding variables. Importantly, we do not match on outcome variables in order to evaluate common pre-trends in criminal activity with our event-study approach (Gathmann et al., 2018).[18]

We use a Mahalanobis nearest neighbor matching procedure to find suitable control municipalities. This algorithm minimizes the standard Euclidean distance of all matching variables. In particular, this algorithm uses municipalities as controls which show the smallest sum of normalized squared differences. Following Stuart and Rubin (2008) Mahalanobis matching should not employ too many matching variables. Mahalanobis matching is preferred in this setting because the number of treatment events is relatively low at the yearly level, e.g. 66 station closure in 2004 at maximum in the first post-treatment year and 6 station closures at minimum in 2008.

In order to find suitable control units, we impose a couple of restrictions.[19] In total, we observe 235 police station closures between 1996 and 2011. We first (*i*), restrict the control units to not be direct neighbours of the treated municipality, which excludes the fact that spatial spillovers drive our results. Second (*ii*), we exclude 22 municipalities that have experienced station closures since 1996 to exclude that the results may be driven by long-term effects of events prior to the reform. Overall, this restriction leads to the exclusion of 36 treatment events due to multiple treatments over time. Third (*iii*), we exclude 128 municipalities that received police officers from the closing stations, to ensure that we measure the effect of genuine closures and do not simply compare treated towns with towns that increased staff levels. This restriction also excludes five treated municipalities with an increase in police officers before 2004. Fourth (*iv*), we drop five municipalities with changes on the precinct–level. Fifth (*v*) and last, we drop treated localities which still possess

[18] Matching on outcomes does, however, not change our results (see Table 3).

[19] We relax these restrictions by showing results on crime outcomes using alternative control group units in Table 3.

a police station after a closure event. That is, we focus only on closure municipalities that have no police station after the closure event and where distance to the next nearby police station increases after the station closure. This allows us to identify the effect of having a police station on criminal activity at the municipal level and avoid confounding effects from other remaining stations on local crime rates.

Altogether, this leaves us with 167 municipalities affected by station closures in 2004 or thereafter, and 757 potential control localities. Supplementary Appendix Table B.1 (Panel A) shows the results of the difference in means test for our 167 treated and 167 matched control municipalities measured before the treatment year. Panel B shows the respective results for all matching variables 4 years before treatment. Importantly, the matching approach works well in terms of matched control variables. Notably, there are no statistical differences between treated (column 1) and matched control (column 2) units at conventional statistical levels, which improves group similarity as compared to mean group differences of observables between treated and all other non-treated municipalities (column 1 and 5). Figure B.1 shows the spatial allocation of treated and control units across the state on a year-to-year basis. The map indicates that the both treated and control municipalities are not clustered in certain regions.

4.2 Identification and estimation procedure

Difference-in-differences. Using our matched treated and control municipalities, we compare levels of crime outcomes in municipalities with police closures with outcomes in control municipalities without closures before and after the closure events. We first estimate a simple generalized difference-in-differences model of the following form:

$$log(crime_{i\tau t}) = \beta_1 closure_{it}^{\tau} + \mu_i + \lambda_t + \theta_\tau + \sigma_c t + \varepsilon_{it} \quad (1)$$

where τ denotes relative year and t actual calender year. $crime_{i\tau t}$ refers to the number of reported crime cases in municipality i and year t. We focus on total theft as well as three important theft subcategories, namely vehicle theft (car, moped and bicycle theft) as well as burglary into residential and commercial property.

Since we log-linearize crime outcomes to ease interpretation and some types of crime were not committed in all municipality–year pairs, we add values of one to counter zero values.[20] In the empirical analysis, we choose five years before the treatment (including the treatment year) and five years after the treatment. Thus, τ takes values between -4 and 5. The main variable of interest, $closure_{it}^{\tau}$, is an indicator equal to 1 for municipalities with a police station closure following the years after the reform (i.e. $\tau \geq 0$) and zero otherwise.

We introduce relative year (θ_τ) and calender effects (λ_t) to ensure that we compare treated and control regions in the same calender year as well as relative before and after the treatment. Note that λ_t and θ_τ can differ because closure events do not occur in the same year but actual events do vary across calender years. μ_i represents municipality-fixed effects. The model further includes a

[20]Our results are robust to using sine hyperbolic transformation of zero values instead (see Table 3).

county-level specific trend $\sigma_c t$ to capture differences in time trends at the county level. This also approximates policy changes at the police presidium-level. Given that the variation comes from the municipality level, we cluster standard errors at the level of the municipality (Bertrand et al., 2004). β_1 measures the average treatment effect of police closures on crime.

Event-study design. We extend the previous model by estimating treatment effects for every year before and after police closures. We estimate the following event-study model:

$$log(crime_{i\tau t}) = \sum_{\tau=-4}^{5} \beta_\tau closure_{it}^{\tau} + \mu_i + \lambda_t + \theta_\tau + \sigma_c t + \varepsilon_{it}. \quad (2)$$

where we bin closure indicators at the endpoints as is standard in modern event-study applications (Fuest et al., 2018; Schmidheiny and Siegloch, 2019). This model allows us to assess whether the effect of the reform varies over time. As with the generalized difference-in-differences approach, the main identifying assumption of the event-study regressions is that municipalities with and without police closures follow similar trends in criminal activity before the closure. Since we do not match on outcome variables, we can assess the plausibility of this notion by comparing pre-treatment trends in outcomes with treated and untreated municipalities, respectively. Specifically, we test whether β_τ for $\tau < 0$ differs from zero. We show in Section 5 that these β_τ coefficients are indeed close to zero and statistically insignificant. Hence, the underlying common trend assumption for the difference-in-differences and event-study design appears to be valid.

Moreover, estimating year-specific treatment effects of police station closures in equation (2) allows us to measure short and medium-run effects after police station closures. Note that all treatment effects of police station closures on our outcome variables are evaluated relative to the pre-reform year $\tau = -1$. Given that we run our event-study estimations on a well matched sample of treated and control municipalities, we identify the causal effect of police station closures on crime under very weak assumptions: we identify a causal effect of police closures as long as the unobserved selection bias (beyond our matching variables) is changing over time. However, evidence of similar pre-trends between municipalities with and without police station closures across different crime categories provides suggestive evidence for the plausibility of the common trends assumption. We also provide different robustness tests to support this notion, including placebo tests and the use of different control groups as well as empirical tests for whether our matching variables develop differently after station closures between our comparison groups. This should further alleviate potential worries on time-varying selection bias.

5 Empirical results

5.1 Baseline results

Difference-in-differences. We first present the average treatment effects of police station closures using generalized difference-in-differences and then proceed with illustrating the effect dynamics and pre-trends with the event-study design.

Panel A of Table 2 shows the difference-in-differences results with respect to reported crime for total theft and the main sub-categories. Panel B shows the respective sub-categories for burglary with respect to residential as well as commercial property. The point estimate for total theft is negative and insignificant.[21] Insignificant effects may not come as a surprise here, given that there were no layoffs of local police forces but forces were merely reallocated in the course of station closures. Police station closures, however, lead to significant increases of car theft and residential burglary. Both effects are highly significant at the 1% level and large in magnitude with about 14 and 11% more reported crimes, respectively. According to Panel B, there is a consistent and strong increase across all types of residential burglary. Effects are somewhat stronger for apartment burglary as compared to theft from residential cellars or framings. Changes in moped theft, bicycle theft, and commercial burglary are not statistically significant at conventional levels. In contrast to burglary in residential properties, we do not see significant changes in any sub-category of commercial burglary either, including burglary in financial institutions, offices, hotels, or shops.

Table 2: Baseline results, reported theft crime

	Total theft (1)	Car theft (2)	Moped theft (3)	Bicycle theft (4)	Residential burglary (5)	Commercial burglary (6)
Panel A: baseline outcomes						
Reform	-0.026	0.136***	0.047	-0.044	0.106***	-0.033
	(0.028)	(0.045)	(0.043)	(0.038)	(0.045)	(0.041)
Observations	4,008	4,008	4,008	4,008	4,008	4,008
# Municipalities	334	334	334	334	334	334
Average	196.39	2.45	4.13	27.19	10.27	106.06

	Residential burglary			*Commercial burglary*			
	Apartment burglary (1)	Basement burglary (2)	Core constr. burglary (3)	Fin. Inst. burglary (4)	Office burglary (5)	Hotel burglary (6)	Shop burglary
Panel B: detailed residential and commercial burglary							
Reform	0.106***	0.083**	0.080**	0.039	0.002	-0.040	-0.080
	(0.044)	(0.036)	(0.040)	(0.044)	(0.046)	(0.049)	(0.052)
Observations	4,008	4,008	4,008	4,008	4,008	4,008	4,008
# Municipalities	334	334	334	334	334	334	334
Average	7.19	2.05	1.03	13.76	6.00	46.81	39.27

Notes: The table reports difference-in-differences estimation results for log reported theft crime. Panel (A) presents the results using the baseline categories. Panel (B) presents the results using detailed residential and commercial burglary categories. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^+$.

Our findings suggest that reorganizing local police forces by means of station closures provides a salient opportunity for potential criminals. This seems to be especially true for relatively pricey theft categories since car theft and residential burglary represent on average the most valuable opportunities for thieves (see for example Draca et al., 2018 for the role of prices on property crime). Positive effects of station closures on reported car theft and residential burglary are in line with lower salience of policing in closure municipalities given that they henceforth only contract

[21] The point estimate on overall theft appears to be negative as a result of insignificant negative effects on bicycle theft and commercial burglary, two sub-categories with relatively high average reported cases.

policing services from other stations. Specifically, local police buildings closed down but also the distance to nearby stations increased as a result of the reform.[22] Station closures may thus deter fewer crimes through a decrease in the perceived risk of sanctions which may in turn increase the expected returns from crime for potential offenders.

Event-study design. Thus far, the results represent average effects of the reform and neglect dynamic treatment effects. We will now turn to the event-study approach from equation (2) to study treatment effect dynamics, and to validate the identifying assumption of common trends regarding reported crime in closure versus non-closure localities. We present relative effects with respect to the pre-reform year, $\tau = -1$. This allows us the assess the common pre-trend assumption directly as well as test whether the effects differ by post-reform years.

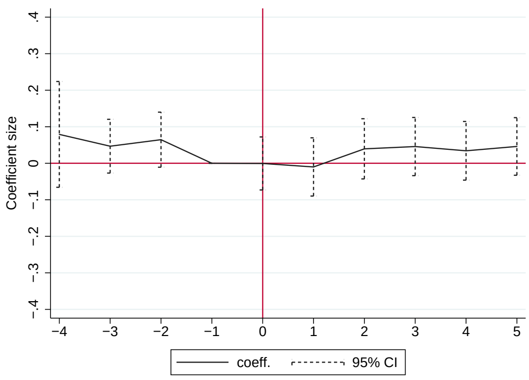
Figure 2 shows the results of the event-studies which confirm our baseline results, e.g. more car theft and residential burglary. For instance, car theft rates increase continuously after the closure event. Similar observations of immediate and permanent increases hold for residential burglary. Supplementary Appendix Figure C.1 show that these increases in residential burglary are driven by both burglary in apartments, cellars as well as frames. Results of the remaining theft crime categories are also in line with our difference-in-differences findings. Moreover, theft via breaking and entering into commercial properties does not respond to police station closures at conventional levels (see Supplementary Appendix Figure C.2). While we do observe positive effects on burglary into financial institutions and they do increase somewhat over time, we cannot identify significant effects in our sample. Importantly, all event-study plots show similar crime developments before the closure events in the respective theft category. This gives us confidence that we can provide credible and causal estimates with respect to these theft variables given that the underlying assumption of common trends for our difference-in-differences and event-study approach seems to hold.

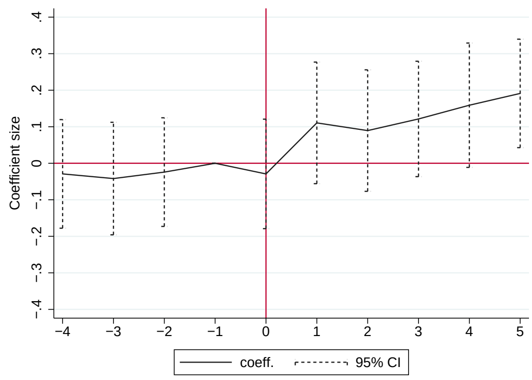
5.2 Robustness of results

Empirical specification. This subsection provides evidence on the robustness of our baseline results with respect to various sensitivity checks. First, we provide evidence using different control groups. In particular, we match on the same variables as before and additionally condition on a dummy variable equal to one if the municipality possesses one (or more) police station(s) immediately before the reform in 2003. This leaves us with 180 potential control units for our matching procedure. The second control group consist of matched municipalities in the Federal State of Hesse.[23] As a further robustness check, we use crime rates per 100,000 inhabitants as alternative

[22] Recent survey evidence, for instance, suggests that residents in places which contract police services from other municipalities tend to be less confident in the local police and its ability to clear crime (Chermak and Wilson, 2018).

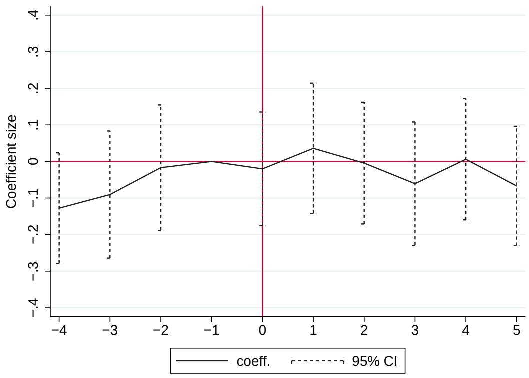
[23] Supplementary Appendix Figure E.3 presents the distribution of total theft within each state and visualizes the buffer zones of 20 km and 80 km to the boarder of Baden–Wuerttemberg. Supplementary Appendix Table E.1 shows the matching procedure with municipalities from Hesse. The first year where we observe crime rates in Hesse is 2001.

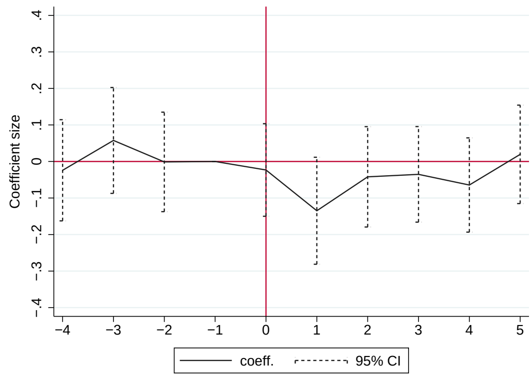




Total theft

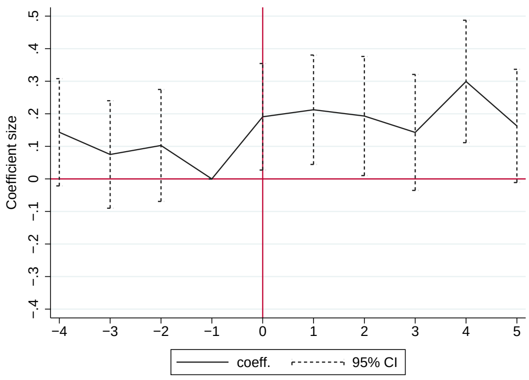
Car theft

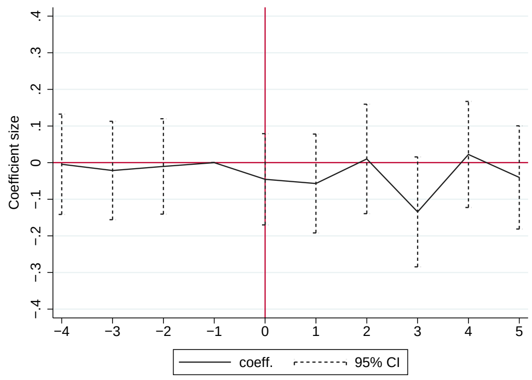




Moped theft

Bicycle





Residential burglary

Commercial burglary

Notes: The figure reports event study estimation results for log reported theft crime. Standard errors are heteroscedasticity robust and clustered at the municipality level. 95% confidence intervals are displayed by vertical bars.

Figure 2: Treatment effect of police closure on theft crime categories

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Thus, we match on municipal-level observables in $\tau-1$ and $\tau-3$. The table suggests that the matching procedure leads to substantial improvements in the balancedness of treatment and control units. Four variables still remain significantly different from zero. Except for the unemployment rate and the share of females, the variables do not develop differently after the reform (Supplementary Appendix Table E.2). The unemployment rate decreases by 0.8% points in treated municipalities and the share of females increases by 1.1% points.

outcome variables. We also apply population weighting as a sensitivity check. The next specification relates to the matching process. Specifically, we match on total theft crime rates in addition to the covariates used before. At last, we also use the inverse hyperbolic sine transformation as an alternative way to account for zero values in our crime outcomes.

Panel A of Table 3 illustrates the respective results for these sensitivity checks for our baseline theft categories. Our results are highly robust to all sensitivity checks. Hence, we find robust evidence that police station closures indeed represent salient crime opportunities for local criminals but these crime opportunities seem to be specific to car theft and residential burglary.

Reporting behavior. Section D of the Supplementary Appendix also discusses potential implications of reporting bias for our results. While changes in reporting could bias our results on higher theft rates downward, we do not find direct evidence on different reporting behavior due to station closures.

Accounting for police employment. Our baseline effects may also be confounded by changes in local police employment. Recall that according to Figure 1 employment of local police officers did not change differently for treated and untreated precincts. However, we now test whether our estimated treatment effects hold even if we control for employment levels of police officers in Panel B of Table 3. We find that explicitly controlling for police employment does not change our main findings. Moreover, we also control for precinct-specific time trends to account for varying regional law and order responses due to station closures. This may occur due to organizational changes in the transitory period for local police forces in the course of station closures. Panel C of Table 3 provides the respective results. Related effects are again robust when controlling for precinct-specific trends.

Evidence from suspect statistics. We find further supporting evidence for our baseline results by analyzing official suspect statistics from the so called *Tatverdächtigenstatistik* of the State Criminal Office. Specifically, we study the reform effects on the number of suspects in total and across crime categories as well as various demographic characteristics. Please note that legal suspects are not necessarily criminals.[24] Finding similar effects of station closures on the number of suspects for a specific type of theft would be reassuring for our baseline findings regarding the reform effects on the number of reported crime cases. Moreover, using demographic information of suspects we can additional gain insights on what type of offenders likely react to police station closures.[25]

Supplementary Appendix Table D.2 reports the results across theft categories. Panel A shows the results for all suspects. In line with the findings on reported cases, we find that the number of

[24] To be called a suspect, the mere suspicion suffices that one can be possibly convicted later on for the crime in question based on objective grounds, i.e. based on factual evidence, not on pure conjecture from conducting criminal investigations (§152 S.2 STPO). An suspect is not necessarily a convict.

[25] Unfortunately, we cannot study where offenders are coming from, given the available suspect data. This is because the suspect data is recorded according to where delinquencies take place but not where offenders originate from. Knowing this would have allowed us to see whether criminals are mobile and in-migrate to closure municipalities.

Table 3: Robustness of regression results, reported theft crime

	Total theft (1)	Car theft (2)	Moped theft (3)	Bicycle theft (4)	Residential burglary (5)	Commercial burglary (6)
Panel A: empirical specification						
Police station in 2003	-0.036	0.211***	0.065	-0.064	0.185***	-0.009
	(0.029)	(0.045)	(0.056)	(0.039)	(0.048)	(0.040)
# Municipalities	334	334	334	334	334	334
Average	170.8	2.11	3.20	23.66	9.16	84.98
Control group Hesse (20km buffer)	0.002	0.217***	-0.044	-0.134**	0.213**	-0.081
	(0.040)	(0.077)	(0.082)	(0.059)	(0.090)	(0.059)
# Municipalities	334	334	334	334	334	334
Average	119.15	1.48	2.54	14.75	4.75	59.89
Control group Hesse (80km buffer)	-0.020	0.170**	-0.076	-0.079	0.238**	-0.123^{+}
	(0.042)	(0.088)	(0.088)	(0.061)	(0.099)	(0.068)
# Municipalities	334	334	334	334	334	334
Average	119.15	1.48	2.54	14.75	4.75	59.89
Crime per 100,000 inhabitants	34.394	3.919**	3.445	-6.348	10.423**	-6.448
	(28.159)	(1.853)	(2.214)	(6.266)	(4.793)	(28.140)
# Municipalities	334	334	334	334	334	334
Average	1,293.19	17.79	26.69	161.71	74.92	560.08
Population weighted	-0.045	0.137***	-0.004	-0.060^{+}	0.119***	-0.072^{+}
	(0.037)	(0.045)	(0.054)	(0.031)	(0.039)	(0.039)
# Municipalities	334	334	334	334	334	334
Average	196.34	2.45	4.13	27.2	10.27	105.98
Matching on total theft crime	-0.024	0.143***	0.072	-0.028	0.106**	-0.026
	(0.025)	(0.043)	(0.046)	(0.040)	(0.045)	(0.045)
# Municipalities	334	334	334	334	334	334
Average	161.16	2.06	2.95	20.58	8.68	83.14
Inverse hyperbolic sine	-0.029	0.164***	0.061	-0.046	0.114**	-0.037
	(0.025)	(0.051)	(0.048)	(0.043)	(0.051)	(0.041)
# Municipalities	334	334	334	334	334	334
Average	196.39	2.45	4.13	27.19	10.27	106.06
Panel B: control for # officers at precinct level						
Reform	-0.036	0.210***	0.065	-0.063	0.186***	-0.008
	(0.030)	(0.045)	(0.056)	(0.039)	(0.048)	(0.039)
# Municipalities	334	334	334	334	334	334
Average	170.8	2.11	3.2	23.66	9.16	84.98
Panel C: precinct-specific time trend						
Reform	-0.036	0.208***	0.053	-0.074^{+}	0.194***	0.003
	(0.030)	(0.047)	(0.059)	(0.039)	(0.050)	(0.040)
# Municipalities	334	334	334	334	334	334
Average	170.8	2.11	3.20	23.66	9.16	84.98

Notes: The table reports difference-in-differences estimation results for various empirical specifications. Panel A reports the results of the reform on log reported crime for different control groups, matching procedures and manipulation of the outcome variable. Panel B shows the results controlling for the number of police officers at the next higher police organization level, the precinct level. Panel C shows results including precinct-specific time trend. The control group in Panels B and C comprises municipalities which have one or more police stations. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^{+}$.

suspects for car theft and residential burglary increase significantly after station closures. Thus, these findings provide another piece of support for our baseline findings of more reported crimes in these theft categories.

Panels B to F of Supplementary Appendix Table D.2 show the results by socio-demographic characteristics of suspects. Effects are driven by German males. Moreover, both young and adult suspects steal more cars after station closures, whereas individuals between 18 and 21 years of age do not. Interestingly, there is a significant increase in the number of burglary suspects for all age groups. Insignificant negative effects on reported bicycle theft cases are driven by adolescent suspects.

Local economic and policy conditions. Another threat to our findings are worsening local labour market conditions after police station closures. If closures lead to lower labour market activity or selective out-migration, these confounding factors may explain our observed crime effects. Note that we match on similar conditions prior to treatment, such as real daily wages, number of employees, and local tax income for closure and non-closure municipalities. Supplementary Appendix Table B.1 shows balanced pre-treatment values of local labour market conditions between both comparison groups. However, labour market outcomes, for example, could worsen from a decline in local amenities (here, local law enforcement quality) due to police station closures. Hence, we provide balancing tests (Pei et al., forthcoming) by testing whether the matching variables change after station closures. Supplementary Appendix Table D.3 shows that these variables do not change differently for treated and control units. Two variables out of 20 variables, however, are significant at the 5% level. Treated municipalities experience 0.7% points lower high-skilled shares and 1.1% points lower shares of salary workers. Evaluated at the respective means of 10% and 14%, these effects are considered to be low in economic terms and unlikely to explain the results. Besides their low economic magnitudes, the random chance that two or more coefficients out of 20 are significant at the 5% level is 26.4%.

5.3 Placebo results

This section provides first another piece of evidence on the validity of our common trend assumption by employing placebo tests with a fictitious reform event before the actual implementation. We perform these placebo tests using a treatment date of 1999 on a reduced sample of 1996–2003. Doing so, we maintain 5 post-treatment years which do not overlap with actual treatment years. However, this also reduces the pre-reform years down to 3. Panel A of Table 4 shows that all theft outcomes pass the placebo test which further alleviates worries about potential pre-trends. Panels B and C provide results on violent crime rates, which include all crimes against life comprising homicide, manslaughter, and illegal abortions. Regressions on violent crimes can be considered as placebo specifications because the criminal police, not local police stations, are legally responsible.[26]

[26] A further argument for violent crimes to be a placebo specification relates to incentive effects across different types of crime. Although a change in expected punishment reduces the propensity to (re)commit crime (Helland and Tabarrok, 2007; Drago et al., 2009; Abrams, 2012), incentives to commit violent crimes within a stable legal environment might be relatively weak (Freeman, 1999, Machin and Meghir, 2004). Contextual and environmental factors, such as stress and helplessness (Kaiser, 1996), as well as frustration/euphoria (Munyo and Rossi, 2013, Card and Dahl, 2011) are likely to be more important. Opportunities for violent crimes, e.g. in pubs or bars, are affected by, for example, pub management and liquor policies. Individual selection into to a pub in the first place is, however, still important.

Table 4: Placebo regression results

	Total theft (1)	Car theft (2)	Moped theft (3)	Bicycle theft (4)	Residential burglary (5)	Commercial burglary (6)
Panel A: placebo treatment 1999						
Reform	-0.030	0.001	-0.005	-0.071^{+}	0.030	-0.012
	(0.028)	(0.042)	(0.046)	(0.041)	(0.052)	(0.044)
Average	219.27	3.88	3.86	28.28	13.04	120.46
# Municipalities	334	334	334	334	334	334

	All crime against life (1)	Homicide (2)	Manslaughter (3)	Illegal abortion (4)
Panel B: violent crime				
Reform	-0.001	-0.002	0.002	0.004
	(0.023)	(0.018)	(0.021)	(0.006)
Average	0.35	0.09	0.17	0.04
# Municipalities	334	334	334	334
Panel C: violent crime (control group Hesse)				
Reform	-0.018	-0.019	0.004	-0.014
	(0.050)	(0.030)	(0.037)	(0.010)
Average	0.21	0.06	0.10	0.01
# Municipalities	334	334	334	334

Notes: The table reports difference-in-differences estimation results for log reported crime. Panel A reports the results on the baseline theft crime categories using a placebo treatment for the year 1999. Panel B reports the results of the reform on violent crime using treatment and control municipalities from the baseline specification. Panel C reports the results using municipalities from the Federal State of Hesse as control units. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^{+}$.

Panel B of Table 4 shows that crime against life, such as homicide and manslaughter, are not affected by station closures. Also the respective event study results in Supplementary Appendix Figure D.1 confirm that police reorganization did not change violent crime rates. Reassuringly, the common pre-trend assumptions are justified for all of these crime categories. Moreover, Panel C of Table 4 shows the respective placebo results using municipalities from the neighboring state of Hesse as control units. Again, the results show null effects on violent crime rates.

6 Mechanisms

We find that police station closures open up tempting opportunities of crime, which increases car theft and residential burglary, i.e. property crimes with high monetary rewards. What remains unclear, however, is why this is the case. Besides reduced deterrence as the main channel of increased theft, there may be other reasons that can explain our results.

First, larger theft may be due to incapacitation, and police forces may be less able to apprehend criminals from their new assigned police stations. Second, instead of a direct effect on closure municipalities, crime may be merely displaced from surrounding municipalities. Hence, we thoroughly test for related spillovers. Third, we also discuss the role of incentives given heterogenous income distributions across municipalities, which may facilitate opportunities for theft. Fourth, we test heterogenous effects of police station closures: we study salience effects from closures of larger versus smaller stations, the crime effects of closing stations of different quality, and the respective crime effects of reform-related changes in geographical distance.

6.1 Analyzing detection rates

In order to disentangle whether our findings are driven by deterrence of local policing or by changes in incapacitation (Chalfin and McCrary, 2017*b*), we first investigate changes in local law enforcement efficiency. If station closures were to improve (decrease) the probability of local police forces to apprehend criminals and take them off the streets, related crime figures would decrease (increase). Hence, we use changes in detection rates in response to police station closures as alternative outcome variables. Detection rates are defined as the number of detected cases per reported crime case.[27] Thus, we provide evidence whether or not our estimated effects on reported crime occur due to incapacitation of local law enforcement or via crime deterrence. Note that we do not measure incapacitation by means of actual incarcerations but as detection of suspects by police forces only. Detecting suspects, however, is an important legal perquisite of clearing crimes and should thus increase the odds of capture and legal punishment. Accordingly, we argue that detection rates indeed represent a valid proxy of local police efficiency.

Table 5 suggests that detection rates do not respond to station closures. We find null results for overall theft, vehicle theft, and burglary. Supplementary Appendix Figure E.1 shows the respective event-study results.[28] This rules out that incapacitation of local police forces drives our findings. Interestingly, station closures do not lead to higher detection rates due to the intended gains in professionalization in policing. Recall that staff remains constant, but closure towns henceforth contract policing.[29]

Table 5: Regression results analyzing detection rates

	Total theft (1)	Car theft (2)	Moped theft (3)	Bicycle theft (4)	Residential burglary (5)	Commercial burglary (6)
Reform	-0.004	0.027	0.029	-0.003	-0.019	0.005
	(0.008)	(0.036)	(0.031)	(0.014)	(0.023)	(0.017)
Observations	4,004	2,394	2,537	3,550	3,400	3,941
# Municipalities	334	327	329	333	333	334
Average	0.29	0.42	0.26	0.09	0.17	0.59

Notes: The table reports difference-in-differences estimation results for detection rates. The number of observations and the number of municipalities differ compared to the baseline because of zero reported crime cases. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^{+}$.

We thus provide evidence that changes in theft after police station closures are not driven by incapacitation but may rather come from lower crime deterrence of local police forces. This is consistent with previous studies on the reallocation of police forces (e.g. Di Tella and Schargrodsky, 2004).

[27] The police defines a crime to be detected or cleared if one or more suspects to a case are found.

[28] The event-study plots aggregate pairs of two subsequent years into one observation to gain statistical power since we only observe detection rates if a crime is committed in given year and crime category.

[29] Missing effects of police closures, and hence, longer driving distances to a nearby police station on detection rates are in line with Bayley (1996), who argued that response time matters only for crime detection in the first minute. However, police notifications of criminal activity are usually too long to make the speed of any police action relevant (Sherman et al., 1997). Hence, most existing studies agree that response time is not related to detection rates given that most notifications are done relatively late (Nagin, 2013). Blanes i Vidal and Kirchmaier (2018), however, find strong effects of response time on detection rates in the Greater Manchester area.

6.2 Crime displacement

Another explanation for higher criminal activity in closure municipalities may be crime displacement from surrounding municipalities. Displacement would lead to lower crime rates in neighbor municipalities and contribute to higher car theft and residential burglary in closure municipalities. For instance, spillover effects have been recently documented by Blattman et al. (2017) who study police patrols and public services. Spillovers of place-based police interventions are fairly common and occur more often as positive rather than adverse spillovers (see meta-studies by Guerette and Bowers, 2009; Braga et al., 2014; Weisburd and Telep, 2014). Two recent studies found, however, no evidence of spatial displacement (Draca et al., 2011; Di Tella and Schargrodsky, 2004). Unlike prior studies, we do not look at an intervention that intensifies policing but instead reduces state presence through eliminating local police agencies in specific municipalities.

We address crime displacement by calculating various neighborhood crime rates of treated municipalities and comparing the developments after treatment with a sample of matched control municipalities from the neighboring Federal State of Hesse. To ensure that crime rates in Hesse municipalities are not affected, we exclude Hesse municipalities at the adjacent border to Baden-Wuerttemberg in a 20 km buffer (see Supplementary Appendix Figure E.3 for a graphical visualization). We then calculate average crime rates using a contingent neighborhood matrix of direct neighbors and inverse distance matrices in a 10 km radius. Table 6 reports the results for

Table 6: Regression results analyzing crime displacement, neighbourhood crime rates

	Total theft (1)	Car theft (2)	Moped theft (3)	Bicycle theft (4)	Residential burglary (5)	Commercial burglary (6)
Panel A: theft, direct neighbors						
Reform	0.010	0.136**	-0.099	-0.032	0.064	-0.077^{+}
	(0.030)	(0.066)	(0.070)	(0.043)	(0.075)	(0.043)
Observations	3,674	3,674	3,674	3,674	3,674	3,674
# Municipalities	334	334	334	334	334	334
Average	343.35	4.80	7.62	40.25	18.45	178.78
Panel B: theft, inverse distance matrix 10km						
Reform	0.013	0.156**	-0.114^{+}	-0.035	0.078	-0.081^{+}
	(0.030)	(0.065)	(0.069)	(0.042)	(0.074)	(0.044)
Observations	3,674	3,674	3,674	3,674	3,674	3,674
# Municipalities	334	334	334	334	334	334
Average	308.18	4.45	7.17	37.68	16.83	150.0

	All crime against life (1)	Homicide (2)	Manslaughter (3)	Illegal abortion (4)
Panel C: violent crime rates, direct neighbors				
Reform	-0.036	-0.009	-0.001	-0.014
	(0.048)	(0.030)	(0.035)	(0.011)
Observations	3,674	3,674	3,674	3,674
# Municipalities	334	334	334	334
Average	0.58	0.14	0.30	0.01

Notes: The table reports difference-in-differences estimation results for neighborhood crime rates. All three panels use municipalities from the state of Hesse as control units. Panel A reports the results of the reform using the direct neighbors of treated municipalities. Panel B reports the results of the reform using an inverse distance matrix up to 10 km (municipality centroid) from treated municipalities. Panel C reports the results of the reform on violent crime using the direct neighbors of treated municipalities. Results based on the 10 km inverse distance matrix do not differ and are available upon request. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^{+}$.

neighborhood crime rates of localities with police station closures. Panel A to B show results for directly adjacent neighbors and places in a 10 km neighborhood, respectively. Panel C illustrates the results for violent crimes using direct neighbors only.

We do not find that crime displacement from neighboring municipalities contributes to our positive treatment effects for car theft and residential burglary. While there seems to be no crime change in neighboring places with regard to residential burglary, we actually do find that higher car theft in closure municipalities spills over to the surrounding area. This is true for direct neighbors as well for surrounding places in a 10 km area. Thus, effects are understated in our baseline scenario which considers closure municipalities as the only units of treatment. Considering treatment to surrounding areas of 10 km, the respective treatment effect would increase up to 29.2% more car theft. We do not find spillovers with respect to other theft types at the 5% level.

Moreover, we also do not find spillover effects for violent crimes which are not in the jurisdiction of local police stations (Panel C of Table 6). Again, point estimates are insignificant and close to zero. This provides further credibility for our empirical approach. Our results are also in line with Blattman et al. (2017) given that we find that property crimes are more likely to spill over than violent crimes. In our setting, only car theft shows positive spillovers, while it also representing the theft category with the highest monetary value per case. Therefore, one can assume that motives to victimize criminal opportunities are more sustained for this category, and more crime spills over from municipalities with station closures to nearby jurisdictions. Please note that while we do find spillover effects for car theft, our baseline effects are robust to matching procedures with and without adjacent neighbor municipalities (results are not reported).

6.3 Income inequality and crime opportunities

Local income inequalities may also affect the incidence of policing on property crime (Stucky et al., 2016; Rufrancos et al., 2013).[30] Higher income inequality within a municipality is correlated with a more unequal distribution of consumption goods and could thus lead to larger effects. Broadly speaking, higher income inequality could increase the chances that potential offenders victimize available crime opportunities. To test this mechanism, we divide the sample into municipalities being above and below the 75th percentile of the inequality distribution, respectively, and test whether our treatment effects differ between unequal and more equal municipalities. Supplementary Appendix Figure E.2 shows the income inequality distributions of treated versus untreated places before and after the reform.

Table 7 supports the claim that more unequal places faciliate higher property crimes in the wake of police station closures. Specifically, higher car theft is driven by more unequal places. Higher inequality both indicates more valuable goods to be present that are worth stealing but also large numbers of residents with relatively low incomes. This increases the returns of criminal activities (Chiu and Madden, 1998) in the standard model of economics of crime (Becker, 1968). Police station closures arguably offer salient crime opportunities for potential offenders. Theft

[30] However, some studies also find that inequality does not affect property crime (Kelly, 2000, Doyle et al., 1999 and Kang, 2016).

opportunities such as stealing cars, which offer high pecuniary rewards, are more tempting than others. Hence, in our sample, only car theft is affected by high degrees of inequality in the context of police station closures. This is consistent with heterogeneous deterrence effects. Effects on other theft crimes are not driven by income inequality at conventional levels.

Table 7: Regression results by local income distribution, reported theft crime

	Total theft (1)	Car theft (2)	Moped theft (3)	Bicycle theft (4)	Residential burglary (5)	Commercial burglary (6)
Interaction with income inequality before the reform						
Reform	-0.031	0.077	0.078^{+}	-0.026	0.095^{+}	0.006
	(0.036)	(0.052)	(0.047)	(0.046)	(0.052)	(0.049)
× income inequality (above p75)	0.017	0.175**	-0.093	-0.056	0.032	-0.118
	(0.044)	(0.074)	(0.072)	(0.069)	(0.081)	(0.071)
Observations	4,008	4,008	4,008	4,008	4,008	4,008
# Municipalities	334	334	334	334	334	334
Average	196.39	2.45	4.13	27.19	10.27	106.06

Notes: The table reports difference-in-differences estimation results for reported crime interacting the treatment indicator with a dummy variable equal to 1 if the pre-reform income inequality is in the upper quartile of the inequality distribution and 0 otherwise. Income is measured by real daily wages in 2010 prices times days in employment per year and aggregated at the municipality level. Income inequality is measured by the ratio of the 75th to the 25th percentile. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^{+}$.

6.4 Heterogeneity of police station closures

Size of police stations. Closing heterogenous stations may have different effects on crime. An important motive of the reform was to close small stations and rely on larger stations for local law enforcement. Hence, we exploit heterogenous pre-reform station sizes and test whether our average treatment effects vary with station size. Police station closures may both affect crime through the number of re-allocated police forces but also through the salience of the closed down police unit to local citizens, as represented by the police building. Interestingly, we find that closing down comparably large stations decreases the treatment effect of car theft somewhat compared to closing smaller stations. Panel A of Table 8 gives the results for interaction models of our treatment effect with stations above the median pre-reform station size of 3 officers. Results are robust to using the continuous number police officers instead. We argue that closing small stations is relatively more salient to local citizens, partially since smaller stations do engage less in patrolling and citizens have to go to the station facilities to contact local police forces. Hence, smaller stations may undergo more salient closures and drive higher reported crime in the wake of station closures. The effects of heterogeneous station sizes on closure related crime effects are robust to controlling for changes in of police forces at the precinct level.

Quality of stations. The quality of police organizations may also matter for local policing. Hence, we test whether our baseline effects are also driven by closures of relatively effective i.e. high-quality stations. Closing such stations would arguably decrease the risk of detection for potential offenders more than closing stations which are known to be unsuccessful. This can be true, despite well known misperceptions of criminals about their exact risks of detection (Lochner, 2007; Apel, 2013). Specifically, we measure overall effectiveness of police stations as the detection rate for all reported crimes in the pre-reform year 2003. Panel B of Table 8 shows that closures of stations which were in the upper quartile of the quality distribution significantly increases the number of reported burglaries for treated municipalities. This result is in line with the finding of Bindler and Hjalmarsson (2018) that only the creation of efficient police forces reduce crime. We do not find an effect of police force quality of closed down police stations on car theft or overall theft. We, however, find that closing high quality stations not only increases reported cases for residential burglary but also decreases detection rates for related crimes by about 10% (not reported). Thus, while lower incapacitation cannot explain average treatment effects, it may indeed contribute to higher theft for "bad" station closure decisions of the state authorities.

Table 8: Regression results by station-specific characteristics, reported theft crime

	Total theft (1)	Car theft (2)	Moped theft (3)	Bicycle theft (4)	Residential burglary (5)	Commercial burglary (6)
Panel A: interaction with police station size before the reform						
Reform	-0.025	0.316***	0.080	-0.071	0.193***	0.003
	(0.034)	(0.051)	(0.062)	(0.051)	(0.059)	(0.050)
× # officer (above 2)	-0.022	-0.222***	-0.033	0.016	-0.018	-0.025
	(0.034)	(0.056)	(0.064)	(0.055)	(0.065)	(0.054)
Observations	4,008	4,008	4,008	4,008	4,008	4,008
# Municipalities	334	334	334	334	334	334
Average	170.8	2.11	3.20	23.66	9.16	84.98
Panel B: interaction with police station efficiency before the reform						
Reform	-0.049	0.205***	0.068	-0.071^{+}	0.146***	-0.029
	(0.032)	(0.048)	(0.059)	(0.041)	(0.050)	(0.042)
× detection rate (above p75)	0.054	0.024	-0.011	0.029	0.157**	0.082
	(0.044)	(0.064)	(0.065)	(0.059)	(0.077)	(0.068)
Observations	4,008	4,008	4,008	4,008	4,008	4,008
# Municipalities	334	334	334	334	334	334
Average	170.8	2.11	3.20	23.66	9.16	84.98
Panel C: interaction with distance > 6 km at the time of the reform						
Reform	-0.015	0.151***	0.063	-0.026	0.108**	-0.044
	(0.032)	(0.048)	(0.046)	(0.043)	(0.048)	(0.045)
× distance (above 6 km)	-0.043	-0.061	-0.063	-0.072	-0.008	0.040
	(0.037)	(0.069)	(0.061)	(0.064)	(0.077)	(0.062)
Observations	4,008	4,008	4,008	4,008	4,008	4,008
# Municipalities	334	334	334	334	334	334
Average	196.39	2.45	4.13	27.19	10.27	106.06

Notes: The table reports difference-in-differences estimation results for reported crime using different interaction model specifications. Panel A interacts the treatment indicator with a dummy variable equal to 1 if the pre-reform police station size above or equal to 3 and 0 otherwise. Panel B interacts the treatment indicator with a dummy variable equal to 1 if pre-reform overall detection rates are in the upper decile of the detection rate distribution and 0 otherwise. Panel C interacts the treatment indicator with a dummy variable equal to 1 if the distance to the next police station after the reform is more than 6 km from the municipal centroid and 0 otherwise. Specifications in Panels A and B use, as control units, all municipalities with at least one police station. Specification in Panel C uses the baseline control units. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^{+}$.

Increasing distances to the next station. Closing local police stations increases distances to nearby police units by about 6 km on average. Given that police officers are still responsible for the municipality where their police station closed down, the closures caused an increase in driving time. We now test whether changes in distance matter for effects of station closures on crime (Blanes i Vidal and Kirchmaier, 2018). Station closures increase the distance to the nearest station mechanically as we focus on municipalities losing their last station. Changes in distance vary between 2.2 km to 13 km. Panel C of Table 8 suggests that increased distances do not drive our results on car theft and burglary in closure municipalities. Results are also robust to other distance interactions (other threshold dummies and continuous distance measure; available upon request).

6.5 Discussion

Mechanisms. Our results suggest that station closures do not affect theft in total but open up tempting opportunities for relatively valuable property goods. This holds both for car theft as well as for burglary of residential property. Average treatment effects of station closures are unlikely to be driven by incapacitation of local police forces but are instead driven by a lower visibility of local policing and partially by unequal local income distributions that may arguably facilitate the odds of victimizing more valuable goods. Moreover, we rule out alternative explanations to our findings such as a decline in police deployment, changes in distance to nearby stations, or crime displacement from neighboring municipalities. In fact, we find some diffusion of car theft to neighboring jurisdictions. Finally, closing high quality stations increases residential burglary.

Costs versus benefits. Finally, we also use simple back-of-the-envelope calculations and information on the average size of insurance claims across theft categories to quantify the direct costs of police station closures. Direct costs are moderate but permanent because thieves seem to shift attention to more costly goods. Direct costs amount to about 0.5% of pre-reform police expenditures in treated municipalities. While we are not able to quantify the true social costs of police station closures in our set-up, it is worthwhile to note that the direct costs of increased crime rates likely provide a lower bound for the real economic costs of closing down entire local police agencies. Given a lack of layoffs and better detection rates through station closures, reform benefits are not obvious, and are unlikely to cover the costs of reform. Section F of the Supplementary Appendix discusses the cost-benefit calculations in detail. It is worth noting that closing police stations may, of course, lead to substantial fiscal gains due to the opportunities to decrease employment levels and possibly more efficiently organized staff. In fact, layoffs are a frequent way to cope with budget cuts by means of station closures. If a reduction of police units goes along with layoffs, comparing costs with benefits was still far from trivial since one would have to account for additional direct costs of higher crime rates due to a reduction in police employment (Chalfin and McCrary, 2017*a*).

7 Conclusion

Does the administrative organization of police forces affect local crime? This paper provides novel causal evidence for this question by exploiting a quasi-experiment where a reform induced the closure of hundreds of local police agencies in a large German state. We exploit police station closures, which did not lead to layoffs but mere reallocations of police forces to nearby stations, in order to identify causal effects on theft in a combination of matching and event-study models.

We find that station closures do not affect theft in total but open up tempting opportunities for relatively valuable property goods. Car theft and residential burglary increase strongly and permanently through police station closures. Average treatment effects of station closures are unlikely to be driven by incapacitation but work instead through less visible local police forces and partially via unequal local income distributions that facilitate the odds of victimizing more valuable goods. Moreover, we rule out alternative explanations to our findings such as a decline in police deployment, changes in distance to nearby stations, or crime displacement from neighboring municipalities. In fact, we even find some diffusion of car theft to neighboring jurisdictions. Finally, we also find that closing relative successful stations increases theft and contributes to our findings. Our results are consistent with lower crime deterrence due to a salient closure of local police organizations. Thieves seem to take up opportunities that are particularly rewarding, i.e. being of high monetary value, if police forces are relocated through agency closures.

While reorganizations through station closures (and implied staff reallocations) may not necessarily increase overall crime, they give rise to behavioral reactions through reduced crime deterrence. Our estimates may even provide a lower bound on potential increases of property crimes since layoffs of local police forces were explicitly not part of the reform. Often, however, police closures are also accompanied by a reduction in the number of local police officers in order to increase fiscal space. Decreasing manpower in addition to closing stations would likely add to crime and reduce efficiency (Chalfin and McCrary, 2017*a*). We find that reform costs – while modest in size – are likely larger than the widely absent benefits of restructuring local police forces.

Regarding the external validity of our findings, the following remarks must be made. Most of our identifying variation comes from relatively rural places, as we focus on municipalities losing their last police station in contrast to larger cities where several stations might be available. Losing a police station may not be as salient in a town where several other stations continue to exist, compensating any negative effects from closures. Hence, we only expect our effects to be valid for other rural settings but cannot necessarily extend our findings to more urban settings.

Another interesting avenue for future research may be the effects of other reforms of police forces organizations such as the redistricting of local police wards on criminal activity. Researchers should also analyse further aspects of administrative organization of police forces in order to inform the ongoing debate on the optimal allocation of police forces.

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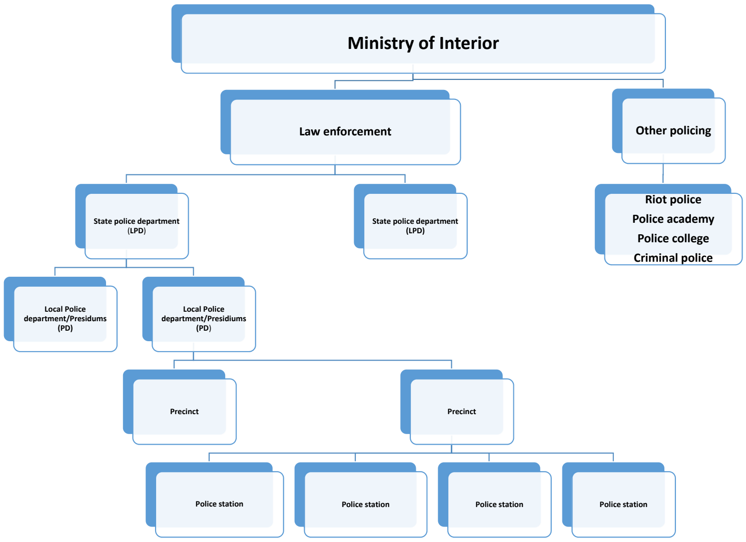
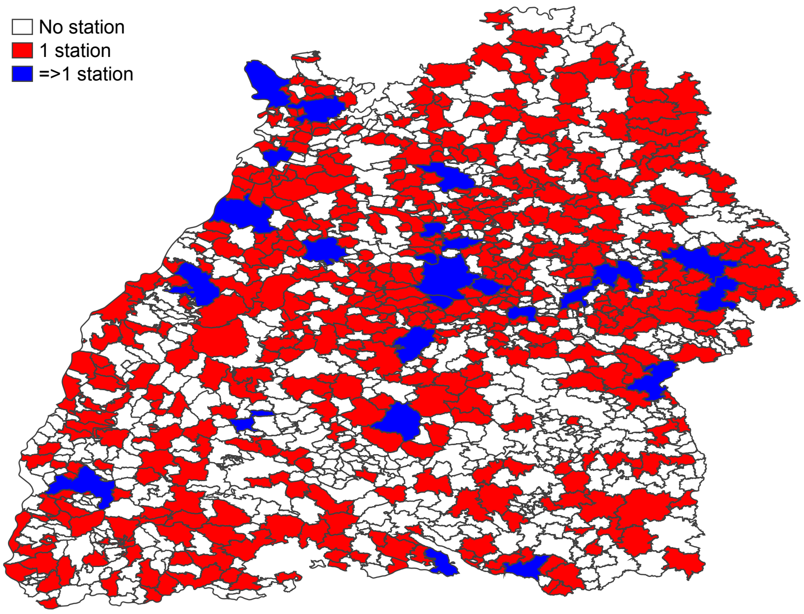
Supplementary Appendix

A Background information and summary statistics

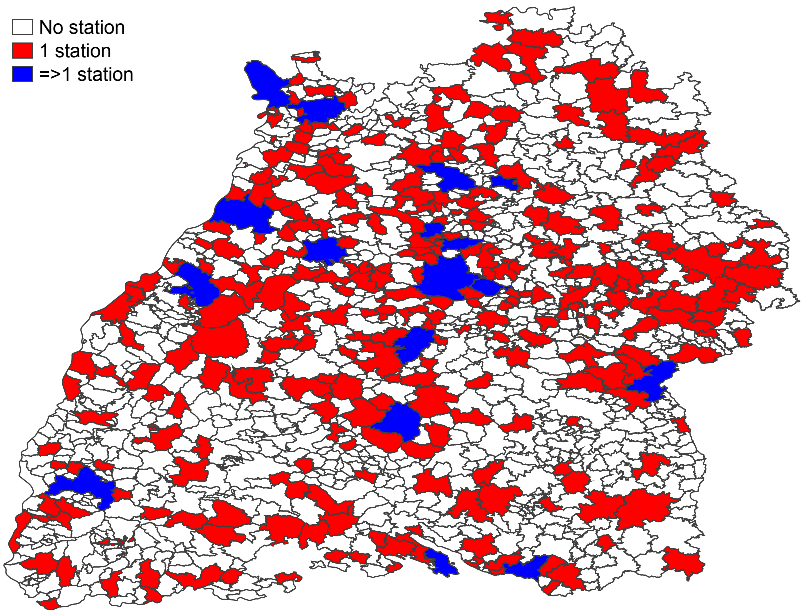


Notes: The figure shows the organization and structure of the local law enforcement in Baden-Wuerttemberg. *Source:* Own compilation based on Innenministerium Baden-Wuerttemberg (2012*a*).

Figure A.1: Organization of local law enforcement in Baden-Wuerttemberg in 2011



Panel (A) Year 2003



Panel (B) Year 2011

Notes: The figure plots the number of police stations for every municipality for the year 2003 (Panel A) and 2011 (Panel B). The dark blue color indicates the presence of more than 1 police station in a given municipality. Red color indicates the presence of 1 police station and white municipalities do not have any police station in the respective years.

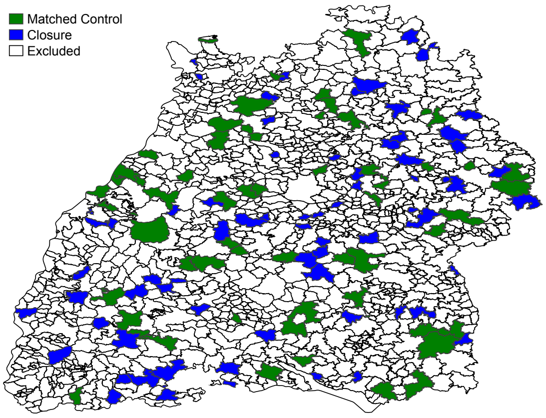
Figure A.2: Allocation police stations over time

B Matching approach

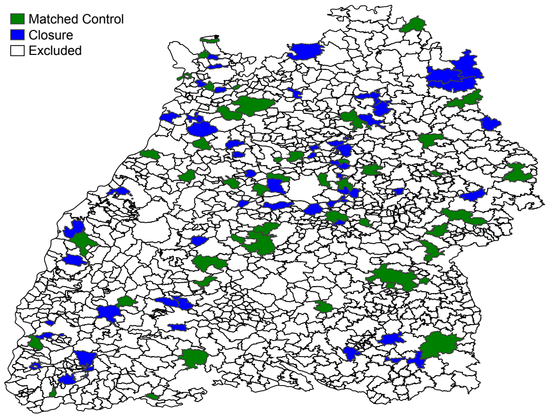
Table B.1: Treated vs control municipalities

	Treated (1)	Control (2)	p-value (3)	All municipalities (4)	p-value (5)
Panel A: 1 year before treatment τ = - 1					
Demographics					
Population density	334.345	345.946	0.733	318.530	0.569
Population share $<$ 15	0.173	0.176	0.296	0.171	0.327
Population share $>$ 65	0.169	0.165	0.160	0.169	0.928
Unskilled	0.180	0.193	0.070^{+}	0.197	0.013**
Skilled	0.726	0.717	0.264	0.711	0.089^{+}
High-skilled	0.095	0.090	0.465	0.089	0.361
Female	0.469	0.467	0.770	0.470	0.965
Foreigners	0.080	0.084	0.538	0.071	0.070^{+}
Labour market indicators					
Real daily wage	199.255	194.245	0.427	202.328	0.462
Unemployment rate	0.048	0.048	0.951	0.051	0.423
ALMP	0.128	0.093	0.077^{+}	0.103	0.036**
Occupational structure					
Agriculture	0.012	0.010	0.150	0.014	0.434
Production	0.316	0.324	0.409	0.319	0.758
Salary	0.135	0.132	0.499	0.137	0.790
Sale	0.062	0.060	0.584	0.065	0.614
Clerical	0.244	0.242	0.748	0.233	0.127
Service	0.230	0.232	0.778	0.230	0.972
Revenue/expenditure					
Public safety expenditures	51.254	52.259	0.797	52.515	0.649
Law and order expenditures	18.044	21.144	0.072^{+}	17.595	0.760
Public deficit	-0.203	17.230	0.197	3.521	0.564
Panel B: 4 years before treatment τ = - 4					
Demographics					
Population density	329.156	339.955	0.746	314.608	0.595
Population share $<$ 15	0.181	0.184	0.226	0.181	0.976
Population share $>$ 65	0.154	0.151	0.230	0.154	0.920
Unskilled	0.198	0.204	0.428	0.204	0.428
Skilled	0.718	0.714	0.625	0.707	0.243
High-skilled	0.084	0.082	0.737	0.083	0.847
Female	0.463	0.456	0.388	0.464	0.895
Foreigners	0.080	0.080	0.952	0.071	0.061^{+}
Labour market indicators					
Real daily wage	186.988	186.393	0.905	197.790	0.004***
Unemployment rate	0.036	0.036	0.969	0.043	0.011**
ALMP	0.087	0.100	0.557	0.091	0.767
Occupational structure					
Agriculture	0.011	0.011	0.761	0.014	0.163
Production	0.332	0.340	0.461	0.340	0.461
Salary	0.131	0.127	0.457	0.131	0.974
Sale	0.064	0.060	0.252	0.063	0.728
Clerical	0.235	0.240	0.454	0.232	0.779
Service	0.227	0.222	0.558	0.225	0.819
Revenue/expenditure					
Public safety expenditures	44.120	47.717	0.210	49.144	0.035**
Law and order expenditures	16.816	19.698	0.080^{+}	16.875	0.967
Public deficit	-14.912	0.483	0.241	4.785	0.002***

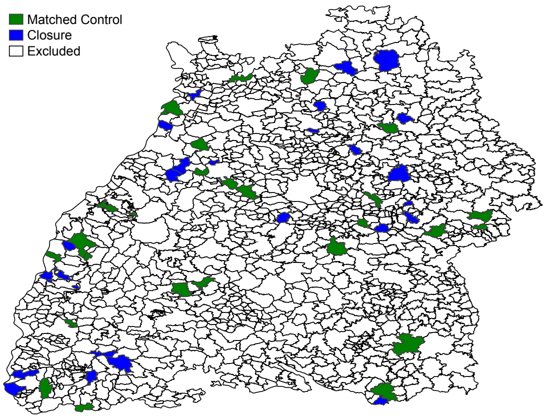
Notes: The table compares treated municipalities in column (1) with the matched control sample in column (2) and all available control municipalities in column (4) by performing standard t-tests. Treatment events are measured between 2004 and 2008 and are equal to 167. Out of the observed 167 treated municipalities, 66 event occur in 2004, 56 events occur in 2005, 25 events occur in 2006, 14 events occur in 2007, and 6 events occur in 2008. $\tau = -1$ indicates the year before treatment. Matching variables are measure the year before the treatment, $\tau = -1$ (Panel A) and 4 years before the treatment, $\tau = -4$ (Panel B). Significance levels: 1% ***, 5% ** and 10% $^{+}$.



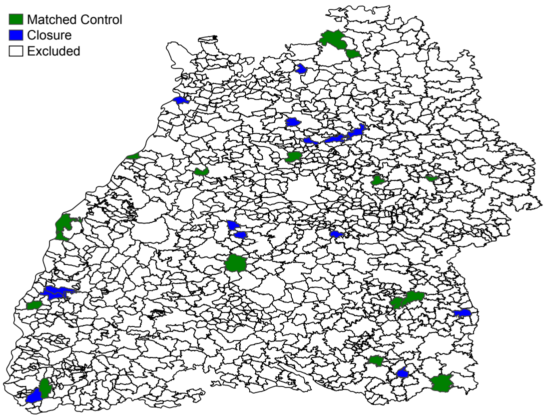
Year 2004



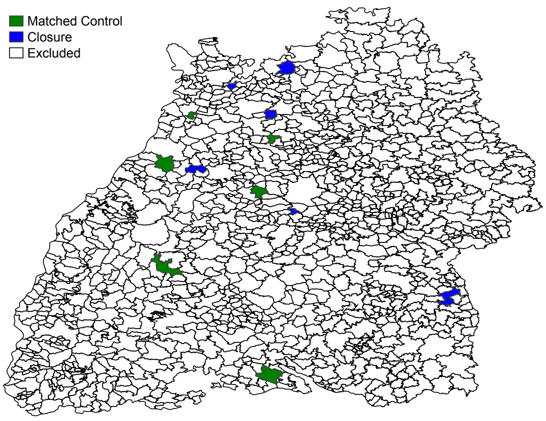
Year 2005



Year 2006



Year 2007



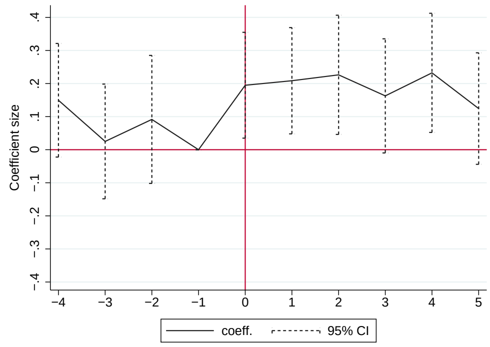
Year 2008

Notes: The figure shows the spatial allocation of treated and matched control municipalities in the Federal State of Baden-Wuerttemberg on a year-to-year basis. Treated municipalities are municipalities with a police station closure between 2004 and 2008. The figure includes 167 treated municipalities and 119 unique matched control units. 48 control municipalities are non-unique, i.e. are nearest neighbor matches in several instances: 22 matches occur twice, 20 occur three times and 6 matches occur four times. In the year 2004, 2005, 2006, 2007 and 2008 there were 66, 55, 25, 14 and 6 municipalities with police station closures, respectively.

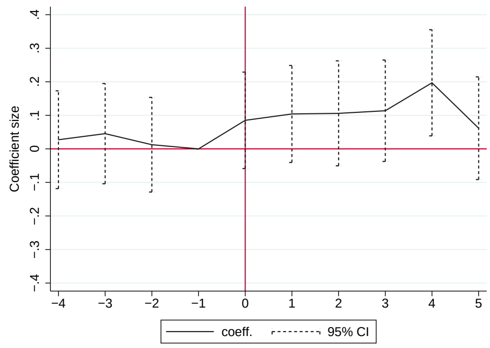
Figure B.1: Location of year-to-year matches of treated and control municipalities

C Event study results

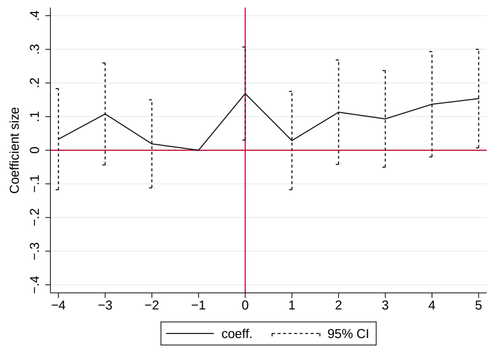
Detailed residential and commercial burglary



Apartment burglary



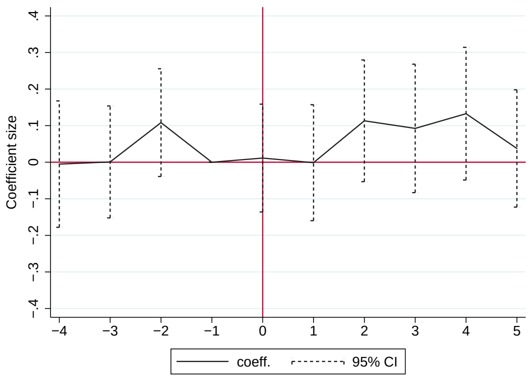
Basement burglary



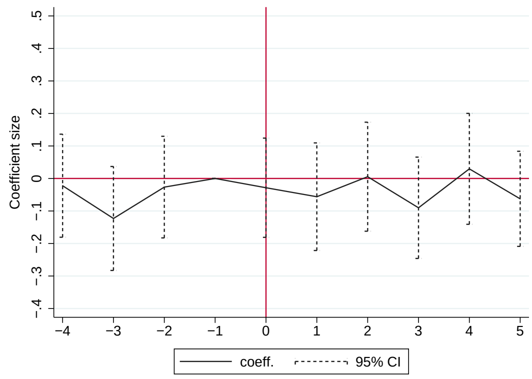
Core construction burglary

Notes: The figure reports event study estimation results for detailed residential burglary. Standard errors are heteroscedasticity robust and clustered at the municipality level. 95% confidence intervals are displayed by vertical bars.

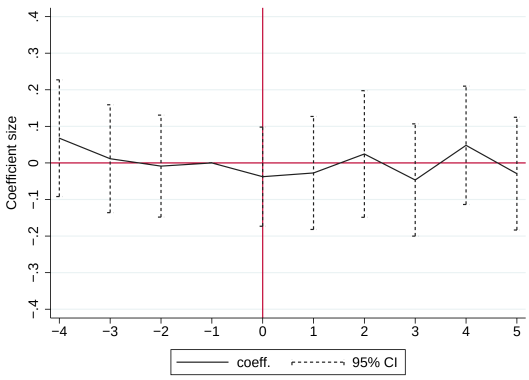
Figure C.1: Treatment effect of police closure on detailed residential theft crime categories



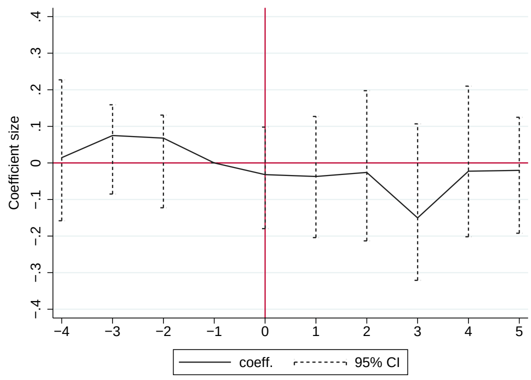
Financial institution burglary



Office burglary



Hotel burglary



Shop burglary

Notes: The figure reports event study estimation results for commercial burglary. Standard errors are heteroscedasticity robust and clustered at the municipality level. 95% confidence intervals are displayed by vertical bars.

Figure C.2: Treatment effect of police closure on detailed commercial theft crime categories

D Robustness checks

Reporting behavior

Our findings may also understate positive crime effects due to lower incentives of residents to report crime as a result of police station closures. Less reporting could occur because of higher travel distances to the next police station and, thus, higher time cost of crime reporting after police station closures. One would expect that the less severe cases were then less likely to be reported, which would, in turn, increase monetary damages (here, insurance claims) per reported case. Thus, we estimate whether the average monetary damage per reported case changes as a response to police station closures. If average monetary damages per reported case increased, this would provide suggestive evidence that residents had less incentive to report. However, related difference-in-differences results across theft categories suggest that police station closures do not affect damages per case in either theft type (see Table D.1). Nevertheless, we cannot fully rule out

Table D.1: Testing for reporting bias using monetary damage per case

	Total theft (1)	Car theft (2)	Moped theft (3)	Bicycle theft (4)	Residential burglary (5)	Commercial burglary (6)
Reform	34.232	-119.440	234.792	-41.492	-629.545	-165.894
	(138.967)	(1,522.301)	(246.147)	(72.682)	(1,346.857)	(330.558)
# Municipalities	334	334	334	334	334	334
Average	935.02	6965.23	1167.69	378.86	2296.65	1010.12

Notes: The table reports difference-in-differences estimation results of monetary damage per reported case across crime categories on the reform dummy. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% *** and 5% **.

that a reporting bias has some negative effect on our estimated treatment effects of police station closures, partially due to imprecise nature of monetary damage data and relatively large estimated standard errors. Despite null results with respect to average monetary damages per case, fewer incentives to report crime may play a role in explaining the negative but insignificant point estimate for bicycle theft in our baseline estimations reported in Table 2.

Number of suspects

Table D.2: Effects on the number and demographics of suspects

	Total theft (1)	Car theft (2)	Moped theft (3)	Bicycle theft (4)	Residential burglary (5)	Commercial burglary (6)
Panel A: All suspects						
reform	-0.0445	0.107***	0.0159	-0.0538	0.159***	-0.175^{+}
	(0.0375)	(0.0376)	(0.0421)	(0.0388)	(0.0416)	(0.0956)
Average	61.46	1.18	1.16	1.92	0.93	35.86
Panel B: Males						
reform	-0.0230	0.0950**	0.0125	-0.0398	0.153***	-0.0246
	(0.0379)	(0.0376)	(0.0416)	(0.0377)	(0.0395)	(0.0698)
Average	43.86	1.1	1.14	1.81	0.81	50.3
Panel C: German nationality						
reform	-0.0274	0.0854**	-0.00278	-0.0350	0.144***	-0.0284
	(0.0380)	(0.0395)	(0.0404)	(0.0369)	(0.0370)	(0.0723)
Average	44.4	0.84	0.96	1.42	0.77	56.0
Panel D: Youth						
reform	0.0286	0.0856**	0.00130	-0.0357	0.0609***	-0.00576
	(0.0442)	(0.0338)	(0.0413)	(0.0306)	(0.0230)	(0.0580)
Average	13.1	0.36	0.75	0.76	0.21	14.82
Panel E: Adolescent						
reform	-0.0186	0.0166	-0.0181	-0.0586***	0.0524***	-0.0109
	(0.0473)	(0.0262)	(0.0195)	(0.0220)	(0.0151)	(0.0490)
Average	5.81	0.23	0.18	0.25	0.15	5.18
Panel F: Adult						
reform	-0.0209	0.0747***	-0.00266	0.0141	0.102***	-0.0373
	(0.0384)	(0.0284)	(0.0167)	(0.0287)	(0.0349)	(0.0633)
Average	35.04	0.56	0.12	0.60	0.51	47.04
Observations	4,008	4,008	4,008	4,008	4,008	4,008
# Municipalities	334	334	334	334	334	334

Notes: The table reports difference-in-differences estimation results for the log number of suspects within the theft crime categories. Panel A shows the results for the total number of suspects. Panels B to F differentiate between gender, nationality and age groups. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^{+}$.

Local economic, socio-demographic and policy conditions

Table D.3: Regression results analyzing matching variables

	Population	Pop. share < 15	Pop. share > 65	Unskilled	Skilled	High-skilled	Female	Foreigners
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: demographics								
Reform	-0.001	-0.001	0.000	0.003	0.003	-0.007**	-0.002	-0.004
	(0.003)	(0.001)	(0.001)	(0.004)	(0.005)	(0.003)	(0.003)	(0.005)
Observations	4,008	4,008	4,008	4,008	4,008	4,008	4,008	4,008
# Municipalities	334	334	334	334	334	334	334	334
Average	8.50	0.16	0.18	0.18	0.72	0.10	0.08	0.47

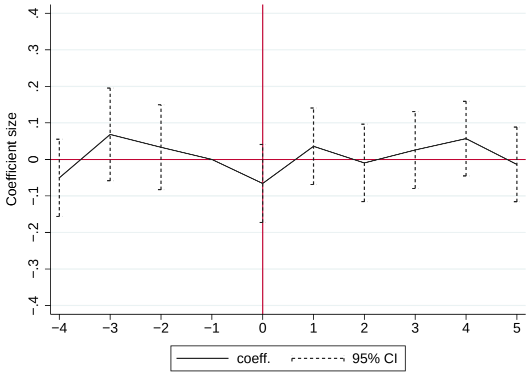
	Real daily wage	Unemployment rate	ALMP
	(1)	(2)	(3)
Panel B: labour market indicators			
Reform	-0.009	-0.004^{+}	-0.017
	(0.007)	(0.002)	(0.016)
Observations	4,008	4,008	4,008
# Municipalities	334	334	334
Average	5.27	0.04	0.14

	Agriculture	Production	Salary	Sale	Clerical	Service
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: occupational structure						
Reform	0.001	0.002	-0.011**	-0.003	0.006	0.005
	(0.001)	(0.005)	(0.004)	(0.003)	(0.005)	(0.005)
Observations	4,008	4,008	4,008	4,008		4,008
# Municipalities	334	334	334	334	334	334
Average	0.01	0.31	0.14	0.07	0.24	0.24

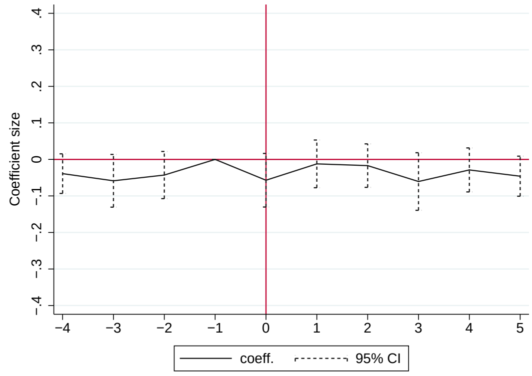
	Public safety expenditures	Law and order expenditures	Public deficit
	(1)	(2)	(3)
Panel D: revenue/expenditure			
Reform	-0.014	-0.032	-0.093
	(0.025)	(0.020)	(0.082)
Observations	4,008	4,008	4,008
# Municipalities	334	334	334
Average	1.63	0.99	2.97

Notes: The table reports difference-in-differences estimation results for the matching variables used in the baseline specification. The dependent variables in Panel A are the local demographic characteristics. Population is measured in logs. The dependent variables in Panel B are local labour market characteristics. Real daily wage is measured in logs. The dependent variables in Panel C are local occupational shares. The dependent variables in Panel D are local revenue and expenditure. All variables are measured in logs. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^{+}$.

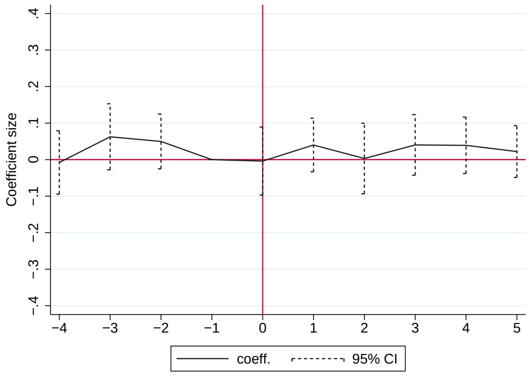
Violent crime



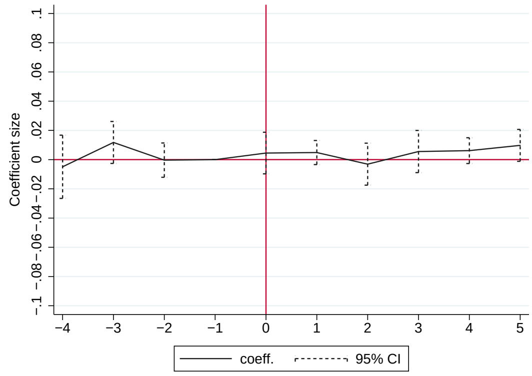
Crime against life



Homicide



Manslaughter



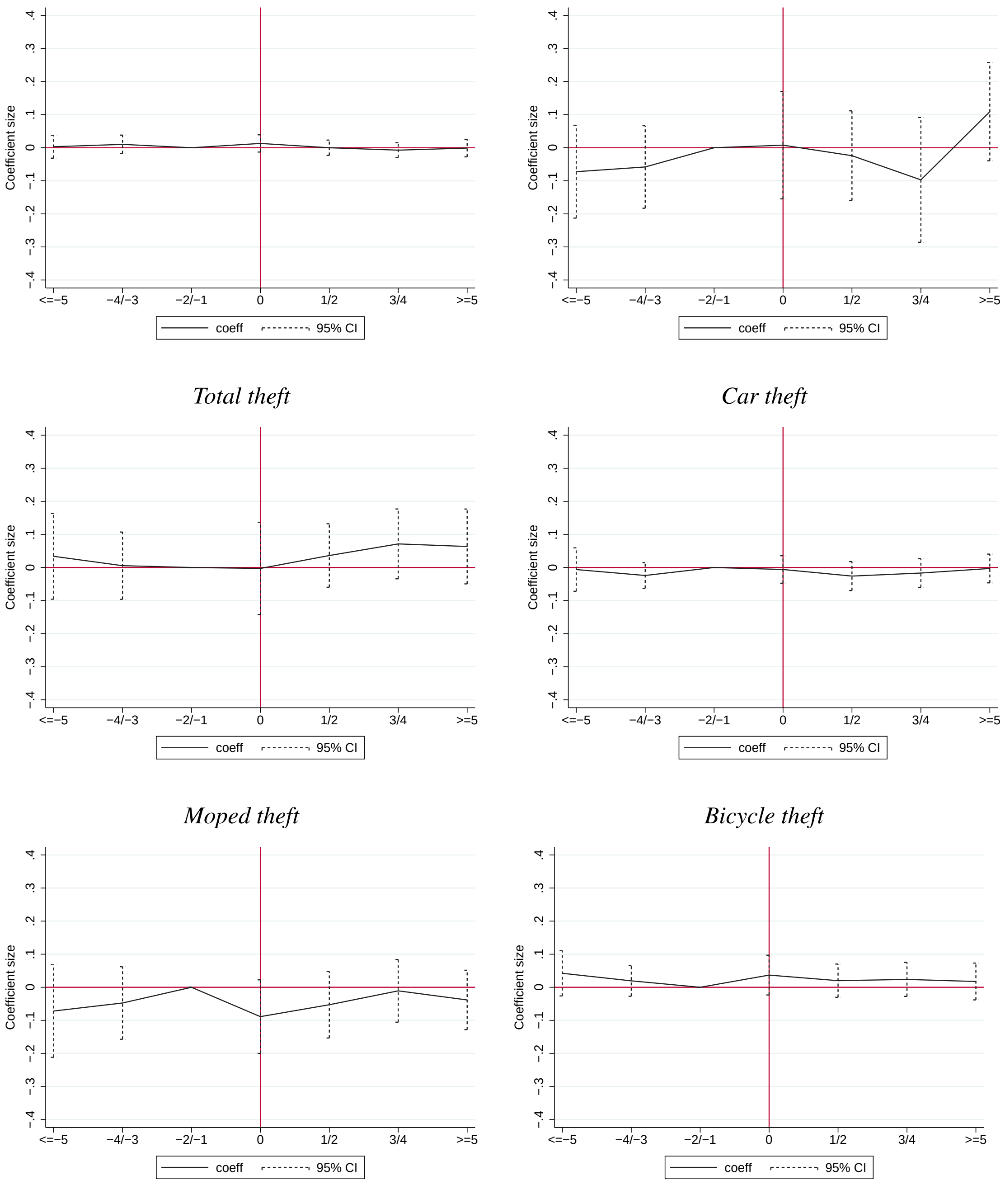
Illegal abortion

Notes: The figure reports event study estimation results on the log number of reported crime rates for violent crime categories. The categories are overall crime against life, homicide, manslaughter, and illegal abortion. Standard errors are heteroscedasticity robust and clustered at the municipality level. 95% confidence intervals are displayed by vertical bars.

Figure D.1: Treatment effect of police closure on violent crime categories

E Mechanisms

Detection rates



Total theft *Car theft*

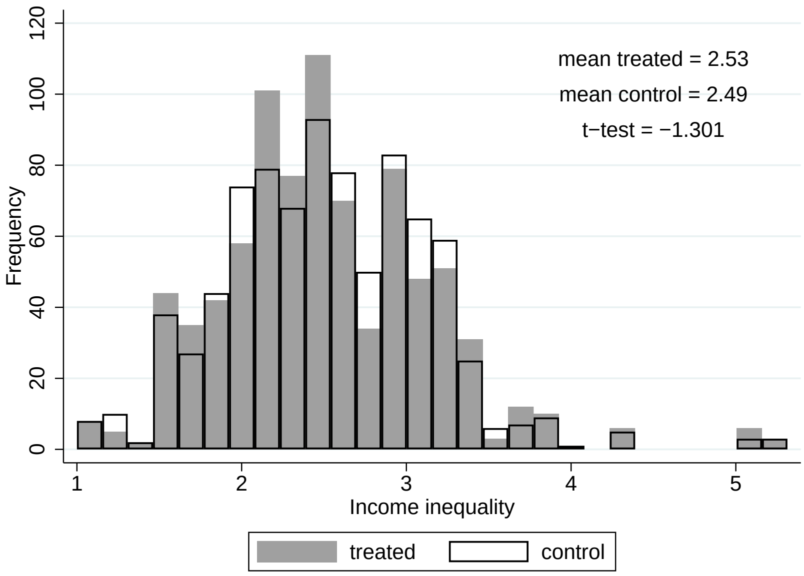
Moped theft *Bicycle theft*

Residential burglary *Commercial burglary*

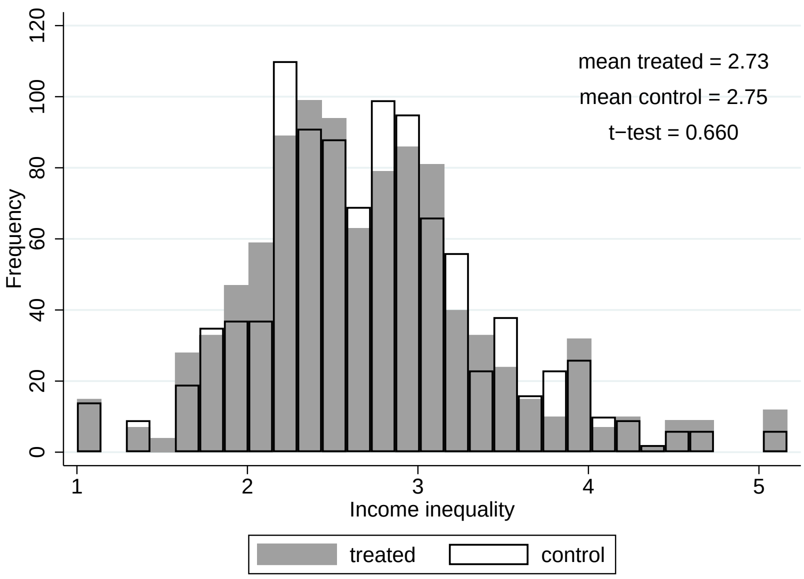
Notes: The figure reports event study results on detection rates. Due to fact that estimation is conditional on positive reported crime case for municipality i and time t, the regression model pools always two years. Standard errors are heteroscedasticity robust and clustered at the municipality level. 95% confidence intervals are displayed by vertical bars.

Figure E.1: Treatment effect of police closure on detection of theft crimes categories

Income inequality distribution



(A) Before treatment

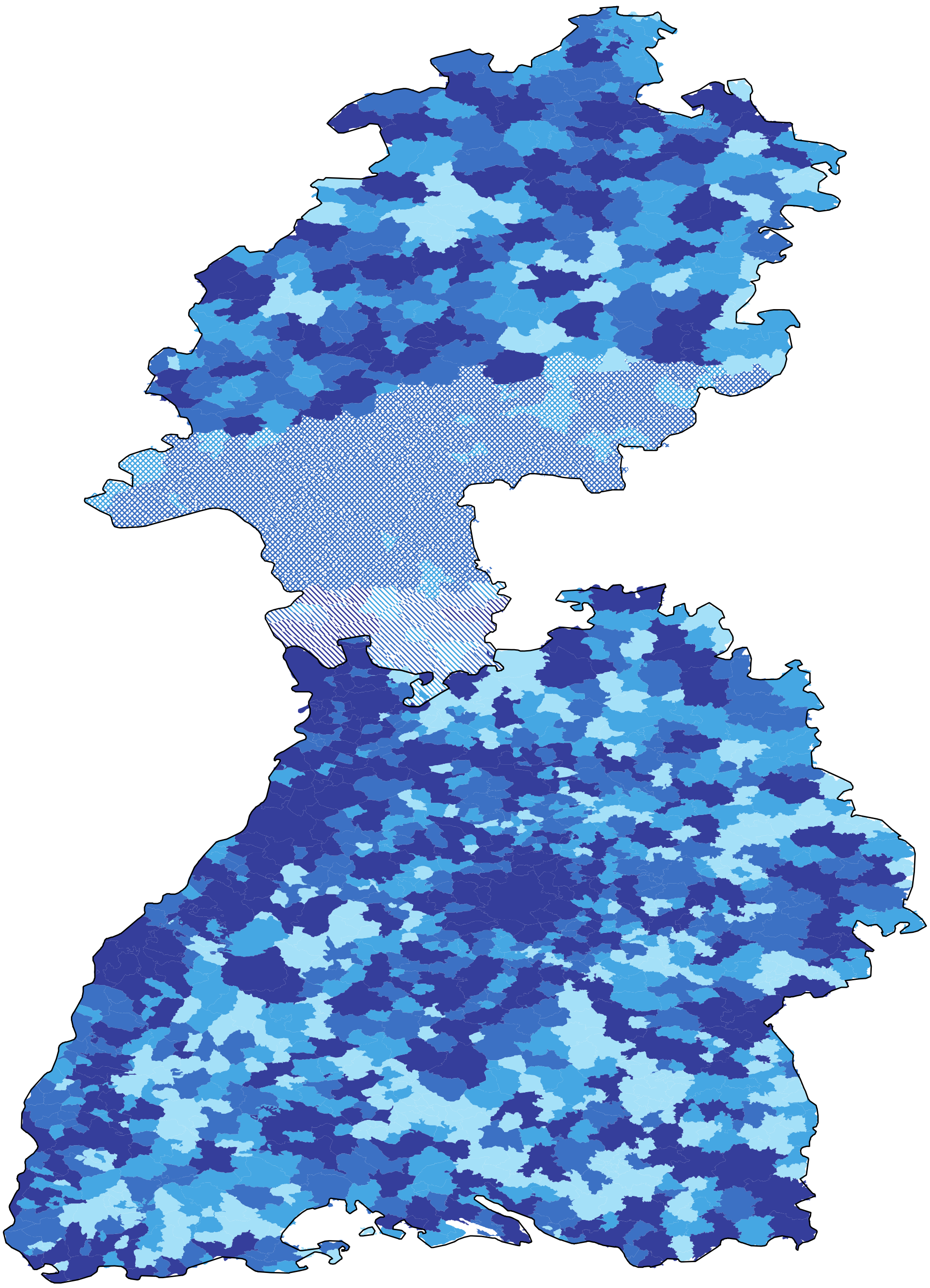


(B) After treatment

Notes: The figure plots income inequality measured as the ratio of the 75th to the 25th income percentile. Income is measured by real daily wages in 2010 prices times days of employment per year. Panel A plots the distribution before the police closure reform. Panel B plots the distribution after the police closure reform.

Figure E.2: Income inequality distribution

Total theft distribution in Baden–Wuerttemberg and Hesse



Notes: The figure plots quartiles of the total theft distribution in municipalities from Baden-Wuerttemberg and Hesse for the year 2010. Darker colors indicate higher total theft rates. The shaded area from the boarder of Baden–Wuerttemberg represents the 20 km buffer. The crosshatched area represents the 80 km buffer including the shaded area.

Figure E.3: Theft distribution and distance of Hesse areas to the border of Baden–Wuerttemberg

Matching approach, control municipalities from Hesse

Table E.1: Treated vs control municipalities, control group Hesse

	Treated (1)	Control (2)	p-value (3)	All municipalities (4)	p-value (5)
Panel A: 1 year before treatment τ = - 1					
Demographics					
Population density	335.184	323.120	0.715	323.019	0.668
Population share $>$ 65	0.174	0.182	0.001***	0.179	0.008***
Unskilled	0.177	0.178	0.838	0.188	0.068^{+}
Skilled	0.726	0.730	0.604	0.719	0.401
High-skilled	0.097	0.092	0.379	0.092	0.373
Female	0.466	0.467	0.913	0.473	0.390
Foreigners	0.079	0.068	0.089^{+}	0.066	0.006***
Labour market indicators					
Real daily wage	198.021	169.855	0.000***	190.570	0.054^{+}
Unemployment rate	0.047	0.062	0.000***	0.052	0.080^{+}
ALMP	0.111	0.110	0.975	0.113	0.852
Occupational structure					
Agriculture	0.013	0.012	0.639	0.014	0.536
Production	0.310	0.291	0.083^{+}	0.302	0.359
Salary	0.138	0.132	0.332	0.137	0.949
Sale	0.064	0.065	0.690	0.066	0.483
Clerical	0.244	0.251	0.419	0.241	0.684
Service	0.231	0.249	0.035**	0.239	0.273
Panel B: 4 years before treatment τ = - 3					
Demographics					
Population density	332.850	322.139	0.744	321.921	0.699
Population share $>$ 65	0.100	0.103	0.708	0.133	0.000**
Unskilled	0.105	0.107	0.886	0.107	0.886
Skilled	0.438	0.439	0.986	0.558	0.000***
High-skilled	0.061	0.059	0.760	0.070	0.067^{+}
Female	0.289	0.283	0.805	0.366	0.000***
Foreigners	0.047	0.040	0.253	0.051	0.350
Labour market indicators					
Real daily wage	127.529	106.558	0.064	149.380	0.000***
Unemployment rate	0.022	0.029	0.035**	0.038	0.000***
ALMP	0.083	0.065	0.357	0.081	0.878
Occupational structure					
Production	0.178	0.164	0.396	0.164	0.396
Salary	0.089	0.084	0.521	0.107	0.003***
Sale	0.038	0.042	0.471	0.051	0.000***
Clerical	0.152	0.158	0.689	0.189	0.000***
Service	0.141	0.152	0.449	0.184	0.000***

Notes: The table compares treated municipalities in column (1) with the matched control sample in column (2) and all available control municipalities in column (4) by performing standard t-tests. Treatment events are measured between 2004 and 2008 and are equal to 167. $\tau = -1$ indicates the year before treatment. Matching variables are measured the year before the treatment, $\tau = -1$ (Panel A) and 4 years before the treatment, $\tau = -4$ (Panel B). Significance levels: 1% ***, 5% ** and 10% $^{+}$.

Table E.2: Regression results analyzing matching variables, control group Hesse

	Population	Pop. share < 15	Unskilled > 65	Skilled	High-skilled	Foreigners	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: demographics							
Reform	0.007	-0.001	-0.002	0.006	-0.004	-0.001	0.011**
	(0.006)	(0.001)	(0.006)	(0.006)	(0.003)	(0.003)	(0.005)
Observations	3,340	3,340	3,340	3,340	3,340	3,340	3,340
# Municipalities	334	334	334	334	334	334	334
Average	4.44	0.19	0.18	0.72	0.10	0.07	0.47

	Real daily wage	Unemployment rate	ALMP
	(1)	(2)	(3)
Panel B: labour market indicators			
Reform	-0.025	-0.008**	0.024
	(0.020)	(0.004)	(0.025)
Observations	3,340	3,340	3,340
# Municipalities	334	334	334
Average	5.19	0.05	0.14

	Agriculture	Production	Salary	Sale	Clerical	Service
	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: occupational structure						
Reform	0.001	0.002	0.001	0.001	-0.003	-0.003
	(0.002)	(0.006)	(0.004)	(0.003)	(0.005)	(0.006)
Observations	3,340	3,340	3,340	3,340	3,340	3,340
# Municipalities	334	334	334	334	334	334
Average	0.01	0.29	0.14	0.07	0.25	0.24

Notes: The table reports difference-in-differences estimation results for the matching variables used in the crime displacement specification with municipalities from the state of Hesse as control units. The dependent variables in Panel A are local demographic characteristics. Population is measured in logs. The dependent variables in Panel B are local labour market characteristics. Real daily wage is measured in logs. The dependent variables in Panel C are local occupational shares. All variables are measured in logs. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% $^{+}$.

F Cost-benefit analysis

We quantify costs and benefits using simple back-of-the-envelope calculations. First, there are direct costs associated with more reported crime rates due to station closures. We find robust positive treatment effects on reported crime throughout our paper for costly property crimes such as car theft and residential burglary. We measure the direct monetary costs as follows:

$$Direct costs = \sum^{s} \beta^{s} * crime^{s} * damage^{s}$$

where we add up the costs of additional reported crimes from station closures across different crime types s, comprising car theft and residential burglary. For additional costs of each category s, we multiply the average treatment effect from our baseline results β^{s}, the total number of related crimes in all treated municipalities and average insurance claims per case (damage). We measure the number of reported crimes and the size of insurance claims in pre-reform year t_{-1}.

For instance, our results suggest that 38.9 (0.14*278) more cars per year are stolen in treated towns with average insurance claims of 7,917 Euros. Residential burglary has 119,8 (0.106*1,130) extra reported cases at an average cost of 3,471 Euros. This amounts to 308,153 and 415,815 Euro per year additional insurance claims for car theft and residential burglary as a result of station closures, respectively. That adds up to 723,968 Euros additional costs per year in total. Average year-to-year savings in direct crime costs thus translate to about 0.5% of police expenditures in treated municipalities before the reform in 2003. Given that we see permanent increases in crime, overall absolute direct costs amount to 4,343,808 Euros in our 6 post-treatment year period.[31]

Second, crimes may also have indirect and intangible costs which do not involve explicit monetary exchange. To the best of our knowledge, there is, unfortunately, no study to evaluate these costs in the German context. For instance, intangible costs experienced by victims, potential victims or society as a whole represent important costs to crime. According to the review of Wickramasekera et al. (2015), however, indirect costs or intangible social cost for vehicle theft and burglary are typically somewhat smaller than the direct costs. Therefore, the direct costs of increased crime rates likely provide a lower bound regarding the real economic costs of closing down local police agencies.

There may be several benefits from police station closures. First, closing presumably less efficient stations could lead to higher risks of detection through more specialized police forces. However, we find no significant average treatment effects on detection risks for any crime category. Second, closures could yield cost reductions both in terms of layoffs as well as current spending, e.g. outlays on material or rent. Unfortunately, there is no detailed information on budgetary costs available to evaluate fiscal implications of station closures. However, both personnel and

[31] Recall that while car theft and burglary increase, total theft is constant. We argue that the direct costs of eliminating entire police units can still increase given that reported crimes increased in relatively costly property crimes. Moreover, negative spillover effects to neighboring places for car theft increase costs up to 1.549 million Euros a year. 104.2 (0.14*744.3) more cars are stolen in the nearby towns using Hesse municipalities as control. This amounts to costs of 825,030 Euro year. Since there were no negative spillovers for burglary, no extra costs arise in this respect.

administrative costs per capita of state policing increase from 2003 to 2011 by 20.28% and 33.26% in 2010 prices on the state-level, respectively. Moreover, we do not find evidence of layoffs due to the reform.

Thus, intended cost savings of police station closures are unlikely to be large enough to compensate accompanying crime-related cost increases in the short-run. However, costs of the reform due to extra insurance claims at about 0,5% per-annum levels of pre-reform police spending may be relatively small but thieves permanently move towards more costly property crimes through station closures. This may increase economic reform costs in the long run. Related costs from higher reported crime numbers, moreover, fall on residents (and by extension, insurance companies) while budgetary benefits, if present, are enjoyed by the state.