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The place-based effects of police stations on crime: Evidence from station closures ☆

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ABSTRACT

Many countries consolidate their police forces by closing down local police stations. Police stations represent an important and visible aspect of the organization of police forces. We provide novel evidence on the effect of centralizing police offices through the closure of local police stations on crime outcomes. Combining matching with a difference-in-differences specification, we find an increase in reported car theft and burglary in residential properties. Our results are consistent with a negative shift in perceived detection risks and are driven by heterogeneous station characteristics. We can rule out alternative explanations such as incapacitation, crime displacement, and changes in police employment or strategies at the regional level. We argue that criminals are less deterred due to a lower visibility of the local police.

1. Introduction

Recent crises such as the Great Recession as well as the ongoing Corona Pandemic put large pressure on public budgets, which creates the need for fiscal consolidation. Policy makers, in turn, often choose expenditure-based consolidation measures (e.g. Schuknecht, 2020) and cut public spending which may be economically less costly than raising additional revenues (Alesina et al., 2019). However, spending cuts may have lasting effects on public goods provision in general and particularly so for subnational governments through public disinvestments and structural reorganizations of local public authorities (Phillips-Fein, 2013). Indeed, public entities centralize their decision-making in fiscal crises (Raudla et al., 2015), often leading to a centralization of local public good provision with little effect on expenditures (Blom-Hansen et al., 2016) but possibly far reaching consequences for the regional access to public goods (Harjunen et al., 2021).

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This paper studies a yet overlooked but prominent aspect of centralized public good provision in the domain of public safety, i.e. the closure of law enforcement agencies.[1] Indeed, many countries have centralized their police organizations in past decades by substantially decreasing the number of local law enforcement agencies (Fyfe et al., 2013).[2] Despite its prominence for recent reforms in law enforcement and the judicial system as a whole, there is no empirical evidence of the impact of this policy intervention on crime. Using detailed hand-collected information on local police station closures, this paper provides the first causal evidence of permanent place-based (dis)investments in salient police infrastructure on crime.

While policy makers centralize police agencies typically for budgetary reasons and, in order to improve efficiency of policing, shutting down entire police agencies may have ambiguous effects on crime. For instance, changes in local law enforcement strategies such as relocating entire agencies away from certain places may create criminal opportunities (Cook, 2017) and increase crime in places left with no visible reassurance of police presence. Station closures likely lead to a lower presence of police forces, and criminals may find it less costly to engage in crime (for a review on crime deterring effects of police deployment, see Chalfin and McCrary, 2017b). Likewise, visible physical infrastructure, i.e. in the form of local police agencies, represents itself a relevant parameter in the cost-benefit considerations of criminal offenders. Removing local policing infrastructure may decrease the expected or perceived value of getting caught and change the expected benefits of crime. However, local police forces may be also more efficient if closures of small and less productive police stations lead to larger and more professional police services. Hence, the effect of police station closures on crime is ultimately an empirical question.

This paper is the first to provide causal evidence on the physical organization of police forces by asking whether the regional access to local police infrastructure affects crime outcomes. For this purpose, we exploit a large-scale police reorganization reform in the German state of Baden–Württemberg that resulted in the closure of about 37% of existing local police stations. Prior to the reform year of 2004, the state had a very decentralized system of law enforcement with 579 local police stations (*Polizeiposten*).[3] Importantly, the reform merely reallocated local police forces to nearby stations from which they typically kept patrolling their old jurisdictions. This generates a unique setting which allows us to study the effect of place-based disinvestments into physical police infrastructure, i.e. local police station closures, on crime outcomes.

Causal inference is challenging since it is unlikely that these closures are implemented at random. Instead, policy makers attempt to close stations in low crime areas and target efforts at crime hotspots (Braga et al., 2014).[4] Selection would, in turn, bias our estimates of police station closures in a simple before–after comparison. In order to estimate causal effects of regional closures of police infrastructure on crime, we use a combined approach of matching and difference-in-differences to account for these endogeneity concerns. The matching approach allows us to compose a control group that is most similar to municipalities that undergo station closures by using various pre-treatment municipal-level characteristics on demographics and local labor markets. Based on a comparison of treated municipalities and similar matched control municipalities, event study estimates allow us to trace treatment effect dynamics after the closures and, importantly, let us falsify the identifying assumption of common trends of local crime before the reform.

In our empirical analysis, we focus on the impact of local police station closures on property crimes. We study detailed measures of theft and burglary since in our set-up local agencies are not responsible for most violent crimes. Our results suggest that police station closures do not affect overall theft. However, we find substantial changes in the way local criminals conduct property crimes, i.e., we observe an increase in reported car theft and burglary in residential buildings. We do not, however, observe more crime with respect to other theft categories, including other vehicle theft or burglary in commercial buildings. Importantly, our event-study estimates indicate that related effects are not driven by pre-existing trends that differ across groups.

We find that our effects are driven by facility-specific features of local police agencies. While closing police stations in residential neighborhoods outside of town centers drives higher residential burglary, the result of more car theft can be partially explained by the closure of relatively effective police stations. We argue that station closures change the perception of the incentives and the opportunities available to local criminals which are, in turn, less deterred from local policing. More car theft and residential burglary after police station closures are thus both consistent with a permanent negative shift in the perceived risk of detection for the respective criminal actions. In line with economic models of crime (Becker, 1968; Ehrlich, 1973), individuals commit crimes depending on incentives available to them. Specifically, individuals choose between criminal and legitimate activity based on a cost-benefit calculation under uncertainty in which they trade-off the expected benefits of crime (comprising illegitimate income net of the probability of being caught) and the expected opportunity costs of crime through foregone legal income. How can station closures be reconciled in this framework? The reform does neither affect the strength of sanctions for illegal behavior nor expected incomes from legal and illegal work differently for municipalities that lose a police station as compared to jurisdictions that do not undergo closures. However, restricting the visible availability of local stations as physical police infrastructure may change the perceived risk of being caught. Police stations are arguably a salient reassurance of regional police availability, representing a parameter for the expected value of getting caught and changing the expected benefits of crime (Becker, 1968). Closing police agencies may thus provide an opportunity for crime due to a salient change in perceived risks of being caught.[5] Our results suggest that both the quality and location of stations matter for perceived risks of detection.

We extensively discuss alternative mechanisms. Using detection rates as a metric to approximate conviction risks in our context, we do not find changes in the actual effectiveness of police stations after the reform. On average, incapacitation of the local

[1] 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 are small local law enforcement agencies that clear crime at the municipal or county level.

[2] Examples for this ongoing trend are police agency closures in the US (Brunet, 2015), New Zealand (New Zealand Parliament, 2017) as well as in several European countries. Among them are, for instance, Germany (this study), the UK (Metropolitan Police, 2016), Switzerland (Aargauer Zeitung, 2017), Belgium (Vereniging van Vlaamse Steden en Gemeenten, 2017), Finland (Haraholma and Houtsonen, 2013), Austria (Bundesministerium für Inneres, 2014), Denmark, Scotland, and the Netherlands (Mendel et al., 2017).

[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 rates differ with demographic and labor market characteristics, e.g. unemployment (Entorf and Spengler, 2000), wages (Machin and Meghir, 2004), and shares of foreigners (Bell et al., 2013).

[5] The opportunity-of-crime literature argues that the situational context is important for theft and burglary (Felson and Clarke, 1998; Clarke, 2012).

police should thus not play a role for our main effects. This speaks for the interpretation that salient station closures changed the perception of detection risks rather than their actual levels. This is in line with anecdotal evidence stating that subjective perceptions of public safety decreased in communities that experienced a closure of a local station (e.g. Reutlinger General-Anzeiger, 2007). We further rule out that our baseline effects arise due to crime displacement from neighboring towns.

Overall, our baseline results are consistent with lower crime deterrence due to a salient closure of local police agencies. It appears that thieves respond to the new opportunities of crime provided by local police station closures (Cook, 2017). We perform several sensitivity and robustness tests. Most notably, our main findings are robust to alternative control group definitions. In the course of the reform, police officers were merely reallocated to a nearby station and typically patrolled their old jurisdiction from their new station. Our baseline results are robust to explicitly controlling for changes in police employment at the regional precinct level. Moreover, our results remain unaltered when controlling for potential changes in police strategies at the regional precinct level. While these sensitivity checks make it unlikely that changes in police manpower confound our main results, we cannot entirely exclude the possibility that our results are in part driven by differences in police manpower and changed patrolling intensity. This is because we lack more detailed data on police patrol records to assess all changes in police activities due to station closures.

This paper makes several contributions to the literature. First, we study the crime deterrence effects of a permanent regional reallocation of police forces at the extensive margin through police station closures. By doing so, we contribute to a large literature about the effects of police deployment on crime and show that not only a visible deployment of police manpower, but also the salient allocation of police infrastructure can deter crime.[6] Prior studies use, for instance, exogenous variation in police force allocation from large scale terror threats and find that place-based increases in policing reduce crime through more crime deterrence (Klick and Tabarrok, 2005; Draca et al., 2011; Di Tella and Schargrodsky, 2004). We find similar effects for theft and residential burglary when studying the effects of local station availability. According to Blanes i Vidal and Mastrobuoni (2018), however, increasing patrols in normal times does not curb crime. Weisburd (2021) also documents 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. Importantly, previous studies analyze the effect of temporary rather than permanent shifts of police staff on crime. Since we focus on a permanent reallocation of local police forces rather than temporary changes in police effort, our paper is most closely related to Bindler and Hjalmarsson (2021). They study a permanent change in policing, namely the creation of the London Metropolitan Police on crime and document that more policing deters violent crime. By contrast, we study a reduction in the local access to police stations and, hence, the crime effects of visible police infrastructure rather than via a change of manpower. Morales-Mosquera (2019) complements our contribution in his recent paper by studying the willingness to pay for crime control through new police stations. Specifically, he exploits police station openings in Columbia to show that, due to their crime deterring effects, citizens indeed value having local police stations. The average marginal willingness to pay in order to avoid crime through new police stations is $4,500 per household.

Second, we specifically speak to a growing literature on place-based policing strategies (see Chalfin and McCrary, 2017b for a review). There are many ways through which policing could deter crime, ranging from simply adding more manpower and resources (Machin and Marie, 2011; Mello, 2019) to the use of various police tactics, including rapid response to calls for service (Weisburd, 2021), problem-oriented targeting (Kennedy et al., 2001), local community policing (Maguire et al., 2017), environmental features such as more street lighting (Chalfin et al., 2021) as well as hot-spot policing (e.g. Weisburd and Green, 1995; Rosenfeld et al., 2014; Braga and Bond, 2008; Braga et al., 2014; Blattman et al., 2017). Unlike previous papers on the strategy of hot-spot policing, we provide novel evidence on place-based and permanent disinvestments into physical infrastructure of the police. Thus, we evaluate the effects of a new margin of place-based policing which represents a permanent negative shift in police visibility as compared to other interventions which temporarily increased police efforts in certain areas and were at the heart of previous contributions (see above).

Third, we also add to the long-standing literature of fiscal federalism and, thus, to aspects of efficiency of governance. Previous work studied the optimal size of governments and documents that larger public bodies would benefit from cost savings through economies of scale (Oates, 1972; Bolton and Roland, 1997; Alesina and Spolaore, 1997). These savings, however, may likely come at the cost of more heterogeneous populations with more diverse needs in their jurisdiction (Oates, 1999). The benefits of centralizing public good provision are ultimately ambiguous on ex-ante grounds and depend on the details of the reform and the public good itself. Empirical evidence on (de)centralizing the provision of specific goods is scarcer. There is evidence on (de)centralizing public good provision in areas such as health care (Avdic, 2016; Avdic et al., 2018), schools (Brummet, 2014), as well as centralized supervision regarding environmental regulation (Zhang et al., 2018) and public employment services (Mergele and Weber, 2020). Additionally, Blom-Hansen et al. (2016) and Blesse and Baskaran (2016) study the effectiveness of consolidating entire local governments through municipal mergers. Complementing these papers, our study offers new insights regarding the efficiency of governance in the domain of law enforcement agencies.[7] We find that closing local providers of law enforcement increases property crimes in the affected area. These negative effects on crime are not compensated for by enlarged prevailing stations nearby which absorb the affected police officers from closed stations.

The paper is structured as follows. Section 2 describes local law enforcement in Baden-Württemberg 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 the mechanisms of our main findings, respectively. Section 7 discusses alternative outcomes and treatment definitions. Section 8 concludes.

[6] A related set of papers in economics and criminology studied whether more police officers lead to less crime and found that despite discernible differences about the exact elasticities (Levitt, 1997; Evans and Owens, 2007; Lin, 2009; Chalfin and McCrary, 2017b), more police forces are indeed effective in combating crime (Chalfin and McCrary, 2017b).

[7] There is a relatively small related literature on the effects of changes in the design and structure of law enforcement institutions (for a review see Weisberg, 2013). Ater et al. (2014), for example, study an organizational change where responsibilities for housing arrestees were shifted from the police to prison authorities. Other papers find improvements for police productivity via specific organization and management practices such as COMPSTAT (Garicano and Heaton, 2010) or similar programs (Soares and Viveiros, 2017). Fu and Wolpin (2017) estimate a structural model of crime in order to evaluate several targeting schemes that allocate federally-sponsored additional police across cities. They find that decentralized decisions on resource allocation are more efficient than centralized actions. However, not much progress has been made in the literature on police (re) organizations on crime since the early review of Bayley (1992, p. 509) stating "that very little is known about the effects of police organization on goals and objectives."

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 organized at the state-level. There are, however, large disparities across federal states regarding the effectiveness and organization of local law enforcement. Baden-Württemberg 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 Fig. 1, state policing covers both law enforcement and other areas, such as state criminal police. Criminal police addresses criminal cases with special demands for crime clearance and a high degree of severity or hardship which demands specialized expertise and investigation efforts, e.g., murder, sexual assault, and organized crime which all can be subsumed under serious (violent) crime cases. The present study focuses 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.[8] These units function as an intermediate layer and were integrated into administrative areas *(Regierungsbezirke)* in 2005. For the sake of simplicity, these layers are spared from Fig. 1. Second, each of the state police departments is divided into local presidiums which usually comprise a county or a county-free city. Altogether, there were 38 presidiums in our sample period.

Panel (A) of Figure A.1 of the Supplementary Appendix further provides a visualization of the administrative boundary at the county level. Third, police departments comprise several precincts that deal with local criminal cases. The black X in Panel (A) of Figure A.1 shows the distribution of precincts across space. In our sample there are 156 precincts in the year 2003. This study focuses on the lowest layer, the police stations *(Polizeistationen)*, represented by the red dots in Panel (A) and as can be seen at the bottom of Fig. 1.

Police stations are direct subordinates of their respective precincts and represent the primary means of contact for residents with local police forces. Thus, they are both a preventive and corrective arm of the executive branch. 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 of the state. 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 and cases of organized crime are not in the domain of local police stations but are treated by specialized forces like the criminal police. Local stations ultimately record and process evidence for crimes in their jurisdiction and perform the final processing of crime cases.

From stations (and precincts in extension) officers go on patrols, offer consultation, and act as a point of crime notification for residents. Panel (B) of Figure A.1 of the Supplementary Appendix shows the administrative boundaries of the municipalities within the county of Ostalbkreis (located in the North-East). Each dot in Panel (B) represents the location of a police station with the color indicating different precinct affiliation where the particular station is attached to. In this example, police stations belong to three different precincts. Usually, officers' responsibilities stop at the border of the county. Within the county, officers go on patrols in close-by municipalities that are typically predetermined. In the example provided Panel (B) of Figure A.1 of the Appendix, police officers located in stations organized by the precinct of *Eilwangen* (green dots) patrol the North-East of the county, whereas police officers located in stations organized by the precinct of *Schwäbisch Gmünd* (black dots) patrol municipalities in the West of the county. In an emergency call, however, police officers that are usually closest to the case or the crime spot take over. This is irrespective of whether the municipality is covered by the precinct of *Schwäbisch Gmünd* or *Eilwangen*.

2.2. Background of the 2004 police station reform

In 2003, the year before the reform, Baden-Württemberg had 579 local police stations, indicating, on average, one station for every second municipality. Inspired by a recommendation of the state audit court of Baden-Württemberg 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). Among others, the reform was expected to create opportunities for improving personnel usage and a reduction of costs of running police offices (Landtag Baden-Wuerttemberg, 2004). On October 21, 2003, the state government of Baden-Württemberg announced optimizations for police structures as part of a larger structural reform of the public sector (Innenministerium Baden-Wuerttemberg, 2012b). Hence, the MI instructed the presidiums 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 four employees in order to provide reliable and professional local enforcement. This restricted the focus to 332 stations with less than four officers although a considerable amount of such stations remained after the reform. Other criteria were to *(ii)* optimize workload of local police forces, *(iii)* preserve small distances to nearby police stations, *(iv)* maintain staff-resident ratio of about 5,000 inhabitants per police officer, and *(v)* prevent systematic criminal hot spots in the affected areas (Landtag Baden-Wuerttemberg, 2003). Station closures were supposed to improve police work for local residents and not result in negative effects for public safety. Despite the closures, the reform also wanted to maintain existing employment levels among police officers (Landtag Baden-Wuerttemberg, 2004).

Based on these criteria, on January 15, 2004, the presidiums 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 200 of its 579 stations and that these closures should not have any significant consequences for public safety (Schwäbische Zeitung, 2004). Specifically, between 2004 and 2011 we observe 216 changes in the availability of police stations at the municipality-year level, i.e., a reduction in the absolute number of one or more stations. 95% of these events are observed between 2004 and 2008. At the level of the police station, however, 232 stations are closed during this time period. Therefore, not every station closure led to a reduction of stations at the municipality level, for instance, through a creation of a new enlarged station which may have absorbed a smaller old station within that same municipality. Out of the 232 station-level closure events, six events did not cause fewer stations at the municipal level as they were integrated into new stations in the

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

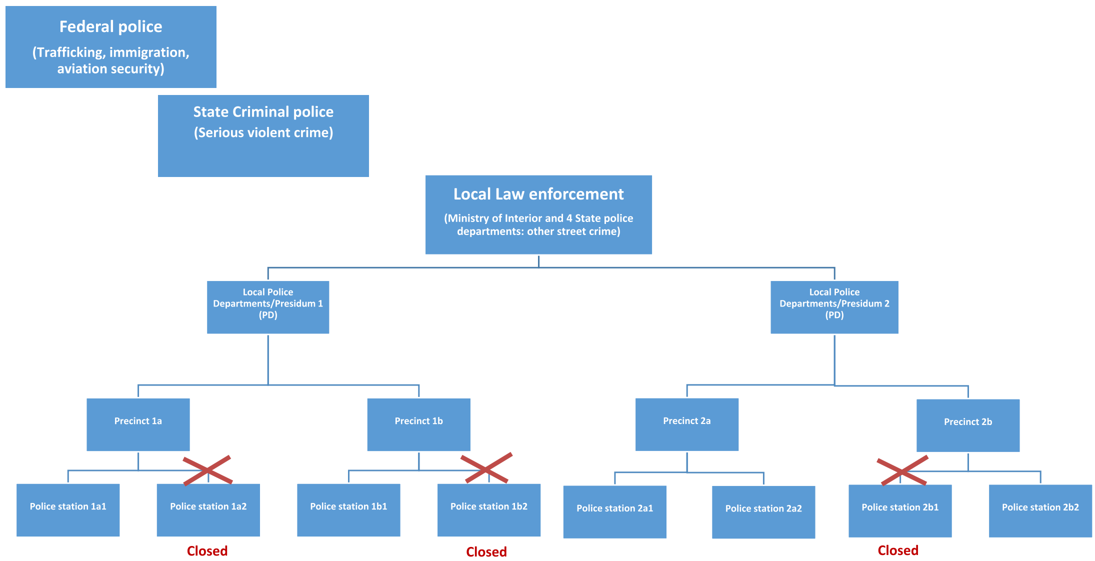


Fig. 1. Reforming local law enforcement in Baden-Württemberg through station closures. *Notes:* The figure shows the organization and structure of the local law enforcement in Baden-Württemberg, including the tasks of federal and state criminal police forces. For the sake of simplicity, the most upper layer of the Ministry of Interior and the subsequent layer comprising the state police departments are spared out and mentioned only by name. *Source:* Own compilation based on Innenministerium Baden-Wuerttemberg (2012a).

same municipality. Moreover, in 46 station closure events the respective officers were integrated into already existing stations in the same municipality. Given that our level of observation for crime outcomes is the municipality, we cannot exploit this variation. This generates 180 (=232-6-46) station closure events with an actual reduction in the number of available police stations where officers where relocated to stations outside of their municipality.[9]

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 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).[10] Hence, police stations closed from 2004 onwards, although at a decreasing rate (see Panel A of Fig. 2). Most closure events at the municipality-year level occurred in the years of 2004 and 2005 with 81 and 71 police closures, respectively.[11] By the end of 2011, the number of police stations dropped to 365.[12]

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

According to Panel B of Fig. 2 the average number of police officers per station increased from about 4 to roughly 5.5 (solid line). In line with the priority of closing police stations with less than four police officers, the reform substantially decreased the number of these smaller stations. After the reform, however, there were still stations with less than four assigned officers. These stations remained because of special local circumstances, distance to the other police stations, or differences in local crime levels (Steinmauern Gemeindeanzeiger, 2021).

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 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)[13], increase the presence of police forces at locations with higher crime incidence, and improve professionalism. Importantly, the number of police officers did not change in the course of the reform. Panel B of Fig. 2 shows police officer employment figures for regions affected by closures and those that were not. 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. Treated precincts are precincts with at least one station closure during the event window. For both types of precincts, we observe slightly negative but parallel trends, indicating that the reform kept the number of officers constant. Thus, the reform did not lead to employment losses from affected stations but rather to employment shifts within the same police precinct. This allows us to capture the pure effect of local station availability in our empirical analysis which is not compromised with potentially confounding police layoffs.

Police stations are restricted to the jurisdiction and the catchment area of their superior precincts. Since precincts did not change and closed police stations either merged with other stations within the same precinct or were integrated into the precinct

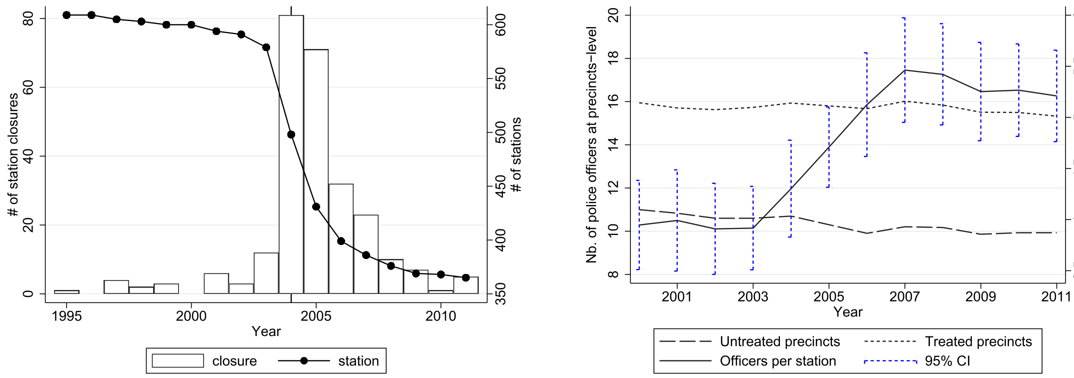
[9] The 180 police stations are located in 178 unique municipalities. In Aalen, we observe two closures in 2004. In Pforzheim, we observe a closure event in 2007 and in 2009. At the municipality level, 57.3% of all municipalities do not have a police station in 2003. 40.3% of all municipalities have one station and 1.35% have more than one police station. By 2011, there are 0.33 police stations per municipality and 72.9% of all municipalities have no police station.

[10] 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).

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

[12] At the municipality level, the 216 changes in the availability of police stations are associated with different numbers of closure events. In 211 cases we observe a reduction of one station from one year to the next. In four municipalities, two stations are closed and in Stuttgart we observe a reduction of three stations between 2003 to 2004.

[13] 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*

Fig. 2. Timing of police station closures and employment 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 261 closing events at the station-level in total which lead to a reduction of police stations at the municipality-year level during 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 between 2000 to 2011. The left y-axis shows the number of police officers for treated (dotted line) and untreated precincts (dashed line) and the solid line (right y-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 linearly for that year.

offices, police officers were merely reassigned to workplaces through station closures. Yet, their responsibilities remained at their old jurisdiction after the reform (Landtag Baden-Wuerttemberg, 2005; Amtsblatt Eichstetten – Eichstetter Nachrichten, 2004).[14] According to information from the Ministry of Interior, affected precincts did not give instructions to their police officers to treat closure municipalities differently with respect to daily routines and patrolling. According to interviews with police men affected by the reform (Reutlinger General-Anzeiger, 2007), police officers still spent about half their time in their old jurisdictions indicating that administrative processing and desk work were now performed in the new office. Hence, these officers still patrolled their previous pre-closure town but had to travel farther from their new assigned workplace to their old jurisdiction. This preserved valuable local knowledge about criminal suspects, residents and the area. Note that despite the absence of explicit instructions of precincts to change policing for the treated municipalities, we explicitly test for related changes in policing within precincts over time in Section 5.3. The transferred officers also offered consultation hours in their old jurisdiction.[15] According to the head of the state police labor 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. This may have led to lower subjective perceptions of safety among local residents as interviews with involved police officers suggest (Reutlinger General-Anzeiger, 2007), a hypothesis which was also brought forward by the aforementioned head of the state police labor union (KA-News, 2009).

[14] Overall, only eight police stations closed across the boundaries of their respective precinct.

[15] The reform was accompanied by other changes to the police organization (Innenministerium Baden-Wuerttemberg, 2012b). 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 h. Importantly, these changes are similar for municipalities with or without police closures and should thus not bias our empirical results on crime effects from station closures.

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-Württemberg 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 complement the station closure information with the date of closure and the type of reorganization, i.e., which stations were integrated into which prevailing stations. We exploit various web-based sources of local 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 1998 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, 2012a).[16]

Data on police station closures is complemented with administrative data on local police employment from the State Ministry of Interior.[17] 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 stations. We also hand-collect detailed background information on the closed police stations themselves based on their addresses, such as their location within the municipality (i.e. being located in town halls, at a market place, or being located in residential areas) as well as their utilization after the closure. Location information will be used in detail to shed light on the mechanisms of our main findings in Section 6.

Crime data. We combine the police station data with rich information on crime from the State Criminal Office (*Landeskriminalamt*) in Baden-Württemberg. Our crime data covers detailed information at the municipal-year level with respect to reported

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

[17] 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.

crime cases and detection rates (see Supplementary Appendix Table A.1 for a description of the crime variables). We focus on property crimes through theft, such as vehicle theft and burglary. Vehicle theft comprises car theft and two-wheel theft (i.e. 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. To complete the picture, we report results on personal theft (e.g. pickpocketing, bag snatching) and robbery (e.g. financial institutions, money transports).[18]

Municipality characteristics. In addition, we exploit municipal-level variation in various socio-demographic characteristics and labor market information (see Panel B of Supplementary Appendix Table A.3). 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 and the share of foreigners in the population from the Statistical Office of Baden-Württemberg.[19] Moreover, we draw administrative information of local labor market indicators from the Institute of Employment Research (IAB), including average real daily wage, the unemployment rate and the share of individuals in active labor market programs.[20] The IAB also provides the local occupational structure for *1*-digit occupations. Both local labor market conditions and the 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–Württemberg, we add similar municipal data from the neighboring state of Hesse as an alternative control group for non-reformer municipalities in Baden-Württemberg. By doing so we add further credibility to our baseline estimates. We also use Hesse municipalities to study potential spillover effects of police station closures on local crime in Baden-Württemberg.

Summary statistics. Table 1 provides summary statistics on various theft crimes across all municipalities over time. Vehicle theft accounts for about 15% of total theft in 2010, whereas burglary and personal theft account for 64% and 18%, respectively. The data suggests that there is substantial variation across all of these dimensions of criminal activity. Overall, Baden-Württemberg is among the states with the lowest crime incidence rates in Germany, with only 5,390 overall reported cases per 100,000 residents in 2016 (Bundesministerium des Inneren, 2017). 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 and robbery. While robbery offences are relatively constant over the time window, car theft already started to decline somewhat earlier.

Panel B of Table 1 reports detection rates for each crime category. On average, about one third of all theft crime cases are detected. The table reports sizeable differences in detection rates across crime categories. Robbery and commercial burglary have the highest detection rates, whereas only about one in ten cases are detected for two-wheel theft. For comparison, violent crimes or crimes against life have relatively high detection rates of 92% and 96%, respectively. Supplementary Appendix Table A.3 further provides monetary damage per crime case. Per crime case, car theft generates the highest damage incurred of more than 8,000 Euros, which increased between 2004 and 2010 by around 2,000 Euros. This is followed by residential burglary with average damage incurred of 2,500 Euros.

4. Empirical strategy

In our empirical strategy we combine matching with a difference-in-differences 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., 2020 for a similar procedure at the regional level and Schmieder et al., 2018 at the individual level). We first present the matching approach and then show the identification strategy for our difference-in-differences model.

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 in means tests for municipalities with a police station closure between 2004 and 2008, and municipalities not used as control units (Panel B) measured before the treatment event. For instance, municipalities experiencing police closures have a different age structure, more high-skilled workers, and a higher share of foreigners compared to other municipalities. More importantly, the development of some variables during the three years before treatment is significantly different between treated and potential control units (column (7)). Hence, a treated municipality may be on a different crime trajectory not only because of the police station closure but also due to other confounding factors such as differential pre-trends in the number of high-skilled.

Thus, we construct a comparable control group by matching a similar control municipality to each treated municipality affected by the police reform starting in 2004. Our baseline matching variables cover municipal demographics (population density, age, skill level, gender, and share of foreigners) and local labor market indicators (average real daily wage, unemployment rate, share of individuals in active labor market programs and the *1*-digit occupational structure). For these variables, matching is done based on the figures reported for one to four years prior to police station closures in order to thoroughly capture differences in levels and, more importantly, in potential pre-trends of the selected confounding variables. We do not match on outcome variables in order to be able to evaluate common pre-trends in criminal activity with an event-study approach.[21]

[18] Other observable theft categories in official crime statistics comprise theft of guns, theft in/from ATMs and theft of antiques/art or religious goods. However, these residual categories only account for about 2% of total theft.

[19] We also collect data on relevant municipal expenditures on public safety and law and order, as they can confound potential changes of crime due to state-level reforms in local policing. Section 7 discusses potential adjustments.

[20] 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-Württemberg and Hesse, respectively. For detailed information see for e.g. Oberschachtsiek et al. (2008).

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

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						
Total theft	232.159	242.965	237.796	201.747	184.361	177.033
	(1,167.443)	(1,235.795)	(1,165.193)	(1,045.694)	(914.702)	(915.504)
Two-wheel theft	29.225	30.559	32.576	31.572	30.047	25.908
	(137.789)	(140.731)	(156.055)	(148.270)	(137.244)	(126.137)
Car theft	3.327	3.320	2.794	2.318	1.435	1.133
	(14.599)	(14.147)	(11.696)	(8.581)	(5.152)	(4.269)
Personal theft	48.251	52.445	49.567	41.425	35.664	33.036
	(312.923)	(360.318)	(326.662)	(287.853)	(229.155)	(218.865)
Robbery	0.744	0.969	0.875	0.751	0.741	0.880
	(3.391)	(4.386)	(4.318)	(3.516)	(4.070)	(4.950)
Residential burglary	12.212	12.111	12.072	9.935	9.715	10.881
	(61.683)	(60.884)	(60.934)	(53.907)	(50.739)	(59.435)
Commercial burglary	135.010	136.942	137.145	113.576	104.586	102.861
	(663.417)	(671.561)	(635.166)	(569.976)	(510.669)	(523.837)
Panel B: detection rates (in %)						
Total theft	0.370	0.365	0.386	0.362	0.364	0.368
	(0.240)	(0.233)	(0.223)	(0.218)	(0.229)	(0.236)
Two-wheel theft	0.108	0.120	0.123	0.118	0.117	0.125
	(0.197)	(0.210)	(0.208)	(0.201)	(0.192)	(0.223)
Car theft	0.415	0.429	0.441	0.461	0.369	0.348
	(0.390)	(0.390)	(0.395)	(0.397)	(0.408)	(0.408)
Personal theft	0.206	0.194	0.221	0.246	0.235	0.234
	(0.230)	(0.232)	(0.237)	(0.260)	(0.257)	(0.255)
Robbery	0.451	0.429	0.485	0.479	0.530	0.536
	(0.433)	(0.420)	(0.432)	(0.426)	(0.438)	(0.432)
Residential burglary	0.163	0.207	0.181	0.159	0.172	0.150
	(0.263)	(0.298)	(0.264)	(0.268)	(0.266)	(0.245)
Commercial burglary	0.550	0.575	0.567	0.517	0.567	0.548
	(0.324)	(0.314)	(0.305)	(0.303)	(0.310)	(0.323)
Panel C: Police information						
# Police station	0.545	0.537	0.453	0.363	0.342	0.335
	(1.158)	(1.124)	(1.008)	(0.922)	(0.879)	(0.847)

Notes: The table reports means and standard deviations in parentheses and refers to the full sample containing all municipality-year pairs (N = 1,103). Panel A shows reported crime cases. Panel B reports the number of detected cases over all crime cases. Panel C reports the number of police stations.

We use a Mahalanobis nearest neighbor matching procedure with replacement to find suitable control municipalities. This algorithm minimizes the standard Euclidean distance of all matching variables. In particular, the 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 (see Fig. 2).

In order to find suitable control units, we impose three restrictions. We first (*i*) restrict the control units to not be direct neighbors and not be located in the same county of the treated municipality. This accounts for the fact that officers patrol in close-by municipalities and presidiums at the county level decide on the closure event. Second (*ii*), we only use municipalities as potential control units if they do not serve as a receiving municipality where officers from closed stations are absorbed. Third (*iii*) and last, we drop treated localities which still possess a police station after a closure event. That is, we focus only on closure municipalities that have no police station after the closure event. This allows us to identify the causal effect of having a police station on criminal activity at the municipal level in a clean way and avoid confounding effects from other remaining stations on local crime rates. We relax these restrictions by showing results on crime outcomes using alternative control group definitions in Section 5.3.

Altogether, this leaves us with 166 municipalities affected by station closures in 2004 or thereafter, and 777 potential control localities. Supplementary Appendix Table B.1 (Panel A) shows the results of the difference in means test for our 166 treated and 166 matched control municipalities measured before the treatment. The table shows the exemplary differences in means for 1 and 3 years before treatment.[22] The matching approach works well in terms of matched control variables. With one exception on the female share, we do not observe differential pre-treatment trends between treated and matched control municipalities at conventional statistical levels (column (7)). The female share decreases by 0.8% points in treated municipalities, whereas it increases slightly by 0.1% points in the control units. Overall, the approach improves group similarity as compared to the development with all other municipalities significantly (column (7) of Panel B). Supplementary Appendix Tables B.2 and B.3 provide information on crime level differences and crime development before the reform, respectively. While we indeed find lower crime levels in closure municipalities in general and, thus, indication that closures effectively aimed at targeting police efforts away from low-crime areas as intended, the development of crime is statistically not different in our matched sample. By contrast, Panel B of Supplementary Appendix Table B.3 provides evidence for diverging trends using all municipalities.[23]

[22] Recent contributions in the difference-in-differences design highlight challenges that arise when there is (*i*) variation in the timing of treatment and (*ii*) heterogeneous treatment effects resulting in possible negative weights (De Chaisemartin and d'Haultfoeuille, 2020). Choosing among a never-treated pool of municipalities limits the extent of negative weights. In fact, in our estimation sample no municipal treatment effect receives negative weights. If we were to restrict the sample of municipalities to ever-treated, 52.9% would receive negative weights in turn.

[23] Supplementary Appendix Figurer B.1 shows the spatial allocation of treated and control units across the state on a year-to-year basis. The map indicates that both, treated and control municipalities are not clustered in certain regions.

4.2. Identification and estimation procedure

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 estimate a difference-in-differences model of the following form:

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

where τ denotes the relative year, e.g. $\tau = -1$ for the year before the treatment, and t actual calender year. $crime_{i\tau t}$ refers to the number of reported crime cases in municipality i, year t, τ periods before or after the reform.

We log-linearize crime outcomes and add values of one to counter the problem of zero values.[24] In the empirical analysis, we use information for the years between 1998 and 2011 and analyze crime development for an event window of seven years before and seven after treatment. Thus, τ takes values between -7 and 7 (for municipalities treated in 2004, the minimum value is -6). The main variable of interest, $closure_i^\tau$, is an indicator equal to 1 for municipalities with a police station closure following the years after the reform (i.e. $\tau \geqslant 0$) and zero otherwise.

We introduce relative year (θ_τ) and calender year (λ_t) effects to ensure that we compare treated and control regions in the same calender year as well as relative before and after the treatment. These fixed-effects (λ_t and θ_τ) are identified because not all closure events occur in the same year but actual reform dates vary across calendar years. For example, at calendar year 2006, τ equals 2 for localities with a police station closure at calendar year 2004, whereas τ equals 1 for localities with a police station closure at calendar year 2005. μ_i represents municipality-fixed effects.

The model further includes a county-level specific trend $\sigma_c t$ to capture differences in time trends at the county level. County-specific time trends should approximate for any common policy changes at the police presidium-level, i.e., the level at which station closures were decided. 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 station closures on crime.

5. Empirical results

5.1. Baseline results

Table 2 shows the difference-in-differences results with respect to reported crime for total theft and the main subcategories. The point estimate for total theft is close to zero and insignificant. 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 18 log points and 12 log points increase in reported crimes, respectively. Other theft categories do not show statistically significant effects at conventional levels.

The documented effects suggest that restricting the local availability of police agencies through closures has differential effects on theft crime. In particular, we observe an increase within categories that are associated with relatively high insurance claims. One way to rationalize the results is that local offenders may be less deterred through a decrease in the perceived risk of detection, which may in turn increase the expected returns from crime for potential offenders. A negative shift in perceptions regarding detection risks in response to police station closures would be consistent with recent survey evidence suggesting that residents in places that contract police services from other municipalities tend to be less confident in the local police and its ability to clear crime (Chermak and Wilson, 2020). We discuss potential mechanisms in Section 6.

5.2. Pre-trends and timing of treatment effects

Thus far, the results represent average effects of the reform and neglect dynamic treatment effects. We extend the previous model by estimating treatment effects before and after police closures. We estimate the following event-study model:

$$log(crime_{i\tau t}) = \sum_{\tau=-4, \tau\neq-1}^{6} \beta_\tau closure_i^\tau + \mu_i + \lambda_t + \theta_\tau + \sigma_c t + \epsilon_{it} \quad (2)$$

where we bin the closure indicator at the endpoints, as is standard in modern event-study applications (e.g. Fuest et al., 2018; Schmidheiny and Siegloch, 2019). We bin the closure indicator at event dates of -4 and 6 and normalize coefficients to event time $\tau = -1$. The event design allows us to assess common pre-trends directly and to test whether the effects differ by post-reform years. Since we do not match on outcome variables, we can assess the plausibility of this notion by comparing pre-treatment trends in outcomes for treated and untreated municipalities. Specifically, we test whether β_τ for $\tau < 0$ differs from zero.

Fig. 3 shows the results of the event-studies for car theft and residential burglary. The remaining crime categories are delegated to Supplementary Appendix Figure C.1. Importantly, all event-study plots show similar crime developments before the closure event in the respective theft category. This provides us with confidence that we can interpret our findings as causal estimates.

Moreover, estimating year-specific treatment effects of police station closures in Eq. (2) allows us to measure short- and medium-run effects after police station closures. 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 weak assumptions. We identify a causal effect of police station closures as long as the unobserved selection bias (beyond our matching variables) does not change over time. Fig. 3 shows an immediate and permanent increase in car theft and residential burglary after police station closures.

5.3. 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 on the robustness of the matching procedure. In particular, we match on the same variables as before and additionally condition on a dummy variable equal to *1* if the municipality possesses one (or more) police station(s) immediately before the reform in 2003. This leaves us with 187 potential control units for our matching procedure. We further match on total theft crime rates in addition to the covariates used before. The next two robustness checks relate to the transformation of the outcome variable. First, we use crime rates per 100,000 inhabitants as alternative outcome variables. Second, we use the inverse hyperbolic sine transformation as an alternative way to account for zero values in our crime outcomes. Finally, we follow Cengiz et al. (2019) and use a staggered difference-in-differences approach over time with an unmatched sample. Using a staggered difference-in-differences approach is the basic intuition in Callaway and SantAnna (2020)'s methodological contribution (see Baker et al., 2021 for a comparison of recent contributions in the field). In our application, we observe treatments in five con-

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

Table 2
Baseline results, reported theft crime.

	Total theft (1)	Two-wheel theft (2)	Car theft (3)	Personal theft (4)	Robbery (5)	Residential burglary (6)	Commercial burglary (7)
Reform	−0.012	−0.029	0.181***	−0.037	−0.047	0.121***	0.027
	(0.031)	(0.037)	(0.045)	(0.042)	(0.035)	(0.044)	(0.040)
Observations	4,508	4,508	4,508	4,508	4,508	4,508	4,508
Municipalities	332	332	332	332	332	332	332
Average	224.03	32.52	2.97	38.37	.89	11.63	133.52

Notes: The table reports difference-in-differences estimation results for log reported theft crime. It presents the effects of police station closures on overall theft as well as the main theft sub- categories. Standard errors in brackets are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% +.

Table 3
Robustness of regression results, reported theft crime.

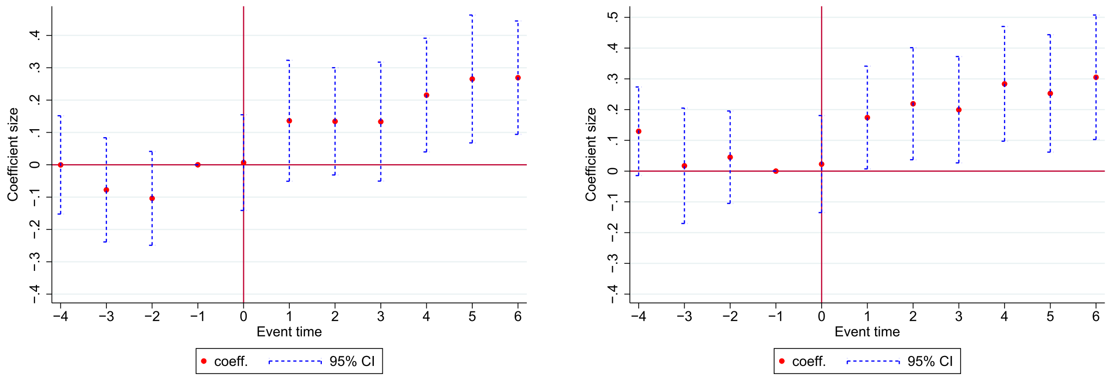
	Total theft (1)	Two-wheel theft (2)	Car theft (3)	Personal theft (4)	Robbery (5)	Residential burglary (6)	Commercial burglary (7)
Panel A: empirical specification							
Police station in 2003	−0.012	−0.046	0.225***	0.003	0.012	0.176***	−0.009
	(0.032)	(0.040)	(0.045)	(0.041)	(0.035)	(0.048)	(0.042)
Municipalities	332	332	332	332	332	332	332
Average	176.98	29.65	2.41	31.33	.73	9.94	99.22
Matching on total theft crime	−0.028	−0.071+	0.160***	−0.058	−0.030	0.147***	0.004
	(0.026)	(0.038)	(0.047)	(0.041)	(0.034)	(0.044)	(0.037)
Municipalities	332	332	332	332	332	332	332
Average	188.23	30	2.54	32.57	.820	10.26	108.56
Crime per 100,000 inhabitants	20.093	−8.401	5.400***	21.508+	−0.230	15.975***	−19.401
	(36.251)	(7.471)	(1.620)	(11.229)	(0.606)	(4.985)	(30.374)
Municipalities	332	332	332	332	332	332	332
Average	1133.96	189.85	18.69	234.56	5.21	76.18	582.24
Inverse hyperbolic sine	−0.018	−0.029	0.215***	−0.067	−0.065+	0.135***	0.012
	(0.029)	(0.041)	(0.050)	(0.045)	(0.035)	(0.050)	(0.039)
Municipalities	332	332	332	332	332	332	332
Average	224.03	32.52	2.97	38.37	.89	11.63	133.52
Staggered differences	−0.034	−0.057+	0.059**	−0.038	0.013	0.103***	−0.054
	(0.025)	(0.030)	(0.030)	(0.033)	(0.018)	(0.037)	(0.033)
Municipalities	691	691	691	691	691	691	691
Average	145.23	24.08	1.93	25.89	0.60	7.93	82.24
Panel B: control for # officers at precinct level							
Reform	−0.012	−0.046	0.226***	0.004	0.012	0.176***	−0.008
	(0.032)	(0.040)	(0.045)	(0.041)	(0.035)	(0.048)	(0.042)
Municipalities	332	332	332	332	332	332	332
Average	176.98	29.65	2.41	31.33	.73	9.16	91.87
Panel C: precinct-specific time trend (exclude mover in matching)							
Reform	0.031	−0.037	0.206***	0.049	0.017	0.187***	0.046
	(0.031)	(0.044)	(0.050)	(0.041)	(0.040)	(0.053)	(0.044)
Municipalities	316	316	316	316	316	316	316
Average	176	29.49	2.41	31.28	.73	9.85	98.59

Notes: The table reports difference-in-differences estimation results for various empirical specifications. Panel A reports the results of the reform on reported crime for different control groups, matching procedures and manipulation of the outcome variable. The staggered differences specification in Panel A has 35,768 observations. 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 trends, allowing for a deviation from the time trend when the precinct is first treated. Eight municipalities change their administrative assignment. Panel C performs the matching without these eight municipalities in the treatment group and also has eight places less in the control group. The control group in Panels B and C comprises municipalities that have one or more police stations prior to the reform. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% +.

secutive years, thus, we stack five years and run the specification with time-varying control variables that we have used in the baseline matching approach. Importantly, we saturate the municipality and time-fixed effects with indicators.[25]

Panel A of Table 3 illustrates the respective results for these sensitivity checks for our baseline theft categories. Results are highly robust to all sensitivity checks. Hence, we find robust evidence that restricting the availability of visible local police infrastructure through police station closures increases crime rates but the effects appear to be specific to car theft and residential burglary.

[25] Similar as above, we exclude municipalities from the control group following restriction (*i*) to (*iii*) described in Section 4.1. The remaining set of potential control municipalities are very heterogeneous. This is in particular relevant with respect to population size of the municipalities. Our reform municipalities have on average 8,232 inhabitants at the time of the reform. In 2004, the average population size of the control group is 6,398. The 10th percentile of the control group sample has 1,020 inhabitants, whereas the 10th percentile among the treated is 3,150. This is a particular problem because small municipalities increase the likelihood of large outliers just by chance (Kahneman, 2011, page 109ff). For this reason, we trim the sample and exclude municipalities below the 3rd percentile of the population distribution among the treatment group. This excludes 241 small municipalities (among them 4 treated municipalities).



Car theft *Residential burglary*

Fig. 3. Treatment effect of police closure on theft crime categories. *Notes:* The figure reports event study estimation results for car theft and residential burglary. The figure provides point estimates and 95% confidence intervals by event time. Estimates are normalized to the pre-treatment year. Standard errors are heteroscedasticity robust and clustered at the municipality level. Number of treated municipalities: 166. Number of matched control municipalities: 166.

Accounting for police employment and different police behavior. Our baseline effects may also be confounded by changes in local police employment as well as by changes in local police strategies over time. Recall that according to Fig. 2 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. Explicitly controlling for police employment does not change the main findings.

Moreover, our findings may also be explained by different policing strategies in the wake of station closures. While we lack more detailed data on the intensity of local patrols (e.g. from patrolling transcripts), we argue that changes in police behavior are unlikely to drive our main results. We now provide several pieces of supporting evidence for that. First, recall from Section 2.2 that local police stations are direct subordinate units of their precincts. The vast majority of station closures was implemented within the boundaries of individual precincts such that the respective chain of command, and accordingly, police strategies did in all likelihood not change. Moreover, the Ministry of Interior of the state did not give instructions to precincts and their respective police officers to treat closure municipalities differently with respect to daily routines and patrolling. Thus, affected officers which were assigned to new office desks at a new station usually still patrolled their old jurisdiction. However, there may be still some scope for behavioral adjustments of police forces due to the reform process. We now explicitly test for related changes in policing within precincts over time by allowing for precinct-specific linear time trends. Specifically, we allow the precinct-specific time trend to change after the first municipality within the precinct is experiencing a station closure.

Panel C of Table 3 provides the respective results. In order to include precinct-specific time trends, this specification drops eight treated and control municipalities from our matching procedure which had police stations that were integrated into stations of another precinct. Allowing for precinct-specific time trends, we do not find evidence that different responses of precincts to the reform change our main estimates of station closures on local property crimes. Hence, we argue that our baseline findings are robust to potential changes in local police strategies.

Inference. Standard errors in our baseline results are clustered at the municipality level. We choose this as default because the variation of the treatment variable is at this level and the control group has duplicates of certain municipalities due to nearest neighbor matching with replacement. We further provide evidence by using two different clustering approaches. The decision of the police station closure is taken at the presidium level, which corresponds to the administrative area of the county. Clustering at this level yields similar standard errors. Eight municipalities change the jurisdiction because the police station got integrated into another station outside of their precinct area. Two-way clustering at the municipal-precinct level also does not affect inference. Alternatively, we conduct randomization inference in order to overcome potential imprecision problems (Young, 2019). Specifically, we follow Fouka and Voth (2016) and perform 2,999 random permutations clustered at the county level of the dependent variable within the sample of matched treated and control municipalities and re-estimate model (1) for each permutation. This approach reshuffles the dependent variable over the observation window and randomly assigns an outcome to each municipality-year pair. To calculate p-values, we combine these with the non-permutated estimates. Supplementary Appendix Table C.1 shows that inference is robust across the different tests. Supplementary Appendix Figure C.2 shows the distribution of estimated coefficients and, for comparison, the treatment effect coefficient from the baseline specification. A further test relates to the fact that we provide results of the reform on a number of theft crime categories using the same sample. As a means of multiple testing and multiple comparison, we make use of a system of equations, which allows for a dependence structure that is captured by a correlation of the error terms (see Supplementary Appendix Tables C.2 and C.3). The results are robust and do not change significantly after imposing a correlation structure in the disturbance term.

Evidence from suspect statistics. We also 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 across various demographic characteristics. Note that legal suspects are not necessarily criminals.[26] We exploit the number of suspects for a specific type of theft as an alternative measure of related

[26] 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). A suspect is not necessarily a convict. The suspect statistics have the advantage of no double counting. If a person, for example, steals a car and a bicycle, he or she would be counted for each category but only once for all theft crimes.

criminal activity. Moreover, using demographic information of suspects we can additionally gain insights on what type of offenders likely react to the reform.[27] Supplementary Appendix Table C.4 reports the results across theft categories. Panel A shows the results for all suspects. In line with the findings on reported cases in our baseline estimates, we find that the number of 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 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, the effects on burglary are driven by younger age groups.

5.4. Spillover effects

The main threat to the identification of the effects from the reform on crime comes from potential spillover or displacement effects.[28] Spillover effects in the crime literature have been recently documented by Blattman et al. (2017) who study police patrols and public services as well as by Maheshri and Mastrobuoni (2020) who study spillovers of bank robberies in the context of private security guard hirings and firings from guarded to unguarded banks. 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 find, 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 the regional availability of police agencies.

Such effects can affect our results in two important ways. On the one hand, if the criminal activity in a treated municipality is driven by criminals who move into the treated municipality from surrounding areas, our reform effects would be upward biased. On the other hand, the reform could generate higher criminal activity not only in treated municipalities but instead spill over to nearby areas. In this situation, our baseline effects are downward biased. The reason for this is that, even with our approach on finding suitable control municipalities that are not direct neighbors and not in the same county, control municipalities might be partially treated.[29]

In order to identify a potential bias in our estimates that is driven by spillovers, we propose the following empirical strategy. Specifically, we utilize data from the Federal State of Hesse to construct an alternative control group. The downside of control units from Hesse is that we have no information on personal theft and robberies. To ensure that crime rates in Hesse municipalities are not affected by the reform, we exclude Hesse municipalities at the adjacent border to Baden-Württemberg in a 60 km buffer (see Supplementary Appendix Figure C.3 for a graphical visualization of the buffer zone of 60 km to the border of Baden-Württemberg).

In order to find suitable control municipalities, we perform the same matching procedure as in our baseline. Supplementary Appendix Table C.5 shows the matching procedure with municipalities from Hesse. The first year where we observe crime rates in Hesse is 2001. Thus, we match on municipal-level observables in $\tau-1$ to $\tau-3$. Similar to Eq. (2), we specify the empirical model as:

$$log(crime_{i\tau t}) = \sum_{\tau=-3,\tau\neq-1}^{6} \beta_\tau closure_i^\tau + \mu_i + \lambda_t + \theta_\tau + \epsilon_{it} \tag{3}$$

This model does not include a county-specific time trend because treated and control units are located in different states. Using this sample, we analyze neighborhood crime rates of municipalities $j \neq i$ of each treated municipality i. Fig. 4 visualizes the setting by zooming into one example of a station closure in the North-West of the *Ostalbkreis* (blue line). Municipalities in grey represent direct neighbors to the treated municipality in the middle of the two circles. In our baseline neighborhood specification, we use a contingent neighborhood matrix of direct neighbors which corresponds to an average radius of 10 km. Increasing the radius to, for example, 15 km (larger circle) would already cover municipalities that are closer to other treated municipalities and in many cases have other treated municipalities as neighbors. With this approach, we minimize the situation that neighboring municipalities are themselves treated.[30] The outcome variable becomes $log(\sum_j crime_j)$ (sum of crime in the grey area). In the neighborhood crime specification, we additionally control for observable municipality-level characteristics (matching variables) of the neighborhood to account for the fact that neighbors j might be different compared to treated municipalities i.

Fig. 5 presents the results for both the baseline specification using Hesse municipalities as controls as well as the neighborhood specification. The baseline effect on car theft turns out to be significantly stronger compared to Fig. 3 with point estimates between 0.3 to 0.7 log points two to six years after the reform. Residential burglary increases up to four years after the reform and turns insignificant thereafter. Thus, using municipalities from Hesse as control units overall confirms the documented effects. In grey, we provide the effect of the reform on neighborhood crime rates. For car theft, we observe positive effects of the reform also in the direct neighborhood, whereas effects on residential burglary in the neighborhood are less clear and turn negative towards the end of the observation window.

One way to think about the effects of the reform on car theft presented in Section 5.2 is that matched controls are partly treated which induces a downward bias. For residential burglary, however, the effects five and six years after the reform are likely to be upward biased because neighborhood crime rates are decreasing. Supplementary Appendix Figure C.4 provides the results for two-wheel theft and commercial burglary indicating no major reallocation or spillover effects.[31]

[27] 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 directly test whether criminals were mobile.

[28] See, for example, Glaeser and Gottlieb (2008), Ehrlich and Seidel (2018) and Falck et al. (2019) for spillover effects of place-based policy interventions.

[29] The median distance between the treated municipalities and its matched control unit is 102 km. Estimating the distance to the closest control municipalities in the sample shows a median distance equal to 10 km.

[30] Supplementary Appendix Table C.6 provides descriptive statistics on crime rates, population size, treatment information, and the number of neighboring municipalities for different definitions of the neighborhood. For instance, the table shows that, on average, a treated municipality has 5.86 neighboring municipalities. Moreover, treated municipalities have, on average, 0.83 other treated municipalities among their direct neighbors. To be more precise, about 44% have no other treated municipality as a neighbor, 35% have 1 and 18% have 2 others treated as a neighbor. There is even one municipality with 4 treated neighbors. Defining the neighborhood area to be, for example, within a 15 km radius (see larger circle in Fig. 4), shows that each treated municipality has, on average, 3.48 other treated municipalities in the neighborhood area (and 8.13% have no other treated as a neighbor).

[31] Supplementary Appendix Table C.7 provides average effects on neighborhood crime rates for different definitions of the neighborhood. Changing the definition of the neighborhood (i.e. labeling municipalities as neighbors if the distance is within a certain radius) shows that spillover effects, in particular for car theft, where we have strong indications of such effects, disappear with larger neighborhood definitions.

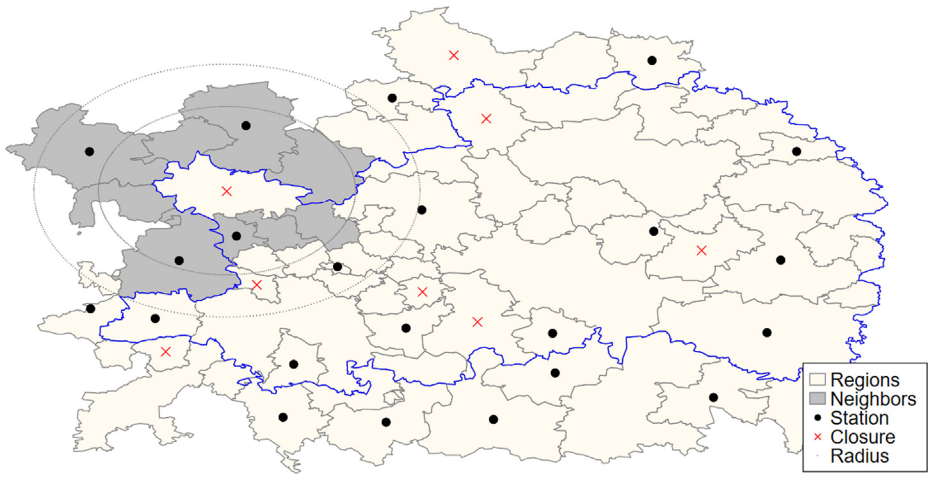
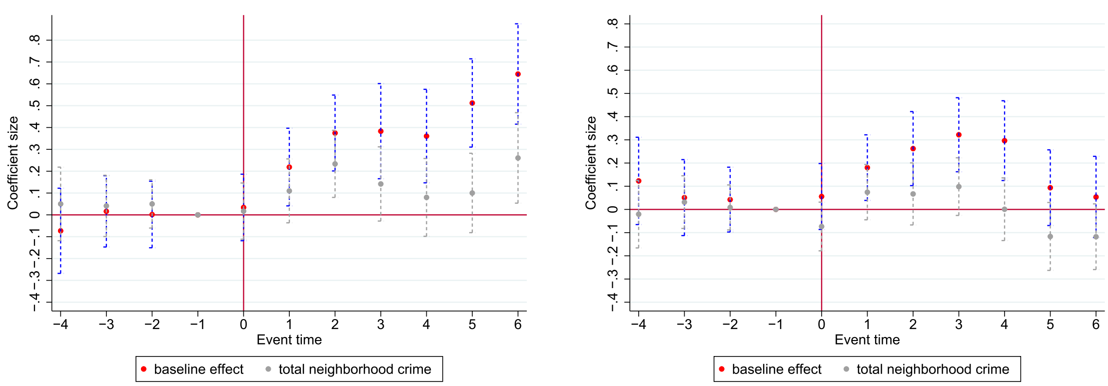


Fig. 4. Stations closure and the regional neighborhood. *Notes:* The figure shows for the example of the county of *Ostalbkreis* (blue line) treated regions with stations closures and all direct neighboring municipalities. Black dots indicate police stations, whereas red X'ses indicate the closure of the police stations. All dots and X'ses correspond to the centroid of the municipality. The circles provide different distances around the centroid of the municipality. The smaller circle shows a 10 km radius. The larger circle shows a 15 km radius. The grey areas in the top left corner represent all direct neighbors of the treated municipality in the middle of the circles. The administrative boundary refers to the year 2016.



Car theft *Residential burglary*

Fig. 5. Effect of police station closure on theft crime, control group Hesse. *Notes:* The figure reports event study estimation results using matched control municipalities from Hesse. The figure provides point estimates and 95% confidence intervals by event time. Point estimates in red represent the direct effect of the reform in crime rates. Point estimates in grey are based on total crime rates in all directly adjacent neighborhood municipalities $j \neq i$ of treated municipalities *i*. This specification additionally controls for average observable characteristics of the neighborhood of treated municipalities. Observable characteristics of the control municipalities in Hesse are measured at the municipality level *i*. All regressions are weighted by the local population size. For the spillover specification, the weighting corresponds to the sum of the population in the neighboring area. Standard errors are heteroscedasticity robust and clustered at the municipality level. Number of treated municipalities: 166. Number of matched control municipalities: 166.

6. Mechanisms

This section addresses the underlying mechanisms for our main results. Police station closures in our set-up introduce quasi-experimental variation in the regional availability of local police agencies which are a visible reassurance of policing for local residents. Thus, we argue that local police agencies themselves represent a relevant parameter for the expected value of getting caught and change the expected benefits of crime (Becker, 1968). Our main results imply that restricting the local availability of stations negatively shifts the perceived risk of being caught for potential offenders which leads to an increase in criminal activity as seen for car theft and residential burglary.[32]

[32] Other determinants of criminal activity in the Becker model are unlikely to change in the given context. First, we estimate crime effects for treated and control municipalities which are not affected differently in terms of sentences since they all belong to the same criminal law. The respective legislation was not changed in the course of the reform. Second, earning potentials in legitimate and illegitimate work (e.g. due to price changes of goods which are subject to property crime) are also unlikely to change in response to local agency closures as shown in Supplementary Appendix Figure C.5.

First, we provide evidence on heterogeneous effects of station closures across several characteristics of local police stations that underwent closures: the quality and location of stations. Second, we discuss changes of detection rates across crime categories to rationalize the role of deterrence and incapacitation effects. We discuss other adjustments and treatment mechanisms that could confound our main estimates, including potential responses of municipal spending after police station closures, reform-induced changes in the market of private security firms as well as alternative reorganization aspects in the course of the police reform in Section 7.

6.1. Heterogeneous police station closures

Quality of stations. Recent evidence of Bindler and Hjalmarsson (2021) points to the fact that the quality of police forces matters for crime outcomes. We test whether our baseline effects are driven by closures of relatively effective, high-quality stations. Closing such stations could 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 the effectiveness of local police forces for a given crime category using total theft detection rates during the pre-reform years (results are robust for category-specific detection rates). A difference in means test shows that detection rates prior to the reform do not differ significantly. Among the treated observations, the share of detected cases is 35.8%, whereas 37.1% of crime cases are detected among the control municipalities (p-value: 0.230). Detected crime cases for municipal i at time t are defined as:

$$d_c = \sum_{\forall c \in C} \mathbb{1}(\mathbb{suspect}_c > \mathbb{0}) \tag{4}$$

Thus, a crime case is detected or cleared if the police found at least one suspect for a crime case of crime category c. The detection rate is then calculated as $dr_c = d_c / \sum crime_c$.

Fig. 6 shows graphically (i.e. via a coefficient plot) that closures of stations which were in the upper half of the quality distribution (high effectiveness) significantly increases car theft by an additional 15 log points (total reform effect is equal to 31.1 log points). The effects on residential burglary of high and low effective stations do not significantly differ from each other. This result corroborates with the finding of Bindler and Hjalmarsson (2021) that only the creation of efficient police forces reduce crime. Their paper, however, defines 'efficient' forces as sufficiently large in manpower. Our results suggest that the quality of local police forces explains a large part of the positive effect of police station closures on reported car theft. Supplementary Appendix Table C.8 provides the respective results of the interaction model for all other crime categories.[33]

Police station location. Next we turn to location-specific treatment effects. Location characteristics of treated stations could matter both for the salience of the reform process in the eyes of the local population as well as for potential offenders. The specific locations of the closed police offices in our sample are shown in Panel A of Supplementary Appendix Table A.2. It shows that 28% of all stations were located in the city hall of the municipalities and 9% were situated at the local market place. In total, about 37% were in the town center. The majority of 55.6% of all closed police stations were located in areas characterized by residential housing.[34]

Fig. 6 provides heterogeneous effects with respect to the specific location information of the police station at the municipal level. Specifically, it shows the respective results for closures of stations that were located in residential areas as well as the regional center of the respective municipality. We find that station closures in residential areas are driving the effects on residential burglary, whereas the closure of stations in the regional center does not cause an increase.[35] Note that we do not find significant interaction effects regarding location characteristics for other theft categories (see Panel B of Supplementary Appendix Table C.8). Taken together, these results suggest that increased residential burglary is driven by stations in residential neighborhoods. We argue that potential burglars may be more aware of the visibility of their actions in town centers and react more elastically to station closures away from town centers. This may be due to several reasons, among which are a lower intensity of street-lighting (Chalfin et al., 2021), a lower frequency of (pedestrian) witnesses and their "eyes on the street" (Carr and Doleac, 2018) or a likely lower degree of CCTV applications in more remote residential neighborhoods as compared to public spaces in town centers (Priks, 2015) which provide information in the market of crime.

We argue that the fact that both neighborhood and station-specific characteristics explain the observed patterns of crime responses to the reform points to heterogeneous effects of police station closures depending on the nature and place of their exact implementation. The effects are consistent with a decreased level of local deterrence and can be rationalized through a decreased perception of detection probabilities specific for car theft and residential burglary.

The specific characteristics of the reform also determine the new incentives available to potential offenders. If closed stations are located in public spaces where criminal actions would be relatively salient and would arguably have more witnesses, potential criminals appear to respond less to a change in police availability. However, burglars respond more elastically in more residential neighborhoods outside the town center which also represent the majority of all station closures. In contrast, the closure of high-quality stations seems to provide an incentive for car theft.

6.2. Changes in detection rates

In order to further disentangle whether our findings are driven by deterrence of local policing or by changes in incapacitation (Chalfin and McCrary, 2017b), we now investigate changes in local detection rates, i.e., the ability of police forces to bring up suspects for crime cases. Our finding might be explained by fewer people being physically prevented from committing a crime. Ideally, we would use individual-level data with information on re-offending/first time offending and arrests/convictions. The rationale to use detection rates at the municipality level to approximate for incapacitation is the following. A decrease in detection rates would indicate that the police is less able to bring up a suspect, and thus, prevents fewer people physically from committing a

[33] Results are robust to alternative definitions of effective stations, for instance, when considering stations ranked above the average or when considering stations in the upper quartile of the pre-reform detection rate distribution.

[34] Panel B of Supplementary Appendix Table A.2 further provides information on the utilization of the respective buildings after the closure event. While only a small fraction of 3% was vacant after the closure, the distribution between public, commercial and private use is rather equal and indicates an immediate transformation and restructuring of the building or police offices. We found no differences in results across various types of post-closure use of station buildings, i.e. public or private use, or the fact that the buildings remained vacant after closure as illustrated in Panel B of Table A.2. Results are available upon request.

[35] In the interaction model, the reference category is the regional center. As shown in Supplementary Appendix Table A.2, 6% of the closed station are either located in commercial areas or on a major federal road. Therefore, each specification controls for the interaction effect with the category 'other'.

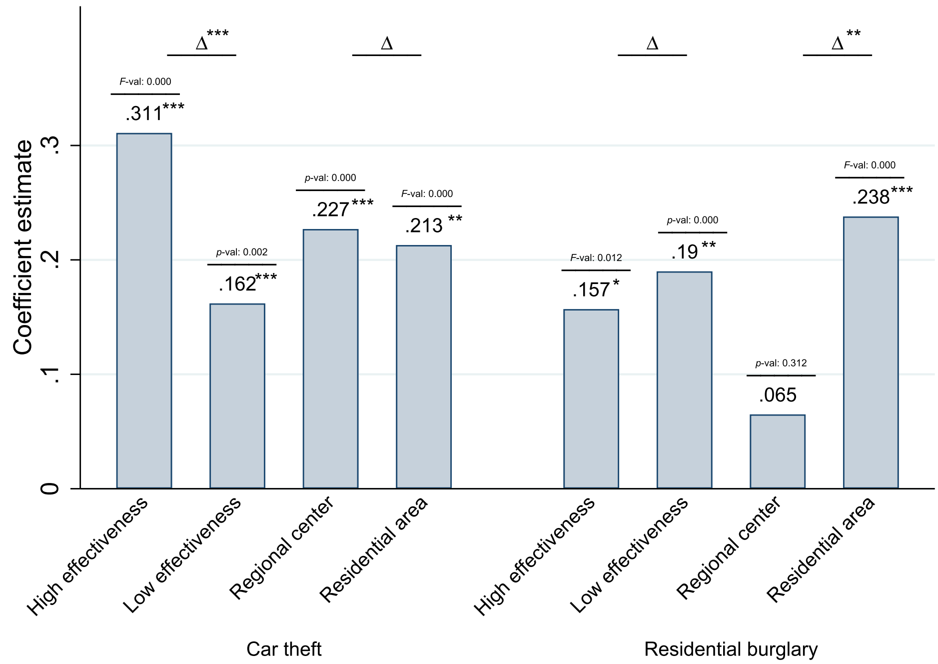
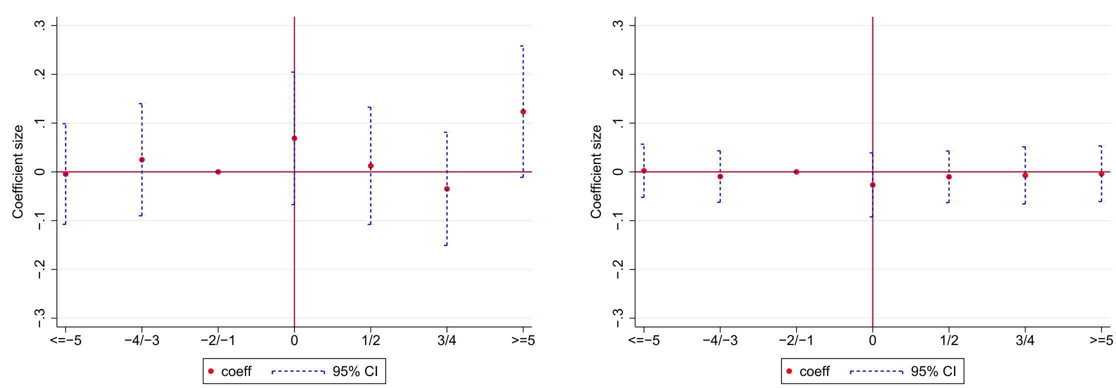


Fig. 6. Regression results by station-specific characteristics, car theft & residential burglary. *Notes:* The figure shows difference-in-differences estimation results for reported car theft (left) and residential burglary (right) using different interaction model specifications. All estimations are based on the sample of municipalities with at least one police station before the reform. The specification on high/low effectiveness interacts the treatment indicator with a dummy variable equal to *1* if pre-reform detection rates are in the upper half of the detection rate distribution and *0* otherwise. The baseline coefficient refers to the treatment effect from municipalities that experience a station closure with low effectiveness. The high effectiveness effect refers to the baseline effect plus the interaction effect. Regional center/residential area interacts the treatment indicator with a dummy variable equal to *1* if the police station was located in a residential area of the municipality and *0* otherwise. The baseline coefficient refers to the treatment effect from municipalities that experience a station closure in a regional center of the municipality (i.e. a town hall and/or market place). The residential area effect refers to the baseline effect plus the interaction effect. Joint *F*-tests for the total effect are performed as $\beta_{reform} + \beta_{interaction} = 0$ and are displayed above the coefficient estimates. The upper Δ shows significance between the baseline effect and the interaction effect. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% *.



Car theft *Residential burglary*

Fig. 7. Effect of police station closure on detection rates. *Notes:* The figure reports event study estimation results for detection rates using the baseline matched control municipalities from Baden-Württemberg. The regression model always pools two years. Estimates are normalized to the two pre-treatment years. The figure provides point estimates and 95% confidence intervals by event time. 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. Number of observations for car theft outcome is 2,980 (329 municipalities). Number of observations for residential burglary is 4,150 (332 municipalities).

crime. This could result in an increase in theft crime. At the same time, however, a lower detection rate would decrease deterrence also for, e.g., individuals who have not been criminal in the first place. Therefore, a decrease in detection rates likely leads to lower incapacitation *and* lower deterrence. Note that we cannot study incapacitation by means of actual incarcerations of criminal offenders but only by means of the detection of suspects by police forces. This is because there are no individual or even local data on conviction rates available in Germany. Detecting suspects for a criminal case, however, is arguably an important legal prerequisite and a necessary condition for clearing crimes and should thus increase the odds of capture and legal punishment.

Fig. 7 along with Supplementary Appendix Figure C.6 show that detection rates do not respond to station closures.[36] This provides

[36] 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 a given year and crime category.

evidence that station closures do not lead to higher detection rates as it could be expected due to the intended gains of professionalization in policing. This result suggests that it is unlikely that fewer criminals are actually taken off the street as the driving mechanism behind the document effects on reported theft crime. In fact, a zero coefficient indicates that the total number of detected cases increase proportionally with the total number of reported cases (recall Eq. 4 for the definition of detection rates). Therefore, the additional crime cases induced by the reform have about the same probability of being detected as the baseline crime cases in the absence of reform. In absolute levels, this effect might even hint to an increase in incapacitation since crime cases increase and the odds of detection remain constant. An increase in incapacitation, however, would mean that the reform mobilized additional offenders from the local population such that more people are being physically prevented relative to the local population. It is not clear what happens at this level if the reform effect on reported theft crime was driven by already active criminals who increased their crime intensity. Due to the lack of conviction data at the individual-level, we cannot estimate this empirically in our set-up. The fact that detection rates do not decrease and incapacitation may even go up, favors the interpretation of deterrence as the main mechanism of our results.

While our main results and station-level mechanisms speak for a permanent decrease in perceived risk of detection due to a restriction of (visible) police infrastructure, we do not find actual levels of detection rates to change. This systematic discrepancy in perceived detection risks complements evidence from Lochner (2007) who finds that criminals update beliefs about subjective probabilities of arrest upward after own convictions in the previous year, but fail to do so based on actual observed local conditions.

Our results, however, point to the fact that criminals seem to adjust their subjective probabilities of being detected depending on a salient change in the provision of local police buildings and especially with regard to their specific station and neighborhood characteristics.

7. Other outcome variables and treatment definitions

7.1. Local governments, the private security market and economic conditions

We established that the reform of police station closures and its heterogeneous implemention in affected municipalities in particular increased car theft and residential burglary without increasing overall theft rates significantly. We now study different margins of potentially confounding behavior that could have changed our treatment effects of police station closures on crime. First, we study how municipal spending on public order changed due to the reform of local police stations which are an explicit part of the state but do not accrue to local municipal budgets. Municipalities can, however, provide some public safety services through the municipal order office (*Ordnungsamt*) which has the duty to avert hazards that are endangering public security. An apparent task of the *Ordnungsamt* is handing out warnings and tickets for petty offenses such as for parking violations but also running patrols.[37] Larger municipalities typically also hire private security firms which may go on patrol, thereby increasing local law and order expenditures.

Second, self-protective measures or private security firms may be an alternative and thus effective way to deter crime (Vollaard and Van Ours, 2011; Maheshri and Mastrobuoni, 2020). However, private security markets may be affected by police station closures themselves. Depending on whether they function as local substitutes or complements to police forces, they may increase or decrease in response to the withdrawal of police forces from closure municipalities. While a higher demand for private security services could alleviate the effects of police station closures on crime, complementary private security services, however, would also retract from closure municipalities and may then even amplify the negative crime effects for closure municipalities.

Municipal spending on public order. This paragraph studies the treatment effects on public order expenditures. We collect information on local public order expenditures at the municipal level that do not relate to state policing but relate instead to public order actions of the municipal government, such as public order offices and regulatory agencies staff.

Column (1) of Table 4 presents the difference-in-differences results. Local public order spending does not change after the police reform. Thus, the results indicate that municipalities do not compensate for any negative effect of the police reform induced by the state government and our baseline effects are not confounded by adjustments in public safety at the municipal level.

Effects on private security firms. According to official information from the state government, private security firms are not viewed as substitutes to the state police and do not act upon governmental but upon private legal contracts (Landtag Baden-Wuerttemberg, 2006). Private security firms also explicitly do not exert duties of the state police. While these statements suggest rather a complementary role of private security firms for police services, the empirical role of private demand for security services with respect to local closures of state police units remains open. We address this question by utilizing geo-coded information on the universe of German private security firms (sector classification 801) as well as of firms for surveillance and alarm systems (sector classification 802) from the Mannheim Enterprise Panel (Bersch et al., 2014) in order to study firm dynamics in these sectors. If police station closures led to a crowding in (out) of private demand for security services, there should be an increase (decrease) of firms in these localities. We are, of course, aware that this is an imperfect measure for the private demand for security measures, given that security firms do not necessarily serve only the municipality in which their headquarters are located. Detailed data on firm dynamics in the private security sector still provides suggestive evidence on expectations of firms in these very local security markets. On average, there are 0.23 private security firms per municipality but only 21% of all places have at least one such firm.

Columns (2) to (3) of Table 4 report the respective results for the likelihood to observe a firm in a municipality-year pair in the market of private security as well as for surveillance and alarm systems, respectively. Columns (4) to (5) illustrate the related effects of police station closures on the likelihood of firm entry in each of these markets. While there are no significant effects of police station closures on the market of surveillance and alarm system products (see columns (3) and (5)), we find that the reform significantly decreases the probability of observing firms for private security services by 10% points. This effect is not driven by lower firm entry (column (4)), indicating that firms are exiting post treatment. Although during our sample period there was no official contract of cooperation of the state of Baden-Württemberg with private security firms, our results seem to suggest that private security firms do cooperate and locate near local police forces. This is in line with the fact that private security services cannot enforce the law themselves but have to contact official police forces in order to apprehend offenders (Landtag Baden-Wuerttemberg, 2006). Specifically, we find that police station closures have a negative effect on local security firm supply. Private security services are

[37] Note that the financial means of municipalities that address public order and safety is typically small with about 5% of overall municipal spending in Germany as of 2010.

Table 4
Regression results analyzing public order expenditures and security firm dynamics.

		Stock of firms		Entry of firms	
	Public order expenditures (1)	Private security services (2)	Surveillance and alarm systems (3)	Private security services (4)	Surveillance and alarm systems (5)
Reform	−0.036	−0.105***	−0.020	−0.005	−0.002
	(0.099)	(0.032)	(0.013)	(0.010)	(0.006)
Observations	4,508	3,320	3,320	4,508	4,508
Municipalities	332	332	332	332	332
Average	358035.01	.21	.03	.03	.01

Notes: The table reports difference-in-differences estimation results for public order expenditures and security firm dynamics. Public order expenditures are measured in log euros. The stock of security firms equals one if there is at least one relevant firm present in a given municipality-year pair. Firm entry of security firms is also set to one if we observe a new security firm in a municipality-year pair. The matched control group consists of municipalities with at least one police station prior to the reform. Fully measuring of the stock of firms starts in 2002, whereas firm entry is fully covered since 1990. Standard errors are heteroscedasticity robust and clustered at the municipality level. Significance levels: 1% ***, 5% ** and 10% +.

therefore not alleviating negative crime effects from police station closures. The presence of fewer private security firms in these municipalities may in fact even amplify related problems from local police station closures of the state.

Local economic conditions. Another adjustment channel through which station closures may affect economic incentives for crimes is related changes in local labor market conditions. If closures led to lower labor market activity or selective out-migration, these confounding or simultaneous factors may explain our observed crime effects. Note that we match on similar conditions prior to treatment, such as socio-demographic characteristics, labor market information, and the occupational structure and their respective development, which have been shown to be similar prior to the treatment. However, labor 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 by testing whether the matching variables change after station closures. Supplementary Appendix Figure C.5 shows the respective point estimates before and after the reform. Out of these variables, we do not observe systematic or large changes post treatment. Overall, socio-economic and labor market adjustments do not explain the baseline effects on theft.

7.2. Receiving municipalities

Finally, the reform not only affected municipalities that lost a police station. We now provide evidence on theft crime outcomes for municipalities that received police officers from the closed police stations. Estimated higher crime rates in closure municipalities could theoretically be compensated for with lower crime statistics in receiver municipalities with enlarged stations after the reform through professionalized and larger offices. The 180 station closure events with an actual reduction in the number of available police stations where integrated into 140 unique municipalities that serve as a receiver for the officers. We focus our analysis on 114 unique receiving municipalities that do not experience either a closure themselves or the creation of a new station. Out of these, six municipalities serve as a receiver in two different years. For the analysis, we focus on the first event and compare crime outcomes before and after this event with matched control municipalities (using the matching algorithm described above) that do have at least one police station in the year before the reform.[38]

Supplementary Appendix Figure C.7 provides the event study estimation results of our alternative treatment, i.e., a municipality which integrated closed stations, on theft crime. Apparently, receiving municipalities do not experience a change in crime rates after the reform. Overall, this supports our argument that not necessarily the number of allocated police officers but the physical presence of a police station may play an important role in explaining local crime.

Taken together, it appears that losing a police station may have unintended negative effects on local property crimes, whereas concentrating police officers from closed stations at larger nearby stations does not improve crime outcomes similarly. Local police agency closures may thus create crime increases which are not compensated for by creating larger stations nearby and, thus, increase specific property crimes overall.

8. Conclusion

Does the regional availability of physical police infrastructure deter criminal behavior? This paper provides novel causal evidence on a permanent reallocation of police forces at the extensive margin by exploiting a quasi-experiment where a reform induced the closure of hundreds of local police agencies and the respective police buildings. We exploit police station closures that did not lead to layoffs but were merely reallocations of the affected local police forces to nearby stations.

We find that station closures do not affect theft in total but significantly increase car theft and residential burglary. Our effects are driven by facility-specific features. For instance, the closure of visible police infrastructure in residential neighborhoods away from town centers is driving the increase in residential burglary. Crimes in these areas are less salient and have likely fewer witnesses. More car theft can be partially explained by the closure of relatively effective police stations. We argue that these results are consistent with a negative shift in perceived detection risks after station closures. We find that the mode of implementation matters greatly for the subsequent incentives of potential offenders. Our findings are unlikely to be driven by actual incapacitation effects, changes of police employment, or different policing strategies at the regional level. Overall, our results are consistent with lower crime deterrence due to a salient closure of local police infrastructure. We also find that enlarged stations receiving additional officers from treated areas do not experience improved crime rates after the reform. Therefore, negative effects of closures on property crimes cannot be counterbalanced by a subsequent concentration of police forces in nearby stations. Permanent regional disinvestments regarding public safety provision and a reduction of visible police infrastructure across localities may therefore ultimately come with negative crime effects and less public safety.

[38] The average receiver municipality has 6.73 neighbors as compared to 5.86 neighbors for closure municipalities indicating that receivers are larger. Moreover, the receiving municipalities are having on average 1.67 treated neighbors; 17 receiving municipalities have no treated neighbor. It indicates that receivers are by design of the reform relatively closely located to treated stations.

Our main estimates can be seen as lower bounds on property crimes since layoffs of local police forces were explicitly not part of the reform.[39] Decreasing manpower in addition to closing stations would likely add to crime and reduce efficiency (Chalfin and McCrary, 2017a). Simple back-of-the-envelope calculations and information on the average size of insurance claims across theft categories allows us to quantify the direct costs of police station closures. Direct costs are moderate but are likely to be permanent. Direct costs amount to about 0.7% 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 overall better detection rates, reform benefits are not obvious, and are unlikely to cover the costs of reform. Section D 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 and facilities in the long run.

Our results are especially informative for restructuring rural police forces since we focus on places losing their last police station. Nonetheless, our findings may provide an interesting starting point for future work in public economics. For instance, one may answer questions related to the optimal organization and employment of police forces under the consideration of deterring effects of a certain (minimum) stock of local police infrastructure. Moreover, it may be also worthwhile studying the role of salience and visibility in the effectiveness of public goods provision in a broader sense. Specifically, what are the political repercussions of the departure from a ubiquitous presence of local public good providers to a more dispersed provision of public services? It remains to be seen if reducing visible efforts of the state to provide local services also undermines public trust towards the state or may even lead to political polarization.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2022.104605.

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[39] Similar reforms also did not change police employment at large, for instance, in Austria (2014), Finland (2017), Scotland (2013), Ireland (2019), Norway (2002 & 2012–2015), New Zealand (since 2009) as well as the canton Aargau in Switzerland (2017). Often, related layoffs are also unlikely in the short-run for sworn officers, given their tenured job contracts (except for early retirement options). Other reforms, however, may involve layoffs in times of budget cuts, as seen in the UK (The Guardian, 2015; The Independent, 2017).

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