Journal of Public Economics 115 (2014) 62–71

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Organizational structure, police activity and crime ☆

Itai Ater [a,1], Yehonatan Givati [b,2], Oren Rigbi [c,*]

[a] The Faculty of Management, Tel Aviv University, Tel Aviv 69978, Israel
[b] Hebrew University Law School, Mt. Scopus, Jerusalem 91905, Israel
[c] Department of Economics, Ben-Gurion University of the Negev, Beer-Sheva 84105, Israel

ARTICLE INFO

Article history:
Received 22 July 2013
Received in revised form 24 March 2014
Accepted 7 April 2014
Available online 28 April 2014

JEL classification:
H10
K14
K40
L30

Keywords:
Organizational structure
Economics of crime
Jails
Arrests

ABSTRACT

How does the organizational structure of law enforcement agencies affect police activity and crime? We examine the consequences of an organizational reform in Israel that transferred the responsibility for housing arrestees from the police to the prison authority. Using the staggered rollout of the reform in different regions of the country, we document strong evidence that this organizational change led to an increase of 11% in the number of arrests and to a decrease of 4% in the number of reported crimes, with these effects concentrated in more minor crimes. The reform also led to a decrease in the quality of arrests, measured by the likelihood of being charged following an arrest. These findings are consistent with the idea that the reform externalized the cost of housing arrestees from the police's perspective, and therefore led the police to increase its activity against crime.

© 2014 Elsevier B.V. All rights reserved.

1. Introduction

To enforce the law and prevent crime, the state must investigate crimes, adjudicate criminal cases, and house criminals upon conviction. These functions are typically undertaken, respectively, by three separate agencies: the police, the court and the prison authority. However, these functions may be organized in a different manner. For instance, in adversarial legal systems the investigative and adjudicative functions are independent of each other, while in inquisitorial legal systems the court is actively involved in investigating facts. Likewise, the investigative function and the function of housing criminal upon conviction may not be independent of each other, as in the case of military prisons, which are often operated by the military police. How do the organizational boundaries between law enforcement agencies affect their activities and crime?

To address this question we investigate the consequences of an organizational reform that transferred the responsibility for housing arrestees from the police to the prison authority in Israel, thereby adjusting the organizational boundaries between the two agencies. Before the reform, arrestees were housed either at local police stations or at regional jails controlled and managed by the police. After the reform, arrestees were no longer housed at police stations, and the control over regional jails was transferred to the prison authority along with the personnel working at these jails.

Theoretically, what should be the consequences of the organizational reform we investigate? We assume that the police serves as an agent of the state, and in this agency relationship the police is incentivized to minimize crime. It does so subject to various constraints it faces, including budgetary and managerial time constraints. The transition of responsibility for arrestees from the police to the prison authority externalizes both the financial and the managerial costs of housing arrestees from the police's perspective. It should therefore result in an increased number arrests. Furthermore, if the police chooses optimally which crimes to pursue, focusing first on more severe crimes and on arrestees that are more likely to be charged, then the additional arrests following the reform should be concentrated in relatively minor crimes and in arrests that are less likely to result in charges. Lastly, the

☆ We are grateful to the editor, Brian Knight, and two anonymous referees for helpful comments which significantly improved our paper. We are also grateful to Ran Abramitzky, Philip Cook, Daniel Chen, Raj Chetty, Liran Einav, Alon Eizenberg, Jonah Gelbach, Mark Gradstein, Oliver Hart, Justin McCrary, Dotan Persitz, Andrei Shleifer, Ron Siegel, Ity Shurtz as well as seminar participants at Harvard Labor lunch, Hebrew University, Tel-Aviv University, University of Pennsylvania Crime, Law & Economics Workshop and Yale Law, Economics & Organization Workshop for helpful comments. We also benefited from discussions with Besora Regev and Shai Amram (Ministry of Public Security), Israel Tal, Ilan Mor (Israeli Police) and Beni Naveh (Israel Prison Authority). Eran Hoffmann and Dror Nachimov provided outstanding research assistance.

* Corresponding author. Tel.: +972 8 6482314.
E-mail addresses: ater@post.tau.ac.il (I. Ater), givati@huji.ac.il (Y. Givati), origbi@bgu.ac.il (O. Rigbi).
[1] Tel.: +972 3 6406319.
[2] Tel.: +972 2 5882509.

http://dx.doi.org/10.1016/j.jpubeco.2014.04.003
0047-2727/© 2014 Elsevier B.V. All rights reserved.

increased police activity should lead to a decrease in crime. This effect should be more significant in crimes that the police more actively pursued after the reform.

The organizational reform we investigate has particular relevance to a reform undertaken in California in October 2011. That reform, known as California's Corrections Realignment Plan, shifted responsibility from the state to counties for the custody, treatment, and supervision of individuals convicted of specified crimes. That reform was in the opposite direction to the reform we investigate, since instead of relieving local police of the responsibility for housing arrestees, the reform in California imposed on local police an additional responsibility for some prisoners.[3]

In our empirical analysis we use individual-level administrative data on the universe of arrests undertaken in Israel, as well as detailed data on the universe of reported crimes. Our empirical strategy relies on two important aspects of the organizational reform. First, the reform can be considered exogenous to police activity and crime because the decision to implement it was a direct consequence of a surprise escape of a notorious serial rapist from the hands of the police. Second, our analysis exploits the staggered rollout of the reform across geographical regions of Israel, starting in April 2007 and ending in January 2008.

The research design and the data we use enable us to identify the effects of the reform on various measures of police activity and crime. We begin by investigating how the reform influenced the number and duration of arrests. Fig. 1 shows the total number of arrestees before and after the control over jails was transferred from the police to the prison authority, using the date of the transition in each region as time zero. The figure indicates that following the reform there was a large increase in the total number of arrestees held in custody each week. Panel data regression estimates further indicate that the increase in the number of arrestees can be decomposed into an 11% increase in the number of arrests and a 38% increase in the duration of arrests.

A central strength of our dataset is that it enables us to investigate the impact of the reform on a quality measure of police activity. We assess quality according to the likelihood of an arrestee being charged. This seems a natural measure of arrest quality, since arrests can be undertaken only when there is probable cause, i.e. a reasonable belief that the suspect has committed a crime. Thus, the likelihood that an arrestee will be charged reflects the threshold level of probable cause that the police sets for undertaking arrests. Our regression estimates imply a reduction of 2 percentage points in the likelihood of an arrestee being charged following the reform. Given the 11% increase in the number of arrests, back-of-the-envelope calculations suggest that individuals arrested after the reform were 20 percentage points less likely to be charged compared with individuals who were arrested before the reform. These findings are consistent with the idea that the police pursued suspects that are less certain to be charged following the reform, and relates to the theoretical literature on the effect of public sector reforms on service quality (Hart et al., 1997).

We also examine the effect of the reform on the severity of crimes for which arrests were undertaken. We do this in two different ways. First, we measure a crime's severity using the maximum possible prison time associated with it. Our regression estimates suggest a reduction of 6% in the average maximum possible sentence of arrestees following the reform. Given the increase of 11% in the number of arrests, back-of-the-envelope calculations suggest that, relative to the original population of arrestees, individuals arrested after the reform were arrested for crimes whose maximum possible sentence was, on average, 60% lower. Second, we look at the composition of arrests, focusing on three categories of crime that account for 80% of arrests: public order, property, and bodily harm. We find that the increased number of arrests was driven by arrests in the public order and property categories of crime, rather than in the more severe category of bodily harm. These findings are consistent with the idea that the police pursued more minor crimes following the reform.

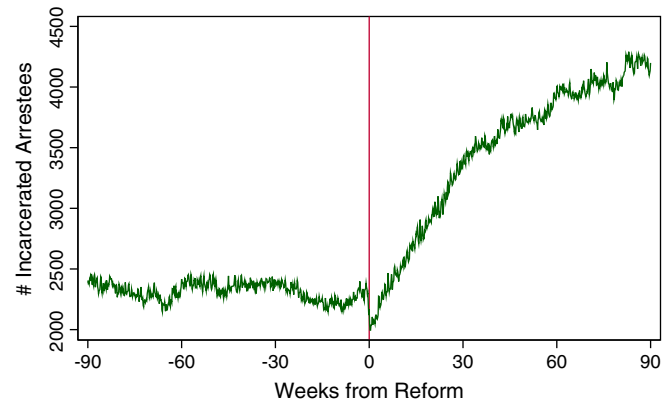

Fig. 1. Organizational reform and number of incarcerated arrestees. The figure plots the daily number of incarcerated arrestees (the stock of arrestees), aggregated over regions. Week zero marks the date of reform implementation in each region. The horizontal axis covers the 90 weeks before and after the reform. Because the reform date varies across regions, the number of incarcerated arrestees on any given day following the reform is the sum of the numbers of arrestees in the different regions on different dates. There is a small drop in the number of arrestees immediately following the transition due to some difficulties in adjusting to the new structure, primarily in the southern region.

Our final analysis examines the impact of the reform on reported crimes. Regression estimates suggest that the reform led to a reduction of 4% in crime. Focusing on the three categories of crime mentioned above, we find that the reform led to a decrease in property and public order crimes, while it had no effect on bodily harm crimes. These findings lend further support to our conjecture that the reform enabled the police to pursue relatively minor crimes, while had little effect on more severe crimes. Interestingly, the reduction in crime that we document is comparable in magnitude to the effect on crime of a 10% increase in police resources, found in other studies (Levitt, 1997; DiTella and Schargrodsky, 2004; Evans and Owens, 2007; Machin and Marie, 2011; Draca et al., 2011).

The theoretical literature on the boundaries of the firm has established that organizational structure has important implications for economic outcomes (Williamson, 1985; Grossman and Hart, 1986). The empirical literature, however, has focused mostly on the determinants of integration decisions, with only few studies examining the effects of vertical integration (see, e.g., Mullainathan and Scharfstein, 2001; Afendulis and Kessler, 2007; Lafontaine and Slade, 2007; Forbes and Lederman, 2010). In their review of the literature on vertical integration, Bresnahan and Levin (2012) write that "in a very few cases, an attempt is made to link the integration decision to economic outcomes". Studying public sector agencies is particularly important because traditional market mechanisms, such as prices and side payments, which can be used to align incentives, are usually not applicable to the public sector.

Following Becker (1968), the literature on the economics of crime has investigated how different factors affect crime, including police (e.g. Levitt, 1997; Klick and Tabarrok, 2005; Draca et al., 2011; Vollard and Hamed, 2012; Chalfin and McCrary, 2013), incarceration (e.g. Levitt, 1996; Drago et al., 2009; Barbarino and Mastrobuoni, forthcoming) and the length of imprisonment (e.g. Lee et al., 2009; Kuziemko, 2011; Abrams, 2012). Our study demonstrates that the organizational structure of law enforcement agencies should also be considered an effective policy instrument in the fight against crime.

The remainder of the paper is organized as follows. Section 2 provides institutional background about the organizational reform, describes the data we use and discusses our empirical strategy. In Section 3 we present our results. In Section 4 we discuss the results and in Section 5 we offer concluding remarks.

[3] For more details on the Californian realignment reform, see http://www.calrealignment.org, The Economist – http://www.economist.com/node/21555611 and The New York Times – http://www.nytimes.com/2011/10/09/us/california-begins-moving-prisoners.html.

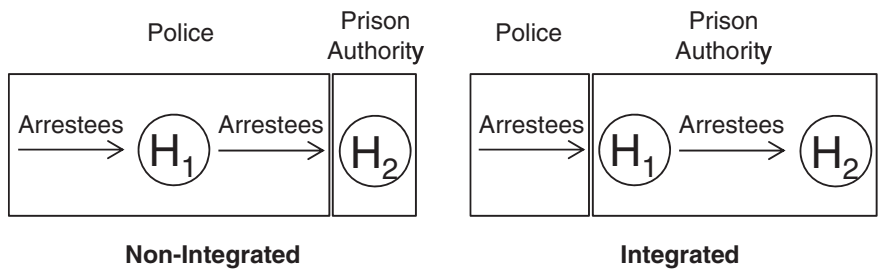

Fig. 2. Non-integrated (left) and integrated (right) organizational structures.

2. Setting, data and empirical strategy

2.1. The reform in Israeli jails

In Israel, the prison authority and the police are independent national agencies operating under the Ministry of Public Security. The main duties of the Israeli police include crime prevention, traffic control and the maintenance of public order. The Israeli police is responsible for investigating virtually all types of crimes, and in most cases police prosecutors decide whether to prosecute a suspect.

According to Israeli law, police officers can detain a suspect for up to 24 h. After 24 h the police must bring the arrestee to court. At that point, if the suspect is not charged and the investigation continues, the police may ask the court to extend the suspect's arrest. The court will do so if it thinks that a freed suspect is likely to interfere with the investigation, escape, or constitute a danger to the public. After the suspect is charged, the police may ask the court that the suspect remain under arrest until the trial is completed. The court approves such a request when the suspect is charged with a severe crime (such as drug trafficking, violent crime, crime punishable with life in prison). The court also approves such requests if it thinks that a freed suspect is likely to interfere with the trial, influence witnesses, or constitute a danger to the public. In some cases, the arrestee is confined to house arrest instead of being sent to jail, or is released on bail.

During the years 2007 and 2008 Israel undertook a large reform in the handling of arrestees and the management of jails. Prior to the reform, the police was responsible for the transportation and the housing of arrestees. Arrestees were detained either in police stations or in jails that the police operated and controlled. The police was also responsible for transporting arrestees from jails to courts and back. When suspects were convicted, they were moved to prisons, controlled by the prison authority. Under the new arrangement, the police was no longer responsible for housing arrestees or transporting them. Jail facilities were handed over "as is" to the prison authority, and arrestees were no longer detained at police stations (except for a few hours). Twice a day, the prison authority's transportation unit would pick up new arrestees from police stations and take them to jails or to courts.

Fig. 2 illustrates the change made by the reform. The reform did not alter the basic process that criminals go through, i.e., being arrested and sent to jail, then upon conviction being sent to prison. What has changed is how the different stages of this process are divided between the police and the prison authority. Before the reform, responsibility for a criminal was transferred from the police to the prison authority only upon conviction. Following the reform, the transfer of responsibility occurs when an arrestee is sent to jail.

As part of the reform, all police personnel working in jails were transferred to employment under the prison authority. Thus, following the reform Israeli police manpower decreased from a total of 28,338 employees to a total of 28,049, reflecting the transition of jailers from the police to the prison authority. Furthermore, the police's budget associated with the management of jails and the handling of arrestees was fully transferred to the prison authority. For each region where the reform took place, the police and the prison authority signed a long contract, detailing precisely the transfer of manpower, budget, facilities and equipment from the police to the prison authority. To illustrate, according to the contract for Israel's southern region, the police committed to transferring to the prison authority 121 employees and the yearly budget associated with their salaries of 19.36 million NIS (New Israeli Shekels), a yearly maintenance budget of 4.35 million NIS, 7 commercial vehicles and their associated yearly operational budget of 0.77 million NIS, and 2 trucks and their associated yearly operational budget of 0.22 million NIS. The contracts went on to describe in extreme detail the equipment in each jail that the police would hand over to the prison authority. For example, the contract for Israel's southern region stated that the following items (among others) would be handed over to the prison authority: 52 guns, 70 mattresses, 170 blankets, 50 pairs of socks, 35 prayer books and one *shofar* (a ram's horn, used on the Jewish holiday of Rosh Hashanah).

What led to this organizational reform? On November 24th 2006 a notorious serial rapist named Benny Sela escaped from police custody while on his way to court. Immediately following his escape a nationwide manhunt was launched, and a committee was appointed to investigate the circumstances leading to it. The committee submitted its recommendations on December 7th 2006, a day before Benny Sela was recaptured. The committee's main recommendation was the transfer of responsibility for jails and arrestees' transportation from the police to the prison authority.[4] The idea was that unlike the police, the prison authority specializes in handling the incarcerated, and therefore if it is responsible for arrestees such an escape will not occur again. That the comparative advantage of the prison authority in handling the incarcerated is the reason for the reform is explicitly stated in Section 1(b) of each of the regional contracts between the police and the prison authority noted above.

The committee also made a recommendation as to the order for the rollout of the reform in the different regions of Israel. This order was determined based on the administrative readiness of the prison authority in each region to accept the new responsibility for arrestees. Importantly, to the best of our knowledge no factor related to police activity was considered in determining the rollout of the reform. The Minister of Public Security adopted the committee's recommendations and the implementation of the reform across Israel was scheduled to take place gradually throughout 2007 and early 2008. The different police regions and the timing of the reform in each region are shown in Fig. 3. As will be further discussed in Section 2.3 our identification strategy relies on this staggered rollout.

2.2. Data

We obtained from the Israeli police full data on every arrest undertaken in Israel between September 2006 and September 2009. These data cover 153,960 arrests and 95,521 arrestees. For each arrest we know the arresting unit, the date of arrest and its duration (i.e. time spent in jail excluding time spent in house arrest). We also observe for

[4] The report (in Hebrew) is available at http://mops.gov.il/Documents/Publications/Reports/YaronCommittee.pdf.

	Region	Date
1	Tel-Aviv Region	04/2007
2	Northern Region	06/2007
3	Southern Region	09/2007
4	Jerusalem Region	11/2007
5	Central Region	01/2008

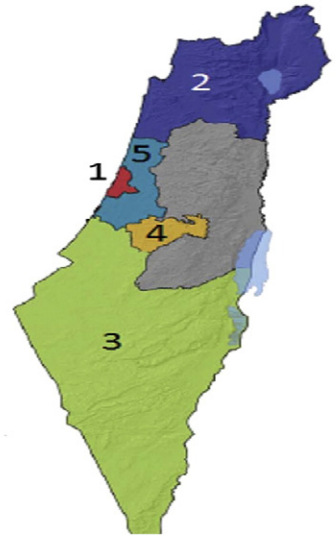

Fig. 3. Police regions and timing of reform. The map represents the five regions of Israel. The table lists the month of reform implementation for each of the regions.

each arrest the specific type of offense that led to it, and the maximum sentence that can be imposed for that offense. Additionally, we know whether the arrestee was charged following the arrest. Lastly, for each arrestee we have demographic information (age, gender, marital status and ethnicity) as well as an anonymous identification number.

In addition to the arrest data we also have full data on each of the nearly 834,000 crimes reported to the police during the same time period. For each crime reported we know the date the complaint was filed, the type of crime, and the location where it was reported. The use of the number of reported crimes as a measure of crime is standard in the economic literature on crime. In Table 1 we present descriptive statistics of the outcome variables, constructed at the week-region level based on the individual level data.

To get a general sense of the effects of the reform on the number of arrests, on the mean duration of arrests and on the number of reported crimes, we calculated, for each region, the number of arrests, the mean duration of arrests (in days), the share of arrestees charged, and the number of reported crimes in the 90 weeks before and after the organizational reform. We then averaged these values across the five regions, using for each region the date of the organizational reform in that region as time zero. The results of the calculation, in 2-week bins, are presented in Fig. 4. The figure shows that the organizational reform led to an increase both in the number of arrests and in their duration, and to a decrease in share of charged arrestees and the number of reported crimes. The effect of the reform can also be graphically seen in Fig. 5, in which we separately plot a time-series of the number of incarcerated arrestees in each region.

2.3. Empirical strategy

We use a standard differences-in-differences research design, exploiting the gradual transfer of responsibility from the police to the prison authority to study the effects of the organizational reform. Our baseline specification is as follows:

$$y_{rt} = \alpha + \beta \times Post_{rt} + \gamma_r + \delta_t + \epsilon_{rt} \tag{1}$$

where y_{rt} is the outcome variable of interest in region r in week t. The dummy $Post_{rt}$ assumes the value one in regions and weeks in which the transfer of control over jails has already taken place. γ_r represents regional fixed effects, which control for time-invariant differences across regions. To account for the volatility of criminal activity we also include δ_t — weekly fixed effects. We also acknowledge the possibility of criminal and police activity trends that may vary between regions by incorporating linear region-specific time trends in some of the specifications. Each observation is weighted by the population size of its corresponding region. Finally, we account for the serial correlation in the outcome variables by clustering the error terms at the region-month level. In the Robustness section we explore alternative methods for deriving the estimates' standard errors.

This specification allows us to estimate the correlation between the implementation of the organizational reform, reflected in the variable $Post_{rt}$, and the outcome variables conditional on time and regional effects. The difference-in-difference approach implies that the impact of the reform is derived by comparing the change over time in the outcome variable in a region that has experienced the reform with the corresponding change in a region that has yet to experience the reform. For this equation to have a causal interpretation, the timing of the organizational reform and the order of the rollout need to be independent of unobservables that directly affect the dependent variables. Indeed, as indicated above, the decision to implement the reform and, hence, its timing were a direct consequence of the surprise escape of a serial rapist in November 2006. Furthermore, the order in which the responsibility over jails was transferred to the prison authority was determined according to the administrative readiness of the prison authority in each region, and not based on factors relating to police activity. In Section 3.6 we also conduct a formal test that validates the independence of outcomes from the order of the rollout of the reform. Thus, we do not believe that the order of the rollout constitutes a threat to the identification.

In what follows, we study the effect of the reform on four groups of outcome variables and separately also conduct the analysis for the main categories of crime:

1. Cost measures for arrests — the number of arrests and the mean arrest duration;
2. Quality of arrests — the share of arrestees charged;

Table 1
Descriptive statistics.

	Mean	S.D.	10P	90P
Number of arrests	192.45	83.77	107	309
Arrest duration (days)	15.85	9.86	6.53	29.5
Maximum sentence (months)	74.45	13.73	59.95	91.92
Share indicted	0.36	0.11	0.24	0.51
Reported crimes	1041.8	326.8	558	1553

All figures are at the week/region level. "Reported crimes" refers to the number of crime files opened by the police.

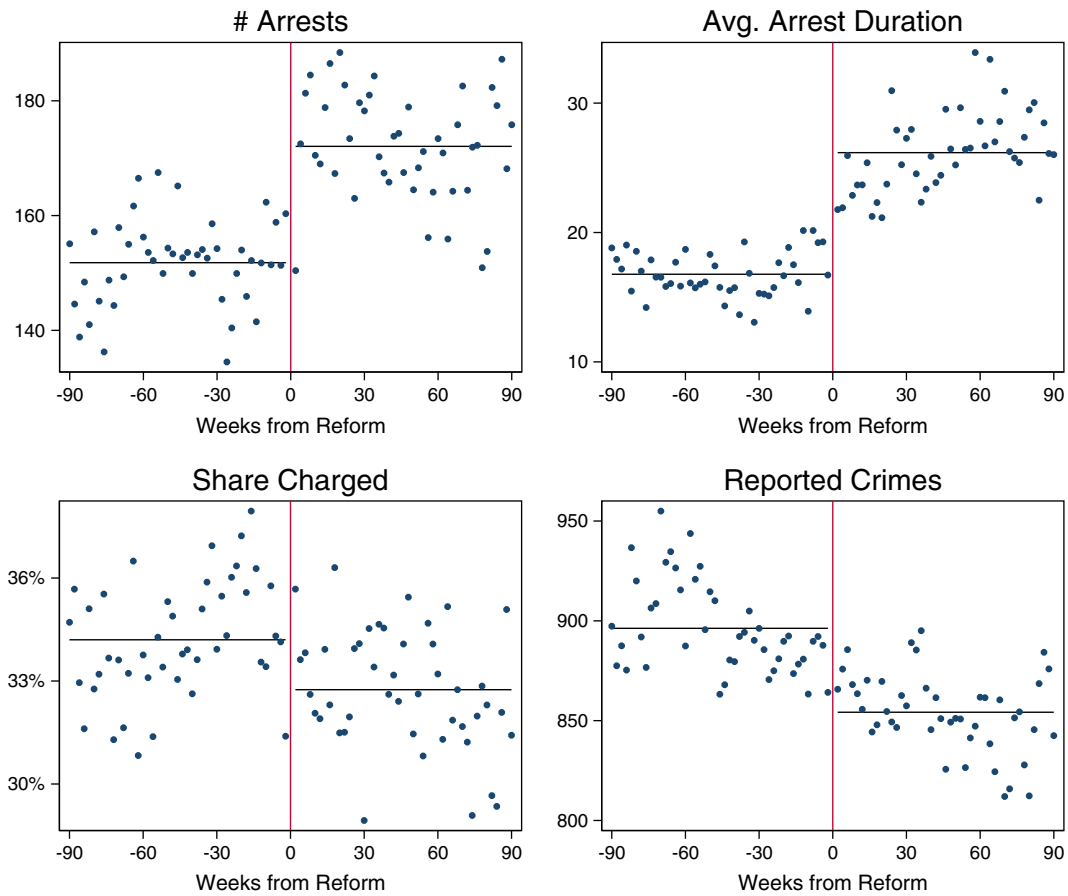

Fig. 4. Organizational reform—number, duration and quality of arrests and reported crimes. The figure plots the number of arrests, the mean arrest duration, the share of charged arrests and the number of reported crimes. Each dot corresponds to a time period of two weeks. Week zero marks the date of reform implementation, for each region. The horizontal axis covers the 90 weeks before and after the reform. Horizontal lines represent the average values over the 90 weeks before or after the reform. Since the reform implementation date varies across regions, each dot aggregates values collected on different dates.

3. Severity of arrests — the average maximum possible sentence associated with the crimes for which arrests were made, and the composition of arrests (categories of crime);
4. Crime — the number of crimes reported to the police.

3. Results

3.1. Number of arrests and mean arrest duration

We argue that the organizational reform externalized the costs of housing and transporting arrests from the police to the prison authority. These costs include both financial and non-financial costs of handling arrestees (e.g., food, gasoline, hassle and managerial time). Thus, we expect that the number and duration of arrests will increase after the reform.[5] As can be seen in Table 2, this prediction is supported by the regression analysis. In columns (1) and (2), we focus on the effect of the reform on the number of arrests in logs, without and with region-specific time trends, respectively. We find that the reform led to an increase of 11.1 or 12.6% in the average number of weekly regional arrests. Columns (3) and (4) consider the effect of the reform on arrest duration in logs, also presented without and with region-specific time trends. Our findings suggest that the reform led to a statistically significant increase of 38.5 or 23.1%, in mean arrest duration.[6]

3.2. Quality of arrests

By law, the police can arrest an individual if at the time of the arrest there is probable cause, i.e., sufficient evidence to indicate that the individual has committed a crime. The police therefore adopts a threshold level of probable cause above which it undertakes an arrest. A natural proxy for the threshold level that the police adopts is the share of arrestees charged — our measure of arrest quality. How should this measure of quality be affected by the reform? Following the reform, the police increased the number of arrests; presumably, they did so by adopting a lower probable cause threshold level. The increased number of arrests following the reform should therefore be concentrated among arrestees that are less certain to be charged, which means we expect to see a smaller share of arrestees being charged.

We estimated Eq. (1) using the fraction of arrests that led to charges being filed in each week and region as the dependent variable. Columns (1) and (2) in Table 3 present the estimation results. We find that the reform led to a decrease of approximately 2 percentage points in the

[5] At the end of an arrest, the suspect is either released or charged.

[6] These findings suggest that following the reform, courts more leniently approved requests for longer arrest periods. In a separate analysis, available upon request, we provide evidence that indeed requests for longer arrest periods are strongly correlated with court decisions to approve such requests.

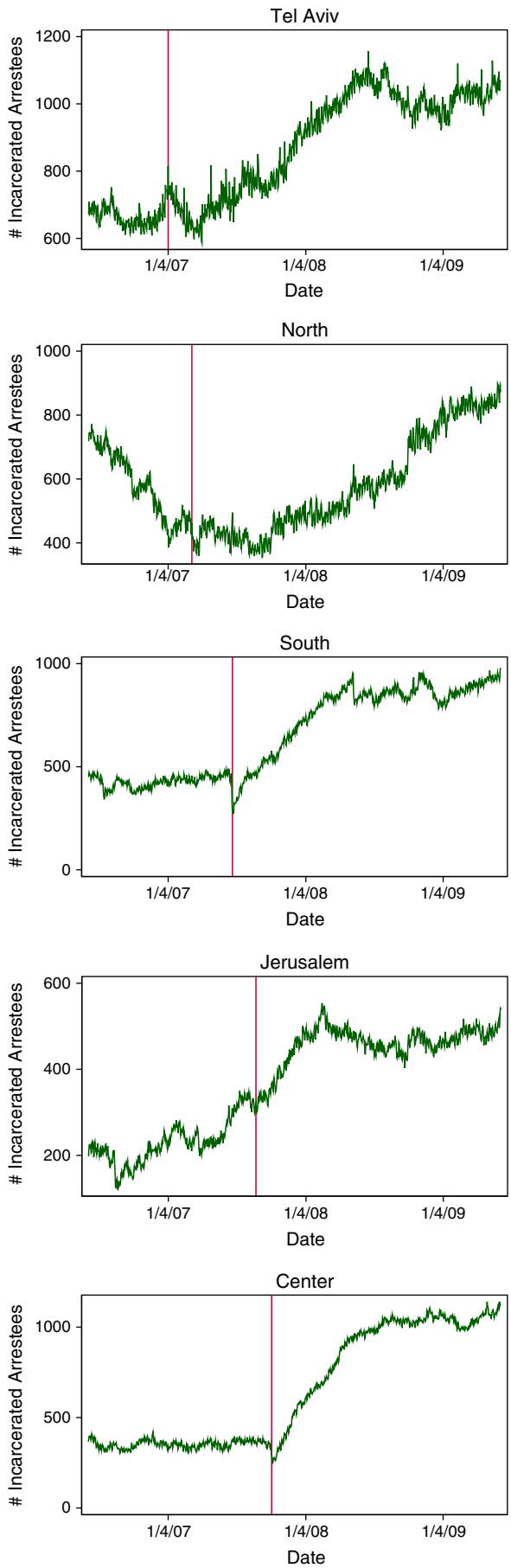

Fig. 5. Number of incarcerated arrestees in different regions. Each figure plots the daily number of incarcerated arrestees in different regions. The vertical lines mark the organizational reform date. The horizontal axis covers the time period of September 2006–September 2009.

Table 2
The effect of the reform on the number and duration of arrests.

Dep. var:	Log (number of arrests)		Log (arrest duration)	
	(1)	(2)	(3)	(4)
Reform	0.111***	0.126***	0.385***	0.231**
	(0.0357)	(0.0349)	(0.104)	(0.102)
Week/region fixed effects	✓	✓	✓	✓
Region-specific time trends	✗	✓	✗	✓
R-squared	0.911	0.919	0.621	0.677
Observations	785	785	785	785

The unit of observation is a region-week cell. The reform variable corresponds to the dummy variable, which assumes the value one in regions and weeks in which the transfer of control over jails from the police to the prison authority has already taken place. The regression includes week and regional fixed effects. Even columns also include region-specific time trends. Observations are weighted by each region's population. Standard errors are robust and clustered by region/month.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

share of arrests leading to charges being filed, our measure for arrest quality. To get a better sense of the magnitude of these estimates, we performed a simple back-of-the-envelope calculation and compared the post-reform population of arrestees with the pre-reform population. Given an 11% increase in the number of arrests, and assuming that the new population of arrestees is the cause of the change in the share of arrestees being charged, the new population of arrestees was 20 percentage points less likely to be charged compared to the original population.

3.3. Severity of offenses and composition of arrests

We also examined whether the reform affected the severity of crimes that the police pursued. Arguably, when the police incurs lower costs of handling arrestees – as would be expected to occur due to the reform – it may choose to pursue types of crimes that it did not pursue previously. In particular, we expect an increase in its activity toward "less important" crimes, which we measure by their severity. We used two approaches to empirically examine this conjecture.

First, we used the maximum sentence (in months) that could be imposed for each offense as a measure of crime severity. Columns (3) and (4) in Table 3 consider the effect of the reform on the average maximum possible sentence for arrestees in logs, without and with region-specific time trends, respectively. We find that the reform led to a decrease of approximately 6%, in the average maximum possible sentence of arrestees. Given an 11% increase in the number of arrests, and assuming that the new population of arrestees is the cause of the change in the average maximum possible sentence, this means that the average maximum possible sentence of the new population of arrestees was 60% lower than that of the original population.

The second approach we used to examine the effect of the reform on the severity of crimes that the police pursued was to distinguish between different categories of crime. We focused on three categories of crimes that accounted for 80% of arrests: public order (34%), property (30%) and bodily harm (15.5%). While public order crimes (e.g. trespassing, disrupting police activity and disturbing the peace, violations of the immigration law) are relatively minor offenses, property crimes (e.g. burglary, robbery, auto theft, "theft from an auto") are more severe. Bodily harm crimes (e.g. murder, assault and aggravated assault) are more severe than the two other categories.

Table 3
The effect of the reform on the share of arrestees charged and maximum sentence.

Dep. var:	Share charged		Log (maximum sentence)	
	(1)	(2)	(3)	(4)
Reform	−0.0195***,*	−0.0278***	−0.0592***	−0.0635***
	(0.00845)	(0.00903)	(0.0219)	(0.0232)
Week/Region Fixed Effects	✓	✓	✓	✓
Region-Specific Time Trends	✗	✓	✗	✓
R-squared	0.826	0.834	0.632	0.633
Observations	785	785	785	785

The unit of observation is a region-week cell. The reform variable corresponds to the dummy variable, which assumes the value one in regions and weeks in which the transfer of control over jails from the police to the prison authority has already taken place. The regression includes week and regional fixed effects. Even columns also include region-specific time trends. Observations are weighted by each region's population. Standard errors are robust and clustered by region/month.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

Table 4 presents estimation results of Eq. (1) for each of the three crime categories. The table shows the effect of the reform on the (log) number of arrests in each category. The results indicate that the reform led to an increase of 13–15.4% in the number of arrests for public order crimes and to an increase of 8.8–13.3% in the number of arrests for property crimes. The effect of the reform on arrests for bodily harm crimes is statistically indistinguishable from zero. These findings are consistent with our conjecture that the reform enabled the police to pursue more minor crimes.

3.4. Crime

Our final set of results examines the effect of the reform on crime rates. We report the results for the crime regressions in Table 5. In columns (1) and (2) we study the effect of the reform on all types of crimes, and in columns (3) to (8) we separately explore its effect on the three crime categories. The regression results suggest that the reform led to an average decrease of 2–4% in overall crime, and that this effect was mostly driven by reduction in public order and property crimes. In particular, we find that the reform led to a decrease of 1.9–4.4% in the number of public order crimes, and a decrease of 3.2–5.9% in the number of property crimes. The effect of the reform on bodily harm crimes is statistically indistinguishable from zero. These findings seem consistent with the results of our earlier analysis that indicated that the reform led to an increase in the number of arrests for public order and property crimes, but not for bodily harm crimes. The magnitude of the reduction in crime that we document is comparable to the effect of a 10% increase in police resources found in other studies on the relationship between police and crime (e.g. Klick and Tabarrok, 2005; Evans and Owens, 2007; Draca et al., 2011).

3.5. Cross-regional differences

While the nature of the organizational reform was identical across regions, its actual impact could have been different. Specifically, we can expect that regions, in which more "managerial" police resources were allocated to managing jails and to the handling of arrestees, experienced a larger change in police activity following the reform. To empirically test this, we construct a reform intensity variable for each region which equals the ratio between the number of beds in jails located in the region and the total number of policemen in that region. Specifically, the range of the reform intensity variable ranges between 7 beds to 100 policemen to 16.5. We replace the reform variable, *Post* by the new variable in observations for which the reform variable has previously assumed the value 1 and then estimate Eq. (1). Table 6 presents the corresponding estimation results for the five outcome variables. The results demonstrate that for all outcome variables, except arrest duration, regions with a larger beds-to-policemen ratio experienced a greater response to the reform. For example, a 0.01 increase in beds-to-policemen ratio is associated with an increase of nearly 1% in the number of arrests. These results can at least partially explain the differences across regions observed in Fig. 5.

3.6. Robustness

In this subsection we present additional results that demonstrate the robustness of our findings (full results are available in the Online Appendix).

First, employing a difference-in-difference approach using panel data may lead to over-rejection of the null hypothesis when outcome variables, such as crime and police activity measures, exhibit serial correlation (Duflo et al., 2004). In the paper we address this concern by clustering the standard errors at the region-month level. In the online appendix, we also show that our results are similar when we calculate the standard errors in alternative ways, such as clustering by region; clustering by region times $Post_{rt}$; using the Moulton correction and wild bootstrap. Second, we collected monthly unemployment data and yearly data on the share of minority groups and the fraction of young men (age 18–25) in each region's population. These variables undergo very little variation over time, so they are nearly fully absorbed

Table 4
The effect on number and duration of arrests — by crime category.

	Log (number of arrests)					
	Public order		Property		Bodily harm	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.130**	0.154***	0.0883**	0.133***	0.0146	−0.0143
	(0.0548)	(0.0533)	(0.0448)	(0.0460)	(0.0491)	(0.0497)
Week/region fixed effects	✓	✓	✓	✓	✓	✓
Region-specific time trends	✗	✓	✗	✓	✗	✓
R-squared	0.854	0.865	0.804	0.815	0.740	0.744
Observations	785	785	785	785	785	785

The unit of observation is a region-week-crime category cell. The reform variable corresponds to the dummy variable, which assumes the value one in regions and weeks in which the transfer of control over jails from the police to the prison authority has already taken place. The regression includes week and regional fixed effects. Even columns also include region-specific time trends. Observations are weighted by each region's population. Standard errors are robust and clustered by region/month.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

Table 5
The effect of the reform on crime.

Dep. Var:	Log (crime)							
	All categories combined		Public order		Property		Bodily harm	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Reform	−0.0399***	−0.0193*	−0.0440***	−0.0189	−0.0589***	−0.0318***	0.0161	−0.00250
	(0.0118)	(0.0103)	(0.0151)	(0.0133)	(0.0134)	(0.0104)	(0.0193)	(0.0195)
Week/Region Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Region-Specific Time Trends	✗	✓	✗	✓	✗	✓	✗	✓
R-squared	0.982	0.986	0.948	0.962	0.978	0.983	0.933	0.936
Observations	785	785	785	785	785	785	785	785

The unit of observation in the first two columns is a region-week cell. The unit of observation in the remaining columns is a region-week-crime cell. The number of reported crimes refers to the number of crime files opened by the police. The reform variable corresponds to the dummy variable, which assumes the value one in regions and weeks in which the transfer of control over jails from the police to the prison authority has already taken place. The regression includes week and regional fixed effects. Even columns also include region-specific time trends. Standard errors are robust and clustered by region/month.
** $p < 0.05$.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

in the regional fixed effects. We verified that our results hold when these variables were included in the analysis. In addition, we verified that our findings are qualitatively unchanged even when we allow the coefficients on these background variables to vary on a yearly basis. Third, given that the analysis was based on five regions only, we verified that the results were not driven by any single region. We did so by re-estimating the effects of the reform using each subset of four regions. The results presented in Table **7** demonstrate the robustness of our findings to the exclusion of any single region from the analysis. Likewise, the results were also qualitatively similar when we added the West Bank region as a sixth region. We excluded this region from the main analysis because the police's *modus operandi* in the West Bank is different from that in the other regions of the country. Fourth, we verified that the results were qualitatively the same when we normalized the dependent variables – number of arrests and number of crimes – by region population size, or alternatively when we assigned equal weights to all the regions instead of weighting them by population size.

Furthermore, we verified that the pre-reform crime rates and police activity measures were not associated with the order of the rollout. If, for example, the order in which the organizational reform was implemented was dictated by region-specific crime time trends, then our estimates might have captured those trends rather than the effect of the reform. To analyze this issue, we conducted a placebo test by considering a sample that started on September 1, 2006 and ended on March 31, 2007, i.e., the day before the reform began to be implemented. We then re-estimated our crime regression, defining a fictitious date for the implementation of the reform in each of the regions. We set a fictitious reform date for the first region in which the reform was implemented (Tel-Aviv). The fictitious reform dates for the remaining regions were set to maintain the order of implementation and the relative difference in the time of implementation between regions. In this way, we reproduced the exercise as if the organizational reform had occurred during the pre-reform period. The results, which show no significant effect of the fictitious reform, are also presented in the Online Appendix. These results validate our empirical approach as they reveal no association between the pre-reform crime dynamics and the order of the organizational reform.

Finally, we tested whether the timing of the change in police activity coincided with the timing of the organizational reform. For this, we conducted a test for a structural break by estimating a series of regressions with fictitious organizational reform dates defined for every month starting from 7 months before the true implementation date of the reform up to 15 months after it. The dependent variable in each regression was the weekly number of arrests. The independent variables were a continuous week variable and its interaction with a variable indicating implementation of the organizational reform. We maintained the order of the reform among regions as well as the time difference between their implementation dates. The structural break date was defined as the date for which the regression R^2 is maximized. We find that the regression R^2 s ranges from 0.032 to 0.077, with the largest R^2 estimated in the regression with the actual implementation dates.

3.6.1. Crime displacement effects

Our results are potentially driven by spatial displacement effects, which imply that criminal activity is diverted from regions in which the reform has been implemented into other areas where the reform has not yet taken place.[7] If spatial displacement did occur, then our estimates for both arrests and crime rates are potentially biased downward. To test for spatial displacement effects, we focused on the 10,827 individuals who were arrested multiple times during the analyzed time frame, and were arrested at least once before April 1, 2007 (the transition date in the first region). We used the information on the first arrest (performed during the pre-reform period) to identify the "home" region of the repeat offender. If spatial location displacement effects are important then, conditional on being arrested again, we expected that the likelihood of being arrested in a different region during the interim period (April 1, 2007 to January 1, 2008) would be greater than the corresponding conditional probability following the completion of the rollout (after January 1, 2008). The idea is that during the interim period, the benefits from diverting efforts to other regions are higher than the benefits of doing so after the full implementation of the reform. Using this approach, however, we do not find evidence for spatial displacement. In fact, conditional on being arrested again, the likelihood of the second arrest being in a different region was higher during the post-rollout period than during the interim period.[8]

[7] A different type of displacement is time displacement, which implies that the criminal activity is postponed until the extra police activity levels off is not relevant to our study because of the non-transient nature of the reform we investigate. In addition, studies that exploited terror attacks to identify the effect of terror on crime (i.e. Draca et al., 2011) emphasized that correlated shocks posed a major concern with regard to identification because terror events have a dislocating impact on the economy and the population. In other words, the concern was that crime rates fell not only due to increased deployment of police forces but also because of other factors (Becker and Rubinstein 2011; Gould and Stecklov, 2009). Given that we study a reform that had little effect on the general public in Israel, and in light of the direct evidence we present on arrests, we believe that this concern is not relevant to our study.

[8] An alternative approach, in which we added a dummy variable equal to one in region r and week t if the reform has yet to be implemented in that region, but has already been implemented in one of region r's adjacent regions, has not supported a displacement effect either. These results are also presented in the Online Appendix.

Table 6
The effect by the reform by its intensity.

Dep. var:	Log (number of arrests)		Log (arrest duration)		Share charged		Log (maximum sentence)		Log (crime)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Reform intensify	0.984***	0.952***	−1.116	0.179	−0.301***	−0.260***	−0.442**	−0.497***	−0.217**	−0.156*
	(0.260)	(0.273)	(1.019)	(0.844)	(0.0740)	(0.0735)	(0.177)	(0.182)	(0.0942)	(0.0825)
Week/region fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region-specific time trends	✗	✓	✗	✓	✗	✓	✗	✓	✗	✓
R-squared	0.912	0.918	0.605	0.670	0.830	0.835	0.631	0.633	0.982	0.986
Observations	785	785	785	785	785	785	785	785	785	785

The unit of observation is a region-week cell. The reform intensity corresponds to a variable that takes the value zero in regions and weeks before the transfer of control over jails from the police to the prison authority. In the remaining regions and weeks this variable assumes the ratio between the number of beds region *r* jails and the total number of policemen in that region in the month before the reform was implemented. The regression includes week and regional fixed effects. Even columns also include region-specific time trends. Observations are weighted by each region's population. Standard errors are robust and clustered by region/month.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

4. Discussion

A question that arises with respect to our findings is whether they are mainly driven by a change that affected the police as an organization (*top-down effect*), or whether the observed patterns were triggered by a behavioral change among individual policemen (*bottom-up effect*). In other words, the larger number of arrests may have been driven either by the lower costs of undertaking an arrest by an individual patrol officer, or alternatively by senior police officers who exploited the reform to improve the performance and efficiency of their units resulting in more arrests.

We think that our findings reflect a top-down effect, for several reasons. First, the analysis presented in Section 3.5 suggests that regions in which more managerial time was allocate to jails exhibited a larger effect on police activity. Second, the fact that arrest duration increased suggests that the effect of the reform was not limited to patrol policemen, who bear the direct cost of arrest, but rather that police investigators and police prosecutors were affected as well. Third, during the investigated time period and irrespective of the reform, police station commanders were evaluated according to the total number of arrestees being charged in their precinct, among other things. In that sense, the reform helped these commanders accomplish their own and the police's goals. Finally, police officers we spoke with noted that the direct costs of undertaking arrests for individual police officers did not necessarily decrease after the reform, mainly because the prison authority was procedurally much stricter than the police when accepting arrestees. For instance, the prison authority requires the presence of a police officer while it conducts a thorough health check-up for each new arrestee.

Another question that we have not yet touched upon is whether the reform was desirable from a normative perspective. Although it is difficult to provide an exact welfare measure of the consequences of the reform, we believe it is still important to offer at least a rough estimate. The total annual costs of property crimes in Israel are estimated at about $1.823 billion, and the costs of crimes which do not fall into the property and bodily harm crime categories is about $0.316 billion dollars. [9] Thus, a reduction of nearly 6% in property crimes and 4% in public order crimes amount to a saving of roughly $115.7 million. As Fig. 1 illustrates, following the reform the total number of arrestees increased from around 2500 to 4000. The average yearly cost of holding a prisoner in Israel, based on the prison authority's data, is $25,000. Thus, an increase of 1500 arrestees is associated with an increased cost of $37.5 million. Taking into account both the costs and benefits, we find that the annual net benefit of the reform is about $78 million. Furthermore, even if we focus only on the reduction in property crimes, we find that the estimated net benefit of the reform is larger than $70 million dollars.

The former calculation, however, is likely imprecise in two ways. First, to estimate the cost of holding an arrestee we used the average cost of a prisoner. As the marginal cost of holding an arrestees is likely to be significantly lower than the average cost, our assessment of the increased cost due to the higher number of arrestees may be overstated. Second, we must also consider the cost of arrests that did not lead to charges being filed. We found that the reform led to a decrease of 2 percentage points in the share of arrests leading to charges being filed. Since following the reform the total yearly number of arrests increased by 7,300 (the weekly average regional number increased by 28), this means that in the year following the reform about 1500 individuals were arrested but not charged. Arguably, integrating into the welfare calculations the costs incurred as a result of these arrests would make the bottom line of the welfare analysis less obvious.[10]

Finally, our findings regarding the increased number of arrestees and the corresponding reduction in crime may suggest that the reduction in crime is a result of the incapacitation of criminals. The fact that we do not find evidence for crime displacement (Section 3.6.1) further supports incapacitation rather than deterrence. This interpretation of the results is somewhat different than other studies on the relationship between police and crime, which have often emphasized the deterrent role of police (e.g. Draca et al., 2011).

5. Conclusion

In this paper we provide evidence regarding the consequences of an organizational reform in Israel that adjusted organizational boundaries between the police and the prison authority. We find that the reform led to an increase in the number and duration of arrests. At the same time, the quality of arrests, measured by the likelihood of arrestees being charged, decreased, as did the severity of offenses for which arrests were undertaken. In addition, we find that the effect of the reform on police activity also translated into significantly lower crime rates. Taken together, our results indicate that institutional details, such as organizational structure, have a substantial effect on police activity and crime, and that these effects should be taken into consideration when designing the structure of law enforcement agencies (Weisberg, 2013).

Though we focus on law enforcement agencies, we believe that there are other settings in the public sector to which our findings

[9] See the full report (in Hebrew) at http://mops.gov.il/Documents/Publications/CrimeDamage/CrimeDamageReports/CrimeDamageReport2008.pdf.

[10] One way to integrate the welfare loss of arresting non-charged arrestees is by using the $1000 for 90 days of arrest value of freedom figure offered by Abrams and Rohlfs (2011). Using this value suggests that the total welfare cost of these arrests is in the order of a quarter of million dollars, and therefore it does not change the conclusion regarding the desirability of the reform.

Table 7
Robustness to excluding regions.

	Benchmark (all regions included)	Excluding Northern district	Excluding Tel-Aviv district	Excluding Southern district	Excluding Center district	Excluding Jerusalem district
Log (number of arrests)	0.111***	0.138***	0.0650*	0.116***	0.0719*	0.133***
	(0.0357)	(0.0443)	(0.0357)	(0.0395)	(0.0423)	(0.0383)
Log (arrest duration)	0.385***	0.418***	0.405***	0.393***	0.0419	0.530***
	(0.104)	(0.113)	(0.142)	(0.115)	(0.108)	(0.0958)
Share charged	−0.0195**	−0.0262***	−0.00272	−0.0198**	−0.0188**	−0.0237**
	(0.00845)	(0.00941)	(0.0103)	(0.00985)	(0.00860)	(0.00928)
Log (maximum sentence)	−0.0592***	−0.0794***	−0.0144	−0.0657***	−0.0618***	−0.0603**
	(0.0219)	(0.0240)	(0.0222)	(0.0246)	(0.0236)	(0.0246)
Log (crime)	−0.0399***	−0.0488***	−0.0202*	−0.0434***	−0.0471***	−0.0363***
	(0.0118)	(0.0133)	(0.0114)	(0.0136)	(0.0180)	(0.0110)

The unit of observation is a region-week cell. The reform variable corresponds to the dummy variable, which assumes the value one in regions and weeks in which the transfer of control over jails from the police to the prison authority has already taken place. The regression includes week and regional fixed effects. The table presents estimates of the parameter β from estimating Eq. (1). The first column is served as a benchmark which includes all five regions. Each of the remaining columns exclude the region indicated at the top of the column. Observations are weighted by each region's population. Standard errors are robust and clustered by region/month.

*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

might apply. In many instances, decision-makers in the public sector do not bear the costs of their decisions. Furthermore, it is important to understand the relationship between structure and performance in the public sector, not only because this relationship can shape public policy, but also because many market mechanisms that economists often propose are unlikely to apply to public sector agencies. This implies that the consequences of integration decisions within the public sector are potentially far-reaching.

Appendix A. Online appendix

An online appendix to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2014.04.003.

References

Abrams, David S., 2012. Estimating the deterrent effect of incarceration using sentencing enhancements. Am. Econ. J.: Applied Econ. 4 (4), 32–56.
Abrams, David S., Rohlfs, Chris, 2011. Optimal bail and the value of freedom: evidence from the Philadelphia bail experiment. Econ. Inq. 49 (3), 750–770.
Afendulis, Christopher, Kessler, Daniel, 2007. Tradeoffs from integrating diagnosis and treatment in markets for health care. Am. Econ. Rev. 97 (3), 1013–1020.
Barbarino, Alessandro, Mastrobuoni, Giovanni, 2014. The incapacitation effect of incarceration: evidence from several Italian collective pardons. Amer. Econ. J.: Econ. Policy 6 (1), 1–37.
Becker, Gary, 1968. Crime and punishment: an economic approach. J. Polit. Econ. 76, 169–217.
Becker, Gary S., Rubinstein, Yona, 2011. Fear and the Response to Terrorism: An Economic Analysis. Mimeo 76.
Bresnahan, Timothy, Levin, Jonathan, 2012. The Handbook of Organizational Economics. Princeton University Press, Princeton, NJ.
Chalfin, Aaron, McCrary, Justin, 2013. Are U.S. cities underpoliced?: Theory and evidence.
DiTella, Rafael, Schargrodsky, Ernesto, 2004. Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack. Am. Econ. Rev. 94 (1), 115–133.
Draca, Mirko, Machin, Stephen, Witt, Robert, 2011. Panic on the streets of London: police, crime, and the July 2005 terror attacks. Am. Econ. Rev. 101, 2157–2181 (August).
Drago, Francesco, Galbiati, Robertoand, Vertova, Pietro, 2009. The deterrent effects of prison: evidence from a natural experiment. J. Polit. Econ. 117 (2), 257–280 (April).
Duflo, Esther, Mullainathan, Sendhil, Bertrand, Marianne, 2004. How much should we trust difference in differences estimates? Q. J. Econ. 119 (1), 249–275 (February).
Evans, William N., Owens, Emily G., 2007. COPS and crime. J. Public Econ. 91 (1–2), 181–201.
Forbes, Silke J., Lederman, Mara, 2010. Does vertical integration affect firm performance? Evidence from the airline industry. RAND J. Econ. 41 (4), 765–790 (Winter).
Gould, Eric D., Stecklov, Guy, 2009. Terror and the costs of crime. J. Public Econ. 93, 1175–1188.
Grossman, Sanford, Hart, Oliver, 1986. The costs and benefits of ownership: a theory of vertical and lateral integration. J. Polit. Econ. 94, 691–719.
Hart, Oliver, Shliefer, Andrei, Vishny, Robert W., 1997. The proper scope of government: theory and an application to prisons. Q. J. Econ. 112 (4), 1127–1161.
Klick, Jonathan M., Tabarrok, Alexander, 2005. Using terror alert levels to estimate the effect of police on crime. J. Law Econ. 48 (1), 267–280.
Kuziemko, Ilyana, 2013. How should inmates be released from prison? An assessment of parole versus fixed-sentence regimes. Q. J. Econ. 128 (1), 371–424.
Lafontaine, Francine, Slade, Margaret E., 2007. Vertical integration and firm boundaries: the evidence. J. Econ. Lit. 45 (3), 629–685.
Lee, David S., Justin McCrary, "The Deterrence Effect of Prison: Dynamic Theory and Evidence," *Mimeo*, 2009
Levitt, Steven D., 1996. The effect of prison population size on crime rates: evidence from prison overcrowding litigation. Q. J. Econ. 111 (2), 319–351.
Levitt, Steven D., 1997. Using electoral cycles in police hiring to estimate the effect of police on crime. Am. Econ. Rev. 87 (3), 270–290.
Machin, Stephen, Marie, Olivier, 2011. Crime and police resources: the street crime initiative. J. Eur. Econ. Assoc. 9 (4), 678–701 (August).
Mullainathan, Sendhil, Scharfstein, David, 2001. Do firm boundaries matter? Am. Econ. Rev. 91 (2), 195–199.
Vollard, Ben, Hamed, Joseph, 2012. Why the police have an effect on violent crime after all: evidence from the British crime survey. J. Law Econ. 5 (4), 901–924.
Weisberg, Robert, 2013. Empirical criminal law scholarship and the shift to institutions. Stanf. Law Rev. 65, 1371–1401.
Williamson, Oliver E., 1985. The Economic Institutions of Capitalism. Free Press, New York.