

The Economics of Rights: Does the Right to Counsel Increase Crime?[†]

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We examine the broad consequences of the right to counsel by exploiting a legal reform in Israel that extended the right to publicly provided legal counsel to suspects in arrest proceedings. Using the staggered regional rollout of the reform, we find that the reform reduced arrest duration and the likelihood of arrestees being charged. We also find that the reform reduced the number of arrests made by the police. Lastly, we find that the reform increased crime. These findings indicate that the right to counsel improves suspects' situation, but discourages the police from making arrests, which results in higher crime. (JEL K10, K41, K42)

The Sixth Amendment to the US Constitution guarantees that “in all criminal prosecutions, the accused shall enjoy the right ... to have the Assistance of Counsel for his defense.” The US Supreme Court, in the landmark decision *Gideon v. Wainwright* (1963), established that guaranteeing this right requires counsel to be publicly provided in criminal cases to defendants who are unable to pay for their own representation, in both state and federal courts. The importance of the right to counsel was recently emphasized by the US Supreme Court in two decisions that expanded this right to include a right to effective lawyers during plea negotiations (*Missouri v. Frye* 2012, *Lafler v. Cooper* 2012). The right to counsel is also protected by the European Convention on Human Rights (Article 6(3)(c)) and by the Charter of Fundamental Rights of the European Union (Article 47). Figure 1 shows that, since the end of World War II, the share of country constitutions that provide a right to counsel has increased dramatically, from 16 percent to 78 percent, indicating

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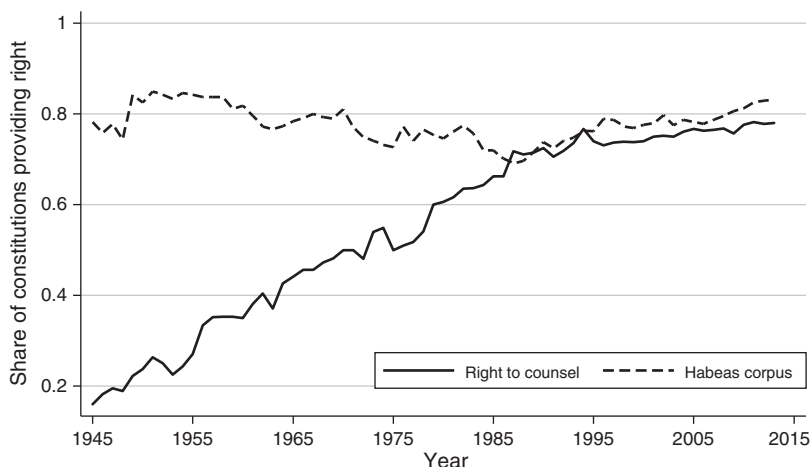


FIGURE 1. SHARE OF CONSTITUTIONS THAT PROVIDE A RIGHT TO COUNSEL AND PROTECTION FROM UNJUSTIFIED RESTRAINT (*habeas corpus*)

Source: Comparative Constitutions Project

the increased importance of this right across the world, especially relative to more traditional rights, such as the protection from unjustified restraint (*Habeas Corpus*). But what are the actual consequences of the right to counsel for society?

To address this question we focus on a legal reform in Israel that extended the right to counsel to indigent *suspects* in arrest proceedings. Before the reform, only indigent *defendants* were entitled to publicly provided legal counsel. In other words, before the reform, indigent defense was provided once one was charged, while after the reform indigent defense was provided earlier in the process, upon one's arrest. Thus, the extension of the right to counsel to suspects may serve as a natural experiment to investigate its social consequences.

Israel offers a good setting to investigate the consequences of a state-recognized right to counsel, given its very simple law enforcement system: only one police force, only one judicial system, and only one provider of indigent defense—the Office of the Public Defender. In such a setting it is relatively easy to identify changes to the right to counsel, and measure their effect on law enforcement. As a comparison, the United States has various types of police forces (federal, state, county, and municipal), two parallel judicial systems (federal and state), and indigent defense is provided by a myriad of entities and organizations, as well as by private attorneys.

Theoretically, what should be the consequences of the legal reform we investigate? If public defenders are effective in representing arrestees, then their presence in court should lead to better outcomes for arrestees. Thus, the reform should lead to a reduction in the likelihood of an arrest receiving court approval, in arrest duration, and in the likelihood of an arrestee being charged. Furthermore, if public defenders are effective, one would expect the police to take into account, in their activities, the prospect of confronting public defenders in court. Thus, the police may be more hesitant to make arrests, especially those that are less likely to be approved by the court in the presence of counsel, such as arrests for less severe crimes. Lastly, the reduction in police activity should lead to an increase in crime. The increase in

crime should be especially apparent in the types of crime that the police are more hesitant to pursue following the legal reform.

In our empirical analysis we use individual-level administrative data on all arrests for property crimes made in Israel, as well as detailed data on reported property crimes. Our empirical strategy relies on the staggered rollout of the reform across geographical regions of Israel, starting in November 1998 and ending in November 2002. This allows us to employ a difference-in-differences approach, measuring the impact of the reform by comparing, at each point in time, regions where the legal reform has been implemented with regions where the legal reform has not yet been implemented.

We begin by investigating the effectiveness of public defenders. First, we show that the legal reform reduced the likelihood of the court approving arrests made by the police by 5.3 percentage points. Second, we show that the legal reform led to a reduction of 16.7 percent in the duration of arrests. Third, we look at the effect of the legal reform on arrest outcomes. Conditional on arrest, the best possible outcome, from an arrestee's perspective, is for the arrestee to be released because he is classified as "no longer a suspect," since this means that the arrest leaves no police record.¹ In contrast, the worst possible outcome, from an arrestee's perspective, is for the arrestee to be charged. Accordingly, the two outcomes we look at are the share of arrestees that were released as non-suspects, and the share of arrestees that were charged. We find that the reform led to an increase of 3.8 percentage points in the share of arrestees that were released as non-suspects, and a decrease of 2.6 percentage points in the share of arrestees that were charged. These changes, which are desirable from arrestees' perspective, together with the findings on the reduction in likelihood of an arrest receiving court approval and on the shorter arrest duration, strongly indicate that public defenders are effective.

After examining the effectiveness of public defenders, we turn to investigating the effect of the reform on police activity. Our aim is to explore whether the police took into account the presence of public defenders in arrest proceedings and changed their activities outside the court. Our first finding is that the reform led to a reduction of 5.7 percent in the number of total arrests made by the police. To further investigate the impact of the reform on police activity, we also examine the effect of the legal reform on police activity with regard to different offenses classified by their severity. We find that the legal reform led to an 11.9 percent reduction in the number of arrests for less severe crimes, but we do not find a statistically significant change in the number of arrests for more severe crimes. Furthermore, we find that the reduction in the number of arrests for less severe crimes was concentrated in new arrestees, while the number of arrests of repeat arrestees has not declined. These findings indicate that, when faced with the prospect of confronting public defenders in court, the police are more hesitant to make arrests, especially of new arrestees for less severe crimes, probably because these type of arrests are less likely to be approved by the court in the presence of counsel.

¹Two other stated reasons for a release are "lack of sufficient evidence to prosecute," and "public interest does not require a prosecution." If an arrestee is released for these reasons the arrest leaves a police record.

Our final analysis examines the impact of the reform on reported crime. We find that the reform led to a 3.3 percent increase in crime. Focusing on the two categories of crime mentioned above, we find that the reform led to an increase in less severe crimes, but it had no effect on more severe crimes. These findings, which parallel the prior findings on the reform's effect on the number of arrests for less severe crime but not for more severe crimes, are consistent with the idea that the reduction in police activity due to the reform, in particular the reduction in the number of arrests and their duration, led to an increase in crime.

Altogether, these findings indicate that public defenders are effective in helping their clients, but at the same time may discourage the police from making arrests, which results in higher crime rates. That is, providing a right counsel has benefits, but also involves significant social costs.

We conclude the paper by conducting a cost-benefit analysis, to evaluate the social desirability of the reform. Since the reform reduced the number of arrests and their duration, and increased crime, its desirability depends on the social cost of a day of false arrest. According to one of our main estimates, if the social cost of a day of false arrest is less than \$1,000, which may well be the case, then the extension of the right to counsel to suspects may not have been socially desirable.

The remainder of the paper is organized as follows. Section I reviews related literature. Section II provides institutional background about the legal reform that extended the right to counsel to suspects, describes the data we use, and discusses our empirical strategy. In Section III, we present our results. In Section IV, we present some robustness tests. We discuss the results in Section V, where we use hand coded data to show that the legal reform led to an increase in suspects' representation in arrest proceedings, and consider the possibility of a selection bias affecting our results. We offer concluding remarks in Section VI.

I. Related Literature

The right to counsel is of central importance to legal scholars. In 2013, the Yale Law Journal dedicated a 600 page symposium issue, with 25 papers, for the 50 year anniversary of the US Supreme Court's landmark decision *Gideon v. Wainwright*. Some of the legal literature on the right to counsel centers around the philosophical justification for this right (e.g., Fried 1976, Pepper 1986, Luban 1988). Others have focused on issues of race and the right to counsel (e.g., Ogletree 1995, Stuntz 1997, Meares 2003). Still others have focused on the underfunding of the public defense system (e.g., Bright 1994, Brown 2004). Many more papers have addressed different aspects of this right and its implementation in practice.

The empirical work on the right to counsel has focused on micro-level outcomes. Specifically, much attention has been given to the effect that the quality of representation has on case outcomes. Abrams and Yoon (2007) use the random assignment of felony cases among public defenders within the public defender office in Clark County, Nevada to examine the effect of attorney ability on case outcomes. They find that attorneys with longer tenure in the public defender's office achieve better outcomes for the client, but that law school attended or gender seem to have no effect on case outcomes. Iyengar (2007) analyzes the performance of attorneys in

the federal indigent defense system, using the fact that cases are randomly assigned between salaried government workers (public defenders) and hourly wage-earning, court-appointed private attorneys. Using data from 51 districts she finds that public defenders perform significantly better than court-appointed private attorneys, in terms of lower conviction rates and sentence lengths. Further analysis suggests that attorney experience, wages, law school quality, and average caseload account for over half of the overall difference in performance. Anderson and Heaton (2012) undertake a similar exercise, but focus on murder cases in Philadelphia, which are randomly assigned between court-appointed private attorneys and public defenders. They find that, compared to appointed counsel, public defenders reduce their clients' rate of murder conviction, lower the probability of their clients receiving a life sentence, and reduce the overall expected time served in prison by their clients.

Like these papers we find that having counsel improves suspects' situation, by decreasing the likelihood of an arrest receiving court approval, arrest duration, and the likelihood that arrestees will be charged. However, our paper also looks at what one could call macro-level outcomes of the right to counsel, such as different measures of police activity and crime. In other words, unlike previous studies, we also examine the impact of the right of counsel outside the court, and not only with respect to particular cases that were brought before a judge. We are thus the first to show that the right to counsel leads to reduction in police activity and an increase in crime.

There is a small theoretical literature in economics that analyzes the effects of individual constitutional rights. For example, Seidmann (2005) and Mialon (2005) analyze the effects of a right to silence, and Gay et al. (1989) analyze the effects of a right to trial by jury. There is also a small empirical literature on these issues. Anwar, Bayer, and Hjalmarsson (2012) find that trial by jury introduces racial biases into court decisions. Atkins and Rubin (2003) find that crime increased following the adoption of the exclusionary rule, i.e., a rule that excludes from criminal trials evidence obtained in violation of the prohibition on unreasonable searches and seizure.

Our study is also related to the large literature on the economics of crime. Following Becker (1968), the literature has investigated the effect of various elements of the criminal justice system on crime, such as police activity (e.g., Levitt 1997; Klick and Tabarrok 2005; Draca, Machin, and Witt 2011; Vollaard and Hamed 2012; Chalfin and McCrary 2013), the deterrent and the incapacitating effect of prison (e.g., Levitt 1996, Drago, Galbiati, and Vertova 2009, Abrams 2012, Kuziemko 2013, Barbarino and Mastrobuoni 2014), and the organizational structure of law enforcement (Ater, Givati, and Rigbi 2014). The possibility that the right to counsel may reduce police activity and increase crime has not been considered.

II. Institutional Background, Data, and Empirical Strategy

A. *The Extension of the Right to Counsel*

Israel does not have a formal constitution that explicitly protects the right to counsel. Nevertheless, defendants' right to counsel is guaranteed by law (Section 15 of The Criminal Procedure Law, and Section 18 of the Public Defender Law).

Furthermore, several Israeli Supreme Court decisions acknowledged a natural right for counsel.²

The Office of the Public Defender in Israel operates under the Ministry of Justice.³ Its duties are to represent criminal defendants that are entitled to publicly funded legal counsel in court proceedings, most notably indigent defendants. Indigent defendants are defendants with a yearly income that is lower than two-thirds of the average yearly income in Israel. The Office of the Public Defender performs its duties by relying both on salaried government workers and on private attorneys contracted by it.⁴

On July 26, 1998 new regulations were passed, that extended the rights to counsel to suspects in arrest proceedings.⁵ Before these regulations were passed, indigent defendants had a right to publicly funded counsel only once they were charged, during the trial proceedings.⁶ Suspects had no right to counsel in arrest proceedings, though judges could appoint suspects' counsel at their discretion. Following the adoption of these regulations, the Office of the Public Defender began maintaining a staff of public defenders on call, from 7 AM until late at night and over weekends, ready to go to police stations and different courts to meet suspects and to represent them in arrest proceedings.

The extension of the right to counsel to suspect was scheduled to be implemented across Israel gradually, over four years, starting five months after the passage of the regulations. The different administrative regions of Israel and the timing of the reform in each region are shown in Figure 2.⁷ As will be further discussed in Section IIC, our identification strategy relies on the staggered implementation of the legal reform. Conversations that were held with officials at the Office of the Public Defender indicate that order of the reform across the different regions of the country was determined by taking into account the ease with which national officials could monitor the implementation of the reform, as well as the administrative readiness in each region to assume the new responsibility for representing suspects. Importantly, no factor related to police activity or crime was considered in determining the roll-out of the reform.

²For example, H CJ 1843/93 *Pinchasi v. The Israeli Knesset*, 48(4) PD 492 (1993), and Cr.A. 134/89, *Aberjil v. State of Israel*, 44(4) P.D. 20. For expanded historical background and legal analysis of the right to counsel in Israel see Ogletree and Sapir (2004).

³Though administratively under the Ministry of Justice, the Office of the Public Defender is an independent body, and reports to a committee of five members that includes the Justice Minister, a retired Supreme Court Justice, an academic representative, and two representatives of the Israeli Bar.

⁴For additional information on the Israeli Office of the Public Defender see: <http://www.justice.gov.il/En/Units/PublicDefense/About/Pages/default.aspx>.

⁵For expanded background to this reform see Lernau (2001).

⁶Following the commission of a crime, for the police to make a lawful arrest the arresting officer must have either probable cause to arrest, or a valid arrest warrant. After no more than 24 hours from an arrest without a warrant, the police must bring the suspect in front of a judge who can extend the arrest or release the suspect (often under bail). Upon arrest the suspect is interrogated. This interrogation may lead to the suspect being charged with specific crimes, at which point the suspect turns into a defendant. The charge leads to trial, which may lead to the conviction of the defendant.

⁷Though Israel's administrative regions are of different size, the country's population is relatively evenly spread across regions. For the year 1995, the share of the country population for the Jerusalem region, northern region, Haifa Region, central region, Tel Aviv region, and southern region was 12 percent, 16.9 percent, 13.2 percent, 21.7 percent, 20.3 percent, and 13.4 percent, respectively (Central Bureau of Statistics 2012).

	Region	Date
1	Tel Aviv region and central region	11/1998
2	Jerusalem region and southern region	01/1999
3	Northern region	12/2000
4	Haifa region	11/2002



FIGURE 2. THE TIMING OF LEGAL REFORM IN THE DIFFERENT REGIONS OF ISRAEL

The Israeli Police are a national agency, operating under the Ministry of Public Security.⁸ The main duties of the Israeli Police are crime prevention, traffic control, and the maintenance of public order. The Israeli Police are responsible for investigating virtually all types of crimes, and in most cases police prosecutors decide whether to prosecute a suspect.⁹

According to Israeli law, police officers can detain a suspect for up to 24 hours. After 24 hours, the police must obtain court approval for the arrest. 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. At the end of the arrest the suspect may be charged, released and charged later, or released and never charged.

Israel serves as a good setting to investigate the consequences of a state-recognized right to counsel. This is because Israel has a very simple law enforcement system. There is only one police force, which is managed on a national, rather than local, level. Furthermore, Israel has only one judicial system. More importantly, there is

⁸For further information on the Israeli Police see: <http://mops.gov.il/english/policingeng/police/pages/default.aspx>.

⁹Since the Israeli Police operate under the Ministry of Public Security, and the Office of the Public Defender operates under the Ministry of Justice, the increase in the budget of the Office of the Public Defender did not come directly from a reduction in the police's budget. Furthermore, in all of our many conversations with officials both at the Israeli Police and at the Office of the Public Defender, no one has ever argued that the operations of the Office of the Public Defender were somehow funded by cutting the budget of the police.

only one provider of indigent defense—the Office of the Public Defender, which is also managed on a national, rather than local, level. This allows the identification of a natural experiment of a change in the right to counsel, and the measurement of the consequences of this change.¹⁰

B. Data

We obtained from the Israeli Police full data on arrests for property crimes in Israel in the years 1996–2003. These data cover 112,445 arrests and 60,584 arrestees. For each arrest we know the arresting unit, the date of arrest, and its duration. We also observe for each arrest the specific offense that led to it, and the maximum prison sentence that can be imposed for that offense.¹¹ Additionally, we know whether the arrestee was charged following the arrest, and if the arrestee was not charged, the official stated reason for his release.

In addition to the arrest data we also have full data on 2,208,687 property 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 individual-level data. Panel A presents the data for all types of crime. Panels B and C divide the data into the two main legal categories used in Israeli criminal law: more severe crimes (“Pesha”), which are crimes that carry a sentence greater than three years in prison (this category is equivalent to felonies class A–D in the United States); and less severe crimes (“Avon”), which are crimes that carry a sentence of up to three years in prison (this category is equivalent to felony class E and misdemeanors in the United States).

Note that in Table 1, mean arrest duration is much longer than median arrest duration. This indicates that the distribution of arrest durations is skewed to the right, with a long right tail representing few arrests that are very long. Because of this we conduct all our statistical analysis on arrest duration using median arrest duration. However, nothing in the analysis changes if mean arrest duration is used instead of median arrest duration.

¹⁰ As a comparison, the United States has various types of police forces. There are federal level police forces (for example, FBI, DEA, ATF), state level police forces (state police, state bureaus of investigation), county level police forces (sheriff, county police), and municipal level police forces (municipal or metropolitan police departments). Furthermore, the United States has two parallel judicial systems: federal and state. Most important, indigent defense is provided in the United States in many different ways and by many different organizations. At the federal level, there are Federal Public Defender Organizations, whose staff are all full-time federal employees. There are also Community Defender Organizations that are nonprofit legal service organizations, and are not part of the federal system. Last, indigent defense is often provided by private “panel attorneys,” who are approved by the court. At the state level, some states operate public defender programs in which the Public Defender’s office has full authority over the provision of defense services statewide. Other states do not have a state public defender program, and have instead public defender programs that are organized, funded, and operated on a county, regional, or local level.

¹¹ If the suspect was involved in multiple offenses, our data includes the most severe offense among those offenses.

TABLE 1—DESCRIPTIVE STATISTICS

	Mean	SD	10P	90P
<i>Panel A. All crime</i>				
Number of arrests	45.05	23.49	24	84
Likelihood of court approval	0.57	0.12	0.41	0.73
Mean arrest duration (days)	9.57	7.06	3.67	18.21
Median arrest duration (days)	2.36	1.40	1.00	4.00
Share charged	0.43	0.13	0.26	0.59
Share not a suspect	0.29	0.13	0.14	0.47
Crime	885.89	426.86	349	1,455
<i>Panel B. Less severe crime</i>				
Number of arrests	16.88	14.78	5	42
Likelihood of court approval	0.46	0.19	0.21	0.71
Mean arrest duration (days)	6.75	8.86	1.50	14.69
Median arrest duration (days)	2.04	3.28	1.00	4.00
Share charged	0.43	0.19	0.20	0.68
Share not a suspect	0.25	0.17	0.00	0.50
Crime	369.90	148.66	186	579
<i>Panel C. More severe crime</i>				
Number of arrests	28.17	11.47	15	44
Likelihood of court approval	0.62	0.14	0.44	0.79
Mean arrest duration (days)	11.13	9.18	3.85	22.27
Median arrest duration (days)	3.10	2.29	1.00	5.50
Share charged	0.43	0.15	0.25	0.63
Share not a suspect	0.31	0.15	0.13	0.50
Crime	516.99	288.21	159	906

Notes: The unit of observation is a region-week cell. $N = 2,496$.

Note also that the number of arrests is approximately 5 percent of the number of crimes. Though this may seem low, from our discussion with police officials this ratio is typical of property crimes.

Property crime accounted for around 70 percent of crime in Israel in the period analyzed (Central Bureau of Statistics various years). We focus on these crimes both because of data availability and because it strengthens our claim for external validity. Israel is unique in its political and security conditions, and therefore non-property crime, such as violent crime and public order crime, could in theory be politically motivated. According to officials at the Office of the Public Defender, their general policy was, and still remains, to treat all arrestees equally, regardless of the crime they were arrested for, and therefore arrests for property crimes were not treated any differently than arrests for other types of crime.

C. Empirical Strategy

We use a standard difference-in-differences research design, exploiting the gradual extension of the right to counsel to study the effects of this right. Our baseline specification is as follows:

$$(1) \quad y_{rt} = \alpha + \beta \times \text{Counsel}_{rt} + \gamma_r + \delta_t + \epsilon_{rt},$$

where y_{rt} is the outcome variable of interest in region r in week t . The dummy Counsel_{rt} assumes the value one in regions and weeks in which the right to counsel

has been extended to arrest procedures. γ_r represents regional fixed effects, which control for time-invariant differences across regions. To account for the volatility in police and criminal activity we also include δ_t —weekly fixed effects (416 fixed effects, for each week in the 8 years of data we have). 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. Finally, we account for the serial correlation in the outcome variables by clustering the error terms at the region-month level. In Section IV, we explore alternative methods for deriving the estimates' standard errors.

This specification allows us to estimate the correlation between the implementation of the legal reform, reflected in the variable *Counsel_{rt}*, and the outcome variables, conditional on time and regional effects. The difference-in-differences 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. Importantly, as noted earlier, no factor related to police activity or crime, our outcome variables, was considered in determining the rollout of the reform.

To get a general sense of the effects of the reform on arrest duration, the number of arrests, and the number of reported crimes, we present in Figure 3 the residuals of these three outcome variables, after accounting for region and time fixed effects. The results are presented in 4-week bins, and are averaged across the 5 regions, using for each region the date of the legal reform in that region as time 0, for 52 weeks before and after the legal reform in each region. The figure indicates that the legal reform that extended the right to counsel to suspects reduced arrest duration and the number of court approved arrests, and increased crime. We now turn to analyzing the effect of this legal reform more rigorously.

III. Results

We first investigate the effectiveness of public defenders. Then, we look at the effect of the legal reform on police activity. Lastly, we look at the effect of the legal reform on crime.

A. Effectiveness of Public Defenders

Likelihood of Court Approval of Arrest.—How did the extension of the right to counsel to suspects, and the introduction of public defenders into arrest proceedings, affect the likelihood of the court approving arrests made by the police? To address this question we recall that in Israel the police may arrest suspects for up to 24 hours without court approval, but any arrest longer than 24 hours must be court approved. Thus, to look at the effect of the reform on the willingness of the court to approve arrests, we can measure how likely an arrest was to be longer than one day, and therefore approved by the court.

In columns 1 and 2 of Table 2, the dependent variable is the share of arrests that were longer than one day, and therefore were court approved. The regressions, as all other regressions in the paper, includes week and regional fixed effects, and standard

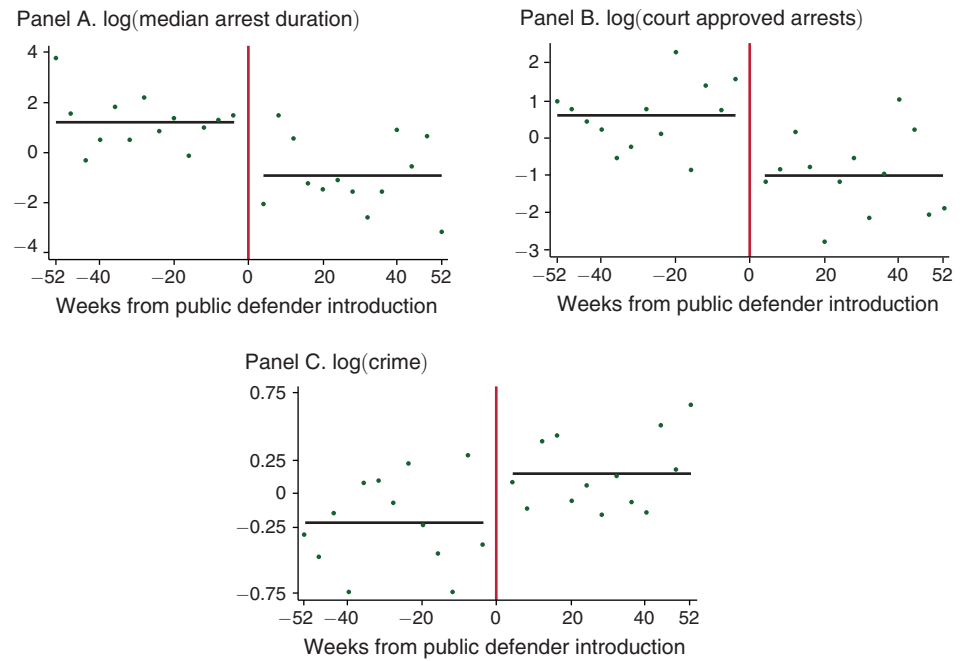


FIGURE 3. THE EFFECT OF THE LEGAL REFORM ON ARREST DURATION, THE NUMBER OF COURT APPROVED ARRESTS, AND CRIME

Notes: The three panels present partial-regression plots of regressions that control for region and time fixed effects. The results are presented in four-week bins, and are averaged across the five regions, using for each region the date of the legal reform in that region as time zero.

TABLE 2—EFFECT OF REFORM ON COURT APPROVAL OF ARRESTS AND ARREST DURATION				
Dep. variable:	Likelihood of court approval		log (median arrest duration)	
	(1)	(2)	(3)	(4)
Right to counsel	−0.0534 (0.00937)	−0.0505 (0.00937)	−0.167 (0.0278)	−0.166 (0.0276)
Week/region fixed effects	✓	✓	✓	✓
Region-specific time trend		✓		✓
Observations	2,496	2,496	2,496	2,496
R ²	0.337	0.387	0.309	0.344

Notes: The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.

errors are robust and clustered by region-month. Recall from Table 1 that, on average, 57 percent of arrests were court approved. We find that the reform reduced the likelihood of court approval by 5.34 percentage points, or 5.05 percentage points when controlling for region-specific time trends. We obtain the same results when dividing the data into the more severe and less severe crime categories.

That the likelihood of the court approving an arrest went down due to the reform is an indication of the effectiveness of public defenders. When public defenders are present in court, the court is less likely to approve an arrest made by the police. This finding, however, can also be the result of an indirect effect of public defenders, which is that, when faced with the prospect of confronting public defenders in court, the police chose to bring to court fewer arrestees.

Arrest Duration.—Next, we turn to investigating the effect of the right to counsel on arrest duration. The duration of arrest in our data is the time suspects spent in jail. That is, the arrest period, as we measure it, ends at the earliest of the two following dates: the date the suspect is released from jail and the date the suspect is charged. The dependent variable in columns 3 and 4 of Table 2 is the median number of days arrestees were held under arrest, in logs.

We find that the reform led to a decrease of 16.7 percent in median arrest duration, and when accounting for the possibility of region-specific time trends the decrease is of 16.6 percent. We obtain the same results when using mean, rather than median, arrest duration, and when dividing the data into the more severe and less severe crime categories.¹² These findings confirm that public defenders are effective. When they are present in court, arrest duration is shorter.

Arrests Outcomes.—How did arrest outcomes change because of the legal reform that extended the right to counsel to suspects? We look at two important arrest outcomes. First, we look at the official stated reason for a suspect's release, when a suspect was not charged. The best outcome of an arrest, from a suspect's perspective, is if the stated reason for the release is that he is no longer a suspect. In such a case, the arrest leaves no police record. Other stated reasons for release are "lack of sufficient evidence to prosecute," and "public interest does not require a prosecution." If an arrestee is released for these reasons his arrest leaves a police record. Second, we look at whether the arrestee was charged following the arrest. From an arrestee's perspective, of course, being charged is the worst possible outcome of an arrest.

In column 1 of Table 3, we estimated equation (1) using the fraction of arrests that ended up with the arrestee being released because he was no longer a suspect, as the dependent variable. Recall from Table 1 that, on average, 29 percent of arrests ended up with the arrestee being released because he was no longer a suspect. We find that the reform led to a 3.8 percentage point increase in the share of arrests that ended up with the arrestee being released because he was no longer a suspect. In other words, the reform led to more arrests ending up with the best possible outcome from an arrestee's perspective.

In column 3 of Table 3, we use as the dependent variable the fraction of arrests that led to charges being filed, in each week and region. Recall from Table 1 that, on average, 43 percent of arrests ended up with charges being filed against the arrestee. In column 3 of Table 3, we find that the reform led to a 2.6 percentage point decrease

¹² If we use the log of the mean, rather than of the median, arrest duration as the dependent variable, we find that the reform led to a statistically significant decrease of 17.6 percent in mean arrest duration, and when accounting for the possibility of region-specific time trends, the decrease is a slightly larger decrease of 19.3 percent.

TABLE 3—EFFECT OF REFORM IN ARREST OUTCOMES

Dep. variable:	Share not a suspect		Share charged	
	(1)	(2)	(3)	(4)
Right to counsel	0.0377 (0.0149)	0.0079 (0.0106)	−0.0257 (0.0105)	−0.0109 (0.0097)
Week/region fixed effects	✓	✓	✓	✓
Region-specific time trend		✓		✓
Observations	2,496	2,496	2,496	2,496
R ²	0.402	0.494	0.448	0.496

Notes: The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.

in the share of arrests ending up with charges being filed. In other words, the reform led to fewer arrests ending up with the worst possible outcome from an arrestee’s perspective.

Both these findings seem to indicate that public defenders are effective. Because of their presence, fewer arrests ended up with the arrestee being charged, and more arrests ended up with the arrestee being released because he is no longer a suspect. However, note that these findings are sensitive to the inclusion of region-specific time trends. In columns 2 and 4 of Table 3, when region-specific time trends are included, both effects disappear. Nevertheless, with quadratic region-specific time trends these results hold, both in magnitude and with statistical significance.¹³

Before concluding let us note that to the extent that the police started arresting different types of arrestees due to the extension of the right to counsel to suspects, some of our findings on the effectiveness of public defenders could, in theory, be driven by this selection bias. However, as we discuss in detail in Section 6.2, the selection bias seems to work against our findings here, and therefore our estimates are a lower bound measure of the effectiveness of public defenders.

B. Police Activity

Number of Arrests.—How did the extension of the right to counsel to suspects, and the introduction of public defenders into arrest proceedings, affect police activity? We look at the effect of this legal reform on the number of arrests. The dependent variable in columns 1 and 2 of Table 4 is the number of arrests, in logs. We find that the reform led to a reduction of 5.7 percent in the average number of weekly arrests, or 4.9 percent when controlling for region-specific time trends.

Our interpretation of this finding is that, when faced with the prospect of confronting public defenders in court, the police are more hesitant to make arrests. The reason for that is probably that the police know that arrests that were previously

¹³ With quadratic region-specific time trends, we find that the reform led to a 2.18 percentage point increase in the share of arrests ending up with the arrestee being released because he was no longer a suspect (*p*-value: 0.084), and to a 2.3 percentage point decrease in the share of arrests ending up with charges being filed (*p*-value: 0.057).

TABLE 4—EFFECT OF REFORM ON THE NUMBER OF ARRESTS

Dep. variable:	log (number of arrests)		log (number of court approved arrests)	
	(1)	(2)	(3)	(4)
Right to counsel	−0.0570 (0.0206)	−0.0486 (0.0206)	−0.156 (0.0275)	−0.143 (0.0283)
Week/region fixed effects	✓	✓	✓	✓
Region-specific time trend		✓		✓
Observations	2,496	2,496	2,496	2,496
R ²	0.785	0.79	0.622	0.631

Notes: The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.

approved by the court when no counsel was present, may not be approved in the presence of counsel. They therefore do not waste time on such arrests. This finding is consistent with other research that notes that police care about the outcome of their arrests. For example, Goodman (1990) notes that “Problems with the criminal justice system are ever present for police officers. The officers may feel that the arrest process is useless since many criminals are released as a result of the present system of justice.” Miller and Braswell (1992) similarly note that “officers become demoralized when they invest their time and risk their lives to make an arrest only to find the offender is given a minimum sentence or released.” Thus, the police internalize the effect of public defenders in their law enforcement activities.

In columns 3 and 4 of Table 4, the dependent variable is the number of court approved arrests, that is, arrests that are longer than one day and therefore had to be approved by the court. We find that the reform led to a reduction of 15.6 percent in the average number of court approved arrests, or 14.3 percent when controlling for region-specific time trends. This reduction is greater than the reduction in the number of arrests, because it combines two separate effects of the reform: the effect of the reform on police activity, as well its effect on the likelihood of the court approving arrests.¹⁴

Severity of Crimes for Which Arrests Were Made.—We also examine whether the reform affected the severity of crimes for which arrests were made by the police. To do so we divide the data into the two legal categories used in Israeli criminal law: More Severe Crimes (“Pesha”), and Less Severe Crimes (“Avon”).

Columns 1 and 2 in Table 5 consider the effect of the reform on the number of arrests for crimes in the more severe crime category, in logs. We do not find that the reform led to a statistically significant reduction in the number of arrests for more severe crimes. Columns 3 and 4 in Table 5 look at arrests for crimes in the less severe crime category. We find that the reform led to an 11.9 percent reduction

¹⁴ When looking only at arrests that are shorter than one day, we find the reform led to a statistically significant increase of 8 percent in these arrests.

TABLE 5—EFFECT OF REFORM ON THE NUMBER OF ARRESTS, BY SEVERITY OF CRIMES FOR WHICH ARRESTS WERE MADE

Dep. variable:	log (number of arrests)			
	More severe		Less severe	
	(1)	(2)	(3)	(4)
Right to counsel	−0.0310 (0.0258)	−0.0266 (0.0263)	−0.119 (0.0407)	−0.114 (0.0401)
Week/region fixed effects	✓	✓	✓	✓
Region-specific time trend		✓		✓
Observations	2,496	2,496	2,496	2,496
R ²	0.585	0.597	0.721	0.727

Notes: The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.

in the number of arrests for less severe crimes, or 11.4 percent when controlling for region-specific time trends. This means that the reform led the police to reduce the number arrests for less severe crimes, but not for more severe crimes.

Our interpretation of this finding is that, when faced with the prospect of confronting public defenders in court, the police devote less effort to less severe crimes, probably because the police expect that such arrests are less likely to be approved by the court in the presence of counsel. This is consistent with the descriptive statistics in Table 1, where one can see that the likelihood of the court approving an arrest for a more severe crime is 62 percent, while the likelihood of court approving an arrest for a less severe crime is 46 percent.¹⁵

Since we know that the number of arrests for less severe crimes decreased following the reform, we can focus on those arrests, and investigate which type of arrestees the police avoided following the reform. To do so we use the data we have on all arrests in the years 1996–2003 to identify repeat arrestees, which we define as people who were arrested more than once during this time period.¹⁶ We then reestimate equation (1) for less severe crime, separately for repeat arrestees and new arrestees, that is, people who were arrested only once during this time period.

Columns 1 and 2 in Table 6 consider the effect of the reform on the number of arrests of repeat arrestees, for less severe crimes, in logs. We do not find that the reform led to a statistically significant reduction in the number of arrests of repeat arrestees. Columns 3 and 4 in Table 6 look at the number of arrests of new arrestees, for less severe crimes. We find that the reform led to an 9.7 percent reduction in the number of arrests of new arrestees, or 10.0 percent when controlling for region-specific time trends. This means that the reform led the police to reduce the number arrests of new arrestees, but not of repeat arrestees.

¹⁵If we use the (log) average weekly maximum sentence for arrests as the dependent variable, we find that, for court approved arrests, the reform led to a 4 percent increase in the average maximum sentence (with and without regional time trends). This is consistent with the finding that the police devote less effort to less severe crimes.

¹⁶The results do not change if we define repeat arrestees as people who were arrested three, four, five, or six times during this time period.

TABLE 6—EFFECT OF REFORM ON THE NUMBER OF ARRESTS FOR LESS SEVERE CRIME, BY ARRESTEE TYPE

Dep. variable:	log (number of arrests for less severe crime)			
	Repeat arrestee		New arrestee	
	(1)	(2)	(3)	(4)
Right to counsel	−0.0434 (0.0447)	−0.0222 (0.0450)	−0.0966 (0.0423)	−0.100 (0.0438)
Week/region fixed effects	✓	✓	✓	✓
Region-specific time trend		✓		✓
Observations	2,496	2,496	2,496	2,496
R ²	0.721	0.731	0.590	0.594

Notes: The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.

We view this finding as consistent with our prior finding. Just like the court is more likely to approve, in the presence of counsel, arrests for more severe crime than for less severe crime, the court is more likely to approve arrests for less severe crime that was committed by a repeat arrestee than by a new arrestee. Thus, we see again that, when faced with the prospect of confronting public defenders in court, the police devote less effort to arrests that are less likely to be approved by the court in the presence of counsel.

C. Crime

Finally, we look at how the legal reform that extended the right to counsel to suspects affected crime. In column 1 of Table 7, we use reported property crime, in logs, as the dependent variable. We find that the reform led to a 3.3 percent increase in crime. In column 2, when controlling for region-specific time trends, we find that the reform led to a 5.9 percent increase in crime.

The effect of the legal reform on crime is relatively large. The magnitude of the increase in crime that we document is comparable to the effect of a 10 percent reduction in police force or police activity, found in studies on the relationship between police activity and crime (e.g., Klick and Tabarrok 2005, Evans and Owens 2007, Draca, Machin, and Witt 2011).¹⁷

The basic intuition for why the extension of the right to counsel to suspects led to an increase in crime is that public defenders do not do a perfect job distinguishing between innocent and guilty arrestees. Representation by public defenders means that fewer innocent arrestees are arrested, and those that are arrested are released sooner. But it also means that guilty arrestees that should be arrested are either not arrested, or arrested for shorter durations. This latter effect is probably the source of the increase in crime.

¹⁷ As noted earlier, and as all other studies do, we use reported crime as our measure of crime.

TABLE 7—EFFECT OF REFORM ON CRIME

Dep. variable:	log (crime)					
	All		More severe		Less severe	
	(1)	(2)	(3)	(4)	(5)	(6)
Right to counsel	0.0330 (0.0130)	0.0595 (0.0102)	0.0009 (0.0160)	0.0324 (0.0126)	0.0891 (0.0128)	0.112 (0.0112)
Week/region fixed effects	✓	✓	✓	✓	✓	✓
Region-specific time trend		✓		✓		✓
Observations	2,496	2,496	2,496	2,496	2,496	2,496
R ²	0.965	0.983	0.960	0.980	0.949	0.965

Notes: The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.

We also examined which types of crime increased due to the reform, using again the two standard categories of crime: more severe crimes (crimes that carry a sentence that is greater than three years in prison) and less severe crimes (crimes that carry a sentence of up to three years in prison).¹⁸ In specifications 3 and 4 of Table 7, we find that the reform did not lead to a statistically significant increase in more severe crimes (without region-specific time trends), or led to a relatively small increase in more severe crimes (with region-specific time trends). By contrast, in specifications 5 and 6 of Table 7, we find that the reform led to a 8.9 percent increase in less severe crimes, or 11.2 percent when controlling for region-specific time trends.

These findings, together with our findings on the number of arrests for different categories of crimes (Table 5), shed light on the mechanisms through which the extension of the right to counsel affected crime. The starting point is the argument that public defenders helped release not only innocent arrestees, but also arrestees that should not have been released. Recall that we found in Table 5 that the reform did not lead to a decrease in the number of arrests for more severe crimes. Thus, the increase we find in more severe crimes (3.24 percent in column 4 of Table 7) is arguably not driven by a change in the number of arrests but rather by a decrease in deterrence following the reform. In other words, the increase in more severe crimes is because, in the presence of counsel, criminals who commit such crimes were less likely to be charged, conditional on arrest. In contrast, recall that we found in Table 5 that the reform led to a decrease in the number of arrests for less severe crimes. Thus, the larger increase we find in less severe crimes (11.2 percent in column 6 of Table 7) is probably driven by a combination of deterrence and incapacitation effects. The deterrence effect is due to the lower likelihood of being charged conditional on arrest, while the incapacitation effect is because, in the presence of counsel, fewer criminals who commit less severe crimes are arrested by the police.

¹⁸Unlike our arrest data, in which each arrest was categorized as an arrest for a more severe crime or a less severe crime, our crime data does not include such categorization. To derive this categorization we used the arrest data and categorized crimes as more severe or less severe based on the median maximum possible sentence assigned to them (whether greater than three years or not). We then used this categorization of each crime to divide the crime data into more severe and less severe crimes.

To strengthen our argument that the increase in crime following the extension of the right to counsel to suspects was at least partially the result of a reduction in deterrence, we focused on suspects who were arrested for the first time in the three months preceding the reform or the three months following the reform. We then investigated whether there is any difference in the likelihood of these arrestees recidivating. We found that those who were arrested after the reform were approximately 3 percent more likely to recidivate relative to those who were arrested before the reform.¹⁹ This finding does not change when extending the window of time we look at around the reform, from three months before and after, up to eight months.²⁰ We are aware that this result may be driven by the change in the composition of arrestees following the reform. Still, one possible interpretation of this result is that suspects who were arrested after the reform, when counsel was provided freely to arrestees, learned about the existence and effectiveness of public defenders, which reduced deterrence and thus led to a higher recidivism.

IV. Robustness

A. *Excluding Regions*

One concern that may arise with respect to the findings in Section 4 is that they are driven by a specific region in the country. To address this concern we estimate our main outcome variables—arrest duration, the number of arrests, and crime, each time with one region excluded. Table 8 presents the coefficients of 42 regressions, each estimating the effect of the reform on one of three outcomes noted at the top of each column, with the region noted at the beginning of each row excluded from the regression. We undertake this exercise both with and without region-specific time trends.

As one can see from Table 8, our findings are not driven by one specific region in the country, as excluding any region does not fundamentally change the results.

B. *Alternative Derivations of Standard Errors*

Employing a difference-in-differences approach using panel data may lead to an over-rejection of the null hypothesis, when outcome variables, such as crime and police activity measures, exhibit serial correlation (Bertrand, Duflo, and Mullainathan 2004). As noted, we address this concern by clustering the standard errors at the region-month level. However, alternative approaches to addressing this issue are possible.

In Table 9, we pursue alternative methods of deriving standard errors for the paper's main results, and present the p -values resulting from estimating the regressions while employing these methods. We cluster standard errors by region-quarter

¹⁹ Using individual level data, we regressed an indicator for recidivism on a dummy indicating arrests that were made after the reform took place in the relevant region. We obtain a coefficient of 0.027, with a p -value of 0.06.

²⁰ Relative to the three months window, in those regressions the coefficient generally increases to around 0.03, and the p -value decreases to around 0.01.

TABLE 8—EFFECT OF REFORM—EXCLUDING INDIVIDUAL REGIONS

Dep. variable:	log (median arrest duration)		log (number of arrests)		log (crime)	
	(1)	(2)	(3)	(4)	(5)	(6)
Excluded region						
None	−0.167 (0.0278)	−0.166 (0.0276)	−0.0570 (0.0206)	−0.0486 (0.0206)	0.0330 (0.0130)	0.0595 (0.0102)
Tel Aviv region	−0.188 (0.0309)	−0.180 (0.0310)	−0.0625 (0.0222)	−0.0531 (0.0225)	0.0350 (0.0146)	0.0696 (0.0107)
Central region	−0.175 (0.0286)	−0.171 (0.0278)	−0.0533 (0.0217)	−0.0430 (0.0216)	0.0342 (0.0146)	0.0639 (0.0106)
Jerusalem region	−0.125 (0.0291)	−0.138 (0.0295)	−0.0680 (0.0208)	−0.0584 (0.0211)	0.0393 (0.0122)	0.0500 (0.0104)
Southern region	−0.172 (0.0295)	−0.180 (0.0294)	−0.0631 (0.0217)	−0.0495 (0.0218)	0.0164 (0.0148)	0.0570 (0.0110)
Northern region	−0.162 (0.0349)	−0.145 (0.0357)	−0.0148 (0.0261)	−0.0107 (0.0260)	0.0473 (0.0163)	0.0787 (0.0134)
Haifa region	−0.183 (0.0376)	−0.177 (0.0353)	−0.0767 (0.0307)	−0.0750 (0.0290)	0.0280 (0.0171)	0.0327 (0.0131)
Region-specific time trend		✓		✓		✓

Notes: The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.

TABLE 9—ALTERNATIVE METHODS OF DERIVING STANDARD ERRORS

Dep. variable:	log (median arrest duration)		log (number of arrests)					
			All		Court approved		Less severe	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cluster by region-quarter	0.000	0.000	0.024	0.053	0.000	0.000	0.005	0.009
Cluster by region-year	0.000	0.000	0.075	0.087	0.000	0.000	0.059	0.050
Moulton factor correction	0.000	0.000	0.016	0.043	0.000	0.000	0.007	0.012
Wild bootstrapping	0.098	0.012	0.129	0.000	0.098	0.000	0.129	0.000
Region-specific time trend		✓		✓		✓		✓
Dep. variable:	log (crime)							
	All		Less severe					
	(9)	(10)	(11)	(12)				
Cluster by region-quarter	0.106	0.000	0.000	0.000				
Cluster by region-year	0.346	0.003	0.005	0.000				
Moulton factor correction	0.057	0.000	0.000	0.000				
Wild bootstrapping	0.002	0.518	0.043	0.106				
Region-specific time trend		✓		✓				

and by region-year. We also use the Moulton Factor Correction (Moulton 1986). Lastly, we use Wild Bootstrap with Mammen’s weights, as described in detail in Appendix B of Cameron, Gelbach, and Miller (2008). Each outcome variable is considered both with and without region-specific time trends.

As one can see from Table 9, our results are largely unaffected when employing alternative methods of deriving standard errors.

C. Other Robustness Checks

We collected yearly data on the share of minority groups and the fraction of young men (age 15–24) in each region's population. These variables undergo very little variation over time, so they are nearly fully absorbed in the regional fixed effects. We verified that our results hold when these variables are included in the analysis. We also verified that the results are qualitatively the same when weighting each observation by regional population or when normalizing the outcome by the corresponding regional population. Results are presented in the online Appendix.

Furthermore, we verified that the pre-reform crime rates and police activity measures were not associated with the order of the rollout of the legal reform. To do so we conducted a placebo test by reestimating the regressions for our three main outcomes, arrest duration, number of arrests, and crime, using earlier fictitious dates for the implementation of the reform in different regions. We set a fictitious reform date for the two regions in which the reform was first implemented (Tel Aviv and Central Regions). The fictitious reform dates for the remaining regions were set in each case to maintain the order of implementation and the relative difference in the time of implementation between regions. In this way, we reproduced our main estimations as if the legal reform started in the pre-reform period. We did this 122 times, for each week in the data preceding the first implementation of date of the true reform. The results show no significant effect of the fictitious reform.²¹ These results validate our empirical approach as they reveal no association between the pre-reform dynamics and the order of the legal reform.

Another robustness test we conducted was to divide the data into violent crimes (such as robbery and arson) and nonviolent crimes, instead of the division to less severe and more severe crimes, which we use in the paper. We verified that the results are qualitatively the same when using this alternative division. Results are presented in the online Appendix.

Our results are potentially driven by spatial displacement effects, which imply that criminal activity is diverted from regions in which the legal reform has not been implemented into other regions where the reform has been implemented. If spatial displacement did occur, then our estimates for both arrests and crime are potentially biased upwards. To test for spatial displacement effects, we focused on individuals who were arrested multiple times during the analyzed time frame, and were arrested at least once before November 1998 (the first date of the implementation of the legal reform). We used the information on the first arrest (made 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 (November 1998 to November 2002) would be greater than the corresponding conditional probability following the completion of the rollout (after November

²¹ We find that in less than 1 percent of the cases the fictitious reform had an effect on crime and arrest duration that is statically significant at a 1 percent level, which is the significance level we obtain for the findings in the paper. We also find that in 4 percent of the cases, the fictitious reform had an effect on the number of arrests that is statically significant at a 1 percent level.

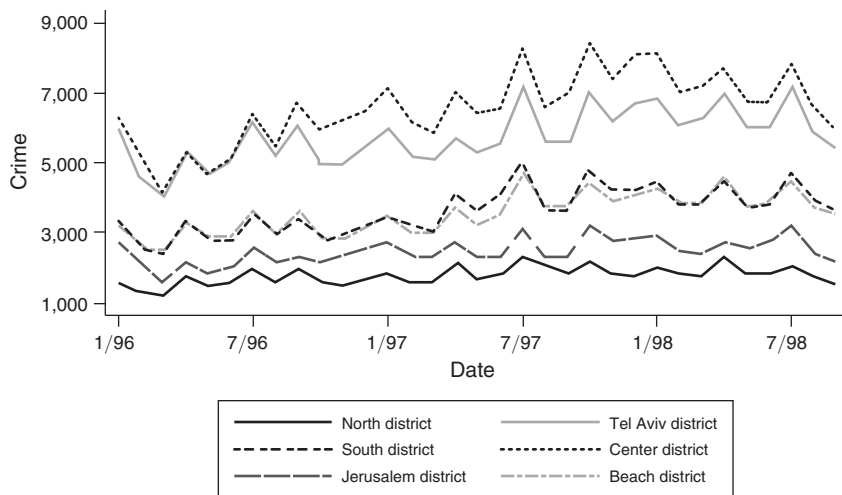


FIGURE 4. PRE-REFORM CRIME TRENDS, BY REGION

Note: The figure presents total monthly crime for each region.

2002). 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. This finding suggests that there was no spatial displacement effects, which is consistent with other research on the issue of spatial displacement (e.g., Weisburd et al. 2006).

Lastly, our difference-in-differences identification strategy uses each region as a control group for the other regions. Though in our estimation we control for regional fixed effects, as well as region-specific time trends, it is reassuring to know that the regions look similar before the legal reform was implemented. Figure 4 presents a time series of regional crime levels from January 1996 to September 1998, which is the time period before the first implementation of the legal reform. One can clearly see that in this period all regions experienced similar crime patterns.

V. Discussion

A. The Effect of the Reform on Representation

In our analysis we use the dates in which the legal right to counsel was extended to suspects in each region of the country, to analyze the effect that the right to counsel has on various outcomes. But did the extension of the right to counsel to suspects actually lead to increased representation of suspects in arrest proceedings?

Measuring actual representation of suspects in arrest proceedings during the years 1998–2002 turns out to be rather complicated. Though some digitized data for individual court cases are available in Israel since 2007, and from 2010 data

with broad coverage are available, for the years 1998–2002 no digitized data of court cases, and in particular of arrest proceedings, are available. Thus, in order to investigate whether the extension of the right to counsel to suspects led to an actual increase in the representation of suspects in arrest proceedings, one needs to look at court protocols, and hand code the data.

We use hand coded data on arrest proceedings in the Tel Aviv Magistrate's Court. The data were derived from the analysis of a random selection of two-thirds of the protocols of arrest proceedings during August and September of the years 1995, 1998, and 1999. For each case, we know whether the suspect was represented at all, and if the suspect was represented, we know whether the attorney was a privately retained attorney or a public defender. Since the extension of the right to counsel took place in the Tel Aviv region on November 1998, the data from 1995 and 1998 reflect the situation before the legal reform, and the data from 1999 reflect the situation after the legal reform.

As one can see in Table 10, after the right to counsel was extended to suspects in arrest proceedings, the share of suspects who were represented in court doubled, from 42–43 percent in 1995 and 1998, to 85 percent in 1999. This change was due to the dramatic increase in the number of suspects who were represented by public defenders, from 12–14 percent in 1995 and 1998, to 54 percent in 1999.²² When looking at representation by offense type, we find that the increase in representation occurred in all type of offenses. The findings in Table 10 support the idea that the effects we document in Section III are driven by the presence of counsel for suspects in arrest proceedings.

B. *Selection Bias*

Throughout the paper, we have not considered the possibility of a selection bias affecting our results. In particular, our findings on the effectiveness of public defenders, in Section IIIA, may be contaminated by the change in police activity we document in Section IIIB. For example, if the composition of arrests has changed due to the reform, as we show in Section IIIB, then the effect we are showing of the reform on the duration of arrests and the likelihood of court approval of arrests, may not reflect the true effect of the right to counsel on these outcomes.

Note, however, that the selection bias seems to work against our findings. As we show in Section IIIB, following the reform the police devote less effort to less severe crimes, probably because such arrests are less likely to be approved by the court in the presence of counsel, which is consistent with our descriptive statistics in Table 1. If this is indeed the case then in theory it would have been plausible for us not to find any effect of the reform on arrest duration and the likelihood of court approval of arrests, or even to find that the reform led to an increase in these outcomes (Knowles, Persico, and Todd 2001). However, despite this selection bias, we

²² We do not get 100 percent representation following the reform since indigent suspects were eligible to publicly provided counsel only if their yearly income was lower than two-thirds of the average yearly income in Israel. According to officials at the Office of the Public Defender, at the initial stages of the reform this eligibility requirement was strictly enforced.

TABLE 10—TYPES OF REPRESENTATION IN ARREST PROCEEDINGS

Year	Number of cases (1)	Not represented (%) (2)	Represented (%) (3)	Type of representation	
				Hired (%) (4)	Public (%) (5)
1995	539	56.4	43.60	31.7	11.9
1998	805	57.5	42.50	28.5	14.0
1999	460	14.6	85.40	31.7	53.7

Note: Column 3 = column 4 + column 5.

do document, in Section IIIA, that the reform led to a reduction in arrest duration and in the likelihood of court approval of arrests. Thus, the estimates we present in Section IIIA are in fact a lower bound of the true estimates of the effect of the right to counsel on these outcomes.

A selection bias could be driven by other measures than the severity of crime. But for the selection bias to undermine our findings on the effectiveness of public defenders, it must be that following the extension of the right to counsel the police decided to focus their efforts specifically on the types of arrests that are short, less likely to be approved by a court, and less likely to end with the arrestee being charged. We do not find this possibility very likely.

C. Cost-Benefit Analysis

Was the reform that extended the right to counsel to suspects 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 nonetheless important to offer at least a rough estimate.

On the cost side, the average annual costs of property crimes in Israel are estimated at about \$1.4 billion (Division of Planning, Budget, and Monitoring 2009). Thus, an increase of 3.3 percent in property crimes amounts to an increase in the cost of crime of roughly \$46 million. If we take the estimate we get when we include region-specific time trends, an increase of 5.9 percent in property crimes amounts to an increase in the cost of crime of roughly \$83 million. However, these costs have to be reduced, to reflect the fact that the increase in crime was concentrated in less severe crime. Let us therefore arbitrarily reduce the costs by 50 percent, to \$23–41.5 million.

In addition to the cost of crime, the direct cost of employing public defenders to represent suspects has to be included. These costs were 10 percent of the annual expenses of the public defender (Public Defender 2002), which comes up to \$3.2 million.

On the benefit side, the reform led to a decrease of approximately 30,000 arrest days per year.²³ The average yearly cost of holding a prisoner in Israel, based on the

²³ As shown, the average weekly regional number of arrests went down by 5.7 percent. Using the descriptive statistics, and recalling that there are 6 regions, this means 800 arrests a year, with an average arrest duration of 9.57 days. For the remaining arrests that were made, arrest duration went down by 17.6 percent, or 1.7 days per arrest.

Prison Authority's data, is \$26,000. Thus, the reduction in arrest days amounts to \$2.2 million in savings. Note, however, that these savings may be overstated, since the marginal cost of holding an arrestee is likely to be significantly lower than the average cost, which we used here. Another factor on the benefit side is that when people are not under arrest, they can work. We use the minimum daily wage in 2002 to evaluate this benefit of the reduction of arrest days. This benefit comes out to \$1.5 million.

In addition to the direct savings from the reduction in arrests, there also could be social savings. One can argue that the "right" number of arrests is obtained only when suspects are represented, and therefore the reduction in arrests and their duration following the reform represents the elimination of socially undesirable, or "false" arrests. However, since crime went up due to the reform, one can argue that these were not all false arrests, which means that public defenders helped release arrestees that should not have been released. In any event, the question is what is the social cost of a day spent under false arrest. Whatever that value is, one can multiply it by 30,000, to get the maximal social benefit of the reform in terms of eliminating false arrests.

Lastly, one can argue that there is an inherent value in having suspects represented. The question is what is the precise social value of this right.

Altogether, the cost of the reform, without considering the reduction in false arrests and the inherent value of having suspects represented, is \$22.5–\$41 million. How can we assess the reform's desirability? One way to look at this question is to divide the cost of the reform by the number of residents in the country. Taking the cost at \$30 million (roughly the middle figure of \$22.5–41 million), and dividing by 6.2 million, the number of residents in Israel in the year 2000, this means that every resident bore a yearly cost of roughly \$5 because of the extension of the right to counsel. If we think that the per year inherent value of representation is worth more than \$5 to each resident, which may well be the case, then the extension of the right to counsel was desirable.

Another way to look at this question is to divide the cost of the reform by the number of false arrest days that were avoided. Taking the cost at \$30 million, and dividing it by the 30,000 false arrest days that were avoided due to the reform, we get that every day of false arrest that was avoided resulted in \$1,000 of crime costs. If we think that social cost of one day of false arrest is less than \$1,000, which may well be the case,²⁴ then the reform was undesirable. These two calculations show that the desirability of the reform may depend on what we choose as our unit of comparison.

VI. Conclusion

In this paper, we provide evidence regarding the broad consequences of a legal reform in Israel that extended the right to counsel to suspects. We find that publicly provided legal counsel reduced the likelihood of arrests receiving court approval, arrest duration, and the likelihood of arrests leading to charges being filed. We also

²⁴ US Federal law provides for a compensation of \$50,000 for each 12-month period of wrongful incarceration, or \$137 per day (28 USC § 2513).

find that publicly provided legal counsel affected police activity, in particular, by reducing the number of arrests made by the police. Lastly, we find evidence that publicly provided legal counsel increased crime. These findings indicate that the right to counsel improves suspects' situation, but discourages the police from making arrests, which results in higher crime.

In addition to providing a better understanding of the social consequences of the right to counsel, our findings have implications for the policy debate around the scope of the right to counsel. Unlike the United States, other countries have a more limited right to counsel. For example, in Canada the right to counsel during interrogation is limited (*R. v. Sinclair* 2010). In France, access to a lawyer is not guaranteed on arrest, is often limited to a 30 minute consultation, and lawyers can be excluded from the interrogation. In Germany, suspects do not have a right to a lawyer, and in Italy, access to a lawyer may be delayed for up to 48 hours by a prosecutor, and up to 5 days by a judge (Cape et al. 2010). Our findings suggest that, if the social costs of the right to counsel are large, one can make an argument for a more limited right to counsel, of the type provided in the aforementioned countries.

Our findings may have broader implications. Though the right to counsel is currently awarded in the United States only in criminal cases, there has been a growing demand to extend this right to other realms. In 2006, the American Bar Association passed a resolution that asserted a right to counsel also in civil cases involving "adversarial proceedings where basic human needs are at stake, such as those involving shelter, sustenance, safety, health, or child custody" (American Bar Association 2006). Similarly, some have argued for the extension of the right to counsel to deportation proceedings, where currently persons facing deportation have only a privilege to retain counsel at their own expense (Eagly 2013, Johnson 2013). The question of whether enemy combatants, such as those held at Guantanamo Bay detention camp, should be awarded the full right to counsel still remains (Katyal 2013, Metcalf and Resnik 2013). That the right to counsel involves not only benefits, but also significant social costs, means that before this right is extended to other realms, more rigorous assessment of its benefits and costs in specific contexts is in order.

More generally, the language of rights dominates political and legal debates around the world. This discourse often reflects the view that certain fundamental rights are absolute. In this paper we adopt a different position. We approach rights as economists, weighing their benefits against their costs. Like the right to counsel, one would expect many fundamental rights to involve benefits and costs. Our approach can therefore be applied to other contexts, leading to a better understanding of the social consequences and desirability of other fundamental rights.

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