The Right to Counsel: Benefits and Costs*

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Abstract

What are the benefits and costs of the right to counsel? To address this question we exploit a legal reform in Israel that extended the right to publicly provided legal counsel to suspects in arrest proceedings. Using the staggered rollout of the reform in different regions of the country, we find that publicly provided legal counsel was effective, since it reduced arrest duration as well as the likelihood of arrests leading to charges being filed. We also find that publicly provided legal counsel affected police activity, in particular by reducing the number of arrests made by the police. Lastly, we find 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 could result in higher crime.

1 Introduction

Constitutional rights often involve benefits and costs. For example, protecting the right to freedom of speech generates a "marketplace of ideas" that is crucial for the development of any democracy (Mill 1869, *Abrams v. United States* 1919), but inhibits the government from intervening when this marketplace experiences failures due to externalities and consumer ignorance

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(Coase 1974). Similarly, providing the right to trial by jury is a check upon governmental abuse of power (Hamilton 1788), but introduces biases into court decisions (Anwar, Bayer and Hjalmarsson 2012). Lastly, recognizing the right to freedom of religion protects members of minority religions from oppression by the majority (Madison 1785), but inhibits the state from providing adequate education to all children (*Wisconsin v. Yoder* 1972). This paper investigates the benefits and costs of guaranteeing another important constitutional right: the right to counsel.

The Sixth Amendment to the U.S. Constitution guarantees that "in all criminal prosecutions, the accused shall enjoy the right. . . to have the Assistance of Counsel for his defense." The U.S. Supreme Court, in the landmark decision *Gideon v. Wainwright* (1962), 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 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). 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, once they were charged, during their trial proceedings. Thus, the extension of the right to counsel to suspects may serve as a natural experiment to investigate its social consequences.

Israel serves as a good setting to investigate the consequences of a state-recognized right to counsel. The reason is that Israel has a 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 in the right to counsel, and measure their effect on law enforcement. As a comparison, the U.S. 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.

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 led to a reduction of 17.6% in the duration of arrests. Second, 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 criminal 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 shorter arrest duration finding, indicate that public defenders are effective.

After investigating the effectiveness of public defenders, we turn to investigating the effect of the reform on police activity. First, we look at the number of arrests. We find that the reform led to a reduction of 5.7% in the number of arrests. When focusing on arrests that lasted longer than one day, and therefore had to be brought for court approval (as will be explained in the paper), we find a reduction of 15.6% in the number of arrests following the reform, which indicates that the effects we find are driven by the presence of counsel in court. Second, we look at the effect of the legal reform on arrest type, focusing on two standard categories of crimes used in Israeli criminal law: More severe crimes (crime that carry a sentence that is greater than three years in prison) and less severe crimes (crime that carry a sentence up to three years in prison). We find that the legal reform led to an 11.9% 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. These findings indicate that, when faced with the prospect of confronting public defenders in court, the police are more hesitant to make

[1] Two other stated reason 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 criminal record.

arrests, especially for less severe crimes.

Our final analysis examines the impact of the reform on reported crime. We find that the reform led to a 3.3% increase in crime. Focusing on the two categories of crime mentioned above, we find that the reform led to a increase in less severe crimes, but it had no effect on more severe crimes. These findings, which parallel the findings on the effect of the reform on arrest type, 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 could ultimately result in higher crime rates. That is, providing a right counsel has benefits, but also involves significant social costs.

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 U.S., 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 suspects do not have guaranteed access to a lawyer upon arrest, and do not have a right to have a lawyer present during police questioning. In Germany suspects do not have a right to a lawyer, and in Italy access to a lawyer delayed for up to forty-eight hours on a prosecutor's authority, or delayed up to five days on a judge's authority (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 U.S. 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 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.

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 U.S. 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 of 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 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.[2] 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

[2] Roach (2014) repeats the same exercise using data from state courts, and obtains the same results.

probability of their clients receiving a life sentence, and reduce the overall expected time served in prison by their clients.

These papers all examine the effect of different types of representation on case outcomes, and not the effect of having counsel. Our paper finds that having counsel improves suspects' situation, by decreasing arrest duration and the likelihood that arrestees will be charged. 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.

Following Becker (1968), the empirical literature on the economics of crime 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 et al., 2011, Vollardand and Hamed 2012, Chalfin and McCrary 2013), the deterrent and the incapacitating effect of prison (e.g. Levitt 1996, Lee et al. 2009, Drago et al. 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 affect police activity and therefore crime has not been considered.

The remainder of the paper is organized as follows. Section 2 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 3 we present our results. In Section 4 we present some robustness tests. We discuss the results in Section 5, 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 social desirability of the legal reform. We offer concluding remarks in Section 6.

2 Setting, Data and Empirical Strategy

2.1 The Extension of the Right to Counsel

The Office of the Public Defender in Israel operates under the Ministry of Justice. Its duties are to represent criminal defendants that are entitled to publicly funded legal counsel in court proceedings, most notably to 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.

	Region	Date
1	Tel-Aviv Region & Central Region	11/1998
2	Jerusalem Region & Southern Region	01/1999
3	Northern Region	12/2000
4	Haifa Region	11/2002

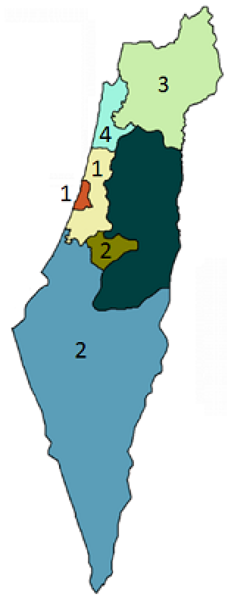


Figure 1: The timing of legal reform in the different regions of Israel

On May 26th, 1998 new regulations were passed, that extended the rights to counsel to suspects in arrest proceedings. Before these regulations were passed, indigent defendants had a right to publicly funded counsel only once they were charged, during the trial proceedings. 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 1998 regulations were 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 1. As will be further discussed in Section 2.3, our identification strategy relies on the staggered implementation of the legal reform.

The reform was scheduled to be implemented in a staggered manner because of budgetary considerations.[3] To the best of our knowledge the order of the rollout of the reform in the different regions of Israel was determined based on the administrative readiness of the Office of the Public Defender 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 rollout of the reform.

The Israeli Police is a national agency, operating under the Ministry of Public Security. The main duties of the Israeli Police are 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 the Israeli law, police officers can detain a suspect for up to twenty-four hours. After twenty-four hours 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.

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 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 clean natural experiment of a change in the right to counsel, and the measurement of the consequences of this change.[4]

[3]For example, the implementation of the reform in the Haifa Region was delayed due to "lack of budget" (Public Defender 2002, p. 10).

[4]As a comparison, the U.S. 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 U.S. has two parallel judicial systems, federal and state. Most importantly, indigent defense is provided in the U.S. 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. Lastly, 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

Table 1: Descriptive Statistics

	Mean	St. Dev.	10P	90P
Number of Arrests	45.05	23.49	24	84
Arrest Duration (days)	9.57	7.064	3.667	18.21
Maximum Sentence (months)	75.32	13.56	58.97	93.33
Share Charged	0.429	0.128	0.264	0.595
Share Not a Suspect	0.292	0.127	0.139	0.466
Crime	884.9	426.9	348	1454

The unit of observation is a region-week cell. $N = 2496$.

2.2 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 sentence that can be imposed for that offense. 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.

Property crime accounted for around 70% of crime in Israel in the period analyzed (Israel Central Bureau of Statistics 1997-2004). 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 violent crime and public order crime in Israel could in theory be politically motivated. By contrast, there is no reason to think that property crimes in Israel are any different than in other countries, as they are all driven by economic considerations.

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.

2.3 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:

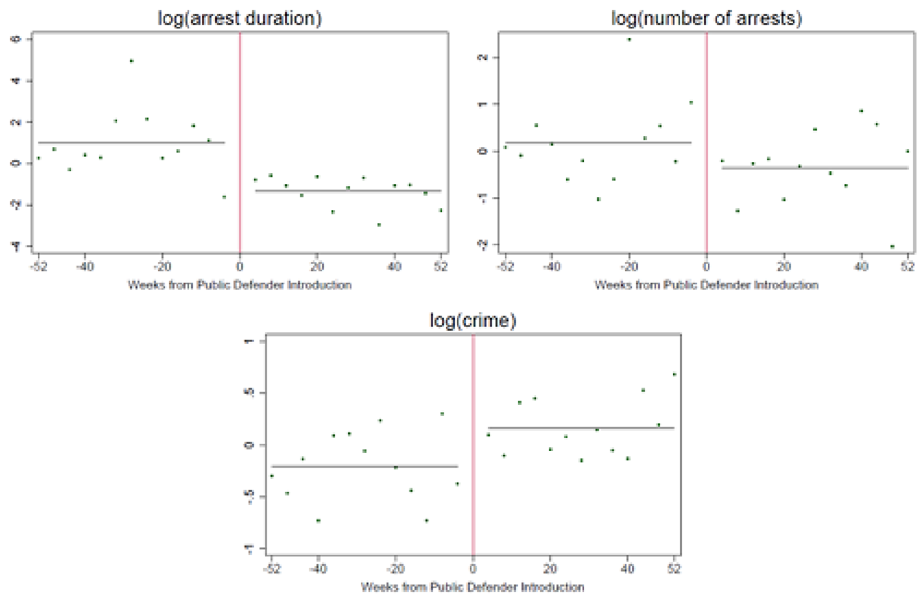
$$y_{rt} = \alpha + \beta \times Counsel_{rt} + \gamma_r + \delta_t + \epsilon_{rt} \tag{1}$$

where y_{rt} is the outcome variable of interest in region r in week t. The dummy $Counsel_{rt}$ 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. 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 4 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.

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 2 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 five regions, using for each region the date of the legal reform in that region as time zero, 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 arrests made by the police, and increased crime.[5] We now turn to analyzing the effect of this legal reform more rigorously.

[5] For additional figures, see online appendix.



Partial-regression plots of regressions that control for region and time fixed effects. The results are presented in 4-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.

Figure 2: The effect of the legal reform on arrest duration, the number of arrests, and crime.

3 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.

3.1 Effectiveness of Public Defenders

3.1.1 Arrest Duration

How did the extension of the right to counsel to suspects, and specifically the introduction of public defenders into arrest proceedings, affect arrest duration? The duration of arrest in our data is the time suspects spent in jail. That is, at the end of an arrest period, as we measure it, a suspect is either released or charged. The dependant variable in Table 2 is the number of days arrestees were held under arrest, in logs. The regression, as all other re-

Table 2: Effectiveness of Public Defenders

Dep. Variable:	log (arrest duration)	
	(1)	(2)
Right to Counsel	-0.176^{***} (0.0410)	-0.193^{***} (0.0414)
Week/Region Fixed Effects	✓	✓
Region-specific Time Trend		✓
Obs.	2496	2496
R^2	0.361	0.406

The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.
$^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

gressions in the paper, includes week and regional fixed effects, and standard errors are robust and clustered by region-month.

We find that the reform led to a decrease of 17.6% 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%. These findings confirm that public defenders are effective. When they are present in court, arrest duration is shorter.

3.1.2 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 officialy 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 criminal record. Other stated reasons for release are "lack of sufficient evidence to prosecute," and "public interest does not require a prosecution." If the arrestee is released for these reasons his arrest leaves a criminal record. Second, we look at whether the arrestee was charged at the end of the arrest. From an arrestee's perspective, of course, being charged is the worst possible outcome of an arrest.

In columns (1) of Table 3 we estimated Equation 1 using the fraction of

Table 3: Effectiveness of Public Defenders

Dep. Variable:	Share Not a Suspect		Share Charged	
	(1)	(2)	(3)	(4)
Right to Counsel	0.0377**	−0.0079	−0.0257**	−0.0109
	(0.0149)	(0.0106)	(0.0105)	(0.0097)
Week/Region Fixed Effects	✓	✓	✓	✓
Region-specific Time Trend		✓		✓
Obs.	2496	2496	2496	2496
R^2	0.402	0.494	0.448	0.496

The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.
$^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

arrests that ended up with the aresstee being released because he was no longer a suspect, as the dependent variable. Recall from Table 1 that, on average, 29% 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% 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 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 finding 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.

Table 4: Effect of Reform on the Number of Arrests

Dep. Variable:	log (number of arrests)	
	(1)	(2)
Right to Counsel	-0.0570^{***} (0.0206)	-0.0486^{**} (0.0206)
Week/Region Fixed Effects	✓	✓
Region-specific Time Trend		✓
Obs.	2496	2496
R^2	0.785	0.79

The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.
$^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Nevertheless, with quadratic region-specific time trends these results hold, both in magnitude and with statistical significance.[6]

3.2 Police Activity

3.2.1 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 dependant variable in Table 4 is the number of arrests, in logs. We find that the reform led to a reduction of 5.7% in the average number of weekly of arrests, or 4.9% 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 approved by the court when no counsel was present, may not be approved in the presence of counsel. Thus, the police internalizes the

[6] With quadratic region-specific time trends, we find that the reform led to a 2.18 percentage point increase in the share of arrests endeding 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 5: Effect of Reform on the Number of Arrests, by Arrest Duration

Dep. Variable:	log (number of arrests)			
	Arrests longer than 1 day		Arrests up to 1 day	
	(1)	(2)	(3)	(4)
Right to Counsel	-0.156^{***} (0.0275)	-0.143^{***} (0.0283)	0.0807^{**} (0.0330)	0.0797^{**} (0.0322)
Week/Region Fixed Effects	✓	✓	✓	✓
Region-specific Time Trend		✓		✓
Obs.	2496	2496	2496	2496
R^2	0.622	0.631	0.695	0.717

The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.
$^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

effect of public defenders in their law enforcement activities.

In Table 5 we divide the data into two groups: arrests that lasted more than one day, and arrests that lasted up to one day. The reason for this is that, as noted, in Israel the police may arrest suspects for up to twenty-four hours without bringing them to court. Thus, we know that arrests that are longer than one day had to be brought for court approval, and that arrests that are shorter than one day may not have been approved by the court.

Columns (1) and (2) consider the effect of the reform on the number of arrests that lasted more than one day. We find that the reform led to a reduction of 15.6% in the average number of weekly arrests, or 14.3% when controlling for region-specific time trends. Columns (3) and (4) consider the effect of the reform on the number of arrests that lasted up to one day. We find that the reform led to an increase of 8% in the number of weekly arrests.

These finding are another indicator of the effectiveness of public defenders. The number of arrests for more than one day went down by more than the overall number of arrests, and this is despite the fact that the number of arrests up to one day actually increased. These results indicate that, in the presence of counsel, the court released more suspects. 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

Table 6: Effect of Reform on the Number of Arrests, by Arrest Type

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		✓		✓
Obs.	2496	2496	2496	2496
R^2	0.585	0.597	0.721	0.727

The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.
$^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

police chose to bring to court fewer arrestees.

3.2.2 Arrest Type

We also examined whether the reform affected the types of arrests that were made by the police. Specifically, we are interested in the severity of crimes that the police pursued. In Israeli criminal law, crimes are divided into two categories: More Severe Crimes ("Pesha") are crimes that carry a sentence that is greater than three years in prison (this category is equivalent to Felonies class A-D in the U.S.). Less Severe Crimes ("Avon") 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 U.S.)

Columns (1) and (2) in Table 6 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 6 look at arrests for crimes in the less severe crime category. We find that the reform led to an 11.9% reduction in the number of arrests for less severe crimes, or 11.4% 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.

Table 7: Effect of Reform on Crime

Dep. Variable:	log (crime)	
	(1)	(2)
Right to Counsel	0.0330*** (0.0130)	0.0595*** (0.0102)
Week/Region Fixed Effects	✓	✓
Region-specific Time Trend		✓
Obs.	2496	2496
R^2	0.965	0.983

The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.
$^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Our interpretation of this finding is that, when faced with the prospect of confronting public defenders in court, the police focuses their effort on more severe crimes, probably because the police expects that such arrests are more likely to be approved by the court in the presence of counsel, whereas arrests for less severe crime are less likely to be approved.

3.3 Crime

Finally, we look at how the legal reform that extended the right to counsel to suspects affected crime. In columns (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% increase in crime. In column (2), when controlling for region-specific time trends, we find that the reform led to a 5.9% 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% 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 et al. 2011).

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

Table 8: Effect of Reform on Crime, by Crime Type

Dep. Variable:	log (crime)			
	More Severe		Less Severe	
	(1)	(2)	(3)	(4)
Right to Counsel	0.0009 (0.0160)	0.0324** (0.0126)	0.0891*** (0.0128)	0.112*** (0.0112)
Week/Region Fixed Effects	✓	✓	✓	✓
Region-specific Time Trend		✓		✓
Obs.	2496	2496	2496	2496
R^2	0.960	0.980	0.949	0.965

The unit of observation is a region-week cell. Standard errors are robust and clustered by region-month.
$^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Severe Crimes (crimes that carry a sentence of up to three years in prison).[7] Table 8 presents these results. 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, we find that the reform led to a 8.9% increase in less severe crimes, or 11.2% when controlling for region-specific time trends. These findings are consistent with the results of our earlier analysis, which indicated that the reform led to a decrease in the number of arrests for less severe crimes, but not for more severe crimes.

What explains the increase in crime that we find? As shown, the reform led to a reduction in the number and duration of arrests. These changes can decrease the deterrent effect of arrests, and also the incapacitating effect that arrests have on criminals. The decrease in both of these effects may explain the increase that we find in crime.

[7] Unlike 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 3 years or not). We then used this categorization of each crime to divide the crime data into more severe and less severe crimes.

4 Robustness

4.1 Excluding Regions

One concern that may arise with respect to the findings in Section 3 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 9 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 9, our findings are not driven by one specific region in the country, as excluding any region does not fundamental change hem.

4.2 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 (Duflo, Mullainathan and Bertrand 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 10 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 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 10, our results are largely unaffected when employing alternative methods of deriving standard errors.

Table 9: Effect of Reform - Excluding Individual Regions

Dep. Variable:	log (arrest duration)		log (num. of arrests)		log (crime)	
Excluded Region	(1)	(2)	(3)	(4)	(5)	(6)
None	−0.176***	−0.193***	−0.0570***	−0.0486**	0.0330***	0.0595***
	(0.0410)	(0.0414)	(0.0206)	(0.0206)	(0.0130)	(0.0102)
Tel-Aviv Region	−0.203***	−0.209***	−0.0625***	−0.0531**	0.0350**	0.0696***
	(0.0434)	(0.0434)	(0.0222)	(0.0225)	(0.0146)	(0.0107)
Central Region	−0.204***	−0.212***	−0.0533**	−0.0430**	0.0342**	0.0639***
	(0.0418)	(0.0426)	(0.0217)	(0.0216)	(0.0146)	(0.0106)
Jerusalem Region	−0.125***	−0.169***	−0.0680***	−0.0584***	0.0393***	0.0500***
	(0.0425)	(0.0428)	(0.0208)	(0.0211)	(0.0122)	(0.0104)
Southern Region	−0.173***	−0.192***	−0.0631***	−0.0495**	0.0164	0.0570***
	(0.0449)	(0.0448)	(0.0217)	(0.0218)	(0.0148)	(0.0110)
Northern Region	−0.169***	−0.194***	−0.0148	−0.0107	0.0473***	0.0787***
	(0.0523)	(0.0533)	(0.0261)	(0.0260)	(0.0163)	(0.0134)
Haifa Region	−0.187***	−0.172***	−0.0767**	−0.0750**	0.0280	0.0327**
	(0.0589)	(0.0576)	(0.0307)	(0.0290)	(0.0171)	(0.0131)
Region-specific Time Trend		✓		✓		✓

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

$^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table 10: Alternative Methods of Deriving Standard Errors

Dep. Variable:	log (arrest duration)		log (number of arrests)					
			All		Longer than 1 day		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	0.002	0.000	0.075	0.087	0.000	0.000	0.059	0.050
Moulton Factor Correction	0.007	0.008	0.036	0.117	0.002	0.007	0.023	0.117
Wild Bootstrapping	0.126	0.008	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	0.016	0.029	0.000	0.003
Wild Bootstapping	0.002	0.518	0.043	0.106
Region-specific Time Trend		✓		✓

4.3 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 re-estimating 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 considered four fictitious reform dates (January 1^{st} 1997, July 1^{st} 1997, January 1^{st} 1998, and July 1^{st} 1998) 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. The results, which are presented in the online appendix, show no significant effect of the fictitious reform. 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 and non-violent 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 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.

Lastly, our difference-in-differences identification strategy uses each district as a control group for the other districts. 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 3 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.

5 Discussion

5.1 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 guaranteeing 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

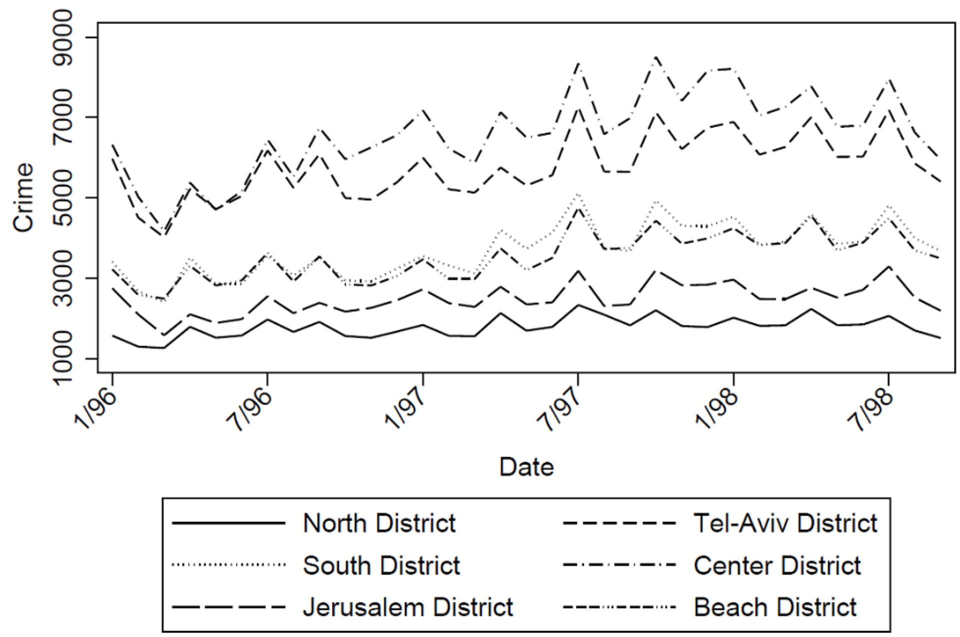


Figure 3: Pre-reform Crime Trends, by Region

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 11, after the right to counsel was extended to suspects in arrest proceedings, the share of suspects who were represented in

Table 11: Types of Representation in Arrest Proceedings

Year	Number of Cases	Not Represented	Represented	Type of Representation	
				Hired	Public
	(1)	(2)	(3)	(4)	(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)

court doubled, from 42-43% in 1995 and 1998, to 85% in 1999. This change was due to the dramatic increase in the suspects who were represented by public defenders, from 12-14% in 1995 and 1998, to 54% in 1999. The findings in Table 11 support the idea that the effects we document in Section 3 are driven by presence of counsel for suspects in arrest proceedings.

5.2 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 (Ministry of Public Security 2009). Thus, an increase of 3.3% 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% 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 reduce the costs by 50%, to $23-41.5 million.

In addition to the cost of crime, the direct cost of employing public defenders to represent suspects have to be included. These costs were 10% of 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.[8] The average yearly cost of holding a prisoner in Israel, based on the prison authority's data, is $26,000. Thus, the reduction in arrest days amounts to savings of $2.2 million. Note however that these saving may be overstated, since the marginal cost of holding an arrestees 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 could also 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. 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 social benefit of the reform in terms of eliminating false arrests. However, one can also argue that, since the reform led to an increase in crime, the arrests that were eliminated were not false ones, or that many of them were not false, and therefore the reduction in the number and duration of arrests due to the reform is not a clear social benefit.

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 asses 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 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

[8] As shown, the average weekly regional number of arrests went down by 5.7%. 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%, or 1.7 days per arrest.

$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 $1000 of crime costs. If we think that social cost of one day of false arrest is less than $1000, which may well be the case, 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.

6 Conclusion

In this paper we provide evidence regarding the consequences of a legal reform in Israel that extended the right to counsel to suspects. We find that publicly provided legal counsel reduced arrest duration and the likelihood of arrests leading to charges being filed. We also find that publicly provided legal counsel affected police activity, in particular by reducing the number of arrests made by the police. Lastly, we find 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 could result in higher crime. Our findings may have broader implications, since there has been a growing demand to extend the right to counsel to other realms, such as civil cases and deportation proceedings. That the right to counsel involves significant social costs, in addition to benefits, means that before this right is extended to other realms, more rigorous assessment of its benefits and costs in specific contexts is in order.

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