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 ending up with 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, the right to trial by Jury (Article 39 of the Magna Carta; Article III, Section 2 and Seventh Amendment to the U.S. Constitution) is a check upon governmental abuse of power, but introduces biases into court decisions, and makes each trial a costly affair. Similarly, freedom of religion (First Amendment to the U.S. Constitution) is important for the protection of minority religions

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*We are grateful to seminar participants at Columbia Law School. We are also grateful to Hagit Lernau for providing us with data on arrest proceedings in the Tel-Aviv magistrate court.

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from oppression by the government or majority religions, but comes at the cost of allowing parents to provide their children with what many consider is an inadequate education. Lastly, intellectual property rights (noted in Article I, Section 8 of the U.S. Constitution) generate incentives for future innovation, but only at the cost of hindering present sale of goods. This paper investigates the benefits and costs of another important constitutional right, the constitutional 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*, established that this right means that counsel must be publicly provided in criminal cases, to defendants who are unable to pay for their own representation, both in state and federal courts. The court emphasized the importance of the right to counsel, and its relationship to fundamental values of equality, by noting that "From the very beginning, our state and national constitutions and laws have laid great emphasis on procedural and substantive safeguards designed to assure fair trials before impartial tribunals in which every defendant stands equal before the law. This noble ideal cannot be realized if the poor man charged with crime has to face his accusers without a lawyer to assist him." 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, indigent defendants were entitled to publicly provided legal counsel only once they were charged, during the 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 the 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. By contrast, 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-difference approach, measuring the impact of the reform by comparing, at the same point in time, regions where the legal reform has taken place with regions where the legal reform has not taken place yet.

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. 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. Second, we look at the effect of the legal reform on the type of arrests that were made. Specifically, we look at the effect of the reform on the severity of crimes for which arrests were made, measured using the maximum possible prison time associated with each arrest. We do not find that the reform changed the average maximum sentence for arrests. These findings indicate that, when faced with the prospect of confronting public defenders in court, the police are more hesitant to make arrests, but they do not change the types of crimes they pursue.

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. This finding is 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, the right counsel, like other constitutional rights, has benefits and costs.

The right to counsel has been of central importance to legal scholars. Some of the 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, Chin 2013, Richardson and Goff 2013). 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 actual implementation.

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. 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. Roach (2014) repeats the same exercise as Iyengar (2007), and obtains the same results using data from state courts.

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 how different factors affect crime, including police (e.g. Levitt 1997, Klick and Tabarrok 2005, Draca et al., 2011, Vollard and Hamed 2012, Chalfin and McCrary 2013), incarceration (e.g. Levitt 1996, Drago et al. 2009, Barbarino and Mastrobuoni 2014), the length of imprisonment (Lee et al. 2009, Kuziemko 2011, Abrams 2012), 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, indigent defendants. Indigent defendants are defendants with a yearly income that is lower than 2/3 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.

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 them counsel at their own discretion. Following the adoption of these regulations, the Office
The Right to Counsel: Benefits and Costs

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

<table>
<thead>
<tr>
<th>Region</th>
<th>Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tel-Aviv Region &amp; Central Region</td>
<td>11/1998</td>
</tr>
<tr>
<td>Jerusalem Region &amp; Southern Region</td>
<td>01/1999</td>
</tr>
<tr>
<td>Northern Region</td>
<td>12/2000</td>
</tr>
<tr>
<td>Haifa Region</td>
<td>11/2002</td>
</tr>
</tbody>
</table>

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

of the Public Defender began maintaining a whole 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.

An important aspect of the 1998 regulations was that they 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 2.4, our identification strategy relies on the staggered implementation of the legal 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 24 hours. After 24 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.

2.2 Why Israel?

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 judicial system. More importantly, there is only one provider of indigent defense—the Office of the Public Defender, which is managed as well on a national, rather than local, level.

In contrast, the U.S. has various types of police forces. There are federal level police forces (e.g. 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/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, 58 Federal Public Defender Organizations serve 66 of the 94 federal judicial districts. Each falls under the supervision of a local Federal Public Defender, who is selected for a four year term by the Court of Appeals of the Circuit in which the District Court is located. The staff of the Federal Public Defender are all full-time federal employees. Additionally, 15 Community Defender Organizations have been established in seventeen federal judicial districts. These are nonprofit legal service organizations incorporated under state law, usually operating under the supervision of a board of directors. They are funded, for the most part, by grants from the Federal Judicial Conference. These organizations and their employee are not part of the federal system. Lastly, federal criminal cases are often handled by private "panel attorneys," who are approved by the court. They do so in federal judicial districts where there is neither a Public Defender Organization nor a Community Defender Organization, and in other districts where some other factor precludes federal defender representation.

At the state level, things are more complex. Sixteen states operate state public defender programs in which the public defender office has full authority for the provision of defense services statewide. Other states have commissions which oversee the setting of standards and the provision of some services, but do not have a state public defender program. Within the state, however, there
may be an independent county funded public defender program operated within the state guidelines. If the local public defender program meets state guidelines, it may qualify for state funding. There are also a number of public defender programs that are organized, funded, and operated on a county, regional, or local level. Sixteen states have combined trial and appellate state public defender offices. Twelve other states have no statewide public defender providing trial representation, but do have statewide appellate offices.

To investigate the social consequences of the right to counsel, using real world data, one would like to identify a clean natural experiment of a change in the right to counsel, and measure the consequences of this change. In this respect Israel, because of the simplicity of its law enforcement system, especially relative to that of the U.S., serves as a good setting to investigate the consequences of the right to counsel.

2.3 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.

2.4 Empirical Strategy

We use a standard differences-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:

\[ y_{rt} = \alpha + \beta \times \text{Post}_{rt} + \gamma_r + \delta_t + \epsilon_{rt} \]  \hspace{1cm} (1)
The Right to Counsel: Benefits and Costs

Table 1: Descriptive Statistics

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

The unit of observation is a region-week cell. \( N = 2496. \)

where \( y_{rt} \) is the outcome variable of interest in region \( r \) in week \( t \). The dummy \( Post_{rt} \) assumes the value one in regions and weeks in which the right to counsel has been extended to arrest procedures. \( \gamma_r \) 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 \( \delta_t \) - weekly fixed effects. Finally, we account for the serial correlation in the outcome variables by clustering the error terms at the region-month level.

This specification allows us to estimate the correlation between the implementation of the legal reform, reflected in the variable \( Post_{rt} \), and the outcome variables, conditional on time and regional effects. The difference-in-difference approach implies that the impact of the reform is derived by comparing the change over time in the outcome variable in a region that has experienced the reform with the corresponding change in a region that has yet to experience the reform.

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.
Table 2: Effectiveness of Public Defenders

<table>
<thead>
<tr>
<th>Dep. Variable: log (arrest duration)</th>
<th>Share Not a Suspect</th>
<th>Share Charged</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reform</td>
<td>-0.176***</td>
<td>0.0377***</td>
</tr>
<tr>
<td>(0.0410)</td>
<td>(0.0149)</td>
<td>(0.0105)</td>
</tr>
<tr>
<td>Week/Region</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Obs.</td>
<td>2496</td>
<td>2496</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.361</td>
<td>0.402</td>
</tr>
</tbody>
</table>

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

* $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

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 dependent variable in column (1) of Table 2 is the number of days arrestees were held under arrest, in logs. The regression, as all other regressions 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. This finding confirms 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 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 criminal record. Other stated reason 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 column (2) of Table 2 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% of arrests ended up with the arrestee being released because he was no longer a suspect. In column (2) of Table 2 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 2 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 2 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 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.

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 dependent variable in column (1) of Table 3 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 arrests.

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
Table 3: Effect of Reform on the Number of Arrests

<table>
<thead>
<tr>
<th>Dep. Variable:</th>
<th>log (number of arrests)</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All Arrests</td>
<td>Arrests longer than 1 day</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td></td>
</tr>
<tr>
<td>Reform</td>
<td>-0.0570***</td>
<td>-0.156***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0206)</td>
<td>(0.0275)</td>
<td></td>
</tr>
<tr>
<td>Week/Region</td>
<td>✓</td>
<td>✓</td>
<td></td>
</tr>
<tr>
<td>Fixed Effects</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Obs.</td>
<td>2496</td>
<td>2496</td>
<td></td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.785</td>
<td>0.622</td>
<td></td>
</tr>
</tbody>
</table>

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

*p ≤ 0.1, **p ≤ 0.05, ***p ≤ 0.01.

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 effect of public defenders in their law enforcement activities.

In column (2) of Table 3 we use data on arrests that are longer than one day. The reason we do that is that, as noted, in Israel the police may arrest suspects for 24 hours without bringing them to court. Thus, we know that arrests that are longer than one day had to be brought for court approval. We find that, when looking at arrests that are longer than one day, the reform led to a reduction of 15.6% in the average number of weekly arrests.

This finding is 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, probably because, 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 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. We use the maximum possible sentence (in months) that could be imposed on the suspect if he is convicted, as a measure of crime
Table 4: Effect of Reform on the Maximum Sentence of Arrestees

<table>
<thead>
<tr>
<th>Dep. Variable:</th>
<th>All Arrests</th>
<th>Arrests longer than 1 day</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Reform</td>
<td>0.0203</td>
<td>0.0425**</td>
</tr>
<tr>
<td></td>
<td>(0.0135)</td>
<td>(0.0179)</td>
</tr>
<tr>
<td>Week/Region</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Obs.</td>
<td>2496</td>
<td>2496</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.413</td>
<td>0.326</td>
</tr>
</tbody>
</table>

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

* $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

Columns (1) in Table 4 considers the effect of the reform on the average maximum possible sentence for arrestees, in logs. We do not find that the reform led to an increase in the average maximum possible sentence of arrestees. This means that the reform did not change the type of crimes the police pursued.

In column (2) of Table 4 we use data on arrests that are longer than one day. As noted, these are arrests that had to be approved by the court. We find that the reform led to an increase of 4.2% in the average maximum possible sentence of arrestees.

This finding seems to be another indicator of the effectiveness of public defenders. Although the type of crimes the police pursued did not change, only arrestees who were arrested for more severe crimes ended up under arrest for more than one day, probably because, in the presence of counsel, the court released the suspects who were arrested for more minor crime. 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 only arrestees who have been arrested for more severe crimes.
The Right to Counsel: Benefits and Costs

Table 5: Effect of Reform on Crime

<table>
<thead>
<tr>
<th>Dep. Variable:</th>
<th>log (crime)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Reform</td>
<td>0.0330***</td>
</tr>
<tr>
<td></td>
<td>(0.0130)</td>
</tr>
<tr>
<td>Week/Region</td>
<td>√</td>
</tr>
<tr>
<td>Fixed Effects</td>
<td></td>
</tr>
<tr>
<td>Obs.</td>
<td>2496</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.965</td>
</tr>
</tbody>
</table>

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

* $p \leq 0.1$, ** $p \leq 0.05$, *** $p \leq 0.01$.

3.3 Crime

Finally, we look at how the legal reform that extended the right to counsel to suspects affected crime. In Table 5 we use reported property crime, in logs, as the dependent variable. We find that the reform led to a 3.3% increase in crime.

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 and crime (e.g. Klick and Tabarrok 2005, Evans and Owens 2007, Draca et al. 2011).

What explains the increase in crime? 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 we find in crime.

4 Robustness

4.1 Region-specific Time Trend

One concern that may arise with respect to the findings in Section 3 is that they are driven by different region-specific time trends in the outcome variables. To address this concern we account for the possibility of criminal and police activity trends that may vary among regions by incorporating linear
Table 6: Effect of Reform - With Region Specific Time Trend

<table>
<thead>
<tr>
<th>Dep. Variable:</th>
<th>log (arrest duration)</th>
<th>log (num. of arrests)</th>
<th>log (crime)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(2)</td>
<td>(1)</td>
<td>(3)</td>
</tr>
<tr>
<td>Reform</td>
<td>−0.193***</td>
<td>−0.0486**</td>
<td>0.0595***</td>
</tr>
<tr>
<td></td>
<td>(0.0414)</td>
<td>(0.0206)</td>
<td>(0.0102)</td>
</tr>
<tr>
<td>Region-specific Time Trend</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Week/Region Fixed Effects</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Obs.</td>
<td>2496</td>
<td>2496</td>
<td>2496</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.406</td>
<td>0.790</td>
<td>0.983</td>
</tr>
</tbody>
</table>

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

*p ≤ 0.1, **p ≤ 0.05, ***p ≤ 0.01.

As one can see, accounting for the possibility of region specific time trends does not change the results. In fact, for crime we get that the increase in crime due the reform is significantly larger, 5.95% instead of 3.3%, when accounting for the possibility of region specific time trends in crime.

4.2 Excluding Regions

Another 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 estimates our main outcome variables – arrest duration, the number of arrests, and crime, each time with one region excluded. Table 6 presents the coefficients of 21 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.

As one can see from Table 6, our findings are not driven by one specific region in the country, as excluding any region does not fundamental change them.
Table 7: Effect of Reform - Excluding Individual Regions

<table>
<thead>
<tr>
<th>Excluded Region</th>
<th>Dep. Variable: log (arrest duration)</th>
<th>log (num. of arrests)</th>
<th>log (crime)</th>
</tr>
</thead>
<tbody>
<tr>
<td>None</td>
<td>-0.176***</td>
<td>-0.0570***</td>
<td>0.0330***</td>
</tr>
<tr>
<td></td>
<td>(0.0410)</td>
<td>(0.0206)</td>
<td>(0.0130)</td>
</tr>
<tr>
<td>Tel-Aviv Region</td>
<td>-0.203***</td>
<td>-0.0625***</td>
<td>0.0350**</td>
</tr>
<tr>
<td></td>
<td>(0.0434)</td>
<td>(0.0222)</td>
<td>(0.0146)</td>
</tr>
<tr>
<td>Central Region</td>
<td>-0.204***</td>
<td>-0.0533**</td>
<td>0.0342**</td>
</tr>
<tr>
<td></td>
<td>(0.0418)</td>
<td>(0.0217)</td>
<td>(0.0146)</td>
</tr>
<tr>
<td>Jerusalem Region</td>
<td>-0.125***</td>
<td>-0.0680***</td>
<td>0.0393***</td>
</tr>
<tr>
<td></td>
<td>(0.0425)</td>
<td>(0.0208)</td>
<td>(0.0122)</td>
</tr>
<tr>
<td>Southern Region</td>
<td>-0.173***</td>
<td>-0.0631***</td>
<td>0.0164</td>
</tr>
<tr>
<td></td>
<td>(0.0449)</td>
<td>(0.0217)</td>
<td>(0.0148)</td>
</tr>
<tr>
<td>Northern Region</td>
<td>-0.169***</td>
<td>-0.0148</td>
<td>0.0473***</td>
</tr>
<tr>
<td></td>
<td>(0.0523)</td>
<td>(0.0261)</td>
<td>(0.0163)</td>
</tr>
<tr>
<td>Haifa Region</td>
<td>-0.187***</td>
<td>-0.0767**</td>
<td>0.0280</td>
</tr>
<tr>
<td></td>
<td>(0.0589)</td>
<td>(0.0307)</td>
<td>(0.0171)</td>
</tr>
</tbody>
</table>

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

*p ≤ 0.1, **p ≤ 0.05, ***p ≤ 0.01.

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.

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.

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 of the right to counsel on various outcomes. But did the extension of the right to counsel to suspects actually led to more 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 is available in Israel since 2007, and from 2010 data with broad coverage is 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 was 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 8, 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% 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 8 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 still 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 $1.3 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 $42 million. Furthermore, the direct cost of employing public defenders to represent suspects was 10% of annual expenses of the public defender (Public Defender 2002), which come up to $2.5 million.

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

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As shown, the average weekly regional number of arrests went down by 5.7%. Using
The Right to Counsel: Benefits and Costs

based on the prison authority’s data, is $28,000. Thus, the reduction in arrest days amounts to savings of $3 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 the social cost of a day spent under false arrest is. 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 one, 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 the precise social value of this right is.

Altogether, to assess the desirability of the reform, it seems that one has to consider whether the benefit from eliminating false arrests, as well as the inherent value of representation, are greater or smaller than $40 million.

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 ending up with 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.

More generally, the findings in the paper indicate that the right to counsel, like other constitutional rights, involves benefits and costs. The benefits and costs of other constitutional rights may be investigated in a similar manner.

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.
References


