On the Tradeoffs between Privacy and Security:
Evidence from Wiretap Orders

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Abstract

Tradeoffs between privacy and security in the use of court-issued wiretap orders are examined. We collect all wiretap orders issued by the U.S. district and state court systems for narcotics-related offenses during the years 1998-2005 and all arrests and convictions resulting from those orders in the subsequent years. We extend the Beckerian theory of crime to explain the relationship between the number of people surveilled and the number of arrests and convictions. Our analysis suggests diminishing marginal returns from surveilling more people via wiretap orders; however, few data are observed near or beyond the point at which the arrests and convictions start decreasing in the number of people intercepted. This indicates that agencies, given their preferences, can choose optimal allocations between privacy and security.

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1 Introduction

A lawful interception of a wire, oral, or electronic communication by a federal and local law enforcement agency (henceforth, “wiretap”) is one of the most intrusive investigative methods in our modern-day society. For instance, mobile communications and online activities monitored covertly by law enforcement raise significant concerns about the violation of privacy and constitutional rights in criminal courts (Gunther and Sullivan, 1997). As a consequence, wiretapping is regulated in many countries by laws that aim to strike a balance between privacy preservation and crime control.

Concurrently, the growing global concern about terrorism as well as revelations about the NSA’s wide-reaching surveillance programs have brought the justification of wiretapping to the forefront of policy debates. However, the amount of empirical research has not caught up to the heightened interest in the policy domain. Of particular concern, there is no clear empirical evidence on the tradeoff between privacy and security, as demonstrated by administrative records. Policy discussions and legal discourses are often based on people’s ‘preference’ for privacy protection rather than on the extent of the tradeoffs involved between privacy and security.\(^1\) Hence, we examine the actual ‘production’ of crime prosecution resulting from wiretap surveillance that can guide policy discussions.

To be more specific, we propose a theoretical framework and document the empirical relationship between wiretap surveillance and prosecution outcomes. We conduct our empirical analysis using a panel data of federal and state wiretap orders issued during 1998-2005. We focus on Narcotics-related offenses because it comprises the vast majority of wiretap orders issued by the district (over 80 percent) and state (over 70 percent) court systems, and federal and state prosecutors are often highly motivated to infiltrate drug rings using wiretap orders. To effectuate wiretaps, policing agencies request wiretapping orders from

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\(^1\)For instance, in 2016 the German Interior Minister introduced a proposal to recalibrate the balance between security issues and privacy concerns after a shooting rampage and a bomb threat in Munich, based on popular public preferences. According to a 2015 poll, 56% of Germans said they would accept less privacy and data protection in exchange for greater security, which influenced the decision to pass the draft legislation for increasing video surveillance in public areas.
federal or local prosecutors who then file for wiretap applications in a district or state court. Primary evidence on the relationship between privacy and security is derived based on the administrative records from these orders.

An improvement in policing outcomes gained from more intensive wiretapping surveillance may constitute an acceptable reason for accepting a greater degree of privacy invasion. However, if more people are subjected to surveillance without increasing the effectiveness of criminal convictions, then there is little value in expanding surveillance and authorizing more wiretap orders. Under such conditions, the optimal policy decision would be cutting back on the extent of wiretap operations. On the other hand, if empirical evidence suggests that increased surveillance does result in greater efficiency in criminal prosecution, then it may be reasonable to trade off some erosion in privacy in exchange for enhanced prosecutorial effectiveness, particularly for important criminal cases.

The relationship between the extent of surveillance and prosecution outcomes has been relatively neglected in the literature. Furthermore, existing models in the economics of crime literature do not yield a specific prediction on the nature of the relationship between surveillance and prosecution (e.g., proportional, more than or less than proportional returns). In addition to documenting the relationship between surveillance and prosecutorial effectiveness, despite the fact that economists tend to consider diminishing marginal returns intuitive in any sort of a scale economy, it is important to understand the theoretical mechanism behind the phenomenon, and to provide empirical support for the nonlinear relationship. Such empirical evidence would be useful in determining the appropriate allocation of an agency’s budget, since wiretap operations are costly.

We follow Becker (1968) in assuming that potential criminals are rational actors. That is, criminals assess the probability of being caught and the penalties from convictions as well as the returns from committing crime. Given that drug trafficking can involve a great deal of wealth, status, and power for criminals (Campbell and Hansen, 2012) and that convictions often lead to significant penalties on their future income stream (Lott, 1992),
it seems reasonable to assume that a Beckerian rational-choice model can be applied to drug traffickers.\footnote{While it is possible that a nontrivial fraction of drug traffickers have nonrational motivations in committing other types of crime (such as Gambling, Murder and Racketeering), offenses for which Narcotics wiretap orders are issued are more likely to involve conscious efforts or decisions on the part of criminals given the timeframe, scale and complexity of drug trafficking operations.} Hence, we refine Becker’s rational-choice framework by introducing the probability of being surveilled (or searched, more generally) and show that this leads to sharp predictions regarding the nonlinear relationship between surveillance and prosecution as well as differences between federal and state orders.

There are some related theoretical works in the literature. For instance, Persico (2002) studies tradeoffs between fairness and effectiveness of law enforcement. In a model where citizens choose whether to engage in illegal activities and police audits citizens, Persico identifies conditions on the distribution of citizens’ legal earnings opportunities under which increasing the fairness of search would reduce the total amount of crime. Similarly, Mialon and Mialon (2008) develop an economic model of a society in which citizens choose whether to commit crime and law enforcement decides whether to search suspects based on some prior information. Mialon and Mialon focus on the effect of the Fourth Amendment’s exclusionary rule (that courts cannot use illegally-obtained evidence in criminal trials) and show that a stronger exclusionary rule may increase crime.

On the empirical side, the literature that studies the relationship between crime and law enforcement is rather extensive, wherein the main emphasis lies on utilizing strategies for better empirical identification. For instance, several authors rely on instrumental-variable approaches to estimate the causal effect of prison population (Levitt, 1996), police staffing (Levitt, 1997) and electronic monitoring (Henneguelle et al., 2016) on crime. Others make use of legislative changes and court rulings to directly measure the effects of the exclusionary rule (Atkins and Rubin, 2003), sentence enhancements (Owens, 2009; Vollaard, 2013), and DNA databases (Doleac, 2017) on crime. In contrast to this stream of work, our study focuses on the objective of validating the presence of diminishing returns of police surveillance, as predicted by our theoretical model.
In the criminology literature, scholars have in fact documented a number of empirical regularities (see, e.g., Chalfin and McCrary, 2014, for a recent survey). For instance, Liedka et al. (2006) examines the relationship between imprisonment and crime, demonstrating diminishing returns as the scale of imprisonment increases. Nunn (2008) studies the time trends of aggregate wiretap orders and finds that the number of individuals covered by wiretaps, interceptions and incriminating interceptions increased over time. While more individuals are intercepted and more arrests are made, convictions are found to decline over time. Chalfin and McCrary (2017) finds that U.S. cities are substantially underpoliced after taking into account the extent of measurement errors in policing data. Our finding is in line with theirs in that the use of wiretap orders seems to be largely below the point at which the returns from wiretap start decreasing.

Finally, our paper is related to the growing literature on the economics of privacy (e.g., Posner, 1981; see Acquisti et al., 2016, for a recent survey). While researchers in this area tend to focus on online advertising and other media markets, privacy issues can have nontrivial effects for wider ranging industries such as medical and financial markets (e.g., Miller and Tucker, 2009; Kim and Wagman, 2015). This paper attempts to expand the existing domain of privacy research to include crime and law enforcement, which remains an underexplored area. In the age of the Internet, privacy issues can play an important role, as digital means of monitoring individuals are increasingly deployed in policing efforts (e.g., Chan et al. 2016). The understanding of the tradeoffs between privacy and security seems to be the first step towards an optimal policy design.

The remainder of the paper is organized as follows. Section 2 lays out our theoretical framework and the model’s main predictions. Section 3 describes the dataset, and Section 4 investigates the empirical relationship between wiretap surveillance and prosecution outcomes. Section 5 concludes.
2 A Positive Model

Consider a law enforcement agency that can surveil potential criminal activity with surveillance technology. A unit mass of the broader citizenry is suspected by law enforcement of being engaged or planning to engage in crime. Suspected individuals are each privately endowed by Nature with a realization of a random variable, $v$, denoting the monetary returns associated with the crime, with support $[\underline{v}, \bar{v}]$ and a twice differentiable cumulative density function $F(v)$. Thus, while law enforcement can assess the fraction of the mass who may commit crime in a given environment, it is unable to observe which individuals may actually do so.

Following Becker (1968), we assume that those who commit crime ("criminals") and are surveilled by law enforcement are arrested with probability $\sigma \in (0, 1)$; and those who are arrested are ultimately convicted with probability $\rho \in (0, 1)$. Hence, $\sigma$ represents the efficacy of surveillance methods, which may depend on available equipment, geographic scope and other investigative tactics of enforcement agencies, and $\rho$ represents the ability of the prosecution and legal system to follow through on successful surveillance, which to a certain extent would depend on the exclusionary rule applicable to an issued wiretap order.

Suppose a potential criminal’s outside option value is normalized to zero, and the criminal’s disutility from being caught and punished by conviction is denoted by $\Gamma > 0$. It follows that the expected utility of a type $v$ individual who commits a crime is given by

$$EU(v) = v - \gamma\sigma\rho\Gamma,$$

where $\gamma \in (0, 1)$ denotes the probability that law enforcement surveils the suspect, or, equivalently, the fraction of suspects who are surveilled. Thus, only those individuals whose value from crime satisfies $v \geq \gamma\sigma\rho\Gamma \equiv \bar{v}$ would choose to commit crime.

Following the literature (e.g., Persico, 2002; Mialon and Mialon, 2008), we assume that the extent of surveillance, $\gamma$, is exogenously given conditional on the level of ‘street-level’
drug crimes. This is because prosecution needs a probable cause to apply for and be issued a wiretap order, and the lead flowing from undercover agents and/or other informants is likely beyond the control of prosecution in a given period of time. It is possible that the amount of effort put in to infiltrate a crime organization also depends on policing priorities; however, our predictions would remain robust as long as they are driven mostly by political forces rather than necessarily by crime levels.

Therefore, for a given parameterization of the model, the fraction of suspects who are arrested and ultimately convicted can be written as

$$C(\hat{v}) = \gamma \sigma \rho (1 - F(\hat{v})).$$  \hfill (2)

A change in $\gamma$, then, has a non-monotonic effect. Holding constant other parameters of the model, differentiating $C$ with respect to $\gamma$ yields

$$\frac{\partial C(\hat{v})}{\partial \gamma} = \sigma \rho (1 - F(\hat{v})) - \gamma \sigma^2 \rho^2 \Gamma f(\hat{v}),$$ \hfill (3)

which is positive (negative) when $\gamma$ is below (above) a certain threshold. The second-order derivative is given by

$$\frac{\partial^2 C(\hat{v})}{\partial \gamma^2} = -\sigma^2 \rho^2 \Gamma (2f(\hat{v}) + \hat{v}f'(\hat{v})), \hfill (4)$$

which is negative everywhere if $2f(v) + vf'(v) > 0$ for all $v \in [\underbar{v}, \overline{v}]$.

The following proposition provides an alternate condition under which there are tradeoffs between surveillance and prosecution, and surveillance exhibits diminishing returns to scale in the relevant range.

**Proposition 1** Suppose $F(\hat{v})$ exhibits an increasing generalized hazard rate. Then there exists a value, $\hat{v} \in (\underbar{v}, \overline{v})$, such that $C(\hat{v})$ exhibits diminishing returns over $[\underbar{v}, \overline{v}]$, and $\frac{\partial C(\hat{v})}{\partial \gamma} > 0$.

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3The fraction of suspects who are arrested can be obtained by substituting $\rho = 1$ into $C(\hat{v})$. Since $\rho$ enters $C(\hat{v})$ in a multiplicative manner, an analogous set of results applies to the fraction of arrests as they apply to the fraction of convictions. We will test our model’s predictions using both convictions and arrests; however, since the derivation is the same, we present here the case of the fraction of convictions.
0 if and only if $\hat{v} < \tilde{v}$.

**Proof.** Let $C(\hat{v}) = \hat{v}w(\hat{v})$, where $w(\hat{v}) \equiv (1 - F(\hat{v}))/\Gamma$. Since $\hat{v}$ is strictly increasing in $\gamma$, it follows that $C'(\hat{v})$ and $C''(\hat{v})$ have the same signs as in (3) and (4), respectively. Moreover, $C'(\hat{v})$ can be written as

$$C'(\hat{v}) = w(\hat{v})[1 + \epsilon(\hat{v})], \quad (5)$$

where $\epsilon(\hat{v}) = \frac{d\hat{w}(\hat{v})}{d\hat{v}} \frac{\hat{v}}{w(\hat{v})}$ gives the elasticity of $w(\hat{v})$, a measure proportional to the fraction of the population who commits crime, with respect to the threshold crime value, $\hat{v}$. Thus, it follows that $C''(\hat{v}) > 0$ if and only if $\epsilon(\hat{v}) > -1$.

Differentiating (5) again yields

$$C''(\hat{v}) = w'(\hat{v})[1 + \epsilon(\hat{v})] + \epsilon'(\hat{v})w(\hat{v}). \quad (6)$$

Since $w(\hat{v}) = (1 - F(\hat{v}))/\Gamma$ is decreasing in $\hat{v}$, $w'(\hat{v}) < 0$. From the above, $C'(\hat{v}) > 0$ if and only if $\epsilon(\hat{v}) > -1$. Hence, the first term in (6) is negative whenever $C'(\hat{v}) > 0$.

Let $\tilde{v}$ denote the right-most value at which $\epsilon(\tilde{v}) \leq -1$, whereby $C'(\hat{v}) \leq 0$. A sufficient condition to give diminishing returns (i.e., $C''(\hat{v}) < 0$) is that $\epsilon'(\hat{v}) \leq 0$ holds over $[\tilde{v}, \tilde{v}]$. Let $g(\tilde{v}) = \frac{\epsilon'(\tilde{v})}{1 - F(\tilde{v})}$ denote the generalized hazard rate function, and notice that we have

$$\epsilon(\tilde{v}) = \frac{d\tilde{w}(\tilde{v})}{d\tilde{v}} \frac{\tilde{v}}{w(\tilde{v})} = -\frac{\tilde{w}(\tilde{v})}{1 - F(\tilde{v})} = -g(\tilde{v}).$$

Then, it follows that $\epsilon'(\tilde{v}) < 0$ holds if $g'(\tilde{v}) > 0$, that is, if $F(\tilde{v})$ has an increasing generalized hazard rate (Lariviere and Porteus, 2001). \hfill \blacksquare

It follows from Proposition 1 that as long as the percentage change in the mass of potential criminals that results from a percentage change in the threshold value for committing crime diminishes (i.e., becomes more negative, whereby $F(v)$ exhibits an increasing generalized hazard rate), then the marginal gain from surveillance is initially positive (i.e., there exist tradeoffs), and eventually becomes negative after a certain level of surveillance activities is reached (i.e., surveillance beyond this point is inefficient). The increasing generalized hazard rate assumption thus roughly entails an increase in the magnitude of the elasticity of crime with respect to enforcement.
As we will show in the Data section, one notable difference between federal and state wiretap orders is that federal orders have about twice as high arrest and conviction rates as state orders. Therefore, in terms of the model, we can derive an additional prediction regarding the security-privacy tradeoff across the type of the issuing courts. In the proceeding, we illustrate the case of increasing the parameter $\rho$ under the assumption that $\rho$ is higher for federal orders.\(^4\) Since both $\sigma$ and $\rho$ affect the fraction of suspects who are arrested and convicted in a multiplicative way, the analysis of increasing the parameter $\sigma$ is analogous.

We refer to district-court issued wiretap orders with a subscript $d$ and state-court issued wiretap orders with a subscript $s$, whereby $\rho_d > \rho_s$ holds. We expect that the difference in $\rho$ will generate different tradeoffs in terms of the extent of surveillance, $\gamma$, and the fraction of criminals who are arrested and convicted, $C(\hat{v})$. Interestingly, the following result says that the peak of the non-linear relationship occurs at a higher level of surveillance for state wiretap orders than for federal wiretap orders.

**Proposition 2** Suppose $F(\hat{v})$ exhibits an increasing generalized hazard rate. Then we have $\arg \max_\gamma C(\hat{v}_d) < \arg \max_\gamma C(\hat{v}_s)$ if and only if $\rho_d > \rho_s$.

**Proof.** The derivative of the fraction of suspects who are arrested and convicted with respect to the scope of surveillance, $\gamma$, is

$$\frac{\partial C(\hat{v}_s)}{\partial \gamma} = \sigma \rho_s (1 - F(\hat{v}_s)) - \sigma^2 \rho_s^2 \gamma \Gamma f(\hat{v}_s) \quad (7)$$

at the state level, and

$$\frac{\partial C(\hat{v}_d)}{\partial \gamma} = \sigma \rho_d (1 - F(\hat{v}_d)) - \sigma^2 \rho_d^2 \gamma \Gamma f(\hat{v}_d) \quad (8)$$

at the federal level.

\(^4\)It has been widely reported that the requirements needed to obtain a wiretap order through a U.S. attorney (e.g., Department of Justice) is distinctively higher than those met by a state or local prosecutor (see, e.g., [http://usat.ly/1M2wPCB](http://usat.ly/1M2wPCB) and [http://usat.ly/1NL0unL](http://usat.ly/1NL0unL)). The higher standards of obtaining a federal wiretap order means that a probable cause exists to convince the judge to issue an order; therefore, it is less likely that the evidence from the surveillance will be suppressed in criminal courts.
Solving for the maximizer, \( \tilde{\gamma}_\theta \), for \( \theta \in \{s, d\} \), we have

\[
\tilde{\gamma}_s = \frac{1 - F(\sigma s \gamma \Gamma)}{\sigma s \Gamma f(\sigma s \gamma \Gamma)} = \frac{1 - F(\hat{v}_s)}{\hat{v}_s f(\hat{v}_s)} = g^{-1}(\hat{v}_s)
\]

(9)

\[
\tilde{\gamma}_d = \frac{1 - F(\sigma d \gamma \Gamma)}{\sigma d \Gamma f(\sigma d \gamma \Gamma)} = \frac{1 - F(\hat{v}_d)}{\hat{v}_d f(\hat{v}_d)} = g^{-1}(\hat{v}_d),
\]

(10)

where \( \hat{v}_d > \hat{v}_s \) is satisfied, ceteris paribus, and \( g(\hat{v}) = \frac{\hat{vf}(\hat{v})}{1-F(\hat{v})} \). Thus, if \( F \) exhibits an increasing generalized hazard rate, with \( g'(\hat{v}) > 0 \), then \( \tilde{\gamma}_d < \tilde{\gamma}_s \). ■

That federal surveillance achieves its maximum conviction at a lower level of surveillance than state surveillance can be understood intuitively. Notice that increasing the probability of conviction amounts to increasing the threshold value to commit crime, ceteris paribus. If the fraction of suspects who commit crime exhibits an elasticity that is increasing in magnitude with respect to the threshold value of crime, then at the maximum conviction an exactly proportional fraction of suspects will be deterred from committing crime as the proportional increase in surveillance.

If the probability of conviction increases at the maximum, then it means that the fraction of suspects who commit crime would decrease more than proportionally. This reduction in the fraction of suspects committing crime reduces the fraction of suspects who are arrested and ultimately convicted independent of the initial probability of arrest or conviction. This implies that the scope of surveillance has to be cut back in an offsetting manner to restore the maximum level of conviction. Thus, federal surveillance peaks at a lower level of surveillance.

As previously indicated, if we assume that federal orders are alternatively (or additionally) characterized by more effective surveillance operations, such that the arrest probability conditional on surveillance is higher than state orders (i.e., \( \sigma_d > \sigma_s \)), then a result similar to the one in Proposition 2 applies. That is, if \( \sigma_d > \sigma_s \) and \( F(\hat{v}) \) exhibits an increasing generalized hazard rate, then, ceteris paribus, federal surveillance would peak at a lower level of surveillance than state surveillance (the proof is analogous, hence, omitted).
3 Data

We digitized the information contained in the annual Wiretap Reports published by the Administrative Office (AO) of the U.S. Courts. Section 2519(3) of 18 U.S.C. requires the AO to report the number of federal and state “applications for orders authorizing or approving the interception of wire, oral, or electronic communications pursuant to this chapter and the number of orders and extensions granted or denied pursuant to this chapter during the preceding calendar year,” along with other data concerning the interceptions such as offenses under investigation, costs and duration of interception and number of arrests and persons convicted in association with each order.\(^5\)

Our primary data source is the four tables in the Appendix of the AO’s wiretap reports which list all individual wiretap orders issued by each federal and state court having wiretap jurisdiction, terminated and reported in the previous calendar year.\(^6\) For instance, if an order were issued in November 2000, then the order may not be reported in the 2000 report unless its operation was terminated in 2000 and reported. If its operation extends to, say, February 2001, then this order will be reported in the 2001 report. Since each order has information on the date of application, however, we can identify the orders by the year in which the application was filed.

The second requirement, that the order is reported to the AO, is rather important because wiretap orders may not be reported in the same year in which they are terminated. These orders are reported eventually but with some lag. For instance, the 2001 report may contain an order that “was terminated during 2000, but was not reported at that time because it was part of an ongoing investigation.” There is a sizable number of backdated reports with

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\(^5\)The Wiretap Reports do not include orders issued pursuant to the Foreign Intelligence Surveillance Act of 1978. The FISA courts only started reporting limited statistics in 2015, so little is known. However, the FISA orders are issued for foreign intelligence purpose, so its omission may not have a critical impact on our sample of Narcotics orders.

\(^6\)Specifically, Appendix Table A1 lists all district court-issued orders that are terminated and reported in a particular calendar year. Similarly, Appendix Table B1 lists all state court-issued orders that are terminated and reported in a particular calendar year. See [http://www.uscourts.gov/statistics-reports/analysis-reports/wiretap-reports](http://www.uscourts.gov/statistics-reports/analysis-reports/wiretap-reports).
such notes in the tables in the Appendix of the AO’s reports. We found that the lag can be one to three years (mostly one year) and it is rare to see an order that was reported more than three years after its date of application.

While we initially digitized the wiretap reports from 1998 to 2010, we restrict our analysis to orders that were issued during the eight year period 1998-2005. By doing so, we are able to capture backdated orders as well as delayed prosecution outcomes that were reported in later years, so that our final data set yields a complete picture of the scope of surveillance and the outcomes of prosecution during our sample period. That is, Tables A1 and B1 of the AO reports document the number of arrests and persons convicted in the same calendar year in which an order was terminated and reported.\(^7\) However, arrests and convictions may occur in the years subsequent to the year in which an order was terminated and reported.

To account for the delayed observation of prosecution outcomes, we supplement our wiretap order data with additional prosecution data that occur in years after the initial reporting of wiretap orders. These data are retrieved from Appendix Table A2 (Detail of Supplementary Data for Intercepts Authorized by U.S. District Courts and Terminated in Prior Years as Reported in A1) and Table B2 (Detail of Supplementary Data for Intercepts Authorized by State Courts and Terminated in Prior Years as Reported in B1). For some reason, Tables A2 and B2 are only available up to 2011. Hence, our choice to limit the wiretap orders up to 2005 allows for a six year lag in prosecution outcomes.\(^8\)

We limit our sample to wiretap orders that were issued for the ‘Narcotics’ crime category because otherwise there could be systematic differences in the effectiveness of wiretap operations across crime categories, so the composition of offenses can potentially affect our results. Since the loss in the observations from other offense categories is relatively small, focusing on Narcotics orders appears to be an effective way to test the model’s predictions. Though we

\(^7\) Convictions can include plea bargains. That is, a guilty or no contest plea results in a criminal conviction. However, the wiretap reports do not tell us at what stage convictions were made (i.e., plea bargain or conviction at trial).

\(^8\) There are very few cases in which arrests and convictions are reported with a lag of more than six years. Given the small number of such instances, we think that these are unlikely to bias our estimation results in a meaningful way.
are considering the sample of Narcotics-related wiretap orders, we track and thus include in our data and analysis the number of arrests and convictions subsequently made for offenses other than Narcotics (e.g., Conspiracy), for which wiretap applications were originally filed under Narcotics charge. This way we will obtain a full picture of the prosecutorial outcomes.

Our unit of analysis is at the state-year level, in which all Narcotics orders issued by federal and state courts are aggregated to the state to which the court belongs and to the year in which the wiretap application was filed. This corresponds well to our theoretical model in which there is a unit mass of population who can commit a crime. Since we normalize our data by the state population, one can think of each state and year as a sample of observation that fits the above model, with observable and unobservable differences, for which we try to control. We aggregate the individual order data from the district court system (Tables A1 and A2) and from the state court system (Tables B1 and B2) separately because the prosecutorial environment, and hence the parameters of the model, are likely to differ between federal and state orders.

The reason for choosing states as the unit of aggregation has to do with the criminal jurisdiction. According to Rule 18 of the Federal Rules of Criminal Procedure, “the government must prosecute an offense in a district where the offense was committed.” According to the Sixth Amendment, “the accused shall enjoy the right to a speedy and public trial, by an impartial jury of the state and district wherein the crime shall have been committed.” The difficulty with aggregating data at the judicial district level within state is that there are likely to be concurrent jurisdictions in which the boundaries between jurisdictions for wiretap orders are unclear. That is, wiretap applications can be concentrated in a certain judicial district within a state even though the order’s reach may not be limited to the court’s territorial boundaries, in which case we will be either over- or under-projecting the scope of surveillance and prosecution depending on the size of the judicial district’s population.

Thus, we believe that state-level aggregation is the most natural choice to test our model’s predictions and correctly infer the extent of security-privacy tradeoffs. Furthermore, aggre-
gating data at the judicial district level has a technical problem of having an ill-defined mapping between judicial territories and administrative boundaries (e.g., counties). That is, the orders listed in the wiretap reports are ordered by judicial districts, which may or may not coincide with a county. This means that defining critical control variables such as population and local police (which come from administrative units) for judicial districts would involve some amount of ad-hoc adjustments to the original data or produce a fair number of missing entries.  

Therefore, our data panel structure involves 400 observations of 50 states over the eight year period. First, there is a balanced panel that aggregates orders from district courts (Tables A1 and A2). Despite having the federal statutes (§ 2510–§ 2520) authorizing the interception of communication, it is possible that not every district court or state reports any issuance of wiretap orders in a given year. There are 98 state-year observations with no orders issued. Given that every district court can authorize a wiretap order and are under obligation to report wiretap orders, such state-year observations are treated as having zero orders. Under this coding scheme, the largest number of orders issued in a single year is 157 (New York in 2004), followed by 97 (California in 2004).

Second, there is an unbalanced panel that aggregates orders from state courts (Tables B1 and B2). Here, 55 state-year observations have no wiretap orders issued because these states did not have statutes that would allow state courts to authorize wiretap orders. Specifically, six states (AL, AR, KY, MI, MT, VT) did not have a wiretapping statute during the entire sample period while two states (ME, SC) enacted wiretap statutes during the sample period. One could simply treat these observations as having zero orders; however, we chose to drop them from our analysis because the resulting sample would be more homogeneous—in the sense that the states we consider have made similar decisions in regards to having wiretapping jurisdiction—which would lessen the issue of introducing extraneous factors in our analyses.  

Moreover, there are cases in which the state attorney generals or other special prosecutorial units (e.g., NYC Special Narcotics Bureau) report to the AO, in which case the corresponding judicial district cannot be identified other than at the state level.

However, we acknowledge that the selection issue can be present in our context. Having no state wiretap
Table 1 shows the summary statistics of the federal and state wiretap orders issued for the Narcotics offense. On average, a state had 11 and 24 orders per year issued by district and state courts, respectively. The 11 federal orders ran for a total of 442 days in a year, had over 25,000 intercepts from 1,144 persons, of which 4,420 were incriminating intercepts. The 24 state orders ran for a total of 961 days in a year, had over 45,000 intercepts from 2,806 persons, of which 14,365 were incriminating intercepts. Thus, the rate of incriminating intercepts is higher (31.7%) for state orders than for federal orders (17.5%) while the number of days in operation as well as the number of intercepts and persons intercepted per order is similar between federal and state orders.

On the other hand, the average number of arrests and convictions resulting from the orders issued by a state in a given year is 57 and 25 for federal orders, and 53 and 23 for state orders. The generally low rate of prosecutorial outcomes relative to the number of persons intercepted via wiretapping is perhaps not so surprising. However, the fact that the average number of arrests and convictions are roughly the same while the number of state orders are twice as many as those of federal orders indicates that state orders are less effective in actual prosecutions. This is all the more so despite the fact that state wiretap orders have a higher rate of incriminating intercepts. Hence, it supports the claim that the requirements needed for approving state orders are less onerous compared to those for federal orders.11

We supplement our state-year panel dataset of wiretap orders with other state-year level data to control for potential confounding effects on prosecution outcomes from wiretap operations. These controls include the number of (sworn) local law enforcement officers (i.e., excluding civilian employees), the number of arrests made by local law enforcement for all drug abuse violations (possession or sales, across all types of drugs) as reported by the FBI’s laws is a conscious choice of the state, and we do not know if such states would have had non-zero state wiretap orders, should their state courts had the wiretap jurisdiction in place. When we fill out the missing entries with zeros for a robustness check and run our regressions, the coefficients reported in Table 3 below change very little and there is no change in any of the statistical significance results.

11See, e.g., http://usat.ly/1M2wPCB and http://usat.ly/1NL0unL. Further, our conversation with former state prosecutors in major U.S. cities strongly confirm the fact that state orders can be issued much more promptly with less bureaucratic controls.
Uniform Crime Reporting (UCR), and the number of public payphones, as reported by the FCC’s Trends in Telephone Service for each state and year. We divide these variables as well as the wiretap-related statistics by the state population estimates that are reported for each year in the FBI’s UCR.

4 Empirical Evidence

The primary empirical predictions of our model are the non-linear relationship between the fraction of suspects who are surveilled and the fraction of suspects who are arrested and convicted from the surveillance activity, and the change in the non-linear relationship depending on the type of wiretap jurisdiction. It is relatively straightforward to measure these variables using our data. Specifically, we observe the number of persons intercepted for each order issued by courts, as well as the number of arrests and convictions that result from the orders in at least six years subsequent to the year of termination. As discussed in the previous section, these data were aggregated to the state to which the court belongs and the year in which the order was applied.

Because our theoretical framework is based on a unit measure population, we normalize the data by the size of state population (in thousands). Also, the theoretical model assumes a well-defined probability distribution from which the monetary returns in association with a crime are drawn for each individual. By restricting attention to wiretap orders that were issued for the same (Narcotics) category, this assumption is also reasonably satisfied. However, it is still likely that states will have different levels of prosecution outcomes that reflect conditions of the state other than the direct theoretical relationship from the extent of surveillance. Consequently, we include state and year fixed effects to account for spurious correlation due to heterogeneity.

Our baseline specification is:

\[
Outcome_{st} = \beta_1 Surveil_{st} + \beta_2 Surveil^2_{st} + X_{st}' \gamma + \delta_s + \zeta_t + \epsilon_{st},
\]  

(11)
where $\text{Outcome}_{st}$ is the number of arrests (or convictions) resulting from Narcotics wiretap orders issued by district (or state) courts in state $s$ and year $t$ per thousand population; $\text{Surveillance}_{st}$ is the number of persons intercepted per thousand population through the corresponding set of orders in state $s$ and year $t$; $X'_{st}$ includes the number of law enforcement officers, drug-related arrests and public payphones, divided by the state population (in thousands); $X''_{st}$ also includes characteristics of wiretap operations such as the sum of total costs, divided by the number of wiretap orders; $\delta_s$ and $\zeta_t$ are the state and year fixed effects and $\epsilon_{it}$ is an i.i.d. error term. The above equation is then estimated by Ordinary Least Squares where the standard errors are clustered at the state level.

The rationale for the inclusion of the external control variables in $X'_{st}$ is as follows. Ceteris paribus, a larger number of local law enforcement officers may increase the number of arrests and convictions made out of wiretap operations because more manpower is available in any contingencies to work with state or federal agencies (such as DEA and FBI) that initiate the wiretap orders. That is, wiretap operations involve significant overtime hours and also require a dispatch of contingent forces to protect the officers in the field.

The number of arrests made for drug offenses (due to sale or possession of various drugs) by local police is also indicative of the prevailing level of drug-related street crimes. That is, a state laden with many drug-related arrests is, on average, more likely to draw drug-trafficking bosses who are the main targets of the wiretap operations than a state with few drug-related arrests. We note here that the arrests made for drug offenses as reported in UCR are most likely to be independent of the wiretap operations.\(^{12}\)

Investigative journalists have argued that, in the past, drug dealers set up a field office around public payphones, where a caller’s identity can remain anonymous. Thus, the presence of a larger number of payphones can make wiretap operations less successful. However, the number of public payphones has been declining during our sample period, because it

\(^{12}\)We inquired multiple times and consulted with Crime Stats staff on the possibility of an overlap in arrest data, and were told that “If the data are reported by the local agency, it would fall under ‘all other arrests' and could not be isolated at the national Program level. Therefore, the UCR Program would not know if arrests were the result of wiretaps.”
became less and less profitable given the high mobile penetration rate. Hence, we include
the number of payphones to control for this changing environment.\textsuperscript{13}

In some specifications, we will also include in $X'_{st}$ the state-year level characteristics of the
wiretap operations as additional controls. Specifically, they are the sum of days in operation
(in thousands), the sum of incriminating intercepts (in thousands), and the sum of total
costs (in million dollars) for wiretap operations, divided by the number of wiretap orders
issued in state $s$ and year $t$. The only issue here is that we lose some samples if we include
these characteristics because they are missing variables in the state-years in which no order
was issued (despite having wiretap jurisdictions).

Table 2 shows the estimation results using the balanced panel of federal wiretap orders.
The first two estimated coefficients show that both the linear and quadratic terms have the
expected signs and are statistically significant with the exception of those in column (4)
where the estimates are only marginally significant. Thus, we find evidence that generally
supports the first prediction of our Beckerian model. To be more specific, the non-linear
relationship between the extent of surveillance and the outcomes of prosecution is supported
regardless of whether or not the control variables are included. (In an unreported analysis,
the results are unaffected if we drop all six control variables.)

Using the specifications in columns (2) and (4), the peak point at which the number
of arrests and convictions per thousand population starts decreasing can be calculated as
1.21 and 1.23 persons intercepted per thousand population, respectively. The support of the
persons intercepted is heavily skewed to the right and, more importantly, most observations
lie well below the estimated peak point (see Figure 1). Indeed, there are only six occasions in
which the number of persons intercepted per thousand population is greater than one. Thus,
it appears that most states in most years are making effective tradeoffs between security and
privacy in the sense that the number of persons intercepted is positively associated with the
\textsuperscript{13}It is a priori unclear whether mobile or payphones are more difficult to surveil. However, it was later
discovered that drug agencies have often provided local law enforcement with far-reaching surveillance tools
that can identify and locate mobile devices within a certain radius by emulating a cell tower (a device known
as a “stingray”).
number of arrests and convictions made out of the wiretap operations.

Figure 1: Histogram of persons intercepted per thousand population, federal orders. Arrow indicates convictions peak point.

The lower standards for obtaining state wiretap orders would change the parameterization of the model and thus the data-generating process for these orders. As we showed in Section 2, if state orders are less effective in prosecuting criminals in courts, whereby their conditional probabilities of arrests and convictions decrease, then our model predicts that state orders should have a higher peak point than federal orders in terms of the fraction of people surveilled. We test this hypothesis using the panel data of state orders. To be clear, the main variables, \( \text{Outcome}_{st} \) and \( \text{Surveil}_{st} \), as well as the controls in \( X'_{st} \) are now based on state orders while UCR and FCC data are the same.

Table 3 shows the estimation results using the state order data. The non-linear relationship that was identified using the federal order data between the number of persons intercepted and the resultant number of arrests and convictions remains unaffected, as the first two estimated coefficients have the expected signs and the quadratic term is statisti-
cally significant in all four specifications. The difference is that the peak point is now much larger for state orders than for federal orders, consistent with our prediction. Specifically, the peak points for arrests and convictions from state orders can be calculated as 4.61 and 4.14, respectively, using the specifications in columns (2) and (4).

![Figure 2: Histogram of persons intercepted per thousand population, state orders. Arrow indicates convictions peak point.](image)

The number of state-year observations that exceed the peak points in the state order panel remains low, and a large majority of the observations are still well below the peak points (see Figure 2). Specifically, there are only three occasions in which the number of persons intercepted per thousand population is greater than four. This is somewhat surprising because one might have expected that state prosecution tends to overachieve the scale of surveillance activities given the purportedly weaker standards for applying and receiving wiretap orders from state courts. However, this turns out to be more subtle; even though state orders have a larger magnitude of surveillance in terms of the number of people intercepted, the peak point in the security-privacy tradeoff are not overstepped.
Table 4 lists the state-year observations from the two datasets (i.e., federal and state order panels) where the number of persons intercepted per thousand population is either near the respective peak point or significantly exceeds it. The exact cause for these inefficient observations is difficult to pin down. It is possible that during those years, these states were dealing with particularly secretive drug trafficking operations, which may have led law enforcement to intercept a large number of innocent individuals. Alternatively, law enforcement may have pursued a large number of marginal drug trafficking operations in those years due to local agenda-setting. Given the small number of these outliers relative to the sample, it is difficult to relate any factor with a sufficient statistical power.

Nonetheless, we note that those states that were near or went over the peak point in the federal wiretap operation are located on the West Coast, which may indicate that federal agencies such as the DEA and FBI were battling to block a major drug trafficking route from the Pacific. On the other hand, it is New York (and Delaware) which went over the peak point in the state wiretap operation. In fact, New York has in place dedicated drug units that regularly pursue state wiretap orders (specifically, NY Organized Crime Task Force and NYC Special Narcotics Bureau), and Mayor Giuliani in November 1997 announced that a major antidrug strategy would begin, involving the deployment of a thousand officers against drug traffickers. Thus, the surge in the scope of wiretap in 1998 could be due to the Mayor’s policy initiative.

5 Conclusion

We presented a theoretical framework and showed evidence of clear diminishing marginal returns from wiretap orders for Narcotics offenses. Consistent with our model’s predictions, the number of arrests and convictions from wiretap orders increases at a decreasing rate and exhibits a peak at a higher level of surveillance for state orders than for federal orders. We further showed that few state-year observations are located near or beyond the point at

See, e.g., https://nyti.ms/2f9IJCk.
which arrests and convictions start decreasing in the number of persons intercepted. This indicates that wiretap operations during our sample period are largely located on the far left side of the wiretapping Laffer curve, whereby a reduction in the extent of surveillance tends to decrease prosecutorial outcomes.

This suggests that the extent of current U.S. wiretap operations is likely strictly limited by budget considerations, as wiretapping tends to be one of the most expensive enforcement activities, largely due to overtime payments and the manpower involved. Of course, harms to privacy may also result from overreaching surveillance activities. For instance, there are obvious concerns over using wiretap orders in violation of the Fourth Amendment. Therefore, a clear direction for future research is to conduct a complete cost-benefit analysis by assessing damages from drug trafficking crimes and comparing them with wiretapping costs. Nonetheless, the analysis contained in this paper allows us to get a glimpse into where American society stands in the debate on privacy-security tradeoffs.

References


<table>
<thead>
<tr>
<th></th>
<th>Federal Orders (N=400)</th>
<th>State Orders (N=345)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>From Wiretap Reports:</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of arrests (all years)</td>
<td>57.3</td>
<td>52.8</td>
</tr>
<tr>
<td></td>
<td>(88.3)</td>
<td>(130.5)</td>
</tr>
<tr>
<td>No. of convictions (all years)</td>
<td>25.0</td>
<td>23.0</td>
</tr>
<tr>
<td></td>
<td>(40.8)</td>
<td>(59.9)</td>
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<tr>
<td>No. of wiretap orders issued</td>
<td>10.6</td>
<td>23.9</td>
</tr>
<tr>
<td></td>
<td>(18.0)</td>
<td>(73.9)</td>
</tr>
<tr>
<td>No. of days in operation</td>
<td>441.9</td>
<td>961.3</td>
</tr>
<tr>
<td></td>
<td>(779.8)</td>
<td>(3,956)</td>
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<tr>
<td>No. of persons intercepted</td>
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<td>2,806</td>
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<tr>
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<td>(3,972)</td>
<td>(11,127)</td>
</tr>
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<td>No. of intercepts</td>
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<td>45,360</td>
</tr>
<tr>
<td></td>
<td>(44,084)</td>
<td>(195,879)</td>
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<tr>
<td>No. of incriminating intercepts</td>
<td>4,420</td>
<td>14,365</td>
</tr>
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<td></td>
<td>(8,007)</td>
<td>(88,771)</td>
</tr>
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<td>Total costs (in $1,000)</td>
<td>738.4</td>
<td>892.9</td>
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<td></td>
<td>(1,298)</td>
<td>(2,828)</td>
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<tr>
<td><strong>From UCR Program:</strong></td>
<td></td>
<td>[Same Source]</td>
</tr>
<tr>
<td>No. of sworn police officers</td>
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<td></td>
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<td></td>
<td>(15,118)</td>
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<tr>
<td>No. of drug-related arrests</td>
<td>29,934</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(44,850)</td>
<td></td>
</tr>
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<td>Population size (in 1,000)</td>
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<td></td>
<td>(5,693)</td>
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<tr>
<td><strong>From FCC Report:</strong></td>
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<td>No. of public payphones</td>
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Only Narcotics orders are aggregated to the state to which courts belong and the year in which orders were applied. The first two columns aggregate federal orders only and the next two columns aggregate state orders only. Arrests and convictions that result in subsequent years are included accounted for in the year of application. The variables represent an arithmetic sum across orders within state and year.
TABLE 2: RESULTS FROM WIRETAP ORDERS (Federal Orders)

<table>
<thead>
<tr>
<th></th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
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<tbody>
<tr>
<td></td>
<td>Number of Arrests</td>
<td>Number of Convicts</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(p.p.)</td>
<td>(p.p.)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Persons Intercepted (p.p.)</td>
<td>.0414 (.0081)***</td>
<td>.0233 (.0077)***</td>
<td>.0190 (.0055)***</td>
<td>.0095 (.0060)</td>
</tr>
<tr>
<td>Number of Persons Intercepted (p.p.) Squared</td>
<td>−.0162 (.0034)***</td>
<td>−.0096 (.0028)***</td>
<td>−.0073 (.0021)***</td>
<td>−.0039 (.0022)*</td>
</tr>
<tr>
<td>Number of UCR-reported Sworn Officers (p.p.)</td>
<td>.0052 (.0058)***</td>
<td>.0069 (.0096)</td>
<td>.0011 (.0039)</td>
<td>.0015 (.0071)</td>
</tr>
<tr>
<td>Number of UCR Drug-related Arrests (p.p.)</td>
<td>.0004 (.0006)</td>
<td>.0006 (.0007)</td>
<td>.0004 (.0004)</td>
<td>.0005 (.0005)</td>
</tr>
<tr>
<td>Number of Payphones (p.p.)</td>
<td>.0001 (.0005)</td>
<td>−.001 (.0007)</td>
<td>−.0002 (.0004)</td>
<td>−.0004 (.0006)</td>
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<tr>
<td>No. of Days in Operation per order</td>
<td>−.0044 (.0874)</td>
<td>.0603 (.0269)**</td>
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<tr>
<td>No. of Incriminating Calls per order</td>
<td>.0004 (.0015)</td>
<td>.0001 (.0008)</td>
<td></td>
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<tr>
<td>Total Operating Cost per order</td>
<td>−.0014 (.0109)</td>
<td>−.0138 (.0061)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>State FE</td>
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</tr>
<tr>
<td>R²</td>
<td>0.5207</td>
<td>0.6007</td>
<td>0.4783</td>
<td>0.5488</td>
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<td>N</td>
<td>400</td>
<td>302</td>
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Dependent variable in (1) and (2) is the number of total arrests resulting from all Narcotics-related wiretap orders issued by a district or a state court in a calendar year, divided by the state population (in thousands); Dependent variable in (3) and (4) is the number of total convictions resulting from the same set of orders, divided by the state population (in thousands). All columns are estimated using a balanced panel of 50 States over the period 1998 to 2005. (p.p.) denotes per thousand population. Standard errors are clustered at the state level and shown in the parenthesis. Statistical significance is denoted by *** for the 1% level, ** for the 5% level, and * for the 10% level.
## TABLE 3: RESULTS FROM WIRETAP ORDERS (State Orders)

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<tr>
<td></td>
<td>Number of Arrests</td>
<td>Number of Convicts</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(p.p.)</td>
<td>(p.p.)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Persons Intercepted (p.p.)</td>
<td>.0050 (.0012)**</td>
<td>.0046 (.0019)**</td>
<td>.0024 (.0013)*</td>
<td>.0024 (.0015)</td>
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<tr>
<td>Number of Persons Intercepted (p.p.) Squared</td>
<td>-.0005 (.0002)**</td>
<td>-.0005 (.0002)**</td>
<td>-.0003 (.0001)**</td>
<td>-.0003 (.0001)**</td>
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<td>Number of UCR-reported Sworn Officers (p.p.)</td>
<td>-.0064 (.0040)</td>
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<td>-.0035 (.0021)</td>
<td>-.0071 (.0038)*</td>
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<tr>
<td>Number of UCR Drug-related Arrests (p.p.)</td>
<td>-.0001 (.0005)</td>
<td>.0003 (.0009)</td>
<td>-.0002 (.0002)</td>
<td>-.0002 (.0003)</td>
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<td>Number of Payphones (p.p.)</td>
<td>-.0015 (.0005)**</td>
<td>-.0019 (.0010)**</td>
<td>-.0008 (.0004)**</td>
<td>-.0010 (.0007)**</td>
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<tr>
<td>No. of Days in Operation per order</td>
<td>-.0744 (.0631)</td>
<td>-.0587 (.0423)</td>
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<td></td>
</tr>
<tr>
<td>No. of Incriminating Calls per order</td>
<td>-.0005 (.0023)</td>
<td>-.0009 (.0011)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total Operating Cost per order</td>
<td>.0363 (.0240)</td>
<td>.0285 (.0201)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>R²</td>
<td>0.7786</td>
<td>0.7761</td>
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<tr>
<td>N</td>
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<td>194</td>
<td>345</td>
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Dependent variable in (1) and (3) is the number of total arrests resulting from all Narcotics-related wiretap orders issued by a district and a state court, respectively, in a calendar year, divided by the state population (in thousands); Dependent variable in (2) and (4) is the number of total convictions resulting from the same respective set of orders, divided by the state population (in thousands). In columns (3) and (4), six states (AL, AR, KY, MI, MT, VT) are excluded because their state court systems did not wiretap jurisdiction during the sample period; some early years from two states (ME, SC) are excluded because they started having wiretap jurisdiction in the middle of the sample period. (p.p.) denotes per thousand population. Standard errors are clustered at the state level and shown in the parenthesis. Statistical significance is denoted by *** for the 1% level, ** for the 5% level, and * for the 10% level.
<table>
<thead>
<tr>
<th>Over Peak</th>
<th>State</th>
<th>Year</th>
<th>NPI (p.p.)</th>
<th>State</th>
<th>Year</th>
<th>NPI (p.p.)</th>
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</thead>
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<tr>
<td></td>
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<td>1998</td>
<td>2.531</td>
<td>NY</td>
<td>1998</td>
<td>8.399</td>
</tr>
<tr>
<td></td>
<td>CA</td>
<td>2000</td>
<td>2.500</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>HI</td>
<td>2003</td>
<td>1.585</td>
<td></td>
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<td>Near Peak</td>
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<td>2005</td>
<td>1.217</td>
<td>NY</td>
<td>1999</td>
<td>4.580</td>
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<td></td>
<td>HI</td>
<td>2005</td>
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<td>2004</td>
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<tr>
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<td>UT</td>
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<td>1.022</td>
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<tr>
<td>Below Peak</td>
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<td>1998</td>
<td>0.767</td>
<td>NV</td>
<td>1998</td>
<td>3.745</td>
</tr>
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NPI (p.p.) is the number of persons intercepted (per thousand population). Below Peak only shows the highest NPI (p.p.) from the rest of the state-year observations.